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THE LOGIC OF THE SOCIAL SCIENCES

First Contribution to the Symposium*

I propose to begin my paper on the logic of the social sciences with two theses which formulate the opposition between our knowledge and our ignorance.

First thesis: We know a great deal. And we know not only many details of doubtful intellectual interest but also things which are of considerable practical significance and, what is even more important, which provide us with deep theoretical insight, and with a surprising understanding of the world.

Second thesis: Our ignorance is sobering and boundless. Indeed, it is precisely the staggering progress of the natural sciences (to which my first thesis alludes) which constantly opens our eyes anew to our ignorance, even in the field of the natural sciences themselves. This gives a new twist to the Socratic idea of ignorance. With each step forward, with each problem which we solve, we not only discover new and unsolved problems, but we also discover that where we believed that we were standing on firm and safe ground, all things are, in truth, insecure and in a state of flux.

My two theses concerning knowledge and ignorance only appear to contradict one another. The apparent contradiction is primarily due to the fact that the words 'knowledge' and 'ignorance' are not used in the two theses as exact opposites. Yet both ideas are important, and so are both theses: so much so that I propose to make this explicit in the following third thesis.

Third thesis: It is a fundamentally important task for every theory of knowledge, and perhaps even a crucial requirement, to

* This was the opening contribution to the Tübingen symposium, followed by Professor Adorno's reply. The translation was revised by the author for the present publication. A few small additions have been made. See also the last contribution to the present volume.

do justice to our first two theses by clarifying the relations between our remarkable and constantly increasing knowledge and our constantly increasing insight that we really know nothing.

If one reflects a little about it, it becomes almost obvious that the logic of knowledge has to discuss this tension between knowledge and ignorance. An important consequence of this insight is formulated in my fourth thesis. But before I present this fourth thesis, I should like to apologize for the many numbered theses which are still to come. My excuse is that it was suggested to me by the organizers of this conference that I assemble this paper in the form of numbered theses [in order to make it easier for the second symposiast to present his critical counter-theses more sharply]. I found this suggestion very useful despite the fact that this style may create the impression of dogmatism. My fourth thesis, then, is the following.

Fourth thesis: So far as one can say at all that science, or knowledge, 'starts from' something, one might say the following: Knowledge does not start from perceptions or observations or the collection of data or facts, but it starts, rather, from *problems.* One might say: No knowledge without problems; but also, no problems without knowledge. But this means that knowledge starts from the tension between knowledge and ignorance. Thus we might say not only, no problems without knowledge; but also, no problems without ignorance. For each problem arises from the discovery that something is not in order with our supposed knowledge; or, viewed logically, from the discovery of an inner contradiction between our supposed knowledge and the facts; or, stated perhaps more correctly, from the discovery of an apparent contradiction between our supposed knowledge and the supposed facts.

While my first three theses may perhaps, because of their abstract character, create the impression that they are somewhat removed from our topic—that is, the logic of the social sciences—I should like to say that with my fourth thesis we have arrived at the heart of our topic. This can be formulated in my fifth thesis, as follows.

Fifth thesis: As in all other sciences, we are, in the social sciences, either successful or unsuccessful, interesting or dull, fruitful or unfruitful, in exact proportion to the significance or interest of the problems we are concerned with; and also, of course, in exact proportion to the honesty, directness and simplicity with which

we tackle these problems. In all this we are in no way confined to theoretical problems. Serious practical problems, such as the problems of poverty, of illiteracy, of political suppression or of uncertainty concerning legal rights were important starting-points for research in the social sciences. Yet these practical problems led to speculation, to theorizing and thus to theoretical problems. In all cases, without exception, it is the character and the quality of the problem—and also of course the boldness and originality of the suggested solution—which determine the value, or the lack of value, of a scientific achievement.

The starting-point, then, is always a problem; and observation becomes something like a starting-point only if it reveals a problem; or in other words, if it surprises us, if it shows us that something is not quite in order with our knowledge, with our expectations, with our theories. An observation creates a problem only if it clashes with certain of our conscious or unconscious expectations. But what in this case constitutes the starting-point of our scientific work is not so much an observation pure and simple, but rather an observation that plays a particular role; that is, an observation which creates a problem.

