Fritz Machlup and Marginalism: a Reevaluation*

Richard N. Langlois
University of Connecticut

Roger Koppl
Fairleigh Dickinson University

*The authors would like to thank Lawrence Boland, Bruce Caldwell, Ludwig Lachmann, Brian Loasby, Richard Nelson, Gerald O'Driscoll, Mario Rizzo, and John Thorkelson for helpful comments on earlier drafts.

Introduction

With few exceptions, writers who wish to assault the citadel of neoclassical economic theory have found it necessary first to attack two methodological positions widely perceived as the principal guard-posts of orthodoxy — those of Milton Friedman and the late Fritz Machlup.

Friedman's discussion is by far the better known and the more often cited. Indeed, "The Methodology of Positive Economics" (Friedman, 1953 [1977] remains a source of controversy — and confusion — three decades after it was written. In response to a hoary line of criticism that objects to the premises of neoclassical theory (especially the postulate of maximizing behavior) because they are "unrealistic", Friedman asserted that realism is irrelevant as an attribute of the assumptions of a scientific theory: all that matters is the ability of the theory's predictions to survive empirical tests.

Criticisms of this position have been many and varied.[1] Most critics have focused in on the question of what it means for an assumption to be realistic. If to be realistic means that an assumption about behavior must cast that behavior in a form a human being would recognize as human, then to say that assumptions may (or should) be unrealistic is to endorse a methodology with an impoverished conception of explanation; moreover, it is to proscribe the use of subjective knowledge in a theory and to restrict economists to the sort of objective or external knowledge to which natural scientists are limited. (Coddington, 1972.) Indeed, such an interpretation of the assumptions-don't-matter thesis would commit one to a view of scientific theories called instrumentalism — "the view that theories are only useful tools or instruments and they are not intended to be true." (Boland, 1982, p. 143.) Friedman has acknowledged that such instrumentalism is precisely what he had in mind.[2] (*Ibid*, p. 171)

Machlup's arguments, though usually lumped together with Friedman's, are actually strikingly different.[3] Unlike Friedman, Machlup is not an instrumentalist or even a falsificationist. In fact, he was one of very few economists skeptical of naive empiricist doctrines at a time when they were all the rage. (Machlup, 1978, ch. 5.) Moreover, Machlup's metodological perspective has always been informed by the German-language tradition associated with names like Max Weber and Alfred Schutz, a tradition stressing the importance of the ideal type and the desirability of including in theories certain kinds of subjective knowledge gained by putting oneself in the position of the economic agent. (Machlup, 1978, chs. 8-11.)

Some of the blame for the homogenization of Friedman and Machlup must rest with Machlup himself. First of all, there is arguably one core proposition on which the two authors agree: that the premises or assumptions of a theory need not be checked directly against empirical data. As we shall argue, though, this assertion arises out of two methodological systems that are otherwise fundamentally different. At another level, Machlup identified himself with Friedman's position by suggesting in a footnote that he saw but one "serious flaw in the otherwise excellent essay...by Milton Friedman." (Machlup, 1955, p. 17 [1978, p. 153].) As we shall see, however, this single flaw ought perhaps to weigh as heavily in the comparison with Machlup as the entire remainder of Friedman's article.

But the object of this essay is not to compare Machlup's position with that of Friedman, except perhaps incidentally. Rather, we intend to reexamine Machlup's position in light both of present-day orthodoxy and of present-day heterodoxy. For what is inevitably overlooked in discussions of Machlup as defender of marginalism is that what he defends is actually a position more than a little *different* from neoclassical economics as it is usually practiced — a position, in fact, that is almost as much an attack on orthodoxy as it is a justification of marginalism.

Machlup on Methodology

In order to understand Machlup's defense of marginalism, we need first to understand his methodological starting point. And to understand Machlup's position on — and his contribution to — methodology, we need to begin with a little intellectual history. An oversimplified, but we hope not misleading, account of the history of methodological discussion might run as follows. (Blaug, 1980; Caldwell, 1982.)

As early as Cairnes and J.S. Mill in the nineteenth century, methodologists had rejected induction as an approach to formulating scientific theories. Instead, they endorsed a deductive or *a priori* method, in which one derives conclusions from postulates whose source need not be direct observation.[4] This was not by any means a complete rejection of induction, however, as many writers (especially the early ones) were confident that, while direct observation may be a poor *source* of theories, it could nonetheless be counted on to *confirm* theories — to determine whether a particular hypothetico-deductive structure is true.

Despite barrages of criticism from various Historicist and Institutionalist writers, the mainstream of methodological discussion in economics after Mill moved resolutely further in the direction of *a priori* theory. By the time the second great wave of subjectivism struck economics in the 1930s (the first having arrived in the 1870s, of course), the extreme apriorism of Mises, Robbins, and Knight was arguably the dominant methodological position in the discipline. These writers argued that the basic postulates of economics are so fundamental to human experience that they require no direct check against observation, except perhaps to determine their applicability in particular instances; moreover, since valid conclusions follow from valid premises, the truth of a theory can be determined without subjecting even its *conclusions* to empirical testing.

This apriorist position also entailed a doctrine called methodological dualism, "the startling view that theories or hypotheses about social questions should be validated in ways that are radically different from those used to validate theories or hypotheses about natural phenomena." (Blaug, 1980, p.47.) This dualist position is closely bound up with the notion of understanding (*Verstehen*), about which we'll have more to say presently. For the methodological dualist, the possibility of using understanding in the social sciences throws up an impenetrable barrier between the methods appropriate to those sciences and the methods of the natural sciences. The doctrine was particularly significant in that, on the other side of the barrier, the philosophy of science had moved in a direction opposite to the one being taken in economics. The logical positivists of the Vienna Circle sought to expunge from science all modes of thought they viewed as metaphysics, and tried to locate the criterion of meaning solely in the correspondence of a statement with sense data. In the early decades of the century, this positivist enterprise, which was continued with greater sophistication by the logical empiricists, was as dominant in its own province as apriorism was in economics. (Caldwell, 1982, chs. 2 and 3.

The twain met in 1938, when Terence Hutchison challenged economists to take up the positivist program. Following the positivist doctrine that the only meaningful statement is one that can be confirmed by observation, he argued that the basic postulates of economics as then practiced were mere tautologies, and that meaningful postulates would be possible only if they were casted in a form susceptible to direct empirical verification. (Hutchison, 1938.)

And it is here, of course, that Machlup enters the methodological discussion — to mediate an argument between positions he characterizes as "extreme apriorism" and "ultra-empiricism." But in order fully to appreciate Machlup's methodological contribution, we need to let the reel play all the way to the end.

In the wide middle ground that Machlup rightly sees as lying between these two camps, one figure came very quickly to dominate the terrain. In a work first published in German in the mid '30s, Karl Popper introduced the notion of falsificationism. (Popper, 1959.) Both the apriorists and the positivists were wrong, he argued in effect, for believing that one could ever *confirm* a theory by any means. In particular, empirical evidence can at most prove a statement false — since the presence of corroborative evidence does not demonstrate that disconfirming evidence will not eventually turn up. Thus, the criterion for the empirical meaningfulness of a statement — or the scientific character of a theory — should not be its ability to be confirmed but its capacity to be falsified by observation.

The next episode in our hasty history takes place in the early 1960s with the publication of Thomas Kuhn's *The Structure of Scientific Revolutions*. (Kuhn, 1970.) As the story is normally told, the effect of Kuhn's well-known and controversial book was to call into question the notion (shared by Popper and the positivists) that science is essentially a dialectic between theory and observation — the notion that, one way or another, facts test theories. In Kuhn's story of paradigms and revolutions, it appears that the principal dialectic is (ought to be?) between one theory — one scheme of thought — and another. Moreover, Kuhn's work had the effect of calling into question the very idea that science is a business of discovering the truth (or even falsity) of theories; science may be nothing more than the business of choosing "better" theories — not of discovering true theories or weeding out false ones. Effectively, then Kuhn legitimized and placed on the agenda the doctrine of *conventionalism*, which holds, broadly speaking, that theories (or their constituent terms) are to be judged by (or should be thought of as) conventions established by scientists. (We shall be more precise about this shortly.)

Whatever the merits of Kuhn's analysis as philosophy of science (as distinct, perhaps, from the sociology of science) it nonetheless precipitated the dethronement of Popperian falsificationism as a dominant and unifying methodological position. No single position has risen to take its place, and the present-day discussion is characterized by a number of divergent views. (Blaug, 1980, chapter 2; Caldwell, 1982, chapter 5.) But a first among equals is clearly the methodology of scientific research programs (MSRP) put forward by Imre Lakatos (1970). This approach is perhaps best understood as a kind of compromise between Popper and Kuhn, one that "captures the advantages" of apriorism, falsificationism, and conventionalism, "without sharing some of their disadvantages' (Latsis, 1976b, p. 2.) In particular, the MSRP joins with apriorism in refusing to relinquish the hard core of a research program in the face of any empirical evidence; it accords with conventionalism in its tolerance of anomalies, those inconsistencies or apparent points of disagreement with empirical evidence; and it attempts to incorporate the spirit of falsificationism by proscribing *ad hoc* hypotheses as a way of patching up an anomaly-ridden program. (Latsis, 1976b, p. 3.)

