

## Praxeology and its critics: an appraisal

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### I. Introduction: Methodological Diversity in the Austrian Camp

The Austrian approach to methodology has never been monolithic. Two recent studies show that since Menger's time Austrians have differed, at times dramatically, in their views on methodology (White 1977; Hutchison 1981). This diversity continues to be evidenced in the writings of modern-day Austrians.

Praxeology is the dominant methodological approach in contemporary Austrian economics. The most comprehensive statement of the praxeological position is found in Ludwig von Mises' magnum opus, *Human action: a treatise on economics* (1949). However, few modern Austrians follow a purist interpretation of Mises' thought in their writings on methodology.

Murray Rothbard and Israel Kirzner come closest to being true Misesians, yet even these scholars have their differences with their former teacher. For example, Rothbard contends that the fundamental axioms of human action are "broadly empirical" (1976, 24–28). This is at variance with Mises' assertion that the postulates of his system are known to be true a priori. For his part, Israel Kirzner approaches methodological questions rather pragmatically. He is less interested in arguments about the logical status of axioms than he is in the insights that can be derived about the workings of an economy from viewing human action in individualistic, subjectivistic, and purposive terms.<sup>1</sup>

Other Austrians are even farther from Mises in their methodological views. One is struck by the paucity of references to Mises found in the chapter on methodology in Gerald O'Driscoll and Mario Rizzo's new book, *Knowledge and time: foundations of Austrian-subjectivist economics* (1984). Instead of mouthing the tenets of praxeology, O'Driscoll and Rizzo offer a reconstruction of Austrian methodology that integrates three themes: a thoroughgoing and dynamic subjectivism, the concept of a mind construct

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1. This assessment of Kirzner's position is based on conversations I had with him while I was on research leave at New York University in 1981–82.

(a theoretical device with antecedents in the works of Max Weber, Alfred Schutz, and Fritz Machlup), and an approach to institutional development that draws heavily upon ideas found in the writings of Carl Menger, Friedrich von Hayek, and Ludwig Lachmann. Lachmann's beliefs on methodology have more in common with the radical subjectivism of G. L. S. Shackle than with the apriorism of Mises. And though Hayek opposes 'scientism' with the same vehemence that Mises exhibited towards 'positivism,' he never embraces apriorism (Lachmann 1978, 1–18; Hayek 1952, 17–182).

To conclude, there exists no single Austrian position on methodology. The purpose of this article is to assess some of the criticisms that have been directed against praxeology and to offer some new ones. Although the praxeological approach is emphasized here, it should be clear that alternative formulations of what it means to 'do' Austrian economics are currently being advocated by its practitioners.

### II. Apriorism and the Fundamental Postulate of Human Action

#### The praxeological position

The fundamental postulate of human action is that all action is rational. Praxeologists assert that this postulate is known to be true with apodictic certainty; that is, it is a priori true. Mises argues that since attacks on the postulate require purposeful human action, attempts to refute it necessarily involve an inconsistency:

The scope of praxeology is the explication of the category of human action. All that is needed for the deduction of all praxeological theorems is knowledge of the essence of human action. It is a knowledge that is our own because we are men; no being of human descent that pathological conditions have not reduced to a merely vegetative existence lacks it. . . . [1949, 64].

The fundamental logical relations are not subject to proof or disproof. Every attempt to prove them must presuppose their validity. It is impossible to explain them to a being who would not possess them on his own account. . . . They are ultimate unanalyzable categories. The human mind is utterly incapable of imagining logical categories at variance with them [1949, 34].

In addition to the action postulate, examples of praxeological axioms are the categories of causality and teleology. Related theorems state that action takes place through time and that the future is uncertain (Mises 1949, 22–23, 99–105).

Some philosophies of science claim that statements that are a priori true

are equivalent to true, analytic statements. According to this view, all cognitively significant statements must be either synthetic (hence conceivably testable) or analytic and true (hence definitional, but empirically empty). This approach to the logical status of sentences originated with the logical positivists. Although it was retained in some form by later logical empiricists, more recently philosophers have questioned its validity and usefulness.<sup>2</sup> In any case, Mises rejects the analytic-synthetic distinction and adopts a neo-Kantian perspective by arguing that the axioms of praxeology are *both a priori true and empirically meaningful*:

Apriorist reasoning is purely conceptual and deductive. It cannot produce anything else but tautologies and analytic judgements. All its implications are logically derived from the premises and were already contained in them. Hence, according to a popular objection, it cannot add anything to our knowledge.

