1

Values and Social Science

The Value Dispute in Perspective

A dramatic chapter in the history of German social science reached its climax on January 5, 1914, in Berlin, at a meeting of the enlarged committee of the Verein für Sozialpolitik.[1] The circumstances of this meeting were strange enough. Before the discussion began, the more than fifty participants passed a number of resolutions that in themselves would have sufficed to ensure their meeting a place in history and legend: they sent the stenographers home, ruled any taking of minutes out of order, formally vowed to disclose nothing about the proceedings to any nonparticipant, and forbade publication of the written papers that had been submitted by eminent scholars as a basis for discussion. The fears that probably gave rise to this secrecy proved justified. The discussion ended in a passionate clash of convictions and personalities, a clash that divided German social scientists into two groups for years and in some respects divides them to the present day. The subject that led to such extraordinary measures and results was precisely the subject of these reflections: values and social science.

Even now it is difficult to reconstruct in detail the prehistory and history of the memorable *Werturteilsstreit* (as the Value Dispute was called at the time), and impossible to do so without taking sides.

1 This "Association for Social Policy," which had been founded in 1872, was in fact the professional organization of social scientists; it subsequently became, and is today, the German Economics Association. In its early years, it was dominated by the intellectuals who originated and supported Bismarck's welfare state policies. For a detailed history of the Verein, see Franz Boese (6).

Whatever one may think about the feasibility and desirability of a value-free social science, the subject of value-free science itself cannot, it appears, be discussed in a value-free, or even dispassionate, manner. So far as the historical Value Dispute is concerned, we know that from the beginning of the century on, the question of the place of "value judgments" in social science arose with increasing frequency and intensity in the debates of the Verein für Sozialpolitik. When in 1904 Edgar Jaffe, Werner Sombart, and Max Weber took over as editors of the *Archiv für Sozialwissenschaft und Sozialpolitik*, they published in their first issue a statement of policy that said, among other things: "Inevitably, problems of social *policy* will . . . find expression in the columns of this journal . . . alongside those of social *science*. But we would not dream of pretending that such discussions can be described as 'science,' and we shall see to it that they are not confused with it." (24: 157.)

This statement was meant to be polemical, and in fact constituted an outright attack on the prevailing mood of the Verein and its then almost undisputed head, Gustav Schmoller. It was Schmoller who had prescribed for the "science of economics" not merely the tasks "of explaining individual phenomena by their causes, of helping us understand the course of economic development, and if possible of predicting the future," but also that of "recommending" certain "economic measures" as "ideals" (23: 77). At the very next meeting of the Verein, the Mannheim meeting of 1905, a violent exchange on this issue took place between Schmoller and Weber, which led to the characterization of Weber and others as a "radical left wing" and had other lasting consequences. A few years later, in 1909, the "radical left wing" founded the German Sociological Society, whose 1910 statutes stated in no uncertain terms: "It is the purpose [of the Society] to advance sociological knowledge by undertaking purely scientific investigations and surveys, and by publishing and supporting purely scientific studies. . . . It rejects all concern with practical (ethical, religious, political, esthetic, etc.) goals of any kind." (8: v.)

In order to make clear the polemical nature of this paragraph, and of the founding of the German Sociological Society as such, the

explicit reference to the Verein in the report of the Society's executive committee at its second meeting in 1912 was hardly needed: "In contrast to the Verein für Sozialpolitik, whose very purpose is to make propaganda for certain ideals, our purpose has nothing to do with propaganda, but is exclusively one of objective research." (9:78.) Indeed, the founding charter of the Verein für Sozialpolitik had included such goals as "supporting the prosperous development" of industry, "stimulating timely, well-considered state intervention to protect the just interests of all participants" in the economy, and helping to accomplish "the supreme tasks of our time and our nation." (6: 248ff.) Despite the controversy, however, the "pure scientists" remained members of the Verein; indeed, they were the ones who proposed, in a circular letter of November 12, the explicit discussion of value judgments that turned into the Value Dispute. "To lay the groundwork for the meeting, the letter listed four topics for discussion: (1) the position of moral value judgments in scientific economics; (2) the relation of economic development to value judgments; (3) the determination of the goals of economic and social policy; and (4) the relation of general methodological principles to the particular requirements of academic teaching." (6: 145.)

