EPISTEMOLOGY AND ECONOMICS CONTRIBUTION TO THE LOGICAL ANALYSIS OF ECONOMIC THEORY

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INTRODUCTION

The present study is intended mainly as a criticism of a position in the methodology of economics and a position in methodology in general which I want to call *literalism*. Now, for literalism I am going to understand the property of a methodological system (most conspicuous in aprioristic epistemological conceptions but not confined to them) which treats language as something given and inert, not subject to essential revision or to any dialectical change of its own. Literalism sees language either as a "filing system" not affected by the content it harbors, or as "knowledge-ofour-own-mind" which is not a result of its commerce and intercouse with the life of experience.

The literalistic position is common to both apriorists and objectivists in economic methodology. Therefore it is suitable to formulate that position in the fashion of a dilemma, the *literalistic dilemma*: "Either economic theory is abstract (the result of the abstractive operation of the mind upon empirical information) or it is a priori (the elaboration of something one finds in his own intellect); one cannot have it both ways." This writer does not accept the validity of this dilemma; the answer to its challenge, though, cannot be straight-forward. The *prima facie* force of the dilemma is the product of an oversimplification of the problem of knowledge and it is only through a complete examination of the epistemological context of the question that one can be enabled to see that the exclusive argument does not hold. This examination will appear in the first two parts of this study.

I will begin by discussing a conception of scientific knowledge which makes it ultimately dependent on intellectual passions and values to which scientists are committed. I will also point to the fact that it is not possible to make a sharp distinction between pure theory and pure empirical happening. Furthermore, I will discuss the fact that values and theories are always encountered in interrelation, forming systems, and that these systems, dwelt in by different persons, are likely to conflict with one another. The upshot of all this is a plea for an epistemology based on tolerance and a hope that conflicting bona fide views might eventually be reconciled by means of mutual respect and heuristic discussion.

I will then examine the claim that there is an essential distinction between *common sense* and *science* which is basic for the further distinction between the natural sciences and the social sciences. I will conclude that the relevant distinction is not that between common-sense and scientific knowledge, but rather the one between formal and non-formal, even unformalizable knowledge. I will conclude that it is impossible to support objectivity and formalism in an objective and formal way. The only reassurance we can expect to obtain in this field is the one based on confidence in the responsibility of the scientist. That responsability, coupled with an efficient use of formalism, is what I want to call the *professional sense*.

The central idea of this essay will then be developed around the distinction between two levels of subjectivity present in the social sciences: subjectivity in the explanation and subjectivity in the subject-matter. The latter aspect relates to the fact that the objects of study in the social sciences do have purposes, knowledge, desires, and the like. The former aspect relates to the fact that the scientist himself has also purposes, beliefs, passions, and the like which are determinante in the way in which he actually makes science. In connection with this distinction I will construe the temporation of the social scientist as the tendency to identify two levels of analysis which should be kept separate. I will draw the conclusion that all sciences are methodologically homogeneous in the twofold sense that both social and physical sciences are ultimately dependent on subjectivity (that of the scientist) and both use subjectivity-in-the-subject-matter or an alternate to it, i.e., theoretical terms.

I will present in the second part of the study an analysis of the nature and uses of abstraction. I will try show that the abstractionist's "analytical realism" is an insufficient account of the multi-dimensionality of the real world. In order to do this, I will present a philosophical examination of the logical requirements of *completeness* and *consistency*. The conception that all theoretical thinking is totalizing and systematic, and at the same time somehow limited or incomplete, will be the re-encounter in the strictly methodological realm of the fundamental truth of the interrelateness of values and the conflicting character of the relationships between different systems of value. Ambiguity, I will conclude, is inherent to all thinking bearing on reality. We cannot hope *to see through* our theoretical framework to capture the "real world." Our way to truth must be related to the analysis of competing comprehensive views, which I call *paradigms*, according to some internal criteria of coherence and fullness of account they are capable of giving of the different aspects of reality.

Having taken sides with the coherence criterion of truth I will then move to overcome the risk of relativism in such an outlook. The capacity to be coherent is not unlimited for a paradigm. Its ability to make ultimate sense can and in fact does become exhausted. This possibility of exhaustion or depletion of a paradigm or set of paradigms may be regarded as a limiting psychological property of the person asserting or entertaining the paradigm. At the empirical end, the capability of the person for making changes in a paradigm in order to save it from adverse evidence may become exhausted; the paradigm must then be dropped as false. At the theoretical end, the ability of the person to imagine alternative explanations can also become depleted, and the paradigm must be asserted as necessarily true.

The two points of exhaustion of paradigms are only ideal points, never fully attainable, only complementary poles asymptotically approached. In between those two foci one finds the real instances of knowledge, indefinite in gradation, in a state of tension from (ideal) necessary theory to (ideal) empirical fact. Of any concrete piece of information one can say that it is both theoretical and empirical, according to the role it plays in relation to other, higher or lower, pieces of knowledge. The concepts of *model* and *assumption* will then be introduced. A piece of information is a model, a close enough representation of reality, if one works on it from a higher level of knowledge; a piece of information is an assumption, a theoretical or paradigmatic configuration, if one is dwelling in it in order to work from it on something else (the models). Provided we are at a sufficient distance from both necessary assumptions and overwhelming evidence, there is much leeway to alternative organization of models and assumptions. Many approaches may prove equally fruitful; several alternative laws equally true. We call this view the *gradualistic interpretation of scientific theory*.

With these epistemological weapons at hand, I will examine, in the third chapter, the claim of the social scientist who defends *Verstehen* as an independent, intuitive, way to knowledge. I will conclude that the literalist's "knowledge-of-our-own-mind" is in fact a knowledge-of-our-own-language, of paradigmatic sort. I will also examine, in the seventh chapter, the claim of the economist who defends "praxeology" as a deductive, purely a priori, way of constructing economic theory. I will conclude that the literalist's transcendental deduction is in fact a dialectical process of inference, of empirical sort. Finally, in the fourth part, I will try to build a direct criticism of literalism by means of a construction of alternate models for the basic elements of economic value theory. These models will also serve as a disproof of the possibility of an a priori, literalistically formal, definition of economics. As a result of all this, the formal professional conception of economic science will be arrived at, a conception which actually merges with the gradualistic interpretation of scientific theory.

PART I

THE NON-FORMAL ASPECTS OF THEORY

CHAPTER I

SCIENCE AND VALUES

Toute philosophie est practique, même celle qui paraît d'abord la plus contemplative; la méthode est une arme social et politique ...¹

I am not going to support this epigram. Neither am I about to attempt its refutation. I am using it only to call attention to the truth, I think a very fundamental one, that it is impossible to state a sharp distinction, much less an opposition, between contemplative, pure theory, on the one hand, and praxis, historical action, or values on the other. I think, although the purpose of this study is not to prove it, that the radical position of values in the methodology of science, as emphasised, for example, by Max Weber in regard to the social disciplines and by Michael Polanyi with respect to the natural sciences, cannot be denied. I will take this conviction as basic for the sequel, and I hope that the fruitfulness of this premise for the treatment of our problem will tend to add strength to the premise itself. I will suppose in what is to follow that formalism as a method of science, the paramount usefulness of which I fully recognize, can function meaningfully and profitably only within a context made out of intellectual passions, heuristic and persuasive tendencies, and commitment to values.² Nevertheless, I am going to take for granted that values are objective, in the sense that they are not identical with circumstantial interest or arbitrary individual caprice. Rather, they are always asserted as having universal intent and are discovered as having-being-there-all-along. I am going to quarrel, however, with the interpretation of this objectivity as reducible to physical or empirical objectivity (whatever that might mean).

As against a naturalistic interpretation of value I will set forth the position that values, although dependent on empirical knowledge, do not let themselves be empirically deduced and are best perceived in conflict and struggle with the values of other persons.³ This struggle, however, takes place under a (partially) common firmament of values to which each one of the contending parties keeps some kind of responsible attachment and which each participant contributes somehow progressively to change. This common firmament of "superior knowledge"⁴ cannot possibly be reduced to purely empirical categories. Due to the widespread support that the naturalistic conception of values seems to have, especially in English-speaking countries, and also to the fact that its being true would be fatal for the position that I am going to maintain here, I will devote the next pages to the exposition and refutation of its principal arguments.

4 Polanyi, pp. 374-380.

¹ Jean-Paul Sartre, Critique de la raison dialectique (Paris: Librairie Gallimard, 1960), p. 16.

² Michael Polanyi, Personal Knowledge: Towards a Post-Critical Philosophy (New York and Evanston: Harper & Row, 1964).

³ Max Weber, Methodology of the Social Sciences, trans. and ed. Edward A. Shils and Henry A. Finch (Glencoe, Ill.: The Free Press, 1949), pp. 49-112.

EPISTEMOLOGY AND ECONOMICS

I will take C. E. Ayres's position, as it is expressed in a recent book,⁵ as a forceful articulation of the naturalistic interpretation of value. His central thesis is, I think, that in order to be significant for the progress of mankind, values have to be objective; otherwise, nobody could tell whether progress is being made or not. The model here is mechanical progress, the evolution of machines. It is originally with respect to tools that the concepts "good" and "bad" have meaning. With respect to other matters, ceremonial or sacred, values have just a derivative and erroneous meaning; mores and ritual are only simulacra of technical causality. Value is thus reducible to technical knowledge, i.e., the knowledge of the regularities of nature. Morality is reducible to expedience and ethics, the study of value, to science or the quest for empirical truth.

One feature of Ayres's otherwise excellent book strikes the reader unfavorably: its preaching mood, purporting to be a descriptive presentation of fact.⁶ It is to the credit of the author that he presents a forceful case for the prevalence of reason in social life. One of the shortcomings of his work is that it is written as if the arguing for its case were altogether unnecessary. The result risks being completely different from the attempted furtherance of reason in human communication, insofar as the attempt itself is not totally faithful to the defended policy: it tends to rest too heavily upon emotional persuasion. Witness the correlative abundant use of persuasive definitions: "There is only one form of knowledge, the knowledge man has acquired in the course of his technological activities"7 (and we lose the distinction between "knowledge" and "technology" necessary to formulate the liberal-arts criticism against technological specialism). "Our freedom is virtually synonymous with the fullness of life as it has been realized by industrial society"s (and we cannot say that industrial society is un-free). "Virtues prove on examination to be simplifi-cations of the technical necessities of organization"⁹ (and organization is virtuous always, provided it is technical). In all these cases it is clear that there is a pattern of attack on the possibilities of thought (and criticism). It produces a contraction of language that deprives us of some useful intellectual means of analysis and polemic. This, I think, is not reasonableness!

One could reply that it is unfair to see in those acute statements just cases of persuasive definitions for the simple reason that they can better be conceived as sharp and clear main theses of the book. I am ready to accept it. But then the conclusion forces itself upon us that it is all but impossible to present *non-persuasive* theses, that a definite assertion always affects in one way or another the very means of expressing assertions, language itself. This means that in the final analysis there is no real separation between *meaning* and *truth*. These conclusions are admitted by this writer, but I am convinced they are in clear contradiction with the thesis of the *natural* objectivity of value. Persuasion is essential to a sincere expression of belief natural objectivity is an impossible substitute for the inter-subjective participation that must prevail among bona fide contending parties.

Apart from that, Ayres's construction of an argument in favor of the objectivity of value is a good one, and very important. His insistence on the common nature of the process that originates all values is quite sound. I do not see any reason why one should have to indentify it with the development of tools. Nevertheless it is

- 7 Ibid., p. 107.
- 8 Ibid., p. 177.
- 9 Ibid., p. 267.

⁵ Clarence Edwin Ayres, Toward a Reasonable Society: The Values of Industrial Civilization (Austin: University of Texas Press, 1961).

^{6 &}quot;I am not saying that this is how we should make decisions. This is how we do make decisions..." (*ibid.*, p. 17).

true that a close relation between knowledge of cause-and-effect relationships and moral questions does exist and is determinant. It is as well true, I think, that in all societies two sets of values, sacred and profane, are distinguishable. It is not at all evident, however, that it is "the former which differ so widely from people to people, whereas the latter are the same for all."10 On the contrary, it is argued in philosophy of science¹¹ that scientific knowledge is dependent upon the acceptance of conceptual paradigms that not only vary with time but are even incommensurable among one another. On the other hand one can make a strong case for the basic coincidence of all religious world-views, as both an analysis of the nature of religious attitudes and the existence today of ecumenical movements would tend to demonstrate (religious strife being more often than not the result of worldly and possessive interests and of the clash between scholastic, quasi-scientific, theological paradigms). I do not see, furthermore, how one can consistently maintain both that man is culturally conditioned, as Ayres does,12 and that "good tools" means the same for everybody.¹³ One has only to consider the extremely difficult task that teaching the use of modern machinery is in the underdeveloped regions of the world in order to realize that a tool is not the straightforward thing the author wants us to take it to be. I would be inclined rather to say, with phenomenological philosophy, that the essence of a tool is altogether a function of our being-in-the world, and varies together with individual purposes and habits, and, in general, with the changes of the conditions of human existence.

I think the author is basically right in saying that "it is the dissociation of truth and value that defines the moral crises of the twentieth century."14 I even will go with him as far as accepting that "every culture has two aspects, which the terms secular and supernatural identify."¹⁵ I consider the term "supernatural" unfortunate and would have "transcendent" instead because of its relative freedom of emotional connotations. Again, I would admit also that "the significant distinction is not between * description and evaluation. It is rather between secular efforts of description and evaluation and those exercises of description and evaluation which employ supernatural premises."16 But I think that Ayres makes a blunder both in identifying "secular" with "true" or "valid" and "supernatural" with "false" or "invalid". In my opinion he is, in doing so, both too naive and too bold, too uncritical and much too critical. In particular, he shows a serious misunderstanding of the nature of myth in human life and in human history.17 I consider Ayres wrong when he says that "no one is ever called upon to decide whether to accept the universe or not... whether the existence of the human species is 'a good thing' or not." As a matter of fact, one is always being called to do precisely that, to the extent, that is, that one is a religious animal. After all, this is what it means to be religious. Ayres is begging the question when he adds, skillfully enough, that "no one could possibly do so, since the term 'good' has clear and definite meaning only with reference to the on-going life-process of mankind."18

¹⁰ Ibid., p. 6.

¹¹ Thomas S. Kuhn, The Structure of Scientific Revolutions (Chicago: The University of Chicago Press, 1964).

¹² Ayres, pp. 74 ff.

¹³ Ibid., p. 8.

¹⁴ Ibid., p. 49.

¹⁵ Ibid.

¹⁶ Ibid., p. 50.

¹⁷ Without making very much of it, I would define myth as the intellectual reaction of the human mind to problems of totality and stability; I strongly feel that man has to have a way of dealing with transcendent matters, i.e., matters which go beyond part articulation and change determination.

The problem with Ayres is that he wants to take "our existence as a species" and "the system of activities by which we live and to which we are irrevocably committed" as fundamentally given.19 This is fine as an individual or cultural decision, much in the spirit of the radical theologian's proposal of discontinuing transcendent discussion for a time, while doing our best to humanly solve our human problems. But it could be very wrong, I think, to the extent that it amounts to an obscurantist injunction to arrest all transcendent or religious inquiry. Again, even as a practical decision it would be open to question, because of its entailing the rejection of all social organization principle other than the technological one, i.e., the rejection of all status or ceremonial considerations. Ayres recognizes this when he says: "all mores define what is right and proper for persons of designated status ... As an organization its principal function is not that of getting things done, but rather that of preventing change."20 That might to be so; but still one has to acknowledge that man needs some stability in order to be able to live at all. "Regularities in nature" are not the only regularities that are relevant for human life. "Regularities in society" may prove as important! The author is right in warning against the dangers of the traditionalist principle: "Ceremonial adequacy require that tradition shall always be honored regardless of consequences—that is, of mere technological consequences "21 But one should complete the picture guarding against this absolutization of the progressivist principle: "Technological teamwork is efficient only to the extent that all considerations of status are ignored in single-minded commitment to getting the job done"22 regardless of mere personal or transcendent consequences. Where is one to stop, following this principle, short of Nazi-styled "final solutions"? One would prefer in this connection to repeat with Knight: "Values conflict and no one value can be absolute. ... The only near-absolute value is that of the best possible compromise . . . "23

I consider the notion of interrelatedness of value as the main "valid precipitate" (to borrow a Parsonian expression) of Ayres's position.24 His central contention in this respect is that nothing has value "in itself", that is, independent of the inter-connectness of all experiences in the life process. Immanent coherence or all-encompassing harmony, i.e., contribution to the total enhancement of human life is the criterion of value. Where I part company with the author is only in his view that because there is an immanent criterion of value ultimate questions are to be regarded as superstitious. For in fact every opinion of value is ultimate, in the sense that there is no attribution of value which does not take the whole of human life, although in an ideal or virtual fashion, as the root and foundation of its validity. Of course, in a way this is what Ayres is saying in his reference to the "life-process". But the evidence is obscured in the writer's outlook due to the fact that he believes in the unambiguous existence of a single common system of values, the objective (in the naturalistic sense) system. And what really exists is a set of value systems always in conflict and always changing, in dialogue -or polemic- with one another and in process of becoming clearer, more consistent and encompassing, somehow asymptotically approaching the (ideal and single) objective system. Value is the attitude

- 19 Ibid., p. 117.
- 20 Ibid., p. 126.
- 21 Ibid., p. 137.
- 22 Ibid., p. 136.
- 23 Frank H. Knight, "Theory of Economic Policy and the History of Doctrine," *Ethics*, LXIII (July, 1953), 281.
- 24 It is fair to say, though, that one can find that notion also elsewhere, sometimes even in a better formulation. Cf. for example Charner Perry, "The Semantics of Political Sciences," *The American Political Science Review*, Vol. XVIV (June, 1950).

of the person in his quest for ultimate truth. It is always provisional but also passionate and persuasive. A problem of values is not primarily a problem as to the validity of those personal attitudes. We cannot but think that they are valid. The primary and fundamental problem is how we can change or improve those attitudes so that the asymptotic approach to the ideal continues to be realized.²⁵ It is not a matter of wondering whether a value opinion is possible. We cannot avoid having one or another. It is precisely because of this unavoidability that value judgments are meta-empirical and necessary. Our problem is rather the problem of reformation of value opinion. That is why intellectual discussion is so important. Society can exist, that is can be free, only insofar as interests in conflict are subordinated to opinions about values. Moreover, it can be free only insofar as the disagreeing parties face their disagreement as a problem, recognizing in each other the will and capacity for heuristic discussion. Group action is free only if unanimous but hopeful discussion can be an efficacious substitute for actual unanimity. The collective pursuit of truth is an indispensable component in the definition of value, as it is of the essence of the ideal society.²⁰

We have come very far from the conception that all decisions or exercise of value opinions in life or society "are made by the same process by which one decides what to eat for breakfast—that is, by a simple calculation of cause and effect."²⁷ The distance consists mainly in the fact that Ayres assumes that there is no real problem in any decision, that value opinions must of necessity coincide, no justified conflict being at all possible. One has only to read out the regularities of nature! Contrariwise, it is fundamental to realize that a personal commitment to a system of value is involved in any important question, especially if it must affect several persons. Then the rational procedure is to iron out the differences of opinion, which is ideally done through the process of orderly intellectual discussion. In summation: Values do form systems, they are interrelated; but value systems are multiple and, more often than not, they conflict with one another.²⁸ We need systematism, multiplicity, and conflict. But also openness to dialogue and to possible modification of opinion.

One could still point to a missing trait in this axiological conception and try to complete the picture. Value is not simply opinion, however conflicting we would put it. It spells also habit and eagerness to resist change. Change of value opinion must be worked out as change of attitude, if it is at all to be effective. Work is indispensable,²⁹ even toilsome work. The final result is that "intellectual discussion" can be very painful, even violent and costly. Struggle might be needed to clarify values, both at the private and at the social levels. So, the quasi-Platonic conception drawn in the last paragraph must be corrected with a down-to-earth realistic element: the Parsonian category of "effort"³⁰ whose fate seems to be its being easily forgotten in academic presentations of the problem of value.

But even in the case of struggle and violence, one needs to be affiliated to the system in order to fight with it. Multiplicity of outloocks and inertial tendency to resist change do not mean impossibility of communication. One needs a common language to express and maintain one's polemic point. That common language

30 Parsons, p. 719.

²⁵ Cf. Frank H. Knight, "Social Science", Ethics, Vol. LI (January, 1941).

²⁶ Ibid.

²⁷ Ayres, p. 16.

²⁸ For an extensive and suggestive investigation into this problem, cf. Talcott Persons, The Structure of Social Action: A Study in Social Theory with Reference to a Group of Recent European Writers (New York and London: McGraw-Hill, 1937).

²⁹ Sartre, p. 20.

is the firmament which the parties both contribute to create and work under, the History that makes us and which we do make.³¹ This is also the cosmos whose objectivity claims a prior validity to the objectivity of empirical data and which makes sense observation and scientific reasoning possible, the non-formal social matrix in which all science develops.³²

31 Sartre, p. 64.

32 Cf. Knight, Ethics, LI, 143n. Also cf. Alfred Schutz. "Concept and Theory Formation in the Social Science," The Journal of Philosophy. Vol. LI (April, 1954).

CHAPTER II

COMMON SENSE AND SOCIAL SCIENCE; THE QUESTION OF TELEOLOGY

A fundamental point frequently made in the writings on the methodology of economics is the assertion that this science, as social science in general, is no more than an elaboration of the content and categories that one finds in common-sense knowledge. This argument is usually employed in order to support the concomitant assertion that social sciences and natural sciences have methods completely different from each other even at the logical level. On the other hand, one also encounters writers in sharp opposition to these views maintaining, on the contrary, that the categories of common sense must be superseded by more abstract categories in order for a particular science to exist at all and, as a corollary, that there is one, and only one, homogeneous method of science. I side in this controversy with the former authors, although I do not think that the continuity between social science and common sense is sufficient ground for maintaining the fundamental diversity of the methods of the social and the natural sciences. My view on the matter of how these two types of sciences are to be distinguished, and related, is to be made clear in the sequel. I consider entirely gratuituous and almost mythical the notion that the rise of a scientific conception must be understood as a rupture with the historical past and a creation of a self-contained, wholly new, interpretation of reality. On the contrary, I believe that here, as in the case of values, previous opinions are always fundamental. The problem of the advance of science has always been a problem of reformation of opinion, of clarification of accepted knowledge which we all, even scientists, have in a systematic form. On the other hand, it is not clear where to draw a line between common and "un-common" sense or knowledge, since the knowledge in the possession of intellectual elites has always been a source of utmost importance for the formation of common knowledge-the knowledge of the common people.

Even more importantly, if one is to treat this problem rigorously one should better avoid the use of such common-sense terms as "science" and "common sense." One should rather, in this context talk about formalism and nonformal or unformalized knowledge, even if these terms do not correspond completely in their extension of meaning with the former. If one does this, then one can easily see that not only the historical origin of sciences, but also the regulation of its formal apparatus and the application of its formal systems to concrete cases must be controlled by unformalized, even ultimately unformalizable thought. The attempt to formalize rules of application, for example, is bound to incur infinite regression.³⁸ That this regression does not in fact present itself in the actual functioning of science is what we refer to when we speak of a professional setting or a professional atmosphere in the practice of science.³⁴

³³ In order to formalize the application of a rule one has to have another rule, of a higher level, which says how the former rule is to be applied. To formalize the new rule one has to have still another rule, of an even higher level, and so on and so forth—to infinity.

³⁴ Cf. Milton Friedman, Essays in Positive Economics (Chicago: University of Chicago Press, 1953), p. 25.