All geometrical theorems are already implied in the axioms. The concept of a rectangular triangle already implies the theorem of Pythagoras. This theorem is a tautology, its deduction results in an analytic judgement. Nonetheless nobody would contend that geometry in general and the theorem of Pythagoras in particular do not enlarge our knowledge. . . . [1949, 38].

The theorems attained by correct praxeological reasoning are not only perfectly certain and incontestable, like the correct mathematical theorems. They refer, moreover, with the full rigidity of their apodictic certainty and incontestability to the reality of action as it appears in life and history. Praxeology conveys exact and precise knowledge of real things [1949, 39].

#### *Criticisms of the praxeological position: Apriorism is unintelligible*

Among the criticisms of the a priorist approach to the status of the fundamental postulates of human action, three views are recurrent. First, "Apriorism is unintelligible." Economists who have read Mises have not always understood his position on praxeology. The Misesian assertion that all action is rational is one that certain economists have found particularly puzzling. In his Ely lecture, Lionel Robbins writes:

. . . our explanations must to some extent be *teleological*. This is not to argue with von Mises and some of his followers that we must regard human action, if not purely vegetative, as at all times *rational* in the sense that, given belief in the range of technical knowledge available to individuals or collections of individuals action must be

2. For a more thorough treatment of these developments, see Caldwell 1982, chs. 2–4.

*consistent*. I confess that I have never been able to understand this contention.<sup>3</sup>

Lord Robbins' confusion is due to his failure to recognize that Mises' definition of rationality differs significantly from definitions usually put forth by economists. For praxeologists, all action is rational because all action is *by definition* purposeful (Mises 1949, 11, 13, 19–20). Simply put, rational and action define each other; the opposite of rational behavior is not irrational behavior but "a reactive response to stimuli on the part of bodily organs and instincts which cannot be controlled by the volition of the person concerned" (p. 20).

Some might object that habitual behavior cannot be rational. But since habitual behavior still involves choice, it too is rational by definition. Is an individual whose choices diverge from his scale of relative valuations irrational? Such a situation cannot arise in the praxeological system. Because the *act* of choice *reveals* preferences, choices that are made cannot diverge from the chooser's preferences.<sup>4</sup>

If all action is rational is error impossible? The answer is no. Action takes place in time, and the future is uncertain; thus knowledge is incomplete (Mises 1949, 92–93, 99–103). In addition, the acting agent bases his actions on his subjective perceptions of reality. Though at the *time of action* the acting agent intends to improve his position, he may discover that his action did not accomplish his purpose. Thus in the praxeological system error can occur, causing the actor to alter future actions. By implication, consistency in choice (à la Robbins) is *not* the way in which rationality is defined in the Misesian system.<sup>5</sup>

One could go on and on. The point is that many of the Misesian methodological claims that may seem ludicrous initially become far less controversial once properly understood. While his definitions of rationality and action may appear strange to some, Mises is not guilty of being unintelligible. This criticism of praxeology may be rejected.

#### *Criticism: Apriorism is dogmatic*

Some economists assert that all apriorist approaches to the study of economics should be avoided because such approaches lead to dogmatism.

3. Robbins 1981, p. 2; emphasis in the original. Lord Robbins should know better. In the preface of his classic work on methodology (1932; revised edition 1935), he praises both Mises and Hayek for their contributions to the development of his thought.

4. Mises 1949, 46–47, 94–97. This position has been dubbed the 'demonstrated preference' approach. It differs from Samuelson's revealed preference approach in that the Austrians do not insist on constancy of preferences, nor are they interested in operationalizing their theory. See Rothbard 1977 for an Austrian critique of standard utility theory.

5. Hutchison 1981, 209–10, complains with some justification that Mises is less than lucid on these points. Later writers, especially Kirzner, develop and explicate the Austrian position on perception, ignorance, and error far more clearly. See Kirzner 1973 and 1979, ch. 8, for a more comprehensive treatment of these themes.