In accordance with the proposal set forth in the letter, a number of members wrote "position papers" on the four topics to be discussed. The authors of these statements included Franz Eulenburg, Wilhelm Oncken, Joseph Schumpeter, Othmar Spann, Eduard Spranger, Max Weber, and Leopold von Wiese—to mention but a few of the most important names. At Schmoller's suggestion, the meeting took place on January 5, 1914, in the conspiratorial atmosphere described above, which was intended (in the words of a partisan, pro-Schmoller report written in 1939 by Franz Boese, then secretary of the Verein) "to preserve the wholly intimate character of the discussion, and also to make sure that the expected differences of opinion would not be used by outsiders against the Verein or against science." (6: 147.) After that, passion held sway: Weber and Sombart on one side, Karl Grünberg and the majority of those present on the other, clashed violently, until in the end—to quote

Boese's report again—Weber "once again rose to deliver a weighty statement, which, without mincing words, informed his opponents that they did not understand what he (Weber) was talking about," whereupon he "angrily" left the meeting (6: 147).

If we can trust such reports as we have, the Value Dispute ended with the clear "defeat" of the "pure scientists." Even seven years later, after the First World War and Weber's death, Paul Honigsheim observed somewhat apologetically: "Of all the things Max Weber did, said, and wrote, nothing has been as much talked about, commented on, misunderstood, and laughed off as his doctrine of a value-free approach in sociology." (13: 35.) But the "victory" of the "social politicians" proved ephemeral. The development of social science since the Value Dispute has been, in the words of the German economist Karl Schiller, a continuous "retreat from subjective value-tables to the toolbox" (22: 19). Indeed, it would seem that the relative strengths of the two parties to the debate have been reversed, so that today the defenders of a combination of values and social science feel themselves in the minority, and the "radical left wing" of our own day would be more likely to advance, at least in discussions of methodology, a position nearer Schmoller's.[2] Despite the duration and the undiminished intensity of the dispute, however, many of the questions underlying it have been repressed rather than resolved, with the result that they still require elucidation.

II

It would be wrong to describe the Value Dispute of 1914 as a matter of a few individuals. Yet its course was determined primarily by the one man with whose name it is today inseparably linked and for whom it was more than a scholarly debate: I mean, of

[2] The "conservatism-radicalism" debate is the main concern of American sociologists today; for a balanced, if partisan, discussion, see Jürgen Habermas, "Kritische und konservative Aufgaben der Soziologie" (1). A more general account of the state of the value debate can be found in the proceedings of the German Sociological Society meeting on the occasion of the one hundredth anniversary of Weber's birth, in 1964 (see 10).

course, Max Weber. The polemical statement quoted from the *Archiv für Sozialwissenschaft,* the profound differences with Schmoller, the founding of a German Sociological Society with statutes stressing the need for "pure science," the report of this Society's executive committee at its second meeting, the suggestion that an open debate be held in the Verein für Sozialpolitik—all this was Weber's work. In his essays, collected later under the title *Gesammelte Aufsätze zur Wissenschaftslehre,* and even more vividly in his famous Munich lecture on "Science as a Vocation," the intensity and passion of the bitter struggle to establish a value-free social science come to life. Weber took sides on the question that interests us here; he did so more radically than anyone else. For that very reason it seems useful to relate the following considerations, explicitly or implicitly, above all to Max Weber.

