

true, then the logical consequences built upon it must be empirically true as well. But this is not the sort of empiricism welcomed by the positivists, for it is based on universal reflective or inner experience, as well as on external physical experience. Thus, the knowledge that human beings have goals and act purposively to attain them rests, not simply on observing that human beings exist, but also on the introspective knowledge of what it means to be human possessed by each man, who then assents to this knowledge. While this sort of empiricism rests on broad knowledge of human action, it is also prior to the complex historical events that economists attempt to explain.

Alfred Schütz pointed out and elaborated the complexity of the interaction between the individual and other persons, the “interpretive understanding” or *Verstehen*, upon which this universal, prescientific understanding rests. The common-sense knowledge of the universality of motivated, intentional human action, ignored by positivists as “unscientific,” actually provides the indispensable groundwork on which science itself must develop.³ For Schütz this knowledge is empirical, “provided that we do not restrict this term to sensory perceptions of objects and events in the outer world but include the experimental form, by which common-sense thinking in everyday life understands human actions and their outcome in terms of their underlying motives and goals.”⁴

³ It is . . . not understandable that the same authors who are convinced that no verification is possible for the intelligence of other human beings have such confidence in the principle of verifiability itself, which can be realized only through cooperation with others by mutual control. (Alfred Schütz, *Collected Papers*, vol. 2: *Studies in Social Theory*, A. Brodersen, ed. [The Hague: Nijhoff, 1964], p. 4)

⁴ Alfred Schütz, *Collected Papers*, vol. 1: *The Problem of Social Reality*, Maurice Natanson, ed. (The Hague: Nijhoff, 1962), p. 65; see also pp. 1–66, as well as Peter Winch, “Philosophical Bearings,” in *Philosophy of the Social Sciences: A Reader*, Maurice Natanson, ed. (New York: Random House, 1963). On the importance of the commonsense, prescientific presuppositions of science from a slightly different philosophical perspective, see Van Melsen, *Philosophy of Nature*, pp. 6–29.

The nature of the evidence on which the praxeological axiom rests is, moreover, fundamentally similar to that accepted by the self-proclaimed empiricists. To them, the laboratory experiment is evidence because the sensory experience involved in it is available to each observer; the experience becomes “evident” to all. Logical proof is in this sense similar; for the knowledge that B follows from A becomes evident to all who care to follow the demonstration. In the same way, the fact of human action and of purposive choice also becomes evident to each person who bothers to contemplate it; it is just as evident as the direct sense experience of the laboratory.

From this philosophical perspective, then, all disciplines dealing with human beings—from philosophy to history, psychology, and the social sciences—must take as their starting point the fact that humans engage in motivated, purposive action and are thus different from the unmotivated atoms and stones that are the objects of the physical sciences. But where, then, does praxeology or economics differ from the other disciplines that treat human beings? The difference is that, to the praxeologist, economic *theory* (as distinct from applied economics, which will be treated below) deals, not with the content of human valuations, motivations, and choices, but with the formal fact that people engage in such motivated action. Other disciplines focus on the content of these values and actions. Thus, psychology asks how and why people adopt values and make choices; ethics deals with the problem of what values and choices they should adopt; technology explains how they should act in order to arrive at chosen ends; and history tries to explain the content of human motives and choices through recorded time. Of these disciplines, history is perhaps the most purely *verstehende*, for the historian is constantly attempting to describe, understand, and explain the motivations and choices of individual actors. Economic theory, on the other hand, is the least *verstehende*, for while it too begins with the axiom of purposive and intentional human action, the remainder of its elaborated structure consists of the deduced logical—and therefore true—implications of that primordial fact.

An example of the formal structure of economic theory is the well-known economic law, built up from the axiom of the existence of motivated human action, that if the demand for any product increases, given the existing supply, the price of that product will rise. This law holds regardless of the ethical or aesthetic status of the

product, just as the law of gravity applies to objects regardless of their particular identity. The economic theorist is not interested in the content of what is being demanded, or in its ethical meaning—it may be guns or butter or even textbooks on philosophy. It is this universal, formal nature of economic law that has earned it among laymen the reputation of being cold, heartless, and excessively logical.

Having discussed the nature of the axiom on which the praxeological view of economics is grounded, we may now turn to examine the deductive process itself, the way in which the structure of economic laws is developed, the nature of those laws, and, finally, the ways in which the praxeological economist applies these economic laws to the social world.

One of the basic tools for the deduction of the logical implications of the axiom of human action is the use of the *Gedankenexperiment*, or “mental experiment.” The *Gedankenexperiment* is the economic theorist’s substitute for the natural scientist’s controlled laboratory experiment. Since the relevant variables of the social world cannot actually be held constant, the economist holds them constant in his imagination. Using the tool of verbal logic, he mentally investigates the causal influence of one variable on another. The economist finds, for example, that the price of a product is determined by two variables, the demand for it and its supply at any given time. He then mentally holds the supply constant, and finds that an increase in demand—brought about by higher rankings of the product on the value scales of the public—will bring about an increase in price. Similarly, he finds, again using verbal deductive logic, that if these value scales, and therefore public demand, are mentally held constant, and the supply of the product increases, its price will fall. In short, economics arrives at *ceteris paribus* laws: Given the supply, the price will change in the same direction as demand; *given* the demand, price will change in the opposite direction from supply.

One important aspect of these economic laws must be pointed out: they are necessarily *qualitative*. The fact that human beings have goals and preferences, that they make choices to attain their goals, that all action must take place over time, all these are qualitative axioms. And since only the qualitative enters into the logical process from the real world, only the qualitative can emerge. One can only say, for example, that an increase in demand, given the supply, will raise the price; one *cannot* say that a 20 percent increase in demand

will bring about a 25 percent increase in price. The praxeologist must reject all attempts, no matter how fashionable, to erect a theory consisting of alleged quantitative laws. In an age that tries desperately to imitate prestigious physics, with its emphasis on mathematics and its quantitative laws, many social scientists, including many economists, have ignored the praxeological method because of this very insistence on the qualitative bounds of the discipline.

There is a basic reason for the quantitative—qualitative dichotomy between the physical and the social sciences. The objects of physical science do not act; they do not choose, change their minds, and choose again. Their natures may therefore be investigated, and the investigations replicated indefinitely, with quantitative precision. But people do change their minds, and their actions, all the time; their behavior cannot be predicted with exact and therefore scientific precision. Among the many factors helping to determine the demand and the supply of butter, for example, are the valuations placed by each consumer on butter relative to all other products available, the availability of substitutes, the climate in the butter-producing areas, technological methods of producing butter (and margarine), the price of cattle feed, the supply of money in the country, the existence of prosperity or recession in the economy, and the public's expectations of the trend of general prices. Every one of these factors is subject to continuing and unpredictable change. Even if one mammoth equation could be discovered to "explain" all recorded prices of butter for the past 50 years, there is no guarantee, and not even the likelihood, that the equation would have anything to do with *next* month's price.

In fact, if empirical success is the test, it is surely noteworthy that all the determined efforts of quantitative economists, econometricians, and social scientists have not been able to find one single quantitative constant in human affairs. The mathematical laws in the physical sciences contain numerous constants; but the imitative method in the social sciences is proven vain by the fact that not a single constant has ever emerged. Moreover, despite the use of sophisticated econometric models and high-speed computers, the success rate of forecasting economic quantities has been dismal, even for the simplest of aggregates such as Gross National Product, let alone for more difficult quantities; the record of GNP forecasting by economists has been poorer than a simple layman's extrapolation of

recent trends.⁵ In fact, the federal government has had notably poor success even in forecasting the one variable under its own absolute control—its *own* expenditure in the near future. Perhaps we will revise our critical opinion of econometric science if and when the econometricians prove themselves able to make flawless predictions of activity on the stock market—and make themselves vast fortunes in the process.

Except for the fact that they are not quantitative, however, the predictions of the praxeologist are precisely the same kind as those of the natural scientist. The latter, after all, is not a prophet or sooth-sayer; his successful prediction is not what *will* happen in the world, but what *would* happen if such and such should occur. The scientist can predict successfully that if hydrogen and oxygen are combined in proportions of two to one, the result will be water; but he has no way of predicting scientifically how many scientists in how many laboratories will perform this process at any given period in the future. In the same way, the praxeologist can say, with absolute certainty, that if the demand for butter increases, and the supply remains the same, the price of butter will rise; but he does not know whether the public's demand for butter will in fact rise or fall, let alone by how much it will change. Like the physical scientist, the economist is not a prophet, and it is unfortunate that the econometricians and quantitative economists should have so eagerly assumed this social role.⁶

⁵See Victor Zamowitz, *An Appraisal of Short-Term Economic Forecasts* (New York: National Bureau of Economic Research, 1967). For a record of the problems of forecasting see "Bad Year for Econometrics," *Business Week* (December 20, 1969): 36–40.

⁶The English economist Peter T. Bauer properly distinguishes between scientific prediction and forecasting:

Prediction, in the sense of the assessment of the results of specified occurrences or conditions, must be distinguished from the forecasting of future events. Even if the prediction that the producers of a particular crop respond to a higher price by producing more is correct, this prediction does not enable us to forecast accurately next year's output (still less the harvest in the more distant future), which in the event will be affected by many factors besides changes in price. (Peter T. Bauer, *Economic Analysis and Policy in Underdeveloped Countries* [Durham, N.C.: Duke University Press, 1957], pp. 10–11; see also pp. 28–32)

The English economist John Jewkes suggests the properly limited role for economic forecasting, as well as for applied economics generally:

I submit that economists cannot, without stepping outside their discipline, predict in the sense of telling us what will happen in the future. . . .

In the most general sense, there is, indeed, no such thing as the *economic* future. There is only *the* future in which economic factors are bound together, inextricably and quite without hope of separate identification, with the whole universe of forces determining the course of events. . . . Anyone who proposes to look at it [the future] before the event must take as his province the whole of experience and knowledge. He must cease to behave as a specialist, which means that he must cease to behave as an economist. . . .

The economist's claim to predictive authority must be false in that it leads to a palpable absurdity. If the economic future can, indeed, be described, why not also the scientific future, the political future, the social future, the future in each and every sense? Why should we not be able to plumb all the mysteries of future time?⁷

What, then, is the praxeological view of the function of applied economics? The praxeologist contrasts, on the one hand, the body of qualitative, nomothetic laws developed by economic theory, and on the other, a myriad of unique, complex historical facts of both the past and the future. It is ironic that while the praxeologist is generally denounced by the positivist as an "extreme apriorist," he actually has a far more empirical attitude toward the facts of history. For the positivist is always attempting to compress complex historical facts into artificial molds, regarding them as homogeneous and therefore manipulable and predictable by mechanical, statistical, and quantitative operations in the attempt to find leads, lags, correlations, econometric relations, and "laws of history." This Procrustean distortion is undertaken in the belief that the events of human history can be treated in the same mechanistic way as the movements of atoms or molecules—

⁷John Jewkes, "The Economist and Economic Change," in *Economics and Public Policy* (Washington, D.C.: Brookings Institution, 1955), pp. 82–83.

simple, unmotivated, homogeneous elements. The positivist thereby ignores the fact that while atoms and stones have no history, man, by virtue of his acts of conscious choice, creates a history. The praxeologist, in contrast, holds that each historical event is the highly complex result of a large number of causal forces, and, further, that it is unique and cannot be considered homogeneous to any other event. Obviously, there are similarities between events, but there is no perfect homogeneity and therefore no room for historical "laws" similar to the exact laws of physical science.

While accepting that there are no mechanical laws of history, however, the praxeologist holds that he can and must use his knowledge of other nomothetic sciences as part of his *verstehende* attempt to understand and explain the idiographic events of history. Let us suppose that the economic historian, or the student of applied economics, is attempting to explain a rapid rise in the price of wheat in a certain country during a certain period. He may bring many nomothetic sciences to bear: agronomy and entomology may help reveal that an insect mentioned in the historical record was responsible for a drastic fall in wheat production; meteorological records may show that rainfall was insufficient; he may discover that during the periods people's taste for bread increased, perhaps imitating a similar preference by the king; he may discover that the money supply was increasing, and learn from economic theory that an increase in the supply of money tends to raise prices in general, including therefore the price of wheat. And, finally, economic theory states that the price of wheat moves inversely with the supply and directly with the demand. The economic historian combines all of his scientific knowledge with his understanding of motives and choices to attempt to explain the complex historical phenomenon of the price of bread.

A similar procedure is followed in the study of such infinitely more complex historical problems as the causes of the French Revolution, where, again, the historian must blend his knowledge of causal theories in economics, military strategy, psychology, technology, and so on, with his understanding of the motives and choices of individual actors. While historians may well agree on the enumeration of all the relevant causal factors in the problem, they will differ on the weight to be attached to each factor. The evaluation of the relative importance of historical factors is an art, not a science, a matter of personal judgment, experience, and *verstehende* insight

which will differ from one historian to another. In this sense, economic historians, like economists (and indeed other historians), can come to qualitative but not quantitative agreement.

For the praxeologist, forecasting is a task very similar to the work of the historian. The latter attempts to “predict” the events of the past by explaining their antecedent causes; similarly, the forecaster attempts to predict the events of the future on the basis of present and past events already known. He uses all his nomothetic knowledge, economic, political, military, psychological, and technological; but at best his work is an art rather than an exact science. Thus, some forecasters will inevitably be better than others, and the superior forecasters will make the more successful entrepreneurs, speculators, generals, and bettors on elections or football games.

The economic forecaster, as Professor Jewkes pointed out, is only looking at part of a tangled and complex social whole. To return to our original example, when he attempts to forecast the price of butter he must take into consideration the qualitative economic law that price depends directly on demand and inversely on supply; it is then up to him, using knowledge and insight into general economic conditions as well as the specific economic, technological, political, and climatological conditions of the butter market, as well as the values people are likely to place on butter, to try to forecast the movements of the supply and demand of butter, and therefore its price, as accurately as possible. At best, he will have nothing like a perfect score, for he will run aground on the fact of free will altering values and choices, and the consequent impossibility of making exact predictions of the future.⁸

⁸We may mention here the well-known refutation of the notion of predicting the future by Karl Popper, namely, that in order to predict the future, we would have to predict what knowledge we will possess in the future. But we cannot do so, for if we knew what our future knowledge would be, we would *already* be in possession of that knowledge at the present time. See Karl R. Popper, *The Poverty of Historicism* (New York: Harper and Row, 1964), pp. vi–viii.

THE PRAXEOLOGICAL TRADITION

The praxeological tradition has a long history in economic thought. We will indicate briefly the outstanding figures in the development of that tradition, especially since these economic methodologists and their views have been recently neglected by economists steeped in the positivist world view.

One of the first self-conscious methodologists in the history of economics was the early-nineteenth-century French economist Jean-Baptiste Say. In the lengthy introduction to his *magnum opus*, *A Treatise on Political Economy*, Say laments that people

are too apt to suppose that absolute truth is confined to the mathematics and to the results of careful observation and experiment in the physical sciences; imagining that the moral and political sciences contain no invariable facts of indisputable truth, and therefore cannot be considered as genuine sciences, but merely hypothetical systems.

Say could easily have been referring to the positivists of our day, whose methodology prevents them from recognizing that absolute truths can be arrived at in the social sciences, when grounded, as they are in praxeology, on broadly evident axioms. Say insists that the “general facts” underlying what he calls the “moral sciences” are undisputed and grounded on universal observation.

Hence the advantage enjoyed by every one who, from distinct and accurate observation, can establish the existence of these general facts, demonstrate their connection, and deduce their consequences. They as certainly proceed from the nature of things as the laws of the material world. We do not imagine them; they are results disclosed to us by judicious observation and analysis. . . . That can be admitted by every reflecting mind.

These general facts, according to Say, are “principles,” and the science of

political economy, in the same manner as the exact sciences, is composed of a few fundamental principles, and of a great number of corollaries or conclusions drawn from these principles. It is essential, therefore, for the advancement of this science that these principles should be strictly deduced from observation; the number of conclusions to be drawn from them may afterwards be either

multiplied or diminished at the discretion of the inquirer, according to the object he proposes.⁹

Here Say has set forth another important point of the praxeological method: that the paths in which the economist works out the implications of the axioms and the elaborated system which results will be decided by his own interests and by the kind of historical facts he is examining. Thus, it is theoretically possible to deduce the theory of money even in an economy of primitive barter, where no money exists; but it is doubtful whether a primitive praxeologist would have bothered to do so.