I have now reached the point where I can formulate my *main thesis,* as thesis number six. It consists of the following.

Sixth thesis:

(a) The method of the social sciences, like that of the natural sciences, consists in trying out tentative solutions to certain problems: the problems from which our investigations start, and those which turn up during the investigation.

Solutions are proposed and criticized. If a proposed solution is not open to pertinent criticism, then it is excluded as unscientific, although perhaps only temporarily.

(b) If the attempted solution is open to pertinent criticism, then we attempt to refute it; for all criticism consists of attempts at refutation.

(c) If an attempted solution is refuted through our criticism we make another attempt.

(d) If it withstands criticism, we,accept it temporarily; and we accept it, above all, as worthy of being further discussed and criticized.

(e) Thus the method of science is one of tentative attempts to solve our problems; by conjectures which are controlled by

severe criticism. It is a consciously critical development of the method of 'trial and error'.

(f) The so-called objectivity of science lies in the objectivity of the critical method. This means, above all, that no theory is beyond attack by criticism; and further, that the main instrument of logical criticism—the logical contradiction—is objective.

The basic idea which lies behind my central thesis might also be put in the following way.

Seventh thesis: the tension between knowledge and ignorance leads to problems and to tentative solutions. Yet the tension is never overcome. For it turns out that our knowledge always consists merely of suggestions for tentative solutions. Thus the very idea of knowledge involves, in principle, the possibility that it will turn out to have been a mistake, and therefore a case of ignorance. And the only way of 'justifying' our knowledge is itself merely provisional, for it consists in criticism or, more precisely, in an appeal to the fact that *so far* our attempted solutions appear to withstand even our most severe attempts at criticism.

There is no positive justification: no justification which goes beyond this. In particular, our tentative solutions cannot be shown to be probable (in any sense that satisfies the laws of the calculus of probability).

Perhaps one could describe this position as *the critical approach* ('critical' alludes to the fact that there is here a relation to Kant's philosophy).

In order to give a better idea of my main thesis and its significance for sociology it may be useful to confront it with certain other theses which belong to a widely accepted methodology which has often been quite unconsciously and uncritically accepted and absorbed.

There is, for instance, the misguided and erroneous methodological approach of naturalism or scientism which urges that it is high time that the social sciences learn from the natural sciences what scientific method is. This misguided naturalism establishes such demands as: begin with observations and measurements; this means, for instance, begin by collecting statistical data; proceed, next, by induction to generalizations and to the formation of theories. It is suggested that in this way you will approach

the ideal of scientific objectivity, so far as this is at all possible in the social sciences. In so doing, however, you ought to be conscious of the fact that objectivity in the social sciences is much more difficult to achieve (if it can be achieved at all) than in the natural sciences. For an objective science must be 'value-free'; that is, independent of any value judgment. But only in the rarest cases can the social scientist free himself from the value system of his own social class and so achieve even a limited degree of 'value freedom' and 'objectivity'.

Every single one of the theses which I have here attributed to this misguided naturalism is in my opinion totally mistaken: all these theses are based on a misunderstanding of the methods of the natural sciences, and actually on a myth—a myth, unfortunately all too widely accepted and all too influential. It is the myth of the inductive character of the methods of the natural sciences, and of the character of the objectivity of the natural sciences. I propose in what follows to devote a small part of the precious time at my disposal to a critique of this misguided naturalism.*

Admittedly, many social scientists will reject one or other of the theses which I have attributed to this misguided naturalism. Nevertheless this naturalism seems at present to have gained the upper hand in the social sciences, except perhaps in economics; at least in English-speaking countries. I wish to formulate the symptoms of this victory in my eighth thesis.