Understanding these more recent developments in the philosophy of science is useful for understandiing Machlup's methodological views. For, in many respects, his position is precisely an *anticipation* of these modern developments.

Latsis is quite correct in seeing Machlup as basically a conventionalist. But there is also an important sense in which Machlup's compromise between apriorism and ultra-empiricism bears a strong resemblance to the Lakatosian compromise between Kuhn and Popper.

It is important to recognize that Machlup is a conventionalist in a very specific sense. In the introductory material to his 1978 collection of methodological essays, he addresses Latsis on this point. "He labels me a conventionalist," writes Machlup, referring to Latsis — "in the sense of one who accepts as meaningful and useful basic propositions that make no assertions but are conventions (resolutions, postulates) with regard to analytic procedure. I accept this label."[5] (Machlup, 1978, p. 460.)

As Latsis explains it, conventionalism is an outgrowth of Kantian apriorism in which, however, the mind "is not imprisoned in Kant's eternal categories, but can freely chose its framework and then, by imaginative adjustments, adjust it to accommodate all experience." (1976,b, p. 9.) The implication is that, to a conventionalist, facts are never decisive among theories; in Pareto's words, "the same facts may be explained by an infinity of theories, equally true...." (Pareto, 1909, p. 31, quoted in Latsis, *loc. cit.*) Even though Latsis's depiction of conventionalism is actually far broader than the specific form to which Machlup admits,[6] it does not seem unfair as a description of Machlup's view. Using the word "model" instead of "theory," Machlup makes throughout his writings a number of assertions similar to that of Pareto. For example, in 1952 he suggested that "there can be many different models schematizing the same set of facts. Whether one model or another is 'better' is often merely a matter of taste or habit of thought." (1978, p. 98; See also 1963, p. 58.) Moreover, Machlup cites approvingly Einstein's view that concepts in physics are "free creations of the human mind" not uniquely determined by the external world. (1955, p. 9[1978, p. 145].)

To a large extent, Machlup's conventionalism serves a function similar to that of the protective belt concept in the Lakatosian MSRP — which shouldn't be surprising, since the protectiive belt is precisely the conventionalist part of the Lakatosian compromise. For Machlup as for Lakatos, conflicts between a theory (or theoretical system) and observation are never decisive in refuting that theory (or system). "Empirical research designed to verify or disprove marginal productivity theory...is beset with difficulties," Machlup wrote in 1946. "Few systematic endeavors have been made and none has led to any suggestion, however vague or tentative, of an alternative theory." (1946, p. 547.) These two themes — the difficulty of empirical verification (or falsification) and the importance of competing theories — reappear often in his writing. In 1952 he challenged those who complain about the lack of reality in a model to "provide a substitute model with more realistic assumptions which nevertheless does all that the rejected model can do." (1978, p. 79.) And, in 1955, he argued that the assumptions of a model "may well be rejected, but only with the theoretical system of which they are a part, and only when a more satisfactory system is put in its place." [7] (1955, p.11 [1978, p. 147].)

In his 1955 piece "The Problem of Verification in Economics." Machlup constructs a "model of an analytic apparatus," couched in a mechanical metaphor, to illustrate his thesis. "The machine consists," he says, "of many parts, all of which represent assumptions or hypotheses of different degrees of generality. The so-called *fundamental assumptions* are a fixed part of the machine; they make the machine what it is; they cannot be changed without changing the character of the entire machine. All other parts are exchangeable, like coils, relays, spools, wires, tapes, cylinders, records, or mats, something that can be selected and put in, and again taken out to be replaced by a different piece of the set." (1955, p. 12 [1978, p. 148], emphasis original.) In other words, a theoretical system must have a hard core as well as a (multi-layered) protective belt.

It is in this sense that Machlup anticipates the conventionalist trend in the modern philosophy of science. As he writes elsewhere in his 1955 article, "the strength of belief in a hypothesis depends, even more than on any direct empirical tests that it may have survived, on the place it holds within a hierarchical system of interrelated beliefs."[8] (1955, p. 5 [1978, p. 141.].) It is not without justice that some writers have found puzzling Machlup's apparent ambivalence toward the empirical. (Blaug, 1980, p. 115.) But the picture clears quickly as soon as one understands Machlup's ultimate criterion for appraising a theoretical system: the criterion of *ah-haahhness*, the "feeling of relief and satisfied curiosity — often expressed in the joyous exclamation 'ah haahh!' — [that] comes to most analysts only when the observed regularities can be deduced from general principles which are also the starting point — foundation or apex as you like — of many other chains of causal derivation."[9] (1955, p. 9[[1978, p. 145].)

In one sense, observation plays essentially *no* role for Machlup in the process of theory choice. "Observed regularities" serve as the givens a theory is supposed to explain, and it is in this way alone that theories are connected with observation. Since many theories can be applicable to the same facts, those data can never *distinguish* among alternative applicable theories.[10] Moreover, "observed" regularities to Machlup are themselves the product of theorizing.

> The "facts" of the social sciences cannot be "observed" with the five senses. If Mr. A is observed carrying an object from Mr. B's store, the observers cannot know whether he has bought it, borrowed it, stolen it, or was given it as a present. Perhaps A and B do not agree, one regarding the transaction as a gift, the other as a theft. The subjective meanings of the actors and the interpretations by record keepers and record analysts are essential. (1960, p. 581 n. 48 [1978, p. 187].)

Nevertheless, there remains a sense in which, for Machlup, observation *does* play a role in theory choice. Machlup shares with Lakatos a desire to test (verify) models and theoretical systems, but none of the more-or-less well-defined procedures suited to the physical sciences can be exported to the social sciences. The need to grasp subjective meanings requires us to be content with the somewhat vague standard of ah-haahhness. And that standard calls for what is in effect a dialogue between theory and observation — a dialogue whose success is to be appraised by the ah-haahhness it elicits.[11]

Machlup's attitude toward the possibility of systematic testing procedures in the social sciences accounts for some of the dissimilarities between his ideas and those of Lakatos. In particular, Machlup does not see the replacement of parts within the protective belt as a process that reveals the progressivity or degeneration of the theoretical system. He shows little of the falsificationist ethic that calls for changes in the protective belt to yield problem shifts that have "excess empirical content." (Lakatos, 1970, p. 134.) To Machlup, the exchangeable parts are varied so as to make the fixed parts applicable to the particular problem at hand. If the theoretical structure is logically consistent, "one may say that the machine and its parts are always 'correct,' regardless of what goes on around us"; what may be wrong is our choice of the exchangeable parts or our perception of the nature of the problem. (Machlup, 1955, p. 18[1978, p. 154].)

On the one hand, this view embodies a conventionalist's impatience with the falsificationist quest for potential falsifiability in a theory; to the conventionalist, the falsificationist has not shown that the theory is potentially wrong, but that it... is potentially inapplicable. (Machlup, 1955, pp. 18-9 [1978, pp. 154-5].) On the other hand, however, one shouldn't jump to the conclusion that Machlup's view is totally at odds with the Lakatosian desire to appraise the progressivity of a research program. For another way to put the matter is that, to a conventionalist (or to Machlup at any rate), what it *means* to say that one has falsified (or disconfirmed) a theory is to say that one has *shown it inapplicable*. The distinction between having shown something wrong and having shown it inapplicable may in some (though not in all) cases be one without a difference.[12] Moreover, the notion of progressivity in a research program is arguably not unrelated to the Machlupian criterion of ah-haahhness: the feeling that comes when "observed regularities can be deduced from general principles which are also the starting point...of many other chains of causal derivation."

It is not perhaps entirely without cause that Machlup's approach is routinely characterized as a defensive methodology (Latsis, 1976b, p. 2) — one designed largely to protect existing theoretical structures from methodological or empirical attack. But what is inevitably overlooked in discussions of Machlup's position is the existence of a strong aggressive component. That is, he did in fact set down clear and specific requirements that an economic theory must meet. In part, these requirements constitute Machlup's version of the marginalist positive heuristic; and we will examine his defense of marginalism from this standpoint presently.

In another sense, however, the aggressive component in Machlup's methodology is best understood as a vestige of apriorism. Just as Lakatos tried to rid conventionalism of its (supposedly) untoward defensive streak by cross-breeding it with falsificationism, Machlup achieved a modicum of the same result by blending in a bit of apriorism — which is, in its own way, quite as prescriptive an approach as falsificationism.

Machlup did not, of course, hold that the elements of economic theory are somehow *true a priori*. But he did insist on a remnant of the methodological dualism ancillary to the apriorist position. Following in the tradition of writers like Max Weber, Ludwig von Mises, and (especially) his friend Alfred Schutz, Machlup insisted that the social sciences have certain requirements imposed on them by the very nature of their enterprise — requirements not imposed on the natural sciences. "This, indeed, is the essential difference between the natural and social sciences," he writes: "that in the latter the facts, the data of 'observation,' are themselves results of interpretations of human actions by human actors. And this imposes on the social sciences a requirement which does not exist in the natural sciences: that all types of action that are used in the abstract models constructed for purposes of analysis be 'understandable' to most of us in the sense that we could conceive of sensible men acting (sometimes at least) in the way postulated by the ideal type in question." (1955, p. 16[1978, p. 152].) Here is the crux of Machlup's methodology. Far from converting theories to mere "tools for prediction" (Latsis, 1976b, p. 14), Machlup is concerned centrally with *explanation*.[13] He rejects as unsophisticated the notion that theories need be "true" or "realistic"; but, instead of falling back to instrumentalism, he takes the diametrically opposite tack and proposes what is effectively a more sophisticated alternative to truth and realism: understandability.