EPISTEMOLOGY AND ECONOMICS

The overpreoccupation with formalism, and the making of sharp distinctions between common sense and science, is no doubt based on sound concerns and on the realization of an important problem. We have to have some criteria to distinguish between authentic science and "metaphysics"; we must be able to defend science against the pseudo-scientist.³⁵ Nevertheless, the only real protection against the pseudoscientist turns out to be reliance on organized science and on the sense of responsibility of the professional scientist. Other safeguards are at best insufficient. ³⁶ At worst, overpreoccupation with formal guarantees tends to degenerate into a new brand of obscurantism which could eventually arrest the development of the sciences. Objective tests are not enough to defend science against intruders. How are the tests themselves to be tested? The important question is whether the rules are to be applied always in the same way by the professional people. Consensus is of the essence of science—as much as it is of the essence of truth and of value.³⁷

In summation: One could profitably reinterpret the meaning of "common sense" as signifying non-formal, responsible thinking, and the meaning of "science" as formal, responsible thinking. Of course, the professional reality of science is more than pure formalism, and real common sense is more than purely non-formal thought. But the polarization is illuminating in that it shows more clearly what the issues involved are. It is interesting to note that this polar distinction makes of common sense (identified as non-formal thought) the governing level, because it supplies the conditions for the good operation of (formal) science. Compare this scheme with the claim sometimes made as to the correspondence of common sense with an objectlanguage and of science with the respective meta-language.38 This distinction of levels can, no doubt, also be made, but it certainly cuts across the boundaries between science and common sense. As a matter of fact, common sense itself moves in different levels of discourse, as the ample treasury of popular wisdom about the object of the social sciences (people) might very well demonstrate. There are "common-sense object-language" and "common-sense meta-language" as much as there are different levels of language within formal science itself. Here again it seems that we come to the same conclusion: Not the distinction between science and common sense, but rather the distinction between formal and non-formal, even unformalizable, thought or knowledge is fundamental. And this is only an ideal or abstract distinction. In practice formalism is absolutely incapable of existence without the full simultaneous cooperation of non-formal intellectual powers. In the face of this, we cannot look upon "objectivity" as the easy solution for our restlessness and need of security. We can only receive the reassurance which comes from responsibility in the use of formal and analytic devices by professional people. This responsibility coupled with a careful development and application of formalism is what I want to call the professional sense, an essential defining characteristic of a scientist.

Closely related to the problem of the relationships between science and common sense is the problem of the role of teleology in the explanation of the phenomena of the social sciences. Conflicting views are also encountered in this area. I will concentrate here on a significant pair of them, represented by the following revealing quotations:

- 35 T. W. Hutchison. The Significance and Basic Postulates of Economic Theory (London: Macmillan, 1938), p. 13.
- 36 Polanyi, pp. 53, 203-207.
- 37 Frank H. Knight, The Ethics of Competition and Other Essays (London: Bradford & Dickens, 1951), p. 118. Cf. also Frank H. Knight, On the History and Method of Economics (Chicago: The University of Chicago Press, 1956), pp. 153-155.
- 38 See Herbert A. Simon, "Discussion (of Perry's article)," The American Political Science Review. Vol. XLIV (June, 1950). "Meta-language" is a language which speaks about another language.

.....

Under a wide range of circumstances individual [business] firms behave as if they were seeking rationally to maximize their expected returns

[Confidence in the maximization-of-returns hypothesis is justified, however, by the fact that] unless the behavior of businessman in some way or other, approximated behavior consistent with the maximization of returns, it seems unlikely that they would remain in business for long....³⁹

The characteristic feature of man is ... that he consciously acts. Man is Homo agens, the acting animal.

All—apart from zoology—that has ever been scientifically stated to distinguish man from nonhuman mammals is implied in the proposition: man acts. To act means: to strive after ends, that is, to choose a goal and to resort to means in order to attain the goal sought.⁴⁰ (Italics mine.)

The first quotation seems to be a very puzzling piece of philosophy of science because expressing a circumstantial rather than a logical argument; hypotheses have, according to that argument, the nature of supreme judges of businessmanship because the falsifying instances will destroy themselves in the process of falsifying the theory. A parallel type of methodological interpretation will make of driving an automobile, or of any other practical skill based on definite "laws of nature," a very queer experience. Confidence in the rules of good driving would be justified because unless the behavior of drivers approximated behavior consistent with those rules it would seem unlikely that they would remain alive for long. In this way, one could hope, • the followers of the rules will be vindicated (!). Mises's solution would not fare very well either. Under rigid interpretation of conscious teleology one would have to assume that the driver makes complicated calculations of ends and means in the fraction of a second necessary for correct response to road stimuli.

One could still "save" Friedman's position by understanding his contention as saying that "as-if" hypothesis express only rules of correct behavior.⁴¹ But this will not eliminate teleology—implicit in the normative prescription. One could also "save" Mises by saying that strictly conscious teleological reasoning is only the prototype of actual decisions. I, for my part, find it easier and more convincing to think that the truth is not in either side. In my opinion, both businessmen and drivers are always making *heuristic guesses* as to the technique of conducting business or cars, rather than strict calculations or sheer random divination. They develop, in their daily personal contact with the trade, an habitual knowledge that we precisely call "art" or "skill." The heuristic framework which they dwell in for making their appraisals supposes teleology, of course, because they *intend* to guess right; but there is no clear awareness of the steps involved, nor minute arithmetics of ends and means. They only try to *guess* right. Contrary to Friedman, one has to accept that teleology is allpervading, purpose being connatural to human thought. But, contrary to Mises, one has to accept trial-and-error and the instauration of good habits as equally important.

But a complete analysis of the role of teleology in social sciences will have to go deeper. It is indispensable to distinguish between two different, although

39 Milton Friedman, Easays in Positive Economics, pp. 21-22.

⁴⁰ Ludwig Von Mises, The Ultimate Foundation of Economic Science (New York: D. Van Hostrand, 1962), pp. 4-5.

⁴¹ Cf. Felix Kaufmann, Methodology of the Social Sciences (New York: The Humanities Press, 1958), p. 217.

EPISTEMOLOGY AND ECONOMICS

complementary, teleological aspects. On the one hand we have teleology as a dimension of the explanation; on the other we have teleology as an element of the subject-matter. Teleology in-the-explanation corresponds to the purpose of the investigator and has to do with the role of subjectivity in the foundations of science, with the operation of non-formal powers of thought. Teleology in-the-subject-matter corresponds to "purpose" as an essential category for the understanding of behavior. It has to do with subjectivity in the object of thought rather than on the part of the scientist. For clarity's sake, let us refer to the first aspect as "teleology," or "subjectivity," and to the second aspect as "teleology2" or "subjectivity2." Teleology1 relates to the fact that the scientist, as the person that he is, has purposes, knowledge, desires, passions; all of this is, of course, determinant in the way he conducts his scientific activity, even contributes to define him as a professional scientist. Teleology₂ relates rather to the fact that the subject-matter which social scientists study, i.e., people, do also have purposes, beliefs, passions, etc., so that the corresponding terms "purpose," "belief," "passion," are part of the language the scientist must use. Those terms are best interpreted as theoretical terms, as for instance the term "force" is interpreted in the physical sciences: it is impossible to render them directly as descriptions of pure sense data. If one does this, that is, if one takes teleology₂ as a case of theoretical terminology, then the difference between the social and the physical sciences appears less dramatic. Both have to deal with some content-theoretical terms-that they do not fully understand, at least from the point of view of strictly empirical observation, and both have much the same way of dealing with that content that "they do not understand." They treat it in a formal way, applying to them the same kind of logical manipulation and rules of inference they apply to terms directly derived from sense experience.

It is interesting to witness the different approaches of writers on methodology according to their personal intellectual inclination for teleology₁. Let us compare the following two quotations of Knight, and Simon:

We*never succeed entirely in eliminating consciousness from our ideas of material things. . . . Mechanics has not been able to do without the notion of force, though whatever force is, more than motion, is a fact of consciousness. . . . We interpret the behavior of the most material thing by to some degree putting ourselves in its place. . . . 4^2

To say that it is often convenient to use the term "purpose" in social science means no more and no less than to say that it is often convenient to use the term "force" in physics. A physical force is no more directly observable than a social purpose. ... To bar from natural or social science all terms that do not refer to directly observable events is to confuse deduction with induction. All that we require of the language of science is that its propositions, whether directly observable or not, permit us to deduce other propositions that are directly observable and hence testable. Propositions about "purposes" and "desires" do permit such predictions and tests, and hence are not different from propositions in the natural sciences....⁴⁸

We could say that Simon explains "purpose" as a kind of "force," whereas Knight explains "force" as a sort of "purpose." Thus, the authors exemplify both approaches to the issue of teleology. Knight seems to be saying: "We place ourselves

⁴² Knight, The Ethics of Competition and Other Essays, p. 120.

⁴³ Simon, American Political Science Review, XLIV, 409.

in the stead of the object so that we can understand," even if the object is an inanimated thing. Simon seems to be saying contrariwise: "We make for ourselves theoretical terms, so that we can remain outside of the object to be explained." The first attitude exemplifies the dominance of a teleology₁ approach; the second attitude, the dominance of a teleology₂ approach. The former is dialectical or synthetical in the sense of craving for unity and generality; the latter, positive or analytical in the sense of craving for differential elements and objectivity. As absolutization of two complementary principles they may appear inadequate, even intellectually barren, the former because of prima facie tautological emptiness, the latter because self-destruction of the person of the knower. Yet, there is this important difference between the two. The first attitude is, in the end, self-validating, being avowedly circular; the second attitude is, in the end, self-contradictory, total objectivity being completely unobtainable.

There is here a fundamental asymmetry that we could express thus in schematic form. (1) The teleology1 approach-dialectical, synthetical-is not very fruitful at close range, but is self-validating and necessary in the last analysis. (2) The teleology₂ approach-positive, analytical-is in the end self-contradictory, but is very useful at close range and for particular purposes. The former is not fruitful at close range because nobody wants to be saying only things which are directly implied by the way we set up our vocabulary. One does not want to be always pronouncing tautologies. It is unavoidable in the last analysis because all language is ultimately circular—it is always developed through its own application (as we shall see more fully in the next chapter). The latter is in the end selfcontradictory because nobody can expect to know anything without being the one who is there to know about it-non-subjective knowledge would be a flat impossibility. It is useful, nevertheless and very much so, at limited range (within dependent hypotheses) because all logical inferences proceed by making esssential, although transient, use of inconsistencies.44 One can feel in all this the unmistakable scent of the two ultimate requisites of all systematism: the principle of completeness, and the principle of consistency. In the autonomy of the two approaches in particular, one can sense an epistemological version of the metamathematical idea that (sophisticated enough) formal systems cannot be proved both consistent and complete.45 More of that later. For the time being, let us note that we have come to a conclusion very similar to the one reached in the last section: Formalism is useful and indispensable, but its functioning necessitates the complementary full exercise of non-formal, ultimately unformalizable, powers of thought.

- 44 This is clearly so in the case of indirect proof or *reductio ad absurdum*; but it is not less so in the case of direct proof, for instance, *modus ponendo ponens*, where "ppq, p ∴ q" is translatable into "-pvq, p ∴ q." and, under the limited range of the first member of the disjunction, "-p . p"—a contradiction—must be derived to force the separation of "q." Thus, a contradiction within a dependent hypothesis—limited range—can be very useful notwithstanding the fact that as a straight premiss—full range—it would invalidate the whole line of proof (it would permit the "inference" of anything).
- 45 Kurt Goedel, "Ueber formal unentscheidbare Saetze der Principia Mathematica und verwandter Systeme," Monatshöfte fuer Mathematik und Physik, Vol. XXXVIII (1931).

CHAPTER III

VERSTEHEN

Let us now undertake the analysis of the method of *Verstehen*, traditionally regarded as the specific tool of the social sciences. I am going to present criticism to two contrasting texts, one written by a positivist,⁴⁶ and the other by an epistemological apriorist.⁴⁷. I will then introduce a third writer,⁴⁸ whose view on the matter I find much closer to the truth. The commentary of these three texts will offer sufficient opportunity to express my own opinion, as we go along.

I begin by summing up Abel's argument. His main contentation is that unless the operation called *Verstehen* is clearly defined, one cannot say how much validity its results have. He then goes on to work in precisely that definition, in order to be able to criticise the operation thus defined. His point of departure for the explication of *Verstehen* is the presentation of three different cases of application of the operation: to a single case, to a general case, and to a statistical case.

Single case: My neighbor makes a fire *because* there is a drop in temperature. I cannot be certain that this is the correct or true explanation of his conduct, though. He may rather want to show off his fireplace, or even his motive could be unconscious —he might be symbolically burning his house to punish the fellow who harasses him about the mortgage.

General case: "Faced by the insecurity of a changing and hostile world, we seek security by creating 'eternal varieties' in our thoughts." (Lundberg). Clearly the evidence to substantiate the validity of the interpretation is not sufficient. The connection asserted by the generalization is relevant. We understand it and consider it possible.

Statistical case: There has been found a high correlation between the annual rate of crop production and the rate of marriage in a given year in rural areas. The connection is relevant; but the verification is based on objective data.⁴⁹

Now, from the analysis of these three cases it is possible to build a definition or explication of *Verstehen*: "Two particulars... are characteristic of the act of *Verstehen*. One is the "internalizing" of observed factors in a given situation; the other is the application of a behavior maxim which makes the connection between these relevant." For example, in the general case mentioned before, we internalize "change and hostility" into "feeling of inadequacy" and the concept of "eternal verities" into "feeling of security." The corresponding behavior maxim is that a person who feels inadequate will seek security. Behavior maxims are generalizations

- 48 Sartre, pp. 61-98.
- 49 Abel, 212-214.

⁴⁶ Theodore Abel, "The Operation Called Verstehen," The American Journal of Sociology, Vol. LIV (November, 1948).

⁴⁷ F. A. Hayek, "The Facts of the Social Sciences," Ethics, Vol. LIV (October, 1943).

of direct personal experience (we take for granted that the emotions of others function similarly to our own). The result of the operation is to give the certainty that a given interpretation of behavior is a possible one; the sense of relevance is the result of personal experience—the connection has been established before, so one is certain of its possibility.⁵⁰

With this definition at his disposal, Abel proceeds then to criticize the operation of *Verstehen*. The core of this criticism is the assertion that from the affirmation of a possible connection one cannot conclude that it is also probable—the test of actual probability calls for the application of objective methods of observation different from the operation we are analyzing. Since *Verstehen* is totally dependent on knowledge we already possess, it cannot help us in our way to discovery. The operation can be helpful only in setting up hypotheses. It is unable to verify them. It relieves us of the apprehension which should haunt us if we could not understand the connection. But satisfaciton of curiosity produces only subjective increment; it "adds nothing to the objective validity of a proposition."⁵¹ In conclusion, "the operation of *Verstehen* does two things: It relieves us of a sense of apprehension in connection with behavior that is unfamiliar or unexpected and it is a source of 'hunches,' which help us in the formulation of hypotheses." More important is what the operation does *not* do:

The operation of *Verstehen* does not, however, add to our store of knowledge, because it consists of the application of knowledge already validated by personal experience; nor does it serve as a means of verification. The probability of a connection can be ascertained only by means of objective, experimental, and statistical tests.⁵²

Let us now examine Abel's argument, especially its conclusion. The last quoted proposition, in particular, is evidence of an ultra-empiricism that seems to ignore the problems, some of them acute to the point of hopelessness, which have been discussed in recent years on confirmation and probability theory. The plausible conclusion of that discussion is, contrary to Abel's assumptions, that the probability of a connection cannot be ascertained any more than the truth of the connection, apart from some ingredient, very difficult to specify, of a rather subjective nature.53 On the other hand, the contention that Verstehen does not add to our store of knowledge because it consists of the application of knowledge already validated by personal experience, proves a bit too much. If one is to limit himself to knowledge not yet validated by experience, then one has to discard all generalizations that are based on more than two or three directly observed instances. But this procedure would close the door to all acquisition of knowledge, except perhaps to the not interesting knowledge of the single and isolated type of cases which would not repeat themselves. Abel seems to be implying that generalization is not a means of discovery. This is untrue, as any scientist can tell. It is precisely generalization which is the way to discovery, for only general hypotheses can guide the scientist in his investigations.

As for the argument that from the "possible" one cannot get the "probable," I would like to ask a question. If "possible" means "what has been experienced before," and "probable" means (presumably) "what has been experienced before more than a couple of times" (it cannot mean "what is being experienced *now*") then why should I not be allowed to say that my "possible" is also "probable"? If it

⁵⁰ Ibid., pp. 213-216.

⁵¹ Ibid., pp. 216-17.

⁵² Ibid., p. 218.

⁵³ Cf. Israel Scheffler. The Anatomy of Inquiry (New York: Alfred A. Knopf, 1963).

EPISTEMOLOGY AND ECONOMICS

is because my "possible" has been experienced only a couple of times, Abel would then be denying that this is a genuine case of *Verstehen* (it would not consist of a generalization of personal experience). But if alternatively the reason for rejecting my "possible-probable" is that *already validated* knowledge is "all stuff" then all probabilistic knowledge, no matter how "objective," would be subject to equal rejection. All probabilistic knowledge has been already validated!

Abel is wrong, I think, in assuming a sharp distinction between "knowledge," in the sense of discovery or *new* knowledge, and "application" of it; that is, he endorses the positivist dogma which sees language only as a "filing system," and knowledge as a dichotomy of tautologies on the one hand and empirical content on the other. Application of knowledge is, for that view, simply a matter of "filling out" analytical theory with empirical content. That view is blind to the fact that all application of knowledge is also, to some extent, creation of new knowledge. One never fails to modify, however slightly, his previous information by the act of applying it to a "confirming instance." The result of it is that repeated application of the "same" knowlede will gradually change our whole vision of the world, although in each case we "only" apply knowledge "already validated."⁵⁴ Leibniz' adopted motto, "la nature no fait jamais de sauts," should still warn us to be prepared for surprises in this rich world of ours in which no two instances are ever exactly the same.

As to the positive part of Abel's conclusions, he says that the operation of *Verstehen* does two things: it relieves us of the sense of apprehension in the face of the unexpected, and it is a source of "hunches" that help us in the formulation of hypotheses. I think he is right. Moreover, I consider these two characteristics as a remarkable summary of the reasons why man does at all practice science. Thus, science is first of all a work of intellectual creativity, a heuristic rapture, so to speak. Besides, it is a work of intellectual curiosity. We create knowledge—hypotheses, theories—because we are rational animals and we enjoy our own production of ideas. But we also expect this knowledge to be of such a sort that it can satisfy our need of orientation in the world and "save the appearances" of disconcerting events. So, Abel has branded *Verstehen* as a highly scientific method! After that compliment one wonders not whether the operation is a legitimate tool for some fields of analysis, but rather whether it is not the perfect incarnation of the very method of science, for all fields.

Passing now to Hayek's text, there is not for him a question as to the validity of the method of "empathic understanding." Nevertheless, and as a consequence of his schooling in the doctrines of positives, he tries to define a role for the operation that does not endanger the positivist dichotomy between tautological theory and empirical content. In order to do this, he takes a course of reasoning much in the same line as the argument of Simon that we examined in the last chapter,⁵⁵ i.e. to recognize teleology in the subject-matter (teleology₂), but here with the frank eddition of an "aprioristic bridge" to assure adequate relevance to the explanations. Says he:

In discussing what we regard as other people's conscious actions, we invariably interpret their action on the analogy of our own mind: that is ... we group their actions, and the objects of their actions, into classes or categories which we know solely from the knowledge of our own mind....⁵⁶

⁵⁴ This is not to rule out revolutionary linguistic reform whenever the even application to new instances will impose strenuous modification to the old paradigm (see Part II, especially Chapter VI).

⁵⁵ Cf. pp. 25 ff. above.

⁵⁶ Hayek, Ethics, LIV, 5.

The problem that I see here is that "knowledge of our own mind" may not prove enough as a justification of (necessarily social) language: There must be some public criteria to judge words or, in general, meaningful actions, so that one does not have to rely on "inner voices" or secret "intentions" to understand words or actions. The author recognizes this when he states: "When we say that a person possesses food or money, or that he utters a word, we imply that he knows that the first can be eaten, that the second can be used to buy something with, and that the third can be understood...."57 But he fails to understand the real nature of the implication. It is not a matter of knowledge of "our own mind." It is a matter of knowledge of our own language, which implies by itself the existence of other minds. Let us recall the case of the neighbor that shows off his fireplace. There is a sense in which one could say that the "showing-off" is implied in the definition of "fireplace," although this is not the primary sense of the word. This not-being-the-primarysense-of-the-word is what gives us the low probability of this interpretation for the conduct of my neighbor. There are built-in probabilities inherent in every word we use. They reflect what we do and what we are, both socially and personally. They express, as it were, the human nature. This explains the fact thet "we can understand less and less as we turn to beings more and more different from ourselves...."58 What happens here is that in the extreme case the common language is lacking -there is no common nature.

Under this interpretation it is clearly illicit to separate, as the author does,⁵⁹ a system of concepts "that we can understand" from real knowledge about the world. The conceptual aspects of language-classification-are not separable from its pro-positional aspects-assertion-because the way in which our system of concepts is set up does always say a great deal about our experiences. The uses of the word are somehow conveyed in the word itself. Its possible applications are somehow predetermined in it. That is why every actual application is bound to produce a change in the linguistic framework, to the extent, that is, that the current configuration does not prove to be prophetic. Classification already says something about the real world. The relative probability of certain combinations implicit in the different degrees of significance-a fireplace can be used to warm up, but also to show off, and even to alleviate psychological complexes. We use the concepts thus classified as elements in the construction of our hypothetical models or ideal types with which we intend to reproduce the patterns of social relationships. In this sense theory is prior to history. It explains or constructs the terms which history must use.60 But let us not forget that history, specifically the history of the language, is prior, even logically prior, to theory. For we would not understand the concepts were it not for the fact we find ourselves able to perform common judgments. So it is simply not true that the "models can never possess any properties which we have not given to them...."61 The fact is rather, and wonderfully, that models do possess qualities that we have not actually put in them, that they continually surprise us with the heuristic riches they make manifest-of course, to the extent that they are true concepts (or fruitful, or relevant, or useful, or whatever word we prefer to use in this connection).62

- 57 Ibid.
- 58 Ibid., p. 7.
- 59 Ibid., p. 8.
- 60 Ibid., p. 10.
- 61 Ibid., p. 12.
- 62 I cannot avoid thinking in this connection of the magnificent myth of the pre-established harmony; in general, in the high degree of prophetic power that mythological concepts have proved to possess—witness the perennial contemporariness of Plato's vocabulary and images.

EPISTEMOLOGY AND ECONOMICS

The philosophy of Sartre is a practical philosophy. As a matter of fact, as already noted, he maintains that all philosophy, even the most contemplative, is essentially practical. The categories of his epistemology reflect this particular view. What conventional treatises present as duality of "object" and "subject," is introduced by him as the duality of "the given"—material conditions—and "the project"—human strivings or intentions. The project category is developed into a theory of meaning, where the Marxian conceptions of "objectivation" and "alienation" find their right places. Meanings come from man and his project, but they also do materialize all over in things and in the order of things: "L'homme construit des signes parce qu'il est signifiant dans sa réalité même et il est signifiant parce qu'il est dépassement dialectique de tout ce qui est simplement donné..." What we call freedom is the irreducibility of the order of culture to the order of nature.⁶³ Hence the categories can serve a purpose of scientific demarcation as well.

The problem of *Verstehen* can be adequately treated in this context. Understanding, necessary to grasp the sense of human conduct, is not a particular virtue or faculty of intuition, but rather the "dialectical motion" to explain an act by its terminal meaning, starting from the original conditions. "La compréhension n'est pas autre chose que ma vie réelle...", the integrative motion that involves my neighbor, myself, and the environment in the synthetic unity of an objectivation in the process of becoming.⁶⁴ Besides, the scheme permits also a commodious criticism of positivism, Marxian or not: "Il était légitime que les sciences de la nature se délivrassent de l'anthropomorphisme que consiste à prêter aux objects inanimés des propriétés humaines. Mais il est parfaitment absurde d'introduire par analogie le mépris de l'anthropomorphisme dans l'anthropologie..."⁶⁵

Sartre presents his method as heuristic, "both regressive and progressive." I take the term "progressive" to be related to discovery and teleology; "regressive," to empirical verification and purely operational deduction.⁶⁶ Within this framework the dialectic of the method imposes a *va-et-vient* ("back-and-forth") movement, for example from the determination of the biography to the determination of the epoch and vice versa. But we need also a differential element, that aspect of knowledge which can solely originate the progressive motion—like the priming of a pump, in order not to proceed in a vacuum.⁶⁷ I find this approach most attractive and promising. In particular, it seems to be useful in developing a conception of language which will not be forced to make any artificial distinction between meaning and truth, and which can offer a more realistic explanation of the actual operation of our universal means of communication. One learns the meaning of a word when one finds it essentially used in a system of true statements. On the other hand, the truth of the statements is better perceived when one has learned the meaning of the word—again, a back-and-forth motion.

