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## Reason or Revolution?

Karl R. Popper

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KARL R. POPPER

## *Reason or Revolution?*

The trouble with a total revolution [...] Is that it brings the same class up on top: Executives of skilful execution Will therefore plan to go half-way and stop.

Robert Frost

(from "A Semi-Revolution", in *A Witness Tree*).

THE following critical considerations are reactions to the book, *Der Positivismusstreit in der deutschen Soziologie* (1), which was published last year and to which I gave unwittingly the original incentive.

### I

I will begin by telling some of the history of the book and of its misleading title. In 1960 I was invited to open a discussion on "The Logic of the Social Sciences" at a congress of German sociologists in Tübingen. I accepted, and I heard that my opening address would be followed by a reply from Professor Theodor W. Adorno of Frankfurt. It was suggested to me by the organizers that, in order to make a fruitful discussion possible, I should formulate my views in a number of definite theses. This I did: my opening address to that discussion, delivered in 1961, consisted of twenty-seven sharply formulated theses, plus a programmatic formulation of the task of the theoretical social sciences. Of course, I formulated these theses so as to make it difficult for any Hegelian and Marxist (such as Adorno) to accept them; and I supported them as well as I could by arguments. Owing to the limited time available, I confined myself to fundamentals, and I tried to avoid repeating what I had said elsewhere.

(1) H. MAUS and F. FÜRSTENBERG (eds), *Soziologie* (Berlin, Luchterhand, 1969). *Der Positivismusstreit in der deutschen*

Adorno's reply was read with great force, but he hardly took up my challenge—that is, my twenty-seven theses. In the ensuing debate Professor Ralf Dahrendorf expressed his grave disappointment. He said it had been the plan of the organizers to bring into the open some of the glaring differences— apparently he also meant political and ideological differences— between my approach to the social sciences and Adorno's. But the impression created by my address and Adorno's reply was that of sweet agreement, a fact which left him flabbergasted (“*als seien Herr Popper und Herr Adorno in verblüffender Weise einig*”). I was and I still am very sorry about this. But having been invited to speak about “The Logic of the Social Sciences”, I did not go out of my way to attack Adorno and the ‘dialectical’ school of Frankfurt (Adorno, Horkheimer, Habermas, *et al.*) which I never regarded as important unless perhaps from a political point of view. I had not known what was in the mind of the organizers; and in 1960 I was not even fully aware of the great influence of the Frankfurt school. Although today I should not hesitate to describe this influence by such terms as ‘irrationalist’ and ‘intelligence-destroying’, I could never take their ‘methodology’ (whatever that may mean) seriously from either an intellectual or a scholarly point of view. Knowing now a little more, I think that Dahrendorf was right in being disappointed: I should have gone out of my way to attack them, by arguments I had previously published in my *Open Society* (2) and *The Poverty of Historicism* (3) and in “What is Dialectic?” (4), even though I did not regard these arguments as ‘logic’; for words do not matter. My only comfort is that the blame for avoiding a fight rested with the second speaker.

However this may be, Dahrendorf's criticism stimulated a paper (almost twice as long as my original address) by Professor Jürgen Habermas, another member of the Frankfurt school. It was in this paper, I think, that the term ‘positivism’ first turned up in this particular discussion: I was criticized as a ‘positivist’. This is an old misunderstanding created and perpetuated by people who know of my work only at second-hand: owing to the tolerance of some members of the Vienna Circle, my book *Logik der Forschung* (5) in which I criticized the Circle from a realist and anti-positivist point of view

(2) K. R. POPPER, *The Open Society and its Enemies* (London 1945), fifth edition 1969.

(3) *Id.* *The Poverty of Historicism* (London 1957, and later editions).

(4) *Id.* What is Dialectic?, *Mind*, XLIX

(1940), pp. 403 sq. Reprinted in *Conjectures and Refutations* (London 1963), third edition 1969.

(5) (Wien, Julius Springer, 1934). English translation: *The Logic of Scientific Discovery* (London, Hutchinson, 1959).

was published in a series of books edited by Moritz Schlick and Philipp Frank, two leading members of the Circle. Thus the myth was born by those who judge books from their outside (or by their editors) that I had been a member of the Vienna Circle, and a positivist. Nobody who has read that book (or any other book of mine) would agree—unless indeed he believed in the myth to start with, in which case he may of course find ample evidence to support his belief.

