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Philosophy, Science, and Everyday Life

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AT THE END of an illuminating analysis of the extra-scientific presuppositions that went into the making of modern physical science, Edwin A. Burtt wrote almost half a century ago: "An adequate cosmology will only begin to be written when an adequate philosophy of the mind has appeared. . . ."[1] This was the conclusion to which Burtt was led after a survey of historical evidence on the formation of scientific thought during the period from Copernicus to Newton. What is required for a foundation of scientific knowledge is more than a science that analyzes "nature" and its structure. We have such a science and its achievements are well known. What is required is also more than a science that analyzes man as part of nature. We have such a science as this, too, and its results, considerable even today, promise to become more impressive in the future. But what is required in addition is a science that analyzes man as that peculiar part of nature that is not only capable of understanding "nature" but also of understanding itself as part of it. We do have sciences that are beginning to grapple with this peculiarity of man's position in nature, but it must be conceded that our knowledge here is uncertain and there is much confusion about the methods appropriate to such sciences.[2] And,

1. Edwin Arthur Burtt, *The Metaphysical Foundations of Modern Physical Science* (1924; rev. ed., Garden City, N.Y.: Doubleday, n.d. [1932]), p. 324.

2. Modern philosophical anthropology represents the most significant response to the joint challenge to philosophical reflection offered by evolutionist biology, historicism and structuralism in the social sciences, and the beginnings of ethology (Julian Huxley, Jakob von Uexküll, and

finally, what is required is a philosophy that provides clear and reliable methods of reflection on the nature of the evidence on which these various sciences are founded, *and* on the nature of the evidence on which such a process of reflection itself can be firmly based. The philosophy of the mind called for by Burtt must therefore be more than a narrowly conceived philosophy of science. In fact, Burtt postulates the need for a general and critical theory of knowledge, "critical" to be understood as an extension of the Kantian sense of that word. Obviously, this need has not been fully met to this day, but it is my conviction that under certain conditions phenomenology, and especially the program and the achievements of Husserl's last work, can satisfy it.[3]

Husserl saw that modern science, having separated itself from philosophy, no longer provided answers to certain elementary questions that men have asked at all times. He also saw that the empiricist tradition in modern philosophy (going back to Locke and Hume) and modern science was beginning to formulate as a problem something that to him too was a problematic

others). Adolf Portmann is a biologist and ethologist as much as a philosopher. Max Scheler's philosophical anthropology, on the other hand, is still strongly marked by metaphysical speculation. Better known to philosophers in the English language is Ernst Cassirer's study of symbolic forms. Arnold Gehlen's work is directly linked to the theoretical concerns of cultural anthropology and sociology, but for a number of reasons has generally exerted influence only in German social science. The most important nonreductivist and nonidealist philosophical connection between the problems of biological and sociological theory is established in the work of Helmuth Plessner. The relevance of his thesis on the "eccentric position" of man in nature and the implications of this thesis for an account of the "natural artificiality" of man as a body and as a social and historical being are slowly being discovered outside the German cultural milieu despite a lack of translations. An English appreciation of Plessner, as well as of Portmann, Buytendijk, and Kurt Goldstein, can be found in Marjorie Grene's *Approaches to a Philosophical Biology* (New York: Basic Books, 1968). A brief but interesting Marxist critique of modern philosophical anthropology, and of the way in which sociologists use its resources in their theorizing, is presented by Paul Walton, Andrew Gemble, and Jeff Coulter, "Philosophical Anthropology in Marxism," *Social Research*, XXXVII (Summer, 1970), 259–74. For a kind of response *avant la lettre*, see Plessner's "De Homine Abscondito," *Social Research*, XXXVI (Winter, 1969), 497–509.

3. Edmund Husserl, *Die Krisis der europäischen Wissenschaften und die transzendentale Phänomenologie* (The Hague: Nijhoff, 1962); English translation by David Carr, *The Crisis of European Sciences and Transcendental Phenomenology* (Evanston, Ill.: Northwestern University Press, 1970; hereafter cited as *Crisis*).

consequence of the separation of science and philosophy, i.e., the naïve self-sufficiency of science and its inability to examine its own presuppositions. But Husserl was convinced that this tradition was not radical enough in its attempt to clarify the foundations of science, and that it would not be able to resolve the "crisis" of modern science, its divorce from basic and meaningful questions of human life.[4]

There is an urgent need, then, for philosophical clarification of the human activities in which cosmologies originate—including that apparently privileged and indubitable cosmology that is associated in the popular mind with modern physical science. Like its mythological and theological predecessors, this scientific variant of cosmology is self-sufficient, i.e., it claims to provide a satisfactory account of its own foundations. It thus evidently believes that it has already answered the need stated by Burtt, if indeed it is at all inclined to admit the legitimacy of that need. But it is precisely this naïve, metaphysically motivated self-sufficiency that cannot stand up to critical philosophical examination, any more than could its mythological and theological predecessors. No critical theory of science can accept the assumption that the scientific cosmology has found the Archimedean point from which both an understanding of the universe *and* an understanding of this understanding can be reached in one single move. And, in contrast to the nonreflexiveness of the mythological and theological cosmologies, the modern, self-sufficient scientific cosmology has not even succeeded in providing plausible answers to the human quest for a subjectively meaningful location of the self in the universe.[5]

4. "In this perspective empiricism seems to contain a tendency to the scientific discovery of the life-world which is familiar to everyday experience and yet unknown to science. . . . Here it must suffice to note that the achievements of the mathematical sciences, and more generally, of all sciences committed to a physicalist approach necessarily became problematic to empiricism as it directed its attention to the concretely experienced, historically relevant world which is the topic (human society, culture) of scientific investigation in certain scholarly traditions in the humanities. The demand for a clarification of the theoretical activities which produce the constructs of science, a clarification in the context of and in relation to the life-world, was evidently present in this situation. Such a clarification was to establish the meaning and the 'scope' of science" (*Crisis* [German ed.], p. 449; my translation).

5. In an appraisal of Husserl's view of science, Herman Lübbe puts it succinctly: "In its modern form European science for the first time no longer fulfills its old function to provide man with a reasonable grasp

We are, of course, certain of the productivity of scientific methods, and we cannot doubt that useful knowledge has accumulated in various particular sciences. And yet we remain uncertain of the basis of such knowledge, and doubts linger on as to the extent to which scientific methods are capable of reaching the realities of human life. To put it simply, each science taken singly is impressive; all sciences put together make for a sorry cosmology. It is no solution at all to the problem to declare cosmological questions meaningless and to reduce science to the status of a cognitive technology. This merely leaves the field to the irrational ideologies of scientism and antiscientism. It is dangerous to try to suppress the need for a critical clarification of the foundations of science and the activities in which sciences and cosmologies originate. The philosophy of science has wider obligations than those to which, in recent times, it thought it could limit itself as a result of the technical specialization of the sciences and the academic compartmentalization of philosophy.

This is not to denigrate the usefulness of the contributions that have issued from recent philosophy of science within its self-imposed "professional" limits. The reconstructed logic of science, as Abraham Kaplan aptly calls it,[6] tends to an idealization of the complex structure of scientific knowledge. But reconstructions may effectively influence constructions: it is the quality of the reconstruction, not the fact that it is reconstructed, that is important. If the reconstructed logic stands in a sensible relation to what Kaplan calls the logic-in-use, and to what Norwood R. Hanson describes as the logic of theory-finding as opposed to the logic of theory-using,[7] it can help to reveal the syntax by which statements in the sciences are constructed and aid in the clarification of the canons by which the validity of conclusions is evaluated. Having overcome its early inclination to ignore the processes of concrete scientific inquiry, the philosophy of science may contribute to the formulation of rigorous methodologies for science. It may help in the description of the theoretical activities that constitute science as currently carried on. But these contributions form only one part of its legitimate task.

and reasoned consciousness of his existence in the whole of being. To the contrary, it successively dissolved this consciousness. It thereby released man to metaphysical disorientation. In a manner of speaking it threw the individual subject back upon himself and his isolated certainty of himself" ("Husserl und die europäische Krise," *Kant-Studien*, XLIX [1957–58], 228; my translation).

6. Abraham Kaplan, *The Conduct of Inquiry* (San Francisco, Calif.: Chandler, 1964).

7. Norwood Russell Hanson, *Patterns of Discovery* (Cambridge: At the University Press, 1958).

The philosophy of science must not stop short of an investigation of those activities that are the basis for theory of any kind, including scientific theory. These are the activities that it is nowadays fashionable to call *praxis*, referring to the full range of human conscious activities in the intersubjective and historical world of everyday life. Only a clarification of this universal basis of theory can hope to show the significance of scientific knowledge for human life and establish its legitimate place among other forms of knowledge, theoretical or pretheoretical. Karl Popper describes clearly and simply the common theoretical interest of science and philosophy:

> There is at least one problem in which all thinking men are interested. It is the problem of cosmology: *the problem of understanding the world—including ourselves, and our knowledge, as part of the world.* All science is cosmology, I believe, and for me the interest of philosophy as well as of science lies solely in the contribution which they have made to it.[8]

The task of the philosophy of science goes far beyond the immediate concerns of the methodologies of the physical sciences. It consists in giving a convincing account of the relation of science to theoretical activities in general, and of the relation of these activities to common sense and everyday life. It simply will not do to refer this task to an empirical discipline—such as the psychology of cognition, or even the sociology of knowledge—for *final* adjudication. Such a procedure leads to an obviously vicious circle.

An attempt to avoid that circle involves the search for a controlled, independent perspective on both science and common sense. The question about the foundations of science, its place among other forms of knowledge, and their common origin in everyday life is, first of all, a question about an appropriate method of answering questions of that kind. Clearly, the method must be at least as reasonable and subject to control as the methods of scientific reasoning—but it must also be reflexive and give an account of its own presuppositions. Much depends

8. Karl R. Popper, *The Logic of Scientific Discovery* (New York: Basic Books, 1959), p. 15 (italics mine).

on the initial view of the problem. One can easily relapse into the circularity of scientific rationalism instead of retaining the controlled reflexivity of a critical theory of knowledge. This danger, I think, is what Burtt had in mind in juxtaposing a philosophy of the mind to a cosmology. And it is at this point that Popper decides to remain within the universe of scientific discourse. This strategic decision is foreshadowed in a highly personal view of a great philosophical tradition:

> Its most important representatives during the last two hundred years were Kant, Whewell, Mill, Peirce, Duhem, Poincaré, Meyerson, Russell, and—at least in some of his phases—Whitehead. Most of those who belong to this group would agree that scientific knowledge is the result of the growth of common-sense knowledge. *But all of them discovered that scientific knowledge can be more easily studied than common-sense knowledge* [italics mine]. For it is *common-sense knowledge writ large* [italics Popper's], as it were.[9]

This view seems rather one-sided in the case of Peirce and Whitehead, and perhaps also of the Russell of *Human Knowledge*.[10] It definitely does not do full justice to Kant. One may agree that scientific knowledge can be more easily studied than common-sense knowledge. But that, at best, justifies a beginning, not an end, and it does not resolve the question of method. By no means does it justify what is presented as a conclusion: that scientific knowledge is a (magnified) replica of common-sense knowledge. Popper, I am sure, was induced to make this formulation by his understandable polemic against a conception of science that would sever it from any sensible connection with everyday knowledge. His account of "corroboration" alone is sufficient proof that he does not assume a relation of simple isomorphy between science and common sense. Unfortunately, a perfectly legitimate decision to concentrate on an analysis of the structure of scientific knowledge is dressed up, by virtue of this unjustified claim, as a general theory of knowledge.