In conclusion our analysis of *Verstehen* has led us to a germinal conception as to the nature of language which is consistent with the results of the previous chapters. The importance of formalism is recognized—Sartre's regressive element together with the importance, even more radical, of the non-formal, the heuristic or dialectical aspects of thought—Sartre's progressive motion. We are ready, I think, to go into a closer consideration of basic patterns of formalism which we are likely to encounter in the social sciences, especially in economics.

- 63 Sartre, p. 96.
- 64 Ibid., p. 97.
- 65 Ibid., p. 98.
- 66 Ibid., p. 86.
- 67 Ibid., p. 87.

PART 11

THE FORMAL ASPECTS OF THEORY

CHAPTER IV

ABSTRACTION AND THE MULTIPLE DIMENSIONS OF REALITY

It is the essence of the positivist dogma that reality is exhaustible, i.e., unambiguously amenable to be fitted into a (single) "filing system,"68 presumably a logical system similar to that of Principia Mathematica. The dogma operates on the assumption of a basic dichotomy between "meaning" and "truth" which (as we saw in the first part of this study) is very difficult to maintain consistently. A separable tautological frame of reference is conceivable only at a given moment of time, at a given point in the history of knowledge. No advance of knowledge is possible by "filling-out" the framework, because the framework itself must be affected by every application to new material. The reaction against positivism, on the other hand, is found, generally speaking, among authors who stress the multidimensional nature of our understanding of reality by means of the analytical operation of abstraction. Within this "abstractionist" field, however, elements of positivism are still traceable, so that several groups of thinkers must be distinguished according to the degree of remaining positivist outlook.⁶⁹ In the first place, we have writers like Lionel Robbins⁷⁰ and L. Von Mises and other intuitionists, undoubtedly indebted to Kant and to that extent heavily "unidimensional" in outlook. Secondly, there are authors like Max Weber and Talcott Parsons whose position is very close to a fully multidimensional conception of reality. Finally, there is Marxism, whose position in the history of political upheaval makes it very dificult to take its doctrines as a starting point for a corrected epistemology.⁷¹ So, let us now concentrate in examining the

68 Friedman, p. 7.

- 69 By far the most "positivist" of non-positivist epistemologists is Kant himself, the precursor of positivism. The "unidimensionality" of Kant's epistemology is apparent in his naive faith in Euclidean geometry and Newtonian physics as the only true interpretation of reality. Ironically, it is a positivist, Reichenbach, who shows him wrong in that defense of a priviliged status for mathematics and physics. But this does not mean a further step away from metaphysics, as Reichenbach will tend to say. On the contrary, it means that Kant's critic of the Ideal is equally valid in the "Aesthetic" and the "Logic" —all three areas need justification of informal sort. Cf. Immanuel Kant, Kritik der reinen Vernunft (5. aufl., Leipzig: J. F. Hartknoch, 1799); Hans Reichenbach, Philosophie der Raum-Zeit-lehre Berlin und Leipzig: W. de Gruyter, 1928), pp. 41-50.
- 70 Lionel Robbins, An Essay on the Nature and Significance of Economic Science (London: MacMillan, 1932). The author considers the economic as one analytical aspect of all behavior (p. 16); but his ignoring its relations to other aspects "amounts to the implicit assumption that they are random relative to the economic... The result is a profound laissez-faire bias which appears conspicuously in Professor Robbins' other works." Parsons, p. 620.
- 71 Cf. Paul N. Sweezy, The Theory of Capitalist Development: Principles of Marxian Economy (New York: Oxford University Press, 1942), pp. 6-22. The epistemological weakness of Marxism is well recognized, even by sympathetic students: Sartre, e.g., says that "la theorie de la connasissance... reste le point faible du marxisme" (p. 30, n.).

Weber-Parsons position, and in trying to improve on their "analytical realism"⁷² as a way of building up our own epistemological view.

I take the following texts of Parsons as being some of the most revealing of his theory of knowledge:

It is in the nature of the case that theoretical systems should attempt to become "logically closed."... The system becomes logically closed when each of the logical implications which can be derived from any one proposition within the system finds its statement in another proposition in the same system.

Though all theory tends to develop logically closed systems in this sense it is dangerous to confuse this with the "empirical" closure of a system....⁷⁸

Every system...may be visualized as an illuminated spot enveloped by darkness. The logical name for the darkness is, in general, "residual categories." Their role may be deduced from the inherent necessity of a system to become logically closed....⁷⁴

The obviously unattainable, but asymptotically approached goal of the development of scientific theory is...the elimination of all residual categories from science. ... For any one system there will, to be sure, always be residual categories of one or more other systems. For the empirical application of any one system these residual elements will be found to be involved in the necessary data.⁷⁵

As can be seen from the above quotations, there is a fundamental ambivalence in the formulation by the author of what we should understand as "analytical realism." In particular, when he says that for every system there must always be some residual categories, it is not clear whether he means that the other system, in which the positions of residual and positive categories would be reversed, is simply complementary of the former or rather inconsistent with it. In other words, it is not clear whether his contention has, so to speak, Goedelian overtones or is just another way of expressing the optimistic view of a universal science, with several compartments, of course, that would be *both* consistent *and* complete. I am inclined to think that what he means is the optimistic view. I am convinced that the other

- 73 Ibid., pp. 9-10.
- 74 Ibid., p. 17.
- 75 Ibid., pp. 18-19.

⁷² Parsons, pp. 730-757. This author calls his own epistomological position "analytical realism"; his obvious wish is that this position be distinguished from epistemological fictionalism—which Weber would maintain—and also from sheer empiricism of the positivist sort. The present writer does not think the differences between Parsons' and Weber's outlook is very important, at least in relation to our purposes. In any case, to the label "analytical realism" I want to oppose the label "transcendental realism" as the most suitable name for the epistemological position that is to be sketched here. The reasons for my preference for that label, if labels are at all desirable, are related, as the sequel will make clear, to at least the following points: (1) the term "transcendental" stresses the systematic unit of all knowledge, (2) it emphasizes the fact that knowledge is consubstantial with the mind, (3) in juxtaposition with "realism" it clearly suggests the plurality of conflicting analysis of reality, and (4) there is small risk of the position being taken for another variation of positivism.

EPISTEMOLOGY AND ECONOMICS

connotation would make the position closer to the truth. It is along the latter line that I plan to attempt something that we could call a reformulation of non-positivist epistemology. In that vein, I want to take "residual categories" not as a token for the province of another science, but rather as a token of the very inexhaustible character of reality itself.⁷⁶ It is in this light that we should try to evaluate the contention, implicit in the literature of the defenders of economic a priori methodology, that their type of abstraction is essentially different from the type of abstraction of their Marxian counterparts: theirs is "formal," the others' "material." Does "material abstraction" make sense? The truth is that the aspect which Robbins considers important—the "scarcity aspect"—and the aspect that Sweezy considers important—the "exploitation aspect"—are both and equally *abstract*, neither being more formal or material than the other.⁷⁷ But the two kinds of abstraction are different, among other things, in that each has its own paradigm or frame of reference, the one *economical*, the other *political*. Each paradigm is sufficiently "imperialistic" so as to exclude the other; hence both cannot be maintained at the same time.

By this time the reader should be sufficiently uneasy about the plausibility of economics and politics being inconsistent with each other. For his comfort I must say that general paradigms do become reconciled, but this reconciliation comes about in the level of *praxis*, of concrete existence, not in the abstract level of theory where they know nothing of each other, being as they are mutually incommensurable. Also, that their "inconsistency" is not such that each of them could say exactly the contrary of what the other is saying on a particular subject. Each one is speaking its own language, so that there is no danger of contradicting each other in the ordinary sense. In that part of their languages that is reciprocally translatable they say the same thing. In the limit, however, there is always something that only one of the conflicting paradigms could possibly express and in an eminent way, precisely at the point where the other has become exhausted.

The Passonian approach, we have seen, lends primary importance to the "positive" rather than to the "residual" categories. The approach of "transcendental realism,"⁷⁸ on the contrary, considers residual categories as vital. They represent the radically unformalizable assumptions or powers of non-formal thought on which the whole edifice of science is founded. Let us, in this connection, look into the distinction, which may prove important, that Parsons makes between logical and empirical "closure" of a system:

Any given concrete phenomenon is ... a meeting ground... of a number of different laws. So the complete scientific explanation of the concrete phenomenon can only be achieved by the synthetic application of all the theories involved.... Science is always concerned with successive approximations.

⁷⁶ It becomes clear in this context that it is Robbins rather than Parsons who is (epistemologically) right in claining a de facto exclusive universality for his specialized point of view (see p. 205, n. 70) since it is in the very nature of a point of view to tend to that universality; that it is naive to expect a smooth complementarity between an "economic" and a "political" points of view, the latter being of course equally exclusive and universal as the former. No wonder that, as Parsons very well remarked, Robbinsian epistemology is not separable from a conservative political attitude. Another example of that kind of symbiosis is found in the works of L. Von Mises, whose conservative bias is legendary.

⁷⁷ There is, nevertheless, a sense in which the two abstractions might be distinguished: Marxian hypotheses tend to be identifiable with ideal types, whereas marginalism is identifiable with non-formal assumptions—or with the token replica of them within the formal system (see Chapter V).

Thus the element of "necessity" in scientific law inhers only in its logic...But this logical necessity...must...not be carried over to concrete phenomena. The logically closed system of scientific theory must not arbitrarily be made an empirically closed system.⁷⁹...

I interpret this distinction as saying that a paradigm can be mistakenly viewed as an unidimensional representation of reality, as a full rendering of its (empirical) content. This, I think, is what Parsons considers wrong in positivism, that it easily leads us to expect empirical closure of the scientific system. Now, I think that the non-positivist attempt to present a multidimensional representation of reality might result in frustration if it is understood as a conjunction of complementary paradigms. The "dimensions" will then be also conjointed, so that we will have a new-single although conjunctive-dimension. No advance into authentic multidimensionality will have been made at all. We will still have an empirical closure of the system. In order to maintain the distinction between logical and empirical closure we will have to take the "residual categories" as the token of our ignorance as such, not as the token of our ignorance relative to another, complementary in the sense of the conjunction, paradigm. We could probably say that the paradigms are complementary in the sense of the disjunction: one of them at least, in every case, must be true. In this conception, theory, i.e., heuristically advanced hypotheses, will be what is expressed in the residual categories. The positive ones are rather an expression of analyticity and direct empirical confirmation. The "prophetic" empirical content, our anchorage in reality as such, will be implicit in the fundamental assumption that we make when we adopt a particular theoretical point of view or paradigm. "Logical closure" will then coexist with "empirical openness" in the sense that the corpus of a science will be treated as a system precisely through the theoretical notions-not reducible to what we directly know by means of our senses * and operations of logical inference.80 The system will be logically closed to the same extent that its degree of empirical openness may be dealt with as a datum, which is the same as saying that the synthetical character of a theory is not necessarily correlated to its negative degree of analyticity (tautologicalness). All theory, even empirically meaningful or synthetical theory, must be self-validating in order to be theory at all. That is why the intervention of residual categories or theoretical notions is absolutely necessary. But we could also say, in what would amount to a philosophical generalization of Goedel's theorems, that no theory can be consistent except the one which leaves room for continual heuristic achievement and the operation of alternative paradigms.

This resource of taking man's ignorance as a means for advancing knowledge —as has been shown is the case in the employment of "residual categories"—is not a new trick in the historical process of science. It has proved itself a very powerful device in several famous crucial instances. The discovery of the infinitesimal calculus can be counted as one (motion is not easily conceived as subject to number—to wit, Zeno's reknown paradoxes; so, let us take the unintelligible notion of a numbered motion as our starting point!). The discovery of the theory of relativity, as another (the concept of simultaneity at a distance is a scandal to mathematics; so, let us take its defined postulation as our starting point!). The underlying principle of this trick seems to be that if we place the ineradicable darkness at the center, as a result all the rest will come under full light. What is implied here, as in any case of really "dimensional" paradigms, is a sort of interplay between knowledge and ignorance, the one growing at one point while the other dwindles at some other point. The phenomenon

79 Parsons, pp. 184-5.

⁸⁰ For a good discussion of the current polemic on the reductibility of theoretical terms to directly empirical concepts, cf. Scheffler, pp. 127-222.

EPISTEMOLOGY AND ECONOMICS

is also visible in analysis of language; for example in the connection between assertion and possibilities of meaning, emphasized in the discussion of the naturalistic conception of value.⁸¹ That interplay may be compared to the ironing of surfaces topologically⁸² inadequate. You can make sure that some part of the surface will be smooth, but only under the condition that wrinkles will appear somewhere else on the surface. This topological ambiguity seems to be essential to all knowledge bearing on reality. If one tries to get rid of it entirely one would end up destroying all knowledge. One draws precisely and rigorously the arguments one wants to refute. That is why one would better read the opponents of a particular theory to find the perfect summary of it! Why is it, one may also ask, that the only really effective argument in philosophical controversy is the indirect proof, i.e., *reductio ad absurdum?* It it not because every reasonably sound theory is coherent and consistent *up to a point*, and because no theory can be consistent and complete as well?

⁸¹ See p. 189.

⁸² The term is used loosely, so that one can say that, e.g., a cube and a sphere are topologically distinct, which is not true in the case of a strictly technical use of the term.

CHAPTER V

IDEAL TYPES

The literature of social science methodology is rich in discussions on the nature of what is commonly called ideal types. In reviewing part of this literature I am not going to partake in the main front of that discussions, i.e., whether ideal types are "natural" or "artificial," whether they are "concepts" or "theory." I am going rather to confine myself within a more specific problem, being content with considering ideal types as artificial and as natural as any instrument of human thinking, and as "propositional" as any concept should be, as "conceptual" as any theory must be.⁸³ The problem I am going to concern myself with is rather that of the relationships, and possible identity or opposition, between ideal types of two different kinds, i.e., "abstract models" on the one hand, and "assumption," "funda-mental propositions," or "basic postulates" on the other. What is relevant for my present purposes of analysis is again the crucial distinction between the "closed" and the "open," the formal and the non-formal, ultimately between the objective and the subjective which could possibly be discerned in this matter. We have been discussing this distinction all along. What is going to be new in this chapter is the consideration of the two aspects within the "formal side" of theory, rather than one in the nonformal and the other in the formal sides. The reason for this "repercussion" of the formal-informal polarity within the formal side is to be found in the nature of "residual categories" or "theoretical notions," as was shown before.84 In order to operate with theory one needs logical closure, and this implies that the whole of the non-formal side be represented by some tokens within the formal side-these tokens being what we are going to call assumptions in most of what follows.

Milton Friedman states the main problem of economic methodology, as he sees it, in these terms. "Pure theory" is only an analytical filing system. Economics needs to be something more than theory in order to be predictive as it should. So, we have the assumptions, which are useful devices, abstract and perforce *unrealistic*. Being unrealistic, hypotheses must be tested by their implications, not by the realism of their assumptions.⁸⁵ A hypothesis consists of two elements: the "abstract model"

84 See p. 207.

85 Friedman, pp. 7-15. Friedman has been accused of conservatism, in the sense of promoting exemptions of assumptions from empirical control, since implications derived from assumptions or definite hypotheses in economics are very difficult to test: Cf. Tjalling C. Koopmans, "The Construction of Economic Knowledge," *Three Essays on the State of Economic Science* (New York: McGraw-Hill, 1957), pp. 137-142. Witness his approach to monetary problems, as explained by Henry G. Johnson, *Recent Developments in Monetary Theory* (Chicago: By the author, 1966): Monetary policy produces reshuffling of asset portfolios so complicated that one cannot trace a particular path; however, one should be able to find some relationship directly between quantity of money and changes in income (pp. 33-34). The renouncing of theory in favor of mere "empirical laws" is apparent; and the upshot of it all is the advice to take the simpler hypothesis, that is, the traditional (and politically conservative) one, not because of its being true, but simply because we have despaired of finding the truth.

⁸³ See p. 202.

and some "rules" which both define the relevant interpretations which make the model valid and determine the correspondence between certain concepts in the model and certain empirically given properties or relations.

These two parts are very diferent in character. The model is abstract and complete; it is an "algebra" or "logic"....

The rules for using the model, on the other hand, cannot possibly be abstract and complete. ...

In seeking to make a science as "objective" as possible, our aim should be to formulate the rules in so far as possible. ... But, no matter how successful we may be in this attempt, there inevitably will remain room for judgment in applying the rules. Each occurrence has some features peculiarly its own, not covered by the explicit rules. The capacity to judge that these are or are not to be disregarded... [the capacity to judge] what observable phenomena are to be identified with what entities in the model, is something that cannot be taught, it can be learned but only by experience and exposure in the "right" scientific atmosphere, not by rote. It is at this point that the "amateur" is separated from the "professional" in all sciences....⁸⁶

Let us take good heed of the last, very important remark. We most certainly can correlate this characteristic of the "rules", their being ultimately dependent upon professional judgment, with what has been said about the nonformal powers of thought necessary for the operation of formal processes. We can further enrich this concept with our notion of assumptions as residual categories which represent, as it were, in a token form the risk and heuristic commitment inherent in all assertions of theory.⁸⁷ Equipped with this understanding, we can wonder now what to do with Friedman's thesis about assumptions. Should they be unrealistic or rather the "abstract models"? If "assumption" is identified with non-formal power of thought, or perhaps with the token formal version of it, then most surely the assumptions are our wide road to reality, and they must be realistic at least in this sense. But because informal thought is heuristic, most surely the assumptions may be also unrealistic, in the sense that heuristic anticipations can eventually prove wrong. I do not think though, that this is the meaning the author wants to convey. I feel more able to make sense of what he actually says by taking the alternative interpretation. Not the assumptions but the models which the investigator operates with under the assumptions should be unrealistic. These models are at a lower level, so to speak, with respect to the assumptions, and they-the models-should be formal rather than non-formal or unformalizable. Without making very much of this definition, let us say that a process or element is formal if the semantical and pragmatical dimensions of it, as a significant whole, are removed or put within parentheses so that the only remaining concern is the possibility of operation according to syntactical rules. In other words, we must not be concerned with what objects of the real world the formal element represents or what purposes the process serves, but only with what the element or process can produce of itself by the sole application of the (not necessarily formal) rules which govern it. Now, it is possible to see that it is to the benefit of the investigation that the scientist is conducting and its accurate results that the models be unrealistic-that is, formal-so that he can operate more easily with them and through them.

Another question regarding the correspondence of Friedman's terminology with ours arises. When he says that pure theory is an analytical filing system, is he talking about "abstract models" in our sense? Could we possibly say that an abstract

⁸⁷ Polanyi, pp. 308-16.

model is part of a filing system? I think the answer is no. The best description of the way in which a set of abstract models operates is, to my mind, the one presented by Koopmans. For him, economic theory is neither entirely self-evident nor readily tested by experience. It is rather "a sequence of conceptual models that seeks to express in simplified form different aspects of an always more complicated reality."88 Perception of additional aspects of reality precedes their recognition in model formation. Rigor follows along to consolidate gains, "rigor" being presumably a change or refinement of models.89 According to this description, then, models are not, in themselves, anything empirical, being rather akin to a mechanism, a symbolic medium, or a game. Their empirical relevance could only be explained by appealing to the concept of a "pre-established harmony" between the inner nature of the models and external reality, or to some other means of rendering the philosophical content of that important myth. The fact is that abstract or formal models are as closed as monads were thought to be. "They have no windows" to the world. We use them. But the use we make of them is external to them, even to the point of having to change the model altogether in order to alter its empirical significance. They cannot be "files" at all.

In summation: "pure theory" can be identified alternatively with either assumptions or models. If with the former, it must be realistic in the sense of heuristic anticipation. If with the latter, it must be unrealistic in the sense of formal, as such closed to reality but continually revisable and revised. The dialectical motion of theory creation surrounds the models, although they, in themselves, remain unmovable. In this respect we should carefully distinguish the mind as intellectual agent, so to speak, from the passive although equally necessary aspects of intelligence. To put it in an epigramatic way, abstract or formal models cannot avoid being "stupid as a computer." In fact, to the extent that one has used formal procedures in thinking, . one has been using "computers" all along, perhaps with the equivocated notion that they were intelligent (one thought that because one was unable to think of them as different from oneself!). But it is precisely here that we face the need of distinguishing between the model that we use and the assumption that we identify with. From this distinction intellectual efficiency cannot but benefit, as we have seen. I am unable to see that ill results may possibly come about from the professionally sound use of formal devices.

A different analysis of ideal types is provided by C. Hempel. For him there are two classes of ideal types: *intuitive* and *theoretical* ones. The former is only (sic) a heuristic category; the latter can be taken to be formal, more or less in our sense of the term. It is important to note that for this author intuitive ideal types are defined reductively, that is, saying what they are not—theoretical terms. Therefore, they form a "residual category" in approximately the sense of Parsons. As for theoretical ideal types, they are characterized by two related requisites: (1) they are postulates deduced from broader principles, and (2) the area of their application is clearly specified by logically independent criteria.⁹⁰

It is apparent that Hempel's intuitive types do correspond to our "assumptions", notwithstanding the difference in appreciation of their methodological importance. A problem arises in this connection for our own view in the matter. In what sense can we maintain, in the light of Hempel's analysis, the dependence of abstract models (Hempel's theoretical types) on the assumptions (Hempel's intuitive

88 Koopmans, p. 142.

89 Ibid., p. 143.

90 C. Hempel, "Typological Method in the Social Sciences," Philosophy of the Social Sciences: A Reader, ed. Maurice Natanson (New York: Random House, 1963), pp. 222-28. types)? Are not the requisites (1) and (2) a re-statement of the principle of empirical control via direct verification of propositions? If so, am I objecting to that principle by the insistence on the priority of assumptions (intuitive types)? There is much to be said about this matter.⁹¹ For the time being, let us indicate that I am not objecting to the empirical principle itself, but only to the belief that the problem of verification is clearly separable from the problem of theoretical ideation. One verifies hypotheses which have a good chance of being true; so, the problem of empirical verification is not strictly distinct from the problem of theory formation in general.⁹² We verify a hypothesis if we find it coherent with a broader theory to which, and for good reason, we are committed, while putting aside for later investigation whatever adverse evidence happens to occur.⁹³

Besides this implication of separate verification, (1) seems to be saying only that the abstract model must be seen as systematically contextual, i.e., that every model is jointly affirmed with some other models.