In my defence, Professor Hans Albert (not a positivist either) wrote a spirited reply to Habermas's attack. The latter answered, and was answered a second time by Albert. This exchange was mainly concerned with the general character and tenability of my views. Thus there was little mention—and no serious criticism—of my opening address of 1961 and its twenty-seven theses.

It was, I think, in 1964 that a German publisher asked me whether I would agree to have my address published in book form together with Adorno's reply and the debate between Habermas and Albert. I agreed.

But, as now published, the book consists of two quite new introductions by Adorno (100 pages), followed by my address of 1961 (20 pages) with Adorno's original reply (19 pages). Dahrendorf's excellent complaint (9 pages), the debate between Habermas and Albert (150 pages), a new contribution by Harold Pilot, and a "Short and Astonished Postscript to a Long Introduction" by Albert (5 pages). Albert mentions briefly that the affair started with a discussion between Adorno and myself in 1961, and he says quite rightly that a reader of the book would hardly realise what it was all about. This is the only allusion in the book to the story behind it. There is no answer to the question of how the book got a title which quite wrongly indicates that the opinions of some 'positivists' are discussed in the book. Even Albert's postscript does not answer this question.

What is the result? My twenty-seven theses, intended to start a discussion, are nowhere seriously discussed in this longish book—not a single one of them, although one or other passage of my address is mentioned here or there, usually out of context, and to illustrate my 'positivism'. Moreover, my address is buried in the middle of the book, unconnected with the beginning and the end. No reader can see, and no reviewer can understand, why my address (which I cannot but regard as quite unsatisfactory in its present setting) is included in the book—or that it is the unadmitted theme of the whole book. Thus no reader would suspect, and no reviewer did suspect, what I suspect as being the truth of the matter. It is that my opponents

literally did not know how to criticize rationally my twenty-seven theses. All they could do was to label me 'positivist' (thereby unwittingly giving a highly misleading name to a debate in which no 'positivist' was involved); and having done so, they drowned my short paper, and the original issue of the debate, in an ocean of words.

As it now stands, the main issue of the book has become Adorno's and Habermas's accusation that a 'positivist' like Popper is bound by his methodology to defend the political *status quo*. It is an accusation which I myself raised in my *Open Society* against Hegel, whose identity philosophy (what is real is reasonable) I described as a "moral and legal positivism". In my address I had said nothing about this issue; and I had no opportunity to reply. But I have often combatted this form of 'positivism' along with other forms. *And it is a fact that my own social theory, which favours gradual and piecemeal reform, strongly contrasts with my theory of method, which happens to be a theory of scientific and intellectual revolution.*

## 2

This fact and my attitude towards revolution can be easily explained. We may start from Darwinian evolution. Organisms evolve by trial and error, and their erroneous trials—their erroneous mutations—are eliminated, as a rule, by the elimination of the organism which is the 'carrier' of the error. It is part of my epistemology that, in man, through the evolution of a descriptive and argumentative language, all this has changed radically. Man has achieved the possibility of being *critical of his own tentative trials, of his own theories*. These theories are no longer incorporated in his organism, or in his genetic system: they may be formulated in books, or in journals; and they can be critically discussed, and shown to be erroneous, without killing the author or other 'carriers'.

In this way we arrive at a fundamental new possibility: our trials, our tentative hypotheses may be critically eliminated, by rational discussion, without eliminating ourselves.

This indeed is the purpose of rational critical discussion. The 'carrier' of a hypothesis has an important function in these discussions: he is to defend the hypothesis against erroneous criticism, and perhaps to try to modify it if in its original form it cannot be successfully defended.

If the method of rational critical discussion should establish itself,

then this will make the use of violence obsolete : reason is the only alternative to violence so far discovered.

It seems to me clear that it is the obvious duty of all intellectuals to work for this revolution— for the replacement of the eliminative function of violence by the eliminative function of rational criticism. But in order to work for this end, one has to train oneself constantly to write and speak in clear and simple language. Every thought should be formulated as clearly and simply as possible. This can only be achieved by hard work.

## 3

I have been for many years a critic of the so-called “sociology of knowledge”. Not that I thought that everything that Mannheim (and Scheler) said was mistaken. On the contrary, much of it was only too trivially true. What I combatted, mainly, was Mannheim’s belief that there was an essential difference with respect to objectivity between the social scientist and the natural scientist, or between the study of society and the study of nature. The thesis I combatted was that it was easy to be ‘objective’ in the natural science, while objectivity in the social sciences could be achieved, if at all, only by very select intellects : by the “freely poised intelligence” which is only “loosely anchored in social traditions” (6).