Thus, in a sense, Machlup has followed his own advice about attacking one theory with another theory: rather than merely making the negative case that his opponents' quest for greater realism is misguided, he makes the affirmative case that they ought to be looking for understandability rather than realism.

Machlup and Schutz

In saying that "the fundamental assumptions of economic theory" are subject to "a requirement of understandability in this sense in which man can understand the actions of fellowmen" (1955, p. 17[1978, p. 153]), Machlup is adhering to a position that might best be described as *methodological subjectivism*. At first blush, such a position would seem to commit the economist to detailed empirico-psychological studies of the motives and decision-procedures of actual businessmen and households[14] — in short, it would seem to ally Machlup with precisely the position against which he had always argued. The paradox, as we shall see, is only apparent.

There are two themes that run through Machlup's writings on the explanatory function in economics. The first theme comes under the heading of what we might call *understandability*. The second theme is that of *generalizability*. Machlup's concern with these twin themes is perhaps best understood in terms of the method of *ideal types* that Machlup consistently championed, a method put forth by his friend and fellow Viennese Alfred Schutz.

Schutz's approach is an attempt to resolve the following sort of dilemma. With Max Weber, he insisted, on the one hand, that the social sciences be able to make theoretical use of the knowledge of human behavior one gains simply by putting oneself in the place of the theoretical agent. This is what Weber called *Verstehen* (understanding) and what we referred to more generally as methodological subjectivism. But Schutz was also concerned, on the other hand, with a variant of the problem of free will. Since each individual is unique (and has free will), how can subjective knowledge be consistent with statements of general scientific validity? His solution to the dilemma is embodied in the process of typification, by which the analyst abstracts in a formal way from the peculiarities and idiosyncrasies of the "real world" individual to construct an ideal type, a kind of mechanical puppet that captures features of human behavior gained through *Verstehen* but that is also deterministic and manipulable. This compromise position allows one to skirt the Scylla of strict positivism, which seeks generality by eliminating the subjective entirely, and the Charibdis of an anti-theoretical method, which would sacrifice generality to the idiosyncrasy of particular historical cases.

Machlup's requirement of understandability is what Schutz called the requirement of *adequacy*. The ideal type one constructs must be adequate to the situation in which it is placed.[15] This notion of adequacy has several facets. Most importantly, it requires that the motives and thoughts — what Schutz called the *subjective meanings* — that we attribute to the ideal type by perfectly sensible and plausible sources of its action. "This postulate," writes Schutz, "is of extreme importance for the methodology of social science. What makes it possible for a social science to refer at all to events in the life world is the fact that the interpretation of any human act by the social scientist might be the same as that by the actor or by his partner." (Schutz 1943, p. 147)

But adequacy means more than subjective interpretation. It also involves the core of the Schutzian compromise between free will and determinism; in the context of Machlup's work, we might say that adequacy is closely bound up with the idea of single-exit modeling. The ideal type is adequate to its situation if its actions are, in Machlup's words, "acceptable as efficient causes of the effects that are to be explained." (Machlup, 1978, p. 246.) In terms of the mechanical image introduced above, the ideal type must be a cog in the analytical apparatus; its actions must cause the effects to be explained just as the motion of one gear causes the motion of another. This implies that the motives — the behavioral assumptions — of the ideal type must be fixed and invariant. For the ideal type to play its causal role, it cannot do anything unexpected; all but one "exit" must be closed off. "The puppet called 'personal ideal type,'" says Schutz, "is...never a subject of or a center of spontaneous activity. ...His destiny is regulated and determined beforehand by his creator, the social scientist, and in such a perfect pre-established harmony as Leibniz imagined the world created by God." (Schutz, 1943, pp. 144-5.)

Machlup's conception of understandability, then, is largely borrowed from Schutz. It includes the twin requirements of subjective interpretation and single-exit causality. Machlup's contribution—his real innovation—lay in his development of second theme: generalizability.

Machlup explicates the issue of generalizability in a clear and straightforward way in his 1936 article "Why Bother with Methodology?" His thesis is that methodological reasoning can be both simple and useful to practitioners. And he illustrates it by considering three economic propositions.

Statement (1): "If, because of an abundant crop, the output of wheat is much increased, the price of wheat will fall."

Statement (2): "If, because of increased wage-rates and decreased interest rates, capital becomes relatively cheaper than labor, new labor-saving devices will be invented."

Statement (3): "If, because of heavy withdrawals of foreign deposits, the banks are in danger of insolvency, the Central Bank Authorities will extend the necessary credit." (1978, p. 64.)

These three statments rest on different levels of generality. The motivation of the actors — the behavioral assumption — is arguably the same in all three cases (1978, p. 65); but, to Machlup, "the causal relations such as stated in (2) and (3) are derived from types of human conduct of a lesser generality or anonymity. To make a statement about the actions of bank authorities (such as (3)) calls for reasoning in a stratum of behavior-conceptions of much less anonymous types of actors. We have to know or imagine the acting persons much more intimately." (1978, p. 68.)

This warrants a closer examination. It is certainly clear that Statement (1) is more general than Statement (3), in the sense that it asserts a causal relationship in whose validity we are likely to have confidence under a wide range of circumstances. The Bank's decision to inflate or not will depend crucially on the particular directors involved, as well as on the details of their influences, opinions, health, blood-sugar levels, or even biorhythms.[16] By contrast, the fall in wheat prices asserted in Statement (1) cannot so eaxsily be affected by the actions of any particular specific individuals. Generalizability is thus closely related to the question of applicability. Unless we are clear about how anonymous are the ideal types in our model, we cannot be clear about how generally applicable the model is.

But what *is* it that makes the behavior in Statement (1) anonymous? The answer, obviously, is that the *circumstances* assumed in Statement (1) allow us to use an ideal type that is anonymous. Although Machlup never spells it out explicitly, the business of typification is closely linked to the economic situation — or even, if you will, to the institutional structure — one assumes. To put matters another way, it is what we might call the *system constraint* that, by specifying the appropriate level of anonymity for the ideal type, determines the level of generality of theoretical statements.[17] Generality is thus *not* to be gotten merely by choosing an anonymous ideal type.

Machlup is indeed a situational determinist in this sense. But it is important to recognize that situational determinism and single-exit modeling are not identical. One can have a model with only one exit either because the situation prescribes only one plausible exit for the agent or because the agent is psychologically programmed always to select one exit. Schutz's method of ideal types — of programmed puppets — was really single-exit modeling of the second type. In Latsis's terms, Schutz was really a *behavioralist.* (Latsis, 1972.) As an economist, Machlup was sensitive to the role of circumstances — to the role of mass behavior — in social theory. It was his innovation to recognize the connection between the system constraint and the generality of a theoretical statement. This is an important point, to which we will return below in the context of the marginalist controversy.

Summary: Machlup on Methodology

In summary, we might say that Machlup's methodological position was basically derived from Schutz. This is most visible in his methodological subjectivism and his insistence on causal, single-exit explanation. But Machlup modified the Schutzian stance in a number of ways. For one thing, Machlup recognized more clearly than Schutz that our choices are not among individual models but among theoretical systems — among hierarchical or quasi-hierarchical structures of inter-related models. Moreover, Machlup recognized that models are not unique: an infinite number of models can schematize the same causal relationships. Thus, we should be concerned not with the *truth* of theories — they are all necessarily true by construction — but with their *applicability.*

This issue of applicability is closely related to Machlup's development of the situational-determinism approach: our models are most likely to be applicable — to have the greatest general validity — when the single-exit character of the model is enforced by a system constraint rather than by an arbitrary behavioral assumption.

Finally, Machlup recognized that there is a large tacit component to theory and to theory-appraisal; and this led him to the notion of ah-haahhness as a criterion of theory-choice. Many of these innovations come under the heading of conventionalism, and all of them arguably represent an anticipation of recent trends in the philosophy of science.

Machlup versus Orthodoxy

The famous marginalist controversy took place principally in an exchange of articles in the 'forties between Machlup(1946, 1947) and Richard Lester (1946, 1947). Machlup updated and summarized his position in his presidential address to the American Economic Association in 1966. (Machlup 1967.) And a kind of second round of the controversy, along somewhat different lines, may be said to have taken place in an exchange between Machlup (1974) and Spiro Latsis (1972, 1976a).

One of the first lessons a student of Machlup learns is to be precise about the meaning of words. In this case, we need to be careful about what "marginalism" is supposed to mean. For example, one might take the term to mean formal Walrasian general-equilibrium theory in the manner of Arrow, Debreu, and Hahn (Debreu 1959; Arrow and Hahn 1971); one might take it to signify those ubiquitous models that represent the economic agent as maximizing a mathematical function (or functional) over a specified choice set, a task normally accomplished by finding the appropriate marginal conditions; one might take the term as embracing the Marshallian partial-equilibrium comparative statics of the intermediate textbook; or one might take marginalism to be any sort of analysis in which conclusions about economic variables like prices and quantities can be deduced from considering the behavior of a representative economic agent acting "on the margin." Clearly, one can defend one or another of these forms without defending the others; moreover, one can defend the weaker forms of marginalism in ways that leave the stronger versions in an undefended — or even indefensible —position.