But apart from paradigmatic systematism, which Hempel is not maintaining, this is not at all important. Logical simplification can always serve to isolate a particular model from its context, i.e., from "p.q" one can always get "p" alone. Requisite (2) seems to be more interesting, since it opens the door for the discussion of the issue of what we may call "methodological gradualism." If "logically independent criteria" does not mean the same as the "systematic context" of requisite (1) then the only thing it can mean is the necessity of a nonformal basis for formalism. Now, one can choose to say that the passage from formal to non-formal is gradual, so that one can have models functioning as assumptions with respect to other models and assumptions functioning as models with respect to other assumption. In other words, there are types which are "more assumptions than models" and types that are "more models than assumptions.". The gradation between formalism and nonformal context is continuous, with no sharp distinction between the two aspects at any point. This, of course, is different from what Hempel envisions, i.e., a "hypothesis" on the one hand, and an "area of application" of the hypothesis on the other, a clear-cut dichotomy. That such an oversimplified view of the problem of knowledge is possible is due, I think, to the fact that we do in practice identify ourselves with the context of the model we are operating with; at least we identify with it for the time the investigation is in course. For obvious reasons, we always try to be "as close as possible" to the model we are manipulating; however, sometimes we cannot get "that close" and we have to reconcile ourselves with the unavoidable intervention of some context, e.g., a mathematical apparatus of some kind.⁹⁴ More often than not, though, we do get "that close," but only through a great, sometimes painful, change of ourselves, the act of affiliation to particular uses of the profession. We know that this change can be done. We all live informally a great deal of abstruse stuff of our profession.95 In any case, if we identify with some context it is easy not to perceive the variegated nature of that context, especially if we are overinterested in individual hypotheses rather than in the paradigmatic unity of a set of them. If, on the contrary, we are more interested in this organic unity, then we cannot miss the fact that a single piece of information can be used as the methodological presupposition for some inquiry, as well as be made itself the object of direct investigation (with the help of some other presuppositions). Witness, as an analogy, the case of

91 See next chapter.

92 Polanyi, p. 30.

93 Kuhn, pp. 52-65.

94 Like the Psi-function of quantum mechanics; see Nagel, p. 308.

⁹⁵ As for my personal case, I have found myself "conversing" with symbols of logic, as if they were friends, or at least favorite characters of a play.

a hammer; one can drive a nail with it, but also, if broken, it can be repaired with the help of other tools, perhaps another hammer.⁹⁶

Another view on the matter is furnished by T. W. Hutchison.⁹⁷ For him, assumptions are mere definitions, although in the material mode.⁹⁸ Besides, they are arbitrary, their selection resting mostly on the availability of statistics (*sic*). This way of posing the problem is of course naively positivist, sharing abundantly of the dogma that all terms must be definable into sense-data experience. One wonders how such an extreme position can be maintained in spite of the contradiction and total depletion of intellectual force that it ultimately entails. The answer might lie in the "arbitrariness" of definitions which is postulated by this type of reasoning: this concept acts as a back door through which all that the positivist approach excluded comes back again into the picture. In fact, this arbitrariness is equivalent to what we have called heuristic aspect of theory-ideation. To select *this* rather than *that* definition is to identify oneself with some sort of assumption, however unclearly known its implications may as yet be. If these are made explicit and a contradiction appears, then one may drop the "definition" in the hope that something better is available to take its place.

Related to this position, but much more tolerant and interesting, is the way in which Felix Kaufmann attacks the problem. For him, methodology is the theory of scientific decisions, which are the acceptance or elimination of propositions in accordance with rules which state exemptions from the general prohibition to change the corpus of a science. He makes the distinction between "empirical laws" and "theoretical principle." The former are falsifiable by a single instance (sic); the latter are governed by higher rules related to the whole theoretical framework of the science.99 The main objection to this approach has to do with the sharp dichotomy just stated. Why rule out the possibility of a thesis which in a certain context is an "empirical law," and in another functions as a "theoretical principle?"100 On the other hand, what are the grounds for the faith in the rejectability of hypotheses in the face of a single contradictory instance?101 I myself will tend to approach the problem exactly in an inverse manner. Suppose there is no difference between "laws" and "laws," but that we have one (homogeneous) paradigm capable of a gradualistic interpretation, as explained above. We can distinguish, if need be, an aspect or part of the paradigm that tends to function like an empirical law more than like a theoretical principle, although, according to context, it is generally capable of playing both roles. The problem of falsification can then be considered as the problem of the empirical depletion or qualitative exhaustion of the paradigm; it has to do with the requirement that under certain circumstances some hypotheses or the paradigm itself must be dropped. On the other hand, the problem of the necessity of certain

- 99 Kaufmann, pp. 48 and 213-14.
- 100 See n. 96.
- 101 Kuhn, pp. 52-65.

⁹⁶ The concept of "subsidiary awarenees" of tools seems to be relevant in this connection; see Polanyi, pp. 58-59. An interesting case of a piece of information that serves purposes both of assumption and of model is furnished by the economic theory of J. M. Keynes, as presented for example by L. R. Klein, *The Keynesian Revolution* (New York: MacMillan, 1961): to the extent that it determines the instruments of economic analysis it cannot be understood as simply expressing a particular empirical case of neo-classic theory; it must be taken as an altogether different assumption.

⁹⁷ Hutchison, pp. 30-31.

⁹⁸ See R. Carnap, *Philosophy and Logical Syntax* (London: K. Paul, Trench, Trubner, 1935), p. 68.

assumptions¹⁰² must be viewed as the complementary theme of the *rational depletion* or *quantitative exhaustion* of paradigms; this has to do with the requirement that under certain circumstances a given paradigm should be considered as the necessary one (since no other could possibly be available). In this light "assumption" or "theoretical principle" will be understood as the general and systematic assertion of the paradigm, inarticulate or non-formal, and based in intellectual commitment. "Abstract model" or "empirical law" will be the formal elements of the paradigm, its positive aspects, closed to the world, but dialectically used and revisable according to the motion of scientific progress. We must now turn to closer scrutiny of these issues.

¹⁰² It may seem odd to talk about "assumptions" when one is referring to theoretical frameworks that are necessary; nevertheless I stick to it because I am fundamentally interested in epistemological rather than metaphysical aspects of the problem of knowledge. It might well be that we are made to have some propositions as necessarily true; for my purposes it is enough to think that no other assumptions are known to be conceivable by a human being.

CHAPTER VI

THE LIMITS OF CREATIVE IMAGINATION

I have reserved for this chapter the discussion of a position in economic methodology in relation to the interplay between assumptions and models that I consider especially important. It is the view that F. Machlup sets forth in his article on the problem of verification in economics. He presents in that paper an interpretation of what he calls the aprioristic and the ultraempiricist claims which, I think, is particularly worth mentioning. What the apriorist (Mises, Robbins) is trying to express, says he, in spite of defective or inaccurate terminology, is only that fundamental assumptions of a theory cannot be subject to separate or independent verification. They can be rejected, of course, in the face of refuting evidence, but only as a whole and together with the system they belong in and which they contribute to definepresumably on the condition that alternative systems are available. The ultra-empiricist, on his part, is only saying that all propositions of economics should be subject to independent verification. In other words, the ultra-empiricist, as against the apriorist, is asking that all investigation should start from facts. To this request the author answers: What facts? What is the criterion used to select, from the * innumerable amount of facts which make up our environment, those privileged basic facts? He then proceeds, very aptly, to compare economic science to a machine that has several parts to it. Some of them are fixed-the body of the machine, the assumptions-and some of them are more or less movable-models of different degree of generality.103

Machlup's analysis seems to be basically correct. I think, however, that it can be completed with some reflections pertaining to general epistemology, and this is what I will attempt in the present chapter. I begin by recalling that according to the author (and I share his view) the two opposing claims, even if both are radical in their wording, are not both equally removed from the truth. As Machlup's witty but also in content. I do not think that one can consistently develop from it alone, without fundamental corrections, the complete framework of economic science as normally understood. It seems to be impossible, even by definition of what we understand by ultra-empiricism, to make it say, after a purge of radical vocabulary, that theoretical terms are after all essential to illuminate and guide any investigation. On the other hand, it does not seem to be a formidable task to modify the aprioristic claim so as to have it saying: "Fundamental assumptions are irremovable; other less fundamental propositions are, with a gradualism of sorts, variably removable." In spite of the radical sound of the aprioristic claim, this position can be adequately rephrased, as Machlup in fact does, so that one can develop out of it the normal system of economic thought. So, one will do well to take the aprioristic claim as basically sound, although inadequate in wording, and work on it to try to get the expansion or correction which may be needed.

¹⁰³ F. Machlup, "The Problem of Verification in Economics", Southern Economic Journal, Vol. XXII (July, 1955).

For my part, I think that two corrections to the claim should deserve special attention. The first is the addition of an explicit reference to the availability of alternative systems as a condition for the rejection of a set of fundamental assumptions; the second is the clear acceptance of a progression, of a more-or-less in the degree of independence of propositions. The latter seems to be conditio sine qua non for the plausibility of the methodological conception that we are describing: There must be a limiting kind of case in which fundamental propositions can be falsified through the independent test of some of the removable propositions. This is, of course, the grain of truth that the aprioristic claim saves from the sinking boat of empiricism. The former contains the basis for a possible justification of the self-validation or logical necessity of some assumptions which seems to be essential for the plausibility of the aprioristic approach itself. In other words, the position of Machlup is subject to attack from two different flanks. On the one hand, an apriorist will contend that it is not a matter of having pieces of machinery that cannot be removed, except with the system as a whole. The case is rather that it is not possible to remove certain intellectual "equipment" because it is *necessary*, that is, somehow identical with the mind itself. To remove those assumptions, if it is at all possible to call them by this word, would be to throw the baby away together with the bath water. On the other hand, the empiricist is going to say that one cannot save a particular hypothesis against adverse evidence by the easy expedient of changing some "less removable" pieces so as to be able to maintain both the hypothesis and the fundamental assumptions unscathed. As I think that both rejoinders are basically sound, I am going to try the correction of Machlup's approach along these two complementary lines.

Once we have taken sides with the aprioristic claim, however modified the version of it might be, it becomes clear that we have to accept a coherence theory of truth rather than a correspondence theory in the tradition of empiricism. Now, this understanding of truth is open to some objections against which, in contrast, the rival theory seems to be well protected. In particular, it is argued that coherence should not be taken as a criterion of truth, but only as a criterion of stability of belief, since coherence may equally well stabilize an erroneous or a true belief.¹⁰⁴ To this I have to answer that if we are going to seriously take into account the nonformal roots of all knowledge we will have to admit that human imagination is limited and exhaustible in its creative power. We have to recognize two kinds of weaknesses-which may prove our greatest strength against the pitfalls of relativism-in the very person of the scientist. The first in the limitation implicit in the quantitative depletion of (available) paradigms, so that we could be forced to be content with the available-turned-necessary assumptions. The second is the limitation implicit in the qualitative depletion of a given paradigm, whose capability to defend itself against adverse evidence by theoretical maneuver may in fact become exhausted. I will presently atempt to give a more nearly full explanation of these two concepts. The important thing to note now is that, if the presence of the two weaknesses or limitations of the human mind is demonstrated, then what would only be "criterion of stability of belief" in the absence of those weaknesses, can be effectively proposed as a "criterion of truth." For if the range of the beliefs that can be rationally entertained is somehow narrowed, a soundly stabilized belief could not but be accepted as the (humanly attainable) truth on the matter.

104 Polanyi, p. 294. This author describes thus the armory of belief in defense of its own stability: (1) circularity, the fact that any objections can be met one by one, using the rest of the system to destroy them; (2) epicyclicality, the reserve of subsidiary explanantions, the handiest of all being just to label any adverse evidence as "anomaly"; and (3) suppressed nucleation, to deny the ground, even the vocabulary, to any competing paradigm. The intention of the latter is different from that of the former two: While circularity and epicyclicality "protect an existing system of beliefs against doubts arising from any adverse piece of evidence, suppressed nucleation prevents the germination of any alternative concepts on the basis of any such evidence" (pp. 288-91).

Frank H. Knight has a very unusual, and very illuminating, way of arguing the case for the apriorism of economics. His point is that all knowledge is empirical (in a rather extended meaning of the ordinary sense of the word) since all propositions, even of logic and mathematics, are verifiable "to the extent worth the cost, by counting beans,"¹⁰⁵ and that the necessity of a priori propositions comes not from an intuitive *power* of ours, but rather from a *shortcoming* of the mind, i.e., the lack of really creative imagination. We just cannot think of an alternative world where such and such an a priori law would not apply!¹⁰⁶ Thus, if it might be true that any belief is somehow circular, and that all beliefs tend to stabilize themselves, the lack of imagination inherently ours can save us from having to rely completely on sheer subjective commitment. The coherence of an explanation may be so wide in scope, so proximate in character, or even so intimately appealing to our aesthetic or pragmatic sense, that the possibility of furnishing a rival framework is exhausted or depleted for all discernible effects.

What Knight is referring to in the above argument is, I should say, the quantitative depletion or exhaustion of paradigms, which, as has been shown, points to the rational end of knowledge, the circularity or self-validation of all theory. It corresponds, to borrow the phrase of T. Parsons, to the "valid precipitate" of idealism in the building up of what we have called "transcendental realism." This exhaustion or depletion is found at the theoretical end of the problem of knowledge because it consists in the fact that our imagination is much too weak to conjure, out of the pure air, enough paradigms to choose among in all circumstances. Its result is the necessity of accepting the one that we happen to be able to produce under the conditions. The necessary paradigmatic configuration need not be a single all-embracing one. As shown previously (p. 207) it most plausibly will be rather a set of two complementary frameworks, each of them having an exhaustion of its own and being • unable by itself to give a full account of the whole of reality. This "internal" exhaustion, though, must not be confused with the one we are talking about here, the "external" depletion or exhaustion. The former is the exhaustion of the paradigm in its job of giving a consistent explanation of the phenomena of reality, whereas the latter is the exhaustion of the numerically available paradigms.¹⁰⁷

- 105 Frank H. Knight, On the History and Method of Economics (Chicago: The University of Chicago Press, 1956), p. 157. See p. 219, n. 110.
- 106 Ibid., p. 158.
- 107 An illustration of complementary paradigmatic explanations is provided by Polanyi's description of the criticism of objectivism from a positivist point of view and from a heuristic point of view: "Instead of indefinitely shifting an ever open problem within the regress of the objectivist criticism of objectivist claims, our reflections now move from an original state of intellectual hope to a succession of equally hopeful positions"... (p. 324). The regressive criticism can undoubtedly lead us to something equivalent to the "hopeful state" of a progressive heuristic posit, but only indirectly, so to speak, by showing forth the fact of its own depletion. In its turn, the heuristic method is unable to formulate a "bottom state" of pure empiricist intuition, although it somehow points to an indirect or external relation to that basis of reality. One can say that the alternative approaches are both important and necessary, especially because they counteract the possible excesses of each other: danger of verbal inflation or unwarranted speculation, on the one hand; danger of intellectual depression, annulment of heuristic momentum, on the other. And although, as the political economist will say, inflation is always preferable to depression, one ought to try to have neither.

One can say that the two exhaustion points in scientific paradigms may also be viewed as they themselves being complementary paradigms, in the realm of methodology: the rationalist, and the empiricist paradigms. This pair of paradigms has, of course, its own "external" exhaustion point. The postulation of the pair is to be considered as a necessary assumption.
EPISTEMOLOGY AND ECONOMICS

It is argued by Ouine that "our statements about external reality face the tribunal of sense experience not individually but as a corporate body."108 This means that all experience is systematic, in the sense that refutation of particular elements of a system of beliefs by adverse evidence can be absorbed by the paradigm by means of changing itself somewhere in the system. In this way, the system is able to still maintain the "refuted" element or hypothesis. Now, accepting as I do the coherence theory of truth implicit in this conception, I find myself unable to buy the extreme thesis that any hypothesis can be saved from refutation provided we make drastic enough changes somewhere else in the theoretical body. I rather think that the truth of the matter is that there exists, parallel to the quantitative exhaustion of paradigms, their qualitative exhaustion. This is a limit or depletion point which the paradigm reaches when it finds itself incapable of saving a particular hypothesis from empirical falsification. We can say that the paradigm reaches that point whenever the rest of the system loses its fluidity and becomes practically unmodifiable for the purposes of saving a particular hypothesis. The change to be made in order to maintain consistency is then qualitatively and not only quantitatively determined. The change has to be here, not there or at that other place.¹⁰⁹ Alternatively, we can say that the existence of this qualitative exhaustion point means that certain low-level interpretations of appearances cannot be thought away in any of the different versions of the paradigm or different articulations of the theory, and they tend, so to speak, to break away from the paradigm (Machlup's concept of independent verification). These low-level hypothesis or interpretations of appearances, and their rebellious conduct, are what furnishes the practical and theoretical basis for the existence of the empiricist point of view in our understanding of reality. The requirement of this positive depletion of the paradigm-complementary of the rational depletion of it-is, to speak again with Parsons, the "valid precipitate" of extreme empiricism for the building of our epistemological position. The fact that any paradigm should be subject to this point of empirical exhaustion is also interpretable as some weakness or fundamental limitation in our creative imagination. That limitation is the impossibility in which we find ourselves of thinking away the normal data of our sense experience and the results of the basic logical operations of inference.¹¹⁰

108 Willard Van Orman Quine, Methods of Logic (New York: Henry Holt, 1960), p. xii.

- 109 A most convincing argument for the existence of this exhaustion point was the central topic of a lecture given by A. Gruenbaum in The University of Chicago in the Fall of 1965, under the title "The Legacy of Pierre Duhem." He considered the content of the D-thesis to be that for every case of adverse evidence against a hypothesis H there exists a modified version of the theoretical context of H that "saves" H. He claimed the thesis to be unsound. Separate demonstration of the availability of "modified versions" is required in each case. Moreover, some counter-examples are produceable in which the context of H is true beyond any reasonable doubt, at least with no lesser degree of certainty than that which a Duhemian would require to accept the falsification of the system as a whole. The counter-examples alluded to in the lecture were, I think, good illustrations of the loss of fluidity of the paradigm that defines the point of positive or empirical depletion.
- 110 It is important to note that general logic of first order, in particular if one does not include relations, is clearly identifiable as a low-level interpretation of reality, capable of breaking away from a more comprehensive logical paradigm. This, I think, explains the illusion entertained by early forms of logical positivism about logic being the very structure of reality, which was shattered by the dismal fate of the rest of logic in the face of repeated attempts to defining it in completely objective terms. A similar fate seems to be the one reserved for over-simplified dismissals of the problems in the foundations of mathematics, lie Knight's principle of verification "by counting beans": what to do with those parts of logic and mathematics which do not have a decision procedure or even are demonstrably either-inconsistent-or-incomplete? I think that the only why out of this quandary is to interpret logic and mathematics in paradigmatic fashion, seeing their theories on a par with all other theory as being justifiable only heuristically. On the heuristic justification of mathematics, see Polanyi, pp. 124-131.

219

PART III

THE LOGICAL STATUS OF ECONOMIC THEORY

CHAPTER VII

PRAXEOLOGY: A LITERALISTIC SYSTEM

It is doubtful whether any language except possibly the arbitrary symbols of mathematics and symbolic logic, is entirely literal, just as it seems that reality cannot be thought of in purely objective terms....¹¹¹

After our presentation of the paradigmatic interpretation of theory it should be clear in the mind of the reader that a scientific system is not simply a formal framework where formal models find their right place. Basic or fundamental assumptions, indispensable for the existence of a theoretical system, are in themselves of an informal character. The residual categories that represent them within the formal system are *open* rather than *closed* concepts. The shade of formalism that accrues to the assumptions comes only from the fact that they govern formal models and must figure as "token" data for the logical closure of the system. No language is entirely literal since it interacts with the truths it tries to express.¹¹² Not even logic or mathematics is completely literal. If their axioms are sophisticated enough then we never know exactly what they mean. "If we knew, we could avoid the possibility of asserting in one axiom what another axiom denies"—and we have no warranty that this is possible.¹¹³ No language is "water-tight," and every line of reasoning must draw from an inarticulate background of non-formal and ultimately unformalizable knowledge. The non-realization of this fundamental truth is precisely the concurrent mistake of apriorism and positivism. Our language is neither literal nor unidimensional, or capable of a complete and exhaustive formal development.

One can take two authors as good representatives of these two positions: M. Friedman¹¹⁴ and L. Von Mises. The former sees economic theory as only a conventional filing system for the ordering of empirical material.¹¹⁵ The latter sees it as a priori knowledge, a branch of praxeology or the theory of human action, which happens to be applicable or relevant in relation to some historical instances. Says he:

Praxeology is a priori. It starts from the a priori category of action and develops out of it all that it contains. For practical reasons praxeology does not as a rule pay much attention to those problems that are of no use for the study of the reality of man's action, but restricts its work to those problems that are necessary for the elucidation of what is going on in reality. ... This does not alter the purely aprioristic character of praxeology. It merely circumscribes the field that the individual praxeologists customarily choose for their work....¹¹⁶

111 Knight, The Ethics of Competition and Other Essays, p. 135.

- 112 See pp. 201, 202.
- 113 Polanyi, p. 259.

114 Friedman's positivism is not radical: see his statements on the application of rules, pp. 24-25.

- 115 Friedman, p. 7.
- 116 Mises, p. 41.

I think that it is possible to argue against each of these authors reasoning from his own point of view. It is possible to show that their positions are capable of being "stretched-out" to cover much ground of the opponent's conception. Thus, one could be "more empiricist than Friedman" and maintain that our dependence on experience is so great that we would be without a language (without a filing system) but for the creative intervention of experience: in ultimate analysis only experience can originate a language. One could also be "more theoretical than Mises": one could maintain that our dependence on theory is such that we would be unable even to identify the relevant facts for the application of praxeology were it not for the circumstance that theory itself (language) already gives those facts to us. The proof of our first assertion was already given in our discussion of *Verstehen*.¹¹⁷ Substantiation of the second one may require some more words.

The argument for the "stretching-out" of praxeology is connected with the problem of the compatibility of the a priori conception with the possibility of application of a praxeological theorem. I put forward the dilemma that if the theorem is a priori in the unidimensional sense in which praxeology seems to be intended, then the theory as represented in the theorem is inapplicable. If it is not a priori in that sense then praxeology is already defeated. The difficulty I see here has to do with the description of the (empirical) conditions that must form part of the theorem in order to be applicable. Even if the theorem is a priori it has to mention the factual situation under which one is saying that the theorem is valid. But this mention has to be made in a language and the language one has to use must not be a purely formal¹¹⁸ one. If it were, empirical conditions would be inexpressible in the language and the theorem, without the mention of those conditions, would be inapplicable. Again, an empirical language-capable of mentioning the conditions of application of the theorem-would have to have been learned in close intercourse with experience. Furthermore, that experience should have occurred in precisely the area where praxeology is claiming to have something to say. Therefore, we are led to the conclusion that the application of a praxeological theorem supposes already the (empirically acquired) economic language and, by implication, (empirical) economic knowledge. The historical origin and development of praxeology¹¹⁹ lends much strength to the belief that this is precisely the case. It has appeared at the very end of a long and complex evolution of concepts and theory and not at the beginning of it. And one should have expected the latter were the system of praxeology really a unidimensional, literal theoretical construction. One could here reply that historical order needs not to coincide with the logical order, it is true, and I do not consider the argument as a principal or substantial one. But it is difficult to deny that it lends force to the main reasoning, that is, to the dilemma of inapplicability of a pure praxeological theorem.

I will present in the rest of this chapter a concrete analysis of a considerable part of a praxeological system.¹²⁰ I will try to further my case as to the invalidity of the belief that, as logical inference goes, the system is a literal, strictly lineal, or "water-proof" one.¹²¹

Before entering that analysis, I want to call the attention of the reader to an attack on contemporary logic which is very interesting. It is found in the introduction to Rothbard's book together with a defense of the verbal (less formal) logic of the

¹¹⁷ See pp. 200 ff.

¹¹⁸ In the aprioristic sense of formal, that is "transcendentally deduced."

¹¹⁹ That is, of marginal utility theory.

¹²⁰ Murray N. Rothbard, "Fundamentals of Human Action," Man, Economy, and State: A Treatise on Economic Principles (Princeton: D. Van Nostrand, 1962).

¹²¹ I have chosen Rothbard's presentation, although I could equally well have picked Mises. Ludwig Von Mises, *Human Action: A Treatise on Economics* (New Haven: Yale University Press, 1963).