As against this I stressed that the objectivity of natural and social science was not based on an impartial state of mind in the scientists, but merely on the fact of the public and competitive character of the scientific enterprise, and thus on certain social aspects of it. This is why I wrote: “*What the [so-called] “sociology of knowledge” overlooks is just the sociology of knowledge—the social or public character of science*” (7). Objectivity is based, in brief, upon *mutual rational criticism*, upon the critical approach, the critical tradition (8).

Thus natural scientists are not more objectively minded than social scientists. Nor are they more critical. If there is more ‘objectivity’ in the natural sciences, then this is because there is a better tradition, and higher standards, of clarity and of rational criticism.

In Germany, many social scientists are brought up as Hegelians, and this is, in my opinion, a tradition destructive of intelligence and critical thought. It is one of the points where I agree with Karl

(6) *The Open Society...*, *op. cit.* (1969), II, p. 215.

(7) *The Poverty...*, *op. cit.* (1957), p. 155.

(8) *Conjectures...*, *op. cit.* (1969), especially chapter IV.

Marx who wrote: "In its mystifying form dialectic became the accepted German fashion" (9). It is the German fashion still.

## 4

The sociological explanation of this fact is simple. We all get our values, or most of them, from our social environment; often merely by imitation, by a simple process of taking over; sometimes by a revolutionary reaction to the accepted values; and at other times—though rarely—by a critical examination of these values and of possible alternatives. All this is rather obvious. A very special case, but one which is all-important for our purpose is that of intellectual values.

Many years ago I used to warn my students against the widespread idea that one goes to the University in order to learn how to talk, and to write, impressively and un-understandably. Many students did then come to the University with this ridiculous aim in mind, especially in Germany. And many students who, during their University studies, enter into an atmosphere like this—who come, perhaps, under the influence of teachers who in their turn had been brought up in a similar way—are lost. They learn to think—unconsciously—that highly impressive and difficult language is the intellectual value *par excellence*. There is little hope that they will ever understand that they are mistaken; that they will realize that there are other values: truth, the search for truth, the approximation to truth through the critical elimination of error. All I have said here would be for them, *at best*, impressive talk: they do not know any other values.

But what about un-understandability? The cult of impressive and high sounding language was greatly intensified by the, for laymen impenetrable, formalism of mathematics. My thesis is that in some of the more ambitious social sciences and philosophies, and especially in Germany, it is the traditional game, which has largely become the unconscious and unquestioned standard, to state the utmost trivialities in highbrow language. This is what our intelligence exists for. (When I was a young man, a friend advised me to read *Ideology and Utopia*. My judgement was: there is a royal road to success in what calls itself 'philosophy' and 'sociology': say the utmost trivialities in high sounding language; or, alternatively, talk erudite nonsense with truisms interspersed. Then the reader will be flattered; for

(9) Karl MARX, *Das Kapital*, 2. Aufl., this is described as "Preface to second edition", 1872, »Nachwort«. (In some later editions,

not only can he understand parts of such a difficult book but he can even find ideas in it of which he himself has thought before).

If those who have been brought up on this kind of nourishment are presented with a book which is written simply but contains new thought, then they find it difficult or impossible to understand it. For it does not conform to their idea of 'understanding', which for them entails agreement. That there may be important ideas worth understanding with which one cannot at once agree or disagree is to them un-understandable.

## 5

There is here, at first sight, a difference between the social sciences and the natural sciences: in the so-called social sciences and in philosophy, the degeneration into impressive but more or less empty verbalism has gone farther than in the natural sciences. Yet the danger is getting acute everywhere. I assert that even in mathematics there are tendencies to impress people, although the incitement to do so is least there; for it is partly the wish to outdo the mathematicians and the mathematical physicists in technicality and in difficulty which inspires the verbiage of other sciences.

Yet lack of critical creativeness—that is, of inventiveness paired with critical acumen—can be found everywhere; and everywhere this leads to the phenomenon of young scientists eager to pick up the latest fashion and the latest jargon. These 'normal' scientists (10) want a framework, a routine, a common and an exclusive language of their trade. But it is the non-normal scientist, the daring scientist, the critical scientist, who breaks through the barrier of normality, who opens the windows and lets in fresh air; who does not think about the impression he makes, but tries to be well understood.