We are concerned here not with reawakening old passions, but with reformulating the relationship between social science and value judgments and essaying a few general propositions. We shall try especially to distinguish between the various aspects of this problem, which were all too often badly confused in the heat of the dispute fifty years ago. In doing so we shall have to distinguish questions that permit of analytic solutions from others that by their very nature allow plausible, perhaps convincing, but in the last analysis only personal answers. Weber called his position paper for the Value Dispute (reworked in 1917 for publication) "The Meaning of 'Value-free' in the Sociological and Economic Sciences." More precisely, we have to ask: What is the legitimate place of value judgments in sociology? Where must they be eliminated from scientific sociology, and how is this to be done? Where, how, and to what extent may value judgments exert their influence without endangering the goals and results of scientific research? Where might it indeed be necessary to abandon Weber's rigid insistence on value-free scholarship?

Even Weber complained that "interminable misunderstandings, and above all terminological (thus wholly sterile) disputes, have arisen over the term 'value judgment,' which obviously do not contribute anything at all to the substance of the problem." (24: 485.)

It seems indeed possible to set out—without extensive discussion—with the notion that value judgments are statements about what ought or ought not to happen, what is or is not desired, in the world of human action. Weber's definition seems useful: "By 'valuation' we shall understand the 'practical' rejection or approval of a phenomenon capable of being influenced by our actions." (24: 475.) It seems obvious, moreover, that any such value judgment, any statement that concerns a practical obligation, includes assumptions that are neither verifiable nor falsifiable by observable facts. In other words, value judgments cannot be derived from scientific insights.[3] The assertions of social science and value judgments may legitimately be seen as two distinct types of statement. We may ask, therefore, at which points in the sociologist's research he encounters value judgments, and how he should act in these encounters. If with this question in mind we follow the steps in the process of acquiring knowledge in social science, it seems to me that we find six points at which value judgments are an issue. A consideration of these six points may help to advance the discussion of a value-free sociology beyond its explosive and unsatisfactory state at the end of the *Werturteilsstreit*.

III

Scientific inquiry begins, at least in temporal terms, with the choice of a subject, and it is here that we find the first possible encounter between social science and value judgments. That the process of inquiry begins with the choice of a subject is rather a trivial statement; but if we advance one step further and ask on what basis a scholar chooses the themes of his research, we have left the realm of triviality. A sociologist may be interested in, say, "the position of the industrial worker in modern society" for many different reasons. Perhaps he merely believes that this is the subject to which he can contribute most. Perhaps he regards it as a neglected subject, one whose study may help to close gaps in our knowledge. He may have a research grant from an institute or foundation requiring him to work on the subject. Possibly he hopes to provide materials for political action to remedy a social injustice. Not all these motives (and there may be many others) involve value judgments, but some do; and the example is routine enough to make it clear that value judgments are often a factor in choosing a subject. Can or must such value judgments be eliminated? What is their place at this point in a sociological inquiry?

[3] "Critical theorists," i.e., sociologists with a Hegelian bent, prefer to be somewhat vague on this point, because they do not want to admit the possibility of a positive science without critical detachment (see Habermas, 1: 244). But even by these theorists the basic distinction between the "affirmative" and "critical" functions of science is not really denied.

The first, and broader, of these questions is easily answered. Let us assume that four different scholars, each guided by a different one of the four motives mentioned, begin to investigate "the position of the industrial worker in modern society," and further that this subject has been defined precisely, so that we may reasonably describe the four men as investigating the same thing. Obviously all four may come up with the same results, and so indeed they should if they follow the rules of empirical social science. But in this event the reasons influencing their choice of subject have no effect on their findings. Clearly, then, the choice of subject is made in what may be called the antechamber of science, where the sociologist is still free from the rules of procedure that will later govern his research. It is probably unrealistic to insist that value judgments be eliminated from the choice of subjects; in any case it is quite unnecessary, since the reason why a subject is regarded as worth investigating is irrelevant in principle to its scientific treatment.