EPISTEMOLOGY AND ECONOMICS

past. It is interesting because one would think that with the help of a less rigorous logic it might result possible to "formally deduce" praxeological theorems, using nothing but the praxeological axioms or premises. Although the discussion which is to follow will be conducted "with the weapons chosen by the adversary" and I will refrain from using any esoteric formula of mathematical logic, I think it necessary not to let these concepts pass without at a least a perfunctory refutation. Says Rothbard:

It is the great quality of verbal propositions that *each one* is meaningful. On the other hand, algebraic or logical symbols, as used in logistics, are not in themselves meaningful. Praxeology asserts the action axiom as true, and from this (together with a few empirical axioms—such as the existence of a variety of resources and individuals) are deduced, by the rules of logical inference, all the propositions of economics, each one of which is verbal and meaningful. ... Logistics, therefore, is far more suited to the physical sciences, where, in contrast to the science of human action, the conclusions rather than the axioms are known. ...¹²²

It is just not true that the propositions of traditional logic are, all of them, directly or semantically meaningful. What about "S-P"? Moreover, if the logical aspect of a proposition is what is being considered, even in traditional logic, the proposition is not taken as semantically meaningful but rather as a token of some formal structure of general validity. It is clear that when one studies "verbal logic" one is not interested in the mortality of Socrates, even if constantly using the phrase "Socrates is mortal." On the other hand, "logistic" propositions are always mean-ingful in the sense of requiring the full cooperation of non-formal rules. The rules *are explicit rather than implicit, and this seems to be a big difference between the two logics, to the clear advantage of symbolic logic. But logical propositions of the new garb are also meaningful in the sense of the professional identification referred to before.¹²³ The trained logician knows at each step what he is doing, even if the scholarly presentation of the matter, for example in a textbook, requires that he pretends to ignore it. It is also inexact that the physical and the social sciences must differ from each other in a parallel fashion, one being deduction from not-known-tobe-true hypotheses and the other from evident propositions. The demonstration of this could, in a way, follow the same line as the previous argument. More radically, that demonstration is connected with all that has been said in this study; very especially with what we are about to say in criticism of the praxeological system.

The following is our selection of statements from Rothbard's reasoning:124

1") Human action is defined simply as purposeful behavior. ... 125

This, I consider, is the basic axiom or fundamental praxeological postulate. If now we ask what its logical status is, one could try for one of these: either a nominal definition, or a useful device, or a real definition, or an empirical truth, or a category (if one accepts that there is a difference between "category" and "real definition"). We should not be concerned here with that status since (1") is the axiom of the system and presently we are not quarreling with the *status of the axiom* but only with the *character of the deduction*. However, it is very important that we

- 122 Rothbard, p. 65.
- 123 See p. 213, n. 95.

125 Rothbard, p. 1

¹²⁴ The numbers of the propositions are mine; I have respected the order of appearance in the original.

establish in a precise way what the informative content of the axiom is, so as to make certain that some statement are, or are not, implied by it. Now, the question arises whether we are being too demanding in precision for the basic postulate. Did we not say that ambiguity is ineradicable from all knowledge bearing on reality?¹²⁶ Yes, but Rothbard has not said so, since literalism consists in the negation of that ever present ambiguity. To plead for recognition of ambiguity at the very start would be to concede the whole argument. Another important question is this: we are now concerned only with the criticism of the chain of reasoning, and not directly with the primitive proposition and its truth or logical status. Are we going to return to the latter problem once we are through with the former? The answer to this question is no. The logical status of the primitive axiom will be of much less interest once it will have been proved that one cannot derive from it alone the other propositions of the system. The non-recognition of this is still a case of the literalistic fallacy itself. There are some other ways of justifying theory which have nothing to do with the pretended apriorism of certain axioms or principles, as it is hoped this essay itself has been able to show.

If now we concentrate on the issue of establishing a precise sense for the axiom, we will find the task very difficult. The issue is fogged with ambiguity in the praxeological literature. Take for example the efforts in clarification that one finds in the work of Israel M. Kirzner. For him praxeological rationality consists in the "consistent pursuit of one's own purposes."127 The use of the word "consistent" introduces a complication of terminology since clearly it is not only logical consistency that is implied. One should like to say that it is rather the persistence of a purpose as such, as a purpose, that is intended, the invariability of the ends and the respective line of action during a definite span of time. Still, there is the additional declaration that "in the praxeological view, action is rational by definition."128 To that view, even "a man who is swayed from the pursuit of his own best interests by falling prey to a fleeting temptation is yet acting 'rationally' in the praxeological sense. In the praxeological view, the man has simply substituted a new set of ends ..."129 This forces us into concluding that praxeological judgments are intended as true only in relation to assumed (fixed) programs. They depend completely on a tendency of human beings which demands that given programs be respected. It is said that "the selection of an end can never, as such, be judged in regard to its rationality...."130 One cannot avoid the implication that praxeology as an a priori injunction must be somehow equivalent to a plea that ends remain invariable: consilia sunt servanda! Under this light, I think, we can paraphrase (1") as

1') There must be in the world such a thing as persistent conscious motion toward a fixed goal.

Now, if, according to all that has been said, we are going to disregard in the axiom all that is not information, if, in other words, we are going to disregard its existential aspects to concentrate only in its conceptual content, then (1') can be simplified to read simply as a nominal definition:

1) Human action is persistence of ends.

126 See p. 209.

- 127 Israel M. Kirzner, The Economic Point of View (Princeton: D. Van Nostrand, 1960), p. 32.
- 128 Ibid., p. 167.
- 129 Ibid., pp. 168-9.
- 130 Ibid., p. 169.

EPISTEMOLOGY AND ECONOMICS

It is there where one should be able to find the whole content of praxeological knowledge in an embryonic fashion; i.e., the knowledge that we are supposed to encounter afterwards in expanded guise in the rest of the system. Hence, (1) will be our starting point or primitive statement for the critical demonstration game.

- 2) We could not conceive of human beings who do not act purposefully ... 131
- 3) The first truth to be discovered about human action is that it can be undertaken only by individual 'actors'....¹³²

Proposition (2) is reserved for later analysis. Proposition (3) seems not to be a nominal definition simply equating "actor" with "individual." Rather, it seems to be saying that there are no collective actors. It is a *real* rather than a nominal definition. I contend that it is not implied by (1) unless one makes (1) to imply it. I can conceivably take (1) as allowing for collectives "consciously moving toward a goal." If I rather prefer not so to take it, then I am making a dialectical decision, drawing an addition or correction to the original picture, or primitive interpretation of the content of (1). The addition would not be arbitrary, however. It would be based on the tacit knowledge I have about how people act, on an elicitation of what we have come to see as the normal use of the words "human action." This being so, one begins to wonder whether the original interpretation exists as a separate axiom; or rather the proposition "fully, clearly and necessarily present in every human mind"¹³³ is nothing short of the whole of ordinary language-continually reinterpreted by the very use we make of it. More simply, one could accept, at least for the sake of the argument, that there is some original, non-rich, interpretation of (1), and that as one learns about human action, one gradually enriches it with new content. Let us give a name to this operation of altering the interpretation of a given primitive statement in order to convey more information. Let us call it "dialectical redefinition," or perhaps better, "retro-definition." We shall have occasion for repeated use of this newly coined term in the course of this analysis.

4) Action requires an image of a desired end and "technological ideas" or plan on how to arrive at this end.¹³⁴

I think this is another clear case of retro-definition. I can conceivably take (1) to allow for "magical ideas" being used in our pursuing of ends. Why not?

But we tacitly know, independently of (1), that magic just would not work—although "we" in this context should not be identified with the whole of the human race. So, we *prefer* to take "purposeful behavior" as implying "technological ideas."

5) All action aims at rendering conditions at one time in the future more satisfactory for the actor than they would have been without the intervention of the action.¹³⁵

As with (3), here one has to choose between interpreting the statement in a nominal way or in a real way. The difference is in this case that one could not say

- 131 Rothbard, p. 1.
- 132 Ibid., p. 2.

- 134 Rothbard, p. 2.
- 135 Ibid., p. 3.

¹³³ Mises, The Ultimate Foundation of Economic Science, p. 4.

that (5) is a true real definition, for the simple reason that it will not be a true statement. In fact, I know that I sometimes act not for altering the future but merely for enjoying the present, i.e., the action itself, as in play, and artistic or religious contemplation. To maintain the contrary would be equivalent to saying that one never does anything except for the future. The unpalatable result will be that, practically speaking, there is no present at all. But this is clear nonsense. I enjoy, very often, the action of not-being-concerned. This is what we commonly call "relax-ation." Since the real interpretation is false, one has to take the other horn of the alternative: the definition is a nominal one. More properly speaking, this being an alleged inference from (1), this is a (nominal) retro-definition. But I have defined "retro-definition" as a device that tends to make the original definition convey more information; and it is certainly the case that this type of nominal definition tends rather to make it convey less information; hence I am led to propose for it the rather awkward name of "inverse retro-definition." Its function is to "save" the original definition or some of its desired consequences from the assault of adverse experience. It is clear that the intention of (5) is the postulation of homo aeconomicus. Its retro-definitional form makes the postulate appear as a deductive inference from the "basic axiom."136

- 6) Action takes place by choosing which ends shall be satisfied by the employment of means....
- 7) When we must use a means so that some ends remain unsatisfied, the necessity for a choice among ends arises...¹³⁷

There statements simply do not follow from (1); neither do they follow from any of the propositions (1)-(5). We suddenly begin to read about several ends (for a single actor, as understood) when in (1) there is no mention of any kind of multiplicity. One can, of course, reply that this is an auxiliary (and empirical) hypothesis. But this just would not do, because (1) does not talk about multiplicity of ends and hence it is inapplicable to the case envisioned by the hypothesis. We have to reinterpret (again!) the original statement so that it may allow for multiplicity of (simultaneous) ends. But neither would this do because ends may be either compatible or incompatible. If they are the former, they are one (bigger) end, not really several (conjunction is a very simple logical operation). If they are the latter, then they are no end at all (the actor does not know what he wants). Reinterpretation of (1) in terms of compatible ends is superfluous; in terms of incompatible, ends, impossible. Is there a way out? There is, but it implies a full analysis of the crucial notion of "substitution," not present at all in the basic axiom short of reinterpretation beyond possible recognition.

8) All means are scarce...¹³⁸

The analysis could run parallel to the analysis of (5). The postulation which is being made is here the dogma of the applicability or marginal analysis to the real world. This is also an inverse retro-definition, therefore it is of nominal type. It is not clear, however, as in the case of (5) that the *real* interpretation is false. What is claimed, beyond a restatement of the essential content of (5), is that there will never be abundance in the world. The *persistence of insatisfaction* is asserted, this

138 Ibid.

¹³⁶ Is it not the strategy of all Kantian-style apriorism to be realistic half the time and nominalist the other half while pretending to be neither?

¹³⁷ Ibid., p. 4.

EPISTEMOLOGY AND ECONOMICS

a flat addition to the original assertion of *persistence of ends*. Prima facie, that addition seems to be true, although recent technological, medical, and social developments make less improbable that a state of practical non-scarcity could be some time attained.¹³⁹ Therefore, the real definition is not clearly false, although it is not, to my mind, clearly true either. Because of this qualification, then, we can say that (8) could alternatively be interpreted also as a straight retro-definition (adding informative content to the original axiom).

9) The actor may be interpreted as ranking his alternative ends...¹⁴⁰

Is this to say that there must always be only one end? For "ranking" in this context means to assign an ordinal number to every partial end so as to make compatible otherwise conflicting ends. A scale of preferences must be built and the scale itself is to be, from now on, the speaker for the formerly conflicting ends. Its voice is to be now the single end. One could say that while this ranking is being done the actor is not economizing but, perhaps, "philosophizing," since the selection of ends is not the business of praxeology. After the ranking is done, the actor is not economizing either but "mathematizing," since purely tautological operations are not the business of praxeology either. This may be called the dilemma of ranking. Its solution implies, again, an analysis of substitution and of the specific role of subjectivity in economic theory.141 But apart form that, it is also true that (9) is not a deduction from (1). The supposition that ranking must precede action is a clear addition to the original interpretation (1)-one can easily imagine a situation in which action is performed with the inquisitive intention of finding out what the relative weights of one's wishes are. "Ends are more or less defined in the process of realization"142

10) All human choices are continually changing... as a result of changing valuations and changing ideas about the most appropriate means of arriving et ends....¹⁴³

This statement seems to be an empirical generalization about the evolution of mankind in the ethical and the technological fields. In that condition, it cannot be a consequence of (1). Nevertheless, it appears in a section entitled "First Implications of the Concept [of action]". My special interest lies only on part of (10), namely the part before the first ellipsis, and it is interesting because it appears to be in direct contradiction to (1). But nobody wants to assert clear contradictions like this. So we have to save the consistency of the system by understanding (10) as a qualification of (1), i.e., we want to read "(1) or (10)" rather than "both (1) and (10)." Now, "(1) or (10)" is not a contradiction, but unfortunately it is a tautology. It says that "either ends persist or ends are continually changing." Can any one save the informative power of such a proposition? A way of doing that would be to interpret the connection between the two statements in a quantitative sense, that is, the disjunction of them as asserting a differential degree of frequency or probability. If (1) is the main statement and (10) the qualification, the content of the former will be a description of the most probable or common occurrence in the real world.

139 Ayres, pp. 230 ff.

- 140 Rothbard, p. 4.
- 141 See pp., 239 ff.

143 Rothbard, p. 6.

¹⁴² Knight, On the History and Method of Economics, p. 172.

CLAUDIO GUTIERREZ

But this, incidentally, would be irrefutable only in the restricted sense in which one says that every probability statement is irrefutable, not in the sense of a priori praxeology.¹⁴⁴ Again, there is another presumable way out. The proposition that we had reserved for later discussion, that is (2), says that one possibility is excluded from the whole range of possibilities open by the tautology "(1) or (10)": the absolute extreme possibility of all human beings being such that they would be changing ends every discernible unit of time. This interpretation is not completely irrelevant. Some minds could find spiritual comfort in the fact that there is some, at least some—however fleeting—permanence in human endeavors.

- 11) Means... are called goods.... Such goods may all be classified in either of two categories: [consumers' goods or factors of production].¹⁴⁵
- 12) The factors of production may all be divided into two classes: Those that are themselves produced, and those that are found already available in nature...¹⁴⁶

All these are logical divisions. Logical division is not implication from the original proposition that uses the undivided concept. It cannot be because the original proposition does not distinguish between the parts that are a result of the division, which the "derived" propositions speak of. All divisions are either nominal, and arbitrary to that extent, or real, and therefore dependent on experience. If they are real, they amount to empirical generalizations. They are not deductions from purely formal axioms. It is in the nature of logical division to have either one of these two purposes: to serve as a catalog (a filing system) or to serve as a taxonomy (a system of concepts which somehow reflect the natural divisions of the world). It goes without saying that the divisions in (11) and (12) are not intended as a simple catalog. They must be intended as a taxonomy, and so they are. But to make this taxonomic division, one has to use fully the (empirical) economic knowledge that is at hand. One still would want to say that these divisions are also "auxiliary hypotheses" in order to "apply" or make "relevant" economic "pure" theory. So let them be! But then it becomes apparent that the term "relevant" is being used in an awkward way and that there is no difference between the sense of *your* word "relevance" and the sense of *my* word "truth."

- 13) If we wish to trace each stage of production far enough back to original sources, we must arrive at a point where only labor and nature existed and there were no capital goods....
- 14) There is another unique type of factor of production that is indispensable in every stage of every production process. This is the "technological idea"....

[Once learned] it becomes a general condition of human welfare in the same way as air.¹⁴⁷

- 144 This approach will of necessity crash head-on with the praxeologist instinctive repugnance for the statistical method. "There is no such thing as statistical laws"... (Mises, The Ultimate Foundation of Economic Science. p. 56).
- 145 Rothbard, p. 6.
- 146 Ibid., p. 8.
- 147 Ibid., p. 9.

It is inherent to any logical division that it facilitates some talk and hinders some other. In this sense we can say that some divisions are true and others are false. Some divisions are "truer" than other—they cannot be totally arbitrary. In the particular case of divisions (13) and (14) we can run into linguistic trouble. This is another strong reason against the praxeological approach. According to (13) one cannot talk about capital before the humanization of man. Nevertheless, some economists or philosophers might think it profitable so to talk. Knight, for one, likes to talk that way. He insists on the fact that capital is always produced by another capital, *even man himself is capital.*¹⁴⁸ Another hindrance occurs, according to (14), with respect to knowledge, in many a respect the most productive and valuable capital of all. But we cannot call it capital, not even a factor of production, not even a good, not even a means, since it is unlimited! If one cannot treat knowledge as capital in the praxeological setup, then something is wrong with that setup, or perhaps some radical limitations in the nature of economic thinking are being uncovered in this connection.

148 F. H. Knight, "Economics and Welfare", Ethics, LXI (April, 1951), 221.

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CHAPTER VIII

ASSUMPTIONS AND MODELS IS ECONOMICS

In the first part of this study I tried to clarify the role of subjectivity in the social sciences generally. There I came to the conclusion that two different aspects must be distinguished: subjectivity (or teleology or purposiveness) in the subjectmatter (which we decided to name "subjectivity₂"), and subjectivity in the explanation (which we decided to name "subjectivity₁"). The first aspect relates to the fact that the object of study in the social sciences always is an entity, man, who has purposes, knowledge, desires, so that the terms "purpose," "knowledge," "desire" must appear in the social-scientist's language. These terms are best interpreted as theoretical terms, as the term "force" is interpreted in the physical sciences, due to their obvious irreducibility to pure data of empirical observation. The second aspect relates to the fact that the scientist himself, be he a social or a physical scientist, has also purposes, beliefs, passions of his own. These are determinant in the way he does science, and even define him as a professional scientist.¹⁴⁹

A conclusion that can be drawn from this is the fundamental homogeneity of * all sciences, in a double sense. First, both physical and social scientist have to use theoretical terms which are not reducible to empirical sensation. Secondly, both social and physical sciences are dependent, ultimatly, on subjective considerations. Theoretical terms, because of their lack of empirical foundation, have to rest on the creative imagination of man, on his heuristic power, and are, to that extent, subjective (in the sense of teleology₁). The difference between, for example, "force" and "purpose" is not decisive. Both are theoretical terms which are to be operated in much the same way; both within a context of ultimately heuristic or subjective powers. One can point to a distinguishing characteristic of social science, however, but it seems to be rather external to the methodological problem. In fact, one can do social science about the physical scientist, as scientist, whereas one cannot possibly do physical science about the social scientist, or for that matter any other scientist, as scientist. This reflexivity, so to speak, seems to have impressed very much the writers that have occupied themselves with methodology of the social sciences, up to the point of leading them into thinking that social science is an altogether special kind of science, obeying logical canons completely different from the canons of general logic.¹⁵⁰ Now, I think that such reflexivity presents, of course, an unavoidable temptation to the methodologist. But he will do well to resist it and to refrain from seeing it as anything more than an interesting, but irrelevant, characteristic, external to the fundamental epistemological problem.

Two remarks, I think, are here in point. The first one is that not making the distinction between subjectivity₁ and subjectivity₂ is the source of the strength that the "reflexivity temptation" seems to have. If "purpose" must have a place in social

¹⁴⁹ See pp. 196 ff.

¹⁵⁰ Cf. Heinrich Rickert, Kulturwissenschäft und Naturwissenschaft (Tuebingen: J. C. B. Mohr, 1921), p. 36.

science, and who could deny that, the fact that we ourselves, in doing social science, are also purposive seems to shed all the light we need in our methodological problems. We are led into thinking that all we have to do to develop our science is to contemplate the workings of our own mind and somehow to project what we find there into the outer world. I do not want to say that this approach is totally wrong. There is a sound basis to it. But I do think that the approach will be much better off if corrected, in the sense suggested, i.e., by a more nearly full analysis of the role of subjectivity in science generally, and particularly in the social sciences.

The second remark that I want to make moves, as it were, in the opposite direction. The fact that there is subjectivity in two levels of science makes for the existence of a connection between these two levels. The need for theoretical terms and the impossibility of reducing them to empirical observation makes the subject-matter of the science dependent, to a very important extent upon the heuristic creativity of the scientist. This fact alone is capable of providing the needed link between the subjectivity₁ and subjectivity₂. The theoretical terms make sense because the theorist is trying to close the scientific system with the help of his own personality. He does this by making a bet on the "manageability of ignorance" or the prophetic power of the products of reason.¹⁵¹ Two avenues of interpretation for the role of theoretical terms may seem to be open to us. We can treat them as heuristic anticipations of the scientist, assumptions in which he dwells. Or we can rather see them as empirically empty concepts with only operational value which, nevertheless, point via the principle of indiscernibles,¹⁵² to an essential intervention of the personality of the scientist in the configuration of the subject-matter.

This last statement may need an illustration. Consider, for instance, the concept of "perfect rationality" in economic theory. As W. D. Lamont very well puts it: "To assume, for purposes of theoretical analysis, that the person entering into an economic relation is acting with a coldly perfect rationality is the only assumption which could properly be made....⁷¹⁵³ Alternative assumptions, which are not impossible to image, will have in common that they are indiscernible in the methodological sense. There is no sufficient reason to postulate one of them rather than any other. Only the perfect-rationality assumption, even if not empirically rounded as good theoretical term, can be told apart from other alternatives. This is why we prefer it to all the others. The intervention of the scientist as a person in this regard is, of course, the methodological decision of taking the "unfounded" theoretical concept rather than doing without any concept for the purpose of closing the scientific system. What all this means is, of course, that in the last analysis the positivist interpretation of theoretical terms is ultimately reducible to the non-positivist or heuristic interpretation.

In my opinion theoretical terms like "purposiveness," "perfect rationality," "foresight," in economics, as in any other field, are amenable to be rightly treated in either of the two ways which have been described. If we choose to treat them as heuristic anticipations, they are the non-formal context in which the scientist dwells in order to operate with his scientific material. If we rather choose to treat them as well delineated formal concepts, with the aim of closing the system, they are the token representation of the assumptions and may be well construed as ideal types. In the former case they will be pointing to the subjectivity of the explanation. In the latter case they will be in the operational side, where subjectivity is not immediate since objectivity is the intended aim. Both interpretations are possible; both are necessary. No one can do the job of the other and their principles are essentially

151 See p. 208.

152 One must choose "arbitrary" definitions or models, and this choice cannot be random. 153 W. D. Lamont, *The Value Judgement* (New York: Robert Cunningham, 1955), p. 41.

disimilar. In particular, one cannot expect that informal assumptions are able to do well the job reserved for operational concepts. The one thing one can certainly not do with non-formal assumptions is to operate with them. We dwell in them or in something else. But we do operate non-formal assumptions, as assumptions. The proposal implicit in this reasoning suggests the convenience of assuring a fair degree of flexibility to the operational side of theory, undisturbed by any illegitimate direct intervention of the postulational side. I think that much of the confusion that one finds in the methodology of economics comes from the fact that such degree of flexibility in the operation of formalism is not usually fully granted. Flexibility will be guaranteed if the distinction between the two theoretical levels, the one of non-formal assumptions, the other of abstract types, is more adequately recognized. It should become apparent that a single set of assumptions can be dwelt in while operating with alternative models or, as it were, "operational machinery." One must remember that if there is at all a distinction between informal assumption and types they must be essentially different and not fitted for each other's job. In particular, ultimate assumptions must be conceived as ultimately unformalizable, even inarticulable, because they are the atmosphere, as it were, in which formal models are possible. Their formalization, possible if we dwell in higher assumptions, is not even a requisite of scientific activity. The only thing that is necessary is that one recognizes the need of having some theoretical terms representing the assumptions to assure the logical closure of the system.154

Assumptions, of their very nature, are less "replaceable" than are models, they are more permanent and more resistent to change. Models, on the contrary, tend to be movable, and several of them can be operated upon, even simultaneously and without inconsistency, under the same assumptions. As an illustration, take the contrast between the concepts of micro-economics and those of macro-economics. The latter are not logically derivable from the former in any strict sense.¹⁵⁵ Nevertheless, they somehow correspond to the same basic unformalizable assumptions of utility theory. In the face of facts like this, one would tend to view models as a sort of conventions or useful devices.¹⁵⁶ For my methodological perspective this consideration is, however, not meaningful. "Conventional" makes sense only as opposed to "natural" or "unconventional." But here we have theoretical concepts, the only ones one could possibly afford in the situation, and they are called "conventional" without opposing this property of theirs to anything else. Is it the intention to say that they are not true? If we believe what they say and have enough ground for so believing, they are as true as they could possibly be.

The rendering of models as conventions makes sense only in an epistemology that makes a sharp distinction between "historical material" and "theory" (as filing system). Within that methodological framework the problem arises as to how to make formal predictions about purely empirical history. The interpretation of models as conventions is a way out, although it poses more problems than it solves. I think in particular that the way in which the relationship between "assumptions" and "hypotheses" is treated in such a scheme is unfortunate. These are not distinguished as belonging in essentially different levels. Rather, the assumptions are said to be false (unreal) while the hypotheses are supposedly true (confirmable by their impli-

154 The problem of the possible exhaustion of alternative assumptions has already been dealt with, pp. 75 ff. In particular, it is my opinion that the assumption of purposiveness in the subject-matter of economics is ineradicable. The importance of "purposiveness" as an abstract model is another question.