Machlup is explicit that he is defending what he views as the entire marginalist tradition. "Since [marginalist theory] has been developed gradually over a period of more than a century," he writes, "it will not suffice to take any particular writer as one's text." (1946, p. 520.) Nonetheless, his discussion of marginalist theory clearly reveals that he has in mind only a particular subset of the neoclassical tradition. Consider the following passage.

> My charge that there is widespread confusion regarding the purposes of the "theory of the firm" as used in traditional price theroy refers to this: The model of the firm in that theory is not, as so many writers believe, designed to serve to explain and predict the behavior of real firms; instead, it is designed to explain and predict changes in observed prices (quoted, paid, received) as effects of particular changes in conditions (wage rates, interest rates, import duties, excise taxes, technology, etc.). In this causal connection the firm is only a theoretical link, a mental construct helping to explain how one gets from the cause to the effect. This is altogether different from explaining the behavior of a firm. (1967, p. 9[1978, p. 399].)

This passage is resonant with many of the methodological themes we discussed above. To Machlup, the firm in "traditional price theory" is not supposed to correspond to any actual firms or economic actors. Instead, it is the highly anonymous repository for an extremely general subjective meaning-context, *viz.*, a preference for profits. As we have seen, this anonymous subjective ideal type is not intended to answer the sort of questions that organization theorists ask; to answer such questions we would need more concrete ideal types with less general applicability.

More surprisingly, perhaps, the present-day orthodox theory of the firm arguably *also* used ideal types with a lesser degree of generality than those in Machlup's version of price theory. In order to see why this is so, we need to look closer at Machlup's conception of marginalism.

What questions does the "traditional" theory of the firm address? Machlup's answer is contained in the following oft-cited passage.

> Let us again pose four typical questions and see which of them we might expect to answer with the aid of price theory. (1) What will be the prices of cotton textiles? (2) What prices will the X Corporation charge? (3) How will the prices of cotton textiles be affected by an increase in wage rates? (4) How will the X Corporation change its prices when wage rates are increased? Conventional price theory is not equipped to answer any but the third question; it may perhaps also suggest a rebuttable answer to the fourth question. But questions 1 and 2 are out of reach. (Machlup, 1967, p. 8.)

This is by no means a non-controversial analysis. For example, Brian Loasby thinks Machlup's interpretation "is quite false." Formal theory can answer only the first two questions, he says, since the "solution of all equilibrium models rests on given data: there is provision for alternative solutions for different data; but no provision within the model for a response to any data which is not included in the original specification." (Loasby, 1976, p. 45.)

As you might guess, the difficulty turns on the meaning of the phrase "conventional price theory." *Machlup's* version of price theory may be designed for question 3 rather than questions 1 and 2. But Loasby, we would argue, is nearer the mark in identifying the *conventional* version of price theory with formal mathematical equilibrium models. Such models are indeed typically comparative-static. They compare the equilibrium states resulting from two or more sets of data; and, as formal theorists have repeatedly emphasized, differences in equilibrium positions should not be interpreted as movements over time.

"Comparative statics," writes one authoritative source, "is the comparison of the *equilibrium values* of the endogenous variables of an economic model corresponding to alternative values of the parameters selected for study. The parameter values investigated are always taken alternatives, *not* assequential changes." (Baumol, 1977, p. 320, emphasis added.) In Baumol's view, economists should always use the subjunctive mood when presenting the results of a comparative-static analysis: "x *would be* higher *were* y to be lower."

> It is easy but incorrect to say that the analysis shows that the imposition of an excise tax will lead price to *rise* from its previous level, but by less than the amount of the tax. That is an interpretation which is valid only if none of the other relationships happen to shift during the period to which such a statement applies. That is, it assumes implicitly that the production costs or demand patterns are not changed either by the tax rise itself or by other unrelated influences. But, in any event, comparative-static analysis makes no such intertemporal assertions. Instead, its alternatives always

represent substitute scenarios for an identical time interval: either a zero tax rate for the next year or a five per cent tax rate during the same period. (Baumol, 1977, p. 322.)

It is in this sense,then, that comparative statics does not attempt to answer Machlup's third question. As a formal technique, it cannot *directly* say anything about how the price of cotton textiles will be affected by a change in the wage rate.

To what extent can we take Baumol's interpretation as definitive of the orthodox understanding of formal comparative statics? At first glance, one might think that at least one voice of comparable authority — Paul Samuelson — would disagree. To Samuelson, change and change alone is the focus of attention. "The usefulness of our theory emerges from the fact that by our analysis we are often able to determine the nature of the *change* in our unknown variables resulting from a designated *change* in one or more parameters." (Samuelson, 1947, p. 7, emphasis added.) On closer examination, however, it becomes clear that Samuelson's understanding of *comparative statics* is ultimately very close to Baumol's.

Samuelson posed for himself the following sort of methodological problem. On the one hand, Samuelson the practicing economist wished to show that the core propositions of economics — its foundations, as he put it — can be fully reduced to the theorems of the calculus of extrema. That is, the core of economics is, as Baumol suggests, the logical process of deducing a state of affairs — an equilibrium constellation of variables — for a given set of parameters. On the other hand, however, Samuelson the methodologist adhered to a species of positivism, which committed him to believing that such an axiomatic enterprise world be wholly tautologous unless it resulted in theorems that have empirical content—they are, as he put it, "operationally meaningful."[18] Samuelson resolved this dilemma by associating the initial equilibrium with one state of the firm, the final equilibrium with a later state of the same firm, and the difference in parameter values with an empirically identifiable change in data. These three associations are assertions of a strictly empirical (*a posteriori*) nature. Samuelson's famous correspondence principle is a method of pre-screening these associations: we do not assert the correspondence between our comparative-static result and any empirically identified changes unless *both* the initial and final positions correspond to stable equilibria. The comparative-static results that survive this pre-screening are associated with empirically observable phenomena in the aforementioned manner; as a result of this association, they become "meaningful" theorems and, therefore, falsifiable.

It is important to notice that the corresponding dynamic process is not intended to describe any historical sequence of events. "We find ourselves confronted with this paradox," says Samuelson: "in order for the comparative static analysis to yield fruitful results, we must first develop a theory of dynamics.": (Samuelson, 1947, pp. 262-3.) But he immediately warns that the "point made here is not to be confused with the commonplace criticism of comparative statics that it does not do what it is not aimed to do, namely describe the transition paths between equilibria." (1947, pp. 263n.) Thus when Samuelson describes comparative statics as "the investigation of changes in a system from one position of equilibrium to another without regard to the transitional process involved in the adjustment" (Samuelson, 1947, p. 8), what he means — what he logically must mean - is simply the procedure Baumol describes. Indeed, Samuelson says as much himself in his exchange with Joan Robinson over reswitching. Some of the discussion there has to do with whether Samuelson's analysis turns on a confusion of alternative equilibrium positions with movements over time. Samuelson denies guilt, but affirms that "when a mathematician says, 'y rises as x falls,' he is implying nothing about temporal sequences or anything different from 'when x is low, y is high.'"(Samuelson, 1975, p. 45; see also p. 41.) Robinson puts it this way: "Professor Samuelson reminds us that a plane diagram can show relations between only two variables. He observes that a writer of prose may slip into saying: As real wages rise, the rate of profit falls, but a mathematician knows that a functional relationship is timeless and makes no reference to history or to the direction of change." (Robinson, 1975, p. 54.) Comparative statics examines alternative equilibrium positions deduced for given parameters using the mathematics of extrema. It investigates "changes" only to the extent that one asserts the existence of some unspecified real-world process to whose rest states the equilibria are supposed to correspond. Comparative statics—with or without the correspondence principle—abstains from investigating such processes.

Whatever differences there may—or may not—be between the Baumol and Samuelson versions of comparative statics, it remains true for both that the predictive and/or explanatory value of an equilibrium model depends upon the extent to which the equilibrium position of the model approximates an actual state of affairs. To Baumol, for example, the theory of the firm has predictive (or explanatory) power[19] if individual firms are likely to have *somehow* arrived at a constellation of values (prices, marginal costs, output levels, etc.) whose configuration roughly approximates those implied by the first-order conditions of the model.

> The concept of optimality is important to the economist [because] it helps him to understand the behavior of businessmen, consumers, and other members of the economy. It is at least possible that sheer business acumen and experience permit management and other economic units to arrive at decisions which come close to being optimal. Moreover, in business, competition may soon eliminate firms whose decision-making is consistently poor. To the extent that these assertions are valid, optimality analysis should serve as a relatively good predictor of economic behavior; that is, it should provide a reasonably good explanation of actual economic decisions and activities. In economic theory it is therefore customary to employ an optimality premise in discussing the behavior of firms, consumers, and other economic units. It is simply assumed that these units' decisions are approximately optimal, and the consequences of this assumption are presented as a rough description of economic behavior in the real world. (Baumol, 1977, p. 5.)