This theme emerges in a recent book by Terence Hutchison in which the methodological contributions of the Austrians are evaluated:

What needs to be emphasized is the desirability of discarding the remaining residues of the a priorist 'Pretence of Knowledge,' which came down from Wieser, Mises, and Hayek I. For claims to establish *a priori* judgements of 'apodictic certainty' or 'beyond the possibility of dispute,' together with comprehensive denunciations as 'Positivist' and 'Empiricist' of the criteria of testability and falsifiability, may serve to support infallibilist, authoritarian, and anti-libertarian attitudes and to play into the hands of the enemies of freedom.<sup>6</sup>

Followers of Mises bristle at the suggestion that his methodology might lead to dogmatism or authoritarianism. They point out that though his impassioned writings led to his virtual ostracism from the economics profession, Mises was so dedicated a disciple of liberty that he persisted in his advocacy throughout his long career.

Is Hutchison's charge an accurate one? The answer depends largely on how one defines 'dogmatism.' If dogmatism is meant to imply strongly held beliefs, the Austrians are dogmatic (but they certainly hold no monopoly on dogmatism within the profession). A more reasonable interpretation of the charge is that apriorism does not conform to the standard vision of the scientific method. True scientists subject their hypotheses to testing and never claim to have discovered hypotheses that are always and certainly true. According to this view, praxeology is unscientific and hence dogmatic.

More will be said on this topic in the next section, but it can be noted here that this criticism of Austrian methodology is problematical. To assert that there is but one scientific method, and that any analysis which does not conform to it is meaningless or dogmatic, is *itself* a dogmatic and unscientific (Hayek would term it "scientistic") attitude. Those who are skeptical of the view that traditional scientific method contains dogmatic elements should consult the writings of philosopher Paul Feyerabend (1975; 1978).

Although the abrogation of scientific freedom that dogmatism entails is a frightening prospect, it is not clear that such a problem can be avoided by following any one particular methodological approach. Any view can be held dogmatically and used in an authoritarian manner. Alternatively, the desire to protect scientific freedom implicit in the writings of Hutchison is a *value* that is meta-methodological: no particular methodology can ensure its continued existence. Like most freedoms, its perpetuation cannot be guaranteed by following some simple formula.

6. Hutchison 1981, 223. For further evidence of Hutchison's distaste for apriorism, cf. *ibid.* 218–19, 230 n. 26; 1956, 482–83. Blaug 1980, 91–93, shares Hutchison's disdain for apriorism.

*Criticism: Apriorism is unscientific*

Though an apriorist approach to the status of the first postulates of a theoretical system may be neither unintelligible nor (necessarily) dogmatic, it is different from most standard treatments of the topic. Within twentieth-century philosophy of science, logical positivists, logical empiricists, and Popperian falsificationists all believe that the analytic-synthetic division has at least some merit; that even if the dividing line is not always clear between the two, a distinction should be made between, on the one hand, definitional or tautological statements, and on the other hand, testable and potentially falsifiable statements. Though most economists are not experts in such matters, much of mainstream economic methodology finds its philosophical foundation in such doctrines.<sup>7</sup> And although Mises is overtly critical of various empiricist doctrines in his writings, his arguments must strike the modern reader as dated and unconvincing. For example, many of his anti-empiricist attacks are directed against the strawmen of classical positivism, logical positivism, behaviorism, or operationalism. His examples utilizing Euclidian geometry ignore the well-known turning point in the intellectual history of mathematics when the discovery of non-Euclidian geometry undercut the view that geometry begins from foundations which are certain and yet empirically meaningful.<sup>8</sup>

This said, it must also be noted that contemporary philosophy of science has repudiated or greatly modified all of the empiricist doctrines mentioned above. Even Carl Hempel (1958; 1959), the logical empiricist par excellence, acknowledged in the 1950s that certain components of theoretical systems (usually the axioms) might not be testable. Twenty years later Imre Lakatos could write more strongly that all scientific research programs contain untestable, metaphysical hard cores that are improper subjects for investigation or question. From another direction, the revival of realism within contemporary philosophy of science also lends support to the Misesian position. Like praxeologists, realists assert that theories make real references and deny the view that theories are nothing more than empty, albeit useful, hypothetico-deductive calculi.<sup>9</sup>

Of course, it is one thing to say that first postulates are untestable and make real references, and quite another to take the leap that Mises takes and claim that a certain set of postulates are a priori true. While the view that assumptions need not be directly testable is commonplace among philosophers today, few support an apriorist interpretation of their logical status.