155 Cf. Nagel, p. 544: "No proof is available that... macroeconomic assumptions cannot be deduced from microeconomic ones. But there is also no proof that the deduction can be effected, and there is at least a presumption that it cannot be...".

156 Friedman, p. 15.

cations).¹⁵⁷ The approach fails to reveal the most important feature of the assumptions, namely, that they must be informal or ultimately unformalizable. On the other hand, I do not consider "history" and "theory" as the only two possible "states" of knowledge. As a matter of fact, I think these are the only impossible states, being only imaginary limits of an infinite gradation, even if these imaginary limits are taken by the positivist as reality itself. But none of them does actually exist; they are only polar positions or exhaustion points of scientific paradigms. The intermediate positions, in contrast, are the real knowledge we can aspire to have: the abstract models, or systems of such, together with the theoretical terms. They make for logical closure of the system and represent within it the unformalizable conditions of all scientific thinking.

Flexibility of models should be understood as essentially coupled to modifiability of the epistemological "distance" at which the models are seen from the assumptions. We have considered previously¹⁵⁸ that it is in the nature of models to be able to function as assumptions with respect to other pieces of information if the scientist chooses to dwell in the (otherwise a) model. We saw that, within certain limits, the assumption also can be objectivized as a model if the scientist chooses —and can—dwell in higher assumptions. Now, this modifiability of the intellectual focus in the assumption-model relationship provides a new opportunity for scientific decisions. One has not only to choose between models; one has also to choose the "right focus" for the theoretical purposes at hand. New possibilities of success, and failure, necessarily arise. In particular, it is now possible to get *too close* to the empirical limit of the system, with the corresponding poverty of scope. Or on the contrary, *too far removed* from it, with the consequence of an "inflation" of theory that might impair the effectiveness of the system.

A good example of what has just been said and which I am going to develop in the sequel, is the case of the concept of "satisfaction." What we can call the economist's interpretation if it understands it as a (formal) way of comparing the different elements of income,¹⁵⁹ i.e., as the token for the (informal) "sense of direction" that must be postulate for the operational concept of "substitution" to make sense. What we can call the utilitarian's interpretation, on the other hand, tends to understand it as a way of bypassing the fundamental variety of human wants and aims and reducing them to some (objective) state of satisfaction or happiness. The defect of the latter position, to my mind, is that its assumptions are "too far removed" from the phenomena under consideration. "Satisfaction" itself is made an object (model) rather than a postulate.160 Let us refrain from construing man as a being in search of the end satisfaction, as the utilitarian pleasure-and-painmachine. Let us assume instead, with narrower focus, that men are after many different things. Then we will better understand man's passion for objective freedom of choice and for all that can contribute to further that freedom, especially the magic of economic power.

As Lamont aptly puts it, economizing means the ability to hold together, in an unitary consciousness the totality of diverse demands and the attempt to obtain the

¹⁵⁷ See in this connection the very interesting discussion about Friedman's "principle of unreality" in *American Economic Review*, Vol. LIII (May, 1963), especially texts by Hagel, Simon, and Samuelson.

¹⁵⁸ See p. 213 ff.

¹⁵⁹ Cf. Knight, *Ethics*, LXI, 221: "The significance of utility (more properly "satisfaction") is that it provides the correct common denominator of the infinitely diverse components of income ..."

¹⁶⁰ The case may seem not to be relevant to present-day discussions of methodology of economics. Nevertheless, I think it worth mentioning since something like the utilitarian's view still lurks behind many objections to a more professional conception of value in terms of "purchasing power." We will come back to this later.

greatest possible realization of that totality.¹⁶¹ Paradoxically, the utilitarian's conception, by insisting on a unified object of desire, leads to a piecemeal approach to economic calculation. One is envisioned as deciding on alternative courses of action that are, as it were, cut-off from all context. The economist's conception, which respects the fundamental plurality of objects of desire, leads to the full consideration of all future choices represented by the marginal utility of money.¹⁶² To express it again in Lamont's words: "The individual comes to every situation with more in mind than the ostensible alternatives between which he has to choose. There is in the background of his thought, the sense of 'all the rest' of the things which are in actual or potential demand by him....^{''163} This is the sense of the economic value of things. The discussion of it we now must enter.

161 Lamont, p. 55.

162 "The price of a commodity tends to equal its marginal utility, as measured in terms of money, i.e., relatively to the marginal utility of money to its purchaser." H. Henderson, Supply and Demand (Chicago: The University of Chicago Press, 1963), p. 41.

163 Lamont, p. 55.

CHAPTER IX

ECONOMIC VALUE AND THE DEFINITION OF ECONOMICS

One of the deepest conflicts in economic methodology can be clarified by the distinction of levels referred to in the last chapter. It is the issue as to what we talk about when we speak of preferences, prices, profit, usefulness, etc. All those terms designate a quality attributed to things in relation to attitudes of people toward them. This is, of course, the issue of what economic value is and its discussion has received considerable attention from economic thinking. The very posing of the problem-with no distinction of levels implied-in terms of things and our (subjective) attitudes toward them, readily suggests two immediate and opposite solutions. The one will point to objective characteristics in things as the source of value. The other will rather posit subjective considerations as the required source. Thus, one finds some authors directly postulating an objective theory of value, in terms of the hours of labor that it takes to produce a thing. And one finds some others maintaining rather a subjective theory of value, in terms of the utility or satisfaction that it is possible to derive from the thing.¹⁶⁴ It is interesting to note that, as far as this writer knows, there has been no clear attempt of an intermediate solution to this problem. Perhaps it is impossible to have one within the terms in which the problem is usually posed.

Closely connected with the issue of the theory of value one finds in the literature the ever open question as to the definition of economic science and as to the character and nature of its subject-matter. In this particular discussion positions are found that are parallel to those which occur in regard to the theory of value. Thus, the objectivist will tend to define economics in "departmental terms," as the science which studies a physically discernible sector of reality-usually, the production of the means of subsistence. The subjectivist, on the other hand, will tend to define economics in "analytical terms," as the science which studies reality, the whole of it, from a definite point of view.165 Contrariwise to what is the case in the field of theory of value, however, here in the field of the definition of economics one does find intermediate positions. Writers are there who have attempted to find a third road combining the two approaches in a single definition, partly departmental and partly analytical. A. Lowe, without explicitly renouncing the analytical definition, thus retaining the basis for utility-theory analysis, adds nevertheless in the definition of economics the *differentia specifica* of a given relation to "material means."¹⁶⁶ Now, it is the case with Lowe, perhaps due to the close connection of the problem of definition with that of the theory of value, that his position comes apart very quickly into its disparate components. This can best be seen in the fact that, in spite of his retention of utility theory, he postulates an epistemology of economics which completely reduces economic problems to technological ones.¹⁶⁷

165 A good review of the subject is found in Kirzner, pp. 17 ff.

167 Ibid., Chapter I.

¹⁶⁴ I am taking the books of Sweezy and of Rothbard as adequate systematic representations of the two types of theory.

¹⁶⁶ A. Lowe, On Economic Knowlodge: Toward a Sciences of Political Economics (New York and Evanston: Harper & Row, 1965), p. 10.

In what is to follow I am going to attempt neither the building of an intermediate theory of value nor the invention of a new definition of economics. What I will try to do is rather to show that the nature of economic theory is on a par with the nature of all scientific theory. Any reasonable sound definition of economics or reasonably sound theory of value may be reinterpreted within a general methodological conception. The nature of economic theory is such that the terms "objective" or "subjective" do not describe it accurately. "Objective" does not do so because any scientific theory in order to be theory at all must have some formal terms not reducible to empirical observation. So does economic theory. "Subjective" does not because two levels must be distinguished in all theory and, as we have seen,188 subjectivity in the ordinary sense-subjectivity1-can be dispensed with at the operational or formal level. It is evident to me that "satisfaction," being as it is an abstract term, has as much delight in it as "labor" has sweat, in the utility and objective theories of value respectively. These are theoretical terms. They are formal in the sense of being tokens for non-formal assumptions making for logical closure of the scientific system. They are not amenable to straight empirical or intuitive validation. In particular, the use of the term "satisfaction" in utility theory is an insufficient ground for considering the theory as subjective. Of course, all theory is in the last analysis subjective, as has been shown before¹⁶⁹ and in the sense in which Parsons says that a frame of reference is subjective.170 The problem is that in social science the frame of reference is not only used but also studied, and this situation gives rise to the specific temptation of the social-science methodologist (to think that subjectivity, can take the place of subjectivity, at the formal level of theory articulation).

I want to suggest the idea that economic theory be regarded neither as objective nor as subjective but simply as formal, in our sense.¹⁷¹ According to this, one ought to regard "satisfaction" as expressing a dimension of economic theory that belongs in the realm of the unformalizable assumptions. These assumptions, no doubt, must be tokenly represented within the formal context, as we have seen, by a theoretical notion, in this case the concept of "equilibrium." This concept means that the system of economic quantities and operations has to be determined one way or another. But the operational content of "value" is best rendered by the joined relationships of substitution which exist among the elements of the economic system whose determination or solution the equilibrium point is. Thus, economic theory can be viewed as the attempt to organize and analyze the facts of wanting and producing inasmuch as they form equilibrium systems according to relations of substitutability.

The formal aspects of theory function as instruments for such analysis and organization by means of the construction of models. These are ideal systems which reproduce by abstraction the economic phenomena of the world. In such models the concepts of "money," "purchasing power," or simply "relative prices" are crucial, insofar as they stand for the unifying principle that holds the system together—the principle of substitution. There is room for diverse degrees of substitutability. One can distinguish between subsystems to some degree cutoff from one another by low indexes of substitutability "across the border." One can separate for analysis systems with *more* or rather *less* scope than the canonical system. A "less-scope" system is, for example, the economically "productive" system, where some steps connected with distribution are eliminated. A "more-scope" system is the Robinson-Crusoe model

¹⁶⁸ See p. 197.

¹⁶⁹ See p. 217.

¹⁷⁰ Parsons, p. 733.

¹⁷¹ As distinct from the literalist sense of "formal." In the present sense, theory is neither identified with empty tautological filing systems, nor considered as elaboration of a priori intuitions; rather, the formal aspect of theory is recognised as a set of heuristically created devices capable of making as much sense as possible out of the appearances of reality, including the behavior of *subjects*.

where economic calculations are applicable to decisions of isolated individuals. All of this makes for flexibility in the creation of and operation with models. And all of this is possible thanks to the formalism of the approach.

What I am trying to say is fundamentally this. One can best understand what the economists have proposed as objective or subjective theories of economic valuation and calculation by not paying much attention to their own explanations as to the ultimate nature of value. One should take the formal devices, which tend to coincide in both theories, as heuristic creations of the human mind. One should not look for further explanation¹⁷² different from the justification of heuristic methods furnished in general for all the sciences by basic epistemology. This is not possible if we do not -realm of model-construction, and of logical operations. Under this light, the objective theory appears as an imperfect formal (in my sense) methodological theory -together with a questionable social theory and philosophy of history-which nevertheless contains insights not present in other theories.¹⁷³ On the other hand, the subjective theory appears as a basically correct formal (in my sense) theory, ignorant however of its own formal nature due to deficient distinction of the two levels of subjectivity. The theorist or the professional economist who subscribes to the subjective theory tends to overlook the fundamental limits, and eventual possibilities, of economic analysis, and to identify it unnecessarily with a conservative political philosophy.

As I will not concern myself in the rest of this study with the objective theory as such, I am going to write a few words to explain my decision to choose the subjective one as the one best suited for the dialogue. There are two ways to explain that judgment. First, I believe that were one to question the subjective value theory one would become deprived of a most powerful instrument of analysis, namely the main tool of present-day economists, at least in the socalled free part of the world. Robbins seems to be right when he says that a material consideration of value is unable to account for, among other things, many problems of theory of wages and, of course, the economy of war.¹⁷⁴ The other way of explaining my decision is this. I find that valid content of the objective theory is a proximate version of corresponding but clearer laws of the subjective theory. Usually the translation to the subjective theory is easily done by means of the analysis of some concepts taken for granted by the objective theory. For instance, the concept of socially necessary labor¹⁷⁵ or of useful commodity¹⁷⁶ can be so treated for the understanding of the labor-theory equivalent of the law of supply and demand.

- 173 To wit, its treatment of institutions as potential or actual economic variables, with the important consecuences this has for economic dynamics and for social policy.
- 174 Robbins, p. 15.
- 175 "No more labor than that which is 'socially necessary,' that is to say necessary under the existing social conditions, is to be counted in the determination of value." Sweezy, p. 42.
- 176 "If beaver and deer are both useful... they must exchange in proportion to their respective labor times regardless of the relative intensity of the desire for each." *Ibid.*, p. 47.

¹⁷² The objection could possibly be raised that a theory of value must be a theory of the *cause* of value: if so, then it is impossible to have a theory which is divorced from the basic principles of purposiveness or economizing, since it is these principles that establish causal relations. Now, this type of reasoning shows a kind of epistomological immaturity which is typical of most presentations in methodology of economics. I think it can be characterized as a pre-Humean tendency to search for ultimate causes in scientific explanations. But if one recognizes, as he should, that sciences is understandable in itself, i.e., independent at least to some extent of metaphysics, than one has to accept that the formal heuristic approach is the sensible one.

Before closing my treatment of the objective theory, let us consider briefly the position, objectivist of sorts, of A. Lowe, to which I referred above in relation to the definition of economics. I want to criticize the part of his work that turns out to be genuine objectivism, i.e., his reduction of the economic problem to sheer interaction of technological and psychological considerations.¹⁷⁷ In particular, I want to challenge this contention: "Given his final and modal goals and his stocks of resources, the core of Crusoe's economic activity is technological...."178 I contend that it is not the case that the grading of ends in scales, the formulation of the "modal goals," is external to economics. Rather, I think that it defines the economic problem itself. This is so because the "scale" is nothing absolute, independent of the level of provision of means. The scale must read: "Such and such are my preferences for such and such level of provision." Now the concept of "level of provision" must be clarified. I can use, and most of the time have to use, items which, though numerically different, are all of the same kind-they belong to the same class of entities-vessels of water, for example. This is obvious from the very definition of "supply," stock which is "available in specific homogeneous units equally capable of rendering the same service to the actor."¹⁷⁹ It is only in relation to this case that the expression "level of provision" has sense, underlining the impor-tance of the concept of "substitution" for the description of the subject-matter of economics.

The formulation of a modal goal requires a separation of units within each supply to make the ranking according to preferences possible. But the separation that is required need not be physical; it can be rather mental or subjective (in the sense of subjectivity₂). I do not need to actually pour water in different vessels in order to make up my scale of preferences. To the extent that the calculation does not use the means of actual pouring of water, at least to that extent the operation . may not be totally considered as technological but must be considered also as economical (because making essential use of subjectivity₂ in the guise of "purpose," "endin view," or some similar theoretical term).¹⁸⁰ This is so not just because the operation is not physical but only imaginary, but because it fulfills the requirements of a social-science subject-matter, specifically of an economic subject-matter (as described by the principle of substitution). Thus the economic subject-matter is always a collection of items that can be considered as homogeneous; the items are capable of separation from one another (principle of divisibility) and also, and most importantly, capable of taking the place of one another in the event of destruction or deficient provision. As can be seen, the items are not physically different (except numerically) but are different because (mentally) conceived as different, in relation to some purpose; but they are also interchangeable, i.e., they can abandon their former (programmed) goals and come to the rescue, as it were, of more important goals. Note in this context that economic plans are not simply blueprints. They are expressable in terms of curves. And curves are central to economic analysis precisely because they are one of the forms of representation of the relationships of substitution.

177 Lowe distinguishes a man-things level from a man-man level. He considers specifically economic the latter, failing to see that his approach also treats this level as technological: patterns of behavior must be created that will tend to make people behave like a single person, and the Robinson Crusoe approach will again be applicable. Cf. Lowe, pp. 20 ff.

- 178 Ibid., p. 18. Final goal relates to qualitative decisions, whereas modal goal to scale of preferences, that is quantitative decisions.
- 179 Rothbard, p. 19.

180 The subjective character of economic subject-matter consists in the fact that no two parts of a homogeneous material —a supply— can be distinguished otherwise than subjectively; it is the end-in-view that makes two parts of it different. So, plurality of ends is implied in the fact of the postulated distinction between two parts of otherwise identical material.

That substitution is central in the description of economic tools of analysis has been shown, for instance, by F. Knight.¹⁸¹ The idea is also latent in any consideration of the economic problem as one implying plurality of ends and of means. Let us linger on this matter for awhile, so as to make clear what I mean by saying that substitution is central, and how is this centrality of substitution related to the requirement of a determination for the system. Let us take first the case of one single means capable of directly satisfying different ultimate wants, e.g., a given quantity of water useful both for drinking and for personal hygiene. If there is clear medical advice as to what the minima for survival are in both accounts, and further the water barely suffices to cover those minima, then, I claim there is still no economic problem involved. When the supply is somehow above this survival level, still, I think we do not have an economic problem: the possibility of comparison between two quite dissimilar purposes, cleaning and drinking, being lacking, there is no criterion of allocation even for the single actor. The "equilibrium" in this situation will be only a matter of arbitrarily hitting upon some combination which, by definition, will be the one the actor finds "most satisfactory."182 So, where we should expect "satisfaction" the most illuminating-means being immediate to ultimate endseconomic theory cannot even begin to operate as an explanatory principle.

Let us further take the case of more than one means contributing to the production of a third, in its turn directly satisfying more than one ultimate end. The possibility now arises of different combinations of the resources so as to maximize the quantity of the third good. With the advent of quantification the principle of substitution, and economic theory itself, does begin to operate. Satisfaction, a non-formal context, is here represented by the formal notion of sheer maximum, a strictly syntactical requirement; what brings about this maximum is, of course, substitution of units of one resource for units of the other. The requirement of minimization of expenditure is implicit, since it is only a mirror image of the requirement of maximazation of product, and is also amenable to strict syntactical treatment. When more than one product can be originated by the alternative combinahion of resources, all the complications of equilibrium analysis do appear. In this situation it is the notion of "equilibrium" which takes the place of sheer maximization as representing the need of a solution for the system. Equilibrium here corresponds to the maximizing point in simpler problems, and can be viewed as the "determinateness requirement" for problems of composed substitution-substitution of higher degrees. It is of course this notion which "closes" the system and acts as the theoretical token of the nonformal assumptions of the economic framework-those represented by the contextual terms "value," or "satisfaction."

Let us now revert to the main line of our argument. Let us repeat that there is a fundamental connection between the problems of economic value and of the definition of economics. An objective theory of value will tend to associate itself with a departmental or classificatory definition, whereas a subjective theory of value will rather be related to an analytical definition. Intermediate positions do not seem to be either possible or stable, that is, to the extent that they stand within the intellectual context which makes possible the alternative objective-subjective. A stable intermediate position can be reached if substitution systems, which constitute the subject-matter proper of economics, are recognized as capable of formal interpretation. The next chapter contains an elaboration of this idea.

¹⁸¹ Knight, On the History and Method of Economics, p. 175. The role of general economic theory is "to show what can be inferred from the general principles or axioms of diminishing utility and diminishing (technical) returns, both of which may be viewed as particular cases of the more inclusive principle of substitution." Italics mine.

¹⁸² Cf. Rothbard, p. 27: "Value scales do not exist in a void apart from the concrete choices of action ...".

PART IV

THE USES AND LIMITS OF FORMALISM IN ECONOMICS

CHAPTER X

THE CASE FOR A FORMAL CONCEPTION OF ECONOMICS

SUMMARY

Let us briefly review what we have accomplished up to this point in this study. We began by stating in the opening chapter our acceptance of a conception of science that makes it ultimately dependent upon intellectual passions and values to which the scientists are committed. We also pointed to the important truth that it is impossible to make a sharp distinction between pure theory and pure empirical or historical happening. An interaction always exists between those two ideal poles of all knowledge. We further emphasized the fact that value is always encountered in interrelation, forming systems, that some system of values always exists, and that different systems dwelt in by different persons are most likely to conflict with one another. The upshot of this recognition of conflict was not, however, a plea for relativism, but rather for tolerance and hope, hope that conflicting views may eventually be reconciled by means of mutual respect and intellectual, heuristically creative, discussion. This openness means also, given the necessary concomitance of knowledge and habit, the disposition to work and even be worked upon intellectually by our fellows, which can imply painful processes of affiliation and even struggle. But all this is conducted under a firmament of valid, even if revisable, shared-at least to a pointbeliefs and loyalties to values. The objective social cosmos is the only possible ground for the validity of sense observation and of scientific reasoning themselves.

We went on to examine the claim that there is an essential distinction between common sense and science and that in this distinction the difference between the natural and the social sciences could be founded. We were able to see that some writers consider the social sciences pejoratively as only commonsense knowledge, whereas some others consider it to be an asset rather than a defect to be the direct heirs of pre-scientific knowledge. We concluded from that examination that the important distinction is not that between common-sense and scientific knowledge, but rather the distinction between formal and nonformal knowledge, between formalism and the unformalizable basis needed for the meaningfulness of it. We came to the conclusion that it is imposible to objectively found objectivity, to formally support formalism, since that pretension could only lead us into infinite regression. The only reassurance we can expect for our need of ultimate epistemological security is the confidence in the responsibility of the scientist who is trying to fulfill standards of universal intent which he himself sets for his own performance.¹⁸³ That responsibility, coupled with an efficient development and use of the devices of formalism, is what we are entitled to call the professional sense of the practitioners of the sciences.

In this context we developed what could be considered a central idea of this essay, the distinction between two types of teleology or subjectivity: the teleology

of the explanation and the teleology in the subject-matter.¹⁸⁴ In connection with this distinction we construed what we called the temptation of the social scientist, i.e., to identify with each other two levels of analysis that must always be kept conceptually separate. The reflexivity implied in the fact that his subject-matter is as subjective as the social scientist himself has led the methodologist into believing that an altogether special scientific logic must be available to the social scientist which is not the common patrimony of the practitioners of general science. This temptation he should do well in resisting, it being conducive only to intellectual confusion. It is enough for all scientific purposes to treat subjectivity₂ by means of theoretical terminology, as it is done with "unobservables" in all other domains of science.

Closely connected with the issue of teleology and of subjectivity is the problem of the nature and uses of abstraction. I tried to react, in the chapter dealing with this subject, against a conception of abstraction which rests upon a unidimensional interpretation of reality. In that part of this study I took pains to show that the abstractionist's "analytical realism" is an insufficient account of the multi-dimensionality of the real world. I did it especially by exposing an underlying Goedelian overtone in Parsons' epistemology, and branding it as a more nearly correct insight than the "conjunctive" interpretation of the methodological proposal of that writer. The conception that all theoretical thinking is totalizing and systematic, and at the same time somehow limited or not complete enough to render a full account of reality, is the re-encounter in the methodological realm of the fundamental truth of the interrelatedness of value and the conflicting character of the relationships between different systems of value. Again, there is here a case of universality and exclusiveness. But now we find it in the core of reality, or better perhaps, in the means of our interpretation of reality. Conflict is not only ever present; it is ineradicable in the single mind, in its very capabilities for the understanding and generalizing about reality. Ambiguity, we are led to say, is inevitably inherent in all thinking bearing on reality, and we cannot hope to directly dissolve that ambiguity in order to "see through" it to the "real world." Our way to truth, I think, must be much more complicated than the simple "reading-off" of the data of sense experience or of some inscrutable inner intuitions. The way to truth, I propose, must be related to the fortunate fact that the ineradicable ambiguity we have to recognize is not random but systematic. It must make us concerned with the thorough examination of competing comprehensive views or paradigms. The adjudication among them must be connected with their coherence or harmoniousness, together with the fullness of account that they are capable of giving of the diverse elements of reality. Circularity of "long range" is implied, but this kind of circularity is not a defect in ultimate knowledge. Nevertheless, it is still true that some internal criteria must be discovered to tell apart "long" from "short" circularity, once the paradigms have passed the most important test for consistency or inconsistency.