Husserl also made what appears in retrospect as a strategic

9. *Ibid.*, p. 22.

10. Bertrand Russell, *Human Knowledge* (New York: Simon and Schuster, 1948), a work in which Russell devotes considerable space to a discussion and to some descriptive analysis of common-sense knowledge. He does show certain connections and similarities between science and common sense, but his main aim is to point out the differences.

decision: not to accept the claim of modern science to be the ultimate form of human knowledge. In three of his major works, one early and two belonging to a later phase of his thought, Husserl investigated the foundations of formal logic.[11] By the method, first, of phenomenological psychology and later of transcendental phenomenology, he traced the origins of logical and mathematical thinking to the activities of consciousness in what he came to call the *Lebenswelt*, that is, the pretheoretical and theoretical levels of the world of everyday life. But it is his last work, the *Crisis*, which initiated a new phase of philosophical reflection on science. In it he effectively demolished the "ultimacy" pretensions of the modern scientific cosmology. For superficial if not silly reasons, and often from sheer ignorance, this has been taken as an "attack" on science, whatever that may mean.[12] It should not be necessary to stress that Husserl does not question the validity of science—as far as it goes. The question remains, of course, how far *does* it go?[13]

It is significant that in answering this question Husserl im-

11. *Logische Untersuchungen* (1900; Tübingen: Niemeyer, 1968); English translation by J. N. Findlay, *Logical Investigations* (New York: Humanities, 1970); *Formale und transzendentale Logik* (Halle: Niemeyer, 1929); English translation by Dorion Cairns, *Formal and Transcendental Logic* (The Hague: Nijhoff, 1970); *Erfahrung und Urteil* (1938; 2d ed., Hamburg: Claassen and Goverts, 1948); English translation by Spencer Churchill and Karl Ameriks, *Experience and Judgment* (Evanston, Ill.: Northwestern University Press, 1973).

12. Some years ago Maurice Natanson warned against the misappropriations and misrepresentations of phenomenology: "In particular, the legend of the *Lebenswelt* lends itself or has been made to lend itself to a critique of rationalism and natural science which is, I think, not only mistaken but inimical to phenomenology" ("The *Lebenswelt*," *Review of Existential Psychology and Psychiatry*, IV [Spring, 1964], 134). Things have taken a turn for the worse in the meantime. Having first provided general absolution for mild "humanistic" confusions, misconceptions of phenomenology later gave rise to some academic subcultures whose resemblance to phenomenological analysis is purely incidental.

13. Aron Gurwitsch puts it concisely: "*It is the historical significance of Husserl's Galileo analysis to challenge and even to abandon the acceptance of science as an ultimate fact and rather to see in it a problem.* Husserl is far from questioning the technical, or, more precisely, the intrinsic validity of science, and nothing could have been further from his mind than dismissing it in any sense. What is in question is not science itself, nor any particular scientific theory, but the interpretation of science" ("Comments" on Herbert Marcuse, "On Science and Phenomenology," *Boston Studies in the Philosophy of Science*, Vol. II: *In Honor of Philipp Frank*, ed. Robert S. Cohen and Marx M. Wartofsky [New York: Humanities Press, 1965], p. 294; italics original).

mediately confronts the problem of method. In this respect, at least, his importance equals that of Descartes and Kant. If Husserl initiated a new phase in philosophy and in the philosophy of science, it is as much by having developed a method for determining the validity of science as by having destroyed the "ultimacy" pretensions of the scientific cosmology. The elementary phenomenological epochē, the various reductions, eidetic variation, and so on, combine in accounting or being able in principle to account for the presuppositions of phenomenological psychology and transcendental phenomenology—which, in turn, have clarified or are capable of clarifying the constitutive structures of conscious activities upon which everyday life as well as science is founded.[14] In short, Husserl developed a method of philosophical analysis that is rationally controlled (and in this sense "scientific") *and* reflexive. The method permits an approach to the question "How far *does* science go?" that does not end in a vicious circle.

It is perhaps more obvious to this generation than it was to its predecessors that this question goes far beyond the boundaries of a single academic discipline. It is a fundamental philosophical question for modern man. In an age when science—which, after all, *is* a human activity—is either deified or satanized, it is also an eminently political question. Far from being destructive of science, Husserl's demolition of the "ultimacy" claim of the scientific cosmology and his attempt to establish firm foundations for science provide a sound basis for determining its human significance. One may hope, on a smaller scale, that his efforts will also help to overcome the curious arrangement whereby in a prototypical philosophy department a faithful Carnapian "covers" science while an existentialist "takes care" of "subjective meaning" and a neo-quasi-neo-Marxist

14. I know of no resumé of the methods of phenomenological analysis that could adequately substitute for the study of Husserl's work. Dorion Cairns's systematic presentation of Husserl, which served as a source of instruction to a generation of students, has not yet found its way into print. Discussions of Husserl in the work of Alfred Schutz, Aron Gurwitsch, and other first-generation phenomenologists were rarely expository. Among the numerous brief introductions, one that is particularly clear and useful is Maurice Natanson's "Phenomenology: A Viewing," in his *Literature, Philosophy, and the Social Sciences* (The Hague: Nijhoff, 1962); see also Herbert Spiegelberg, "The Essentials of Phenomenological Method," in *The Phenomenological Movement: A Historical Introduction,* 2d ed. (The Hague: Nijhoff, 1965), II, 655–701.

proclaims the political and social irrelevance of what his colleagues are doing.

The Cosmological Paradigm in Social Science

Both the general problem, i.e., the cosmological validity of science, and the question about the proper way to approach this problem are particularly urgent when "cosmology" is taken to refer not only to a systematic explication of the physical universe, but also to a theoretical account of the social world. The reasons why the problems of validity and method should be especially crucial when the reality of human activities is involved are obvious, at least in a general way. They have been taken up under different headings in what is by now a voluminous literature.[15] Nevertheless they will bear some discussion from the point of view I am taking here.

To begin with, one should register profound wonder that anyone should ever have wanted to exclude the social world from the theoretical concern of cosmology. The world of man *was* included in mythological and theological cosmologies, as a matter of course *and* as a matter of overwhelming interest, during most of the history of human thought. This changed in Western thinking after Galileo. Since his time, some philosophers and scientists have excluded the reality of human activities from cosmology. It is my contention that they do so for reasons (perhaps I should say motives) that are not subjected to rigorous and critical reflection. Other philosophers and scientists are once more including the social world in the scientific cosmology. I contend that they do so for reasons that are just as naïve as those of the other group. I may be putting things too sweepingly and dramatically, and I shall have to be more cautious presently. One thing that can be maintained with confidence, for the moment, is that the distinction between a "physical universe" and other domains of reality, e.g., a "social world," is derived from the metaphysical presuppositions that went into the making of

15. Two examples of the literature I have in mind here are the hundreds of books and, probably, thousands of articles on historicism and on the so-called value freedom of social science. For a good overview of the latter, see Hans Albert and Ernst Topitsch, eds., *Werturteilsfreiheit* (Darmstadt: Wissenschaftliche Buchgesellschaft, 1971).

the modern scientific cosmology. This distinction must therefore not be exempt from critical examination. The division of reality into a physical world and a world of human affairs occurred as part of a global process of "rationalization." It was a milestone in the depersonalization of mythological views of the world, a process described by Max Weber as *Entzauberung*.

This division of reality stands at the end of concatenated theological, philosophical, and scientific traditions, and at the beginning of a historical movement of thought that led to the situation characterized by Husserl as the "crisis" of modern science. The motives for this division had their source in the theological cosmology of the Middle Ages. To say that man attains a divine level of knowledge by virtue of mathematics was not merely a *façon de parler* for Galileo. Nor did his opponents underestimate the possibilities inherent in his view. In a universe whose reality is guaranteed by its mathematical structure, man as well as God becomes problematic in a way that was entirely alien to the older cosmologies, however much they may have differed among themselves in other respects. In the new vision of reality man requires ad hoc explanations just as much as God.

The ontological and epistemological sources (to the extent that it is possible to isolate these more technically philosophical components from the general cultural background) that fed into the stream of the new cosmology can be traced to shifting patterns of influence of opposing philosophical traditions; in Galileo, Platonism won over Aristotelianism.[16] However, reference to the complex origins of these perspectives tells only part of the story; the other part consists of the unique blending of these perspectives into a new vision of the universe. This new vision became clear with Copernicus, who postulated the mathematical structure of the universe. Against the opposition of traditional empiricist philosophy, this postulate was transformed into an article of belief which is taken for granted in the modern cosmology. It did not become entirely immune from critical examination, but philosophical questioning generally proceeded along the innocuous line that the postulate should, perhaps, not be given an ontological but "merely" an epistemological interpretation.

The cosmological plausibility of this view was decisively

16. Alexandre Koyré, "Galileo and Plato," in *Roots of Scientific Thought*, ed. Philip Wiener and Aaron Noland (New York: Basic Books, 1957), pp. 147–75.

strengthened when it was combined with Galileo's doctrine of primary and secondary qualities, and still later, with Newtonian mechanics. Galileo's idea of primary and secondary qualities is expressed in his famous statement that "Whoever wants to read a book, must know the language in which that book is written. Nature is a book and the characters in which it is written are triangles, circles and squares."[17] It can be paraphrased less elegantly as follows: Only the primary qualities can be expressed exactly, i.e., mathematically; the real, objective, absolute reality consists of primary qualities; only what can be expressed mathematically is truly, objectively, absolutely real. This is not much of a syllogism and it probably was never formulated in quite that manner. It is an example of what C. S. Peirce analyzed as abductive reasoning.[18] According to Peirce, abduction is a form of reasoning which guides many common-sense procedures and is also an important strategy of theory-finding in science. In this particular instance abductive reasoning on a grand scale seems to have been involved in the historical construction of a cosmology.