Eighth thesis: Before the Second World War, sociology was regarded as a general theoretical social science, comparable, perhaps, with theoretical physics, and social anthropology was regarded as a very special kind of sociology—a descriptive sociology of primitive societies. Today** this relationship has been completely reversed; a fact to which attention should be drawn. Social anthropology or ethnology has become a general social science, and sociology has resigned itself more and more to playing the part of a special kind of social anthropology: the social anthropology of the highly industrialized West European or American forms of society. Restated more briefly, the relationship

* (Note to the English edition.) What my Frankfurt opponents call positivism seems to me the same as what I here call 'misguided naturalism'. They tend to ignore my rejection of it.

** (Note to the English edition.) Since this was written in 1961, there has been a strong reaction to the tendencies here criticized.

between sociology and anthropology has been reversed. Social anthropology has been promoted from an applied descriptive discipline to a key theoretical science and the anthropologist has been elevated from a modest and somewhat short-sighted descriptive fieldworker to a far-seeing and profound social theorist and social depth-psychologist. The former theoretical sociologist however must be happy to find employment as a fieldworker and a specialist: his function is to observe and to describe the totems and taboos of the natives of the white race in Western Europe and the United States.

But one probably should not take this change in the fate of the social scientist too seriously; particularly as there is no such thing as the essence of a scientific subject. This leads me to my ninth thesis.

Ninth thesis: A so-called scientific subject is merely a conglomerate of problems and attempted solutions, demarcated in an artificial way. What really exists are problems and solutions, and scientific traditions.

Despite this ninth thesis, the complete reversal in the relations between sociology and anthropology is extremely interesting, not on account of the subjects or their titles, but because it points to the victory of a pseudo-scientific method. Thus I come to my next thesis.

Tenth thesis: The victory of anthropology is the victory of an allegedly observational, allegedly descriptive and allegedly more objective method, and thus of what is taken to be the method of the natural sciences. It is a Pyrrhic victory: another such victory and we—that is, both anthropology and sociology—are lost.

My tenth thesis may be formulated, I readily admit, a little too pointedly. I admit of course that much of interest and importance has been discovered by social anthropology, which is one of the most successful social sciences. Moreover, I readily admit that it can be fascinating and significant for us Europeans to see ourselves, for a change, through the spectacles of the social anthropologist. But although these spectacles are perhaps more coloured than others, they hardly are, for this reason, more objective. The anthropologist is not the observer from Mars which he so often believes himself to be and whose social role he often attempts to play (and not without gusto); quite apart from the fact that there is no reason to suppose that an inhabitant of Mars would see us more 'objectively' than we, for instance, see ourselves.

In this context I should like to tell a story which is admittedly extreme but in no way unique. Although it is a true story, this is immaterial in the present context: should the story seem improbable to you then, please, take it as an invention, as a freely invented illustration, designed to make clear an important point by means of crass exaggeration.

Years ago, I was a participant in a four-day conference, organized by a theologian, in which philosophers, biologists, anthropologists and physicists participated—one or two representatives from each discipline; in all eight participants were present. The topic was, I think, 'Science and Humanism'. After several initial difficulties and the elimination of an attempt to impress us by exalted depth ['*erhabene Tiefe'* is a term of Hegel's who failed to see that an exalted depth is just a platitude] the joint efforts of roughly four or five participants succeeded in the course of two days in raising the discussion to an uncommonly high level. Our conference had reached the stage—or so it appeared to me at least—-at which we all had the happy feeling that we were learning something from one another. At any rate, we were all immersed in the subject of our debate when out of the blue the social anthropologist made his contribution.

'You will, perhaps, be surprised', he said, 'that I have said nothing so far in this conference. This is due to the fact that I am an observer. As an anthropologist I came to this conference not so much in order to participate in your verbal behaviour but rather to study your verbal behaviour. This is what I have succeeded in doing. Concentrating on this task, I was not always able to follow the actual content of your discussion. But someone like myself who has studied dozens of discussion groups learns in time that the topic discussed is relatively unimportant. We anthropologists learn'—this is almost verbatim (so far as I remember)—'to regard such social phenomena from the outside and from a more objective standpoint. What interests us is not the what, the topic, but rather the how: for example, the manner in which one person or another attempts to dominate the group and how his attempts are rejected by the others, either singly or through the formation of a coalition; how after various attempts of this type a hierarchical order and thus a group equilibrium develops and also a group ritual of verbalization; these things are always very similar no matter how varied the question appears to be which serves as the topic of the discussion.'

