

THE MYTH OF THE FRAMEWORK

- 24 See my 'Quantum Mechanics without "The Observer"', in *Studies in the Foundations, Methodology and Philosophy of Science*, volume II: *Quantum Theory and Reality*, edited by Mario Bunge, Springer-Verlag, New York, 1967. (A revised version of this article now forms a chapter in volume III of my *Postscript to the Logic of Scientific Discovery*; see my *Quantum Theory and the Schism in Physics*, edited by W.W. Bartley, III, Hutchinson, London, 1982.)
- 25 See my *Open Society*, chapter 11, section II, or my 'Quantum Mechanics without "The Observer"', especially pp. 11–15, or my *Conjectures and Refutations*, pp. 19, 28 (section 9), 279, and 402.

FROM "THE MYTH OF THE
FRAMEWORK"³ ed MA NUTTURNO
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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 halfway and stop.

Robert Frost

The following critical considerations are reactions to the book, *Der Positivismusstreit in der deutschen Soziologie*,¹ which was published in 1969, and for which I unwittingly provided 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 was told that my opening address would

This paper came into being as a result of a suggestion by Professor Raymond Aron. My paper 'The Logic of the Social Sciences' was first published in Germany as the third paper of a collection misnamed *Der Positivismusstreit in der deutschen Soziologie* (see note 1, below), in a manner which left it unexplained that it was the paper which had unwittingly sparked off this 'Positivismusstreit'. (For a revised version of 'The Logic of the Social Sciences', see my *In Search of a Better World*, Routledge, London, 1992.) In 1970 I wrote a letter to the *Times Literary Supplement* ('Dialectical Methodology', *TLS* 69, 26 March 1970, pp. 388–9) in criticism of a review of the *Positivismusstreit* volume that had appeared there. Professor Aron suggested that I expand this letter, and explain my objections to the volume more fully. This I did in the present paper, which was first published in *Archives européennes de sociologie*, 11, 1970, pp. 252–62, and which is also appended to the English translation of the *Positivismusstreit* volume. (See Theodor W. Adorno, *et al.*, eds, *The Positivist Dispute in German Sociology*, translated by Glyn Adey and David Frisby, Harper & Row, 1976.) The motto is from Robert Frost, 'A Semi-Revolution', in *A Witness Tree*.

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 or 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.

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 that it had been the intention of the organizers to bring into the open some of the glaring differences – apparently he included 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, he said, one of sweet agreement – a fact which left him flabbergasted (*'als seien Herr Popper und Herr Adorno sich 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 was not aware of the organizers' intention, and in 1960 I was not even aware of the political influence of this school. Although today I would 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 ought to have attacked them using arguments I had previously published in my *Open Society* and *The Poverty of Historicism* and in 'What is Dialectic?',² even though I do not think that these arguments fall under the heading of 'The Logic of the Social Sciences' – for terms do not matter. My only comfort is that the responsibility for avoiding a fight rests squarely on 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 tolerant attitude adopted by some members of the Vienna Circle, my book, *Logik der Forschung*, in which I criticized this positivist Circle from a realist and anti-positivist point of view, was published in a series of books edited by Moritz Schlick and Philipp Frank, two leading members of the Circle.³ And those who judge books by their covers (or by their editors) created the myth 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 evidence to support his belief.

In my defence Professor Hans Albert (not a positivist either) wrote a spirited reply to Habermas' attack. The latter answered, and was rebutted 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 of 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 (in 1969, in German), the book consists of two quite new introductions by Adorno (94 pages), followed by my address of 1961 (20 pages) with Adorno's original reply (18 pages), Dahrendorf's complaint (9 pages), the debate between Habermas and Albert (150 pages), a new contribution by Harold Pilot (28 pages), and a 'Short Surprised Postscript to a Long Introduction' by Albert (5 pages). In this, 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 realize 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 it. Even Albert's postscript does not answer the question.

What is the result? My twenty-seven theses, intended to start a discussion (and so they did, after all), are nowhere seriously taken up

in this longish book – not a single one of them, although one or another passage from my address is mentioned here or there, usually out of context, 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 not a single ‘positivist’ was involved). And having done so, they drowned my short paper, and the original issue of the debate, in an ocean of words – which I found only partially comprehensible.

As it now stands, the main issue of the book has become Adorno’s and Habermas’ 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 ‘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 combated this form of ‘positivism’ along with other forms. And it is a fact that my *social theory* (which favours gradual and piecemeal reform, reform controlled by a critical comparison between expected and achieved results) contrasts with my *theory of method*, which happens to be a theory of scientific and intellectual revolution.

II

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 that 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 any authors or burning books – without destroying the ‘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 has to defend the hypothesis against erroneous criticism, and he may perhaps 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 should make the use of violence obsolete. *For critical reason is the only alternative to violence so far discovered*.

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 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 be achieved only by hard work.

III

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 combated 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 combated was that it was easy to be objective in the natural sciences, 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’.⁴

As against this I stressed that the objectivity of natural and social science is 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 “sociology of knowledge” overlooks is just the sociology of knowledge – the social or public character of*

science.⁵ Objectivity is based, in brief, upon *mutual rational criticism*, upon the critical approach, the critical tradition.⁶

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, 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 a tradition destructive of intelligence and critical thought. It is one of the points where I agree with Karl Marx who wrote: 'In its mystifying form the Dialectic became the ruling fashion in Germany'.⁷ It is the German fashion still.