Equilibrium is not for Baumol merely a heuristic device; it is a simplified, or stylized, picture of what actually obtains "out there." Notice that this view of explanation explicitly disregards the question of *how* things came to be arranged in the equilibrium state; to put it another way, this view does not concern itself with the business of identifying causal mechanisms in any sense.

Baumol's suggestion that competition would eliminate the bunglers is highly reminiscent of Friedman's assertion that, "unless the behavior of businessmen in some way or other approximated behavior consistent with the maximization of returns, it seems unlikely that they would remain in business for long." (Friedman, 1953, p. 22[1977, p. 35].) The passage is worth quoting in full.

> Let the apparent immediate determinant of business behavior be anything at all—habitual reaction, rândom chance, or whatnot. Whenever this determinant happens to lead to behavior consistent with rational and informed maximization of returns, the business will prosper and acquire resources with which to expand; whenever it does not, the business will tend to lose resources and can be kept in existence only by the addition of resources from outside. The process of "natural selection" thus helps to validate the hypothesis—or, rather, given natural selection, acceptance of the hypothesis can be based largely on the judgment that it summarizes appropriately the conditions for survival (Friedman, loc. cit.)

Far from being essential, the "corresponding" dynamic process serves only to reinforce acceptance of a theory that summarizes certain static equilibrium conditions.[20]

In sum, we would argue that present-day orthodox marginalism consists of a combination of two techniques. The first—equilbrium analysis—attempts to deduce an equilibrium constellation of value for a set of economic variables (prices. etc.); it does this using the assumption (and the mathematics) of maximization under constraint. The second technique—comparative statics—consists in the comparison of the results of two (or more) equilibrium analyses performed with different initial conditions or auxiliary assumptions. Both methods require us to interpret the equilibrium position as an ongoing state of affairs that is possible to achieve and that is often closely approximated in reality. If this interpretation is correct, then "conventional" price theory *does* attempt to answer Machlup's first and second questions—and *only* those questions. Machlup's third question lies in the realm of dynamics; it remains outside the province of comparative statics.

Now, economists *do* interpret comparative-static results as movements over time, of course—but not on the basis of the formal theory. Instead, economists typically graft onto the formal theory an informal story about how economic agents adapt to exogenous change.[21] The key to understanding Machlup's argument is to realize that what he is defending *is not* formal price theory (and thus not "conventional" price theory) but rather the informal or appreciative story about adaptation that economists tell in their introductory classes. Machlup's (unconventional) version of marginalism *is* a process story, an attempt to answer question 3. Although nonmarginal factors may shape the *absolute* level of prices and the qualitative features of business behavior, he argues in effect, marginalism *is nonetheless appropriate* for analyzing *changes* in prices or similar variables arising from exogenous changes in conditions. In particular, marginalism considers the response to exogenous change of a representative ideal type who (a) prefers more profit to less, (b) knows of the exogenous change, and (c) knows how to adapt more-or-less profitably to that change. This is very much a marginalist theory, but it is a theory arguably closer in spirit to heterodox process theories than to what we have argued is the neoclassical mainstream.

As we suggested earlier, Machlup requires of an explanation that it identifies a causal relationship and that it meets the criterion of subjective interpretation. Machlup's version of marginalism meets both requirements. The best way to describe it is in terms of a four-step adjustment model.

1.**Initial position**. The analyst first specifies the set of (interrelated)variables of interest to him and identifies a constellation of values for those variables that is self-consistent and thus embodies no "inherent tendency to change." This situation is the *initial equilibrium*.

2.**Disturbing change**. The analyst introduces a change in one of the exogenous variables specified in step 1. It is this cause whose effect the model is intended to isolate.

3.**Adjusting changes.** The analyst now specifies the process by which the endogenous variables specified in step 1 adjust, that is, the process by which those variables change in value in reaction to the change introduced in step 2.

4.**Final position**. This is the *final equilibrium*, a position in which the values of the variables are once again mutually consistent. It is "a situation in which, barring another disturbance from the outside, everything could go on as it is. In other words we must proceed until we reach a 'new equilibrium,' a position regarded as final because no further changes appear to be required under the circumstances." (Machlup, 1963, p. 48.)

It is important to recognize that Machlup — in contrast to Baumol, Samuelson, or Friedman — does not see the initial or final equilibrium as an approximation to an actual state of affairs. In the adjustment model, it is the equilibrium position — not the theory itself — that is merely instrumental. Equilibrium is nothing other than a "methodological device" that "serves as part of a mental experiment designed to analyze causal connections between 'events' of 'changes of variables.'" (1963, pp. 45 and 46.)

> In a nutshell, we have here a mental experiment in which the first and last steps, the assumption of initial and final equilibria, are methodological devices to ensure that Step 2 is the sole cause and Step 3 contains the complete sequence of effects. The function of the initial equilibrium is to assure that "nothing but 2" causes the changes under Step 3; the function of the final equilibrium is to assure us that "nothing but 3" is to be expected as an effect of the change under step 2 (although the "completeness" of the list of effects will always be merely relative to the set of variables included in the equilibrium). (1963, p. 49)

In fact, the association of the equilibrium position in a model with some concrete state of affairs is, to Machlup, merely an instance of the fallacy of misplaced concreteness. (Machlup 1963, p. 57).

As Moss (1984) points out, the dominant approach to the theory of the firm since Marshall has been characterized by a desire to make precisely such an association. Marshall's notion of the representative firm had been one in which the firm or theory reflected the "average" characteristics of the population of firms in an industry—not the characteristics of any particular actual firm. By contrast, the concepts of firm and industry taken up by Pigou, Sraffa, Robinson, and legions of later theorists worked in the opposite direction: one builds up the industry from identical *equilibrium firms* rather than compressing the industry to form the *representative firm*. It was thus an easy step to associate the atomic firm of theory with the firm of real life—something Marshall would have warned against. In this sense, then, we ought to think of Machlup as a true Marshallian rather than as a defender of contemporary (let alone present-day) orthodoxy.

Machlup and Heterodoxy

In sum, the marginalism Machlup defended is indeed marginalism. But it is a form of marginalism that qualifies as "traditional" or "mainstream" only in the limited sense that it reflects the informal or appreciative Marshallian marginalism economists teach in their introductory classes. What Machlup *did not* defend is the formal neoclassical research program practiced by most of his (and of our) contemporaries.

On the one hand, it is odd in a certain sense that Machlup should be seen (even implicitly) as a defender of this more formal orthodoxy. For Machlup's defense of marginalism against the attack of Richard Lester in 1946 consists precisely in arguing that, in effect, Lester takes the formal model too seriously. What Lester calls into question is the "marginal principle," which he translates as "maximizing profits by equating marginal revenue and marginal cost." (Lester, 1946, p. 63.) And equilibrium models surely do portray firms as having effected just such a precise alignment of variables. To Machlup, by contrast, the marginal principle is more basic and less restrictive: it is merely the "*economic principle*—striving to achieve with given means a maximum of ends." (Machlup, 1946, p. 519, emphasis original.) It therefore does not commit him to taking literally the requirements of the formal models. The adaptive firm in the Machlupian adjustment model is an economizer in this sense to exactly the same extent as the heroically maximizing firm of the equilibrium model. The latter — or at least the firm described by the curves and numerical examples of the textbook — is merely a heuristic device to explain the logic of economizing to the student.

On the other hand, the general perception of Machlup as defender of neoclassical orthodoxy *tout court* is less surprising when we remember that it was indeed *all* forms of marginalism that Lester attacked. This instantly made all marginalists allies, and made it easy to gloss over differences of interpretation within the marginalist camp. Moreover — and perhaps more to the point — Machlup himself clearly felt that his own interpretation was in fact the mainstream view. To a body of economic practitioners not known for their extreme interest in methodological issues, the distinction we have made between Machlup's marginalism and that of formal neoclassical analysis may thus easily have gotten lost. My enemy's enemy is my friend. Why look further?

Lester's attack on marginalism was based on the results of questionnaires he had administered to a sample of businesses. The questionnaire asked the businessmen about their decision-making processes, frequently requiring them to rank the importance to their decisions of various factors. Lester construed his results to suggest that businessmen do not in fact employ marginal reasoning and that, furthermore, they do not respond to changes in economic conditions in the way predicted by marginalist theory. For example, Lester's respondents did not rate very highly layoffs or reductions of output—the two actions predicted by marginalism—as likely responses to an increase in the wage rate. Machlup's answer to this challenge is in many ways a *tour de force* of scholarly argumentation. He charges Lester with misunderstanding marginalism, and proceeds to lecture him, in schoolmasterly tones, about the correct—Machlupian—interpretation of that theory. He then proceeds to ridicule the use of questionnaires in economic research, and to pick apart the particular questions Lester used. And he concludes by showing that, if Lester's results demonstrate anything at all, it is that marginalism *was* in fact at work in the businessmen's decision-making.