Within the economics profession, even less consensus exists concerning

7. This theme is extensively developed in Caldwell 1982.

8. This is true even in Mises' more recent writings; see, e.g., his last book (1978).

9. Lakatos 1970. One of the best critical surveys of twentieth-century philosophy of science is contained in the Introduction and Afterword of Suppe 1977.

the logical status of the rationality assumption. Most economists now agree that no direct test of the assumption is possible, since subjective states are non-observable. Indirect tests involving introspection or responses to survey questions also have been ruled out. Indirect tests that utilize market data (i.e. the revealed preference approach) have been rationalized theoretically and attempted empirically: such approaches presume that the rationality postulate is conceivably, if not actually, testable. Others, following Friedman or Machlup, maintain that such testing is unnecessary. Most recently, Lawrence Boland argues that the logical form of the maximization hypothesis renders it untestable. Following Lakatos, he concludes that the postulate is not a tautology but a "metaphysical statement" that is a part of the hard core of the neoclassical research program.<sup>10</sup>

This literature provides few clear-cut answers. If one believes that the rationality assumption is testable and that the testing of assumptions is important, then by implication the Austrian approach can only be viewed as misguided. If one believes that it is unnecessary to test the assumption, or that it is an untestable metaphysical statement, then praxeology is less objectionable. I find the latter view to be more persuasive. In any test situation, a necessary condition for test results to be unambiguously interpretable is that initial conditions be independently checkable. In tests of the rationality assumption in consumer-choice theory, neither tastes and preferences nor states of information (both of which are assumed to be constant for purposes of the test) are independently checkable. In such tests, observed consistency in choice is taken as a confirmation of the assumption of rationality. But if an agent chose consistently and his tastes or information had changed, he would be irrational. Similarly, inconsistency in choice indicates either an irrational consumer or a rational one whose tastes or information had changed. In the absence of independently checkable initial conditions, neither confirming nor disconfirming test results are unambiguously interpretable.<sup>11</sup>

To return to Mises and praxeology: earlier the argument was made that an apriorist approach to the logical status of the axioms of the science of

human action is neither unintelligible nor dogmatic. Given the discussion above, it seems to be an open question whether the apriorist approach is to be considered scientifically acceptable. Future work on that question, by critics and advocates alike, may aid in its resolution. Critics of apriorism should bear in mind that it is much less clear today (as compared to thirty years ago) where the boundary lies between legitimate and illegitimate scientific activity. As such, effective criticisms of apriorism will require considerably more sophistication and subtlety than have been evidenced in the past. In particular, it is not sufficient to argue that the untestability of the action postulate renders praxeology unscientific. For their part, if praxeologists insist on retaining their apriorism, they likewise do well to try to find firmer ground than that provided by Mises on which to erect their methodological structure. One alternative open to praxeologists is a reconstruction of the Kantian synthetic/a priori category.<sup>12</sup> Another option, and one which some modern Austrians seem to find attractive, is to move away from apriorism altogether and towards a more thoroughgoing subjectivism.<sup>13</sup> Future work on these and other research paths may help resolve some of the questions left unanswered above.

### III. *Hypothesis Testing, Prediction, and Theory Choice*

#### *The standard view versus praxeology*

The notion that hypotheses are tested by comparing their predictions with the data is ubiquitous among economists. This view has its philosophical foundation in instrumentalism and in the various empiricist traditions mentioned above: logical positivism, logical empiricism, and falsificationism. Philosophers within these traditions agree that theories should be testable, that a useful means of testing is to compare the conditional predictions of a theory with the data (given that the *ceteris paribus* conditions are met), that predictive adequacy is the most important characteristic a theory can possess (though alternative criteria like simplicity, generality, or mathematical elegance may also be invoked), and that the relative ordering of theories should be determined by the strength of confirmation, or corroboration, of those being compared.<sup>14</sup> Within our profession, it is likely that a majority of economists would consider the construction of theoretical models that are capable of generating testable predictions to be the hallmark of scientific activity. Conversely, a proposed theory which is not expressed in testable (preferably, falsifiable) form is not viewed as scientific and cannot be considered a serious rival to well-established and highly confirmed theories.