Interestingly enough, Say at that early date saw the rise of the statistical and mathematical methods, and rebutted them from what can be described as a praxeological point of view. The difference between political economy and statistics is precisely the difference between political economy (or economic theory) and history. The former is based with certainty on universally observed and acknowledged general principles; therefore, "a perfect knowledge of the principles of political economy may be obtained, inasmuch as all the general facts which compose this science may be discovered." Upon these "undeniable general facts," "rigorous deductions" are built, and to that extent political economy "rests upon an immovable foundation." Statistics, on the other hand, only records the ever changing pattern of particular facts, statistics "like history, being a recital of facts, more or less uncertain and necessarily incomplete." Furthermore, Say anticipated the praxeologist's view of historical and statistical data as themselves complex facts needing to be explained. "The study of statistics may gratify curiosity, but it can never be productive of advantage when it does not indicate the origin and consequences of the facts it has collected; and by indicating their origin and consequences, it at once becomes the science of political economy." Elsewhere in the essay, Say scoffs at the gullibility of the public toward statistics: "Sometimes, moreover, a display of figures and calculations imposes upon them; as if numerical calculations alone could prove anything, and as if any rule could be laid down, from

⁹Jean-Baptiste Say, *A Treatise on Political Economy*, C.C. Biddle, trans. (New York: Augustus M. Kelley, 1964), pp. xxiv, xxv, xlv, xxvi.

which an inference could be drawn without the aid of sound reasoning.”¹⁰

Say goes on to question sharply the value of mathematics in the construction of economic theory, once again referring back to the structure of the basic axioms, or general principles, for his argument. For political economy is concerned with men’s values, and these values being “subject to the influence of the faculties, the wants and the desires of mankind, they are not susceptible of any rigorous appreciation, and cannot therefore furnish any data for absolute calculations. In political science, all that is essential is a knowledge of the connection between causes and their consequences.” Delving deeper into the then only embryonic use of the mathematical method of economics, Say points out that the laws of economics are strictly qualitative: “We may, for example, know that for any given year the price of wine will infallibly depend upon the quantity to be sold, compared with the extent of the demand.” But “if we are desirous of submitting these two data to mathematical calculation,” then it becomes impossible to arrive at precise quantitative forecasts of the innumerable, ever changing forces at work: the climate, the quantity of the harvest, the quality of the product, the stock of wine held over from the previous vintage, the amount of capital, the possibilities of export, the supply of substitute beverages, and the changeable tastes and values of the consumers.”¹¹

Say offers a highly perceptive insight into the nature and probable consequences of the application of mathematics to economics. He argues that the mathematical method, with its seeming exactitude, can only gravely distort the analysis of qualitative human action by stretching and oversimplifying the legitimate insights of economic principles:

Such persons as have pretended to do it, have not been able to enunciate these questions into analytical language, without divesting them of their natural complication, by means of simplifications, and arbitrary suppressions, of which the consequences, not properly estimated, always essentially change the condition of the problem, and pervert all its results; so that no other inference can be

¹⁰Ibid., pp. xix–xx, li.

¹¹Ibid., pp. xxvi, xxviii.

deduced from such calculations than from formula arbitrarily assumed.¹²

In contrast to the physical sciences where the explanatory laws or general principles are always in the realm of the hypothetical, in praxeology it is fatal to introduce oversimplification and falsehood into the premises, for then the conclusions deduced from them will be irredeemably faulty as well.¹³

If mathematics and statistics do not provide the proper method for the political economist, what method is appropriate? The same course that he would pursue in his daily life. "He will examine the immediate elements of the proposed problem, and after having ascertained them with certainty . . . will approximately value their mutual influences with the intuitive quickness of an enlightened understanding."¹⁴ In short, the laws of the political economist are certain, but their blending and application to any given historical event is accomplished, not by pseudo-quantitative or mathematical methods, which distort and oversimplify, but only by the use of *Verstehen*, "the intuitive quickness of an enlightened understanding."

The first economists to devote their attention specifically to methodology were three leading economists of mid-nineteenth century Britain: John E. Cairnes, Nassau W. Senior, and John Stuart Mill. Cairnes and Senior, at least, may be considered as proto-praxe-

¹²Ibid., p. xxvin.

¹³One of the most pernicious aspects of the current dominance of positivist methodology in economics has been precisely this injection of false premises into economic theory. The leading extreme positivist in economics, Milton Friedman, goes so far as to extol the use of admittedly false premises in the theory, since, according to Friedman, the *only* test of a theory is whether it predicts successfully. See Milton Friedman "The Methodology of Positive Economics," in *Essays in Positive Economics* (Chicago: University of Chicago Press, 1953), pp. 3–46. Of the numerous critiques and discussions of the Friedman thesis, see in particular Eugene Rotwein, "On The Methodology of Positive Economics," *Quarterly Journal of Economics* 73 (November 1959): 554–75; Paul A. Samuelson, "Discussion," *American Economic Review: Papers and Proceedings* 53 (May 1963): 231–36; Jack Maltz, "Friedman and Machlup on the Significance of Testing Economic Assumptions," *Journal of Political Economy* 73 (February 1965): 37–60.

¹⁴Say, *Treatise on Political Economy*, p. xxvin.

ologists. Cairnes, after agreeing with Mill that there can be no controlled experiments in the social sciences, adds that they have, however, a crucial advantage over the physical sciences. For, in the latter,

mankind have no direct knowledge of ultimate physical principles. The law of gravitation and the laws of motion are among the best established and most certain of such principles; but what is the evidence on which they rest? We do not find them in our consciousness, by reflecting on what passes in our minds; nor can they be made apparent to our sense the proof of all such laws ultimately resolving itself into this, that, assuming them to exist, they account for the phenomena.

In contrast, however,

The economist starts with a knowledge of ultimate causes. He is already, at the outset of his enterprise, in the position which the physicist only attains after ages of laborious research. If any one doubt this, he has only to consider what the ultimate principles governing economic phenomena are . . . certain mental feelings and certain animal propensities in human beings; [and] the physical conditions under which production takes place. . . . For the discovery of such premises no elaborate process of induction is needed . . . for this reason, that we have, or may have if we choose to turn our attention to the subject, direct knowledge of these causes in our consciousness of what passes in our own minds, and in the information which our senses convey . . . to us of external facts. Every one who embarks in any industrial pursuit is conscious of the motives which actuate him in doing so. He knows that he does so from a desire, for whatever purpose, to possess himself of wealth; he knows that, according to his lights, he will proceed toward his end in the shortest way open to him.¹⁵

¹⁵J.E. Cairnes, *The Character and Logical Method of Political Economy*, 2nd ed. (London: Macmillan, [1857] 1875, repr. 1888), pp. 83, 87–88 (italics in the original). The emphasis of Cairnes and other classical economists on wealth as the goal of economic action has been modified by later praxeological economists to include all manner of psychological satisfactions, of which those stemming from material wealth are only a subset. A discussion similar to that of Cairnes can be found in F.A. Hayek, “The Nature and History of the Problem,” in *Collectivist Economic Planning*, F.A. Hayek, ed. (London: Routledge, 1935), pp. 10–11.

Cairnes goes on to point out that the economist uses the mental experiment as a replacement for the laboratory experiment of the physical scientist.

Cairnes demonstrates that deduced economic laws are “tendency,” or “if-then,” laws, and, moreover, that they are necessarily qualitative, and cannot admit of mathematical or quantitative expression. Thus, he too makes the point that it is impossible to determine precisely how much the price of wheat will rise in response to a drop in supply; for one thing, “it is evident that the disposition of people to sacrifice one kind of gratification to another—to sacrifice vanity to comfort, or decency to hunger—is not susceptible of precise measurement.”¹⁶ In the preface to his second edition, two decades later in 1875, Cairnes reiterated his opposition to the growing application of the mathematical method to economics, which, in contrast to its use in the physical sciences, cannot produce new truths; “and unless it can be shown either that mental feelings admit of being expressed in precise quantitative forms, or, on the other hand, that economic phenomena do not depend upon mental feelings, I am unable to see how this conclusion can be avoided.”¹⁷

Cairnes’s older contemporary, Nassau Senior, was the most important praxeologist of that era. Before Senior, classical economists such as John Stuart Mill had placed the fundamental premises of economics on the shaky ground of being *hypotheses*; the major hypothesis was that all men act to obtain the maximum of material wealth. Since this is clearly not always true, Mill had to concede that economics was only a hypothetical and approximate science. Senior broadened the fundamental premise to include immaterial wealth or satisfaction, a complete, apodictic, and universally true principle based on insight into the goal-seeking nature of human action.

In stating that every man desires to obtain additional wealth with as little sacrifice as possible, we must not be supposed to mean that everybody . . . wishes for an indefinite quantity of everything . . . What we mean to state is that no person feels his whole wants to be adequately supplied; that every person has some unsatisfied desires which he believes that additional wealth would gratify. The

¹⁶Cairnes, *Character and Logical Method*, p. 127.

¹⁷*Ibid.*, p. v.

nature and urgency of each individual's wants are as various as the differences in individual character.¹⁸

In contrast to the physical sciences, Senior pointed out, economics and the other "mental sciences" draw their premises from the universal facts of human consciousness:

The physical sciences, being only secondarily conversant with mind, draw their premises almost exclusively from observation or hypothesis. Those which treat only of magnitude or number, . . . the pure sciences, draw them altogether from hypothesis. . . . They disregard almost entirely the phenomenon of consciousness. . . .

On the other hand, the mental sciences and the mental arts thaw their premises principally from consciousness. The subjects with which they are chiefly conversant are the workings of the human mind.¹⁹

These latter premises are "a very few general propositions, which are the result of observation, or consciousness, and which almost every man, as soon as he hears them, admits, as familiar to his thought, or at least, as included in his previous knowledge."²⁰

During the 1870s and 1880s, classical economics was supplanted by the Neoclassical School. In this period the praxeological method was carried on and further developed by the Austrian School, founded by Carl Menger of the University of Vienna and continued by his two most eminent disciples, Eugen von Böhm-Bawerk and Friedrich von Wieser. It was on the basis of their work that Böhm-Bawerk's student Ludwig von Mises later founded praxeology as a self-conscious and articulated methodology.²¹ As it was outside the

¹⁸Nassau William Senior, *An Outline of the Science of Political Economy* (New York: Augustus M. Kelley, [1836] n.d.), p. 27.

¹⁹Marian Rowley, *Nassau Senior and Classical Economics* (New York: Augustus M. Kelley, 1949), p. 56.

²⁰*Ibid.*, p. 43. See also p. 64, where Rowley points out the similarity between Senior's methodological views and the praxeology of Ludwig von Mises.

²¹The outstanding example is Mises, *Human Action*. See also his *Theory and History* (New Haven, Conn.: Yale University Press, 1957); *The Ultimate Foundation of Economic Science* (Kansas City: Sheed Andrews and McMeel, 1978); and *Epistemological Problems of Economics* (Princeton, N.J.:

increasingly popular intellectual fashion of positivism and mathematics, however, the Austrian School has been greatly neglected in recent years and dismissed as an unsound approximation of the positivist-mathematical theory of the Lausanne School, founded by Léon Walras of Lausanne and continued by the Italian economist and sociologist Vilfredo Pareto.

A few followers or sympathetic observers, however, have carried on investigations into the methodology of the early Austrian School. Leland B. Yeager notes what we now see as the typically praxeological view of the unique advantage of economic theory over the physical sciences: "While the basic elements of theoretical interpretation in the natural sciences, such, he [Menger] says, as forces and atoms, cannot be observed directly, the elements of explanation in economics—human individuals and their strivings—are of a direct empirical nature." Furthermore, "The facts that economists induce from the behavior of themselves and other people serve as axioms from which a useful body of economic theory can be logically deduced, much as in geometry an impressive body of theorems can be deduced from a few axioms." In short, "Menger conceived of economic theory as a body of deductions from basic principles having a strong empirical foundation." Referring to the dominant positivist economists of our own day, Yeager adds perceptively,

Not sharing . . . Menger's understanding of how empirical content gets into so-called "armchair theory," many economists of our own day apparently regard theoretical and empirical work as two distinct fields. Manipulation of arbitrarily-assumed functional relationships is justified in the minds of such economists by the idea that empirical testing of theories against the real world comes afterward.²²

D. Van Nostrand, 1960). See also F.A. Hayek, *The Counter-Revolution of Science* (Glencoe, Ill.: The Free Press, 1955); Lionel Robbins, *An Essay on the Nature and Significance of Economic Science*, 2nd ed. (London: Macmillan, 1949); and Israel M. Kirzner, *The Economic Point of View*, 2nd ed. (Kansas City: Sheed Andrews and McMeel, 1976).

²²Leland B. Yeager, "The Methodology of Henry George and Carl Menger," *American Journal of Economics and Sociology* 13 (April 1954): 235, 238.

Other writers have discovered links between the Austrian method and various strands of the *philosophia perennis*. Thus, Emil Kauder finds a close relationship between this method and Aristotelian philosophy, which was still influential in Austria at the end of the nineteenth century. Kauder points out that all the Austrians were “social ontologists,” and that as such they believed in a structure of reality “both as a logical starting point and as a criterion of validity.” He notes Mises’s statement that economic laws are “ontological facts,” and he characterizes as both ontological and Aristotelian the concern of Menger and his followers to uncover the “essences” of phenomena, rather than to treat superficial and complex economic quantities. Kauder also points out that for Menger and the Austrians, economic theory deals with types and typical relations, which provide knowledge that transcends the immediate, concrete case and is valid for all times and places. Concrete historical cases are thus the Aristotelian “matter” which contains potentialities, while the laws and types are the Aristotelian “forms” which actualize the potential. For the Austrians, and especially for Böhm-Bawerk, furthermore, causality and teleology were identical. In contrast to the functional-mutual determination approach of Walras and of contemporary economists, the Austrians traced the causes of economic phenomena back to the wants and choices of consumers. Wieser especially stressed the grounding of economic theory on the inner experience of the mind.²³

Furthermore, Ludwig M. Lachmann, in contrasting the Austrian and Lausanne Schools, shows that the Austrians were endeavoring to construct a “*verstehende* social science,” the same ideal that Max Weber was later to uphold. Lachmann points out that the older Ricardian economists adopted the “objective” method of the natural sciences insofar as their major focus was upon the quantitative problem of income distribution. In their analysis, factors of production (land, labor, and capital goods) react mechanically to external economic changes. But, in contrast, “Austrian theory is ‘subjective’ also in the sense that individuals . . . perform acts and lend the imprint of their individuality to the events on the market.” As for the contrast between Austria and Lausanne

²³Emil Kauder, “Intellectual and Political Roots of the Older Austrian School,” *Zeitschrift für Nationalökonomie* 17, no. 4 (1958): 411–25.

it is the contrast between those [Lausanne] who confine themselves to determining the appropriate magnitudes of the elements of a system (the conditions of equilibrium) and those [the Austrians] who try to explain events in terms of the mental acts of the individuals who fashion them. Most Austrian thinkers were dimly aware of this contrast, but before Hans Mayer, Mises and Hayek were unable to express it concisely. The validity of the Lausanne model is limited to a stationary world. The background of the Austrian theory, by contrast, is a world of continuous change in which plans have to be conceived and continually revised.²⁴

We may conclude this sketch of the history of the praxeological tradition in economics by treating an important but much neglected debate on economic methodology which occurred at the turn of the twentieth century between Pareto and the philosopher Benedetto Croce. Croce, from his own highly developed praxeological position, opened the debate by chiding Pareto for having written that economic theory was a species of mechanics. Vigorously rejecting this view, Croce points out that a fact in mechanics is a mere fact, which requires no positive or negative comment; whereas words of approval or disapproval can appropriately be applied to an *economic* fact. The reason is that the true data of economics are not “physical things and objects, but actions. The physical object is merely the brute matter of an economic act.”²⁵ Economic data, then, are acts of man, and these acts are the results of conscious choice.

²⁴English abstract of Ludwig M. Lachmann, “Die geistesgeschichtliche Bedeutung der österreichischen Schule in der Volkswirtschaftslehre,” *Zeitschrift für Nationalökonomie* 26, nos. 1–3 (1966): 152–67, in *Journal of Economic Abstracts* 5 (September 1967): 553–54. See also Lachmann, “Methodological Individualism and the Market Economy,” in *Roads to Freedom: Essays in Honor of Friedrich A. von Hayek*, E. Streissler, ed. (New York: Augustus M. Kelley, 1969), pp. 89–103; and Israel M. Kirzner, “Methodological Individualism, Market Equilibrium, and Market Process,” *Il Politico* 32, no. 4 (December 1967): 787–99.

²⁵Benedetto Croce, “On the Economic Principle: I” (1990), *International Economic Papers* 3 (1953): 173, 195. On Croce’s views on economics, see Giorgio Tagliacozzo, “Croce and the Nature of Economic Science,” *Quarterly Journal of Economics* 59 (May 1945): 307–29. On the Croce-Pareto debate, see Kirzner, *Economic Point of View*, pp. 155–57.