It should be clear why Machlup would find little to like in the sort of attack Lester mounted. It is effectively an exercise in "ultra-empiricism"; moreover, his criticism of the marginalist program is backed up with no alternate theory or theoretical structure. Secondly, and perhaps more significantly for present purposes, we should notice the extent to which Lester's questionnaire method fails against Machlup's anonymity criterion. Lester's questionnaires sought to discover, with some specificity, the mind-contents of concrete business decisionmakers when only the presence or absence of the profit motive was relevant. To Machlup, the theory of the firm is not intended to paint a psychologically accurate portrait of the businessman's deliberations. "A mental process in everyday life," says Machlup, "may often be most conveniently described for scientific purposes in a language quite foreign to the process itself." (Machlup, 1946, p. 537.) Any analysis or model based on information about the behavior of particular firms or individuals may well be misleading; theory must strive for statements of general applicability. "To deal with conduct of types of higher intimacy and, therefore, lesser anonymity means to deal with phenomena in a stratum of lesser generality." (Machlup, 1936[1978, p 68].)

One irony, then, is that Machlup is more of an institutionalist than is Lester.[22] For Machlup, it is ultimately the system constraint—not the psychology of the businessman—that drives the theory. This instantly raises a troublesome issue: the possibility of a thoroughly non-marginalist defense of the marginalist theory of the firm (in Machlup's sense) —a defense based solely on the system constraint and dispensing entirely with the profit motive.

Consider a (fairly) large number of firms—like those in Lester's sample — confronted with a sudden exogenous increase in the wage rate, a rise caused, let us say, by minimum-wage legislation. What will happen to the employment level? Machlup would argue in the following way. The representative firm will become aware of this wage increase and will recognize that economizing on labor will lead to greater profit (or, more likely, smaller reduction in profit) than not economizing. The representative firm will thus reduce its labor-force; and, summing such reductions over all the firms, we conclude that total employment will decrease. But consider an alternate way of arguing. Suppose that no firm reduces its labor force. This means that each firm's wage bill must increase in the face of the legislation (assuming compliance). Since there are many firms, this increase cannot be absorbed by a reduction of "monopoly" profits. Thus the firms must raise prices. This must reduce the quantity of output demanded; and that in turn must mean that some firms go out of business — leaving their workers without jobs. In the end, reasoning from the system constraint alone suggests that total employment must decline.[23]

This scenario ought to sound familiar. It is a variant of the natural-selection or survival-of-the-fittest arguments discussed above. But, in this case, the focus of the argument has shifted from an equilibrium to an adaptive context. Rather than employing a natural-selection arguments to connect the equilibrium position of a neoclassical optimization model with an actual state of affairs, our wage-employment scenario uses evolutionary reasoning to tell a dynamic story about how a population of firms adapts to a change in the economic environment. As in Machlup's version of marginalism, our story focuses on the process of change rather than on the initial or final equilibria. Moreover, its conclusions are the general ones in Machlup's sense, since the scenario depicts "*the effects* of the hypothetical reactions of numerous anonymous 'reactors' (symbolic firms)." (Machlup 1967 [1978, p. 398], emphasis added.)

This sort of process argument from the system constraint seems to generate the conclusions of the marginalist theory of the firm without employing the muchberated assumption of profit maximization. A closer look into the nature of this constraint, however, reveals that the profit motive is still at work, so to speak, behind the scenes. The argument was that, in the absence of both lay-offs and monopoly profits, the increased wage-bill forces up prices. But can't we imagine the contrary: that prices don't rise? If the managers of the firms do not raise the prices of their products, the owners of the firms will receive smaller (indeed, infra-normal) dividends.[24] This implies a transfer of income from capitalists to workers. Such a situation will persist *until* capitalists alter their estimates of equity values and readjust their portfolios. When this happens, the equity value of some firms will be judged to be so low that it will be more *profitable* to shut down and sell off its vendible assets than to continue operation.

Our discussion of the role of the system constraint in determining the appropriate levels of anonymity and generality raised the possibility of a non-marginalist theory that answered precisely the Machlupian questions but that didn't assume profit maximaization. Our illustrative model suggests that one cannot dispense with the profit motive entirely, just as Machlup's defense of marginalism suggests that one cannot wholly disregard the nature of the system constraint. In the largest sense, arguing from the constraint and arguingfrom maximization are not really separable. A constraint is constraining only from the point of view of some intention; and, in a world of scarcity, intention is always constrained.[25]

The significance of all this becomes apparent when we consider Machlup's marginalism not in the context of the rather unfocused criticisms of the 1940s but in light of developments that have occurred in the ensuing forty years. Modern heterodoxy is, of course, a rather varied and diverse collection of ideas, even if one can make some case for recognizable common themes within a large and identifiable subset of these ideas.[26] But perhaps the most relevant work for our purposes is the (self-proclaimed) heterodoxy of Richard Nelson and Sidney Winter (1982), who attempt to construct a theory of the firm that is built somewhat along the lines of our simple evolutionary scenario.

How does this modern heterodoxy look under Machlupian spectacles? Unlike Lester's antimarginalism, the Nelson-and-Winter version is not merely an attack on orthodoxy; it is an attempt to develop a detailed and coherent alternative. Moreover, that alternative is a theory of price response, not a theory of how (individual) firms behave. More significantly, the Nelson-and-Winter approach is a theoretical system that arguably accords better with Machlup's conception of marginalism than does orthodox neoclassicism. It is an attempt to address Machlup's third question, an attempt at a theory of response to economic change.

What about the question of generalizability? At first glance, it might seem that this new heterodoxy fails on that score—that it produces theories with a lower level of generality than does Machlup's marginalism. For example, the evolutionary model has at its core a stochastic search process representing innovation; propositions based on this process would have the same status as Machlup's second proposition.[27] And, indeed, "perverse" results are possible in principle from the Nelson-and-Winter model. Not only may the search prove futile, but the search process itself may lead to unexpected results even when successful. For instance, a rise in the price of one input may trigger a search that leads to a profitable way of economizing on a *different* input, leading to higher profits but a perverse change in factor-intensity ratio. (Nelson and Winter, 1982, p. 174.) In marginalist language, unconventional results are possible when a change in exogenous conditions causes the firm to expand its choice set rather than choose from within a known set. But—once again—it is not the behavioral details of the model that give the model its generality. Rather, it is the nature of the *system constraint* that dictates the level of generality. In situations of "competitive mass behavior" (Machlup, 1978, p.4), both models are arguably on the same level of generality.[28]

Now, it remains true that, in situations of lesser anonymity, the model will have lesser generality—it will have a lower probability of being applicable. For example, in modelling the Schumpeterian competition of an innovative oligopoly, which is a major focus of the Nelson and Winter book, the models will have a status closer to that of Machlup's Proposition (2). But orthodox models—or even Machlupian models—applied to situations of lesser generality will also have an attenuated status. Indeed, one might reasonably argue that models of the Nelson-and-Winter type are better adapted to situations of lesser anonymity than are marginalist models.

Contrary to what Latsis would have us believe, neoclassical optimization models - that is, models built according to the neoclassical positive heuristic he describes (1976b, p. 22)—do not qualify as situational-determinism models. The conventional positive heuristic is a prescription for generating models with a specific behavioral assumption. Only when such models are applied to *situations* that have only one plausible exit do they become models of situational determinism. (But this is also true of other types of behavioralist models, including evolutionary models.) When neoclassical optimization models are applied to multiple-exit situations, they lose any situationally deterministic character they may have had; they become frankly behavioralist models—but behavioralist models with a behavioral assumption arguably vulnerable to criticism for implausibility and lack of verisimilitude.[29] Even Machlup's marginalism becomes behavioralism when he applies it to multiple-exit situations, as in his own analyses of oligopoly and monopoly. In general, however, Machlup's was careful in his defense of marginalism to limit himself to the large-numbers situation implicit in "traditional price theory." In this sense, the Machlupian positive heuristic does comprise situational determinism in a way that the modern neoclassical program does not.

A complete appraisal of the Nelson and Winter program in Machlupian terms would require evaluating their models against the criteria of understandability and of ah-haahhness. An outline of such an appraisal might look like this. Does the rule-following firm qualify as an ideal type?[30] On the one hand, there is an argument that the economic principle in Machlup's sense is somehow more fundamental or basic than other candidates for the ideal type. On the other hand, if we could imagine reasonable people behaving in the manner of the "skillful businessman" Nelson and Winter describe (in chapters 3-5) and use to inform their models, then why shouldn't this characterization counts as an ideal type? As they remark (1982, p. 91), it is extremely significant that both Friedman and Machlup concede — indeed stress — that economic action is not actually a matter of continual conscious optimization but is largely a process of following tacit rules and exercising inarticulate skills. Friedman's well-known analogy is that of an expert billiards-player (Friedman 1953); Machlup's equally well-known analogy is that of an automobile driver (Machlup 1946, 1967). Both authors recognize that such skillful and rule-following behavior is in some sense a better characterization of what agents "really" do than is conscious optimization. Is this not testimony for the understandability of the skillful businessman as ideal type?

The question of ah-haahhness is even more complex. On could argue that evolutionary models score lower than purely marginalist ones on this criterion, since marginalist models derive all their results from a single simple behavioral principle, whereas the evolutionary model requires the analysis of several (possibly mutually counteracting) effects. On the other hand, the fact that so many writers have felt it necessary to invoke natural-selection arguments in support of neoclassical theory may count as testimony that an evolutionary model is not without an ah-haahh value.