The Copernican postulate of the mathematical structure of reality, the Galilean doctrine of primary and secondary qualities, and the notions of causality generally associated with Newtonian mechanics (Newton's own strong strain of prudent empiricism notwithstanding) combined to produce a new view of reality whose persuasiveness was probably based as much on its extraordinary aesthetic appeal as on its continuous popular verification in the successes of applied science and technology. As is the nature of systematically articulated views of the world, the cosmology was expansionistic within science and imperialistic outside of it. New areas that were to be investigated scientifically were subordinated to the new cosmology.[19] At the same time, the

17. Quoted in Gurwitsch, "Comments," p. 300 (see Galileo Galilei, *Il Saggiatore* [Florence: Edizione Nazionale, 1965], VI, 232).

18. C. S. Peirce used the terms "hypothesis," "abduction," and "retroduction" to describe the process of abductive reasoning. See his *Collected Papers*, 8 Vols., Vols. I–VI, ed. Charles Hartshorne and Paul Weiss (Cambridge, Mass.: Harvard University Press, 1931–35), especially II, 372–88; V, 112–31; and VI, 311–32. For abduction as an interactional phenomenon, see Richard Grathoff, *The Structure of Social Inconsistencies* (The Hague: Nijhoff, 1970), especially pp. 40–75.

19. "The conviction that sooner or later all science is mechanics dies hard: for three centuries science has been dominated by notions of inertia,

cosmology was not restricted to formulations of "laws" and "hypotheses" about restricted dimensions of the universe. The notions of domains of reality [20] and levels of explanation [21] belong to a later period in which philosophers and scientists had already begun to confront the "crisis" of the scientific cosmology. But as long as the arrogant doctrine of primary and secondary qualities prevailed, the ontologically and epistemologically modest conception of domains or levels had no chance to emerge.

In contradistinction to older cosmologies, the expansionism of the new scientific cosmology was checked, at first, by the problem of fitting man into its scheme. The conception of man was more resistant to change than was the conception of the universe. As long as man was cut from the pattern of the old cosmology, he could not be fitted into the tight clothes of the new. Descartes did not hesitate to deliver the human body (and the poor beasties) to the icy winds blowing in the new universe. But not, alas, the human soul.[22]

For some time the cosmological situation was thus characterized by uncertainty, inconsistency, and incipient implausibility. What was traditionally, and plausibly, connected as part of one comprehensive reality was split asunder. Radically different styles of thought which were completely incompatible with one another were prescribed for the knowledge of man (and God) and for the knowledge of the rest of the universe. This split

impact and resultant velocities. This has affected our understanding of causation" (Hanson, *Patterns of Discovery*, p. 65).

20. Making use of suggestions from various sources, especially from William James, George Herbert Mead, and Husserl, Alfred Schutz developed a theory of "domains" as "finite provinces of meaning," i.e., based on a characteristic experiential and cognitive style. See his "On Multiple Realities," in *Collected Papers*, Vol. I: *The Problem of Social Reality*, ed. Maurice Natanson (The Hague: Nijhoff, 1962), pp. 207–59.

21. The notion of levels of explanation was, of course, one of the central issues in the long debate on "reductionism" in science, especially after its revival in Neurath's program of "unified science." I may refer here to two sensible discussions in the *Symposium on Sociological Theory*, ed. Llewellyn Gross (Evanston, Ill., and White Plains, N.Y.: Row, Peterson, 1959): Abraham Edel, "The Concept of Levels in Social Theory," pp. 167–95, and Herbert Hochberg, "Axiomatic Systems, Formalization and Scientific Theory," pp. 407–36.

22. A very special act is required to account for the human soul: "I had described after this the rational soul and shown that it could not be in any way derived from the power of matter, like the other things of which I had spoken, but that it must be expressly created" (René Descartes, *Discourse on Method*, trans. Elizabeth S. Haldane and G. R. T. Ross [Cambridge: At the University Press, 1931], I, 117–18).

had been prepared for a long time. Judaism, a major step in the *Entzauberung* of the mythological universe, had already singled out man, the member of the elect tribe, as standing in a special compact with God.[23] But the Thomistic reconciliation of Christianity with Greek philosophy successfully refitted man into the over-all scheme of creation. Meanwhile, beneath the official doctrines the world view of the folk tradition had always retained the essential structure of the old mythologies. The separation of man from the universe in the newly emerging cosmology, on the other hand, gave rise to a metaphysical disorientation that within a century or two had repercussions in the culture of all social classes.

In order to cope with this cosmological inconsistency, attempts were begun to justify the expulsion of Adam from the new cosmology or to recast the conception of man so that he would fit into it smoothly. The attempts carry the trade-mark of abductive reasoning; they are neither inferences nor deductions but leaps from familiar premises to an unknown order of things that is made comprehensible by assimilation to the premises. Such movements are not entirely unlike leaps of faith. It is therefore not surprising that two contradictory ways of resolving this cosmological inconsistency emerged in the course of time.

One solution runs as follows (if I may be allowed to retrace the abductive steps in a less than complimentary fashion): Man is part of nature; nature consists of primary qualities; man is reducible to primary qualities; there is no world of human affairs for which scientific understanding is not possible, in principle, through a reduction to the mathematical manifold of space and time (here the Newtonian conception of causality is occasionally thrown in for good measure). The leap to the other solution takes off from the other foot: Man is not reducible to primary qualities; the special origin of his soul—or the uniqueness of his experience—cannot be mathematized; man is not part of reality in the sense in which nature is real (this is one of the sources of the Cartesian difficulty); therefore there can be no science of man (except of course under the wholly subordinate aspect of man as a "machine").

These "conclusions," still generally unexamined, are at the

23. See Max Weber, *Ancient Judaism*, trans. and ed. H. Gerth and D. Martindale (Glencoe, Ill.: Free Press, 1952).

heart of the methodological controversies of social science. It hardly needs to be added that these controversies are the social science variant of the general crisis of modern science. I shall not pursue directly the wider topic of to what extent phenomenology, by clarifying the origins of theoretical activities in the world of everyday life, provides a philosophical foundation for all science. I shall turn instead to look closely at one important part of the wider question, the elementary problem of social science methodology. I hope I have already shown the historical relativity—which does not necessarily mean falseness—of the ideas that prevented the emergence of a social science which did not try to follow the precepts of the new cosmology. I am not afraid of overstating my case by saying that Copernicus, Galileo, and Newton are *malgré eux* the main figures, not of social science, but of the methodology of modern social science. Their new cosmology severed social science from its ancestry.

The rapid growth of social science in the past quarter-century, the specialization of disciplines and the increasing technical sophistication of their methods, the attempts at extending paradigms across disciplinary boundaries and at constructing a unified theory, as well as the urgency of the social problems that can only be resolved—or so it is commonly thought—if social science reaches the "maturity" of, and collaborates with, physical science and technology: all this may explain, and, perhaps, partly justify, the loss of memory that is widespread among contemporary social scientists. The long tradition of systematic reflection on human conduct in society is no longer alive. The historian will no doubt trace some contemporary theoretical paradigm by way of Marx and Hegel to a Gnostic source. In a playful mood, he may point to a similarity between some idea of Veblen's and a half-forgotten observation of Saint-Simon. A political scientist may appeal to hallowed ancestors in Aristotle, Pomponazzi, or Machiavelli. A linguist may make his reverence before Panini, a sociologist may bow to Montesquieu. Occasionally, someone may even want to shake his fist at what he takes to be a dangerous survival of, for example, Platonism. But these ritual invocations are usually mere frills and ornaments in scholarship, whose live, subjectively grasped intellectual sources are much more recent. The works of earlier centuries have less influence on contemporary social science than the discovery of pre-Socratic beginnings of non-Euclidian geometry has on a land

surveyor in Alaska. To the average social scientist the idea of a specialized history of social science appears stranger than did the need for a professional history of physical science to the ordinary physicist of pre-Sartonian days; he sees himself as the practitioner of a science that is in its infancy. Social science as we know it today (and as it knows itself) is a child of Modernity writ large. It traces its descent into the nineteenth, perhaps as far back as the late eighteenth century. Beyond that begins the prehistory of social lore and isolated philosophical speculation. And still further back are the Dark Ages of utter ignorance concerning human conduct.

I do not intend to examine here all the reasons for this peculiar partial amnesia. Nor do I want to plead for the inclusion of Thucydides in the training programs of social scientists. Appeals do not revive dead traditions. The contemporary historian of ideas and the future historian of social science need not be told their business, and the thoughtful social scientist knows more or less clearly that he is carried on "the shoulders of a giant." My reminder here has a different purpose. I want to stress an important consequence of the modernity-image that prevails in social science today. In sharp contrast to physical scientists, social scientists find that they cannot look backward to an autonomous tradition of philosophical reflection on their enterprise. The most promising candidates to membership in such a tradition, Vico, for example, are sweepingly disqualified as mere speculative "metaphysicians." The philosophy of science until very recently was a philosophy of physical science. The methodology of *social* science is a child of Galilean *physical* science.[24] In this perspective it makes little difference whether the child is abjectly docile or wildly rebellious, or, to change the metaphor, whether man is thrown out of the modern scientific cosmology or locked into it.

24. I am using the term "Galilean science" in the sense employed by Husserl in *Crisis*. It corresponds approximately to what I have referred to as the new cosmology or the Copernican-Galilean-Newtonian view of the world. Gurwitsch puts it as follows: "Husserl, when he speaks of Galileo, does not mean the historical figure of that name who lived at a certain time, any more than by Galilean science he means the scientific work actually done by that historical figure. Rather Galilean science denotes the science inaugurated by Galileo. The name is used as a symbol for the historical development of modern science from, roughly speaking, 1600 to 1700, that is, the constitution of classical physics and even beyond" ("Comments," p. 292).

The "Crisis" of Social Science

Most social scientists and almost all those philosophers who like to instruct social scientists on their business persist to this day in allowing the Copernican-Galilean-Newtonian view of the world to impose upon them the basic perspective in which they look on themselves and on the goals of their theoretical activities. They continue to take for granted the assumptions that the universe is deceptive yet fully knowable; that the appearances given to prescientific man, an inferior "subjective" species easily befuddled by secondary qualities, hide a structure of "objective" primary qualities; and that discovery of this ultimate reality depends on the supreme and autonomous form of knowledge, (numerical) mathematics. Despite Hume and others, they cling to an apparently ineradicable push-and-pull notion of causality to explain how it all hangs together.[25] Revolutionary elements of seventeenth-century philosophy and science, abductively transformed into an eighteenth-century cosmology, almost routinely transmitted to the nineteenth century as a paradigm of physical science, thus form the unexamined background of methodological reflection in social science well into the second half of the twentieth century.