The idea of paradigm was then further discussed. Paradigms are alternative, necessarily circular, comprehensive accounts of empirical appearances, competing interpretations of reality, so to speak. They are eminently subjective, in the sense that they are universal in intent in the sense that it is to their truth to what the persons entertaining them are committed. Truth and falsity of a paradigm cannot be separated, in ultimate analysis, from the belief or unbelief of the persons discussing the content of the paradigm. The criterion of stability of belief comes to be also the criterion of truth, and this is ultimate coherence. The property of making ultimate coherence or sense, however, is not unlimited. It does, in fact, become exhausted. The property of exhaustion or depletion of a paradigm, or of a set of them, might be regarded as a fundamental psychological property of the person asserting the paradigm. At the

184 The distinction is of long standing in the literature. What is new is the attempt to dispel some methodological errors by making systematic use of it.

EPISTEMOLOGY AND ECONOMICS

empirical end, the capacity of the person for making changes in the paradigm in order to save it from adverse evidence may become exhausted. The paradigm must then be dropped as false. At the theoretical level, the ability of the person to imagine alternative frameworks of explanation can also become depleted, and the paradigm must be asserted as necessarily true. But these two points of exhaustion are ideal poles, never fully attainable, like complementary foci only asymptotically approached.185 Within these poles the concrete instances of knowledge do occur, as it were in a state of flux from the (ideal) necessary theory to the (ideal) empirical fact, as an indefinite gradation of pieces of information that are both theoretical and empirical, according to the role they play in relation to other pieces of information. In general, one can say that a piece of knowledge is a model, a close enough representation of empirical reality, if one works on it from a higher level of knowledge. On the other hand, a piece of information is an assumption, a theory or paradigmatic configuration, if one dwells in it in order to work from it on something else. Within a belt of knowledge sufficiently distant both from "necessary assumptions" and "overwhelming evidence" there is much leeway to alternative organization of models and assumptions. Many ways might appear to be intellectually fruitful, several approaches equally sound and many conflicting hypotheses equally true, provided the necessary correlations within the paradigm in regard to coherence are always taken care of.

Among the most important of these correlations there are those needed for the logical closure of the particular system of hypotheses. This requirement stems from the fact that logical or formal consideration always calls for a cutting-off of non-formal context. If the living body of knowledge—belief—can be visualized as a picture in depth, the formal version of a segment of it has to be visualized as a plane within that picture. The contextual depth is represented within the flat logical picture, like a sort of projection, by the residual categories or formal tokens of the non—formal indispensable assumptions. Thus, empirical openness, necessary for the true interpretation of the system, and logical closure, necessary for the effective formal operation of it, are both concomitantly assured.

Now, under this tolerant methodology one may wonder what we could possibly say *against* anybodys' position, not clearly inconsistent, provided it does not clash with "necessary assumptions" or "overwhelming evidence," which anyhow are very difficult to characterize. To this I must answer in two ways. The first is touché! It is certainly very easy for this methodology to degenerate into "jelly" epistemology, making room for anything. Again, we must rely on the sense of responsibility of the acientist, in this case the methodologist or the philosopher. The second answer is this. A theoretical view that dissolves a problem rather than solving it, provided of course that the professional considers it a real problem, is not broad enough, not explanatory enough, and should not pass the test for completeness even for a tolerant methodology.¹⁸⁶ Furthermore, one can say that this epistemology is the reply, and I think it is the only valid reply, to a literalistic conception of economic theory, and for that matter to any literalistic conception of scientific theory generally. For "literalistic" I understand the property of a methodological system that is most conspicous in the aprioristic epistemological explanations, e.g., Von Mises' praxeology, but which is

¹⁸⁵ See the contrast between this proposed "transcendental realism" and Parsons' "analytical realism": for the latter, knowledge is asymptotically approached; for the former, what is approached asymptotically is the exhaustion of knowledge. See Parsons, p. 18.

¹⁸⁶ A clarification seems to be in point here: There are different ways of "dissolving" a problem. One of them positive, i.e., the necessary linguistic reform which simplifies a formerly overcomplicated paradigm —the Copernican revolution, for example. Another way is negative, i.e., obscurantist reform that takes away from us the linguistic means for formulating a genuine scientific concern —for example, the objectivist consideration of the problem of wages, where the earnings of a musician are considered as transitive consumption and hence exempt from any possibility of supply-demand analysis.

also in evidence in many middle-of-the-road positivist methodologists, like Robbins, Hayek, or Friedman. I am referring to epistemological theories which conceive of language as something given and inert, incapable of revision or of any dialectic movement of its own, either a filing system not affected by the content of the file or a knowledge-of-our-own-mind not enriched by intercourse with experience. Furthermore, I consider literalistic a system of thought that conceives of reality as essentially exhaustible in terms of human knowledge, and exhaustible in a unidimensional or extensional sense; i.e., a system which makes no essential room for conflict and ambiguity in our interpretation of reality, and postulates as a possible ideal the final acquisition of a body of total beliefs simultaneously both consistent and complete. I have already shown that the gradualist methodology is an effective weapon in the criticism of these positions. In the chapter on Verstehen I attacked literalism in the methodology of the social sciences generally, and in the chapter on praxeology I attacked literalism in the methodology of economics particularly. Those were direct criticisms. In a way, they were, easy, once the general gradualistic approach was set up, as it is always easier to destroy an argument than to construct one. An indirect criticism may prove more difficult. But I think it is also necessary and, of course, possible. It would be the demonstration of the actual feasibility of diverse models of quite general type where the literalistic position will maintain that there is only one valid representation of economic reality. To this task we must now turn. It will imply an endeavor of "contextual formalization" of the kind that was described in the preceding paragraph.

FORMALIZATION OF THE CONTEXT "SATISFACTION"

As a conclusion which can be drawn from the general discussion of Part. Three we can say that the concept of *substitution* is common to the traditional theories of value and also to the attempts to build a definition of economics.187 That concept comes to the foreground only in the formal approach, whereas it tends to remain in the background in the traditional ones. For the objective theory of value is based on the cost of production of goods, but it makes sufficient and essential use of the concept of echange-value. So does the subjective theory, although it is founded on the concept of individual satisfaction of wants. On the other hand, the classificatory definition of economics puts main emphasis on the causes of material welfare, but is also stresses phenomena of exchange, related to the intervention of money. Similarly, the analytical definition, for instance in Robbins' formulation,¹⁸⁸ mentions scarcity of means but also makes use of the concept of alternative uses of resources. Contrasting utility conception with formal conception we have: Utility theory is normally expressed in terms of subjective satisfaction and, secondarily but essentially, in terms of exchange (substitution). The analytical definition is expressed in terms of scarcity and of alternative uses (substitution). The formal conception expresses itself only in terms of substitution, the semantical and pragmatical "sense of direction" of the system being best confined to the non-formal context of the theory.¹⁸⁹ Two consequences follow from here. First, that greater generality is accomplished with the modified view, since the theory of value itself can serve to

188 "Economics is the science which studies human behaviour as a relationship between ends and scarce means which have alternative uses." Robbins, p. 15.

189 For the role of "equilibrium" in the formal system cf. p. 239.

¹⁸⁷ The paramount importance of this concept is emphasized by Knight in the following passage: "The problem of the role of general economic theory in... predictions is... to show what can be inferred from the general principles... of diminishing utility and diminishing (technical) returns, both of which may be viewed as particular cases of the more inclusive principle of substitution." On the History and Method of Economics, p. 175.

EPISTEMOLOGY AND ECONOMICS

identify the economic subject-matter, without the interposition of a—not necessarily fruitful—strict definition of economics.¹⁹⁰ Second, the defense of the formal approach must clarify the role of the key concepts "scarcity" and "satisfaction," and show that their presence in the formal theory of value can be confined to the token necessary to represent within the formal picture the empirical openness of the system. The attempt to meet this challenge will be the content of the remainder of this chapter. It will consist of two steps. First, to construe value analysis without including "satisfaction" operatively within the model. Second, to construe the description of economic analysis without making independent reference to "scarcity."

That value analysis could be construed as the operation of a model not including the concept of "satisfaction" can be shown in two different ways. Both have in common the same strategy of proof: to point to some consecrated, timehonored, use of the term "utility" or "value" in a sense not defined with the help of the concept of "satisfaction," and to offer a demonstration as to how such use can be generalized so that it covers the ground commonly governed by the sense of "utility" or "value" defined in terms of "satisfaction." The first argument is going to draw from the use of "utility" in the field of producers' goods; the second, from the use of "utility" in monetary theory.

My first argument is this. As it is shown in the following quotation, there is a consecrated use of "utility" that makes no mention of satisfaction but rather uses the more concrete idea of *maximization of monetary receipts*, namely, the utility of producers' goods.

The utility which a business man derives from the things which he buys for business purposes is the extra receipts which he obtains thereby...¹⁹¹

Now, one can alternatively consider *both* the business man *and* the consumer trying to maximize his money holdings. The latter, of course, not by producing commodities but by preserving them or, more generally, by saving the means to buy them—purchasing power. There is no reason in principle why this conception of utility could not suffice to render, from the point of view of model construction, all phenomena of maximization, including those corresponding to consumers' goods.¹⁹² The practical problems the consumer has to solve in order to maximize are, of course, different from the practical problems the producer or business man has to solve. Nevertheless they are formally homologous. The business man may be interpreted as trying to save the most money which comes to him as a result of exchange processes, in spite of the inevitable leakage represented by the cost of production—the hard facts of technology! The consumer, on the other hand, may be interpreted as trying to keep the most of the money that comes to him as a result of exchange

¹⁹⁰ Cf. Hutchison, p. 53. "Assignment of a definition to the word 'Economics' does not appear to solve or even help in the solution of, any useful scientific problem whatsoever." As to possible disadvantages of so doing: "The laying-down of rigid frontier lines between the particular sciences seems an unprofitable undertaking..."

¹⁹¹ Henderson, p. 40.

^{192 &}quot;There is a reason," it could be objected, "if it is the purpose of economic theory to explain. Satisfaction, after all, is the cause of economising, and any application of economic theory contains such a causal reference at least implicity." Now, I will not deny that the purpose of theory is to explain, although the sense of "explaining" must not convey an inscrutable revelation of how reality is "in itself." In most scientific contexts "to explain" will mean "to make sense" in a comprehensive fashion and hence to give logical grounds for prediction. But this comprehensive and predictive explaining can be better assured by an intelligent application of formalism, as the use of mathematics in all sciences would tend to demonstrate.

CLAUDIO GUTIERREZ

processes, in spite of the inevitable leakage represented by the necessities of living or the pleasurable expenditure—the hard, or sweet, facts of life! In both cases, of course, allowance must be made for the contrary operation to leakage, which we could call "injection,"¹⁹³ which is the result of sheer chance or of industry and the providence of nature. "Leakage" and "injection" will have in common their not being result of exchange, and so of being somehow external to the model.

My second argument is this. As it is clear from the following quotations, there also exists a professional use of "utility" which makes no direct use of the term "satisfaction" either, namely, the concept as applied in the macroscopic field to the medium of exchange itself, that is, money. To wit:

We shall have to examine more carefully in a moment what we mean by this phrase 'the value of money'; for the present we may define it provisionally as the power of money to purchase the things people want...¹⁹⁴

Let us notice first that the reference to satisfaction ("the things people want") is not essential to the definition, which can be rendered as "the power of money to purchase other things." When the promised, more careful examination comes, moreover, the matter is set in such a way that it points to the formal extension of this sense of "utility" or "value" to the whole economic domain:

By the value of money we mean something exactly analogous to what we mean by the value of anything else, say bread or cloth: that is to say, we mean the amount of things in general which will be given in exchange for a unit of money...¹⁹⁵

Again, I can see no systematic reason why one could not extend this use of "utility," i.e., the concept of value as all the things which can be bought by the marginal unit, to all commodities other than money. If it is true that "money cannot be eaten," this obstacle does not seem unsurmountable from the formal point of view, which is the one suitable for the analysis of a formal apparatus. For one thing, the macro- and the micro-concepts of utility are essentially connected in the fact that prices of anything equal marginal utility as measured in terms of money, i.e., relative to the marginal utility of money to the purchaser. If one takes the subtitutability of one thing for all other things as central, there is no reason to consider the two examples of substitution-things for money, things for thing-as fundamentally different. One can argue that this twist condems the concept of value to hopeless circularity. Yes, but it is a virtuous rather than a vicious circularity, a circularity of the kind that makes all formalism capable of adequate operation. It is saved from meaninglessness by the interpretation in toto of the formal system at the pragmatical level of the science. Secondly, the impossibility of consumption that seems to be the exclusive characteristic of money tends to be not that exclusive under a purely modelic interpretation of "consumption" as leakage from a closed system where everything is considered an object of study while it remains within the system. It is another question whether this conception is artificial or unfruitful, but it is difficult to see how one could say that it is not consistent and operative. One could even turn the tables around and say that the "consumption" of money in the case of

193 See R.C.O. Matthews, The Business Cycle (Chicago: The University of Chicago Press, 1964), p. 8, for a parallel and very instructive use of the terms "leakage" and "injection."

- 194 D. H. Robertson, Money (Chicago: The University of Chicago Press, 1964), pp. 8-9.
- 195 Itid., p. 14.

the miser or other similar cases (the collector) does in fact equalize the cases of the macro— and the microeconomy. The fact that the "consumption" on one case is more abundant than in the other does not essentially change the picture.

The "macro-micro" argument is especially important because it contributes to the emphasis of the formal character of models in economic analysis. Thus, money is the most formal of all commodities because it is in normal circumstances the most substitutable, consisting in the medium of exchange itself. But all other commodities, precisely to the extent that they are commodities-goods produced under a system of exchange—are also formal in the same sense, although perhaps in a different degree. They are also capable of conception under relations of substitutability. Money represents in an ideal or perfect way the unity of the system of substitution. But all other commodities do also in fact represent the same unity, much in the same way as the monads of the Leibnizian world were said to reflect the whole universe. Hence, we are entitled to say that they have value because of the same reason money has value, namely because they can exchange for other things. So, for both money and all other commodities one can define their value as the alternative sum of all the other things that can be bought by the marginal unit of the particular commodity, be it money or anything else. The most noticeable difference between the two cases, which is no disproof of the feasibility of the unitarian conception of utility, is that money is less committed to "get out" of the system. It tends to remain longer within than do the goods it buys. Those are, of course, and by hypothesis, on their "way out" of the system. On the other hand, money is always in danger of disappearing wholesale from the system by monetary reform. An investigation of this difference, without impairing the formal conception of value, could lead to a development of the different chapters of economic theory: the theory of prices, the theory of money, and even the theory of the cycles.196

The conception of economic theory in terms of relations of substitutability, however, must not obscure the fact that a *solution* for the equilibrium system is always needed. That is why my first argument is important; the fundamental homogeneity of utility of producers and utility of consumers point to the essential role of (quantitative) maximization for the determination of the system of substitution. This need of determination, which seems to be a clear case of the general requirement of logical closure of a theoretical system, does not alter the formal or syntactical character of the system. The *interpretation* of the equilibrium point may run in terms of "satisfaction"; but in itself it is capable of being understood as the simultaneous determination of all the functions involved in a particular problem.

We still have to come grips with the major objection of artificiality. Is it not artificial, and hence unfruitful, to present the bulk of economic theory as something having nothing to do with the very reason for the existence of economic activity, namely the satisfaction of individual wants and desires? My answer to this is that in fact it is artificial, in the sense in which all symbolic processes, but most especially the formal ones, are artificial. Is not the very invention of money as a representation of the indefinite mass of goods artificial in the same sense? As a matter of fact, a similar charge of artificiality or unnaturalness has been made against practically every kind of formalism or mechanical device which, in the end, has turned out to be very beneficial to mankind. In particular, I am thinking of the ways and methods of modern science and technology. Now, the chances of a given mechanism being a blessing rather than a curse are enhanced rather than diminished by an adequate understanding of its real nature and of the principles can only result in bad performance of the machine, in tragic misunderstanding of its purposes, and eventually in self-fulfillment of the omen that branded it as inhumane in the first place.

247

¹⁹⁶ No aprioristic claim is made, however, that this development could be otherwise than empirical.

FORMALIZATION OF THE CONTEXT "SCARCITY"

The first step of my indirect criticism of literalistic economic methodology is now complete. Let us try to build up the second step, the one that has to do with the possible description of the tools of economic analysis with no independent reference to the concept of "scarcity." In this argument I shall be contending again with the literalistic position. For one thing, I will be attempting to show that what "scarcity" stands for can be better understood as belonging in the non-formal side of theory, the field of the assumptions. Secondly, as a precipitate of that discussion, we will be establishing that a logically strict definition of economics with the intervention of "scarcity" is not possible and that, perhaps, a definition of economics in general is not possible at all. In provision for the latter we are refraining from trying to accomplish an alternative "definition" and are aiming simply to a "description," not of economics generally, but only of "economic tools of analysis."

Let us begin by re-stating the literalistic definition of economics as an object for examination:

Economics is the science which studies human behaviour as a relationship between ends and scarce means which have alternative uses.¹⁹⁷

There are two parts to this statement: the reference to *scarcity* with respect to means, and the reference to *alternative uses*. As it was hinted in the last section, we are happy with the second part. Our task is only to show that the first part is not formally independent of the second part, at least in what is not confinable to the non-formal assumptions.

Let us now take the concept of "scarcity" and try to explicate it. First of all, let us suppose that its meaning is coincident with the meaning of "limitation," so that when one says "scarce means" he is saying only "limited means." Then one can wonder whether to say that resources are scarce is not the same as saying that the data of the problem must be somehow given, so that one has to consider the resources in fixed quantities.¹⁹⁸ Is it not the case that the goods the economic point of view considers interesting are goods that are first of all *possible?* But, if they are possible, then they are also somehow given. Their limitation is not, so to speak, internal to the problem, but rather a very general condition for all problem-solving, i.e., the requirement that one must have some premises in order to decide an equation. If the goods are impossible to obtain, I do not care about them; if they are possible, then they are given. Is there something in between? Yes! They may be possible *relative to* the relinquishing of some other possible (in the same sense) goods. But this special sense of "limited" is precisely the explication of the normal sense of "scarce" that seems relevant to economic considerations. What it all amounts to is that the meaning of economic "scarcity" is totally dependent on the concept, more amenable to formalization and hence clearer of "substitution." Something is scarce if it is affected by competing ends, that is, if its different units are subjectively differentiated and capable of taking the place of one another in the event of deficient provision, as explained in the last chapter.¹⁹⁹ Scarcity appears as "relative limitation" or, if you wish, as the need of equilibrium determination of a system of substitution.

¹⁹⁷ Robbins, p. 15.

¹⁹⁸ Or, if one is not ready to concede this, at least he might be willing to accept that this is a case of *formalization of context*, and that this interpretation will be the formal projection of the sense of the term "scarcity" within the formal picture, in agreement with what was said before about the matter (p. 243).

Let us now recall what is normally understood by "definition" in the logical sense. To take a classical formula, a definition is always intended per genus proximum et differentiam specificam. In the case under study the former seems to be "the use of scarce means" whereas the latter "for alternativ uses." If one poses the problem in these terms it becomes apparent that something is wrong with this definition, since the term "scarce" seems to be hopelessly dependent on the term "alternative uses." So, either the term "scarce" has not the intended meaning at all-which is difficult to substantiate-or else the definition incurs a very peculiar kind of circularity, i.e., not circularity between definiens and definiendum, but rather circularity between genus and differentia.200 The upshot of this is, to my mind, that a strict logical definition is not possible in this matter, and that the ones in existence, like Robbins', should be understood "mythologically," i.e., as expressing in a quasi-formal way some unformalizable truth, in this case the more-than-logical nature of the economic point of view. In sum, the problem of definition of economics seems not to be independent, but on the contrary seems to be utterly dependent upon the problem of the theory of economic value.201

Moreover, a literal interpretation of the definition of economics can be misleading on several accounts. In the first place, many problems take into consideration "scarce resources" not as objective data, as it were, but rather in relation to the solution, right or wrong, of the economic puzzle itself. In many particular instances the resources are "abundant" for the purposes at hand, for example the making of a chair out of a particular board of wood, provided you solve the problem rightly. If you do not, then, but only then, you will have scarce resources. In this border-line case you really cannot say at the beginning whether you are faced with scarcity or with abundance.²⁰² The question is further complicated by the fact that the votaries of the literalistic conception tend to negate that an abundant means is a resource, by definition of what a resource is. Therefore, one is led into the confusion of not knowing at the beginning whether one has resources at all or not. The chair-board puzzle becomes in this way a rather embarrasing question for the scarcity definition of economics. As an objection, one could be reminded on the point that the economic aspect is always related to the case of more than one end. True, but remember what the nature of my problem is. I am trying to stress the fact that it is precisely that property of economic matter which is fundamental. My argument is precisely that scarcity is scarcity only with respect to ends. Accordingly, we can modify our puzzle and talk, for example, about the chair-table-board problem. Here the question will be that table and chair are both competing for the use of the board and, it could be said, it is here where scarcity begins to be important. But wait. This is relative limitation of resources, in the sense explained above, and this is nothing different from the principle of substitution itself. "Scarcity" is here a subordinate non-separate concept. It could

200 One may object: "But the idea of 'alternative uses' merely adds to 'scarcity' that the scarce resource can be 'economized,' i.e., allocated efficiently with respect to utility." Sure, and this is a way of showing how the circularity mentioned in the text is ultimately a circularity of the ordinary type: you are using now clearly the *definiendum* (by way of 'economized') within the *definiens* (by way of your explication of the term 'alternative use'). As for your 'i.e.'' clause, it clearly shows the dependence of the 'definition approach' onto the 'theory approach,'' as pointed out in the sequel.

201 This statement expresses a truth that seems to be considerably more general than the use I make of it in this context. In fact, the non-independence of the "definition approach" from the general problem of the foundations of theory is valid for all the sciences. The convivial knowledge with a certain type of scientific activity enables the professional to venture some definition of his particular science; but this definition remains unintelligible for the non-initiated, short of a full explanation of the theoretical assumptions that would amount to an initiation. On the other hand, the definition would be superfluous for the initiated, except perhaps as a "clarification of mind." On the concept of conviviality, cf. Polanyi, pp. 203 ff.

202 To work "with economy" is a sign of craftshmanship. Cf. A. L. Macfie, "What kind of experience is economizing?" *Ethics*, LX (October, 1949), 20-21.

not even be thought of without the simultaneous intervention of the concept of "alternative uses". The whole problem of allocation is the problem of how much I desire or value anything *with respect* to all other things that enter into consideration.

Again, it is even possible to conceive of a problem of allocation in the utopian case of unlimited resources (or means). If time comes at all into the picture, not in the sense of actual duration but rather of priority of accomplishment, then one could still be interested in deciding by economic calculation what to do next.203 If this be accepted, then one would be able to say that the economic problem is one of deciding what the state of a (modelic) system of substitution will be, how the facts of wanting and the facts of producing will have to be coordinated for equilibrium to exist within the system. There are here several logical possibilities of combination: (a) one could have a moving level of fixed (at each level) resources and a moving level of fixed wants; (b) one could have fixed resources and fixed wants (board puzzle); (c) and (d) the obvious combinations of the extreme cases. Of course, some of these possibilities are uninteresting or irrelevant, especially for particular cases. But it is convenient to describe them all in order to recognize the crucial importance of the concept of substitution in the models and the method of economic analyses. Robbins has said: "The exchange relationship is a technical incident, a technical incident indeed which gives rise to nearly all the interesting complications, but still ... subsidiary to the main fact of scarcity."204 I would rather like to say that substitution is the general characteristic of economic subject-matter; the interesting complications arise in regard to the incident of scarcity.