It is of interest that the Walrasian economist Joseph Schumpeter, in his only untranslated work, *Das Wesen und der Hauptinhalt der theoretischen*

In his lengthy reply, Pareto reiterates the similarity between economics and mechanics, and, like the positivists of today, defends unrealistic mechanistic assumptions as simple abstractions from reality, in the supposed manner of the natural sciences. Professing, in a typical positivist gambit, not to “understand” the concept of value, Pareto writes: “I see . . . that you employ the term *value*. . . . I no longer use it as I do not know what it would convey to other people.” The concept of value is vague and complex and not subject to measurement; therefore, “the equations of pure economics establish relations between quantities of things, hence objective relations, and not relations between more or less precise concepts of our minds.”²⁶ Criticizing Croce’s evident concentration on the essences of economic action, as exemplified in his insistence that “one ought to study not the things which are the result of actions but the actions themselves,” Pareto complains that this method is an ancient scientific fallacy. “The ancients conjured up cosmogonies instead of studying astronomy, wondered about the principles of the elements water and fire . . . instead of studying chemistry. Ancient science wanted to proceed from the origin to the facts. Modern science starts from the facts and proceeds towards the origin at an extremely slow pace.” Typically, Pareto sets forth the objectivist, positivist position by arguing from the analogy of the method of the natural sciences, thus completely begging the question of whether the methodologies of the natural and the social sciences should or should not be similar. Thus he concludes that “science proceeds by replacing the relationships between human concepts (which relationships are the first to occur to us) by relationships between things.”²⁷

Croce replies by criticizing Pareto’s restriction of economics to measurable quantities as arbitrary; for what of those economic

Nationalökonomie (Leipzig: Duncker and Humblot, 1908), specifically declared that the economist must only treat changes in “economic quantities” as if they were caused automatically, without reference to the human beings who may have been involved in such changes. In that way, causality and purpose would be replaced in economic theory by functional, mathematical relationships. See Kirzner, *Economic Point of View*, pp. 68–70.

²⁶Vilfredo Pareto, “On the Economic Phenomenon” (1900), *International Economic Papers* 3(1953): 187.

²⁷*Ibid.*, pp. 190, 196.

situations where the objects of action or exchange are not measurable? Croce suggests that it is Pareto who is really being metaphysical, while Croce is the true empiricist. For “your implied metaphysical postulate is . . . this: that the facts of man’s activity are of the same nature as physical facts; that in the one case as in the other we can only observe regularity and deduce consequences therefrom, without ever penetrating into the inner nature of the facts. . . . How would you defend this postulate of yours except by a metaphysical monism?” In contrast, writes Croce, “I hold to experience. This testifies to me of the fundamental distinction between external and internal, between physical and mental, between mechanics and teleology, between passivity and activity.” As for value, it is really a simple term wrapped up in human activity: “Value is observed immediately in ourselves, in our consciousness.”²⁸

In his rejoinder, Pareto begins with a typical example of metaphysical obtuseness: He does *not* believe that “the facts of man’s activity are of the same nature as physical facts” because he doesn’t know what “nature” may be. He goes on to reiterate various examples from physical science to demonstrate the proper methodology for all disciplines. He wishes to follow the “masters of positive science” rather than mere philosophers. Pareto concludes with a concise summation of the differences between the two men and the two methodologies:

We experimentalists . . . accept hypotheses not for any intrinsic value they may have but only in so far as they yield deductions which are in harmony with the facts. You, considering the nature of things independently from the rest, establish a certain proposition A, and from it come down to the concrete facts B. We may accept proposition A, but only as a hypothesis, therefore making not the slightest attempt to prove it. . . . Then we see what can be deduced from it. If those deductions agree with the facts we accept the hypothesis, for the time being of course, because we hold nothing as final or absolute.²⁹

²⁸Croce, “On the Economic Principle II” (1901), *International Economic Papers* 3 (1953): 198–99.

²⁹Pareto, “On the Economic Principle” (1901), *International Economic Papers* 3 (1953): 206.

METHODOLOGICAL INDIVIDUALISM

Only an individual has a mind; only an individual can feel, see, sense, and perceive; only an individual can adopt values or make choices; only an individual can *act*. This primordial principle of “methodological individualism,” central to Max Weber’s social thought, must underlie praxeology as well as the other sciences of human action. It implies that such collective concepts as groups, nations, and states do not actually exist or act; they are only metaphorical constructs for describing the similar or concerted actions of individuals. There are, in short, no “governments” as such; there are only individuals acting in concert in a “governmental” manner. Max Weber puts it clearly:

These collectivities must be treated as solely the resultants and modes of organization of the particular acts of individual persons, since these alone can be treated as agents in a course of subjectively understandable action. . . . For sociological purposes . . . there is no such thing as a collective personality which “acts.” When reference is made in a sociological context to . . . collectivities, what is meant is . . . *only* a certain kind of development of actual or possible social actions of the individual persons.³⁰

Ludwig von Mises points out that what differentiates purely individual action from that of individuals acting as members of a collective is the different *meaning* attached by the people involved.

It is the meaning which the acting individuals and all those who are touched by their action attribute to an action, that determines its character. It is the meaning that marks one action as the action of the state or of the municipality. The hangman, not the state, executes a criminal. It is the meaning of those concerned that discerns in the hangman’s action an action of the state. A group of armed

³⁰Max Weber, *The Theory of Social and Economic Organization* (Glencoe, Ill.: The Free Press, 1957), quoted in Alfred Schütz, *The Phenomenology of the Social World* (Evanston, Ill.: Northwestern University Press, 1967), p. 199. For an application of methodological individualism to foreign policy, see Parker T. Moon, *Imperialism and World Politics* (New York: Macmillan, 1930), p. 58. For a more general political application, see Frank Chodorov, “Society Are People,” in *The Rise and Fall of Society* (New York: Devin-Adair, 1959), pp. 29–37.

men occupies a place. It is the meaning of those concerned which imputes this occupation not to the officers and soldiers on the spot, but to their nation.³¹

In his important methodological work, Mises's disciple F.A. Hayek has demonstrated that the fallacy of treating collective constructs as directly perceived "social wholes" ("capitalism," "the nation," "the class") about which laws can be discovered stems from the objectivist-behaviorist insistence on treating men from the outside, as if they were stones, rather than attempting to understand their subjectively determined actions.

It [the objectivist view] treats social phenomena not as something of which the human mind is a part and the principles of whose organization we can construct from the familiar parts, but as if they were objects directly perceived by us as wholes. . . .

There is the rather vague idea that since "social phenomena" are to be the object of study, the obvious procedure is to start from the direct observation of these "social phenomena," where the existence in popular usage of such terms as "society" or "economy" is naively taken as evidence that there must be definite "objects" corresponding to them.³²

Hayek adds that emphasis on the meaning of the individual act brings out that, "what of social complexes is directly known to us are only the parts and that the whole is never directly perceived but always reconstructed by an effort of our imagination."³³

Alfred Schütz, the outstanding developer of the phenomenological method in the social sciences, has reminded us of the importance of going back "to the 'forgotten man' of the social sciences, to the actor in the social world whose doing and feeling lies at the bottom of the whole system. We, then, try to understand him in that doing and feeling and the state of mind which induced him to adopt specific attitudes towards his social environment." Schütz adds that "for a theory of action the subjective point of view must be retained in its fullest strength, in default of which such a theory loses its basic foundations, namely its reference to the social world of everyday life and

³¹Mises, *Human Action*, p. 42.

³²Hayek, *Counter-Revolution of Science*, pp. 53–54.

³³*Ibid.*, p. 214.

experience.” Lacking such a foundation, social science is likely to replace the “world of social reality” by a fictional nonexistent world constructed by the scientific observer. Or, as Schütz puts it succinctly: “I cannot understand a social thing without reducing it to human activity which has created it, and beyond it, without referring this human activity to the motives out of which it springs.”³⁴

Arnold W. Green has recently demonstrated how the use of invalid collective concepts has damaged the discipline of sociology. He notes the increasing use of “society” as an entity which thinks, feels, and acts, and, in recent years, has functioned as the perpetrator of all social ills. “Society,” for example, and not the criminal, is often held to be responsible for all crime. In many quarters “society” is considered almost demonic, a “reified villain” which “may be attacked at will, blamed at random, derided and mocked with self-righteous fury, [and] may even be overturned by fiat or utopian yearning—and somehow, in some way, buses will still run on time.” Green adds that “if on the other hand, society is viewed as people whose insecure social relationships are preserved only by the fealty paid their common store of moral rules, then the area of free choice available in which with impunity to demand, undermine, and wreck, is sharply restricted.” Moreover, if we realize that “society” does not itself exist, but is made up only of individual people, then to say that “society is responsible for crime, and criminals are not responsible for crime, is to say that only those members of society who do not commit crime can be held responsible for crime. Nonsense this obvious can be circumvented only by conjuring up society as devil, as evil being apart from people and what they do.”³⁵

Economics has been rife with fallacies that arise when collective social metaphors are treated as if they were existent objects. Thus, during the era of the gold standard there was occasionally great alarm that “England” or “France” was in mortal danger because “it” was losing gold. What actually happened was that Englishmen and Frenchmen were voluntarily shipping gold overseas and thus threatening the people who ran the banks of those countries with the

³⁴Schütz, *Collected Papers*, vol. 2, pp. 7, 8, 10.

³⁵Arnold W. Green, “The Reified Villain,” *Social Research* 35 (Winter, 1968): 656, 664. On the concept of “society,” see also Mises, *Theory and History*, pp. 250ff.

necessity of meeting obligations to pay in gold which they could not possibly fulfill. But the use of the collective metaphor converted a grave problem of banking into a vague national crisis for which every citizen was somehow responsible.

Similarly, during the 1930s and 1940s many economists proclaimed that in contrast to debts owed overseas, the size of the domestic public debt was unimportant because "we only owe it to ourselves." The implication was that the collective national person owed "himself" money from one pocket to another. This explanation obscured the fact that it makes a substantial difference for every person whether he is a member of the "we" or the "ourselves."

Sometimes the collective concept is treated unabashedly as a biological organism. Thus, the popular concept of economic growth implies that every economy is somehow destined, in the manner of a living organism, to "grow" in some predetermined manner. The use of such analogical terms is an attempt to overlook or even negate individual will and consciousness in social and economic affairs. As Edith Penrose has written in a critique of the use of the "growth" concept in the study of business firms:

Where explicit biological analogies crop up in economics they are drawn exclusively from that aspect of biology which deals with the unmotivated behavior of organisms . . . have no reason whatever for thinking that the growth pattern of a biological organism is willed by the organism itself. On the other hand, we have every reason for thinking that the growth of a firm is willed by those who make the decisions of the firm . . . and the proof of this lies in the fact that no one can describe the development of any given firm . . . except in terms of decisions taken by individual men.³⁶

There is no better summary of the nature of praxeology and the role of economic theory in relation to concrete historical events than in Alfred Schütz's discussion of the economic methodology of Ludwig von Mises:

No economic act is conceivable without some reference to an economic actor, but the latter is absolutely anonymous; it is not you,

³⁶Edith Tilton Penrose, "Biological Analogies in the *Theory of the Firm*," *American Economic Review* (December 1952): 808.

nor I, nor an entrepreneur, nor even an “economic man” as such, but a pure universal “one.” This is the reason why the propositions of theoretical economics have just that “universal validity” which gives them the ideality of the “and so forth” and “I can do it again.” However, one can study the economic actor as such and try to find out what is going on in his mind; of course, one is not then engaged in theoretical economics but in economic history or economic sociology. . . . However, the statements of these sciences can claim no universal validity, for they deal either with the economic sentiments of particular historical individuals or with types of economic activity for which the economic acts in question are evidence. . . .

In our view, pure economics is a perfect example of an objective meaning-complex about subjective meaning—complexes, in other words, of an objective meaning—configuration stipulating the typical and invariant subjective experiences of anyone who acts within an economic framework. . . . Excluded from such a scheme would have to be any consideration of the uses to which the “goods” are to be put after they are acquired. But once we do turn our attention to the subjective meaning of a real individual person, leaving the anonymous “anyone” behind, then of course it makes sense to speak of behavior that is atypical. . . . To be sure, such behavior is irrelevant from the point of view of economics, and it is in this sense that economic principles are, in Mises’s words, “not a statement of what usually happens, but of what necessarily must happen.”³⁷

³⁷Schütz, *Phenomenology of the Social World*, pp. 137, 245.

Praxeology: The Methodology of Austrian Economics

Praxeology is the distinctive methodology of the Austrian School. The term was first applied to the Austrian method by Ludwig von Mises, who was not only the major architect and elaborator of this methodology but also the economist who most fully and successfully applied it to the construction of economic theory.¹ While the praxeological method is, to say the least, out of fashion in contemporary economics—as well as in social science generally and in the philosophy of science—it was the basic method of the earlier Austrian School and also of a considerable segment of the older Classical School, in particular of J.B. Say and Nassau W. Senior.²

Originally appeared in *The Foundations of Modern Austrian Economics*, Edwin Dolan, ed. (Kansas City: Sheed and Ward, 1976), pp. 19–39.

¹See in particular Ludwig von Mises, *Human Action: A Treatise on Economics* (New Haven, Conn.: Yale University Press, 1949); also see Mises, *Epistemological Problems of Economics*, George Reisman, trans. (Princeton, N.J.: D. Van Nostrand, 1960).

²See Murray N. Rothbard, “Praxeology as the Method of the Social Sciences,” in *Phenomenology and the Social Sciences*, Maurice Natanson, ed., 2 vols. (Evanston, Ill.: Northwestern University Press, 1973), vol. 2, pp. 323–35; reprinted in this volume as chapter 3; also see Marian Bowley, *Nassau Senior and Classical Economics* (New York: Augustus M. Kelley, 1949), pp. 27–65; and Terence W. Hutchinson, “Some Themes from Investigations into Method,” in *Carl Menger and the Austrian School of Economics*, J.R. Hicks and Wilhelm Weber, eds. (Oxford: Clarendon Press, 1973), pp. 15–31.

Praxeology rests on the fundamental axiom that individual human beings act, that is, on the primordial fact that individuals engage in conscious actions toward chosen goals. This concept of action contrasts to purely *reflexive*, or knee-jerk, behavior, which is not directed toward goals. The praxeological method spins out by verbal deduction the logical implications of that primordial fact. In short, praxeological economics is the structure of logical implications of the *fact* that individuals act. This structure is built on the fundamental axiom of action, and has a few subsidiary axioms, such as that individuals vary and that human beings regard leisure as a valuable good. Any skeptic about deducing from such a simple base an entire system of economics, I refer to Mises's *Human Action*. Furthermore, since praxeology begins with a true axiom, A, all the propositions that can be deduced from this axiom must also be true. For if A implies B, and A is true, then B must also be true.

Let us consider some of the immediate implications of the action axiom. Action implies that the individual's behavior is purposive, in short, that it is directed toward goals. Furthermore, the fact of his action implies that he has consciously chosen certain means to reach his goals. Since he wishes to attain these goals, they must be valuable to him; accordingly he must have values that govern his choices. That he employs means implies that he believes he has the technological knowledge that certain means will achieve his desired ends. Let us note that praxeology does not assume that a person's choice of values or goals is wise or proper or that he has chosen the technologically correct method of reaching them. All that praxeology asserts is that the individual actor adopts goals and believes, whether erroneously or correctly, that he can arrive at them by the employment of certain means.

All action in the real world, furthermore, must take place through time; all action takes place in some present and is directed toward the future (immediate or remote) attainment of an end. If all of a person's desires could be instantaneously realized, there would be no reason for him to act at all.³ Furthermore, that a man acts implies

³In answer to the criticism that not all action is directed to some future point in time, see Walter Block, "A Comment on 'The Extraordinary Claim of Praxeology' by Professor Gutierrez," *Theory and Decision* 3 (1973): 381–82.

that he believes action will make a difference; in other words, that he will prefer the state of affairs resulting from action to that from no action. Action therefore implies that man does not have omniscient knowledge of the future; for if he had such knowledge, no action of his would make any difference. Hence, action implies that we live in a world of an uncertain, or not fully certain, future. Accordingly, we may amend our analysis of action to say that a man chooses to employ means according to a technological plan in the present because he expects to arrive at his goals at some future time.

The fact that people act necessarily implies that the means employed are scarce in relation to the desired ends; for, if all means were not scarce but superabundant, the ends would already have been attained, and there would be no need for action. Stated another way, resources that are superabundant no longer function as means, because they are no longer objects of action. Thus, air is indispensable to life and hence to the attainment of goals; however, air being superabundant is not an object of action and therefore cannot be considered a *means*, but rather what Mises called a “general condition of human welfare.” Where air is not superabundant, it may become an object of action, for example, where cool air is desired and warm air is transformed through air conditioning. Even with the absurdly unlikely advent of Eden (or what a few years ago was considered in some quarters to be an imminent “postscarcity” world), in which all desires could be fulfilled instantaneously, there would still be at least one scarce means: the individual’s time, each unit of which if allocated to one purpose is necessarily not allocated to some other goal.⁴

Such are some of the immediate implications of the axiom of action. We arrived at them by deducing the logical implications of the existing fact of human action, and hence deduced true conclusions from a true axiom. Apart from the fact that these conclusions cannot be “tested” by historical or statistical means, there is no need to test them since their truth has already been established. Historical fact enters into these conclusions only by determining which branch of the theory is applicable in any particular case. Thus, for Crusoe and

⁴See Mises, *Human Action*, pp. 101–02; and esp., Block, “Comment,” p. 383.

