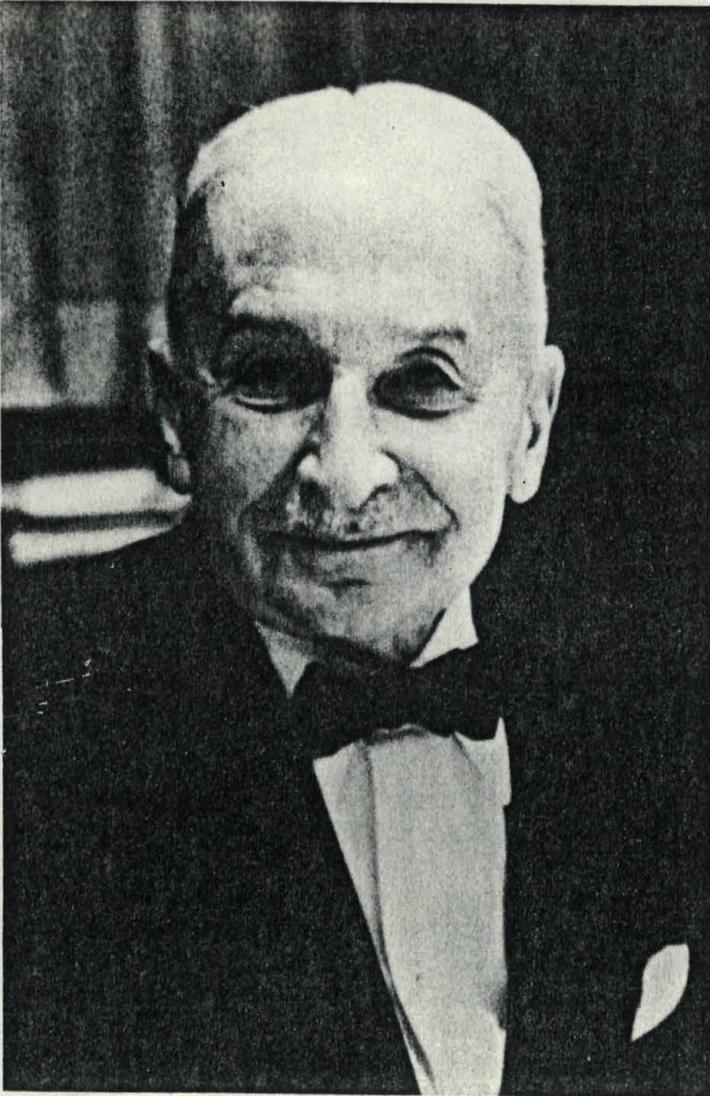


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Method, Process, and Austrian Economics

Essays in Honor of Ludwig von Mises



Courtesy of Margit von Mises

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New York University

1982

LexingtonBooks
D.C. Heath and Company
Lexington, Massachusetts
Toronto

re of the market process could only emerge to the extent that he left his priorism behind and formulated (empirical) hypotheses about the expectations of the actors involved. Whereas I have great difficulty in reconciling these, the praxeologist, with Mises, the economist, such problems do not exist for Rothbard. My claim is (and I interpret Lachmann as saying the same, at least implicitly), that praxeology cannot accommodate expectations in any *meaningful* way. For a more detailed analysis along similar lines see Professor Meyer's recent paper, "Ludwig von Mises und das objektivistische Erkenntnisprogramm."

30. Hayek, *Counter-Revolution*, p. 47.

31. The charge of circular reasoning has already been raised by A.R. Peacock, "The Interpretation of Subjective Value Theory in the Writings of the Austrian Economists," *The Review of Economics Studies* 1 (1933-1934):179.

32. P. Davidson, "Post Keynesian Economics: Solving the Crisis in Economic Theory," *The Public Interest*, special issue (1980):162.

33. J.A. Schumpeter, *History of Economic Analysis* (London: Allen & Unwin, 1954), p. 919n.

34. In this enterprise he has been joined by forceful allies from the post-Keynesian camp. See D. Vickers, "Uncertainty, Choice, and the Marginal Efficiencies," *Journal of Post Keynesian Economics* 2 (Winter 1979-1980):240-254, and D. Vickers, "Real Time and the Choice-Decision Problem," *Journal of Post Keynesian Economics* 3 (Summer 1981):545-551. It is precisely the issue of expectations that is at the heart of a controversy that has been raging for some years now within the Cambridge School between a Keynesian and a Ricardian wing. A flavor of the issues involved may be tasted in P. Garegnani, "Notes on Consumption, Investment and Effective Demand: A Reply to Joan Robinson," *Cambridge Journal of Economics* 3 (June 1979):181-187.

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Mises and Lakatos: A Reformulation of Austrian Methodology

Mario J. Rizzo

Complex phenomena in the production of which various causal chains are interlaced cannot test any theory. Such phenomena, on the contrary, become intelligible only through interpretation in terms of theories previously developed from other sources.

—Ludwig von Mises

It is not that we propose a theory and Nature may shout NO; rather, we propose a maze of theories and Nature may shout INCONSISTENT . . . Nature's actual "INCONSISTENCY" in a pluralistic methodology takes the form of a "factual" statement couched in the light of one of the theories involved, which we claim Nature had uttered and which, if added to our proposed theories, yields an inconsistent system.