The growth of normal science, which is linked to the growth of 'big' science, is likely to prevent, or even to destroy, the growth of knowledge, the growth of great science.

I regard the situation as tragic if not desperate; and the present trend in the so-called empirical investigations into the sociology of the natural sciences is likely to contribute to the decay of science.

(10) The phenomenon of normal science was discovered, but not criticized, by Thomas KUHN in *The Structure of Scientific Revolutions*. Kuhn is, I believe, mistaken in thinking that 'normal' science is not

only normal *today* but always was so. On the contrary, in the past—until 1939—science was almost always critical, or 'extraordinary'; there was no scientific 'routine'.



Superimposed upon this danger is another danger, created by 'big' science: its urgent need for scientific technicians. More and more Ph. D. candidates receive a merely technical training, a training in certain techniques of measurement; they are not initiated into the scientific tradition, the critical tradition of questioning, of being tempted and guided by great and apparently insoluble riddles rather than by the solubility of little puzzles. True, these technicians, these specialists, are usually aware of their limitations. They call themselves specialists and reject any claim to authority outside their specialism. Yet already they do so proudly, and proclaim that specialism is a necessity. But this means flying in the face of the facts which show that all the great advances still come from those with a wide range of interests.

If the many, the specialists, gain the day, it will be the end of science as we know it—of great science. It will be a spiritual catastrophe comparable in its consequences to nuclear armament.

6

I now come to one of my main points. It is this. Some of the great and famous leaders of German sociology who do their intellectual best, and do it with the best conscience in the world, are simple talking trivialities in this high-sounding language, as they were taught. They teach this to their students, who are dissatisfied, yet who do the same. (In fact, the genuine and general feeling of dissatisfaction which is manifest in their hostility to the society in which they live is, I think, a reflection of their unconscious dissatisfaction with the sterility of their own activities).

I will give a brief example: the quotation is from Professor Adorno. The example is a select one, but it is selected by Professor Habermas who begins with it his own contribution to *Der Positivismusstreit* (p. 155). I give on the left the original German text and on the right a translation into modest and unimpressive German and English:

Die gesellschaftliche Totalität führt kein Eigenleben oberhalb des von ihr Zusammengefassten, aus dem sie selbst besteht. Sie produziert und reproduziert sich durch ihre einzelnen Momente hindurch.

Die Gesellschaft besteht aus den gesellschaftlichen Beziehungen.

Die verschiedenen Beziehungen produzieren *irgendwie* die Gesellschaft.

Society consists of social relationships.

The various social relationships somehow produce society.

So wenig jenes Ganze vom Leben, von der Kooperation und dem Antagonismus des Einzelnen abzusondern ist,

so wenig kann irgend ein Element auch bloss in seinem Funktionieren verstanden werden ohne Einsicht in des Ganze, das an der Bewegung des Einzelnen selbst sein Wesen hat.

System und Einzelheit sind reziprok und nur in der Reziprozität zu verstehen.

Unter diesen Beziehungen finden sich Kooperation und Antagonismus; und da (wie schon gesagt) die Gesellschaft aus diesen Beziehungen besteht, kann sie von ihnen nicht abgesondert werden;

aber das Umgekehrte gilt auch: keine der Beziehungen kann ohne die anderen verstanden werden.

(Wiederholung des Vorhergehenden.)

Among these relations are cooperation and antagonism; and since (as mentioned) society consists of these relations, it is impossible to separate it from them.

The opposite is also true: none of the relations can be understood without the totality of all the others.

(Repetition of the preceding thought.)

Comment: the theory developed here of social wholes has been presented and developed by uncounted philosophers and sociologists, sometimes better and sometimes worse. I do not assert that it is mistaken. I only assert the complete triviality of its content. Of course the *presentation* is far from trivial.

It is for reasons such as these that I find it so difficult to discuss any serious problem with Professor Habermas. I am sure he is perfectly sincere. But I think that he does not know how to put things simply, clearly and modestly, rather than impressively. Most of what he says seems to me trivial; the rest seems to me mistaken.