Friday on their desert island, the praxeological theory of money is only of academic, rather than of currently applicable, interest. A fuller analysis of the relationship between theory and history in the praxeological framework will be considered below.

There are, then, two parts of this axiomatic-deductive method: the process of deduction and the epistemological status of the axioms themselves. First, there is the process of deduction; why are the means verbal rather than mathematical logic?⁵ Without setting forth the comprehensive Austrian case against mathematical economics, one point can immediately be made: let the reader take the implications of the concept of action as developed so far in this paper and try to place them in mathematical form. And even if that could be done, what would have been accomplished except a drastic loss in meaning at each step of the deductive process? Mathematical logic is appropriate to physics—the science that has become the model science, which modern positivists and empiricists believe all other social and physical sciences should emulate. In physics the axioms and therefore the deductions are in themselves purely formal and only acquire meaning “operationally” insofar as they can explain and predict given facts. On the contrary, in praxeology, in the analysis of human action, the axioms themselves are known to be true and meaningful. As a result, each verbal step-by-step deduction is also true and meaningful; for it is the great quality of verbal propositions that each one is meaningful, whereas mathematical symbols are not meaningful in themselves. Thus Lord Keynes, scarcely an Austrian and himself a mathematician of note, leveled the following critique at mathematical symbolism in economics:

It is a great fault of symbolic pseudo-mathematical methods of formalizing a system of economic analysis, that they expressly assume strict independence between the factors involved and lose all their cogency and authority if this hypothesis is disallowed: whereas, in ordinary discourse, where we are not blindly manipulating but know all the time what we are doing and what the words mean, we can keep “at the back of our heads” the necessary reserves and qualifications and the adjustments which we have to make later

⁵For a typical criticism of praxeology for not using mathematical logic, see George. J. Schuller, “Rejoinder,” *American Economic Review* 41 (March 1951): 188.

on, in a way in which we cannot keep complicated partial differentials “at the back” of several pages of algebra which assume that they all vanish. Too large a proportion of recent “mathematical” economics are mere concoctions, as imprecise as the initial assumptions they rest on, which allow the author to lose sight of the complexities and interdependencies of the real world in a maze of pretentious and unhelpful symbols.⁶

Moreover, even if verbal economics could be successfully translated into mathematical symbols and then retranslated into English so as to explain the conclusions, the process makes no sense and violates the great scientific principle of Occam’s Razor: avoiding unnecessary multiplication of entities.⁷

Furthermore, as political scientist Bruno Leoni and mathematician Eugenio Frola pointed out,

It is often claimed that translation of such a concept as the maximum from ordinary into mathematical language, involves an improvement in the logical accuracy of the concept, as well as wider opportunities for its use. But the lack of mathematical precision in ordinary language reflects precisely the behavior of individual human beings in the real world. . . . We might suspect that translation into mathematical language by itself implies a suggested transformation of human economic operators into virtual robots.⁸

Similarly, one of the first methodologists in economics, Jean-Baptiste Say, charged that the mathematical economists

⁶John Maynard Keynes, *The General Theory of Employment, Interest, and Money* (New York: Harcourt, Brace, 1936), pp. 297–98.

⁷See Murray N. Rothbard, “Toward a Reconstruction of Utility and Welfare Economics,” in *On Freedom and Free Enterprise*, Mary Sennholz, ed. (Princeton, N.J.: D. Van Nostrand, 1956), p. 227; reprinted in this volume as chapter 17; Rothbard, *Man, Economy, and State*, 2 vols. (Princeton, N.J.: D. Van Nostrand, 1962), vol. 1, pp. 65–66. On mathematical logic as being subordinate to verbal logic, see René Poirier, “Logique,” in *Vocabulaire technique et critique de la philosophie*, André Lalande, ed., 6th ed. rev. (Paris: Presses Universitaires de France, 1951), pp. 574–75.

⁸Bruno Leoni and Eugenio Frola, “On Mathematical Thinking in Economics” (unpublished manuscript privately distributed), pp. 23–24; the Italian version of this article is “Possibilit  di applicazione della matematiche alle discipline economiche,” *Il Politico* 20 (1995).

have not been able to enunciate these questions into analytical language, without divesting them of their natural complication, by means of simplifications, and arbitrary suppressions, of which the consequences, not properly estimated, always essentially change the condition of the problem, and pervert all its results.⁹

More recently, Boris Ischboldin has emphasized the difference between verbal, or “language,” logic (“the actual analysis of thought stated in language expressive of reality as grasped in common experience”) and “construct” logic, which is “the application to quantitative (economic) data of the constructs of mathematics and symbolic logic which constructs may or may not have real equivalents.”¹⁰

Although himself a mathematical economist, the mathematician son of Carl Menger wrote a trenchant critique of the idea that mathematical presentation in economics is necessarily more precise than ordinary language:

Consider, for example, the statements (2) *To a higher price of a good, there corresponds a lower (or at any rate not a higher) demand.*

(2') *If p denotes the price of, and q the demand for, a good, then*

$$q = f(p) \text{ and } \frac{dq}{dp} = f'(p) \leq 0.$$

Those who regard the formula (2') as more precise or “more mathematical” than the sentence (2) are under a complete misapprehension. . . . The only difference between (2) and (2') is this: since (2') is limited to functions which are differentiable and whose graphs, therefore, have tangents (which from an economic point of view are not more plausible than curvature), the sentence (2) is *more general, but it is by no means less precise: it is of the same mathematical precision as (2')*.¹¹

⁹Jean-Baptiste Say, *A Treatise on Political Economy* (New York: Augustus M. Kelley, 1964), p. xxvin.

¹⁰Boris Ischboldin, “a Critique of Econometrics,” *Review of Social Economy* 18, no. 2 (September 1960): 11n; Ischboldin’s discussion is based on the construction of I.M. Bochenski, “Scholastic and Aristotelian Logic,” *Proceedings of the American Catholic Philosophical Association* 30 (1956): 112–17.

¹¹Karl Menger, “Austrian Marginalism and Mathematical Economics,” in *Carl Menger*, p. 41.

Turning from the deduction process to the axioms themselves, what is their epistemological status? Here the problems are obscured by a difference of opinion within the praxeological camp, particularly on the nature of the fundamental axiom of action. Ludwig von Mises, as an adherent of Kantian epistemology, asserted that the concept of action is *a priori* to all experience, because it is, like the law of cause and effect, part of “the essential and necessary character of the logical structure of the human mind.”¹² Without delving too deeply into the murky waters of epistemology, I would deny, as an Aristotelian and neo-Thomist, any such alleged “laws of logical structure” that the human mind necessarily imposes on the chaotic structure of reality. Instead, I would call all such laws “laws of reality,” which the mind apprehends from investigating and collating the facts of the real world. My view is that the fundamental axiom and subsidiary axioms are derived from the experience of reality and are therefore in the broadest sense empirical. I would agree with the Aristotelian realist view that its doctrine is radically empirical, far more so than the post-Humean empiricism which is dominant in modern philosophy. Thus, John Wild wrote:

It is impossible to reduce experience to a set of isolated impressions and atomic units. Relational structure is also given with equal evidence and certainty. The immediate data are full of determinate structure, which is easily abstracted by the mind and grasped as universal essences or possibilities.¹³

Furthermore, one of the pervasive data of all human experience is existence; another is consciousness, or awareness. In contrast to the Kantian view, Harmon Chapman wrote that

conception is a kind of awareness, a way of apprehending things or comprehending them and not an alleged subjective manipulation of so-called generalities or universals solely “mental” or “logical” in their provenience and non-cognitive in nature.

That in thus penetrating the data of sense, conception also synthesizes these data is evident. But the synthesis here involved,

¹²Mises, *Human Action*, p. 34.

¹³John Wild, “Phenomenology and Metaphysics,” in *The Return to Reason: Essays in Realistic Philosophy*, John Wild, ed. (Chicago: Henry Regnery, 1953), pp. 48, 37–57.

unlike the synthesis of Kant, is not a prior condition of perception, an anterior process of constituting both perception and its object, but rather a cognitive synthesis *in* apprehension, that is, a uniting or “comprehending” which is one with the apprehending itself. In other words, perception and experience are not the results or end products of a synthetic process *a priori*, but are themselves synthetic or comprehensive apprehension whose structured unity is prescribed solely by the nature of the real, that is, by the intended objects in their togetherness and not by consciousness itself whose (cognitive) nature is to apprehend the real—as it is.¹⁴

If, in the broad sense, the axioms of praxeology are radically empirical, they are far from the post-Humean empiricism that pervades the modern methodology of social science. In addition to the foregoing considerations, (1) they are so broadly based in common human experience that once enunciated they become self-evident and hence do not meet the fashionable criterion of “falsifiability”; (2) they rest, particularly the action axiom, on universal *inner* experience, as well as on external experience, that is, the evidence is *reflective* rather than purely physical; and (3) they are therefore *a priori* to the complex historical events to which modern empiricism confines the concept of “experience.”¹⁵

Say, perhaps the first praxeologist, explained the derivation of the axioms of economic theory as follows:

Hence the advantage enjoyed by everyone who, from distinct and accurate observation, can establish the existence of these general facts, demonstrate their connection and deduce their consequences. They as certainly proceed from the nature of things as the laws of the material world. We do not imagine them; they are results disclosed to us by judicious observation and analysis. . . .

Political economy . . . is composed of a few fundamental principles, and of a great number of corollaries or conclusions, drawn

¹⁴Harmon M. Chapman, “Realism and Phenomenology,” in *Return to Reason*, p. 29. On the interrelated functions of sense and reason and their respective roles in human cognition of reality, see Francis H. Parker, “Realistic Epistemology,” *ibid.*, pp. 167–69.

¹⁵See Murray N. Rothbard, “In Defense of ‘Extreme Apriorism,’” *Southern Economic Journal* 23 (January 1957): 315–18; included in this volume as chapter 6. It should be clear from the current paper that the term extreme apriorism is a misnomer for praxeology.

from these principles . . . that can be admitted by every reflecting mind.¹⁶

Friedrich A. Hayek trenchantly described the praxeological method in contrast to the methodology of the physical sciences and also underlined the broadly empirical nature of the praxeological axioms:

The position of man . . . brings it about that the essential basic facts which we need for the explanation of social phenomena are part of common experience, part of the stuff of our thinking. In the social sciences it is the elements of the complex phenomena which are known beyond the possibility of dispute. In the natural sciences they can only be at best surmised. The existence of these elements is so much more certain than any regularities in the complex phenomena to which they give rise, that it is they which constitute the truly empirical factor in the social sciences. There can be little doubt that it is this different position of the empirical factor in the process of reasoning in the two groups of disciplines which is at the root of much of the confusion with regard to their logical character. The essential difference is that in the natural sciences the process of deduction has to start from some hypothesis which is the result of inductive generalizations, while in the social sciences it starts directly from known empirical elements and uses them to find the regularities in the complex phenomena which direct observations cannot establish. They are, so to speak, empirically deductive sciences, proceeding from the known elements to the regularities in the complex phenomena which cannot be directly established.¹⁷

Similarly, J.E. Cairnes wrote:

The economist starts with a knowledge of ultimate causes. He is already, at the outset of his enterprise in the position which the physicist only attains after ages of laborious research. . . . For the discovery of such premises no elaborate process of induction is needed . . . for this reason, that we have, or may have if we choose to turn our attention to the subject, direct knowledge of these causes in our consciousness of what passes in our own minds, and

¹⁶Say, *A Treatise on Political Economy*, pp. xxv–xxvi, xlv.

¹⁷Friedrich A. Hayek, “The Nature and History of the Problem,” in *Collectivist Economic Planning*, F.A. Hayek, ed. (London: George Routledge and Sons, 1935), p 11.

in the information which our senses convey . . . to us of external facts.¹⁸

Nassau W. Senior phrased it thus:

The physical sciences, being only secondarily conversant with mind, draw their premises almost exclusively from observation or hypothesis. . . . On the other hand, the mental sciences and the mental arts draw their premises principally from consciousness. The subjects with which they are chiefly conversant are the workings of the human mind. [These premises are] a very few general propositions, which are the result of observation, or consciousness, and which almost every man, as soon as he hears them, admits, as familiar to his thought, or at least, included in his previous knowledge.¹⁹

Commenting on his complete agreement with this passage, Mises wrote that these “immediately evident propositions” are “of aprioristic derivation . . . unless one wishes to call aprioristic cognition inner experience.”²⁰ To which Marian Bowley, the biographer of Senior, justly comments:

The only fundamental difference between Mises’s general attitude and Senior’s lies in Mises’s apparent denial of the possibility of using any general empirical data, i.e., facts of general observation, as initial premises. This difference, however, turns upon Mises’s basic ideas of the nature of thought, and though of general philosophic importance, has little special relevance to economic method as such.²¹

It should be noted that for Mises it is only the fundamental axiom of action that is *a priori*; he conceded that the subsidiary axioms of the diversity of mankind and nature, and of leisure as a consumers’ good, are broadly empirical.

Modern post-Kantian philosophy has had a great deal of trouble encompassing self-evident propositions, which are marked precisely

¹⁸John Elliott Cairnes, *The Character and Logical Method of Political Economy*, 2nd ed. (London: Macmillan, 1875), pp. 87–88; italics in the original.

¹⁹Bowley, *Nassau Senior*, pp. 43, 56.

²⁰Mises, *Epistemological Problems*, p. 19.

²¹Bowley, *Nassau Senior*, pp. 64–65.

by their strong and evident truth rather than by being testable hypotheses, that are, in the current fashion, considered to be “falsifiable.” Sometimes it seems that the empiricists use the fashionable analytic-synthetic dichotomy, as the philosopher Hao Wang charged, to dispose of theories they find difficult to refute by dismissing them as necessarily *either* disguised definitions *or* debatable and uncertain hypotheses.²² But what if we subject the vaunted “evidence” of modern positivists and empiricists to analysis? What is it? We find that there are two types of such evidence to either confirm or refute a proposition: (1) if it violates the laws of logic, for example, implies that $A = -A$; or (2) if it is confirmed by empirical facts (as in a laboratory) that can be checked by many persons. But what is the nature of such “evidence” but the bringing, by various means, of propositions hitherto cloudy and obscure into clear and evident view, that is, evident to the scientific observers? In short, logical or laboratory processes serve to make it evident to the “selves” of the various observers that the propositions are either confirmed or refuted, or, to use unfashionable terminology, either true or false. But in that case propositions that are *immediately* evident to the selves of the observers have at least as good scientific status as the other and currently more acceptable forms of evidence. Or, as the Thomist philosopher John J. Toohey put it,

Proving means *making evident* something which is not evident. If a truth or proposition is self-evident, it is useless to attempt to prove it; to attempt to prove it would be to attempt to make evident something which is already evident.²³

The action axiom, in particular, should be, according to Aristotelian philosophy, unchallengeable and self-evident since the critic who attempts to refute it finds that he must use it in the process of alleged refutation. Thus, the axiom of the existence of human consciousness is demonstrated as being self-evident by the fact that the

²²Hao Wang, “Notes on the Analytic-Synthetic Distinction,” *Theoria* 21 (1995): 158; see also John Wild and J.L. Cobitz, “On the Distinction between the Analytic and Synthetic,” *Philosophy and Phenomenological Research* 8 (June 1948): 651–67.

²³John J. Toohey, *Notes on Epistemology*, rev. ed. (Washington, D.C.: Georgetown University, 1937), p. 36; italics in the original.

very act of denying the existence of consciousness must itself be performed by a conscious being. The philosopher R.P. Phillips called this attribute of a self-evident axiom a “boomerang principle,” since “even though we cast it away from us, it returns to us again.”²⁴ A similar self-contradiction faces the man who attempts to refute the axiom of human action. For in doing so, he is *ipso facto* a person making a conscious choice of means in attempting to arrive at an adopted end: in this case the end, or goal, of trying to refute the axiom of action. He employs action in trying to refute the notion of action.

Of course, a person may *say* that he denies the existence of self-evident principles or other established truths of the real world, but this mere saying has no epistemological validity. As Toohey pointed out,

A man may *say* anything he pleases, but he cannot *think* or *do* anything he pleases. He may say he saw a round square, but he cannot *think* he saw a round square. He may say, if he likes, that he saw a horse riding astride its own back, but we shall know what to think of him if he says it.²⁵

The methodology of modern positivism and empiricism comes a cropper even in the physical sciences, to which it is much better suited than to the sciences of human action; indeed, it particularly fails where the two types of disciplines interconnect. Thus, the phenomenologist Alfred Schütz, a student of Mises at Vienna, who pioneered in applying phenomenology to the social sciences, pointed out the contradiction in the empiricists’ insistence on the principle of empirical verifiability in science, while at the same time denying the existence of “other minds” as unverifiable. But *who* is supposed to be doing the laboratory verification if not these selfsame “other minds” of the assembled scientists? Schütz wrote:

²⁴R.P. Phillips, *Modern Thomistic Philosophy* (Westminster, Maryland: Newman Bookshop, 1934–35), vol. 2, pp. 36–37; see also Murray N. Rothbard, “The Mantle of Science,” in *Scientism and Values*, Helmut Schoeck and James W. Wiggins, eds. (Princeton, N.J.: D. Van Nostrand, 1960), pp. 162–65; included in this volume as chapter 1.