—Imre Lakatos

The methodological writings of Austrian economists have rarely been properly understood. This communication failure has in part been due to the somewhat eccentric terminology of the Austrians and in part to the almost universal sway that naive falsificationism has had among some economic methodologists. One noted methodologist and historian of thought has recently said of Ludwig von Mises that "he made important contributions to monetary economics, business cycle theory and of course socialist economics, but his later writings on the foundations of economic science are so cranky and idiosyncratic that we can only wonder that they have been taken seriously by anyone."¹

The purpose of this chapter is to reconstruct rationally the Austrian methodological framework along lines delineated by Imre Lakatos's methodology of scientific research programs (MSRP).² By so doing, we shall demonstrate both the seriousness and profundity of Austrian methodological work as well as the grave mistake made by methodologists in ignoring this tradition. Rational reconstruction of an area is not the same thing as

I am indebted to Israel Kirzner and Gerald O'Driscoll for helpful discussions. Responsibility for errors remains mine alone. Material quoted in epigraphs comes from the following sources: Ludwig von Mises, *Human Action: A Treatise on Economics*, 3d ed. (Chicago: Henry Regnery, 1966), p. 31, and Imre Lakatos, "Falsification and the Methodology of Scientific Research Programmes," in *Criticism and the Growth of Knowledge*, ed. Imre Lakatos and Alan Musgrave (Cambridge: Cambridge University Press, 1970), p. 130 and p.130, n.2.

rendering a body of ideas more or less as expounded by its leading thinkers. Instead, rational reconstruction involves showing the interconnection of ideas even where their originators had not perceived them and creating new analytical categories or distinctions where that seems implicit in the original treatment. Most importantly, however, rational reconstruction sometimes involves departure from the original ideas where they cannot be made consistent with the central insight or core of the theoretical system. In fact, such deviation is required if our task is to have any importance other than as a footnote in the history of thought. Rational reconstruction is the first, albeit necessary, step in rendering a previously dormant research program viable and fit for continued development in the modern community of scientists.

There are two distinct advantages in rationally reconstructing Austrian methodology in terms of the MSRP. First, Lakatos's system enables us to specify clearly the sense in which economic statements are nonfalsifiable or *a priori*, as well as to understand the proper role of observational or historical evidence.³ Second, the MSRP emphasizes the logical unity of both the natural and social sciences.⁴ Although the content of the abstract categories in the MSRP differs from discipline to discipline, they perform virtually the same logical functions in all areas.⁵ This should dispel the widely held view that Austrian methodology is eccentric and based on antiquated and incoherent logical foundations.⁶

This chapter is divided into three parts. The first section briefly outlines Lakatos's MSRP, emphasizing the major categories of hard core, protective belt, and positive heuristic. The second section shows how the Austrian framework can be reconstructed along these lines. Here we shall stress the senses in which economic statements are *a priori* or untestable. In the third section a proposal is made that Austrians in their applied work adopt Lakatos's criteria for progressive and degenerating problemshifts. In the final part of this section, we address the question of under what circumstances might the Austrian research program be abandoned. In answering this question we hope to throw some light on the *a priori* nature of the program's research policy.

The Methodology of Scientific Research Programs

The unit of analysis in most philosophies of science is the theory; in Lakatos's framework, however, the relative unit is a set of theories bound together by a common logical foundation. This foundation is characterized by three major features: a theoretical hard core, a protective belt of auxiliary assumptions and observational theories, and a positive heuristic or set

of instructions on how to carry out the research program.⁷ Each theory within a program bears a logical relationship to the others in terms of these analytical categories. As a consequence, no single theory stands alone when confronted by complex experience. Inconsistencies between the theory and experience will, sometimes, bring about changes in the former. However, these changes will not adversely affect the research program in the least as long as revisions are made in certain specified directions. In fact, inconsistencies constitute the dynamic whereby the program as a whole ultimately gains ever-increasing applicability.

The theoretical hard core of a research program consists of one or more statements that are rendered irrefutable by the methodological decision of the scientists working within the program. Hard cores frequently make use of purely nonobservable or metaphysical entities like force or gravity. However, the elements of a hard core need not be totally foreign to observation. They can be features of reality that are so general and so pervasive that they are capable of almost infinite variation or manifestation. One such feature is rationality. The pure form of ratiocination is a highly abstract "entity" that is never really seen or perceived. What is perceived, instead, at least in an indirect way, is specific goal-directed behavior such as profit or sales maximization. A research program that includes rationality in its hard core will admit evidence that argues against one of its specific manifestations but will not hear "evidence" that denies the existence of rationality altogether.⁸ Thus, a hard core consists of those basic presuppositions to which the scientist clings tenaciously and which he refuses to change whenever mere observation is inconsistent with his theory.

The protective belt, on the other hand, is the part of a theory that is subject to modification when observations are inconsistent. The impact of recalcitrant facts on a theory is never unambiguous. Any part of the theory may be at fault, and it is impossible to test each part separately. The negative heuristic of the program forbids us from directing the refutations at the hard core itself.⁹ Instead, the components of the protective belt are altered, stretched, and manipulated to accommodate observational inconsistencies. These components consist of two closely related parts: the auxiliary statements and background observational theories.

Auxiliary statements include the initial conditions, the boundary conditions, and the closure clause. The term *initial* conditions is perhaps unfortunate since it appears to limit consideration to those conditions existing prior to the explanandum event. In fact, a better term would be *logically antecedent*, since it is possible to infer an earlier state from later initial conditions.¹⁰ The term is therefore one of logical and not temporal relation.¹¹ Initial conditions accordingly form the logical context of relevant factors in which the system's general laws operate. The second type of auxil-

ary statement is the boundary conditions that specify the values of the relevant factors that exist between the time of the initial conditions and the explanandum event. In economics boundary conditions are usually incorporated in the status of the *ceteris paribus* clause. Finally, the closure clause consists of a statement of the types of variables that are relevant in both the initial and boundary conditions and, at least implicitly, a statement of those that can be ignored.