12. This is attempted in Smith (mimeo).

13. This is clearly the direction taken by O'Driscoll & Rizzo 1984.

14. Suppe's 1977 survey is again useful.

10. The revealed preference approach was developed in a series of papers by Samuelson (1938, 1948, 1950). Houthakker's 1950 statement of the strong axiom of revealed preference completed the research program. Attempts to test the rationality assumption include Koo 1963; Weinstein 1968; and Koo & Hasenkamp 1972. Though Friedman 1953 and Machlup 1955 share similar conclusions, their justifications differ. Friedman is an instrumentalist, as Boland 1979 easily demonstrates. Machlup's position cannot be labeled so easily. It contains ideal-typical constructs, caution regarding the testing of theories, and a heavy dose of subjectivism. Machlup's collected methodological writings are found in his *Methodology* (1978); Boland 1981 contains his argument that the logical form of the maximization hypothesis precludes its testing.

11. I thus agree with Boland 1981 that the maximization hypothesis is untestable as currently stated. However, my argument refers to empirical problems with testing, whereas his examines the logical forms of sentences. Cf. Caldwell 1983.

Mises' view that the predictions which emerge from praxeology cannot and should not be used to test the theory directly conflicts with the standard approach. He offers three arguments.

First, the testing of praxeological theory is unnecessary, because the only way that a false conclusion (prediction) can be generated in a system in which consequences are deduced from a priori true premises is if a mistake is made in the verbal chain of logic leading from premises to conclusions. Those interested in evaluating praxeological theories should focus on the verbal chain of logic rather than on the predictions of the theory (Mises 1949, 67).

Next, praxeological theories cannot be falsified because there are no "constants" in the social world equivalent to those encountered in the natural sciences. There are regularities, but they are not derivable from universal praxeological categories (ibid. 118, 347–48).

Finally, the complexity of social phenomena rules out the testing of praxeological theory. "The experience with which the sciences of human action have to deal is always an experience of complex phenomena. No laboratory experiments can be performed with regard to human action. We are never in a position to observe the change in one element only, all other conditions of the event being equal. . ." (ibid. 31).

Praxeologists do not claim that all empirical work should be eliminated in economics. Forecasting, which occurs when trends in a body of data are extrapolated into the future, can be of great practical value. In addition, empirical work can determine the *applicability* of a given theory to a particular problem (Rothbard 1976, 20–22). But testing is not useful for the confirmation or falsification of theories.

### Assessment

The first of Mises' arguments, that one should check the "verbal chain of logic" for inconsistencies rather than test praxeological theories, is unpersuasive. Surely, one way to discover whether a mistake in reasoning has been made is to see if the chain of logic leads to predictions that are disconfirmed by evidence. The second two arguments, which state that there are no constants in the social world and that that world is complex, may present problems for testing if such testing is for the confirmation or falsification of theories. Because the issues raised here are both complicated and controversial, they will be developed in some detail.

It is perhaps best to begin by emphasizing what is *not* at issue. As mentioned above, praxeologists are not opposed to *all* empirical work and in fact recognize that in economics certain kinds of empirical work are indispensable. They presumably would agree, then, with Hutchison's recent argument that *because* of the complexity of social phenomena and the lack of constants in them, empirical trend-spotting is an important activity for economists (Hutchison 1977, ch. 2). Praxeologists differ in claiming

that theories cannot be tested for purposes of confirmation or falsification. Hutchison, like most economists, would vigorously oppose such a position. It is interesting that similar arguments have occupied philosophers of science for the past two decades.