²⁵Toohey, *Notes on Epistemology*, p. 10; italics in the original.

It is . . . not understandable that the same authors who are convinced that no verification is possible for the intelligence of other human beings have such confidence in the principle of verifiability itself, which can be realized only through cooperation with others.²⁶

In this way, the modern empiricists ignore the necessary presuppositions of the very scientific method they champion. For Schütz, knowledge of such presuppositions is “empirical” in the broadest sense,

provided that we do not restrict this term to sensory perceptions of objects and events in the outer world but include the experiential form, by which common-sense thinking in everyday life understands human actions and their outcome in terms of their underlying motives and goals.²⁷

Having dealt with the nature of praxeology, its procedures and axioms and its philosophical groundwork, let us now consider what the relationship is between praxeology and the other disciplines that study human action. In particular, what are the differences between praxeology and technology, psychology, history, and ethics—all of which are in some way concerned with human action?

In brief, *praxeology* consists of the logical implications of the universal formal fact that people act, that they employ means to try to attain chosen ends. *Technology* deals with the contentual problem of *how* to achieve ends by adoption of means. *Psychology* deals with the question of *why* people adopt various ends and how they go about adopting them. *Ethics* deals with the question of what ends, or values,

²⁶Alfred Schütz, *Collected Papers of Alfred Schütz*, vol. 2: *Studies in Social Theory*, A. Brodersen, ed. (The Hague: Nijhoff, 1964), p. 4; see also Mises, *Human Action*, p. 24.

²⁷Alfred Schütz, *Collected Papers of Alfred Schütz*, vol. 1: *The Problem of Social Reality*, Maurice Natanson, ed. (The Hague: Nijhoff, 1962), p. 65. On the philosophical presuppositions of science, see Andrew G. Van Melsen, *The Philosophy of Nature* (Pittsburgh, Penn.: Duquesne University Press, 1953), pp. 6–29. On common sense as the groundwork of philosophy, see Toohey, *Notes on Epistemology*, pp. 74, 106–13. On the application of a similar point of view to the methodology of economics, see Frank H. Knight, “‘What is Truth’ in Economics,” in *On the History and Method of Economics* (Chicago: University of Chicago Press, 1956), pp. 151–78.

people *should* adopt. And *history* deals with ends adopted in the past, what means were used to try to achieve them—and what the consequences of these actions were.

Praxeology, or economic theory in particular, is thus a unique discipline within the social sciences; for, in contrast to the others, it deals not with the *content* of men's values, goals, and actions—not with what they have done or how they have acted or how they should act—but purely with the fact that they *do* have goals and act to attain them. The laws of utility, demand, supply, and price apply regardless of the type of goods and services desired or produced. As Joseph Dorfman wrote of Herbert J. Davenport's *Outlines of Economic Theory* (1896):

The ethical character of the desires was not a fundamental part of his inquiry. Men labored and underwent privation for “whiskey, cigars, and burglars’ jimmies,” he said, “as well as for food, or statuary or harvest machinery.” As long as men were willing to buy and sell “foolishness and evil,” the former commodities would be economic factors with market standing, for utility, as an economic term, meant merely adaptability to human desires. So long as men desired them, they satisfied a need and were motives to production. Therefore economics did not need to investigate the origin of choices.²⁸

Praxeology, as well as the sound aspects of the other social sciences, rests on methodological individualism, on the fact that only individuals feel, value, think, and act. Individualism has always been charged by its critics—and always incorrectly—with the assumption that each individual is a hermetically sealed “atom,” cut off from, and uninfluenced by, other persons. This absurd misreading of methodological individualism is at the root of J.K. Galbraith's triumphant demonstration in *The Affluent Society* (Boston: Houghton Mifflin, 1958) that the values and choices of individuals are influenced by other persons, and therefore—supposedly—that economic theory is invalid. Galbraith also concluded from his demonstration that these choices, because influenced, are artificial and illegitimate. The fact that praxeological economic theory rests on the universal fact of individual values and choices means, to repeat Dorfman's summary

²⁸Joseph Dorfman, *The Economic Mind in American Civilization*, 5 vols. (New York: Viking Press, 1949), vol. 3, p. 376.

of Davenport's thought, that economic theory does "not need to investigate the origin of choices." Economic theory is not based on the absurd assumption that each individual arrives at his values and choices in a vacuum, sealed off from human influence. Obviously, individuals are continually learning from and influencing each other. As F.A. Hayek wrote in his justly famous critique of Galbraith, "The Non Sequitur of the 'Dependence Effect'":

Professor Galbraith's argument could be easily employed, without any change of the essential terms, to demonstrate the worthlessness of literature or any other form of art. Surely an individual's want for literature is not original with himself in the sense that he would experience it if literature were not produced. Does this then mean that the production of literature cannot be defended as satisfying a want because it is only the production which provokes the demand?²⁹

That Austrian-School economics rests firmly from the beginning on an analysis of the fact of individual subjective values and choices unfortunately led the early Austrians to adopt the term *psychological school*. The result was a series of misdirected criticisms that the latest findings of psychology had not been incorporated into economic theory. It also led to misconceptions such as that the law of diminishing marginal utility rests on some psychological law of the satiety of wants. Actually, as Mises firmly pointed out, that law is praxeological rather than psychological and has nothing to do with the *content* of wants, for example, that the tenth spoonful of ice cream may taste less pleasurable than the ninth spoonful. Instead, it is a praxeological truth, derived from the nature of action, that the first unit of a good will be allocated to its most valuable use, the next unit to the next most valuable, and so on.³⁰ On one point, and on one point alone, however, praxeology and the related sciences of human action take a stand in philosophical psychology: on the proposition that the human mind, consciousness, and subjectivity exist, and therefore action exists. In this it is opposed to the philosophical base of behaviorism and related doctrines and joined with all branches of classical

²⁹Friedrich A. Hayek, "The Non Sequitur of the 'Dependence Effect,'" in Friedrich A. Hayek, *Studies in Philosophy, Politics, and Economics* (Chicago: University of Chicago Press, 1967), pp. 314–15.

³⁰Mises, *Human Action*, p. 124.

philosophy and with phenomenology. On all other questions, however, praxeology and psychology are distinct and separate disciplines.³¹

A particularly vital question is the relationship between economic theory and history. Here again, as in so many other areas of Austrian economics, Ludwig von Mises made the outstanding contribution, particularly in his *Theory and History*.³² It is especially curious that Mises and other praxeologists, as alleged “a priorists,” have commonly been accused of being “opposed” to history. Mises indeed held not only that economic theory does not need to be “tested” by historical fact but also that it *cannot* be so tested. For a fact to be usable for testing theories, it must be a simple fact, homogeneous with other facts in accessible and repeatable classes. In short, the theory that one atom of copper, one atom of sulfur, and four atoms of oxygen will combine to form a recognizable entity called copper sulfate, with known properties, is easily tested in the laboratory. Each of these atoms is homogeneous, and therefore the test is repeatable indefinitely. But each historical event, as Mises pointed out, is not simple and repeatable; each event is a complex resultant of a shifting variety of multiple causes, none of which ever remains in constant relationships with the others. Every historical event, therefore, is heterogeneous, and therefore historical events cannot be used either to test or to construct laws of history, quantitative or otherwise. We can place every atom of copper into a homogeneous class of copper atoms; we cannot do so with the events of human history.

This is not to say, of course, that there are no similarities among historical events. There are many similarities, but no homogeneity. Thus, there were many similarities between the presidential election of 1968 and that of 1972, but they were scarcely homogeneous events, since they were marked by important and inescapable differences. Nor will the next election be a repeatable event to place in a homogeneous class of “elections.” Hence no scientific, and certainly no quantitative, laws can be derived from these events.

Mises’s radically fundamental opposition to econometrics now becomes clear. Econometrics not only attempts to ape the natural

³¹See Rothbard, “Toward a Reconstruction,” pp. 230–31.

³²Ludwig von Mises, *Theory and History* (New Haven, Conn.: Yale University Press, 1957).

sciences by using complex heterogeneous historical facts as if they were repeatable homogeneous laboratory facts; it also squeezes the qualitative complexity of each event into a quantitative number and then compounds the fallacy by acting as if these quantitative relations remain constant in human history. In striking contrast to the physical sciences, which rest on the empirical discovery of quantitative constants, econometrics, as Mises repeatedly emphasized, has failed to discover a single constant in human history. And given the ever-changing conditions of human will, knowledge, and values and the differences among men, it is inconceivable that econometrics can ever do so.

Far from being opposed to history, the praxeologist, and not the supposed admirers of history, has profound respect for the irreducible and unique facts of human history. Furthermore, it is the praxeologist who acknowledges that individual human beings cannot legitimately be treated by the social scientist as if they were not men who have minds and act upon their values and expectations, but stones or molecules whose course can be scientifically tracked in alleged constants or quantitative laws. Moreover, as the crowning irony, it is the praxeologist who is truly empirical because he recognizes the unique and heterogeneous nature of historical facts; it is the self-proclaimed “empiricist” who grossly violates the facts of history by attempting to reduce them to quantitative laws. Mises wrote thus about econometricians and other forms of “quantitative economists”:

There are, in the field of economics, no constant relations, and consequently no measurement is possible. If a statistician determines that a rise of 10 percent in the supply of potatoes in Atlantis at a definite time was followed by a fall of 8 percent in the price, he does not establish anything about what happened or may happen with a change in the supply of potatoes in another country or in another time. He has not “measured” the “elasticity of demand” of potatoes. He has established a unique individual historical fact. No intelligent man can doubt that the behavior of men with regard to potatoes and every other commodity is variable. Different individuals value the same things in a different way, and valuations change with the same individuals with changing conditions. . . .

The impracticability of measurement is not due to the lack of technical methods for the establishment of measure. It is due to the absence of constant relations. . . . Economics is not, as . . . positivists repeat again and again, backward because it is not “quantitative.”

It is not quantitative and does not measure because there are no constants. Statistical figures referring to economic events are historical data. They tell us what happened in a nonrepeatable historical case. Physical events can be interpreted on the ground of our knowledge concerning constant relations established by experiments. Historical events are not open to such an interpretation. . . .

Experience of economic history is always experience of complex phenomena. It can never convey knowledge of the kind the experimenter abstracts from a laboratory experiment. Statistics is a method for the presentation of historical facts. . . . The statistics of prices is economic history. The insight that, *ceteris paribus*, an increase in demand must result in an increase in prices is not derived from experience. Nobody ever was or ever will be in a position to observe a change in one of the market data *ceteris paribus*. There is no such thing as quantitative economics. All economic quantities we know about are data of economic history. . . . Nobody is so bold as to maintain that a rise of A percent in the supply of any commodity must always—in every country and at any time—result in a fall of B percent in price. But as no quantitative economist ever ventured to define precisely on the ground of statistical experience the special conditions producing a definite deviation from the ratio A:B, the futility of his endeavors is manifest.³³

Elaborating on his critique of constants Mises added:

The quantities we observe in the field of human action . . . are manifestly variable. Changes occurring in them plainly affect the result of our actions. Every quantity that we can observe is a historical event, a fact which cannot be fully described without specifying the time and geographical point.

The econometrician is unable to disprove this fact, which cuts the ground from under his reasoning. He cannot help admitting that there are no “behavior constants.” Nonetheless, he wants to introduce some numbers, arbitrarily chosen on the basis of historical fact, as “unknown *behavior constants*.” The sole excuse he advances is that his hypotheses are “saying only that these unknown numbers remain reasonably constant through a period of years.”³⁴ Now whether such

³³Mises, *Human Action*, pp. 55–56, 348.

³⁴Cowles Commission for Research in Economics, *Report for the Period, January 1, 1948–June 30, 1949* (Chicago: University of Chicago Press, 1949), p. 7, quoted in Mises, *Theory and History*, pp. 10–11.

a period of supposed constancy of a definite number is still lasting or whether a change in the number has already occurred can only be established later on. In retrospect it may be possible, although in rare cases only, to declare that over a (probably rather short) period an approximately stable ratio which the econometrician chooses to call a “reasonably” constant ratio prevailed between the numerical values of two factors. But this is something fundamentally different from the constants of physics. It is the assertion of a historical fact, not of a constant that can be resorted to in attempts to predict *future events*.³⁵ The highly praised equations are, insofar as they apply to the future, merely equations in which all quantities are unknown.³⁶

In the mathematical treatment of physics the distinction between constants and variables makes sense; it is essential in every instance of technological computation. In economics there are no constant relations between various magnitudes. Consequently all ascertainable data are variables, or what amounts to the same thing, *historical* data. The mathematical economists reiterate that the plight of mathematical economics consists in the fact that there are a great number of variables. The truth is that there are only variables and no constants. It is pointless to talk of variables where there are no invariables.³⁷

What, then, is the proper relationship between economic theory and economic history or, more precisely, history in general? The historian's function is to try to explain the unique historical facts that are his province; to do so adequately he must employ all the relevant theories from all the various disciplines that impinge on his problem. For historical facts are complex resultants of a myriad of causes stemming from different aspects of the human condition. Thus, the historian must be prepared to use not only praxeological economic theory but

³⁵Ibid., pp. 10–11.

³⁶Ludwig von Mises, “Comments about the Mathematical Treatment of Economic Problems.” (Cited as “unpublished manuscript”; published as “The Equations of Mathematical Economics” in the *Quarterly Journal of Austrian Economics* 3, no. 1 (Spring, 2000): 27–32.

³⁷Mises, *Theory and History*, pp. 11–12; see also Leoni and Frola, “On Mathematical Thinking,” pp. 1–8; and Leland B. Yeager, “Measurement as Scientific Method in Economics,” *American Journal of Economics and Sociology* 16 (July 1957): 337–46.

also insights from physics, psychology, technology, and military strategy along with an interpretive understanding of the motives and goals of individuals. He must employ these tools in understanding both the goals of the various actions of history and the consequences of such actions. Because understanding diverse individuals and their interactions is involved, as well as the historical context, the historian using the tools of natural and social science is in the last analysis an "artist," and hence there is no guarantee or even likelihood that any two historians will judge a situation in precisely the same way. While they may agree on an array of factors to explain the genesis and consequences of an event, they are unlikely to agree on the precise weight to be given each causal factor. In employing various scientific theories, they have to make judgments of relevance on which theories applied in any given case; to refer to an example used earlier in this paper, a historian of Robinson Crusoe would hardly employ the theory of money in a historical explanation of his actions on a desert island. To the economic historian, economic law is neither confirmed nor tested by historical facts; instead, the law, where relevant, is applied to help explain the facts. The facts thereby *illustrate* the workings of the law. The relationship between praxeological economic theory and the understanding of economic history was subtly summed up by Alfred Schütz:

No economic act is conceivable without some reference to an economic actor, but the latter is absolutely anonymous; it is not you, nor I nor an entrepreneur, nor even an "economic man," as such, but a pure universal "one." This is the reason why the propositions of theoretical economics have just that "universal validity" which gives them the ideality of the "and so forth" and "I can do it again." However, one can study the economic actor as such and try to find out what is going on in his mind; of course, one is not then engaged in theoretical economics but in economic history or economic sociology. . . . However, the statements of these sciences can claim no universal validity, for they deal either with the economic sentiments of particular historical individuals or with types of economic activity for which the economic acts in question are evidence. . . .

In our view, pure economics is a perfect example of an objective meaning-complex about subjective meaning-complexes, in other words, of an objective meaning-configuration stipulating the typical and invariant subjective experiences of anyone who acts within

an economic framework. . . . Excluded from such a scheme would have to be any consideration of the uses to which the “goods” are to be put after they are acquired. But once we do turn our attention to the subjective meaning of a real individual person, leaving the anonymous “anyone” behind, then of course it makes sense to speak of behavior that is atypical. . . . To be sure, such behavior is irrelevant from the point of view of economics, and it is in this sense that economic principles are, in Mises’s words, “not a statement of what usually happens, but of what necessarily must happen.”³⁸

³⁸Alfred Schütz, *The Phenomenology of the Social World* (Evanston, Ill.: Northwestern University Press, 1967), pp. 137, 245; also see Ludwig M. Lachmann, *The Legacy of Max Weber* (Berkeley, Calif.: Glendessary Press, 1971), pp. 17–48.

Praxeology, Value Judgments, and Public Policy

Ethics is the discipline, or what is called in classical philosophy the “science,” of what goals men should or should not pursue. All men have values and place positive or negative value judgments on goods, people, and events. Ethics is the discipline that provides standards for a moral critique of these value judgments. In the final analysis, either such a discipline exists and a rational or objective system of ethics is possible, or else each individual’s value judgments are ultimately arbitrary and solely a result of individual whim. It is not my province to try to settle one of the great questions of philosophy here. But even if we believe, as I do, that an objective science of ethics exists, and even if we believe still further that ethical judgments are within the province of the historian or social scientist, one thing is certain: praxeology, economic theory, cannot itself establish ethical judgments. How could it when it deals with the formal fact that men act rather than with the content of such actions? Furthermore, praxeology is not grounded on any value judgments of the praxeologist, since what he is doing is analyzing the fact that people in general have values rather than inserting any value judgments of his own.