Any auxiliary statement, if properly altered, can save a "refuted" theory. The inconsistency between observation and theory may be because of an improper ascertainment of the initial conditions, some disturbing boundary variable, or premature closure of the system (an important variable may have been left out of consideration).¹²

The second major component of the protective belt is the background observational theories. These are the interpretative schemes that act as a filter through which we perceive (1) the initial and boundary conditions and (2) the explananda data or observations. Observational theories are generally part of the accepted intellectual environment in which testing takes place. For example, the theory of optics is a necessary, although perhaps implicit, prerequisite for the corroboration of astronomical theories. Nevertheless, in cases of inconsistency between the "facts" and a theory, the observational theory itself may be challenged. As Lakatos put it, "[We] may reject the 'facts' as 'monsters.'" ¹³ The rejection or alteration of an observational theory is also subject to the general features of the MSRP. This theory also will have an irrefutable hard core, and change will occur only through its protective belt.¹⁴

The third major feature of the MSRP is the positive heuristic. In the first instance, this tells the scientist where to direct the potential explanatory power of the theories deriving from the program. What kinds of problems are within the potential range of applicability? What is the appropriate subject matter of the discipline? In this light one can see the older controversies over the proper subject matter of economics as attempts to formalize a positive heuristic rather than mere disputes over nominal definitions. Economics evolved from the science of wealth to a more general science of human action.¹⁵ The research program expanded to include all manner of subjects undreamt of by Ricardo or Mill. In addition to formulating a general research domain, the positive heuristic also "consists of a partially articulated set of suggestions or hints on how to change, develop the 'refutable variants' of the research programme, how to modify [or] sophisticate, the 'refutable' protective belt."¹⁶ There are proper and improper, fruitful and unfruitful ways of modifying the auxiliary hypotheses. The positive heuristic shows the way.

Austrian Economics as a Scientific Research Program

The Hard Core

The fundamental presupposition of Austrian economics is that man acts or, equivalently, that he engages in purposeful behavior.¹⁷ This is not to say that he never does anything else or that reactive stimulus-response is foreign to our conception of man. It is to argue, at least implicitly, however, that action is of primary importance in a significant domain of social phenomena. This is not obvious a priori, and ultimately the action presupposition must validate itself by bringing forth a fruitful research program.¹⁸ From the simple statement that man acts several important corollaries can be derived, and these constitute a more detailed picture of the hard core. We shall examine each of these corollaries in turn.

Individuals Perceive a Decision-making Environment. Without some idea of the relevant context, no action at all would be possible, because individuals could not then formulate a plan to achieve their ends (assuming they could even formulate ends in such chaos).¹⁹ Planning requires knowledge of technological relationships, the availability of resources, the alternatives sacrificed, and so on. Since all action is oriented toward the future, only the imagined or projected state of these variables matters. In addition, the objective future state is *prima facie* unimportant, because all action is a mental balancing of alternatives.²⁰ In this sense, then, economics is about thoughts and not about things.²¹

Perceptions Take Place in a World of Uncertainty. The concept of action logically implies the existence of uncertainty. To see this, consider first a world in which everything except an individual's actions is certain. Under such conditions, why would anyone ever act? By assumption, men could affect nothing, and hence, strictly speaking, action would be impossible. There could be no such thing as a means to an end.²² Consider now a world in which everything including human action is certain. In such an environment action could not exist as we normally understand it. Instead we would see merely automatic or reflexlike behavior similar to "the involuntary responses of the body's cells and nerves to stimuli."²³ Part of what we mean by human action is its lack of deterministic nature and consequent imperfect predictability.²⁴ Hence action in a world of complete certainty is strictly a logical impossibility.

That action logically entails uncertainty does not mean that it is not sometimes useful to imagine it in a world of certainty. Here action would

take the form of an equilibrium adaptation to the environment: the set of actions that, if performed, would result in the complete compatibility of individual plans. Action, under these circumstances, is a description of certain end-states rather than of a process of achievement. Although the logical incompatibility of genuine action and certainty should give us pause to reflect, it need not be a complete obstacle to the heuristic use of a certainty model.²⁵ Indeed, "some of the most important research programmes in the history of science were grafted on to older programmes with which they were blatantly inconsistent."²⁶ In effect, this is the grafting of a general human-action program on to the older determinist maximizing program. Although they are not strictly compatible, each can illuminate some different aspect of complex reality.

Individuals' Perceptions Are Not Always Correct. The possibility that individuals will act in a mistaken fashion in no way compromises the assumption of rationality. Rationality is a purely formal relationship between means and ends and does not depend on the accuracy of the individuals' information. A decision can be optimal or rational relative to incorrectly perceived data.²⁷ Such a decision could be called inefficient but never irrational.²⁸ On the contrary, the concepts of perception and uncertainty, which were derived from the rationality postulate, would have no importance if perceptions were always correct. For then we could ignore these filters, and the relevant conditions would be the objective circumstances themselves. This would mark a return to the simple neoclassical research program.

The importance of error in the Austrian research program does not demonstrate the dichotomous nature of the subjectivist method. Although for a perception to be incorrect it must be incorrect relative to some "objective" state of affairs, the latter is always perceived in terms of a background observational theory. Hence error really involves the conflict or inconsistency among two or more theories. To the extent that we regard the observational theory as completely noncontroversial, we can predict the revision of agents' perceptions in the direction of objective reality. However, there is nothing automatic about this, since events can be interpreted differently by different people. The road to equilibrium is thus much rougher than that portrayed in simple neoclassical parables.

Action Is Coordinating.²⁹ The tendency toward coordination follows directly from the action postulate itself.³⁰ Discoordination opens up the opportunity for profit. *A* would like to buy *X* at any price below \$75 and *B* would like to sell it at any price above \$70. There is thus a potential arbitrage profit of \$5 to be gained by bringing them together or coordinating their plans. The purposeful pursuit of profit sets in motion a tendency toward elimination of this discoordination.