Much effort has been directed within contemporary philosophy of science at resolving the question of theory appraisal. Neither confirmationism nor falsificationism seems capable of providing adequate grounds for the assessment of theories. Confirmationism encounters a number of problems, both philosophical and pragmatic: a variety of 'paradoxes of confirmation' have yet to be resolved; high confirmation need have no relation to the truth of an hypothesis; often, many competing theories have evidential support, yet the application of supplemental criteria of theory appraisal is itself problematical when such criteria are either difficult to define or conflict with one another. Similarly, falsificationism has problems of its own: it refuses to apply its prescriptions to itself; it ignores that most tests are 'three-cornered affairs' involving alternative theories and the data; perhaps most important, the conditions which would have to be met for a true test of a hypothesis to occur are seldom encountered, even in the physical sciences.<sup>15</sup>

Such findings have led contemporary philosophers of science into a number of different research directions. Thomas Kuhn, Imre Lakatos, and Stephen Toulmin have constructed models of historical change that still contain prescriptive elements (of varying degrees) but which emphasize that no 'algorithms of choice' are available for the assessment of theories. Kuhn argues that objective appraisal may be impossible in periods of revolutionary crisis. Lakatos states that rational theory choice may be possible only in the long run. Most radical is Paul Feyerabend, who posits an anarchistic theory of knowledge in which 'anything goes' is the only prescription. Still others, in both philosophy and the special sciences, have turned to intensive investigations of episodes in the history of particular sciences to see just how theories emerge, survive, change, and decline.<sup>16</sup> Contemporary philosophers of science, then, take a variety of approaches to this subject. And though substantial differences are evident among them, they

15. These comments regarding the current direction of philosophy of science are more thoroughly developed and defended in Caldwell 1982, chs. 4, 5, 6, 11, and 12. Advocates of falsificationism in economics will certainly challenge the views presented here. In anticipation of the impending fracas, the following observation is offered. It strikes me that the term falsificationism has been used by philosophers and economists alike to refer to many doctrines, including everything from a strictly defined set of procedural rules to a hardly objectionable emphasis on critical reasoning among scientists. For debate to be meaningful, we will have to define our terms better.

16. The classic citations are Kuhn 1970, Lakatos 1970, and Feyerabend 1975. Hausman 1981 is an example of a philosopher's investigation of a research program in economics (capital theory). For a similar exercise by an economist, see Wong 1978, in which Paul Samuelson's revealed-preference theory is brilliantly if mercilessly scrutinized.

do agree that the search for a universally applicable, objective scientific methodology is chimerical.

This literature has two implications, one critical and one positive, for praxeology and the testing of hypotheses. First, an exclusive reliance on such empiricist prescriptive methodologies as confirmationism, instrumentalism, and falsificationism is no longer reasonable. The appraisal of theories is a complex affair, involving many elements that cannot be captured in any allegedly universal methodology. If we accept this judgment from contemporary philosophy of science, then Mises' objections to confirmationism and falsificationism, which seemed so strange when compared to the old standards of acceptable scientific practice, seem less otiose.

The second implication takes the form of an imperative, and is one that is already being followed. Philosophers should examine carefully the individual disciplines to see how theory appraisal actually takes place. More than likely it will be discovered that many different types of investigations exist in science, that scientific methodology embraces a plethora of approaches for the appraisal of theories. That kind of result suggests that there are a variety of ways to investigate economic phenomena and offers prima facie support for the Austrian position.

However, some qualifications must be added. 'Anything goes' is not the position being espoused here. Those who propose alternative methodologies must construct persuasive cases showing what insights are to be gained from following them. To his credit, Mises' *Human action* may well qualify. As is shown in Section IV below, however, it too is susceptible to criticism. In any case, the criticism of proposed methodologies and the resultant strengthening or modification of positions is in this reader's eyes the most important goal of methodological work in economics.

A final, related point, before we turn to an internal criticism of praxeology: if we take contemporary philosophy of science seriously, the precise role of empirical work in economic science is an open question. It is clear that there is some role, and probably a very important one; yet neither the standard approaches nor the Misesian contribution provides adequate characterizations. Other authors, however, have offered alternatives. In a series of unappreciated papers, Hayek develops an approach to the study of 'complex phenomena' which integrates subjectivism, Popperian falsificationism, and an emphasis on explanation that may provide a framework for resolving the question of what role testing has to play in economics.<sup>17</sup> In any case, this problem warrants the attention of methodologists even if it turns out to be, like the problem of theory choice, an unsolvable one.