What, then is the proper relationship of praxeology to values or ethics? Like other sciences, praxeology provides laws about reality, laws that those who frame ethical judgments disregard only at their peril. In brief, the citizen, or the “ethicist,” may have framed, in ways which we cannot deal with here, general ethical rules or goals. But in order to decide how to arrive at such goals, he must employ all the

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relevant conclusions of the various sciences, all of which are in *themselves* value-free. For example, let us suppose that a person's goal is to improve his health. Having arrived at this value—which I would consider to be rational and others would consider purely emotive and arbitrary—the person tries to discover how to reach his goal. To do so, he must employ the laws and findings, value-free in themselves, of the relevant sciences. He then extends the judgment of “good,” as applied to his health, on to the means he believes will further that health. His end, the improvement of his health, he pronounces to be “good”; he then, let us say, adopts the findings of medical science that *x* grams of vitamin C per day will improve his health; he therefore extends the ethical pronouncement of “good”—or, more technically, of “right”—to taking vitamin C as well. Similarly, if a person decides that it is “good” for him to build a house and adopts this as his goal, he must try to use the laws of engineering—in themselves value-free—to figure out the best way of constructing that house. Felix Adler put the relationship clearly, though we may question his use of the term *social* before science in this context:

The . . . end being given, the ethical formula being supplied from elsewhere, social science has its most important function to discharge in filling in the formula with a richer content, and, by a more comprehensive survey and study of the means that lead to the end, to give to the ethical imperatives a concreteness and definiteness of meaning which otherwise they could not possess. Thus ethical rule may enjoin upon us to promote . . . health, . . . but so long as the laws of hygiene remain unknown or ignored, the practical rules which we are to adopt in reference to health will be scanty and ineffectual. The new knowledge of hygiene which social science supplies will enrich our moral code in this particular. Certain things which we freely did before, we now know we may not do; certain things which we omitted to do, we now know we ought to do.¹

Praxeology has the same methodological status as the other sciences and the same relation to ethics. Thus, to take a deliberately simple example: if our end is to be able to find gasoline when we pull

¹Felix Adler, “The Relation of Ethics to Social Science,” in *Congress of Arts and Science*, H.J. Rogers, ed. (Boston: Houghton Mifflin, 1906), vol. 7, p. 678.

up to the service station, and value-free praxeological law tells us—as it does—that, if the government fixes a maximum price for any product below the free-market price, a shortage of that product will develop, then (unless other goals supervene) we will make the ethical pronouncement that it is “bad” or “wrong” for the government to impose such a measure. Praxeology, like the other sciences, is the value-free handmaiden of values and ethics.

To our contention that the sciences, including praxeology, are in themselves value-free, it might be objected that it is values or ethics that direct the *interest* of the scientist in discovering the specific laws of his discipline. There is no question about the fact that medical science is currently far more interested in discovering a cure for cancer than in searching for a cure for some disease that might only have existed in parts of the Ukraine in the eighteenth century. But the unquestioned fact that values and ethics are important in guiding the attention of scientists to specific problems is irrelevant to the fact that the laws and disciplines of the science itself are value-free. Similarly, Crusoe on his desert island may not be particularly interested in investigating the science of bridge building, but the laws of that science itself are value-free.

Ethical questions, of course, play a far smaller role in applied medicine than they do in politics or political economy. A basic reason for this is that generally the physician and his patient agree—or are supposed to agree—on the end in view: the advancement of the patient’s health. The physician can advise the patient without engaging in an intense discussion of their mutual values and goals. Of course, even here, the situation is not always that clear-cut. Two examples will reveal how ethical conflicts may arise: first, the patient needs a new kidney to continue to live; is it ethical for the physician and/or the patient to murder a third party and extract his kidney? Second, is it ethical for the physician to pursue medical research for the possible good of humanity while treating his patient as an unwitting guinea pig? These are both cases where valuational and ethical conflicts enter the picture.

In economic and political questions, in contrast, ethical and value conflicts abound and permeate society. It is therefore impermissible for the economist or other social scientist to act as if he were a physician, who can generally assume complete agreement on values and goals with his patient and who can therefore prescribe

accordingly and with no compunction. Since, then, praxeology provides no ethics whatsoever but only the data for people to pursue their various values and goals, it follows that it is impermissible for the economist *qua* economist to make any ethical or value pronouncements or to advocate any social or political policy whatsoever.

The trouble is that most economists burn to make ethical pronouncements and to advocate political policies—to say, in effect, that policy X is “good” and policy Y “bad.” Properly, an economist may only make such pronouncements in one of two ways: either (1) to insert his own arbitrary, *ad hoc* personal value judgments and advocate policy clearly on that basis; or (2) to develop and defend a coherent ethical system and make his pronouncement, not as an economist, but as an ethicist, who also uses the data of economic science. But to do the latter, he must have thought deeply about ethical problems and also believe in ethics as an objective or rational discipline—and precious few economists have done either. That leaves him with the first choice: to make crystal clear that he is speaking not as an economist but as a private citizen who is making his own confessedly arbitrary and *ad hoc* value pronouncements.

Most economists pay lip service to the impermissibility of making ethical pronouncements *qua* economist, but in practice they either ignore their own criteria or engage in elaborate procedures to evade them. Why? We can think of two possible reasons. One is the disreputable reason that, if Professor Doakes advocates policy X and basically does so as an economics professor, he will be listened to and followed with awe and respect; whereas if he advocates policy X as plain Joe Doakes, the mass of the citizenry may come to the perfectly valid conclusion that their *own* arbitrary and *ad hoc* value judgments are just as good as his, and that therefore there is no particular reason to listen to him at all. A second and more responsible reason might be that the economist, despite his professed disbelief in a science of ethics, realizes deep down that there is something unfortunate—we might even say *bad*—about unscientific and arbitrary value judgments in public policy, and so he tries desperately to square the circle, in order to be able to advocate policy in some sort of scientific manner.

While squaring this circle is impossible, as we shall consider further, I believe that this putative uneasiness at making arbitrary value judgments is correct. While it is surely admirable (ethical?) for an

economist to distinguish clearly and carefully between the value-free science and his own value judgments, I contend further that it is the responsibility of any scientist, indeed any intellectual, to refrain from any value judgment whatever *unless* he can support it on the basis of a coherent and defensible ethical system. This means, of course, that those economists who, on whatever grounds, are not prepared to think about and advance an ethical system should strictly refrain from any value pronouncements or policy conclusions at all. This position is of course itself an ethical one. But it relates to the ethical system that is the precondition of all science; for, even though particular scientific laws are themselves value-free, the very procedures of science rest on the ethical norm of honesty and the search for truth; that norm, I believe, includes the responsibility to lend coherence and system to all one's pronouncements including valuational ones. I might add in passing that anyone conceding the necessity of honesty in science *ipso facto* becomes willy-nilly a believer in objective ethics, but I will leave that point to the ethical subjectivists to grapple with.²

Let me clarify with an example. Henry C. Simons, after trenchantly criticizing various allegedly scientific arguments for progressive taxation, came out flatly in favor of progression as follows:

The case for drastic progression in taxation must be rested on the case against inequality—on the ethical or aesthetic judgment that the prevailing distribution of wealth and income reveals a degree (and/or kind) of inequality which is distinctly evil or unlovely.³

My point is that, while it was surely admirable for Simons to make the distinction between his scientific and his personal value judgments crystal clear, that is not enough for him to escape censure. He had, at the very least, the responsibility of analyzing the nature and implications of egalitarianism and then attempting to defend it

²See the critique of the inconsistency of the championing of intellectual honesty by the great opponent of objective ethics, Max Weber, in Leo Strauss, *Natural Right and History* (Chicago: University of Chicago Press, 1953), pp. 47–48.

³Henry C. Simons, *Personal Income Taxation* (1938), pp. 18–19, cited by Walter J. Blum and Harry Kalven, Jr., *The Uneasy Case for Progressive Taxation* (Chicago: University of Chicago Press, 1953), p. 72.

as an ethical norm. Flat declarations of unsupported value judgments should be impermissible in intellectual, let alone scientific, discourse. In the intellectual quest for truth it is scarcely sufficient to proclaim one's value judgments as if they must be accepted as tablets from on high and not be themselves subject to intellectual criticism and evaluation.

Suppose, for example, that Simons's ethical or esthetic judgment was not on behalf of equality but of a very different social ideal. Suppose that instead he had come out in favor of the murder of all short people, of all adults under five feet six inches in height. And suppose that his sole defense of this proposal were the following:

The case for the liquidation of all short people must be rested on the case against the existence of short people—on the ethical or aesthetic judgment that the prevailing number of short adults is distinctly evil or unlovely.

One wonders if the reception accorded to Simons's remarks by his fellow economists or social scientists would have been quite the same.⁴ Yet, of course, the logic of his stance would have been precisely the same.

More usual is an attempt by the economist to place himself in the status of the physician of our foregoing example, that is, as someone who is merely agreeing to or ratifying the values either of a majority in society or of every person in it. But even in these cases, it must be remembered that the physician is in no sense value-free, though he is simply sharing the value of his patient, and that the value of health is so deeply shared that there is no occasion for making it explicit. Nevertheless, the physician *does* make a value judgment, and, even if every person in society shares the same value and goal, the economist who goes along with such a value is still making a value judgment, even if indeed universally shared. He is still illegitimately going beyond the bounds of the economist *per se*, and his value judgments must still be supported by rational argument.

⁴Murray N. Rothbard, *Egalitarianism as a Revolt against Nature, and Other Essays* (Washington, D.C.: Libertarian Review Press, 1974), pp. 2–3; also see Rothbard, *Power and Market* (Menlo Park, Calif.: Institute for Humane Studies, 1970), pp. 157–60.

The weakest path to an economist's adoption of social values is to appeal to the majority. Thus, John F. Due commented on the progressive income tax in his text on public finance:

The strongest argument for progression is the fact that the consensus of opinion in society today regards progression as necessary for equity. This is, in turn, based on the principle that the pattern of income distribution, before taxes, involves excessive inequality (which) can be condemned on the basis of inherent unfairness in terms of the standards accepted by society.⁵

But once again the fact that the majority of society might hold market inequality to be "unfair" does not absolve Due of the fact that, in ratifying that judgment, he himself made that value judgment and went beyond the province of the economist. Furthermore, on scientific standards, the *ad hoc* and arbitrary value judgments of the majority are no better than those of one person, and Due, like Simons, failed to support that judgment with any sort of argumentation. Furthermore, when we ratify the majority, what of the rights or the utilities of the minority? Felix Adler's strictures against the utilitarian ethic clearly apply here:

Other sociologists frankly express their ideals in terms of quantity and, in the fashion of Bentham, pronounce the greatest happiness of the greatest number to be the social end, although they fail to make it intelligible why the happiness of the greater number should be cogent as an end upon those who happen to belong to the lesser number.⁶

Again, with Due as with Simons, one wonders about the treatment of such a position by the American intellectual community if his imprimatur on the "consensus of opinion in society today" had been applied instead to the treatment of the Jews in Germany in the 1930s.

Just as the physician who advises his client commits himself to the ethic of good health, so the economist who advises a client is *not*, much as he would like to think so, a mere technician who is not committing himself to the value judgment of his client and his client's

⁵John F. Due, *Government Finance* (Homewood, Ill.: Richard D. Irwin, 1954), pp. 128–29.

⁶Adler, "Relation of Ethics," p. 673.

goals. By advising a steel company on how to increase its profits, the economist is thereby committed to share in the steel entrepreneur's value judgment that his greater profit is a desirable goal. It is even more important to make this point about the economist who advises the State. In so doing, he commits himself to the value judgments, not simply of the majority of society as in the case of Due, but to the value judgments of the rulers of the State apparatus. To take a deliberately dramatic example, let us suppose that an economist is hired by the Nazis to advise the government on the most efficient method of setting up concentration camps. By agreeing to help make more efficient concentration camps, he is agreeing to make them "better," in short, he is committing himself willy-nilly to concentration camps as a desirable goal. And he would, again, still be doing so even if this goal were heartily endorsed by the great majority of the German public. To underscore this point, it should be clear that an economist whose value system leads him to oppose concentration camps might well give such advice to the German government as to make the concentration camps as *inefficient* as possible, that is to sabotage their operations. In short, whatever advice he gives to his clients, a value commitment by the economist, either for or against his clients' goals, is inescapable.⁷

A more interesting variant of the economist's attempt to make value-free value judgments is the "unanimity principle," recently emphasized by James M. Buchanan. Here the idea is that the economist can safely advocate a policy if *everyone* in the society also advocates it. But, in the first place, the unanimity principle is still subject to the aforementioned strictures: that, even if the economist simply shares in everyone else's value judgment, he is still making a value judgment. Furthermore, the superficial attractiveness of the unanimity principle fades away under more stringent analysis; for unanimity is scarcely sufficient to establish an ethical principle. For one thing, the requirement of unanimity for any action or change begins with and freezes the status quo. For an action to be adopted, the justice and ethical propriety of the status quo must first be established, and of course economics can scarcely be prepared to do that.

⁷Murray N. Rothbard, "Value Implications of Economic Theory," *The American Economist* 17 (Spring, 1973): 38–39; included in this volume as chapter 12.

The economist who advocates the unanimity principle as a seemingly value-free pronouncement is thereby making a massive and totally unsupported value judgment on behalf of the status quo. A stark but not untypical example was the debate in the British Parliament during the early nineteenth century on the abolition of slavery, when early adherents of the “compensation principle” variant of the unanimity principle (which has its own additional and grave problems) maintained that the masters must be compensated for the loss of their investment in slaves. At that point, Benjamin Pearson, a member of the Manchester School, declared that “he had thought it was the slaves who should have been compensated.”⁸ Here is a striking example of the need in advocating public policy of some ethical system, of a concept of justice. Those ethicists among us who hold that slavery is unjust would always oppose the idea of compensating the masters and would rather think in terms of reparations to compensate the slaves for their years of oppression. But what is there for the value-free economist to say?

There are other grave problems with the compensation principle as a salvaging attempt to make it possible for value-free economists to advocate public policy. For the compensation principle assumes that it is conceptually possible to measure losses and thereby to compensate losers. But since praxeology informs us that “utility” and “cost” are purely subjective (psychic) concepts and therefore cannot be measured or even estimated by outside observers, it becomes impossible for such observers to weigh “social costs” and “social benefits” and to decide that the latter outweigh the former for any public policy, much less to make the compensations involved so that the losers are no longer losers. The usual attempt is to measure psychic losses in utility by the monetary price of an asset; thus, if a railroad damages the land of a farmer by smoke, it is assumed that the farmer’s loss can be measured by the market price of the land. But this ignores the facts that the farmer may have a psychic attachment to the land that puts its value far above the market price and that—especially in this kind of situation that does not involve direct action and exchange by the individuals—it is impossible to find out what

⁸William D. Grampp, *The Manchester School of Economics* (Stanford, Calif.: Stanford University Press, 1960), p. 59; also see Rothbard, “Value Implications,” pp. 36–37.

the farmer's psychic attachment to the land may be worth. He may say, for example, that his attachment to the land requires the compensation of \$10 million, even though the market price is \$100,000, but of course he may be lying. However, the government or other outside observer has no scientific way of finding out one way or another.⁹ Furthermore, the existence in the society of just one militant anarchist, whose psychic grievance against government is such that he cannot be compensated for his psychic disutility from the existence of government, is enough by itself to destroy the social-utility and compensation-principle case for any government action whatever. And surely at least one such anarchist exists.

Can praxeological economics, then, say nothing about social utility? Not quite. If we define an "increase in social utility" in the Paretian manner as a situation where one or more persons gain in utility while nobody loses, then praxeology finds a definite, but restricted, role for the concept. But it is a role where social utilities remain unmeasurable and incomparable between persons. Briefly, praxeology maintains that when a person acts, his utility, or at least his *ex ante* utility, increases; he expects to enjoy a psychic benefit from the act, otherwise he would not have done it. When, in a voluntary free-market exchange, for example, I buy a newspaper from a newsdealer for 15 cents, I demonstrate by my action that I prefer (at least *ex ante*) the newspaper to the 15 cents, while the newsdealer demonstrates by his action the reverse order of preference. Since each of us is better off by the exchange, both the newsdealer and I have demonstrably gained in utility, while *nothing* has demonstrably happened to anyone else. Elsewhere I have called this praxeological concept "demonstrated preference," in which action demonstrates preference, in contrast to various forms of psychologizing, which tries to measure other persons' value scales apart from action, and to behaviorism, which assumes that such values or preferences do not exist.¹⁰ The compensation principle that

⁹For a further analysis of this question, see Walter Block, "Coase and Demsetz on Private Property Rights: A comment," *Journal of Libertarian Studies* 1, no. 2 (Spring, 1977): 112–15.