The coordination presumption may appear to be in conflict with the statement that perceptions are not always correct. If a profit opportunity exists but is not perceived, then coordination will not result. Furthermore, if entrepreneurs are not purely profit maximizers, then striving for other goals may interfere with complete coordination of plans. This is undoubtedly true, but it is beside the point. Our claim is only that there is a tendency toward coordination, not that there will be actual coordination (as the simple neoclassical hard core requires). To assert that there is such a tendency is to make no claim about the frequency with which coordination comes about. A tendency law says merely that a specified state of affairs will come about, but only under certain circumstances.³¹ Our attention is then directed to the variables that can interfere with the attainment of coordination. If these variables turn out to be important factors in understanding *observed* market phenomena, then the tendency law has proven its worth. The presumption that action is coordinating does not have direct empirical applicability. Nevertheless, it can still be a useful tool in identifying important features of actual markets.³²

The Protective Belt

Initial Conditions. In economics, initial conditions are typically those factors existing at the time of an assumed disturbance (for example, an increase in demand) that are logically antecedent to the explanandum statement,³³ These include such things as the agent's motivation (for example, profit maximization), the state of knowledge, the condition of expectations, and so on. When observations are inconsistent with the implications of a theory, the problem may be that the initial conditions have been misspecified. For example, suppose that the demand for a commodity has permanently increased. Because suppliers mistakenly think that the shift in demand is only temporary, they decide not to raise prices.³⁴ A hypothesis that assumes perfect foresight, on the other hand, would predict a rise in the price of the commodity, and this would be in conflict with the observed stable price. The inconsistency between our theory and observation could be eliminated, however, by changing the assumption regarding the initial state of expectations.

The key question here is: Must the initial conditions (sometimes referred to as "the assumption") be independently testable, at least in principle? Alternatively, is the corroboration of the entire theory's implications sufficient to justify use of a particular set of assumptions? To anticipate the argument of the subsequent paragraphs, we claim that if initial conditions are not subject to independent tests, at least three important problems will emerge.

Serving to
stimulate
investigation

but
coordination
need not be
monetary only

Consider, for example, the proffered motivational assumption of profit maximization. What can it mean to say that individuals act as if they maximize profits instead of acting in order to maximize profits?³⁵ It can only mean that, although sometimes this motivational assumption is false, predicted market outcomes are nevertheless consistent with profit maximization.

This creates a paradox. It is asserted that individuals who do not maximize profits behave in the same way as those who do. Here we obviously have a puzzle. To say that the motivational assumption is unimportant or irrelevant to the explanation will not suffice. If that were true, then the hypothesis would have been misspecified in the first place. Presumably, if the content of individuals' motivation is unimportant, then it should be possible to invent an explanation that makes no use of the rationality postulate at all.

When a statement constituting the explanans³⁶ is false, we do not have genuinely scientific explanation.³⁷ If the profit-maximization premise of an explanation is false, then the deductions or implications can only be true by accident. Hence such premises are not really involved in the explanation.

Suppose, finally, that we assert that the motivational assumption is true, but, by a methodological decision, we choose to test it only through the success or failure of the central hypothesis. Since the assumed motivation is part of the central hypothesis, the whole idea of the latter's success or failure becomes ambiguous.³⁸ If the model predicts Y , and X occurs instead, what went wrong? There is more than one initial condition that could be inaccurate. It is also possible that the ceteris paribus clause was inapplicable. Or that the range of important variables (closure clause) was defined too narrowly. Finally, the general law may be wrong or, more exactly, inapplicable to this class of phenomena. We have no guidance to the improvement of the hypothesis.

If the foregoing arguments are correct, then initial conditions ought to be independently testable, at least in principle.³⁹ The performance of these tests, however, requires an implicit or explicit observational theory. Such a theory enables us to interpret the data that bear on the initial conditions or assumptions. Consider, once again, the motivational hypothesis of profit maximization. Before we can say that individuals are maximizing profits, we must be able to understand or interpret their actions. The theory that permits us to do so will have much in common with the overall hypothesis. Both will have a hard core consisting of the action or rationality postulate and a set of auxiliary hypotheses. In the case of the observational theory, all the tentative hypotheses about the motivation of certain individuals presuppose rationality. Therefore, we need merely vary the content of the rationality assumption (that is, the auxiliary hypotheses) until we understand the relevant behavior.⁴⁰

what kind of profits? \$ or psychic?

From the perspective of testing the central hypothesis, the hard core of the observational theory is part of the accepted background knowledge. Although the hard cores of both theories are substantively the same, acceptance of the latter does not imply that we *must* choose to insulate the rationality postulate in the major hypothesis from refutation. The decision to render it nonfalsifiable is still merely a methodological decision. It is important to keep in mind the different functions of the postulate in each context. In the observational case its acceptance means that it has been found useful in describing patterns of relatively simple behavior.⁴¹ However, it does not logically follow that such an assumption will be useful in explaining the causes of more complex phenomena like overall market outcomes (for example, the nature of oligopolistic markets). The question is whether presuppositions that are useful in simpler situations are also useful or applicable in more complex situations.⁴²

The decision to regard the action assumption as an irrefutable part of the Austrian research program is thus quite different from the decision to accept it in an observational context. The former decision does not admit the possibility that rationality is inapplicable to understanding complex market outcomes. We could allow this possibility (if we were to go outside the program) and yet believe that individual behavior in simpler settings (everyday life, for example) is best explained on rational grounds. The relevant choice is whether, for a certain class of explananda, the rationality component of the explanans shall be irrefutable. Acceptance of that postulate for one class of (simpler) phenomena need not imply acceptance for another class.

Boundary Conditions and the Closure Clause. Suppose we want to infer an event at t_1 from the state of the system at t_0 . Then knowledge of the initial conditions at t_0 will not be enough. It is also necessary to know something of the conditions prevailing between t_0 and t_1 (the boundary conditions).⁴³ When the conditions at t_0 are unchanged between t_0 and t_1 , we can say that some appropriately specified ceteris paribus clause is applicable. If conditions do not remain unchanged, then, of course, the predictions of the hypothesis may be inconsistent with observed outcomes. Any such inconsistency can potentially be attributed to the inapplicability of some aspect of the ceteris paribus clause. A decrease in prices following an increase in demand, for example, might be explained by changes in technology.

There are two ways of stating ceteris paribus clauses: either they can remain general and their contents left vague, or else they can be explicitly specified (that is, $x_1 \dots x_n$ are held constant). In the former case, they merely insulate the hard core from refutation and provide no guidance as to how the hypothesis might be improved. Since nothing is ever completely unchanged, it is always possible to attribute inconsistencies to changes in

some boundary condition. When, on the other hand, those conditions are specified, we have clues as to where a particular change must be made.⁴⁴

If a prediction is falsified but the values of the initial and boundary conditions are accurately ascertained, the problem may then be with the closure clause.⁴⁵ It is no simple task to determine what are the relevant variables to be included in the auxiliary statements. For example, the requirement that other factors must be held constant does not tell us what or how many these factors are.⁴⁶ Even if x_1, \dots, x_n have been held constant, the relevance of an additional factor, x_{n+1} , may have been overlooked. When that is taken into account, it is possible that the theory-observation inconsistency may be resolved.