Finally, let us consider another argument employed in the defense of the scarcity definition of economics, namely, the presence of scarcity in all problems of human action and human choice. The fact that one has always to choose, that is, to renounce all but one alternative in every action situation, is seen as a proof of the all-important character of the concept of scarcity to practical reasoning. However, this argument can well backfire. This universality of the term means that "scarcity" is a vague and ambiguous concept and it is very plausible that when it is applied to "all problems of human action" it is being taken in a different sense from the sense in which it can be relevant for the definition of economics. In the former sense it means the fact of givenness of the objective conditions with which the chooser is confronted. There must be a quantitatively singular, objective, and concrete configuration in order for a person to have to choose. One may represent that situation with the letter "Y" as the figure of a bifurcation of roads. One can even be confronted with a choice between wholly incompatible alternatives, like marriage and celibacy, and many other ultimate decisions. This is a most general assumption in practical reasoning. Yes! But the sense of scarcity that is relevant to economics is not that scarcity. It is scarcity as progressively more valuable leftover, and in ultimate decisions there cannot be leftover whatsoever! Ultimate decisions are ultimate because of their implying a commitment of the whole person of the chooser, hence an incapability of gradual or partial realization. This being the case, it is logically impossible to treat both ultimate alternatives as parts of a single equilibrium system of competitive or relative realization. They require, by their very nature, each to be considered as a whole system in itself. In other words, ultimate alternatives are not *divisible* and they are not *substitutable* in the economic sense. "Scarcity" is a poor substitute for the metaphysical concept of "contingency." On the other hand, existential choice cannot be

²⁰³ This possibility is obscured by the inveterate tendency of the literalist to consider "time" as a scarce resource on the same footing as all other means. I think one should be able to see time, or at least some aspects of it, as the existential ground of action and not simply as one of the factors of it.

EPISTEMOLOGY AND ECONOMICS

calculated, only phenomenologically described. Two senses of "scarcity" are to be distinguished and only one is relevant to economics, the one dependent on the concept of "substitution." Two senses of "choice" should be distinguished, "existential choice" and "economic choice." We will do well to leave the first alone lest we make a theology out of economics.

"SUBSTITUTION" AND PREDICTION

As we have seen, the operatively important concept in an attempt to build a definition of economics is "substitution" or "alternative uses." I want to say some final words before leaving the topic of formalism about the function of that concept as an instrument of prediction. Prediction is the result of the logical operation of induction. But induction in the ordinary sense is based on the non-formal assumption of regularity of nature, e.g., on a transcendent belief in the idea that nature was constructed following some divine blueprint. Economic induction rests on a similar yet different assumption: regularity of man and belief in the pervading influence or persistence of economic plans. Now, economic plans are no bluprints. There is a basic distinction between an economic plan and a blueprint in the importance of "curves" in economic calculations. Resources, one could say, are assumed as given for every point in the curve. But the strategic element is the motion up and down the curve, which is dependent on the concept not of "givenness" but of "alternative realization," the concept of "substitution" itself. Generality of prediction is all-important in economics as in any other science. But this economic generality is generality through disjunction rather than generality through conjunction as in the common understanding of the principle of induction. The force of economic induction or prediction must lie in the fact that there exists a formal method of solving general problems of "alternative realization" (equilibrium analysis) and that people do tend to employ it, consciously or unconsciously, and with different degrees of success. Economic calculation must provide an answer for wide ranges of possible alternative situations, not simply for multiple single instances. This can only mean that "substitution" is the key term in economic analysis. If one does not realize this, one is in danger of being led into thinking, apart from the literalistic temptation, that all problems are already solved when the economist begins his work, having only a sociologist's job to justify his call.205 Curves, not points, are the fundamental economic "entities"; functions rather than magnitudes. This does not mean, though, that substitution functions must be thought of as being all definite, infinitely determined in both directions. On the contrary, one has to allow for the fact that most of the time our only clear interest is to commit ourselves the least we can, trying only to assure the maximum command upon the universal medium of substitution, whatever it may be in the concrete case. The universal desirability of the token of purchasing power, the most typical case of economic situation, seems thus to be the practical way in which we can expect to have functions—economic plans—at all. This is so, of course, because of our great lack of knowledge about the future states of the world around us and also about our future-and present-needs and desires. This is a general fact not necessarily connected with concupiscence or drive for unlimited power on the part of man, but rather rooted in our weakness and ignorance, and in the fear of what the future could have in store for us. Our modal goal, to borrow Lowe's expression, responds in the last analysis not so much to the question of how much I desire anything relative to all other things: rather it responds to the more indeterminate question of how much I desire things relative to those goods-in general, purchasing power-which commit me the least and assure the maximum freedom for continuous future choice. It is freedom to choose rather than the things we choose which proves to be the determinant in regard to alternative generality.

CHAPTER XI

THE CASE FOR A PROFESSIONAL CONCEPTION OF ECONOMICS

As it has been shown, a strict definition of economic science in terms of "scarcity" is impossible, because of hopeless circularity between the two indispensable parts of the definiens, that is, between genus and differentia specifica. In other words, because of non-separability between "scarcity" and the concept of "substitution." Other approaches to a definition of economics are inadequate for a number of reasons also.²⁰⁶ The conclusion seems inescapable that the attempt to find a definition of economics is misguided. No wonder! The impossibility of giving a formal definition of a science is parallel to the impossibility of giving a formal support to the theory of that science. The problem of definition of a formal apparatus reduces naturally to the problem of justification of the theory which underlies the function of that apparatus. In the concrete case under study, the problem of the definition of economics reduces itself to the problem of the theory of economic value. But the impossibility of finding a definition is not a dismal situation after all, because some solution of the problem of theory is available and this can double as a solution for the problem of definition. We have found that a definition is not possible, but we can also say that it is not indispensable either. The definition is not possible because we cannot hope to support the formal with the formal. Rather, we have to support the formal with the non-formal and, ultimately, with the unformalizable. But the definition is not indispensable because the non-formal tradition which surrounds the described formal tools and makes up the family of professional scientists is there to tell us, in an effective way, what economic science is, not so much by word of mouth but by example. The practitioner of the science must know his formalism and must be tacitly familiar with his non-formal assumptions. He must know what he is doing and that he is doing economics. But it would be too much to ask him to articulate or state in rigorous terms all that knowledge.

There was proposed in the last chapter a characterization of the tools and method of economics in terms of the analysis of the concept of "substitution" It was also stated that the problem of definition of economics is parallel to the problem of theory. One could now add that the matter covered in that chapter corresponded to the modelic side of theory because of its being an examination of the operative aspects of it. As we know, that side is not self-sufficient and must rest upon nonformal foundations, i.e., on the assumptions. In a corresponding way one could say that a necessary complement to the *formal conception* of economics is the *professional conception* of it, the latter being, as it were, a parallel-to-the-assumptions side of the problem of "definition." This professional conception will have in the field of "definition," with respect to the formal conception, the same kind of relation that the assumptions have with respect to the models, in the field of theory. That is, it is nonformal and at the same time offers all the needed support for the formal or operational aspects.²⁰⁷

The main objection for an application of the above concepts to the case of economic methodology stems from the epistemological position of literalism, common ground, as we have seen, of apriorists and positivists. We could best formulate that objection, in a comprehensive fashion, as the *literalistic dilemma:* "Either economic theory is abstract—result of abstractive operations of the mind upon sheer empirical reality—or it is a priori—the elaboration of something you find in your own mind. You cannot have it both ways." As an illustration of the two horns of the dilemma we can use the following quotations:

The conditions under which men produce and exchange vary from country to country, and within each country again from generation to generation. Political economy, therefore, cannot be the same for all countries and for all historical epochs.... Political economy is therefore essentially a historical science. It deals with material which is historical, that is, constantly changing; it must first investigate the special laws of each individual stage in the evolution of production and exchange, and only when it has completed this investigation will it be able to establish the few quite general laws which hold good for production and exchange in general...²⁰⁸

What we know about the fundamental categories of action—action,economizing, preferring, the relationship of means and ends, and everything else that, together with these, constitutes the system of human action—is not derived from experience. We conceive all this from within, just as we conceive logical and mathematical truths, a priori, without reference to any experience....²⁰⁹

The dilemma is an objection to the two-leveled conception of economics because it forces upon us a choice between the pure empirical and the pure rational which seems to destroy the possibility of a heuristic creative interaction between the two poles. It is my conviction that the dilemma is unacceptable in itself, and that the creative interplay does exist. But the answer to the challenge of the exclusive alternative cannot, by its very fundamental nature, be straight-forward. The prima facie extraordinary force of this dilemma is, I think, the product of a tremendous oversimplification of the problem of knowledge. It is only through a complete rebuilding of the context of the question, of its epistemological surroundings, as it were, that one can be enabled to see that this destructive argument does not hold. In order to answer this objection one has to look all the way into the nature of knowledge in general, and of scientific theory in particular, with special emphasis on the problem of language and on the problem of the essence and function of abstraction. Nothing short of this, to my mind, is capable of clearing the ground for a correct posing of the problem of economic knowledge and the "dispelling" of the above exclusive alternative. But all this has been precisely the content of most pages of this study which is conceived as a criticism of the literalistic position.

²⁰⁷ It is not enough "to be told" the formal conception of a science to fully understand what the science is. An interesting case of this dislocation occurs, for example, in the "exportation" of knowledge. "While the articulate contents of science are successfully taught all over the world in hundreds of new universities, the unspecifiable art of scientific research has not yet penetrated to many of these..." Polanyi, p. 53.

²⁰⁸ Frederick Engels, Anti-Dühring: Herr Eugen Dübring's Revolution in Science (Moscow: Foreign Language Publishing House, 1959), pp. 204-205.

²⁰⁹ Ludwig von Mises, Epistemological Problems of Economics, trans. George Reisman (Princeton: D. Van Nostrand, 1960), p. 14.

One would have to review those pages to find the complete answer to the literalistic dilemma.

Some other difficulties for the formal-professional conception of economics seem to arise with respect to the plurality of levels which is implied in this approach. An objection along this line could take the following form. "The professional or formal conception of economics is systematically ambiguous in its use of the concept of 'system,' a word that seems to be used alternately and without much warning either as meaning a theoretical framework of analysis, or rather a quasi-physical functioning machine, the economy itself. This is especially unfortunate because it seems to preclude the possibility of correctly delimiting the field of action of the economist without resort to circular reasoning. The professional conception seems to characterize economic science as the science which is practiced by those concerned with problems of substitution, and these problems are described as those connected with the functioning of the economic 'machine.' How can one get out of the circle?" My answer has several parts to it.

First of all, one would do well in not condemning off-hand all circularity in reasoning since, after all, it is on circularity that all kinds of theoretical thinking are based. Even the most rigorous forms of argument, e.g., those present in formal systems of logic, are circular in some sense, since the theorems are somehow present in the axioms and the axioms might be also considered to be somehow present in the theorems. In fact, and in this sense, to be "circular" is the opposite of to be inconsistent, as the case of the formal systems of logic would tend to demonstrate. The important thing is to safeguard ourselves against poor or prenature circularity, not to eliminate circularity altogether. One has to have a circuit in order for electrical devices to function. But one will do well in avoiding short circuit. Having clarified that, I will readily admit that some kind of circularity is present here. I will contend, • though, that it is the "good kind" of circularity, rather than the short-circuit kind.

Secondly, it is convenient to remember that the intention of the present conception is not to build a definition of economics, but rather to show, as it was explained in the beginning of the chapter, that the definition is both impossible and superfluous, and that the problem of characterization of economic subject-matter or economic activity is nothing separate from the problem of justifying economic theory itself. Now, in relation to its own problem, as it has been stated, the "circularity" of the professional approach is not a shortcoming but rather an asset, because it enables us to better understand the meaning of the assumptions by clarifying the models and the function of the models by a correct appreciation of the general sense of the assumptions.

Finally, the presence of two levels of discourse in all this area of investigation is undeniable. It is precisely one of the main points of this essay to stress this distinction, as against the temptation of the social scientist not to recognize it.²¹⁰ Of course, there are things which exchange for one another with respect to ownership, use or possession of individual or collective persons. They may well be represented by models which form systems of formal sort. There are also theoretical assumptions which govern the models in a systematic way, comprehensive paradigms or frameworks of interpretations. These two levels are furthermore connected in a twofold way. Not only the latter is represented within the former by means of the tokens of theoretical notions, but also some of the empirical elements of the model, those standing for persons, are understood as capable of entertaining assumptions. The distinction of the two levels is not an obstacle, however, for the recognition that in one of them, the modelic, the functioning of a system of substitution is an operative principle. Neither is it an impediment for saying that around this system an accadem-
ically organized group of people may be distinguished as the professionals of economics.

A third type of difficulty that should be met is this. "Is this conception really a different interpretation of economics from the conventional one? Or, in other words, is not the literalistic approach equally good as the formal-professional to render satisfactorily all the relevant practical and methodological problems? What is gained by having this new conception?" I will answer in two stages. First, the reasons to have a "new" approach are not clear because the approach has been around for a long while both in literature and in professional practice. The methodological task is now only a matter of removing some inconsistencies from the framework of the science or, better still, from the idea that the scientist has about this framework and its corresponding application. Also, it is a matter of making explicit what many writers have been implying in one or another way in their treatment of several particular methodological problems. My point is that, in fact, no one nowadays sticks to the full implications of an aprioristic definition of economics for example in relation to the singleness of model that it appears to prescribe. On the contrary, a review of the current economic literature leaves the impression of a healthy diversity of models, approaches and angles of attack to economic problems which seems completely at odds with the unitarian and closed outlook one would expect from aprioristic minded practitioners. All kinds of imaginative devices seem to be the order of the day.²¹¹ The impression one gets from such a review is also that the aprioristic confidence in the predictive power of economics has very much faded away, being replaced by more caution about the results one may expect from the application of theory. This new attitude is seen in the growing use of statistics and the probabilistic method, as against the straight-prediction method cherished by praxeological thinkers.²¹² A possible rejoinder to this argument may be that the discrepancies between the present actual conception of economics and the aprioristic conception are due to the fact that we have taken the latter, as it were, in its germinal or straight-forward formulation-as for example Robbins. But one can elaborate on this germinal form so that it proves able to cope with all the new models and theoretical devices, so that the new comes to find honorable place within the old. The rejoinder is merciful and plausible enough. The fact is, however, that by this method of reconciliation the old paradigm must to be indistinguishable from the new and we would have no disagreement left.

Secondly, I would like to present some concrete reasons why the professional conception of economics should be preferred to the a priori conception. These reasons, I hope, will show that the professional conception is more consistent with actual professional practice, and more fruitful in the way of clarifying the processes theorists, practitioners, and political economists are engaded in.

a) Common usage decides against the aprioristic conception, both in the professional sphere and in ordinary life, when it reputes *economic* the phenomena related to some kind of circulation of money—witness the pejorative connotations as to what is *only* economic, in common parlance. The aprioristic conception brands as potentially economic all realms of social reality. The formal-professional tends to stress the basic difference between two distinct orders of reality: the order of what

²¹¹ For a good summary of this literature, cf. Johnson, and also Matthews.

²¹² Cf. Robbins, (p. 110): "Economic Law describes inevitable implications. If the data they postulate are given, then the consequences they predict necessarily follow..." This is a clear case of assumptions being made to play the role of models. As for probabilistic laws, cf. Mises, *The Ultimate Foundation of Economic Science* (p. 56): "There is no such thing.... People resort to the methods of statistics precisely where they are not in a position to find regularity in the concatenation and succession of events." Clearly the very stuff of which empirical science is made, something between necessary assumptions and sheer historical happening, is denied.

is sustitutable, and the order of what is unique. Furthermore, essential to the professional conception is a gradation of meaning in the different applications of the term "substitution," emminently apt for describing means of living, but only by extension applicable to less physical entities. No such shading is founded in the aprioristic conception, at least, not in the usual presentations of it.²¹³.

b) The aprioristic conception tends to defend itself against refutation by the dubious dialectical resource of distinguishing sharply between "relevance" and "truth." "Of course, if other things do not remain unchanged, the consequences predicted do not necessarily follow...."²¹⁴ This is a short step from self-satisfaction and nonconcern for the development of science. The spuriousness of the distinction has been, I hope, sufficiently demonstrated in the first part of this study and again in the chapter on praxeology. Its main evil is that it forces upon us the oddness of having to talk about relevance in all the cases in which we would want to speak about truth, this last term being reserved, with no sufficient reason, to designate only logical consistency or the necessity of very general and fundamental assumptions. But the fact is that this typically literalistic twist is a perversión of the normal, time-honored use of the word "truth" and in practice to say that what one has to investigate is the relevance of particular "theorems" is nothing substantially different from saying that what one has to investigate is the truth of the hypothesis.²¹⁵ After all, it is-or is it not?-the discovery of truth that is intended as the permanent mission of science. On the other hand, any theoretical formulation is "true" in the weak sense of being logically consistent, self-validating, or "circular." Otherwise it would not be even thinkable! Of course, a "theory" can be inconsistent, but then it is not theory at all.

I am thinking, for instance, of the attitude that over-investment theorists take in relation to the problems of the business cycle.²¹⁶ They assume that the basic principles of utility theory, not enriched by laborious empirical investigation, can suffice directly to give a full account of the concrete phenomena of business life. When their theories are applied and found faulty it is no defense, to my mind, to say that the theory is true, although irrelevant in this particular case—presumably still fully valid, and very relevant, in all the untested cases. I would like to say in this situation that the theory is false, not in the sense that its content, basic principles of economics, are not valid, but in the sense that it is a false modelic representation of reality. One should always distinguish the two roles that a piece of information may play, either assumption or model, and make sure that the assumptions do not take the place of models perverting, by so doing, the very purpose of a particular science, which is to accurately represent empirical reality.

c) I have no doubt about the politically conservative implications of the aprioristic approach. It is only natural that non-tolerant methodology will tend to associate itself with an intolerant political philosophy, defender—first and foremost—

- 215 The difference seems to be only that if one speaks of "relevance" then one has a readymade excuse just in case something goes wrong, i.e., "the relevant conditions were not present."
- 216 Cf. Gottfried Haberler, Prosperity and Depression: A Theoretical Analysis of Cyclical Movements (New York: Atheneum, 1963), pp. 33-72.

²¹³ It is interesting to note that under our conception it becomes transparently clear that pure substitutable and pure unsubstitutable matter do not exist, due to the distinct fact of the asymptotical character of paradigms (see fn. 185). Rather than a clear-cut "economic aspect" being discernible in every problem, I would like to say that every problem is economic up to a certain degree, i.e., the degree of substitutability of its own subjectmatter.

²¹⁴ Robbins, p. 112.

EPISTEMOLOGY AND ECONOMICS

of the status quo. It is argued, contrariwise, that a formal (in the literalistic sense) methodology assures neutrality in the application of economic theory.²¹⁷ But the fact is that man is a passionate animal and if, in trying to assure value-neutrality to economic reasoning, we declare economic theory to be unconcerned with non-formal matters, passion will tend to enter again through the back door, as social injustice and predatory acts performed on the weak by the powerful. It offers no justification that these acts are performed under the pale of contentless, purely formal, "neutral" theory.

An objection can possibly be raised. "The professional conception of economics, it seems, does not necessarily avoid this conservative bias. It can be as 'neutral'-insensitive to social problems-as the aprioristic conception. Furthermore, it shares with the latter a dangerous formalism that would tend to ignore certain types of problems, e.g., the problems of the external cost of goods-tips, bribes, commercial espionage-as belonging in sociological theory, and sociology in turn not occupying itself with them because reputing them-rightly-as strictly economy." The objection does no harm, although it is well conceived. The professional conception precisely tends to emphasize the role of the background or training and responsibility of the scientist in the methodological consideration of his science. This is applicable to the attitude of the scientist in regard to the classification of problems and the organization of his particular discipline and its particular procedures. Hence, the social sensitivity of the professional-again we have to rely on the sense of responsibility of the scientistwill always be determinant in the application of theoretical models to the problems of social policy and the specific questions of social concern. In particular, unorthodox phenomena like external cost, or social cost, which will tend to be overlooked in the aprioristic approach as only "conditions" under which economizing takes place, will be strikingly noticeable in the professional conception. It makes the ethical and practical training of the scientist an essential category in the characterization of his own discipline. No case of no-man's land will present itself. The aprioristic approach makes for sharp boundaries among the sciences. Not so the professional conception. Rigid departmentalization has no place in it. It does rather allow for the common use of particular models and concepts by different social sciences and always assures professional treatment of border-line cases which could not be adequately "plugged" in the conventional divisions of aprioristic classifications of the sciences.

d) Finally, the professional conception will not commit the scientist to the single use of particular models. It will encourage him to look for as many as he can find use for, and to apply with much flexibility an array of such models to his different theoretical and practical problems. Contrariwise, the aprioristic tendency has shown itself committed, if not in theory at least in practice, to a single model, the one very aptly analyzed by A. Lowe as the "extremum principle."²¹⁸ This is nothing but the most primitive model that is possible under the assumptions of utility theory, with no complications arising from the interposition of less general, more empirically founded, hypotheses. Thus it is fair to consider this model, as we did before, as the very assumptions taking up for themselves the role of the models. It is characteristic of the classics of this approach to try to solve all the problems of economic theory by means of the application of this single model. They always try to reduce to a very minimum the interposition of research-originated laws—the "few auxiliary hypotheses" of praxeology. All this against the plurality of theoretical

217 Robbins, pp. 132-34.

218 "When acting as sellers marketers must be inspired by the goal of maximizing money receipts, whereas as buyers their action directive must be the minimization of money expenditures..." (Lowe, p. 36).

resources that the professional conception will induce us to employ. Witness, again, the sharp contrast between the contemporary treatment of monetary problems or of the problems of the cycle, with multiplicity of models considered to operate simultaneously or alternatively, and the "classical" monetary theorist, with his singleminded determination to explain everything by means of unsophisticated derivations from basic utility theory.²¹⁹

One could reply that diversity of models is also possible in the aprioristic view. They would function within the general theory so as to explain different empirical conditions of economizing, e.g., there are "monopoly," "competitive conditions," "cartels," etc. These apply the basic theory—causal principles—to different types of situations. All this is true. Still, the tendency is, as explained above, to conceive of these types "after the image and similarity" of the fundamental assumptions, with no interposition of broad empirical laws which would give positive content to these theoretical generalities. The types are the result of a "transcendental deduction" more than functional devices appropriate for the empirical development of the science. To the extent that this remark is not applicable to a particular "apriorist" then, and by my definitions, he is not as apriorist as he thinks he is.

What bothers me the most in the writers of these aprioristic tendencies is a characteristic two-faced attitude in their methodologic strategy. When they are examining the foundations of their theoretical edifice they take all pains to demonstrate that their schema is completely general and capable of accounting for all kinds of situations and empirical conditions. But when they are not examining those foundations but rather explicitly trying to solve some specific problem, they reason as if no more tools were available than the plain straight application of the most general principles of theory. I think this unfortunate situation is possible among very able people only because they have an insufficient comprehension of the nature of formalism, its uses and its limits. This tends to give rise to a misguided appreciation of intellectual simplicity. When problems seem very complicated we call upon the clear world of ideas to be saved from the roughness of daily life. But complexity arising from the very hardness of the problems facing us must be respected. It should not be taken as an excuse to seek refuge in "simpler theory" and condemn empirical reality to the irrationality of the "historically given." Social science is capable of faring much better than that. It might not given us "all the light" we expect to receive-which science does?-but the failure of "absolute knowledge" must not make us disparage the "relative knowledge" we may still prove able to conceive. Formalism may be said to be the "clear" section of the knowledge we can possess. It will always be accompanied by that other, "obscure," knowledge implicit in our tacit assumptions.

Formalism must, to be a useful device, remain, as it were, under the full command of the person of the theorizer. This control is hardly guaranteed by the "transcendental deduction" of the praxeological approach, which is an unknowing process of dialectical retro-definition, as we have seen. The result of such an approach can only be the artificial simplification of the intellectual resources of economics, much of the same sort as that mystification arising from positivist linguistic rigidities: loss of heuristic momentum and premature closure of the active conceptual ingredients of the science. An aprioristic conception seems to go against the idea of the developing reality of an open science, a science in the making, which economics undoubtedly is and should continue to be. It is to the clear benefit of such a science to use, as the professional conception encourages us to do, a multiplicity of formal approaches. It is also to its profit to use broad experimentation of methods. The professional must be aware both of their limits and of their potentialities, so that he contributes to the progress of this, the still less than exact although most rigorous of the social sciences.