¹⁰Murray N. Rothbard, "Toward a Reconstruction of Utility and Welfare Economics" in *On Freedom and Free Enterprise: Essays in Honor of Ludwig von Mises*, Mary Sennholz, ed. (Princeton, N.J.: Van Nostrand, 1956), pp. 224–32, 243–63; included in this volume as chapter 17.

I have been criticizing rests on the illegitimate psychologizing notion that a scientific economist-observer can know *anything* about someone else's value scale except as it is demonstrated through such action as the purchase or sale of a newspaper. And since the compensation principle is necessarily divorced from demonstrated preference, it cannot be employed by scientific economists. Incidentally, I might note here that "demonstrated preference" is very different from Samuelson's famous concept of "revealed preference," for Samuelson, in illegitimate psychologizing fashion, assumed the existence of an underlying preference scale that forms the basis of a person's action and that remains constant in the course of his actions over time. There is, however, no warrant for the scientific economist to make any such assumption. All we can say is that an action, at a specific point of time, reveals some of a person's preferences *at that time*. There is no warrant for assuming that such preference orderings remain constant over time.¹¹

Now since praxeology shows, by the concept of demonstrated preference, that both the newsdealer and I gain in utility from the exchange, and nothing has demonstrably happened to anyone else, we can conclude scientifically, as praxeological economists, that social utility has increased from the sale and purchase of the newspaper—since we have defined social utility in the Paretian manner. It is true, of course, that third parties may well be grinding their teeth in hatred at the exchange. There may be people, for example, who through envy suffer psychic loss because the newspaper dealer and/or I have gained. Therefore, if we employ the Paretian definition of "social utility" in the usual psychologizing sense, we can say nothing about social utility one way or the other. But if we confine the concept to its strict scientific compass in demonstrated preference, then we can state that social utility increases from the exchange. Still further, we may know as historians, from interpretive understanding of the hearts and minds of envious neighbors, that they do lose in utility. But we are trying to determine in this paper precisely what scientific economists can say

¹¹Ibid., pp. 228–30; also see Ludwig von Mises, *Human Action: A Treatise on Economics* (New Haven, Conn.: Yale University Press, 1949), pp. 102–04. Samuelson's views may be found, among other places, in Paul A. Samuelson, "The Empirical Implications of Utility Analysis," *Econometrica* 6 (October 1938): 334–56; and *Foundations of Economics* (Cambridge, Mass.: Harvard University Press, 1947), pp. 146–63.

about social utility or can advocate for public policy, and since they must confine themselves to demonstrated preference, they must affirm that social utility has increased.

Conversely, since every act of the State involves coercion, at least the coercion of taxation, and since in its every act there is at least one demonstrable loser in utility, we must also conclude that no act whatever of the State can increase social utility. Here, of course, is another good reason why the economic scientist cannot use the concept of “social utility” to establish any sort of unanimity principle or any other case for government action. It has been pointed out that, similarly, we cannot say that any action of the State *decreases* social utility, at least in the short term, and that too is correct.

We must emphasize, however, that the praxeological conclusion that the free market maximizes social utility is not sufficient to enable the praxeological economist to advocate the free market while abstaining from value judgments or from an ethical system. In the first place, why *should* an economist favor increasing social utility? This in itself requires an ethical or value judgment. And, second, the social-utility concept has many other failings, including the fact that while the envious and the egalitarian or the admirer of coercion *per se* may not be included in the social-utility concept, the contemporary historian knows that he is there, lurking in the wings; it therefore requires an ethical judgment, which cannot be supplied by praxeology, to overrule him. Furthermore, many of the strictures against the unanimity principle apply here too; for example, should we really be eager to preserve the utility of the slaveholder against loss? And if so, why?

Let us now turn to the position of Ludwig von Mises on the entire matter of praxeology, value judgments, and the advocacy of public policy. The case of Mises is particularly interesting, not only because he was a leader in the modern Austrian School and in praxeology, but also because he was, of all the economists in the twentieth century, the most uncompromising and passionate adherent of *laissez-faire* and at the same time the most rigorous and uncompromising advocate of value-free economics and opponent of any sort of objective ethics. How then did he attempt to reconcile these two positions?¹²

¹²For a posing of this question, see William E. Rappard, “On Reading von Mises,” in *On Freedom and Free Enterprise*, Mary Sennholz, ed., pp. 17–33.

Essentially, Mises offered two very different solutions to this problem. The first is a variant of the unanimity principle. Essentially this variant affirms that an economist *per se* cannot say that a given governmental policy is “good” or “bad.” However, if a given policy will lead to consequences, as explained by praxeology, that *every one* of the supporters of the policy will agree is bad, then the value-free economist is justified in calling the policy a “bad” one. Thus, Mises wrote:

An economist investigates whether a measure *a* can bring about the result *p* for the attainment of which it is recommended, and finds that *a* does not result in *p* but in *g*, an effect which even the supporters of the measure *a* consider undesirable. If the economist states the outcome of his investigation by saying that *a* is a bad measure, he does not pronounce a judgment of value. He merely says that from the point of view of those aiming at the goal *p*, the measure *a* is inappropriate.¹³

And again:

Economics does not say that . . . government interference with the prices of only one commodity . . . is unfair, bad, or unfeasible. It says, that it makes conditions worse, not better, *from the point of view of the government and those backing its interference.*¹⁴

Now this is surely an ingenious attempt to allow pronouncements of “good” or “bad” by the economist without making a value judgment; for the economist is supposed to be only a praxeologist, a technician, pointing out to his readers or listeners that they will all consider a policy “bad” once he reveals its full consequences. But ingenious as it is, the attempt completely fails. For how could Mises *know* what the advocates of the particular policy consider desirable? How could he know what their value scales are now or what they will be when the consequences of the measure appear? One of the great contributions of praxeology, as I have pointed out above, is that the praxeologist, the economist, doesn’t know what anyone’s value scales are except as those value preferences are demonstrated by a person’s concrete action. In the case of my purchase of the

¹³Mises, *Human Action*, p. 879.

¹⁴*Ibid.*, p. 758; italics in the original.

newspaper, historians or psychologists may make more or less informed estimates of the newsdealers' or my value scales through the process of interpretive understanding, but all that the economist can know scientifically and with certainty is the preference relative to 15 cents or the newspaper as demonstrated through concrete action. Mises himself emphasized that

one must not forget that the scale of values or wants manifests itself only in the reality of action. These scales have no independent existence apart from the actual behavior of individuals. The only source from which our knowledge concerning these scales is derived is the observation of a man's actions. Every action is always in perfect agreement with the scale of values or wants because these scales are nothing but an instrument for the interpretation of a man's acting.¹⁵

Given Mises's own analysis, then, how can the economist know what the motives for advocating various policies really are or how people will regard the consequences of these policies?

Thus, Mises, *qua* praxeologist, might show that price controls (to use his example) will lead to unforeseen shortages of a good to the consumers. But how could Mises know that some advocates of price controls do not *want* shortages? They may, for example, be socialists, anxious to use the controls as a step toward full collectivism. Some may be egalitarians who prefer shortages because the rich will not be able to use their money to buy more of the product than poorer people. Others may be one of the legion of contemporary intellectuals who are eternally complaining about the excessive affluence of our society or about the great waste of energy; they may all delight in the shortages of goods. Still others may favor price controls, even after learning of the shortages, because they or their political allies will enjoy well-paying jobs or power in a price-control bureaucracy. All sorts of such possibilities exist, and *none* of them is compatible with the assertion of Mises, as a value-free economist, that all supporters of price controls—or of any other government intervention—must concede, after learning economics, that the measure is “bad.” In fact, once Mises conceded that even a single advocate of price controls or any other interventionist measure may acknowledge the economic

¹⁵Ibid., p. 95.

consequences and still favor it, he could no longer call any of these measures “bad” or “good” or even “appropriate” or “inappropriate” without inserting into his economic policy pronouncements the very value judgments that he himself held to be inadmissible as a scientist of human action.¹⁶ He would no longer be a technical reporter to all advocates of a certain policy but an advocate participating on one side of a value conflict.

Moreover, there is another fundamental reason for advocates of “inappropriate” policies to refuse to change their minds even after hearing and acknowledging the praxeological chain of consequences. For praxeology may indeed show that all types of government policies will have consequences that most people, at least, will tend to abhor. But, and this is a vital qualification, most of these consequences take *time*, some a great deal of time. No economist has done more than Ludwig von Mises to elucidate the universality of time preference in human affairs—the praxeological law that everyone prefers to attain a given satisfaction sooner than later. And certainly Mises, as a value-free scientist, could never presume to criticize anyone’s rate of time preference, to say that A’s was “too high” and B’s “too low.” But, in that case, what about the high-time-preference people in society who retort to the praxeologist: “Perhaps this high tax and subsidy policy will lead to a decline of capital; perhaps even the price control will lead to shortages, but I don’t care. Having a high time preference, I value more highly the short-run subsidies, or the short-run enjoyment of buying the current good at cheaper prices, than the prospect of suffering the future consequences.” And Mises, as a value-free scientist and opponent of any concept of objective ethics, *could not* call them wrong. There is no way that he could assert the superiority of the long run over the short run without overriding the values of the high-time-preference people; and that could not be cogently done without abandoning his own subjectivist ethics.

In this connection, one of Mises’s basic arguments for the free market is that, on the market, there is a “harmony of the rightly understood interests of all members of the market society.” It is clear from his discussion that he could not merely mean “interests” after

¹⁶Mises himself conceded at one point that a government or a political party may advocate policies for “demagogic,” that is, for hidden and unannounced, reasons (*ibid.*, p. 104n).

learning the praxeological consequences of market activity or of government intervention. He also, and in particular, meant people's long-run interests. As he stated, "For 'rightly understood' interests we may as well say interests 'in the long run.'"¹⁷ But what about the high-time-preference folk, who prefer to consult their short-run interests? How can the long run be called "better" than the short run? Why is "right understanding" necessarily the long run?

We see, therefore, that Mises's attempt to advocate *laissez-faire* while remaining value-free, by assuming that all of the advocates of government intervention will abandon their position once they learn of its consequences, falls completely to the ground. There is another and very different way, however, that Mises attempted to reconcile his passionate advocacy of *laissez-faire* with the absolute value-freedom of the scientist. This was to take a position much more compatible with praxeology, by recognizing that the economist *qua* economist can only trace chains of cause and effect and may not engage in value judgments or advocate public policy. In so doing, Mises conceded that the economic scientist cannot advocate *laissez-faire* but then added that as a citizen he can do so. Mises, as a *citizen*, proposed a value system but it is a curiously scanty one. For he was here caught in a dilemma. As a praxeologist he knew that he could not as an economic scientist pronounce value judgments or advocate policy. Yet he could not bring himself simply to assert and inject arbitrary value judgments. And so, as a utilitarian (for Mises, along with most economists, was indeed a utilitarian in ethics, although a Kantian in epistemology), he made only one narrow value judgment: that he desired to fulfill the goals of the majority of the public (happily, in this formulation, Mises did not presume to know the goals of *everyone*).

As Mises explained in his second variant:

Liberalism (i.e., *laissez-faire* liberalism) is a political doctrine. . . . As a political doctrine liberalism (in contrast to economic science) is not neutral with regard to values and ultimate ends sought by action. It assumes that all men or at least the majority of people are intent upon attaining certain goals. It gives them information about the means suitable to the realization of their plans. The champions of liberal doctrines are fully aware of the fact that their

¹⁷Ibid., p. 670 and note.

teachings are valid only for people who are committed to their valuational principles. While praxeology, and therefore economics too, uses the terms happiness and removal of uneasiness in a purely formal sense, liberalism attaches to them a concrete meaning. It presupposes that people prefer life to death, health to sickness . . . abundance to poverty. It teaches men how to act in accordance with these valuations.¹⁸

In this second variant, Mises successfully escaped the self-contradiction of being a value-free praxeologist advocating *laissez-faire*. Granting in this variant that the economist may not make such advocacy, he took his stand as a citizen willing to make value judgments. But he was not willing, as Simons was, to simply assert an *ad hoc* value judgment; presumably he felt that a valuing intellectual must present some sort of system to justify such value judgments. But for Mises the utilitarian, his system is a curiously bloodless one; even as a valuing *laissez-faire* liberal, he was only willing to make *the one* value judgment that he joined the majority of the people in favoring their common peace, prosperity, and abundance. In this way, as an opponent of objective ethics, and uncomfortable as he must have been with making any value judgments even as a citizen, he made the minimal possible degree of such judgments; true to his utilitarian position his value judgment is the desirability of fulfilling the subjectively desired goals of the bulk of the populace.

A full critique of this position must involve a critique of utilitarian ethics itself, and this cannot be done here. But a few points may be made. In the first place, while praxeology can indeed demonstrate that *laissez-faire* will lead to harmony, prosperity, and abundance, while government intervention leads to conflict and impoverishment,¹⁹ and while it is probably true that most people value the former highly, it is not true that these are their *only* goals or values. The great analyst of ranked value scales and diminishing marginal utility should have been more aware of such competing values and goals. For example, many people, whether through envy or a misplaced theory of justice, may prefer far more equality of income than will be attained on the free market. Many people, *pace* the aforementioned intellectuals, may want less abundance in order to whittle down our

¹⁸Ibid., pp. 153–54.

¹⁹Rothbard, *Power and Market*, pp. 194–96.

allegedly excessive affluence. Others, as I have mentioned, may prefer to loot the capital of the rich or the businessman in the short run, while acknowledging but dismissing the long-run ill effects, because they have high time preference. Probably very few of these people will want to push statist measures to the point of total impoverishment and destruction—although this may happen, as in the case of Communist China. But a majority coalition of the foregoing might well opt for *some* reduction in wealth and prosperity on behalf of these other values. They may well decide that it is worth sacrificing a modicum of wealth and efficient production because of the high opportunity costs of not being able to enjoy an alleviation of envy, or a lust for power, or a submission to power, or, for example, the thrill of “national unity,” which they might enjoy from a (short-lived) economic crisis.

What could Mises reply to a majority of the public who have indeed considered all the praxeological consequences and still prefer a modicum—or, for that matter, even a drastic amount—of statism in order to achieve some of their competing goals? As a utilitarian, he could not quarrel with the ethical nature of their chosen goals: for he had to confine himself to the *one* value judgment that he favored the majority achieving their chosen goals. The only reply that Mises could make within his own framework was to point out that government intervention has a cumulative effect, that eventually the economy must move either toward the free market or toward full socialism, which praxeology shows will bring chaos and drastic impoverishment, at least to an industrial society. But this too, is not a fully satisfactory answer. While many programs of statist intervention—especially price controls—are indeed cumulative, others are not. Furthermore, the cumulative impact takes such a long time that the time preferences of the majority would probably lead them, in full acknowledgement of the consequences, to ignore the effect. And then what?

Mises attempted to use the cumulative argument to answer the contention that the majority of the public prefer egalitarian measures even knowingly at the expense of a portion of their own wealth. Mises’s comment was that the “reserve fund” was on the point of being exhausted in Europe, and therefore that any further egalitarian measures would have to come directly out of the pockets of the masses through increased taxation. Mises assumed that once this became clear, the masses would no longer support interventionist

measures.²⁰ In the first place, this is no argument against the *previous* egalitarian measures or in favor of their repeal. But secondly, while the masses *might* be convinced, there is certainly no apodictic certainty involved; the masses have in the past and presumably will in the future continue knowingly to support egalitarian and other statist measures on behalf of other goals, despite the knowledge that their income and wealth would be reduced. Thus, as William E. Rappard pointed out in his thoughtful critique of Mises's position:

does the British voter, for instance, favor confiscatory taxation of large incomes primarily in the hope that it will redound to his material advantage, or in the certainty that it tends to reduce unwelcome and irritating social inequalities? In general, is the urge towards equality in our modern democracies not often stronger than the desire to improve one's material lot?²¹

Rappard also noted that in his own country, Switzerland, the urban industrial and commercial majority of the country have repeatedly, and often at popular referendums, endorsed measures to subsidize the minority of farmers in a deliberate effort to retard industrialization and the growth of their own incomes. The urban majority did not do so in the "absurd belief that they were thereby increasing their real income." Instead, "quite deliberately and expressly, political parties have sacrificed the immediate material welfare of their members in order to prevent, or at least somewhat to retard, the complete industrialization of the country. A more agricultural Switzerland, though poorer, such is the dominant wish of the Swiss people today."²² The point here is that Mises, not only as a praxeologist but also as a utilitarian liberal, could have no word of criticism against these statist measures *once* the majority of the public take their praxeological consequences into account and choose them anyway on behalf of goals other than wealth and prosperity.

Furthermore, there are other types of statist intervention that clearly have little or no cumulative effect and that may even have very little effect in diminishing production or prosperity. Let us, for

²⁰Mises, *Human Action*, pp. 851–55.

²¹Rappard, "On Reading von Mises," pp. 32–33.