The same paradigm, however, gave rise to elementary methodological positions in social science which seem to their adherents to be irreconcilably opposed. It should be recalled that the early phases of the new cosmology were characterized by an elementary inconsistency which stimulated abductive reasoning along two main lines (as summarized above), depending on whether the inconsistency was to be eliminated or legitimated. The fundamental methodological controversy in social science is at best a continuation and at worst a petrification of these efforts.

One side of the controversy makes the "big leap forward." Man is subordinated to the newly found principles. He *cannot* be merely a bundle of secondary qualities, he *has* to be part of nature. And because "nature" is the mathematical manifold

25. For a discussion of this important aspect of the problem, see Mario Lins, *Foundations of Social Determinism*, trans. George Reed (Rio de Janeiro: Livraria Freitas Bastos, 1959).

representing the primary qualities of true reality, the hunt for the primary qualities of human existence is on. The Cartesian reservation on the human soul is given up and an intrinsically consistent man-machine solution replaces it. Through analogy with astronomy and mechanics, a plausible interpretation of anatomical and even physiological findings appears possible. But while the solution appears logical, the application of the logic to the study of human affairs leads to results whose absurdity is not diminished by the fact that they form part of the routine background of our thinking as social scientists. The hope that, by discovery of primary qualities to which measurable values can be attached, social science will finally become "exact" (a word with powerful emotional appeal for Neo-Pythagorean romantics) waxes eternal. No matter how sophisticated the technical discussion of the logic and the logistics of science, the guiding vision of social science on this side of the methodological controversy is of a closed mechanical universe whose objective qualities are numerical.[26]

Failure of the vision has resulted in two varieties of frustration. The inability to determine the primary qualities of man as a social, political, and historical being by transforming him into a walking inventory of instincts or drives, into a *homo œconomicus*, *homo sociologicus*, game-strategist, personality subsystem of an action system, and the like, inspires recurrent movements of cosmological reductionism. If the "soul" will not play by these particular rules, out with it! The man-machine will play by them, and therefore it is invited into the part vacated by the "soul." The man-machine becomes a symbol of hope to those proponents of reductionism who must try to secure unlimited extensions of credit for their program. The other variety of frustration is that of the sensitive souls who, after an early training in some self-consciously "scientific" discipline, develop ideological guilt feelings or aesthetic phobias about the language of cybernetics,

26. "The physical sciences have dominated applied mathematics for so long that many scientists and mathematicians have mistakenly assumed that non-divergence is necessary before prediction is possible. Consequently the physical sciences have been called the 'exact sciences,' while the biological and social sciences have been mistakenly labelled the 'inexact sciences'" (E. C. Zeeman, "The Geometry of Catastrophe" ["Thinking by Numbers," no. 13], *Times Literary Supplement* [December 10, 1971], pp. 1556–57).

systems analysis, simulation, or even old-fashioned structural-functionalism. They are easily converted to the soft belly faction of the other side of the methodological controversy.

The other side of the controversy not only refuses to leap but, one is tempted to say maliciously, hesitates to walk. Most of its adherents, seeing the absurdity of the consequences, deny the applicability of the premises of the new cosmology to man, while at the same time uncritically accepting those premises for the rest of creation. Man therefore has to be removed as far as possible from nature, "nature" being nothing but a measurable space-time manifold. Furthermore, the pushes and pulls of vulgar matter cannot apply to the "historicity" and the "uniqueness" of the human mind (these are the words of emotional appeal on *this* side of the fence). Therefore there can be no social *science*, there can be only artistic and intuitive reconstructions of the unfolding of the mind. These idiographic narratives have to have a logic, a structure, a style different from the man-machine analysis of human affairs. The palpable inapplicability of the new cosmology to human affairs provokes not merely a legitimate rejection of a numerical-mechanistic conception of social science, but an enduring inability to reexamine the problem of formalization and mathematization independently. The adherents of this side of the methodological controversy do not dream of challenging what they take to be the laws of the land. They are fully satisfied with a cosmological Human Exclusion Act. Let us have our hermeneutics, they insist, and you can have your cybernetics!

Neither side of the watershed, of course, possesses a common program. Neurath's claim that "empiricists" and "rationalists" have joined in a movement of "unified science" whose platform could be called "logical empiricism" or "empirical rationalism" was exaggerated when it was originally made in 1938, and the movement has not become any more unified since then.[27] Since the seventeenth century philosophers have presented a wide variety of positions on logic, scientific method, causality, inference, and so on, which followed most of the imaginable combinations of Baconian empiricism and Cartesian rationalism.

27. Otto Neurath, "Unified Science as Encyclopedic Integration," in *International Encyclopedia of Unified Science*, ed. Otto Neurath, Rudolf Carnap, and Charles Morris, Vol. I, no. 1 (1938; 2d ed., Chicago: University of Chicago Press, 1955).

Nevertheless, logical empiricism serves to designate the modern philosophical center of the one side of the watershed. In science, it combines with various philosophically more or less purified versions of "positivism." It is not *necessarily* reductionist, but it often is; it is not *necessarily* behaviorist, but again it often is.

Meanwhile, the sources on the other side of the watershed can be characterized, in a general way, as Neo-Platonistic and idealistic. Among its more extreme adherents the Hegelian heritage is obvious. Roughly speaking, one may consider the historicists, Dilthey, most Neo-Kantian philosophers, e.g. Rickert, idealistic phenomenologists, and the "picture-book" variety of phenomenologists as belonging here. Winch's interpretation of Wittgenstein,[28] as well as so-called "critical sociology" in Germany and some Neo-Marxists (claiming to go back to the "early" Marx of the *Frühschiften*) in other countries have their *methodological* origins on this side of the divide. Seen through Neo-Kantian and idealistic interpretations Max Weber can also be placed here. But these interpretations miss his heroic effort to tunnel through the range to the other side. Despite the inconsistencies in his methodological rhetoric, Weber seems to me to have passed more successfully than any other major social scientist the Scylla and Charybdis of the cosmological paradigm in social science.[29]

In sum: the search for a *mathēsis universalis* of human affairs was—and is—abandoned by both sides. One side stopped looking because it thought it had found it already, and was content to let the concrete problems and the recalcitrant "facts" of the social sciences look out for themselves. The other side never started looking because it was convinced there was nothing to find. Both sides thus contributed to the social science variant of the crisis of modern science.

The crisis is not to be confused with the "reactionary" and "revolutionary" attacks on the limited but, within this limitation, necessary and legitimate autonomy of science. Nor is it simply a matter of romantic impatience with the also limited but legitimate rationality of science. Attacks and cavils of this kind are

28. *The Idea of a Social Science* (New York: Humanities Press, 1965).

29. For an interesting recent critical appreciation of Weber that does not try to make him over into a positivist or phenomenologist, see W. G. Runciman, *A Critique of Max Weber's Philosophy of Social Science* (Cambridge: At the University Press, 1972).

not new, and despite their present vociferousness they are probably less dangerous today than earlier in the century when they were associated with powerful totalitarian political forces. The crisis of social science is not caused by its technological implications, despite the recurrent fear, recently reacting to Skinnerian chimeras, that social science is about to find the key to the total manipulation of the human mind. Nor is it a crisis of substantive theory or of research procedures, if these terms are understood narrowly. The logical and logistic problems of the organization of ideas arising in social science, as in any similarly complex social institution concerned with the acquisition and transmission of knowledge, are so normal that they hardly deserve to be called "crises," though it is true that the ideological, technological, theoretical, and procedural problems connected with social science appear more serious at the present time than ever before.[30] In part this impression may be attributed to the well-known generation effect: every generation views the state of the world with more profound concern than did the previous one. In part, however, these contemporary alarms are genuine symptoms of the "crisis" of social science in the sense in which I am using it, i.e., in strict analogy to Husserl's application of the term to Galilean physical science.

The fundamental function of theory is to suggest meaningful solutions to basic problems of everyday life, to help men in their orientation to the universe. One would hope that science would do this more successfully than its cosmological predecessors. In order to perform this function, however, theory must first give a meaningful account of the concerns of everyday life. Description and explanation are inextricably interwoven. Furthermore, scientific theory is distinguished from the mythological and theological accounts from which historically it emerged (and which vestigially survive in it) [31] by its degree of *explicit* systematization and formalization of knowledge and by its commitment to a teachable and public method for the acquisition of knowledge. The method is to be rational and empirical (both words subject to varying interpretations but each

30. A thoughtful discussion, by a physical scientist, of some of the recent Doomsday prognostications about science can be found in Harvey Brooks, "Can Science Survive in the Modern Age?," *Science*, CLXXIV (October 1, 1971), 21–30.

31. See Ernst Topitsch, "Mythische Modelle in der Erkenntnislehre," *Studium Generale*, XVIII, no. 6 (1965), 400–418.

roughly having a common core-meaning). The empirical acquisition of knowledge, its rational interpretations, and its public transmission are to be controlled by a community of investigators, to borrow an apt Peircean reformulation of the old concept of the republic of scholars. Communication within this community, however, and just as importantly, with the larger community of noninvestigators, is founded in some presumably determinable but inevitably circular manner on the intersubjectivity of ordinary experience. In short: scientific theory is description as well as explanation; communication in science rests on communication in everyday life.

This circularity (science–ordinary experience–science) has an important consequence for social science. The subject matter of social science, the sociohistorical reality of everyday life, is not only a problem in the sense in which "physical nature" as a universe of "objects" is a problem to science, but a problem of epistemological reflexivity as well. According to Husserl, the crisis of science resulted from the alienation of the idealized and formalized products of theoretical activity, of "logic" and "mathematics" reified as structural principles of nature, from their sources in the *Lebenswelt*. I submit that because of the peculiar reflexivity of social science, its estrangement from its sources is a bigger threat to its elementary theoretical function than the illegitimate reifications in physical science are to *its* cosmological purpose. In social science it is not only the products of theoretical activities that are uncritically reified; under the prevailing cosmological paradigm the producers themselves are in constant danger of reification.

It is of course not enough merely to speak of "alienation" and "crisis" in a general way. I hope I have succeeded in a more specific diagnosis by tracing the present symptoms in the methodology of social science to an underlying cause. The cosmological paradigm of Galilean science was imposed, for a number of reasons, on social science and on the philosophy of social science, and kept them in a state of double naïveté. [32] In the first place, the sources of science in the theoretical activities of idealization and mathematization, and the foundation of these

32. In *Crisis* Husserl distinguishes the naïveté of Galilean science and logic from the naïveté of the natural attitude of everyday life. See also *Cartesian Meditations*, trans. Dorion Cairns (The Hague: Nijhoff, 1960), especially pp. 152–53.

activities in the praxis of everyday life, were suppressed. This is a form of blindness which social science shares with physical science. Social science not only very properly took over the logical form of reasoning (the historic achievement of the combination of empiricism with rationalism) from physical science; it also very improperly pretends to the same (illegitimate) *epistemological* autonomy of scientific knowledge (which is its historical *hubris*). The second aspect of naïveté is the exclusive property of social science. It is a blindness concerning the nature of the subject matter of social science. It consists in a metaphysical elimination of reflexivity.