However one chooses to decide the case, these issues—and not the ones normally debated are the proper focus of a marginalist controversy.

Summary

We have argued that Machlup has been frequently misunderstood — on two levels.

On the methodological level, he has often been misread as a positivist seeking to eliminate all subjective knowledge from economics; as an instrumentalist concerned only with prediction; or as a defensive methodologist concerned only with butressing the *status quo* and unwilling to specify affirmative tests a theory or program must meet. In fact, Machlup is principally a conventionalist, but one who retains a strong affirmative component built around the requirements for generalizability, subjective understanding, and ah-haahhness.

On the level of method, Machlup has been widely misperceived as defender of the formal neoclassical research program as it is conventionally practiced. We have argued that—perhaps surprisingly—his defense is applicable only to the informal or appreciative marginalism of the elementary textbook. In defending this brand of marginalism, he actually sets standards that certain strains of present day antimarginalism find easier to meet than does formal neoclassical theory.

Notes

1. For a review, see Blaug (1980, pp. 103-14).
2. Of course, this admission by Friedman raises a marvelously self-reflexive issue not noticed by Boland. If Friedman believes that the testimony of an economic agent about his beliefs and motives is irrelevant to economic theory, why, on the meta-theoretic level, should Friedman's testimony about what he believes be relevant to the methodological discussion?
3. For example, Nelson and Winter (1982, pp. 92 and 94) are in the habit of talking about the "Friedman-Machlup" arguments. And Loasby (1976, pp. 16 and 156) talks about "the resolute rejection by Friedman and Machlup of any evidence that can be deemed to have passed through any mind—other than the antiseptic mind of the analyst...." This is quite unfair to Machlup, who is a leading methodological subjectivist (see, for example, Machlup, 1978, ch. 12); but it is perhaps understandable why Loasby should misread him in that way, because, as we will suggest below, the approach Machlup defends is, in one specific sense, designed precisely to restrict the psychological assumptions—and thus the kinds of knowledge—that go into the ideal type within a model. More recently, Moss (1984, p. 311), in an otherwise illuminating article, accuses Machlup of promulgating "a less extreme form of positivism" than Friedman. As we will show, Machlup's position was no kind of positivism. Part of Moss's confusion seems to arise from an apparent belief that upholding the positive/normative (or is/ought) distinction is identical with upholding the entire program of logical positivism. The is/ought distinction was surely a tenet of positivism, but it was also a tenet of many pre-, non-, and even anti-positivist positions, including those of J.N. Keynes, Ludwig von Mises, and Machlup. If Loasby lumps Machlup and Friedman together by turning Machlup into Friedman, Latsis (1976b, p. 10) does the same by turning Friedman into Machlup. Both, he says, are engaged in "a conventionalist defence of a research programme against falsificationism."
4. This is probably fair to Machlup—and we will have more to say about the conventionalist aspects of his methodology in the next section—but it misses the point about Friedman, at least if Boland (1982, ch. 9) is right.
5. This approach was eventually formalized as the hypothetico-deductive method. (Hempel and Oppenheim, 1948.)
6. Boland (1982, part III) appears to have a broader and slightly different definition of conventionalism.
7. Indeed, Latsis describes conventionalism in such a way that it subsumes instrumentalism. (The fuller version of the quotation from Pareto cited above is particularly ambiguous in that regard.) This is why he can lump Friedman and Machlup together as conventionalists. But compare Boland (1982, chapter 9).
8. Machlup goes on to cite James B. Conant to the effect that "a theory is only overthrown by a better theory, never merely by contradictory facts." As Caldwell (1982, p. 172n) points out, the Kuhnian sound of this passage should not be entirely surprising when one considers that Conant was a major influence on Kuhn.
9. Contrast Friedman's assertion that "the only relevant test of the *validity* of a hypothesis is a comparison of its predictions with experience. The hypothesis...is accepted if its predictions are not contradicted; great confidence is attached to it if it has survived many opportunities for contradiction." (Friedman, 1953 [1977, p. 27], emphasis original.)
10. To our knowledge, Machlup's published writings contain no statement quite this categorical about his ultimate criterion for theory choice. But we believe that this assertion is at least consistent with his writings. Moreover, during his last graduate course in methodology (which the present authors attended in the spring of 1982 at New York University), Machlup — when pressed by students for his ultimate criterion — did in fact invoke "ah-haahhness" in more-or-less categorical fashion.
11. Of course, observation may help determine whether a particular theory is *applicable*. But that isn't the same as determining whether the theory is "true" or "better." This is a point to which we'll return.
12. In this sense, Machlup's system comes close to Donald McCloskey's (1985) portrayal of economics as rhetoric. The similarity is further underscored by — and helps to explain — Machlup's fascination with economic semantics (Langlois 1985). Of course, as we will see, Machlup's system contains a much stronger prescriptive component than does McCloskey's.
13. For example, if one could show that the neoclassical theory of the firm is inapplicable to almost all the problems to which it has traditionally been applied, that would surely be tantamount to rejecting that theory according to most common-sense meanings of the term. On the other hand, to show that that theory is inapplicable to one or two special cases (because all the right *ceteris paribus* conditions don't hold) would scarcely count as refutation.
14. This is also, of course, the single—but decisive—point of disagreement with Friedman that Machlup cites. (1955, p. 17 [1978, p. 153].)
15. We will, for the most part, ignore marginalism as applied to consumer choice and concentrate—as did the participants in the controversy—on marginalism applied to the firm. But see Langlois (1986b).
16. For example, the ideal type of the profit-maximizing firm must be adequate to the situation that the model portrays the firm as facing. We should point out that this is a somewhat oversimplified rendition of Schutz's formulation. Schutz actually talked in terms of the adequacy of *personal* types in relation to *course-of-action* types. We will use the general term "ideal type" to mean Schutz's personal type, and we will talk of the agent's situation or circumstances as an imprecise shorthand for Schutz's notion of a course-of-action type. Those interested in the complexities of his system should see Schutz (1943, 1967) and Schutz and Luckmann (1973).
17. One thinks of Milton Friedman's opinion that the Great Depression might have been averted or mitigated in severity had Benjamin Strong, Chairman of the New York Federal Reserve Bank, not died just when he did. (Friedman and Schwartz 1963, p. 122.)
18. For a more detailed analysis of the role of the system constraint in economic explanation, see Langlois (1986b).
19. For applications of Machlup's idea of generalizability, see Langlois (1988) and Koppl (1987).
20. For Machlup's views on Samuelson's "operationalism," see Machlup (1964) [1978, chapter 20].

21. In the following quotation at least, Baumol (like Friedman) appears to take explanation as synonymous with prediction.
22. This is in many ways an odd sort of argument for Friedman to make. If his position is in fact an instrumentalist one, he really doesn't need a fallback argument to support the maximization theory. Moreover, to draw support from the natural selection theory in this way is implicitly to admit that that theory is somehow more plausible, more satisfying, or deeper than its maximization "summary"; it is to admit, in effect, that the natural selection theory is somehow more nearly "true" or more "explanatory." And that sort of admission undermines any case that truth is irrelevant or that explanation is merely prediction. It is as if Friedman were making a lawyer's argument, not a philosopher's: "Your honor, I will prove conclusively that my client couldn't possibly have been at the scene of the crime. Moreover, I will show that, even if he *was* there, he couldn't have committed the murder."
23. This distinction between formal and appreciative theory follows Nelson and Winter (1982, p. 46). While there certainly do exist formal dynamic models, these are for the most part of the difference (or differential) equation variety. The simple cobweb system is the best-known example. As Arrow (1959) pointed out, such models are inconsistent with the methodological individualism that (Arrow claims) is universally acknowledged as the basis of all economics; in any case, they certainly violate Machlup's criterion of subjective interpretation. Moreover, Boland (1982, p. 136) argue that such models do not even meet Samuelson's criterion for a corresponding dynamic system. The dynamic models are themselves wholly axiomatic exercises, and they cannot therefore have any greater operational significance by Samuelson's standards than can the comparative-static equilibria to which they are supposed to correspond. We should also note that the recent interest the literature has shown in models using the calculus of variations cannot be interpreted as a trend toward dynamic analysis. The calculus of variations is merely a generalization of the mathematics of extrema in which the extrema sought are not points but functions or functionals. These latter may have a variable called time as an argument, but the effect of the maximization is to generate an equilibrium solution for all values of the argument. This is far from providing a transitional process from one equilibrium to another.
24. We mean the term in the sense of Agassi (1975); compare also Latsis (1976b, p.16).
25. George Stigler makes exactly this observation in his own response to Lester. He argues that Lester was not careful about the populations of firms he was dealing with. By Lester's own count, 16 plants had gone out of business in one industry over the time-period studied. Says Stigler, "by parallel logic it can be shown by current inquiry of health of veterans in 1940 and 1946 that no soldier was fatally wounded [in World War II]." (Stigler, 1947, p. 157.) Stigler, does not, however, develop the implications of this way of reasoning. For ease of exposition, we assume all firms are publicly held corporations.
26. Many of these issues were raised many years ago in an exchange between Gary Becker (1962, 1963) and Israel Kirzner (1962, 1963). See also Winter (1964), especially p. 240n. For a fuller analysis of these issues, see Langlois (1986b).
27. For an attempt to make such a case, see Langlois (1986a). If, because of increased wage-rates and decreased interest rates, capital becomes relatively cheaper than labor, new labor-saving devices will be invented (Machlup, 1936[1978, p. 64].)
28. The relevant evolutionary model displays three effects. When the number of firms is large, the conventional effects will dominate because (a) the Nelson and Winter model *subsumes* Machlup's model as the along-the-rule effect and (b) the other two effects are impelled in the same direction by large numbers. Again, a detailed discussion of the Nelson-Winter model is beyond our scope.
29. It is certainly true that the behavioral assumption of the neoclassical positive heuristic is badly adapted to multiple-exit situations like oligopoly. But that doesn't mean that neoclassical models aren't behavioralist models — it simply means that they aren't appropriate behavioral models in such situations.
30. Compare Latsis (1976b, pp. 12-3): "Yet the adoption of the ideal type or zero-method in the explanation of social phenomena surely *does not* imply the adoption of the neoclassical conception of an empty, transparent economic man. This latter is surely *one* ideal-type model. There may be fruitful alternatives. For instance, a 'satisficer' may serve as an ideal type, as can a 'bankruptcy avoider.' ...[T]he method of ideal types does not stand or fall with neoclassical maximization. it is neutral towards the particular behavioral traits with which we choose to endow the typical economic agent."