This conclusion is by no means new or exciting; long ago Weber rightly dismissed as a "false objection" the claim that the choice of subjects involves a value judgment. But we may still ask whether or not value judgments *should* govern the choice of subjects for sociological research; in other words, whether fruitful scholarship does not in fact require that the choice of subjects be based on certain values. The answer to this question, whether affirmative or negative, does not of course affect our first conclusion. To stay with our metaphor, we are concerned now with the laws, if any, of the antechamber of science, laws that by definition bear no relation to the laws of science itself.

Time and again in the history of sociology, sociologists have been

urged to distinguish between "important" and "unimportant" subjects for research. Robert Lynd, in his essay "Values and the Social Sciences," describes as the "most prominent characteristic of the well-educated scholar" his ability to make this distinction between "important" and "unimportant" problems intelligently on the basis of what Lynd calls "guiding *values*" (15: 191). And indeed, the fact that there is no objective way of ranking people's motives for choosing subjects does not mean that all subjects are equally relevant and important. For example, two "guiding values" that I consider both important and defensible in objective terms are that sociologists should not be deterred by social taboos from investigating certain "objectionable" subjects, and that sociological research should promote people's understanding of their own society. Perhaps it can even be stated as a general proposition that the quality of scientific research improves to the extent that the choice of subject betrays a personal commitment on the part of the researcher. But we have to realize that such a commitment is in itself a value judgment. It is not part of scientific research; indeed it is in principle irrelevant to research except as a sort of precondition or moral environment. Its appeal, therefore, is neither to scientific insight nor to critical perception, but to the sense of evidence or possibly to the consensus of scholars.[4]

IV

Our first encounter between science and values, then, proves to be no encounter at all. Our second, which takes place at the stage of theory formation, is equally trivial. In their *Introduction to Sociology*, Jay Rumney and Joseph Maier warn their readers: "Sociology is not an easy subject to study. . . . Our passions and wishes, conscious and unconscious, too readily enter into the observation, selection, and classification of facts, which are the first steps in all sci-

[4] Modern methods of research planning seem to suggest otherwise, but the appearance is deceptive. Behind the subject priorities of the research planners we can usually find one or more of the following value judgments: research should be oriented toward government goals; research should be guided by the foreseeable chance of success; and research should be informed by a sense of economy of means.

ences. We see what we want to see and turn a blind eye to things we don't want to see." In order to remedy this alleged evil, the authors urge the sociologist to "train himself in attitudes of scientific objectivity" with the assistance of "psychoanalysis and the sociology of knowledge" (19: 27–28).

Now it is certainly true that many sociologists, in dealing with their subjects, see only what they want to see. Weber, for example, in investigating the genesis of industrial capitalism in Europe, saw the influence of Calvinism but not that of technological innovations. Talcott Parsons confines his analyses of the integration of societies largely to the normative level and neglects problems of institutional organization. The sociologist of our example—a conservative, let us assume—might see the position of the industrial worker in modern society solely as a matter of "adaptation" to industrial conditions and the effects of various adaptations on individual "satisfactions" and social "equilibrium." The things he does not want to see, things he dislikes as a citizen because they go against his political convictions (strikes, labor mobility, and the like), he does not see. Must we prescribe for him—must we prescribe for Parsons and Weber—"psychoanalysis and the sociology of knowledge"? By concentrating on certain aspects of a subject and neglecting others, have they improperly mixed social science and value judgments? Must value judgments be radically eliminated from the formulation of scientific theories?

Popper's argument is convincing:

> All scientific descriptions of facts are highly selective. . . . It is not only impossible to avoid a selective point of view, but also wholly undesirable to attempt to do so; for if we could do so, we should get not a more "objective" description, but only a mere heap of entirely unconnected statements. But, of course, a point of view is inevitable; and the naïve attempt to avoid it can only lead to self-deception, and to the uncritical application of an unconscious point of view. (17: ii, 260–61.)