Background Interpretation of the Explananda Events. The explananda, as well as the auxiliary statements, associated with all theories must be interpreted within the context of an observational theory. There are two levels on which this occurs: (1) interpretation of the observational categories themselves (for example, prices and money); and (2) interpretation of the accuracy of the operational counterparts to the theoretical categories (for example, the accuracy with which rents are reported).

The explananda of economics are overall market outcomes rather than merely the behavior of particular individuals. Nevertheless, these market phenomena are often described in terms that imply purposeful orientation or purposeful-interaction patterns.⁴⁷ For example, economics seeks to explain why rents in the unregulated sector of the New York housing market have risen rapidly or whether the oil embargo of several years ago caused a decrease in the purchasing power of money. Merely to understand these questions, however, it is necessary to comprehend the subjective-meaning content of the theoretical terms. A price, for example, is not just a number written on the tag of an item. It refers to the terms on which an exchange is made. Further, money is not merely a piece of paper with some writing on it that happens to change hands when goods change hands. It is a medium of exchange, that is, something that is generally desired or bargained for.⁴⁸

Although the terms of the explananda are viewed as embodying purpose-oriented functions (that is, they are Mengerian institutions), it does not logically follow that causal explanations must incorporate reference to the purposes. There is an important distinction between the purposes that define an institution, like money, and those that may be involved in bringing it about, changing, or maintaining it (that is, the causal factors).⁴⁹ A theory is necessary to demonstrate the connection between the function of an institution and its causal genesis. No one need have been aware of the ultimate function of an institution when his behavior brought it about. It is logically possible to design a causal explanation of market phenomena without reference to their function-associated purposes, or any purposes what-

ever. Teleological explanations are required not by the nature of the explananda but by the positive heuristic of the research program.

Theoretical consideration can be brought to bear on the accuracy with which individuals report economic data. If, for example, landlords are taxed more heavily the higher the rents they charge, and if tenant taxes are invariant with respect to the level of rents, we would expect tenants to be more accurate in reporting than landlords. As in the discussion of initial conditions, the observational theories here also rest on a hard core of rationality. Unless some background theory of data accuracy is accepted, there will be a highly ambiguous relationship between actual collected data and the theory under test. If quantity X , alleged to be the price of A , rises, this does not mean that the prediction of a fall in the price of A is thereby falsified. For that to be so, we must be confident that X indeed measures, within tolerable levels of accuracy, the price of A .

The Positive Heuristic

The positive heuristic specifies the fundamental research policy of the scientific program.⁵⁰ This includes both the basic problem to which work undertaken within the program is addressed and a set of instructions on how to modify auxiliary statements to deflect falsification from the hard core.

The Fundamental Problem. The central research goal of Austrian economics has been stated in two, not altogether compatible, ways. First, Carl Menger believed that the problem of the social sciences is: "How can it be that institutions which serve the common welfare and are extremely significant for its development came into being without a *common will* directed toward establishing them?"⁵¹ Menger thought that the recognition of certain institutions as "organic" or spontaneous in origin did not exclude "striving for the exact (the atomistic) understanding of them."⁵² In more familiar terminology, he believed that they were the unintended outcomes of individual human action.

The Mengerian statement unduly restricts the scope of economics. First, the basic question is directed toward "institutions which serve the common welfare" and not those that may be inimical to it. Second, an institution is an equilibrium phenomenon—it exhibits a certain constant and coordinated pattern of individual behavior.⁵³ It is equally important, however, to understand "noninstitutions," or failures to achieve coordinated activity. Although Menger cannot be fairly characterized as an equilibrium theorist, his formulation of the positive heuristic may restrict our thinking to equilibrium terms.

In Hayek's view, on the other hand, the central problem is "to explain

the unintended or undesigned results of the actions of many men."⁵⁴ This statement is broader insofar as it makes reference to "results" and not to "institutions." Furthermore, there is no attempt to evaluate these results as beneficial or not. Therefore, the Hayekian research policy is to recompose or reconstitute either equilibrium or disequilibrium phenomena from the purposeful activity of individuals. Whether these phenomena "serve the common welfare" can be the subject of a separate, auxiliary investigation, but the central research problem is not restricted to beneficial developments.

The Set of Instructions. As we have seen, when observations are inconsistent with the implications of a theory, the protective belt must be altered. The positive heuristic offers a set of very general instructions on how this ought to be done. It is not possible to set these out in explicit detail. The treatment of anomalies involves scientific creativity rather than mechanical surveying of a checklist. Nevertheless, two broad, and partially overlapping, strategems suggest themselves:

1. Reconsider the expectational component of an explanation. Perhaps observations are inconsistent because inadequate attention has been paid to the content of expectations.

2. Search for the relevant communication breakdown among economic agents. When phenomena appear discoordinative despite the equilibrating tendency, the Austrian economist first asks why information has failed to be transmitted correctly. Perhaps, as in the Austrian business-cycle theory, price signals have been seriously distorted. The neoclassical economist, on the other hand, is more likely to search for changes in the underlying data, because the hard core of his theory presupposes the effective communication between economic agents.⁵⁵

Summary: Apriorism

The previous two sections of this chapter have elucidated Lakatos's concept of a research program and applied it to Austrian economics. From this perspective, the role of nonfalsifiable or aprioristic statements in economics can be greatly clarified. Much of the confusion that has plagued discussions of Austrian apriorism arises from a failure to distinguish four senses in which economic theory is "logically and temporally antecedent to any comprehension of historical facts."⁵⁶ First, the postulate of rationality is immune to falsification as part of the research program's hard core. Second, since the laws of economic theory are of the deductive variety—arising out of implications from the action postulate and a few generally accepted empirical assumptions,⁵⁷—the irrefutability of the hard core is thus passed

on to the theoretical structure. Third, the rationality principle is part of the accepted background theory underlying the interpretation of both auxiliary statements and the explananda themselves. Hence, ". . . any conceivable experiment must use hard-core concepts of that very theory to structure the observations to be utilized."⁵⁸ Finally, the positive heuristic presupposes purposefulness by requiring social phenomena to be recomposed from individual human action. This research policy must obviously be logically prior to any concrete application of it.