I am reasonably confident that the diagnosis is correct. It would be tempting to stop here, because I must confess that I am much less confident of the cure than of the diagnosis. Nevertheless, having come this far in the identification of the problem, I should like to try to suggest the direction in which I think the solution is to be found. In pointing to it I shall use as signposts certain important suggestions in Husserl's *Crisis* [33]

33. The reader beware! I am not giving a faithful rendering of Husserl's work *in toto*. I think that the extrapolation of the suggestions which I am taking mainly from *Crisis* can be justified. But Husserl probably would not accept these extrapolations as being within the frame of transcendental phenomenology. In *Crisis* there are passages which suggest a train of thought with which my proposal is incompatible, at least on the surface. See, *e.g.*: "For the realm of souls there is in principle no such ontology, no science corresponding to the physicalistic-mathematical ideal, although psychic being is investigatable in transcendental universality, in a fully systematic way, and in principle in essential generality in the form of an *a priori* science" (*Crisis*, p. 265 [italics mine]); "for an objectivity after the fashion of natural science is downright absurd when applied to the soul, to subjectivity, whether as individual subjectivity, individual person, and individual life or as communally historical subjectivity, as social subjectivity in the broadest sense" (p. 337). Much depends on what one means by "after the fashion of natural science." If it refers to Galilean science, *d'accord*. But a "science" a priori, a transcendental eidetic phenomenology is not the only alternative. Cf. Husserl's own statement: "[Corrected] experience—either as communal experience and reciprocal correction or as one's own personal experience and self-correction—does not change the relativity of experience; even as communal experience it is relative, and thus all descriptive assertions are necessarily relative, and all conceivable inferences, deductive or inductive, are relative" (p. 336). An "open," empirical, science whose "objects" are "relative" and "descriptive" statements should be just as legitimate as a science whose "objects" are (numerically) mathematized qualities. For an interesting discussion of ambiguities in Husserl's notion of *Lebenswelt* and correlative

and in the opus of Schutz.[34] Again I shall leave aside the general problem of the crisis of science, to whose solution Husserl devoted his last work, and concentrate instead on the methodological consequences of the second aspect of the naïveté of social science mentioned above.

I have suggested that the crisis of social science, to the extent that it is separable from the general crisis of science, is its inability to come to terms with its peculiar epistemological reflexivity. The underground waters of the Galilean cosmology continue to feed the rivers on both sides of the methodological watershed, the only important difference being that some flow east and the others west. The epistemological reflexivity of the enterprise is either wished away because it is bothersome, or else it frightens the practitioners back into a mythological or theological mode of thought. The result is that neither side has produced a *mathēsis universalis* appropriate to human affairs.

I suggest that a solution to the crisis of social science might be found if the search were taken up again. I maintain that a *mathēsis universalis* of the *social* world has not even been programmatically established in what, roughly speaking, is the positivist tradition. I also maintain, however, that the cosmological goal of a *mathēsis universalis* must include human conduct on all its levels; the Human Exclusion Act is just as unconstitutional in science as it is in philosophy. The unity of science does *not* presuppose a reductionist metaphysics. I am convinced, and I hope I have shown the reasons for my conviction, that the goal of a *mathēsis universalis* for human affairs cannot be reached by simple analogy to the ideal of mathematization in Galilean cosmology. This ideal is too closely tied to a numerical conception of what is "objective" and "empirical." A *mathēsis universalis* appropriate to the social world will have to be truly independent of Galilean cosmology. It will have to be based on the premise of the epistemological reflexivity of a science of human conduct. A science that describes and explains the con-

ambiguities in his view of an (a priori) science of the *Lebenswelt*, see Natanson, "The *Lebenswelt*," especially pp. 130–32.

34. See especially "Common-Sense and Scientific Interpretation of Human Action," *Collected Papers*, I, 3–47; "Concept and Theory Formation in the Social Sciences," *ibid.*, pp. 48–66; and "Phenomenology and the Social Sciences," *ibid.*, pp. 118–39. His work constitutes an essential contribution to the philosophical foundation of social science.

structions of social reality must be able to develop a program of formalization (and a theory of measurement) that is appropriate to the constitutive structures of everyday life.[35]

The diagnosis of an illness is not its cure, nor is the naming of a medicine that is not yet on the market. I am aware of that. Yet I think that these two steps are important, at least for redirecting attention from symptoms to causes.

The existence of a crisis in social science is not generally perceived, unless one considers as instances of such perception the utilitarian dissatisfactions with its lack of predictive efficiency on the part of "practical men of research and business" or the amusing conceit of bourgeois sociologists that bourgeois sociology is doomed. Among social scientists the "crisis" in the sense in which I am using the term is only half seen on one side of the methodological Big Divide, and then denied with almost as much conviction as on the other side. This failure of observation of course does not strengthen my case, but it does not necessarily weaken it. If the diagnosis is correct, both sides have good reason to ignore the existence of a crisis common to both sides. It is more flattering to assume that it is not social science but the opposing side that is in critical condition. The epistemological, theoretical, procedural, and ideological difficulties that undeniably exist in social science can then be attributed to the senility of an established church or to the wrongheadedness of a newly emerging sect.

The controversies among various schools of thought *do* concern genuine issues of substantive theory, investigative technique, and occasionally also of methodology. But because every indication of the critical condition of the patient is attributed to a serious but localized infection (structural-functionalism, *structuralisme*, neopositivism, "critical theory," transformational grammar, componential analysis, symbolic interactionism, statistical historiography, games and simulation theory, econometrics, ethnomethodology, etc.), the scattered symptoms are not recognized as forming part of a syndrome which has a single cause.

35. This, I take it, is the wider aim of Aaron Cicourel's *Method and Measurement in Sociology* (New York: Free Press, 1964). At this stage of methodological discussion his book admittedly and understandably has a programmatic flavor. Cicourel's detailed critique of the unreflected use of (numerical) mathematics in various research procedures is particularly valuable.

I have already indicated that in my view the solution to the crisis of social science lies in the formulation of a *mathēsis universalis* appropriate to human affairs. Because of the illustrious history of the concept of *mathēsis universalis* the statement of the program in the abstract makes it appear even bolder than it is. I shall come back to what I see as its main promise and its most serious difficulties; I hope that taking the program at least one step beyond its present degree of abstractness will help prevent unnecessary misunderstandings. But first it should be recognized that the need for such a program is not widely felt, which is not surprising. Empirical theory and research are only possible on a foundation of things taken for granted. It is impossible to make everything problematic at the same time. In addition, the reluctance of the practitioners to make their methodological naïveté a topic for reflection is not only understandable, it is a secondary indication of the crisis we have been discussing. But the intrascientific difficulties of methodological communication have a parallel in the intraphilosophical difficulties of communication *about* social science (and social reality)—not to speak of the traditional difficulty of communication between philosophers and social scientists. There are several reasons why the crisis of social science is not perceived in various philosophical traditions; and I expect that these traditions will find the program of a *mathēsis universalis* for social reality either absurd or superfluous for highly divergent reasons.

I would not be surprised if logical empiricists and positivists of various persuasions should forget their differences and agree on the absurdity of the goal I have advanced. Is not the mathematization of nature including, of course, social reality already achieved in principle? Why should there be an intrinsic relation between the algorithms of theoretical operations, i.e., the generalized syntax of science, and the interpretations of the algorithms with respect to particular domains, i.e., the semantics of specialized sciences? The Galilean paradigm suggests obvious answers to these questions. The need for taking into account the epistemological reflexivity of social science will be denied. As for the implied question about the "conditions of the possibility" (to apply the Kantian expression) of social science, that is an impermissibly idealist and transcendentalist query. The history, psychology, and sociology of science including, of course, social science, and of philosophy including, of course, the philosophy

of science, will provide an empirical account of the conditions under which science and philosophy originate and operate.[36] That must suffice. And as for the sense and import of social science, that is a matter of value judgments, political decisions about social uses of knowledge, and so on, which science can treat as empirical facts and analyze for their consequences. That, again, must suffice. Need I say that the chief weakness of this line of reasoning lies in its faith in the Münchhausen trick of pulling oneself out of a swamp by one's own pigtail. Respect for rationalism and empiricism as essential elements in the historical "rationalization" of cosmologies is one thing. It is another thing to accept as an article of faith the "ultimacy" claim of science and resign oneself to the viciously circular theory of knowledge that claim entails.

Transcendental phenomenologists, on the other hand, will find the program superfluous. They will maintain that Husserl's conception of the universal structures of the *Lebenswelt* is only meaningful in connection with his idea of transcendental phenomenology as a rigorous a priori science. What can social science add to this? Physical science can be presumably left to its naïve theoretical *technē*. But any program for social science other than one that makes it part of a transcendental eidetic enterprise is a relapse into a naïve naturalism.

I have already stated some of the reasons for my reluctance to adopt a position which postulates a supreme "discipline of all disciplines" and anticipates an ultimate "fulfillment" of science and philosophy. It is one thing to consider the crucial question of the transcendental conditions of knowledge and to adopt a rigorous method of philosophical reflection grounded in immediate experience. It is another thing entirely to abandon the unity of science in its cosmological sense and basic logical structure for the sake of an illusory quest for absolute and total certainty of knowledge. To do this, it seems to me, is to abandon the idea of a descriptive-phenomenological foundation of cumulative empirical sciences, confusing it with that of a perennial "first philosophy." [37]

There is another group of academic philosophers who see neither the crisis of social science nor, of course, the need for

36. See Richard Bevan Braithwaite, *Scientific Explanation* (1953; 3d ed., Cambridge: At the University Press, 1956), pp. 20 f.
37. See Natanson, "The *Lebenswelt*," pp. 133 f.

programs to overcome it. The reason here is simple: they do not see social science. A formulation of MacIntyre's is so apt and amusing that I may borrow it despite the difference in context: "The second major head on which the symposiasts seem to agree is their refusal to allow that the natural sciences have as yet happened. . . ." [38]

Universal and Historical Structures of Everyday Life

The program of a *mathēsis universalis* for social reality is stated provocatively—on purpose. It should be made clear at the beginning, however, that the aims of the program are weaker as well as stronger than the two terms *mathēsis universalis* may suggest separately and jointly. The aims of the proposal, stated one way, are to institute a search for possibilities of formalization that are genuinely independent of the Galilean cosmological paradigm. Stated in another way, the aims are to generate some principles for the construction of a metalanguage into which the observational languages of the various social sciences could be translated with a controlled decrease of historical specificity and without loss of the intrinsic significance of observational statements. "Formalization" and "metalanguage" describe the goals of the program but they do not specify them exactly; I am using the terms in a sense that is not identical with their common employment in technical discourse. Before clarifying my use of these terms, however, I want to anticipate an even likelier misunderstanding by stating explicitly that the proposal does not imply a revival of notions concerning a separate "logic" of social science.