We listened to our anthropological visitor from Mars and to all he had to say; and then I put two questions to him. First, whether he had any comment to make on the actual content and result of our discussion; and then, whether he could not see that there were such things as impersonal reasons or arguments which could be valid or invalid. He replied that he had had to concentrate too much on the observation of our group behaviour to have been able to follow our argument in detail; moreover, had he done so, he would have endangered (so he said) his objectivity; for he might have become involved in the argument; and had he allowed himself to be carried away by it, he would have become one of us—and that would have been the end of his objectivity. Moreover, he was trained not to judge the literal content of verbal behaviour (he constantly used the terms 'verbal behaviour' and 'verbalization'), or to take it as being important. What concerned him, he said, was the social and psychological function of this verbal behaviour. And he added something like the following. 'While arguments or reasons make an impression on *you*, as participants in a discussion, what interests *us* is the fact that through such means you can mutually impress and influence each other; and also of course the symptoms of this influence. We are concerned with concepts such as emphasis, hesitation, intervention, and concession. We are actually not concerned with the factual content of the discussion but only with the role which the various participants are playing: with the dramatic interplay as such. As to the so-called arguments, they are of course only one aspect of verbal behaviour and not more important than the other aspects. The idea that one can distinguish between arguments and other impressive verbalizations is a purely subjective illusion; and so is the idea of a distinction between objectively valid and objectively invalid arguments. If hard pressed, one could classify arguments according to the societies or groups within which they are, at certain times, accepted as valid or invalid. That the time element plays a role is also revealed by the fact that seemingly valid arguments, which are at one time accepted in a discussion group such as the present one, may nevertheless be attacked or rejected at a later stage by one of the participants.'

I do not wish to prolong the description of this incident. I imagine that it will not be necessary to point out, in the present gathering, that the somewhat extreme position of my anthro-

pological friend shows in its intellectual origin the influence not only of the behaviouristic ideal of objectivity but also of certain ideas which have grown on German soil. I refer to the idea of philosophical relativism: historical relativism, which believes that there is no objective truth but instead merely truths for this or that age; and sociological relativism, which teaches that there are truths or sciences for this or that class or group or profession, such as proletarian science and bourgeois science. I also believe that the sociology of knowledge has its full share of responsibility, for it contributed to the pre-history of the dogmas echoed by my anthropological friend. Admittedly, he adopted a somewhat extreme position at that conference. But this position, especially if one modifies it a little, is neither untypical nor unimportant.

But this position is *absurd.* Since I have criticized historical and sociological relativism and also the sociology of knowledge in detail elsewhere, I will forego criticism here. I will confine myself to discussing very briefly the naive and misguided idea of scientific objectivity which underlies this position.

Eleventh thesis: It is a mistake to assume that the objectivity of a science depends upon the objectivity of the scientist. And it is a mistake to believe that the attitude of the natural scientist is more objective than that of the social scientist. The natural scientist is just as partisan as other people, and unless he belongs to the few who are constantly producing new ideas, he is, unfortunately, often very biased, favouring his pet ideas in a one-sided and partisan manner. Several of the most outstanding contemporary physicists have also founded schools which set up a powerful resistance to new ideas.

However, my thesis also has a positive side and this is more important. It forms the content of my twelfth thesis.

Twelfth thesis: What may be described as scientific objectivity is based solely upon a critical tradition which, despite resistance, often makes it possible to criticize a dominant dogma. To put it another way, the objectivity of science is not a matter of the individual scientists but rather the social result of their mutual criticism, of the friendly-hostile division of labour among scientists, of their co-operation and also of their competition. For this reason, it depends, in part, upon a number of social and political circumstances which make this criticism possible.