The Method of Appraisal

We have previously discussed the general directions in which Austrian economists will strive to change specific theories when they are inconsistent with observations. However, nothing has been said about the method of appraisal or the standard against which these revised theories are to be judged. Unfortunately, since Austrians have been traditionally more concerned with the logical development of economics than with application, there is little in their work to suggest such a standard. In this section, however, we propose that Austrians adopt Lakatos's criterion for the revision of theories: "[W]e must require that each step of a research programme . . . constitute a consistently progressive theoretical problemshift. . . . [T]he programme as a whole should also display an intermittently progressive empirical shift. We do not demand that each step produce immediately an observed new fact."⁵⁹

Before we can examine this proposal more fully, it is necessary to define some terms. By a "progressive theoretical problemshift" Lakatos means that each new theory in the series under consideration predicts, in addition to all that predicted by its predecessor, some novel fact inconsistent with the previous theory.⁶⁰ A "progressive empirical shift" means that "some of this excess empirical content is corroborated."⁶¹ The novel facts that ought to be predicted by progressive problemshifts need not be future events. Consistent with standard scientific usage, the word *prediction* includes postdiction or retrodiction.⁶² Therefore, the inference of past events from a theoretical model satisfies the criterion. In addition, novel facts need not be those that are unknown to the observer before testing the theory. Facts known to everyone are as much candidates for scientific investigation as those previously unknown. A fact is novel with respect to a given theory if it does not constitute part of the evidence corroborating the auxiliary hypotheses or background observational theories.⁶³ If a fact, a , is used as evidence that the auxiliary assumptions should be changed from $y_1 \dots y_n$ to $z_1 \dots z_n$, and then this revision is used in conjunction with a general law to explain the same fact, we have partly explained a by a .

An example should make all this clearer. Suppose at t_0 individuals each buy three units of Y and then at t_1 , buy five units. If we can explain the outcome at t_0 but not that at t_1 , then the outcome at t_1 is an anomaly within our theory. The Austrian seeking to eliminate this inconsistency should keep at least two things in mind. First, he should never challenge the consistency of preferences: this is part of the program's hard core.⁶⁴ Second, he ought to ask whether the cause to which he attributes the changed behavior is observable independent of the behavior itself. Suppose, for example, the economist tries to explain the altered consumption pattern by claiming the individuals' tastes had changed. If this statement is merely imputed from the observed behavior and not independently ascertainable, then the shift in theories is "degenerating." Instead of predicting additional facts, the new theory merely patches up the prior inconsistency. Since we do not observe independently the auxiliary assumption about tastes, we have no way of knowing whether the assumption is true or whether we have merely explained a by a .

If, on the other hand, the economist revises his previous assumption of static expectations and claims that in the second period consumers believed the price was about to rise dramatically, this may be a progressive problemshift. To the extent that the change in expectations is independently ascertainable, the new theory will predict some novel facts. For example, it may predict in a host of other situations that, although current prices have risen, the quantity demanded will nevertheless increase. The independent testability of the expectations assumption assures us that we can genuinely explain both the anomalous fact and other phenomena previously thought to be inexplicable.

This method of appraising shifts in theories outlined is intimately related to the question of when a research program ought to be abandoned. Even Mises believed that the praxeological or purpose-oriented program could be surpassed. Although the laws of economics are, in a certain sense, nonfalsifiable, the usefulness or applicability range of economics is not beyond question. He left open the possibility that what we today call action may someday be explicable in terms of "physical, chemical, and physiological phenomena." "We may or may not believe," Mises concedes, "that the natural sciences will succeed one day in explaining the production of definite ideas, judgments of value, and actions in the same way they explain the production of a chemical compound."⁶⁵ Although he did not think this was likely, it was at least logically conceivable. Unfortunately, Mises did not pursue the implications of this point and therefore did not specify the conditions under which his research program ought to be abandoned. Lakatos, on the other hand, did give serious attention to this issue. He suggested a criterion that follows immediately from the method of appraising theory revisions: "A research programme is successful if all this [adjustment of

auxiliary hypotheses] leads to a progressive problemshift; unsuccessful if it leads to a degenerating problemshift."⁶⁶ This criterion is not without its ambiguities and difficulties in application. Hard cores do not suddenly arrive ready-made.⁶⁷ Their development can easily be confused with the degeneration of a research program.⁶⁸ To a great extent, these two developments can only be identified ex post, as we see how the insulation of hypotheses is utilized. If ultimately there is an expansion of the program's novel predictions, then it is at least theoretically progressive. Similarly, the corroboration of additional empirical content may not occur immediately. The length of time we should wait, the toleration or patience we accord a research program, is not something for which we can lay down hard and fast rules. Nevertheless, the MSRP provides an anchor or center of gravity in the appraisal of research frameworks. To put matters another way, good scientists exhibit a *tendency* to abandon a program when it has become degenerative. How quickly or how often they are actually observed to abandon such programs is another matter. It is sufficient that they be required to justify their continued adherence to the program in the terms of the MSRP, that is, in terms of the program's expected potential for ever-increasing explanatory power.

Conclusion

This chapter has rationally reconstructed Austrian methodology within the framework of Imre Lakatos's methodology of scientific research programs. We have clarified the several different senses in which economics can be considered a priori as well as clarified the proper role of testing and observation. As interest in Austrian economics continues to grow, and increasing numbers of economists seek to apply its insights to concrete problems, Lakatos's methodological ideas ought to become the solid basis on which we build our science.