Kant's critique of the "transcendent" use of concepts provided a transcendental foundation of knowledge in the activities of human consciousness. In its historical context, however, Kant's critical theory of knowledge can be seen as an attempt to purify the epistemology of physical science of the powerful traces of Galilean cosmology. After Kant—and Hume, who woke Kant from his "dogmatic slumber"—the merger of rationalism and empiricism from which was fashioned the hard core of

38. Wolfe Mays and S. C. Brown, eds., *Linguistic Analysis and Phenomenology* (London and Basingstoke: Macmillan, 1972), p. 43.

modern science was obliged to meet higher standards of epistemological sophistication. The influence of Kant on Peirce may serve as an illustration of this point.

The attempts to provide a philosophically adequate foundation for social science, however, generally still took a Galilean view of physical science, and that well into the first half of the twentieth century. It was this view that was either adopted uncritically or rejected completely. Up to the generation of Rickert, Dilthey, and Weber—and in some quarters for a generation beyond them—the conviction prevailed on both slopes of the methodological Big Divide that in social science one had to opt for the Galilean model or against it. *Tertium non datur!* Those who rightly decided that the model offered neither theoretical nor technical and procedural solutions to the problems they were facing therefore searched for a special form of logic which would permit generalized interpretations of unique, value-oriented human actions and the equally unique products of human action as, for example, art. This search led into one of the major dead-end streets in the philosophy of social science. As far as I can judge, no plausible argument was advanced that justified a radical distinction between a *general* logic of explanation in social science on the one hand and in physical science on the other.[39] That such arguments were tenaciously believed may be

39. The question of a special logical form of social science was recently revived by Karl-Otto Apel (see especially "Szientistik, Hermeneutik, Ideologiekritik," *Man and World*, I [1968], 37–63; and "Wittgenstein und das Problem des hermeneutischen Verstehens," in *Zeitschrift für Theologie und Kirche*, LXIII, no. 1 [1966], 49–87) and Jürgen Habermas (see his two major studies on this problem, "Zur Logik der Sozialwissenschaften," in *Philosophische Rundschau*, special publication 6 [Tübingen: 1967], and *Knowledge and Human Interests*, trans. Jeremy J. Shapiro [Boston: Beacon Press, 1972]; and, among his articles, especially "Der Universalitätsanspruch der Hermeneutik," *Hermeneutik und Dialektik*, ed. Rüdiger Bubner, Konrad Cramer, and Reiner Wiehl [Tübingen: Mohr (Siebeck), 1970], pp. 73–103, in which he defines his agreements and disagreements with the central figure of hermeneutic philosophy, H. G. Gadamer). Apel and Habermas offer a polemical interpretation of physical science and of what they consider its official philosophy, positivism. They subject the foundations of the Galilean cosmology to legitimate criticism but present their discussion, at least by implication, as a general analysis of physical science and its philosophical foundations. No doubt they are right in pointing out the serious shortcomings of "positivist" methodology in social science. To a global view of a reductionist physical science, based on a manipulative and operationalist theory of measurement, they oppose, in the end, a discipline of hermeneutic interpretation of cultural configurations of meaning. Their perspective is not entirely dissimilar to that of

partly attributable to the fact that the refutations of such proposals were generally of a rather simple-minded Galilean variety and sometimes absurdly reductionistic.

But with Dilthey and even more so with Weber the first signs announcing a shift in orientation appear. The question of a special logical form of the social and historical sciences receded into the background. Weber himself clearly recognized it as a pseudo-problem and in his rebuttal of Stammler's "refutation of materialistic history" insisted on the generality of the "logic" of explanation in science.[40] Slowly there emerged another question, in various formulations, in which words as heterogeneous as "meaning," "intention," "purpose," "motivational nexus," "rationality of goals and ends," "semiotic context," "sign-oriented behavior," "norms," and "roles" appeared as key terms. With Dilthey's program of a general descriptive historical psy-

Wittgenstein as described by Winch. Habermas sometimes comes close to the notion of a protosociology (analogous to the protophysics of Lorenzen) but shies away from it, mainly, I think, because he finds it difficult to conceive of a theory of measurement appropriate to sociohistorical reality. Both Apel and Habermas take the methodological leap into language as the transcendental "condition of possibility" of intersubjectivity, of knowledge, and of science, and, at the same time, as the ultimate *explicans* and *explicandum* of social and historical reality. This leap is motivated in part by their serious concern with what I have called the epistemological reflexivity of social science: communication itself shows a "first-order" reflexivity. I agree that the "professional" philosophy of science cannot serve as a substitute for a critical theory of knowledge. But I do not find their solutions plausible. Habermas, for example, suggests at one point a linguistic-psychoanalytic "metapsychology" (*Knowledge and Human Interests*), and at another point a "theory of communicative competence" as the frame of hermeneutic interpretation ("Der Universalitätsanspruch der Hermeneutik"). These suggestions have recently been taken up elsewhere (see, for example, Johannes Fabian, "Language, History and Anthropology," *Journal for the Philosophy of the Social Sciences*, I [1971], 19–47). I cannot agree that the epistemological reflexivity of social science entails a radical dichotomy between social and physical science—let alone a dichotomy based on differences in "logical form." The insistence of Apel and Habermas on the radical difference between the two kinds of science has other than purely methodological motives. It derives from a number of premises on the nature of "nature" as against "history." It is also connected with the curious assumption that physical science is founded on a technological, essentially capitalist praxis, whereas "critical" social science is to become a historical praxis of emancipation.

40. Max Weber, *Gesammelte Aufsätze zur Wissenschaftslehre* (Tübingen: Mohr [Siebeck], 1922), pp. 291–359. Incidentally, this essay should be reread by all those social scientists who have been hypnotized by the recent upsurge of discussion in English and American philosophy on the meaning of "following a rule."

chology and Weber's systematic combination of the "interpretation" of human action with "causal" explanation, the new question began to replace the search for special logical forms as a central issue in philosophical reflection on social science. The question is how the "field" of social science is to be identified and how the constituent elements in the "field" are to be recognized and defined. Kaplan puts it quite simply: "Behavioral science is occupied with what people do, but the 'what' is subject to two very different kinds of specification." [41] It is a mistake to bypass answering the question by pointing to some higher rationality presumably at work in forming the tradition- and context-bound but apparently decisionist definitions of problems by scientists in a given discipline, or even worse to refer it, by default of a critical philosophy of science, to the academic division of labor. It is also a mistake to trivialize the question about the identification of constituent elements or "units" by referring it exclusively to the level of specific investigative techniques, such as participant observation, coding, and the like.[42]

The answer to this question should bring out whatever it is that distinguishes social science from physical science and whatever is common to the various social sciences. It is definitely not a matter of logical form. The logic of social science is the logic of science—if various rather general modes of explanation can indeed be given this designation because of their common origin in human logic as an idealized form of theoretical activity and because of their common cosmological purpose. There are, however, more specific styles of explanation which are determined by specific explanatory aims. These are associated with various disciplines. The specific styles of explanation, while themselves instances of more general modes, are tied to the substantive theoretical problems which the various disciplines confront, and the problems derive in their turn from the subject matter with which a discipline is concerned. I suggest that the family likeness in the explanatory problems faced by the disciplines of social science, from history to linguistics and very definitely including sociology, is due to the peculiarities of the areas over which they claim academic jurisdiction, and that these peculiarities originate in the structure of the domain over which social science implicitly, at least, also claims cosmological jurisdiction. The domain is the domain of human action and of its objectivated results. It is the constitution of the domain and the explanatory aims that are bound to it, rather than the logical form by means of which the domain is explained, that account for the difference between social and physical science.

41. Kaplan, *Conduct of Inquiry*, p. 358. Kaplan introduces here the useful terminological distinction between act and action. To say that social science is interested in the explanation of *actions* does not imply that in their explanatory frames data from ecology, ethology, neurophysiology, and so on, are not to be used in a full causal analysis of a given problem. It does, however, state a level of interest, and withdraws credit from metaphysical reductionism. For an interesting attempt to work back to that level of explanation from positions influenced by some forms of reductionism, see George A. Miller, Eugene Galanter, and Karl H. Pribram, *Plans and the Structure of Behavior* (New York: Holt, Rinehart, and Winston, 1960).

42. This seems to be the tendency of Hans Albert in his highly interesting and highly polemical "Hermeneutik und Realwissenschaft," *Mannheimer sozialwissenschaftliche Studien*, III (1971), 42–47.

It should not be, but it probably is, necessary to add that this does not mean that the domain (and its science) is to be considered autonomous within the over-all paradigm of cosmological "causal explanation." [43] It certainly does not imply that human action is the only thing of interest with respect to man or, for that matter, that an account of human action need not take in data from other explanatory frames which, however, are then subordinated to the explanation of action. What it does mean is that there is an autonomous theoretical interest in a level of explanation that is irreducibly that of human action. The identification of the domain of social science is determined by the human interest in the understanding of human action. The interest is theoretical but prescientific; the theoretical interest, in turn, originates in the praxis of everyday life. This interest is cosmological, entirely legitimate, and ineradicable. The theoretical interests of social science must represent this interest in the logical form of science.

The identification of the domain and of its constituent elements is not merely a matter of several levels of analysis in a "unitary" science, in the sense that different aspects of phe-

43. See Weber, *Zur Wissenschaftslehre*, especially pp. 322–34 and 509 f.; cf. also Geoffrey Madell, "Action and Causal Explanation," *Mind*, LXXVI (1967), 34–48.

nomena may be deducted and placed in a "deductive system." [44] It is that, but in a way which is trivial in the present context: the domain *does* happen to be the domain which is the foundation for the production of cosmologies of science and of operational-instrumentalist decisions on levels of analysis. No doubt the domain, and the production of cosmologies, and the decisions on levels of analysis, can be and should be made objects of empirical analysis. But even the most sophisticated sociology of knowledge is an insufficient answer to the problem of epistemological reflexivity in social science (we are back with Münchhausen, an utterly self-reliant but not entirely plausible gentleman).