I think we may go even further and assert that selectivity of this sort, even if based on a value judgment, is not only inevitable but also no threat to scientific inquiry. To see this, we need only distin-

guish between two aspects of scientific inquiry that are often misleadingly confused: the logic and the psychology of scientific discovery.

A selective point of view, such as the conservative bias of the sociologist described above, does cause a scholar to see what he wants to see and be blind to other things. However, this merely tells us how the scholar has come to formulate a given hypothesis X; it does not tell us whether hypothesis X is true or false, tenable or untenable. Neither the values nor the thought processes of a scientist determine the validity of his hypotheses; their validity is determined only by empirical test. Nor can empirical tests as such affect the values and thought processes of the scientist in any way. In short, the psychological motives behind the formulation of any scientific theory or hypothesis are irrelevant to its truth or validity. It follows that the encounter between social science and value judgments at the stage of theory formation cannot have harmful consequences.[5] Rumney and Maier's exhortation to practice objectivity is just as misplaced on this point as the often heard criticism that a scholar systematically undervalues certain aspects of his subject and overvalues others.

This conclusion—that the selective formation of theories is a pseudo-problem—may at first seem rather too simple. Harmless as selective theories may be, one might argue, sociologists in particular all too often forget the selective character of their assumptions once their research is under way, and end up by making excessively general claims for what are in fact partial theories. Whereas Parsons, for example, begins by trying to examine the normative aspects of social integration, he ends by asserting that the integration of societies occurs exclusively on the normative level. Such extending of theories beyond the area for which they were designed is indeed a fault, one that we shall examine below as "ideological distortion." This problem must not be confused, however, with the pseudo-problem of value-conditioned selection in the formation of

[5] The distinction between the logic and the psychology of science has other uses. Among other things, it can contribute to clearing up the debate about induction (the psychological process of discovery) and deduction (the logical structure of discovery).

scientific theories, which is indeed a pseudo-problem so long as "theories" refers to statements that can be conclusively tested by observation.

V

Next we come to still another pseudo-problem that played a highly confusing role in the Value Dispute: the problem of values as subjects of inquiry. At least since Durkheim, Pareto, and Weber, and particularly since Parsons's *Structure of Social Action*, the study of the normative aspects of social action has occupied a prominent place in scientific sociology. This theme has been even more prominent in recent social anthropology. Theorists and researchers alike have devoted much attention to the prevailing values (and deviant values) found in a given social context, i.e., to the value judgments that mold the behavior of people in society by operating as generalized and sanctioned norms.

This is, to be sure, another point of encounter between social science and value judgments, and one, moreover, that is specific to social science. But no extensive argument should be required to demonstrate that this encounter cannot conceivably lead to any serious confusion. When Weber's opponents interpreted his insistence on a value-free sociology as an effort to eliminate the subject matter of social values from sociological research, he was quite justified in charging them with an "almost unbelievably great misunderstanding." Along with Weber we can reply, "If the normatively valid becomes a subject of empirical investigation, it loses, as a subject of investigation, its normative character: it is treated as 'existing,' not as 'valid.' " (24: 517.) Indeed, it is neither necessary nor sensible to renounce the attempt to study the normative elements of social structure with the tools of empirical social science. Although there are numerous difficulties in such research, it involves no serious confusion of science and value judgments.

The three pseudo-problems discussed so far may seem rather far removed from the emotional, if not indeed from the substantive, core of the Value Dispute. But the objection is only partly valid. Even in Weber's statements a certain lack of clarity in posing the

question is apparent, and subsequent writers have repeatedly confused the pseudo-problems of the choice of subjects, the formation of theories, and the investigation of values with the other, more serious problems to which we shall now turn. It should be useful, then, to know which points of encounter between values and a value-free social science are only seemingly productive of problems. At the same time, we must not be misled by the relative ease with which these pseudo-problems are analyzed and dismissed. The problems to be discussed now—ideological distortion, the application of scientific results to practical problems, and the social role of the scientist—are both considerably more important and much less susceptible to unambiguous solution.