Notes

1. Mark Blaug, *The Methodology of Economics* (Cambridge: Cambridge University Press, 1980), p. 93.
2. For a collection of the relevant articles see Imre Lakatos, "The Methodology of Scientific Research Programmes," in *Philosophical Papers*, vol. 1., ed. John Worrall and Gregory Currie (Cambridge: Cambridge University Press, 1978).
3. Mises uses these terms in several senses, see discussion of a priorism in second section of this chapter.

4. Alfred Schutz, "Concept and Theory Formation in the Social Sciences," in *Philosophy of the Social Sciences*, ed. Maurice Natanson (New York: Random House, 1963), p. 235.

5. On the application of the MSRP to non-Austrian economics see Spiro Latsis, ed., *Method and Appraisal in Economics* (Cambridge: Cambridge University Press, 1976). The application to the natural sciences is discussed in Collin Howson, ed., *Method and Appraisal in Physical Sciences* (Cambridge: Cambridge University Press, 1975).

6. See, for example, Blaug's inclusion of the modern Austrians in the chapter on the nineteenth-century methodology. Blaug, *Methodology of Economics*, pp. 91-93.

7. See, generally, Imre Lakatos, "Falsification and the Methodology of Scientific Research Programmes," in *Criticism and the Growth of Knowledge*, ed. Imre Lakatos and Alan Musgrave (Cambridge: Cambridge University Press, 1970), pp. 132-134.

8. Such evidence can, of course, be heard outside of the research program.

9. Lakatos, "Falsification," p. 133.

10. Israel Scheffler, "Explanation, Prediction and Abstraction," *British Journal for the Philosophy of Science* 7 (1956-1957):293-304.

11. In teleological explanations it is not possible for initial conditions to be later than the explanandum event. An image or projection of the goal to be achieved must precede the explained action. It is, however, possible that the initial condition motives may be inferred after the explanandum event. Thus, we may infer the existence of certain motives at t_0 from the evidence at t_2 which, in conjunction with general laws, produces an effect at t_1 .

12. Whether these factors must be independently observable will be discussed later in chapter.

13. Lakatos, "Falsification," p. 130.

14. This is a point that Lakatos does not pursue, but see the second section of this chapter.

15. Israel M. Kirzner, *The Economic Point of View*, 1960, reprint (Kansas City: Sheed, Andrews and McMeel, 1977).

16. Lakatos, "Falsification," p. 135.

17. Ludwig von Mises, *Human Action: A Treatise on Economics*, 3d ed. (Chicago: Henry Regnery, 1966), pp. 11-13.

18. The meaning of a "fruitful research program" will be discussed later in chapter.

19. See, generally, Mises, *Human Action*, p. 22 and G.L.S. Shackle, *Epistemics and Economics* (Cambridge: Cambridge University Press, 1972), p. 351.

20. "So far as human actions are concerned the things are what the acting people thing they are." Friedrich A. Hayek, *The Counter-Revolution of*

Science (New York: Free Press, 1955), p. 27. This is not to imply that the ex post mistakes are unimportant. This is the reason for the prima facie qualification.

21. Shackle, *Epistemics and Economics*, p. 66.

22. Mises, *Human Action*, p. 105.

23. *Ibid.*, p. 11.

24. See, generally, G.L.S. Shackle, "Imagination, Formalism, and Choice," in *Time, Uncertainty and Disequilibrium: Exploration of Austrian Themes*, ed. Mario J. Rizzo (Lexington, Mass.: Lexington Books, D.C. Heath, 1979), pp. 19-31.

Explanation and prediction are logically symmetrical operations. "If E [the explanandum statement] is given, that is, if we know that the phenomenon described by E has occurred, and a suitable set of statements $C_1, C_2 \dots C_k$ [initial conditions], $L_1, L_2 \dots L_r$ [laws] is provided afterwards, we speak of explanation of the phenomenon in question. If the latter statements are given and E is derived prior to the occurrence of the phenomenon it describes, we speak of prediction. It may be said, therefore, that an explanation of a particular event is not fully adequate unless its explanans, if taken account of in time, could have served as a basis for predicting the event in question." Carl Hempel and Paul Oppenheim, "Studies in the Logic of Explanation," in *Aspects of Scientific Explanation*, ed. Carl Hempel (New York: Free Press, 1965), p. 249. (Hempel and Oppenheim use *prediction* in the literal sense—see both discussion of prediction later in chapter and Lakatos, "Falsification," p. 118.) See also the endorsement of this view by an Austrian economist in Friedrich A. Hayek, "Degrees of Explanation," in *Studies in Philosophy, Politics and Economics*, ed. F.A. Hayek (Chicago: University of Chicago Press, 1967), p. 9, n. 4.

The reason that prediction is usually more difficult than explanation is that *ex ante* not all the initial conditions or boundary conditions (*ceteris paribus* clause) may be known. Shackle's point seems to be that neither prediction nor explanation of human action can be deterministic because, even if all the conditions were known, the laws of action would still imply only a pattern of outcomes rather than a unique one.

25. This is because in a certainty model we can formally change our concept of action to remove any explicit contradiction. The logical problems emerges (1) when we suppose that it is possible to integrate the two models into a continuous and coherent series of approximations (that is, a single research program) and (2) when we try to derive conclusions from the certainty model that are applicable, without modification, to an uncertain world.