17. Hayek 1967, chs. 1 and 2; 1978, ch. 2. Hutchison's claim that Hayek in his later methodological writings is something akin to a closet Popperian seems to me to be completely unfounded, providing further evidence that falsificationist eyeglasses need not improve one's vision. See Hutchison 1981, 214–19.

#### IV. *Internal Criticism of Praxeology*

Most of the criticisms of praxeology reviewed above have taken the form of external criticism: praxeology was found wanting because it failed to meet certain well-accepted criteria of scientific practice. An alternative approach is internal criticism. Internal criticism proceeds by examining a given theory or methodology to see which avenues of criticism its proponents would accept as legitimate. Internal criticism approaches a theory or methodology on its own ground, rather than assessing it from the standpoint of some external standard. Praxeologists would accept two types of criticism of their position. First, one may attempt to question the truth of the axioms and postulates from which the analysis begins, and second, one may inspect the "verbal chain of logic" that leads from the postulates to the derived theorems for inconsistencies.

##### *The postulates of praxeology*

One may begin by asking, What are the basic postulates of praxeology? Clearly, the 'fundamental axiom' that all human action is rational is to be included, but what about such categories as teleology and the valuation process, cause and effect, time, and the uncertainty of the future: are they equally fundamental? And just what is meant by such terms as 'teleology' and 'uncertainty'; their definitions are far less precise than that of rationality in the Misesian system.

Next, what is the logical status of the axioms of praxeology? The action postulate is conceived as apodictically certain, its truth is known to all humans a priori. But what of the others? At least some of them (e.g. that action takes place in time, that the category of cause and effect is meaningful) seem to be statements about phenomenal reality in which the actor is located. Others seem to refer to characteristics of the actor himself (e.g. that a valuation process occurs and is based on subjective perceptions without full knowledge of the future). Are these categories also apodictically certain, or, as Rothbard suggests (1976, 24–30), are they "broadly empirical"?

Such terms as 'a priori' and 'broadly empirical' may themselves be problematical. Philosophers have defined 'a priori' in a variety of ways, and its meaning is not always clear in the Misesian system. Does it mean prior to all experience, definitional, analytic, tautological?<sup>18</sup> The term 'broadly empirical' is equally ambiguous. Presumably, a statement that is broadly empirical refers to phenomenal reality, but is untestable. However, the statement 'unicorns exist' has the same characteristics. Praxeologists might respond that broadly empirical refers to widely held human beliefs concerning phenomenal reality. But this will not do: religious and cultural

18. For an even more exhaustive typology, see Mittelstrass 1977, 113–28.

differences provide just two examples of forces which allow great diversity in the way that humans perceive the world in which they live.

Most of the comments above concern definitional issues. More fundamental is the question of the truth of the postulates of praxeology. The fundamental axiom is that all human action is purposeful or rational. Do examples of non-purposeful action (excluding Mises' "vegetative man") exist? Operantly conditioned behavior may be a plausible candidate. An uninformed observer would interpret such behavior as purposeful, but because it is conditioned behavior it is in fact non-volitional and hence non-purposeful.<sup>19</sup>

No one claims that operant behavior is a commonly encountered phenomenon. It would not be considered a counterexample if praxeologists claimed only that *most* human action is goal-directed. It is because they claim that *all* human action is necessarily purposeful that such a counterexample must be taken seriously. This illustrates the dangers that an apriorist approach to the status of axioms can encounter.

Finally, do other systems of thought based on a priori true first postulates exist? And if so, how does one adjudicate among them? One potential competitor is the "Classical-Marxian" system propounded by Martin Hollis and E. J. Nell in their book *Rational economic man* (1975).

Hollis and Nell chose as their "fundamental concept" an extended version of production, by which they mean "reproduction of the economic system." They note that there exist many candidates for the basic concept. In good rationalist style, they contend further that there can be only one set of *fundamental* axioms: "necessary truths cannot conflict; alternative theories, that is, theories with incompatible implications are not allowable, and even complementary theories must be fit together and made to cohere. We cannot allow the possibility of different fundamental concepts, for different concepts will give rise to different theories, as different as Robbins and Marx" (1975, 243). What reasons do they offer for considering production as essential?