VI

Let us return for a moment to our conservative sociologist and his study of the industrial worker in modern society. Let us assume that this gentleman begins by investigating the relation of the worker to his work setting. He finds that workers derive satisfaction from their membership in small, so-called informal groups. In particular, he finds that the stronger a worker's ties are to such informal groups, the greater his output is, and the higher his morale. This is a relatively precise, testable statement. But now the sociologist takes a further step, suggested by his conservative outlook, and asserts that membership in informal groups is the *only* factor influencing job satisfaction and productivity. Wages, working conditions, and relations with supervisors and subordinates count for nothing; everything, he claims, depends on the functioning of informal groups.[6]

This particular way of confusing social science and value judgments—that is, presenting in the guise of scientific propositions what are demonstrably value statements unsupported by evidence—

[6] The illustration refers vaguely to Elton Mayo's interpretation of the Hawthorne findings (16). The example is of special interest in our context because a number of authors with very different temperaments and values have worked on these data.

I shall call ideological distortion.[7] In sociology we encounter time and again two kinds of such ideologically distorted statements. The first is the kind of overextension of specific propositions that is illustrated in our example. All so-called "single-factor theories," theories that assign absolute determining force to a single factor like race, nationality, or the relations of production, belong in this group; so does the familiar contemporary theory that the tendency toward a leveling-in of certain status symbols in present-day Western societies is transforming them into "classless" societies without structurally generated group conflicts.[8] The second is the presenting of untestable and thus speculative propositions as scientific. An example is the thesis of the alienation of the industrial worker. However much sense this thesis may make in philosophical terms, it has no place in empirical social science, since no amount of empirical research can either confirm or refute it.[9]

All distortions of this kind contain implicit value judgments. Moreover, it is evident that if statements are alleged to be based on scientific investigation when they are in fact drawn from other sources, we are faced with a serious confusion of values and social science. But how is the sociologist to avoid ideological distortions, or to perceive and correct them where they have occurred? Three suggested answers to this question are offered in the literature. The first is the one recommended by Rumney and Maier: training in objectivity with the assistance of psychoanalysis and the sociology of knowledge. More than other scholars, the sociologist, himself inseparably a part of the subject of his research, is in danger of confusing his professional statements with his personal value judgments. The only way to avoid this confusion is by instituting a permanent process of self-observation and self-criticism, in which all propositions are systematically scrutinized for traces of ideological

[7] There are many other notions of ideology. This one is best described by Theodor Geiger (12).

[8] One example is the work of Helmut Schelsky (21). See my discussion of the thesis in *Class and Class Conflict in Industrial Society* (7).

[9] I refer here to Georges Friedmann's use of the notion of alienation to explain away overwhelming evidence of job satisfaction (11).

distortion. Another suggestion is that the sociologist explicitly declare the values that have guided him in his research, so that his readers or listeners will be in a position to analyze any ideological distortions that he might unwittingly be guilty of.

It seems to me, however, that a third suggestion is considerably more promising and effective than the first two. Science is always a concert of many. The progress of science rests at least as much on the cooperation of scholars as it does on the inspiration of the individual. This cooperation must not be confined to the all too popular "teamwork"; rather, its most indispensable task is mutual criticism. Wherever scientific criticism gives way to a careless or quietistic tolerance, the gate is open to dishonest and worthless research. Let us not forget that ideologically distorted statements are always bad scientific statements. And it seems to me that it is the main task of scientific criticism to expose such statements and correct them. In the long run, this procedure alone can protect sociology—though not the individual sociologist—against the danger of ideological distortion.[10]

VII

The problem of ideological distortion played only a minor part in the Value Dispute; it was not until the 1920's that Max Scheler and Karl Mannheim made it a central concern of social scientists. Another problem seemed much more important to those who attended the meeting of January 5, 1914—the relation between social science and social policy, or, as we shall put it, the application of scientific results to practical problems. The desire to relate theories and propositions to practical life is probably as old as science itself. Since the beginnings of human thought, technical problems have time and again stimulated scientific insights. However, our question here is not whether practical problems may properly inspire scientific research—this is merely an aspect of the problem of choice of subjects discussed above—but whether the scholar may properly