So far as I can understand him, the following is his central complaint about my alleged views. My way of theorizing, Habermas suggests, violates the *principle of the identity of theory and practice* of the Marxists and the sociologists of knowledge; perhaps because I say that theory should *help* action, that is, should help us to modify our actions. For I say that it is the task of the theoretical social sciences to try to anticipate the unintended consequences of our actions; thus I differentiate between this theoretical task and the action. But Professor Habermas seems to think that only one who is a practical critic of the existing society can produce serious theoretical arguments about society, since social knowledge cannot be divorced from fundamental

social attitudes. The indebtedness of this view to the "sociology of knowledge" is obvious, and need not be laboured.

My reply is very simple. I welcome any contribution by any critic of society, however revolutionary in his attitude he may be, if he has learned to express himself simply, clearly, and with intellectual modesty and unpretentiousness, aware of our fundamental ignorance and of our responsibilities to others. But I certainly do not think that the debate about the reform of society should be reserved to those who first put in a claim to be recognized as practical revolutionaries.

It may be that revolutionaries have a greater sensitivity to social ills than other people. But obviously, there can be better and worse revolutions (as we all know from history), and the problem is not to do too badly. Most, if not all, revolutions have produced societies very different from those desired by the revolutionaries. *Here is a problem*, and it deserves thought from every serious critic of society. And by thought I mean an effort to put one's ideas into simple, modest language, rather than high-sounding jargon. This is an effort which those who are fortunate enough to be able to study owe to society.

## 8

A last word on the term 'positivism'. Words do not matter, and I do not really mind if even a thoroughly misleading and mistaken label is applied to me. But the fact is that throughout my life I have combatted the positivist epistemology, under the name 'positivism'. I do not deny, of course, the possibility of stretching the term 'positivism' until it can be applied even to an opponent of positivism such as myself. I only contend that such a procedure is neither honest nor apt to clarify matters.

The fact that the label 'positivism' was originally applied to me by a sheer blunder can be checked by anybody who undertakes the task of reading my early *Logik der Forschung* (11).

It is, however, worth mentioning that one of the victims of the misnomers 'positivism' and "*Der Positivismusstreit*" is Dr. Alfred Schmidt, who describes himself as a "collaborator of many years standing" (*Langjähriger Mitarbeiter*) of the Professors Adorno and Horkheimer. In a letter to the newspaper *Die Zeit* (12), written to

(11) *Op. cit.*

(12) 12th June 1970, p. 45.

defend Adorno against the suggestion that he misused the term 'positivism' in *Der Positivismusstreit* or on similar occasions, Schmidt characterizes 'positivism' as a tendency of thought in which "the method of the various single sciences is taken absolutely as the only valid method of knowledge" (*die einzelwissenschaftlichen Verfahren als einzig gültige Erkenntnis verabsolutierende Denken*), and he identifies it, correctly, with an over-emphasis on 'sensually ascertainable facts'.

He is clearly quite unaware of the fact that my alleged 'positivism', which was used to give the book *Der Positivismusstreit* its name, consisted in a fight against all this which he describes (in my opinion fairly correctly) as 'positivism'. I have always been fighting for the right to operate freely with speculative theories, against the narrowness of the 'scientific' theories of knowledge and, especially, against all forms of sensualistic empiricism.

I have fought against the aping of the natural sciences by the social sciences, and I have fought for the doctrine that the positivistic epistemology is inadequate even in its analysis of the natural sciences which, in fact, are not carefully generalising from observation as is usually believed, but are essentially speculative and daring; moreover, I have taught, for more than 38 years (13), that all observations are theory-impregnated, and that their main function is to check and refute, rather than to prove, our theories. Finally I have not only stressed the meaningfulness of metaphysical assertions and the fact that I am myself a metaphysical realist, but I have also analysed the important historical role played by metaphysics in the formation of scientific theories. Nobody before Adorno and Habermas has described such views as positivistic, and these two did not know originally that I held such views. In fact, they were no more interested in my views than I am in theirs.

Terminology does not matter, however. Only it should not be used as an *argument*; and the title of a book ought not to be dishonest; nor should it attempt to prejudge an issue.

On the substantial issue between the Frankfurt school and myself—revolution *versus* piecemeal reform—I shall not comment here since I have treated it as simply and clearly as I could in my *Open Society*. Hans Albert too has said many important things on this topic, both in his replies to Habermas in *Der Positivismusstreit* and in his incisive work, *Traktat über die kritische Vernunft* (14).

(13) See my book, *The Logic of Scientific Discovery*, *op. cit.* (1959), new appendix 1.

(14) H. ALBERT, *Traktat über die kritische Vernunft*<sup>2</sup> (Tübingen, Mohr, 1969).