The general point is simple. Choice depends on choosers, exchange upon traders, labor upon workers, and so on. Choosers need reasons and abilities, traders must have good skills, workers jobs and skills. Hence the agents in question, and their replacements when they grow old or ill, when they die or retire, must be trained and supported, as must the context in which the agents characteristically operate. The reproduction of the system, in short, is primary [ibid.].

How can the objective observer assess the competing claims of Mises and his Classical-Marxian counterparts? Neither side will concede that

19. Nozick 1977 develops the operant-conditioning counterexample.

empirical tests of assumptions or implications can yield worthwhile results. Indeed, their complaints against 'positivism' and 'empiricism' are nearly identical. All we are left with are two weak suggestions, one by each camp. Mises recommends that critics check the "verbal chain of logic" from axioms to conclusions for inconsistencies, and Hollis and Nell suggest that opponents establish that the definitions of the Classical-Marxian system are "unrealistic."

The dilemma of adjudicating between these two competing a priori systems is a generalizable one. Even if all verbal or mathematical deductions are made correctly, the starting point of such systems (unprovable, necessary truths about reality) will always *seem* capable, psychologically if not actually, of multiplication. As long as proponents of such systems eschew any resort to empirical testing or other forms of criticism, there appears to be no way to compare a (possibly) ever growing number of such systems.<sup>20</sup>

#### *The "verbal chain of logic"*

Again definitional issues may be taken up first. Though the phrase "verbal chain of logic" both has aesthetic appeal and carries positive connotations, one is hard-pressed to know what it means. The waters are muddied further when one inquires just what sorts of mistakes in logic are to count against the praxeological system. Is it solely a matter of the validity of argument forms, or are other mistakes possible?

Praxeologists claim that the verbal chain of logic allows the derivation of certain theorems from the postulates of the system. Two such derived theorems which do seem to follow (though they differ in form from similar concepts in standard theory) are the 'law of marginal utility' and the 'law of returns.' But what about some of the more sweeping conclusions about the relative beneficence of markets, or even their stability (in the market process sense)? Certain radical subjectivists (e.g. Shackle and Lachmann) suggest that if one truly accepts a thoroughgoing subjectivist approach, one may also be forced into agnosticism regarding the outcome of the market process in any given case.<sup>21</sup> But if this is true, then the Austrian predilection for markets may constitute a mistake in verbal logic.

Finally, the addition of subsidiary, empirical hypotheses is necessary for the praxeological system to be applied to the 'real world.' One of these is the hypothesis of the disutility of labor for all humans, with the exception of the 'creative genius' (Mises 1949, 131-40). The polemical advantages of such a hypothesis are evident, for it permits the criticism of the notion that under full communism all work is joyously entered into. But a fuller justification of this and similar hypotheses is necessary.

20. At least the Austrians have attempted to respond to Hollis and Nell; see Lavoie 1977. This well-argued piece has elicited no response from the Classical-Marxian competitors to praxeology.

21. Shackle 1972; Lachmann 1978, 1-18; Kirzner 1976, 40-51.

The questions posed above do not exhaust the range of possible criticism of praxeology. One could, for example, focus on the *Wertfreiheit* claims of the Austrians. Or, following Nozick, one might inquire whether methodological individualism, embraced by praxeologists, entails reductionism.<sup>22</sup> But then, it was not my intention to launch a comprehensive assault on praxeology. The point of all of this is simply to suggest that a critique of Misesian methodology which adheres to the categories and methods employed by praxeologists themselves is possible and should be attempted.

### V. Conclusion

The purpose of this article is to critically examine a variety of arguments made by both opponents and proponents of praxeology. Critical rationalism, a position espoused by Popper, insists that both novelty and criticism are desiderata in scientific reasoning. There is no question that praxeology is a novel methodological approach. In addition, it is a somewhat well-developed one. As such, it deserves a fair hearing from members of the economics profession. Unfortunately, critics of praxeology have too often dismissed it after mouthing a few phrases of a rather dated, albeit scientific-sounding, methodological rhetoric. By assessing the arguments of these critics (some of which do demand a response from praxeologists) and by advancing some new arguments, hopefully this essay has advanced the process of criticism.

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22. Nozick 1977, 354, raises the latter question but does not discuss it. Kirzner 1976, 76–88, defends the Austrian claim of *Wertfreiheit*.

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