Thirteenth thesis: The so-called sociology of knowledge which tries to explain the objectivity of science by the attitude of

impersonal detachment of individual scientists, and a lack of objectivity in terms of the social habitat of the scientist, completely misses the following decisive point: the fact that objectivity rests solely upon pertinent mutual criticism. What the sociology of knowledge misses is nothing less than the sociology of knowledge itself—the social aspect of scientific objectivity, and its theory. Objectivity can only be explained in terms of social ideas such as competition (both of individual scientists and of various schools); tradition (mainly the critical tradition); social institution (for instance, publication in various competing journals and through various competing publishers; discussion at congresses); the power of the state (its tolerance of free discussion).

Such minor details as, for instance, the social or ideological habitat of the researcher, tend to be eliminated in the long run; although admittedly they always play a part in the short run.

In a way similar to that in which we have solved the problem of objectivity, we can also solve the related problem of the freedom of science from involvement in value judgments ('value freedom'); and we can do so in a freer, a less dogmatic way, than is usually done.

Fourteenth thesis: In a pertinent critical discussion we may distinguish such questions as: (1) The question of the truth of an assertion; the question of its relevance, of its interest and of its significance relative to the problems in which we are interested. (2) The question of its relevance and of its interest and of its significance for various *extra-scientific problems,* for example, problems of human welfare or the quite differently structured problems of national defence; or (by contrast) of an aggressive nationalist policy; or of industrial expansion; or of the acquisition of personal wealth.

It is clearly impossible to eliminate such extra-scientific interests and to prevent them from influencing the course of scientific research. And it is just as impossible to eliminate them from research in the natural sciences—for example from research in physics—as from research in the social sciences.

What is possible and what is important and what lends science its special character is not the elimination of extra-scientific interests but rather the differentiation between the interests which do not belong to the search for truth and the purely scientific interest in truth. But although truth is our regulative

principle, our decisive scientific value, it is not our only one. Relevance, interest, and significance (the significance of statements relative to a purely scientific problem situation) are likewise scientific values of the first order; and this is also true of values like those of fruitfulness, explanatory power, simplicity, and precision.

In other words, there exist *purely* scientific values and disvalues and *extra*-scientific values and disvalues. And although it is impossible to separate scientific work from extra-scientific applications and evaluations, it is one of the tasks of scientific criticism and scientific discussion to fight against the confusion of value-spheres and, in particular, to separate extra-scientific evaluations from *questions of truth.*

This cannot, of course, be achieved once and for all, by means of a decree; yet it remains one of the enduring tasks of mutual scientific criticism. The purity of pure science is an ideal which is presumably unattainable; but it is an ideal for which we constantly fight—and should fight—by means of criticism.

In formulating this thesis I have said that it is practically impossible to achieve the elimination of extra-scientific values from scientific activity. The situation is similar with respect to objectivity: we cannot rob the scientist of his partisanship without also robbing him of his humanity, and we cannot suppress or destroy his value judgments without destroying him as a human being *and as a scientist.* Our motives and even our purely scientific ideals, including the ideal of a disinterested search for truth, are deeply anchored in extra-scientific and, in part, in religious evaluations. Thus the 'objective' or the 'value-free' scientist is hardly the ideal scientist. Without passion we can achieve nothing —certainly not in pure science. The phrase 'the passion for truth' is no mere metaphor.

It is, therefore, not just that objectivity and freedom from involvement with values ('value freedom') are unattainable in practice for the individual scientist, but rather that objectivity and freedom from such attachments are themselves *values.* And since value freedom itself is a value, the unconditional demand for freedom from any attachment to values is paradoxical. I do not regard this argument of mine as very important; but it should be noted that the paradox disappears quite of its own accord if we replace the demand for freedom from attachment to all values by the demand that it should be one of the tasks of scientific

criticism to point out confusions of value and to separate purely scientific value problems of truth, relevance, simplicity, and so forth, from extra-scientific problems.

I have so far attempted to develop briefly the thesis that the method of science consists in the choice of interesting problems and in the criticism of our always tentative and provisional attempts to solve them. And I have attempted to show further, using as my examples two much discussed questions of method in the social sciences, that this critical approach to methods (as it might be called) leads to quite reasonable methodological results. But although I have said a few words about epistemology, about the logic of knowledge, and a few critical words about the methodology of the social sciences, I have made so far only a small positive contribution to my topic, the logic of the social sciences.