26. Lakatos, "Falsification," p. 142, italics suppressed.

27. Hempel, "Aspects of Scientific Explanation" in *Scientific Explanation*, p. 464.

28. Mises, *Human Action*, p. 20.
29. Compare the similarities and differences between our statement of the Austrian hard core and Latsis's statement of the neoclassical hard core: "(i) Decision-makers have correct knowledge of the relevant features of their economic situation. (ii) Decision-makers *prefer* the best available alternative given their knowledge of the situation and of the means at their disposal. (iii) Given (i) and (ii), situations generate their internal "logic" and decision-makers *act appropriately to the logic of their situation*. (iv) Economic units and structures display stable, coordinated behavior." See Spiro Latsis, "A Research Programme in Economics," in Latsis, *Method and Appraisal*, p. 22.
30. See, generally, Israel M. Kirzner, "Economics and Error," in Louis M. Spadaro, ed., *New Directions in Austrian Economics* (Kansas City: Sheed, Andrews and McMeel, 1978), pp. 57-76.
31. Abraham Kaplan, *The Conduct of Inquiry* (San Francisco: Chandler, 1964), pp. 97-98.
32. Compare Mises, *Human Action*, p. 250.
33. The explanandum statement or explananda statements are the sentences describing the phenomena that we are attempting to explain.
34. The changing of prices is costly to both producers and consumers.
35. Milton Friedman, "The Methodology of Positive Economics," in *Essays in Positive Economics* (Chicago: University of Chicago Press, 1953), pp. 3-43.
36. The explanans, or explanatory apparatus, consists of sentences describing the initial conditions and a general law.
37. Hempel and Oppenheim, "Logic of Explanation," p. 248.
38. Jack Melitz, "Friedman and Machlup on the Significance of Testing Economic Assumptions," *Journal of Political Economy* 73 (February 1965):42-44.
39. All these arguments apply to the independent testability of *ceteris paribus* clauses as well.
40. Hempel, "Scientific Explanation," pp. 475-476.
41. "Daily experience proves . . . that our fellow men are acting beings as we ourselves are." Mises, *Human Action*, p. 26; "It cannot be denied that [the assumption of purposefulness] works." *Ibid.*, p. 24.
42. Hayek, "Degrees of Explanation," p. 10.
43. Hempel, "Scientific Explanation," p. 366. It is possible, of course, to subsume boundary conditions under the term *initial conditions*, since they are both logically antecedent conditions.
44. On the general criteria for revising theories, see later in chapter.
45. Ingvar Johansson "Ceteris paribus Clauses, Closure Clauses and

- Falsifiability," *Zeitschrift für allgemeine Wissenschaftstheorie* 11 (1980): especially 18-19.
46. A closure clause can also specify the relevant initial conditions. In sum, it closes the system.
47. Hayek, *Counter-Revolution of Science*, p. 31.
48. In cases where the subjective element appears absent, as in discussions of the multiplier, velocity of circulation, and so on, it is usually implicit. See Alfred Schutz, "Common-Sense and Scientific Interpretation of Human Action," in Natanson, *Philosophy of Social Science*, p. 334.
49. See the discussion in R.P. Dore, "Function and Cause," in *The Philosophy of Social Explanation*, ed. Alan Ryan (Oxford: Clarendon Press, 1973), p. 72.
50. The positive heuristic is in a sense part of the hard core, because the way in which a program is to be applied obviously must be consistent with the hard core.
51. Carl Menger, *Problems of Economics and Sociology*, 1883, reprint, trans. Francis J. Nock, ed. Louis Schneider (Urbana, Ill.: University of Illinois Press, 1963), p. 146.
52. *Ibid.*, p. 139.
53. These have no direct relevance to the issue of general equilibrium, of course.
54. Hayek, *Counter-Revolution of Science*, p. 25.
55. See, generally, Alex Leijonhufvud, "Schools, 'Revolutions,' and Research Programmes in Economic Theory," in Latsis, *Method and Appraisal*, pp. 87-93.
56. Mises, *Human Action*, p. 32.
57. *Ibid.*, pp. 64-69.
58. Leijonhufvud, "Schools, 'Revolutions,' " p. 80.
59. Lakatos, "Falsification," p. 134.
60. *Ibid.*, p. 118.
61. *Ibid.*
62. Karl R. Popper, *The Logic of Scientific Discovery* (New York: Harper and Row, 1959), p. 60, n. 2; Lakatos, "Falsification," p. 116, n. 4. Prediction and postdiction are logically symmetrical. In the former case a statement describing an event is derived before it occurs, and in the latter it is derived after it occurs. In both cases, however, in contrast to explanation, the explanans is logically given and the explanandum must be derived. See note 24 of this chapter.
63. Elie Zahar defines a novel fact as follows: "A fact will be considered novel with respect to a given hypothesis if it did not belong to the problem situation which governed the construction of the hypothesis." Elie

Zahar, "Why Did Einstein's Programme Supersede Lorentz's?" *The British Journal for the Philosophy of Science* 1 (1973):103. This definition is proper only if the auxiliary hypotheses are not considered independently testable. Then the anomalous observation is automatically the evidence in favor of making a specific change in the protective belt.

64. Israel M. Kirzner, *Economic Point of View*, pp. 171-172.
65. Mises, *Human Action*, p. 18.
66. Lakatos, "Falsification," p. 133.
67. *Ibid.*, p. 133, n. 4.
68. Leijonhufvud, "Schools, 'Revolutions,' " p. 79.

Postscript

From the perspective of more than six months after this chapter was written, I would do a number of things differently. Most important, I would now locate the main differentiating characteristic of Austrian economics in the positive heuristic. In particular, the chapter's emphasis on the covering-law model of explanation (especially in the notes), stressing the logical deducibility of the explanandum statement from the explanans, seems inappropriate. An important distinction between Austrian economics and neo-classical economics is that the latter insists on deterministic explanations of the covering-law variety. Austrians, on the other hand, emphasize that explanations of human actions cannot be deterministic, and thus the most for which we can hope is a nondeterministic explanation of the principle. [For a detailed examination of this view, see my as-yet-untitled forthcoming book (with Gerald P. O'Driscoll, Jr.) to be published by Basil Blackwell in 1983.]

Fortunately, however, I do not believe that the applicability of Lakatos's general framework stands or falls with the covering-law model. One may believe, for example, that a theory in conjunction with initial conditions renders a phenomenon more intelligible (for example, more likely) and not determinate, while believing that Lakatosian distinctions among the hard core, positive and negative heuristics, and so on are useful. In fact, Lakatos's MSRP is on a higher level of generalization than the covering-law model and hence has no direct relationship to it.