[10] The allusion to Popper in these remarks is evident. For further discussion of the problem, see the essay "Uncertainty, Science, and Democracy" below.

bridge the gap between the results of his research and practical action. Let us suppose that our conservative sociologist, having discovered the importance of informal groups to workers' morale, systematically sets about fostering the formation of such groups. Is this a legitimate part of his scientific activity or not?

Obviously, the application of scientific results to practical problems involves an encounter between science and value judgments. In order to do what the sociologist of our example does, one must place some value on the workers' morale. Two utterly different ways of thinking meet here: systematic empirical observation leading to insights into what is, and the strictly meta-empirical conviction of what should be. The latter, the value judgment, is in no way implicit in the former, the scientific insight. It is an additional and different matter; it is above all a matter removed from the domain of the social scientist as such. The application of scientific results, involving as it does an implicit or even explicit decision about goals and purposes, cannot be considered part of the social scientist's professional activity. At this point, science and value judgments have to be strictly separated.

Is there such a thing, then, as a scientific social policy? Or does the sociologist have to renounce all intention of intervening in the destiny of the society he investigates? It seems to me that Weber's answer to these questions is just as valid today as it was half a century ago. If by "intervening" (the concept of "social engineering" often used in England and the United States comes to mind here) we mean action related to goals determined by the sociologist himself, then such action lies outside his strictly scientific competence. He can, however, use the scientific knowledge at his disposal to suggest promising ways of realizing goals formulated by someone else. To try to reorganize a company on one's own initiative and on the implicit assumption that the workers' morale is a value is an undertaking beyond the boundaries of science. To express an opinion, however, about how higher morale might best be achieved remains within these boundaries.[11] To quote Weber again: for the

[11] The rigor of this statement may be open to dispute. Disentangling means and ends is obviously easier in theory than in practice. But perhaps the discussion of the

scientist, "the purpose of discussing value judgments can be no more than to elaborate the final, inherently 'consistent' value axioms from which . . . contradictory opinions are derived . . . ; to deduce the 'consequences' for certain value positions that would follow from certain ultimate value axioms if they, and only they, provided the basis of value judgments in factual matters"; and above all, "to state the consequences that would necessarily follow from putting a given value judgment into effect." (24: 496.)

VIII

The problem of application leads us immediately to a final aspect of the relationship between social science and value judgments, the problem of the sociologist's social role. Almost certainly this problem was the emotional basis of the Value Dispute of 1914, and it is still controversial today. Social role analysis applies not only to non-sociologists, but to sociologists as well. Like the doctor, the tailor, the accountant, and the party secretary, the sociologist has a social position endowed with certain expectations that its incumbent is expected to fulfill. Perhaps only on this level can the sociologist usefully ask whether his task is exhausted by inquiring into what exists, or whether, as a sociologist, he is obliged to state and defend value judgments as well. When Schmoller held that the social scientist as such was obliged to guide society in the "right" direction, and Weber countered by recommending the relentless separation of what belongs "in the lecture hall" from what belongs "in political programs, offices, and parliaments," the two men were really arguing about the role of the scholar, i.e., about what the scholar as such is called upon to do. Science and value judgments are two different matters. The question is: Should the social scientist profess both of them in his teaching and writing, or does his calling confine him to strictly scientific matters?

There is without doubt a certain internal consistency in Weber's position, which requires a value-free approach not only of sociology, but also of the sociologist as such. But consistency by itself is no guarantee of truth. Unlike Weber, and at the risk of sounding paradoxical, I want to advance the thesis that whereas sociology as a value-free science in Weber's sense may be desirable, the sociologist as such must always be morally committed if he is to protect himself and others from unintended consequences of his actions.