An alternative and less problematically circular, because methodologically controlled, reflexive account of the constitution of the domain and its elementary structures is provided by the phenomenology of the *Lebenswelt.* The radical return to the immediate evidence of conscious experience provides an evidential starting point unavailable in common sense and scientific theory. The method of reduction permits controlled reflexiveness on the presuppositions of the method, of the evidence, and of their communication to others. The circle, as Husserl fully recognized, remains. But it is not viciously naïve.

The program for a *mathēsis universalis* of social reality is a proposal for a phenomenology of the universal structures of everyday life; it is to serve a methodological purpose in social science by supplying a matrix for the empirical analyses provided by the disciplines that deal with and "explain" the concrete historical structures of everyday life. The matrix is not "theory," i.e., it has no direct connection to the logic of explanation. Nor is it merely a regional taxonomy based on classificatory decisions. It is founded on a rigorous method that uncovers and clarifies invariant structures of the conscious activities in which human action is constituted.

44. Cf. this quotation from Morris R. Cohen and Ernest Nagel: "It follows that there is a *plurality* of systems, each of which may be explored in isolation from the others. Such a plurality of systems may, indeed, constitute a set of subsystems of a single, comprehensive system, but we have no evidence for such a state of affairs. In any case, it is not necessary to know this comprehensive system in order to explore adequately any one of the many less inclusive systems" (*An Introduction to Logic and Scientific Method* [New York: Harcourt, Brace, 1934], pp. 140–41). See also Hanson, *Patterns of Discovery,* pp. 134 f.

It is now easy to see why it is somewhat inexact to describe the program of a *mathēsis universalis* for social reality as a proposal for formalization appropriate to human affairs. Formalization generally refers to the establishment of systems of symbols and of rules governing the combination of symbols. When I speak of formalization in the present context I am not thinking of the general logic of the rules that govern the combinations of symbols. I am not thinking of a general logistics of operations on statements about social reality as distinct from a logistics of operations on statements about some other kind of reality. Just as I see no reason to accept the claims in favor of a special logical form in social science, I cannot easily conceive of a need for (or indeed the possibility of) a special algorithm for the universe of human action. It may very well be that there are serious difficulties in the "recasting of verbal theories as causal models," [45] and that the specific mathematical operations performed on statements about social reality within some explanatory paradigm leave a large residue of dissatisfaction. This is surely due in some measure to the inappropriateness of the specific operations. But I suspect that in large measure such dissatisfaction can be traced eventually to unsatisfactory solutions to the problems of identifying the "field" and defining the "units" in the field. I suspect, in other words, that the dissatisfaction stems from a frustration of the theoretical interest in the level of human action and its objectivations, not from a failure of the mathematics involved.[46]

45. Hubert M. Blalock, Jr., *Theory Construction* (Englewood Cliffs, N.J.: Prentice-Hall, 1969), p. 27.

46. "Mathematical techniques can often be applied to good effect even when the known facts have to be somewhat distorted to feed them into the mathematical machinery. But in the case of current algebraic grammar the amount of distortion, while not great, conceals just the most important fact about natural human languages: the fact that they are (technically speaking) 'ill-defined' systems, like table-manners or football or governments, rather than 'well-defined' systems like logic or mathematics." Thus Charles F. Hockett, disowning, as gracefully as only a scholar of his stature could, substantial parts of his own essay "Language, Mathematics and Linguistics" (*Current Trends in Linguistics,* Vol. III: *Theoretical Foundations,* ed. Thomas A. Sebeok [The Hague: Mouton, 1966]) in an author's précis of the essay published in *Current Anthropology,* IX (April–June, 1968), 128. Presumably all "natural systems" are ill defined, more or less. It is of course the "more or less" which offers room for further debate on what it is that is being defined. It will hardly do, however, to object to Hockett that "to the extent that natural language is an 'ill-defined' system, it is not language" (Eugene Verstraelen in a comment, *ibid.,* p. 149).

And that leaves us again with the problem of the constitution of social reality in human action. The formalization of "the rules of the game" presents no intrinsic difficulty—for any particular game in "nature" or "social reality." The difficulty arises from the fact that in society everybody is playing many games at the same time, and that the rules define the players, and the players define the rules.[47]

Furthermore, the proposal for a *mathēsis universalis* appropriate to social reality has no *direct* bearing on the question of quantification in social science. In principle, counting noses and performing operations on numerical items are methodologically rather harmless; taken in isolation, the practice is no more dangerous in social science than anywhere else. But it does become methodologically explosive because it is tied to the identification of the units in the field. It has to be decided *what* is to be counted. Indirectly, then, the problem of quantification, like the problem of formalization, is connected with the problem of the constitution of the domain and the level of explanation that is pertinent to a given explanatory interest.

I am led to suggest that what appears as a triad of the simplest theoretical activities: identification (observation)-counting-classification, is anything but that. A burning methodological issue centers around the implicit theory of measurement in social science. This issue is most acute in disciplines with a general explanatory aim, such as sociology and social anthropology, which try to account for *all* the games, less acute in disciplines with a relatively restricted region of investigation, such as linguistics, and least acute in disciplines with a sharply defined explanatory interest, such as economics. But in all social

47. The point is made by Ernest Gellner: *"The constraints, the 'rules' within which social life is played out, are themselves a consequence of the game. . . .* A 'structural' account of a society is an account of how this comes to be: how the game itself generates and sustains the limits within which it is played. This is the really crucial fact about sociological method. This manner of formulating it shows why the task is so much harder than that of a chess analyst, who has no need to explain just why the players will not knock over the board, why the rook will not move diagonally, and so forth" ("Our Current Sense of History," *European Journal of Sociology*, XII, no. 2 [1971], 170; italics original). Gellner maintains, incidentally, that language is less complex than "society," an assertion which makes sense in one way but seems debatable in another. The complexity is not simply a function of the "natural system" out there but also of the ("ill"-) *definition* of the system and its elements. It is instructive to compare Gellner on language with Hockett on football (see n. 46).

science disciplines the theory of measurement carries the imprint of the Galilean cosmology.[48] A methodologically legitimated decision on what is to be counted is generally avoided. In other words, the researcher unwittingly adopts individual or official, bureaucratic, common-sense taxonomies. Such decisions do not produce data whose comparability is warranted. All too readily the problem is glossed over by post hoc technical "validation" and submerged in displays of numerical mathematics whose items originated in "coding decisions."[49] The fact that we all do it cannot defend us against the accusation of methodological naïveté.

No Galilean primary qualities having been discovered in human conduct, assortments of secondary qualities are measured as if they directly represented primary ones. The consequence of social science naïveté concerning its epistemological reflexivity is, on the most prudishly hidden level of "operationalized" research procedure, measurement by fiat.[50] Cicourel describes the problem as follows:

> If the "rules" governing the use of language to describe objects and events in everyday life and in sociological discourse are unclear, then the assignment of numerals or numbers to the properties of objects and events according to some relatively congruent set of rules will also reflect a lack of clarity.[51]

I should merely add that the rules governing the use of language in everyday life are typically unclear in some necessary degree to ordinary speakers. Cicourel's statement should not be misconstrued as implying that the clarity of the rules governing the

48. It is hardly surprising to come across statements such as these: "The terms of classical Newtonian theory are, like those of social science, all concepts referring to what can be observed. The relatively simple structure of such a powerful theory has, to put it moderately, not yet been deployed to its fullest advantage by social scientists" (May Brodbeck, "Models, Meaning, and Theories," in Gross, *Symposium*, p. 401); and: "Talking about the meaning of people's movements does not suggest that the way of ascertaining its presence is not the same as the way of ascertaining the meaning of anything else" (Quentin Gibson, *The Logic of Social Enquiry* [New York: Humanities Press, 1960], p. 52).

49. For an excellent discussion of this and of related problems, see Cicourel, *Method and Measurement*, especially chap. 1.

50. The term was used by Warren Torgerson (*Theory and Method of Scaling*, [New York: Wiley, 1958], p. 21) and taken up critically by Cicourel (*Method and Measurement*, pp. 12 f.).

51. *Method and Measurement*, p. 15.

use of language to describe objects and events in *sociology* presupposes the clarity of the rules governing the *ordinary* use of language. Rules and "rules" in his statement are not strictly equivalent.[52]

Hochberg defines formalization as the replacement of descriptive signs in an axiomatic system by mere marks.[53] The proposal of a *mathēsis universalis* for social reality is thus not a proposal for full formalization but merely for partial formalization. The "descriptive signs" are statements on human conduct in ordinary language or, if one wishes to become technical about it, in an observational language in which the statements of actors are reformulated in one translation step and linked to statements of (actor-) observers.[54] These are to be "represented,"—not by mere marks, e.g., geometrical figures, but by statements on elementary structures of human conduct. It must be added immediately that the specific form of attentiveness to the ordinary language of the actors that characterizes some traditions in social science such as, for example, some symbolic interactionists and ethnomethodologists in sociology, ethnoscientists, "cognitive" anthropologists, etc., in social anthropology, and ethnographers of communication and componential analysts in anthropological linguistics, does not presuppose an established matrix of universal, invariant structures of everyday life which rigorous phenomenological analysis should yield. Nor does such attentiveness suffice in and by itself to generate such a matrix—but such traditions find direct methodological justification in this matrix. This does not mean that other traditions are to be excommunicated by some new methodology, but their research procedures can be shown to be more naïve than absolutely necessary in the

52. Cf. Weber, *Zur Wissenschaftslehre*, especially pp. 322–45, in which he introduced a regional German card-game (*Skat*) to social science literature. Cf. also Gellner's class-game and his discussion of Chomsky's notion of "rules" in language (Gellner, "Sense of History").

53. "Axiomatic Systems," p. 427.

54. Even Felix Kaufmann's well-known phrase, "every interpretation of social facts presupposes a fundamental interpretation, namely, that of the underlying physical fact as a social fact" does not specify adequately the complex constitution of "social facts" and of the recognition of social facts (*Methodology of the Social Sciences* [1944; 2d ed., New York: Humanities Press, 1958], p. 166). For an interesting account of typification of social reality based on an original synthesis of Peirce, William James, Schutz, and Gurwitsch, see Grathoff, *Structure of Social Inconsistencies*, chaps. 4 and 5. See also Kaplan, *Conduct of Inquiry*, pp. 32 f.

matter of operational definitions of the field and its units. Moreover, to the extent that unit-identification procedures, partial formalization of units, theoretical explanation, and methodological reflection on the entire theoretical enterprise are empirically connected, this naïveté does have repercussions.