References

Agassi, Joseph. 1975. "Institutional Individualism," *British Journal of Sociology* **26**: 144-55.

Arrow, Kenneth J. 1959. "Towards a Theory of Price Adjustment," in M. Abramovitz, ed., *The Allocation of Economic Resources*. Standford: Stanford University Press.

Arrow, Kenneth J. and Frank Hahn. 1971. *General Competitive Analysis*. San Francisco: Holden-Day.

Baumol, William J. 1977. *Economic Theory and Operations Analysis*. Englewood Cliffs: Prentice-Hall, fourth edition.

Becker, Gary S. 1962. "Irrational Behavior and Economic Theory," *Journal of Political Economy* **70**: 1-13.

Becker, Gary S. 1963. "A Reply to I. Kirzner," *Journal of Political Economy* **71**: 82-3.

Blaug, Mark. 1980. *The Methodology of Economics*. Cambridge: Cambridge University Press.

Boland, Lawrence A. 1982. *The Foundations of Economic Method*. London: Allen and Unwin.

Caldwell, Bruce J. 1982. *Beyond Postivism: Economic Methodology in the Twentieth Century*. London: Allen and Unwin.

Coddington, Alan. 1972. "Positive Economics," *Canadian Journal of Economics* **5**(1): 1-15.

Debreu, Gerard. 1959. *Theory of Value*. New York: John Wiley.

Friedman, Milton. 1953. "The Methodology of Positive Economics," in *Essays on Positive Economics*. Chicago: University of Chicago Press. Reprinted in William Breittand Harold Hochman, eds., *Readings in Microeconomics*, Hinsdale, Ill: Dryden press, second edition, 1977.

Friedman, Milton, and Anna Jacobson Schwartz. 1963. *The Great Contraction*, 1929-1933. Princeton: Princeton University Press.

Hempel, C.G. and P. Oppenheim. 1948. "Studies in the Logic of Explanation," *Philosophy of Science*.

Hutchison, Terence. 1938. *The Significance and Basic Postulates of Economic Theory*. New York: Augustus M. Kelley.

Kirzner, Israel M. 1962. "Rational Action and Economic Theory," *Journal of Political Economy*, **70**: 380-5.

Kirzner, Israel M. 1963. "Rejoinder," *Journal of Political Economy*, **71**: 84-5.

Koppl, Roger. 1987. "When Will Speculation in Foreign Exchange Be Destabilizing? An Application of Machlup's `Why

Bother with Methodology?'" Paper presented at the History of Economics Society Annual Meeting, Boston, and published in photocopied proceedings.

Kuhn, Thomas S. 1970 *The Structure of Scientific Revolutions*. Chicago: The University of Chicago press, second edition.

Lakatos, Imre. 1970. "Falsification and the Methodology of Scientific Research Programmes," in Lakatos and Musgrave, eds., *Criticism and the Growth of Knowledge*. Cambridge: Cambridge University Press.

Langlois, Richard N. 1985. "From the Knowledge of Economics to the Economics of Knowledge: Fritz Machlup on Methodology and on the 'Knowledge Society.'" *Research in the History of Economic Thought and Methodology* **3**: 225-235.

Langlois, Richard N. 1986a. "The New Institutional Economics An Introductory Essay," in R.N. Langlois, ed., *Economics as a Process: Essays in the New Institutional Economics*. New York: Cambridge University Press.

Langlois, Richard N. 1986b. "Rationality, Institutions, and Explanation," in R.N. Langlois, ed., *Economics as a Process: Essays in the New Institutional Economics*. New York: Cambridge University Press.

Langolis, Richard N. 1988. "Are Economic Models Applicable to Politics?" *Economia delle Scelta Pubbliche/Journal of Public Finance and Public Choice* 1988(2): 83-93 (Summer).

Latsis, Spiro J. 1972. "Situational Determinism in Economics," *The British Journal for the Philosophy of Science* **23**: 207-45.

Latsis, Spiro J. 1976a. "The Limitations of Single-Exit Models: Reply to Machlup," *British Journal for the Philosophy of Science* **27**: 51-60.

Latsis, Spiro J. 1976b. "A Research Program in Economics," in S. J. Latsis, ed., *Method and Appraisal in Economics*. Cambridge: Cambridge University Press.

Lester, Richard A; 1946. "Shortcomings of Marginal Analysis for Wage-Employment Problems," *American Economic Review* **36**: 63-82.

Lester, Richard A. 1947. "Marginalism, Minimum Wages, and Labor Markets," *American Economic Review* **37**: 135-48.

Loasby, Brian J. 1976. *Choice, Complexity, and Ignorance*. Cambridge: Cambridge University Press.

Machlup, Fritz. 1936. "Why Bother with Methodology?" *Economica* N.S., **3**: 39-45.

Machlup, Fritz. 1946. "Marginal Analysis and Empirical Research," *American Economic Review* **36**: 519-54.

Machlup, Fritz. 1947. "Rejoinder to an Antimarginalist," *American Economic Review* **37**: 148-54.

Machlup, Fritz. 1955. "The Problem of Verification in Economics," *Southern Economic Journal* **22**: 1-21.

Machlup, Fritz. 1960. "Operational Concepts and Mental Constructs in Model and Theory Formation," *Giornale degli Economisti*, New Series **19**: 553-582.

Machlup, Fritz. 1963. *Essays on Economic Semantics*. Englewood Cliffs: Prentice-Hall

Machlup, Fritz. 1964. "Paul Samuelson on Theory and Realism," *American Economic Review* **54**: 733-736.

Machlup, Fritz. 1967. "Theories of the Firm: Marginalist, Behavioral, Managerial," *American Economic Review* **57**: 1-33.

Machlup, Fritz. 1974. "Situational Determinism in Economics," *British Journal for the Philosophy of Science* **25**: 271-84.

Machlup, Fritz. 1978. *Methodology of Economics and Other Social Sciences*. New York: Academic Press.

Moss, Scott. 1984. "The History of the Theory of the Firm from Marshall to Robinson and Chamberlin: the Source of Positivism in Economics," *Economica* **51**: 307-318.

Nelson, Richard R. and Sidney G. Winter. 1982. *An Evolutionary Theory of Economic Change*. Cambridge: Harvard University Press.

Pareto, Vilfredo. 1909. *Manuel d'economie politique*. Paris: V. Giard and E. Briere. Translated as *Manual of Political Economy* by A.S. Schwier and A.N. Page. Clifton, H.J.: Augustus M. Kelley, 1971.

Popper, Karl R. 1959. *The Logic of Scientific Discovery*. New York: Harper.

Robinson, Joan. 1975. "Reswitching: Reply,." *Quarterly Journal of Economics* **89**(1): 53-55.

Samuelson, Paul A. 1947. *Foundations of Economic Analysis*. Cambridge: Harvard University Press. (Enlarged edition, 1983.)

Samuelson, Paul A. 1975. "Steady-State and Transient Relations: A Reply on Reswitching," *Quarterly Journal of Economics* **89**(1): 40-47.

Schutz, Alfred. 1943. "The Problem of Rationality in the Social World," *Economica*, N.S. **10**:130-149.

Schutz, Alfred. 1967. *The Phenomenology of the Social World*. Trans., G. Walsh and F. Lehnert. Evanston, Ill.: Northwestern University Press. [First published in 1932.]

Schutz, Alfred, and Thomas Luckmann. 1973. *The Structures of the Life-World*. Trans. R.M. Zaner and H.T. Engelhardt. Jr. Evanston, Ill.: Northwestern University Press.

Stigler, George. 1947. "Professor Lester and the Marginalists," *American Economic Review* **37**: 154-57.

Winter, Sidney. 1964. "Economic 'Natural Selection' and the Theory of the Firm." *Yale Economic Essays* **4**: 225-272.