I do not wish to detain you by giving reasons why I consider it important to identify scientific method, at least in first approximation, with the critical method. Instead, I should like now to move straight to some purely logical questions and theses.

Fifteenth thesis: The most important function of pure deductive logic is that of an organon of criticism.

Sixteenth thesis: Deductive logic is the theory of the validity of logical inferences or of the relation of logical consequence. A necessary and decisive condition for the validity of a logical consequence is the following: if the premisses of a valid inference are *true* then the conclusion must also be *true.*

This can also be expressed as follows. Deductive logic is the theory of the transmission of truth from the premisses to the conclusion.

Seventeenth thesis: We can say: if all the premisses are true and the inference is valid, then the conclusion *must* also be true; and if, consequently, the conclusion is false in a valid inference, then it is not possible that all the premisses are true.

This trivial but decisively important result can also be expressed in the following manner: deductive logic is not only the theory of the *transmission of truth* from the premisses to the conclusion, but it is also, at the same time, the theory of the *retransmission of falsity* from the conclusion to at least one of the premisses.

Eighteenth thesis: In this way deductive logic becomes the theory of rational criticism. For all rational criticism takes the form of an attempt to show that unacceptable conclusions can be derived

from the assertion we are trying to criticize. If we are successful in deriving, logically, unacceptable conclusions from an assertion, then the assertion may be taken to be refuted.

Nineteenth thesis: In the sciences we work with theories, that is to say, with deductive systems. There are two reasons for this. First, a theory or a deductive system is an attempt at explanation, and consequently an attempt to solve a scientific problem—a problem of explanation. Secondly, a theory, that is, a deductive system, can be criticized rationally through its consequences. It is, then, a tentative solution which is subject to rational criticism.

So much for formal logic as the organon of criticism.

Two fundamental ideas which I have used here require a brief elucidation: the idea of truth and the idea of explanation.

Twentieth thesis: The concept of truth is indispensable for the critical approach developed here. What we criticize is, precisely, the claim that a theory is true. What we attempt to demonstrate as critics of a theory is, clearly, that this claim is unfounded: that it is false.

The important methodological idea that *we can learn from our mistakes* cannot be understood without the regulative idea of truth: any mistake simply consists in a failure to live up to the standard of objective truth, which is our regulative idea. We term a proposition 'true' if it corresponds to the facts, or if things are as described by the proposition. This is what is called the absolute or objective concept of truth which each of us constantly uses. The successful rehabilitation of this absolute concept of truth is one of the most important results of modern logic.

This remark hints at the fact that the concept of truth had been undermined. Indeed, this was the driving force which produced the dominant relativistic ideologies of our time.

This is the reason why I am inclined to describe the rehabilitation of the concept of truth by the logician and mathematician Alfred Tarski as the philosophically most important result of mathematical logic.

I cannot of course discuss this result here; I can merely say quite dogmatically that Tarski succeeded, in the simplest and most convincing manner, in explaining wherein the agreement of a statement with the facts lies. But this was precisely the task whose apparently hopeless difficulty led to sceptical relativism—with social consequences which I do not need to spell out here.

The second concept which I have used and which may require elucidation is the idea of explanation or, more precisely, the idea of a *causal explanation.*

A purely theoretical problem—a problem of pure science—always consists in the task of finding an explanation, the explanation of a fact or of a phenomenon or of a remarkable regularity or of a remarkable exception from a rule. That which we hope to explain can be called the explicandum. The tentative solution of the problem—that is, the explanation—always consists of a theory, a deductive system, which permits us to explain the explicandum by connecting it logically with other facts (the so-called initial conditions). A completely explicit explanation always consists in pointing out the logical derivation (or the derivability) of the explicandum from the theory strengthened by some initial conditions.

Thus the basic logical schema of every explanation consists of a (logical) deductive inference whose premisses consist of a theory and some initial conditions,* and whose conclusion is the explicandum.