The proposal of a *mathēsis universalis* for social reality is thus a proposal for formalization only in a loose sense of that term. In fact, it is a proposal for a *mathēsis universalis* in a restricted metaphorical sense. Its basic aim can be stated as the formalization of a matrix of elementary and universal structures of human conduct. In a way that is open to debate, the proposal takes up Husserl's notion of a science of the *Lebenswelt* and reformulates it as a proposal for a phenomenological foundation for the methodology of social science, inasmuch as that methodology cannot avoid being concerned with the peculiar problem of the structure of the domain that is undeniably constituted in human action.

The motive for and the foundation of the program are one:

> as soon as we consider that the life-world does have, in all its relative features, a *general structure*. This general structure, to which everything that exists relatively is bound, is not itself relative. We can attend to it in its generality and, with sufficient care, fix it once and for all in a way equally accessible to all. As life-world the world has, even prior to science, the "same" structures that the objective sciences presuppose in their substruction of a world which exists "in itself" and is determined through "truths in themselves" (this substruction being taken for granted due to the tradition of centuries); these are the same structures that they presuppose as a priori structures. . . .[55]

The phenomenology of the *Lebenswelt* is not to be taken as a substitute empirical method, as a kind of teutonically glorified subjectivistic social psychology or a cousin of analytic philosophy in Continental masquerade clarifying the concepts of ordinary and scientific language.[56] The descriptive phenomenology of the *Lebenswelt* is ultimately based on the phenomenological method of radical reduction and attention to the experience of intentional acts in originary evidence. It is thus philosophically

55. Husserl, *Crisis*, p. 139 (italics original).

56. For a critique of such misconceptions see Maurice Natanson, "Phenomenology as a Rigorous Science," *International Philosophical Quarterly*, VII (March, 1967), 5–20.

legitimated by a critically reflexive account of the knowledge of experience. In other words, the descriptive phenomenology of the natural attitude in everyday life has its methodological foundation in phenomenology as a transcendental critique of knowledge. For this reason it satisfies the need for a "philosophy of the mind" described by Burtt. It can offer the guarantee of continuous epistemological reflexivity, which is an essential condition of the philosophical foundation of social science.

Yet I believe that it is legitimate to suggest a more specific function for the descriptive phenomenology of the invariant structures of the world of everyday life: that is, to provide the general matrix, appropriate to the level of human action, for statements on human conduct articulated in historical vernaculars. Such a matrix offers a satisfactory solution to a fundamental problem of social science, the problem of the comparability of historical data. And, obviously, all the data of social science are historical.

To put it more precisely:

1. The data of social science are preinterpreted.[57] Interpretation of experience (and action) is a constitutive element of the data; we do not have "raw" data to which are added common-sense interpretations which are to be discarded by means of some "purifying instrument," if only we could find one (this is the old cosmology!).[58]
2. Interpretations are made in, and bound to, ordinary historical languages. The data of social science are therefore from the outset irrevocably part of historical worlds of everyday life: they are constituted in human action and experience as historically specific contexts of significance and motivation.

The universal structures of everyday life could serve as a matrix for such data, as a kind of metalanguage for the historical languages in which data on human action must necessarily be presented.

Such a matrix must meet two requirements. It would have to

57. In several methodological writings Schutz clarified the complex interdependence of the interpretations and preinterpretations that constitute the data of social science.

58. In view of misunderstandings (which shall charitably remain anonymous) I hasten to add that Schutz never suggested that *explanations* in social science must reproduce common-sense explanations.

meet the criterion of subjective adequacy in the sense in which Weber introduced the term. The "translations" of the statements (but not necessarily of their theoretical explanations) from the historical languages into the metalanguage or, more appropriately, the protolanguage, would have to be plausible in principle if not in immediate fact to the speakers-actors who produced the statements. The matrix would also have to be based on genuinely universal structures of the world of everyday life. It is not sufficient to generalize some apparently elementary features of a given common-sense view of man and society. This procedure cannot substitute for the phenomenological *epochē*, as witness the ethnocentric "picture-book phenomenology" of the twenties and, I might add, of the sixties as well.

The protolanguage consisting of the strict phenomenological account of the universal structures of the world of everyday life would thus represent a formalization (in the sense that it would not be another ordinary historical language) of statements on human conduct articulated in ordinary languages. The proposal is neutral with respect to the problem of logical form of explanation and of theory construction, except that it is motivated by and subordinated to the cosmological interest in the explanation of human action. At the same time, the proposal is intimately tied to a demand for a theory of measurement in social science. Measurement of human actions and their objectivations must be based on a two-level account: of the invariant structures underlying typifications of social reality (and thus co-constituting social reality), and of the invariant structures underlying linguistic articulation of historically variant concrete typifications of human action in human experience.

This program for the solution of a central problem of the methodology of social science is still just that: a program. Nevertheless, some important prerequisites for accomplishing the aims of the program have been met. Husserl himself, Gurwitsch, Schutz, and some other phenomenologists have been filling in the over-all contours of the *Lebenswelt* originally sketched out by Husserl. Still, much work remains to be done there. As for the specific use of descriptive phenomenology in methodological reflection, this essay is a tentative step in that direction. And finally, as for the replacement of the implicitly Galilean theory of measurement in social science by an epistemologically reflexive one, work on this task has begun in various

movements in social science that are close to or even claim descent from phenomenology (symbolic interactionism, ethnomethodology, ethnoscience, and others), as well as in other quarters that have no particular inclination to look for philosophical foundations in phenomenology, as in the increasingly rewarding work on universals in language by linguists of various theoretical persuasions, anthropological linguists working on ethnographies of speaking, and others who would perhaps dislike to be labelled but whom I would call "structuralists" sensitive to epistemological reflexivity.

A synthesis of these varied efforts in phenomenology and in the various approaches in the several disciplines will require more than an addition of individual results. It will require continual reflection on the sources and consequences of epistemological reflexivity in social reality and social science.[59] Without such reflection the relevance of science in general and social science in particular can be assumed but not explicated. And inability to explicate its relevance is a symptom of crisis.

Postscript on the Circle

Alas, there are no absolute certainties and there are no definitive resolutions of fundamental "crises." It is good to

59. I refer here only to the contribution to the theory of measurement. It is not my purpose to evaluate in detail the merits of the various approaches in producing accurate and relevant descriptions, although I may say that in my opinion some of the best work in this regard is being done in the approaches that pay attention to the "indexical" (to borrow Garfinkel's expression) features of communication. Nor am I proposing to assess the respective contributions of these approaches to theory, and thereby to the cosmological purpose of science. I would merely like to offer my opinion that some of these approaches are lapsing into a new empiricism. This empiricism differs significantly from the old "positivistic" empiricism in its descriptive sophistication, but its romantic self-sufficiency makes its more extreme versions almost as much of a threat to the cosmological purpose of science as its "positivistic" opposite number. The most cogent presentation of a point of view on sociology that is based on concerns similar to mine but whose conclusions are diametrically opposed to the one I have presented here can be found in Harvey Sacks, "Sociological Description," *Berkeley Journal of Sociology*, VIII (1963), 1–16. The *Lebenswelt*-romanticism that can be observed on the fringes of ethnomethodology and symbolic interactionism is generally absent from the converging developments in social anthropology and ethnolinguistics.

have friends who point out the facts of life![60] My argument which contained as an important step in its chain of reasoning a critique of a historical process of cosmological abduction relies in its proposal for a solution to the crisis in social science on abductive premises. I maintain that the circle in which I have become entangled is inevitable but not vicious. I suggest that it offers clarification for a scientific-cosmological enterprise. It does not resign itself to the inner circle of viciousness that characterizes the thinking of the modern descendants of the scientistic theory of knowledge. It is only fair, however, to let the reader judge whether the outer circle is not as tightly closed as the inner.

The physical and social sciences are engaged in a common cosmological enterprise. The enterprise follows certain general rules whose structure is analyzed in the logic of science and whose origin can be reconstructed historically. On that level it can be decided whether the sciences have the structure of hypothetico-deductive systems, whether notions of verification should be replaced by the concept of corroboration, whether or not over-all paradigms change in a revolutionary fashion, and so on.

These theoretical activities presuppose still more general activities of the mind. The idealized rules for these activities can be "operationalized" in a logistic system and traced back to their origins in everyday activities. In radical philosophical reflection following a precise rule of evidence (transcendental reduction, i.e., attending to phenomena as they "present" themselves), the structures of theoretical and pretheoretical activities are clarified and traced back to their foundation in active and passive syntheses of consciousness. This is a process of explication that starts with and returns to the most direct evidence available: inspection of immediate experience. The analysis points at all levels of the foundational structure of experience to its embeddedness in the *Lebenswelt*, the world of everyday life.

The descriptive phenomenology of everyday life which is ultimately founded on this radical method describes the universal structures of subjective orientation and action: lived space, lived time, the elementary structure of face-to-face situations,

60. This particular fact was pointed out by Hansfried Kellner.

the levels of anonymity, the biographical-historical subscript to all experience, the lived intersubjectivity of communication in everyday life, and so on. To the most general discoveries of the "geology" of the *Lebenswelt* these "geographic" analyses of a descriptive phenomenology thus add some basic surface contours.

But now one discovers "correlates" of these descriptions in the descriptive results of "naïve" empirical sciences and, indeed, of aesthetically filtered common-sense observation. That discovery is an invitation to embrace the pretheoretical immediacy of the *Lebenswelt* or to join the traditional cosmological enterprise of "naïve" science. There is no reason to decline the invitation, individually, as long as the differences in cognitive style, method, universe of discourse, and purpose are not extinguished.

And here another round starts. Theory in all sciences involved in the cosmological enterprise takes a number of things for granted which become problematic upon reflection:

1. The unity of experience among men in different societies throughout the course of history. Philosophically speaking, this refers to the problem of whether "mankind," a "transcendental ego," an "empirical species," or whatever, is the transcendental subject of knowledge.
2. The givenness and the possibility of communication. Philosophically speaking, this refers to the problem of *the mathēsis universalis.*

Social science rests upon an additional presupposition: that the ordinary, culturally and historically highly variable common-sense definitions of reality are "objective" data (sales, suicides, fathers, presidents, and so on). Philosophically speaking, this is the problem of the epistemological reflexivity of social science and of the human constitution of the domain under investigation.

With this problem we are back with the *Lebenswelt* as the foundation of science, and as the foundation of the field of social science. And we start with reflection on the presuppositions of a particular historical enterprise, science, in which our elementary cosmological interest is historically invested. The method of reflection must be "rational" and must be based on immediate evidence. And that is where we came in.

The solution to the epistemological reflexivity of social reality and social science which I proposed is not a definitive solution at all. It starts a process of reflection, however, in which the quest

for certainty about "starting points"—to be found in a radical philosophical method—is abductively joined to the cosmological interest in understanding the universe of which we are part—to be satisfied, more or less plausibly, more or less effectively, and perhaps *less* naïvely, by science.