²²*Ibid.*, p. 33.

example, assume—and this assumption is not very farfetched in view of the record of human history—that the great majority of a society hate and revile redheads, perhaps, to cite Simons again, because they find redheads “evil or unlovely.” Let us further assume that there are very few redheads in the society. This large majority then decide that they would like very much to murder all redheads. Here they are; the murder of redheads is high on the value scales of the great majority of the public; there are few redheads so that there will be little loss in production on the market. How could Mises rebut this proposed policy either as a praxeologist or as a utilitarian liberal? I submit that he could not do so.

Mises made one further attempt to establish his position, but it was even less successful. Criticizing the arguments for state intervention on behalf of equality or other moral concerns, he dismissed them as “emotional talk.” After reaffirming that “praxeology and economics . . . are neutral with regard to any moral precepts,” and asserting that “the fact that the immense majority of men prefer a richer supply of material goods to a less ample supply is a datum of history; it does not have any place in economic theory,” he concluded by insisting that “he who disagrees with the teachings of economics ought to refute them by discursive reasoning, not by . . . the appeal to arbitrary, allegedly ethical standards.”²³

But I submit that this will not do; for Mises would have to concede that no one can decide upon *any* policy whatever unless he makes an ultimate ethical or value judgment. But since this is so, and since according to Mises all ultimate value judgments or ethical standards are arbitrary, how then could he denounce these *particular* ethical judgments as “arbitrary”? Furthermore, it was hardly correct for Mises to dismiss these judgments as “emotional,” since for him as a utilitarian, reason cannot establish ultimate ethical principles, which can therefore only be established by subjective emotions. It was pointless for Mises to call for his critics to use “discursive reasoning” since he himself denied that discursive reasoning can be used to establish ultimate ethical values. Furthermore, the man whose ultimate ethical principles would lead him to support the free market

²³Ludwig von Mises, “Epistemological Relativism in the Sciences of Human Action,” in *Relativism and the Study of Man*, Helmut Schoeck and James W. Wiggins, eds. (Princeton, N.J.: D. Van Nostrand, 1961), p. 133.

could also be dismissed by Mises as equally “arbitrary” and “emotional,” even if he takes the laws of praxeology into account before making his ultimately ethical decision. And we have seen above that the majority of the public very often have other goals which they hold, at least to a certain extent, higher than their own material well-being.

The burden of this paper has been to show that, while praxeological economic theory is extremely useful for providing data and knowledge for framing economic policy, it cannot be sufficient by itself to enable the economist to make any value pronouncements or to advocate any public policy whatsoever. More specifically, Ludwig von Mises to the contrary notwithstanding, neither praxeological economics nor Mises’s utilitarian liberalism is sufficient to make the case for *laissez-faire* and the free-market economy. To make such a case, one must go beyond economics and utilitarianism to establish an objective ethics that affirms the overriding value of liberty and morally condemns all forms of statism, from egalitarianism to the murder of redheads, as well as such goals as the lust for power and the satisfaction of envy. To make the full case for liberty, one cannot be a methodological slave to every goal that the majority of the public might happen to cherish.

In Defense of “Extreme Apriorism”

The stimulating methodological controversy between Professors Machlup and Hutchison proves that there are sometimes more than two sides to every question.¹ In many ways, the two are debating at cross-purposes: Professor Hutchison is primarily tilting against the methodological (and political) views of Professor Ludwig von Mises; his most serious charge is that Professor Machlup’s entire position is, at bottom, an attempt to cloak the Misesian heresy in the garments of epistemological respectability. Professor Machlup’s reply, quite properly, barely mentions Mises; for, in fact, their methodological views are poles apart. (Machlup’s position is close to the central “positivist” tradition of economic methodology.) But, in the meanwhile, we find that Professor Mises and “extreme apriorism” go undefended in the debate. Perhaps an extreme apriorist’s contribution to this discussion may prove helpful.

First, it should be made clear that neither Professor Machlup nor Professor Hutchison is what Mises calls a *praxeologist*, that is, neither believes (a) that the fundamental axioms and premises of economics are absolutely true; (b) that the theorems and conclusions deduced by the laws of logic from these postulates are therefore absolutely true; (c) that there is consequently no need for

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¹Terence W. Hutchison, “Professor Machlup on Verification in Economics,” *Southern Economic Journal* (April 1956): 476–83; Fritz Machlup, “Rejoinder to a Reluctant Ultra-Empiricist,” *ibid.*, pp. 483–93.

empirical “testing,” either of the premises or the conclusions; and (d) that the deduced theorems could not be tested even if it were desirable.² Both disputants are eager to test economic laws empirically. The crucial difference is that Professor Machlup adheres to the orthodox positivist position that the *assumptions* need not be verified so long as their deduced consequents may be proven true—essentially the position of Professor Milton Friedman—while Professor Hutchison, wary of shaky assumptions takes the more empirical—or institutionalist—approach that the assumptions had better be verified as well.

Strange as it may seem for an ultra-apriorist, Hutchison’s position strikes me as the better of the two. If one must choose between two brands of empiricism, it seems like folly to put one’s trust in procedures for testing only *conclusions* by fact. Far better to make sure that the assumptions also are correct. Here I must salute Professor Hutchison’s charge that the positivists rest their case on misleading analogies from the epistemology of physics. This is precisely the nub of the issue. All the positivist procedures are based on the physical sciences.³ It is physics that knows or can know its “facts” and can test

²The praxeological tradition, though named only recently, has a long and honored place in the history of economic thought. In the first great methodological controversy in our science, John Stuart Mill was the positivist and Nassau Senior the praxeologist, with J.E. Cairnes wavering between the two positions. Later on, the praxeologic method was further developed by the early Austrians, by Wicksteed, and by Richard Strigl, reaching its full culmination in the works of Ludwig von Mises. Mises’s views may be found in *Human Action* (New Haven, Conn.: Yale University Press, 1949), and in his earlier *Grundprobleme der Nationalökonomie* [translated into English as *Epistemological Problems of Economics* (Princeton, N.J.: D. Van Nostrand, 1960)]. On the similarity between Senior and Mises, see Marian Bowley, *Nassau Senior and Classical Economics* (New York: Augustus M. Kelley, 1949), chap. 1, esp. pp. 64–65. Lionel Robbins’s *Essay on the Nature and Significance of Economic Science* (London: Macmillan, 1932) was emphatically praxeologic, although it did not delve into the more complex methodological problems.

³On the differences between the methodologies of praxeology and physics, see Murray N. Rothbard, “Toward a Reconstruction of Utility and Welfare Economics,” in *On Freedom and Free Enterprise: Essays in Honor of Ludwig von Mises*, Mary Sennholz, ed. (Princeton, N.J.: D. Van Nostrand, 1956), pp. 226ff.; included in this volume as chapter 17.

its conclusions against these facts, while being completely ignorant of its ultimate assumptions. In the sciences of human action, on the other hand, it is impossible to test conclusions. There is no laboratory where facts can be isolated and controlled; the “facts” of human history are complex ones, resultants of many causes. These causes can only be isolated by theory, theory that is necessarily *a priori* to these historical (including statistical) facts. Of course, Professor Hutchison would not go this far in rejecting empirical testing of theorems; but, being commendably skeptical of the possibilities of testing (though not of its desirability), he insists that the assumptions be verified as well.

In physics, the ultimate assumptions cannot be verified directly, because we know nothing directly of the explanatory laws or causal factors. Hence the good sense of not attempting to do so, of using false assumptions such as the absence of friction, and so on. But false assumptions are the reverse of appropriate in economics. For human action is not like physics; here, the ultimate assumptions are what is clearly known, and it is precisely from these given axioms that the corpus of economic science is deduced. False or dubious assumptions in economics wreak havoc, while often proving useful in physics.⁴

Hence, Professor Hutchison is correct in wishing to establish the assumptions themselves. But these premises do not have to be (indeed, cannot be) verified by appeal to statistical fact. They are established, in praxeology, on a far more certain and permanent basis as definitely true. How, then, are these postulates obtained?

⁴This holds also for Professor Machlup’s “heuristic principles” which area allegedly “empirically meaningful” without being verifiable as true.

I do not wish to deny that false assumptions *are* useful in economic theory, but only when they are used as *auxiliary constructs*, *not* as premises from which empirical theories can be deduced. The most important such construct is the *evenly-rotating economy*, or “equilibrium.” It is not intended that this state be considered as *real*, either actual or potential. On the contrary, the empirically *impossible* ERE is constructed precisely in order to analyze theoretically a state of no-change. Only by analyzing a fictive changeless state can we arrive at a proper analysis of the changing real economic world. However, this is not a “false” assumption in the sense used by the positivists, because it is an absolutely true theory of a changeless state, if such a state could exist.

Actually, despite the “extreme *a priori*” label, praxeology contains one Fundamental Axiom—the axiom of *action*—which may be called *a priori*, and a few subsidiary postulates which are actually empirical. Incredible as it may seem to those versed in the positivist tradition, from this tiny handful of premises the whole of economics is deduced—and deduced as absolutely true. Setting aside the Fundamental Axiom for a moment, the empirical postulates are: (a) small in number, and (b) so broadly based as to be hardly “empirical” in the empiricist sense of the term. To put it differently, they are so generally true as to be *self-evident*, as to be seen by all to be obviously true once they are stated, and hence they are not in practice empirically falsifiable and therefore not “operationally meaningful.” What are these propositions? We may consider them in decreasing order of their generality: (1) the most fundamental—variety of resources, both natural and human. From this follows directly the division of labor, the market, etc.; (2) less important, that *leisure is a consumer good*. These are actually the only postulates needed. Two other postulates simply introduce limiting subdivisions into the analysis. Thus, economics can deductively elaborate from the Fundamental Axiom and Postulates (1) and (2) (actually, only Postulate 1 is necessary) an analysis of Crusoe economics, of barter, and of a monetary economy. All these elaborated laws are absolutely true. They are only *applicable* in concrete cases, however, where the particular limiting conditions apply. There is nothing, of course, remarkable about this; we can enunciate as a law that an apple, unsupported, will drop to the ground. But the law is applicable only in those cases where an apple is actually dropped. Thus, the economics of Crusoe, of barter, and of a monetary economy are applicable when these conditions obtain. It is the task of the historian, or “applied economist,” to decide which conditions apply in the specific situations to be analyzed. It is obvious that making these particular identifications is simplicity itself.

When we analyze the economics of indirect exchange, therefore, we make the simple and obvious limiting condition (Postulate 3) that indirect exchanges are being made. It should be clear that by making this simple identification we are not “testing the theory”; we are simply choosing that theory which applies to the reality we wish to explain.

The fourth—and by far the least fundamental—postulate for a theory of the market is the one which Professors Hutchison and

Machlup consider crucial—that firms always aim at maximization of their money profits. As will become clearer when I treat the Fundamental Axiom below, this assumption is by no means a necessary part of economic theory. From our Axiom is derived this absolute truth: that every firm aims always at maximizing its *psychic* profit. This may or may not involve maximizing its *money* profit. Often it may not, and no praxeologist would deny this fact. When an entrepreneur deliberately accepts lower money profits in order to give a good job to a ne'er-do-well nephew, the praxeologist is not confounded. The entrepreneur simply has chosen to take a certain cut in monetary profit in order to satisfy his consumption-satisfaction of seeing his nephew well provided. The assumption that firms aim at maximizing their *money* profits is simply a convenience of analysis; it permits the elaboration of a framework of *catallactics* (economics of the market) which could not otherwise be developed. The praxeologist always has in mind the proviso that where this subsidiary postulate does *not* apply—as in the case of the ne'er-do-well—his deduced theories will not be applicable. He simply believes that enough entrepreneurs follow monetary aims enough of the time to make his theory highly useful in explaining the real market.⁵

We turn now to the Fundamental Axiom (the nub of praxeology): *the existence of human action*. From this absolutely true axiom can be spun almost the whole fabric of economic theory. Some of the *immediate* logical implications that flow from this premise are: the means-ends relationship, the time-structure of production, time-preference, the law of diminishing marginal utility, the law of optimum returns, etc. It is this crucial axiom that separates praxeology from the other methodological viewpoints—and it is this axiom that supplies the critical “a priori” element in economics.

First, it must be emphasized that whatever role “rationality” may play in Professor Machlup’s theory, it plays no role whatever for

⁵I do not mean to endorse here the recent strictures that have been made against the monetary-profit maximization assumption—most of which ignore *long-run* as opposed to short-run maximization.

The curious idea that failure to pursue monetary goals is “irrational,” or refutes economics, is similar to the old notion that consumers were being irrational, or “uneconomic,” when they preferred to pay higher prices in stores nearer to them, or with a more congenial atmosphere.

Professor Mises. Hutchison charges that Mises claims “all economic action was (or must be) rational.”⁶ This is flatly incorrect. Mises assumes nothing whatever about the rationality of human action (in fact, Mises does not use the concept at all). He assumes nothing about the wisdom of man’s ends or about the correctness of his means. He “assumes” only that men *act*, that is, that they have some ends, and use *some* means to try to attain them. This is Mises’s Fundamental Axiom, and it is this axiom that gives the whole praxeological structure of economic theory built upon it its absolute and apodictic certainty.

Now the crucial question arises: how have we obtained the truth of this axiom? Is our knowledge *a priori* or empirical, “synthetic” or “analytic”? In a sense, such questions are a waste of time, because the all-important fact is that the axiom is self-evidently true, self-evident to a far greater and broader extent than the other postulates. For this Axiom is true for all human beings, everywhere, at any time, and could not even *conceivably* be violated. In short, we may conceive of a world where resources are not varied, but not of one where human beings exist but do not act. We have seen that the other postulates, while “empirical,” are so obvious and acceptable that they can hardly be called “falsifiable” in the usual empiricist sense. How much more is this true of the Axiom, which is not even conceivably falsifiable!

Positivists of all shades boggle at self-evident propositions. And yet, what is the vaunted “evidence” of the empiricists but the bringing of a hitherto obscure proposition into *evident* view? But some propositions need only to be stated to become at once evident to the self, and the action axiom is just such a proposition.

Whether we consider the Action Axiom “a priori” or “empirical” depends on our ultimate philosophical position. Professor Mises, in the neo-Kantian tradition, considers this axiom a *law of thought* and therefore a categorical truth *a priori* to all experience. My own epistemological position rests on Aristotle and St. Thomas rather than Kant, and hence I would interpret the proposition differently. I would consider the axiom a *law of reality* rather than a law of thought, and hence “empirical” rather than “a priori.” But it should be obvious that this type of “empiricism” is so out of step with modern empiricism that

⁶Hutchison, “Professor Machlup on Verification in Economics,” p. 483.

I may just as well continue to call it *a priori* for present purposes. For (1) it is a law of reality that is not conceivably falsifiable, and yet is empirically meaningful and true; (2) it rests on universal *inner* experience, and not simply on external experience, that is, its evidence is *reflective* rather than physical;⁷ and (3) it is clearly *a priori* to complex historical events.⁸

The epistemological pigeon-holing of self-evident propositions has always been a knotty problem. Thus, two such accomplished Thomists as Father Toohey and Father Copleston, while resting on the same philosophical position, differ on whether self-evident propositions should be classified as “a posteriori” or “a priori,” since they define the two categories differently.⁹

From the Fundamental Axiom is derived the truth that everyone tries always to maximize his utility. Contrary to Professor Hutchison,

⁷See Professor Knight's critique of Hutchison's Significance and Basic Postulates of Economic Theory. Frank H. Knight, “What is Truth in Economics?” *Journal of Political Economy* (February 1940): 1–32.

⁸Professor Hutchison may have had me in mind when he says that in recent years followers of Professor Mises try to defend him by saying he really meant “empirical” when saying “a priori.” Thus, see my “Praxeology, Reply to Mr. Schuller,” *American Economic Review* (December 1951): 943–44; included in this volume as chapter 7. What I meant is that Mises's fundamental axiom may be called “a priori” or “empirical” according to one's philosophical position, but is in any case *a priori* for the practical purposes of economic methodology.

⁹Thus, Copleston calls self-evident principles “synthetic propositions *a priori*” (though not in the Kantian sense)—synthetic as conveying information about reality not contained logically in previous premises; and *a priori* as being necessary and universal. Toohey virtually obliterates the distinctions and terms self-evident propositions synthetic—a posteriori, because, while being necessary and universals, they are derived from experience. See F.C. Copleston, S.J., *Aquinas* (London: Penguin Books, 1955), pp. 28 and 19–41; John J.H. Toohey, S.J., *Notes on Epistemology* (Washington, D.C.: Georgetown University, 1952), pp. 46–55. All this raises the question of the usefulness of the whole “analytic-synthetic” dichotomy, despite the prominence implicitly given it in Hutchison's “Significance and Basic Postulates of Economic Theory,” *Journal of Political Economy* 49 (1934). For a refreshing skepticism on its validity, and for a critique of its typical use to dispose of difficult-to-refute theories as either disguised definitions or debatable hypotheses, see Hao Wang, “Notes on the Analytic-Synthetic Distinction,” *Theoria* 21 (Parts 2–3, 1955): 158ff.