This basic schema has a remarkable number of applications. One can point out with its aid, for example, the distinction between an *ad-hoc* hypothesis and an independently testable hypothesis. Further—and this might be of more interest to you—one can analyse logically, in a simple manner, the distinction between theoretical problems, historical problems, and problems of applied science. Another result is that the famous *distinction* between theoretical or nomothetic and historical or ideographic sciences can be logically justified—provided one understands here under the term 'science' not merely 'natural science' (as in English) but any attempt to solve a definite, logically distinguishable, set of problems.

So much for the elucidation of the logical concepts which I have employed so far.

The two concepts under discussion, that of truth, and that of explanation, make possible the logical analysis of further concepts which are perhaps even more important for the logic of knowledge or methodology. The first of these concepts is that of

* (Note to the English edition.) In the social sciences, the premises of the explanation usually consist of a situational model and of the so-called 'rationality principle'. These 'explanations of situational logic' are briefly discussed in my twenty-fifth and twenty-sixth theses, below.

approximation to the truth and the second that of the *explanatory power* or the *explanatory content* of a theory.

These two concepts are purely logical concepts since they may be defined with the help of the purely logical concepts of the truth of a statement and of the content of a statement—that is, the class of the logical consequences of a deductive theory.

Both are relative concepts. Although each statement is simply true or false, nevertheless *one* statement can represent a better approximation to the truth than *another* statement. This will be so, for example, if the one statement has 'more' true and 'less' false logical consequences than the other. (It is presupposed here that the true and the false sub-sets of the set of consequences of the two statements are comparable.) It can then easily be shown why we rightly assume that Newton's theory is a better approximation to the truth than Kepler's. Similarly it can be shown that the explanatory power of Newton's theory is greater than Kepler's.

Thus we analyse here logical ideas which underlie the appraisal of our theories, and which permit us to speak meaningfully of progress or regress with reference to scientific theories.

So much for the general logic of knowledge. Concerning, in particular, the logic of the social sciences, I should like to formulate some further theses.

Twenty-first thesis: There is no such thing as a purely observational science; there are only sciences in which we theorize (more or less consciously and critically). This of course also holds for the social sciences.

Twenty-second thesis: Psychology is a social science since our thoughts and actions largely depend upon social conditions. Ideas such as (a) imitation, (b) language, (c) the family, are obviously social ideas; and it is clear that the psychology of learning and thinking, and also, for instance, psychoanalysis, cannot exist without utilizing one or other of these social ideas. Thus psychology presupposes social ideas; which shows that it is impossible to explain society exclusively in psychological terms, or to reduce it to psychology. Thus we cannot look upon psychology as the basis of the social sciences.

What we cannot, in principle, explain psychologically, and what we must presuppose in every psychological explanation, is man's social environment. The task of describing this social environment (that is, with the help of explanatory theories since—as stated before—theory-free descriptions do not exist) is the funda-

mental task of social science. It might well be appropriate to allot this task to sociology. I therefore assume this in what follows.

Twenty-third thesis: Sociology is autonomous in the sense that, to a considerable extent, it can and must make itself independent of psychology. Apart from the dependence of psychology on social ideas (mentioned in my twenty-second thesis), this is due to the important fact that sociology is constantly faced with the task of explaining unintended and often undesired consequences of human action. An example: competition is a social phenomenon which is usually undesirable for the competitors, but which can and must be explained as a (usually inevitable) unintended consequence of (conscious and planned) actions of the competitors. Thus even though we may be able to explain psychologically some of the actions of the competitors, the social phenomenon of competition is a psychologically inexplicable consequence of these actions.

Twenty-fourth thesis: But sociology is also autonomous in a second sense; that is, we cannot reduce to psychology what has often been termed *'verstehende Soziologie'* (the sociology of [objective*] understanding).