My main intention in these reflections has been to show that a value-free sociology represents a much less dramatic program than the impassioned disputants of the *Werturteilsstreit* thought it to be. At many points the encounter of social science and value judgments is harmless, and at others the concert of scholarly opinion may exert a corrective influence. A value-free sociology in this sense certainly corresponds to the ethics of scientific research. But the sociologist has to be more than a man who works at sociology. Whatever he does, says, and writes has potentially far-reaching effects on society. It may be true, generally speaking, that sociologists are neither better nor worse than the societies they live in.[12] But even if sociological research merely helps strengthen such tendencies as are already present in society, the sociologist remains responsible for the consequences of his actions. The conservatism of large sections of American sociology is unfortunate in itself. It is also a convincing refutation of Weber's rigorous separation of roles because it is largely unintentional, so that, for example, the conservative implications of structural-functional theory often contradict the functionalists' explicit political convictions. To protect oneself against such unintended consequences of one's actions, to maintain the integrity of one's moral convictions and one's work as a scholar, is thus a requirement that applies to the sociologist *qua* sociologist.

In a strict analytical sense, Karl Jaspers is surely right when he interprets Weber as follows:

> The scientific impulse to discover the truth and the practical impulse to defend one's own ideals are two different things. This does not mean that they can be acted on independently of each

sociologist's social role in the following section will provide an answer to the questions remaining here.

[12] This thesis has been put forth by Helmut Schelsky (20) in opposition to the claim that empiricism is conservative, theory radical; Schelsky notes that even routine empirical research may be radical in its effect under highly ideological conditions.

other. Weber is simply against confusing the two; only when they are clearly distinguished can they both be acted on efficiently. There is no relation between scientific objectivity and opportunism. The confusion of the two destroys objectivity as well as conviction. (14: 47.)

Indeed, it would probably be unfair to accuse Weber of separating his own scholarly work too rigidly from his political convictions. But the passionate and explosive union of "science as a vocation" and "politics as a vocation" in Weber's personality is so rare, so entirely personal a solution, that it cannot possibly be thought of as a pattern for all sociologists. Perhaps the only difference between my position and Weber's is a slight difference of emphasis. But it seems to me more important today to warn against the radical separation of science and value judgments than to warn against their commingling. Our responsibility as sociologists does not end when we complete the process of scientific inquiry; indeed, it may begin at that very point. It requires no less than the unceasing examination of the political and moral consequences of our scholarly activity. It commits us, therefore, to professing our value convictions in our writings and in the lecture hall as well.

2

Homo Sociologicus

On the History, Significance, and Limits of the Category of Social Role

Ordinarily, we do not much care that the table, the roast, and the wine of the scientist are paradoxically different from the table, the roast, and the wine of our everyday experience. If we want to put down a glass or write a letter, a table seems a suitable support. It is smooth, solid, and even, and a physicist would scarcely disturb us by observing that the table is "in reality" a most unsolid beehive of nuclear particles. Nor can the chemist spoil our enjoyment of the dinner by dissolving roast and wine into elements that we could hardly be tempted to consume as such. As long as we do not approach the paradox of the scientific table and the everyday table with philosophic intent, we solve it in a simple manner. We act as if the table of the physicist and our own table are two different things that have no relevant relation to each other. While we are quite prepared to concede to the physicist that his table is a most important and useful object for him, we are at the same time satisfied with our table precisely because it is not a multiply perforated beehive of moving particles.[1]

The dilemma is less easily solved when we turn to the biological sciences, especially to the biology of man. There is something unsettling about viewing a glass model of a man in an exhibition, or

[1] In an unpublished paper on "Paradox and Discovery," the Cambridge philosopher John Wisdom has discussed the paradox of the two tables at some length. For Wisdom, this paradox and others of its kind are the starting points of a well-considered metaphysics that inquires into the epistemological basis of statements without regard to their logical structure and empirical validity.