Twenty-fifth thesis: The logical investigation of economics culminates in a result which can be applied to all social sciences. This result shows that there exists *a purely objective method* in the social sciences which may well be called the method of *objective* understanding, or situational logic. A social science orientated towards objective understanding or situational logic can be developed independently of all subjective or psychological ideas. Its method consists in analysing the social *situation* of acting men sufficiently to explain the action with the help of the situation, without any further help from psychology. Objective understanding consists in realizing that the action was objectively *appropriate to the situation.* In other words, the situation is analysed far enough for the elements which initially appeared to be psychological (such as wishes, motives, memories, and associations) to be transformed into elements of the situation. The man with certain wishes therefore becomes a man whose situation may be characterized by the fact that he pursues certain objective *aims;* and a man with certain memories or associations becomes a man

* (Note to the English edition.) For a fuller discussion (including some examples) of an *objective* theory of understanding, see my paper 'On the Theory of the Objective Mind', which forms chapter 4 of my book *Objective Knowledge.*

whose situation can be characterized by the fact that he is equipped objectively with certain theories or with certain information.

This enables us then to understand actions in an objective sense so that we can say: admittedly I have different aims and I hold different theories (from, say, Charlemagne): but had I been placed in his situation thus analysed—where the situation includes goals and knowledge—then I, and presumably you too, would have acted in a similar way to him. The method of situational analysis is certainly an individualistic method and yet it is certainly not a psychological one; for it excludes, in principle, all psychological elements and replaces them with objective situational elements. I usually call it the 'logic of the situation' or 'situational logic'.

Twenty-sixth thesis: The explanations of situational logic described here are rational, theoretical reconstructions. They are oversimplified and overschematized and consequently in general *false.* Nevertheless, they can possess a considerable truth content and they can, in the strictly logical sense, be good approximations to the truth, and better than certain other testable explanations. In this sense, the logical concept of approximation to the truth is indispensable for a social science using the method of situational analysis. Above all, however, situational analysis is rational, empirically criticizable, and capable of improvement. For we may, for instance, find a letter which shows that the knowledge at the disposal of Charlemagne was different from what we assumed in our analysis. By contrast, psychological or characterological hypotheses are hardly ever criticizable by rational arguments.

Twenty-seventh thesis: In general, situational logic assumes a physical world in which we act. This world contains, for example, physical resources which are at our disposal and about which we know something, and physical barriers about which we also know something (often not very much). Beyond this, situational logic must also assume a social world, populated by other people, about whose goals we know something (often not very much), and, furthermore, *social institutions.* These social institutions determine the peculiarly social character of our social environment. These social institutions consist of all the social realities of the social world, realities which to some extent correspond to the things of the physical world. A grocer's shop or a university institute or a police force or a law are, in this sense, social institutions. Church,

state, and marriage are also social institutions, as are certain coercive customs like, for instance, harakiri in Japan. But in European society suicide is not a social institution in the sense in which I use the term, and in which I assert that the category is of importance.

That is my last thesis. What follows is a suggestion and a short concluding remark.

Suggestion: We may, perhaps, accept provisionally, as the fundamental problems of a purely theoretical sociology, the general situational logic of and the theory of institutions and traditions. This would include such problems as the following:

1. Institutions do not act; rather, only individuals act, in or for or through institutions. The general situational logic of these actions will be the theory of the quasi-actions of institutions.
2. We might construct a theory of intended or unintended institutional consequences of purposive action. This could also lead to a theory of the creation and development of institutions.

Finally, a further comment. I believe that epistemology is important not only for the individual sciences but also for philosophy, and that the religious and philosophical uneasiness of our time, which surely concerns us all, is, to a considerable degree, the result of uneasiness about the philosophy of human knowledge. Nietzsche called it the European nihilism and Benda the treason of the intellectuals. I should like to characterize it as a consequence of the Socratic discovery that we know nothing; that is, that we can never justify our theories rationally. But this important discovery which has produced, amongst many other malaises, the malaise of existentialism, is only half a discovery; and nihilism can be overcome. For although we cannot justify our theories rationally and cannot even prove that they are probable, we can criticize them rationally. And we can often distinguish better from worse theories.

But this was known, even before Socrates, to Xenophanes who told us*:

> The gods did not reveal from the beginning,
> All things to us; but in the course of time,
> Through seeking we may learn, and know things better . . .

* (Note to the English edition.) Cf. my *Conjectures and Refutations,* p. 152. (The translation is mine.)