IV

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 (simply by taking them over from others), sometimes by a revolutionary reaction to accepted values, and at other times – though this may be rare – by a critical examination of these values and of possible alternatives. However this may be, the social and intellectual climate, the tradition in which one is brought up, is often decisive for the moral and other standards and values one adopts. All this is rather obvious. A very special case, but 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 college in order to learn how to talk and write 'impressively' and incomprehensibly. At the time many students came to college with this ridiculous aim in mind, especially in Germany. And most of those students who, during their university studies, enter into an intellectual climate that accepts this kind of valuation – coming, perhaps, under the influence of teachers who in their turn had been reared in a similar climate – are lost. They unconsciously learn and accept that highly obscure and difficult language is the intellectual value *par excellence*. There is little hope that they will even understand that they are mistaken, or that they will ever realize that there are other standards and values – values such as truth, the search for truth, the approximation to truth through the critical elimination of error, and clarity. Nor will they find out that the standard of 'impressive' obscurity actually clashes with the standards of truth and rational criticism. For these latter values depend on clarity. One cannot tell truth from falsity, one

cannot tell an adequate answer to a problem from an irrelevant one, one cannot tell good ideas from trite ones, and one cannot evaluate ideas critically – unless they are presented with sufficient clarity. But to those brought up in the implicit admiration of brilliance and 'impressive' opaqueness, all this (and all I have said here) would be *at best*, 'impressive' talk: they do not know any other values.

Thus arose the cult of incomprehensibility, of 'impressive' and high-sounding language. This was intensified by the (for laymen) impenetrable and impressive formalism of mathematics. I suggest that in some of the more ambitious social sciences and philosophies, especially in Germany, the traditional game, which has largely become the unconscious and unquestioned standard, is to state the utmost trivialities in high-sounding language.

If those brought up on this kind of nourishment are presented with a book that is written simply and contains something unexpected, controversial, or new, they usually find it difficult or impossible to understand. 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 unfathomable.

V

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 further than in the natural sciences. Yet the danger is getting acute everywhere. Even among mathematicians a tendency to impress people may sometimes be discerned, although the incitement to do so is least here. For it is partly the wish to ape the mathematicians and the mathematical physicists in technicality and in difficulty that inspires the use of verbiage in 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' scientists⁸ want a framework, a routine, a common and 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.

The situation is 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. Superimposed upon this danger is another danger created by Big Science: its urgent need for scientific technicians. More and more Ph.D. candidates receive merely technical 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 specialities. Yet they do so proudly, and proclaim that specialization is a necessity. But this means flying in the face of the facts, which show that 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.

VI

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

I will give a brief example from the writings of Professor Adorno. The example is a select one – selected, indeed, by Professor Habermas, who begins his first contribution to *Der Positivismusstreit* by quoting it. On the left I give the original German text, in the centre this text as translated in the present volume, and on the right a paraphrase into simple English of what seems to be being asserted.⁹

3. REASON OR REVOLUTION?

Die gesellschaftliche Totalität führt kein Eigenleben oberhalb des von ihr Zusammengefassten, aus dem sie selbst besteht.	Societal totality does not lead a life of its own over and above that which it unites and of which it, in its turn is composed.	Society consists of social relationships.
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Sie produziert und reproduziert sich durch ihre einzelnen Momente hindurch. ...	It produces and reproduces itself through its individual moments. ...	The various social relationships somehow produce society. ...
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So wenig aber jenes Ganze vom Leben, von der Kooperation und dem Antagonismus seiner Elemente abzusehen ist,	This totality can no more be detached from life, from the cooperation and the antagonism of its elements,	Among these relations are cooperation and antagonism; and since society consists of these relations, it is impossible to separate it from them.
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so wenig kann irgendein Element auch bloss in seinem Funktionieren verstanden werden ohne Einsicht in des Ganzen, das an der Bewegung des Einzelnen selbst sein Wesen hat.	than can an element be understood merely as it functions without insight into the whole which has its source (<i>Wesen</i> , essence) in the motion of the individual entity itself.	The opposite is also true: none of the relations can be understood without the totality of all the others.
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System und Einzelheit sind reziprok und nur in ihre Reziprozität zu erkennen.	System and individual entity are reciprocal and can only be apprehended in their reciprocity.	(Repetition of the preceding thought.)
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Comment: The theory of the social wholes developed here has been presented and developed, sometimes better and sometimes worse, by countless philosophers and sociologists. I do not assert that it is mistaken. I only assert the complete triviality of its content. Of course Adorno's *presentation* is very far from trivial.

VII

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 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* – 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 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 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. We should welcome any suggestion as to how our problems might be solved, regardless of the attitude towards society of the man who puts them forward: provided that he has learned to express himself clearly and simply – in a way that can be understood and evaluated – and that he is aware of our fundamental ignorance and responsibilities towards others. But I do not think that the debate about the reform of society should be reserved for those who first put in a claim for recognition as practical revolutionaries, and who see the sole function of the revolutionary intellectual in pointing out as much as possible what is repulsive in our social life (excepting their own social roles).

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 this should include an effort to put one's ideas into simple, modest language, rather than high-sounding jargon. This is an effort which those fortunate ones who are able to devote themselves to study owe to society.

VIII

A last word about 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 combated positivist epistemology, under the name 'positivism'. I do not deny, of course, the possibility of stretching the term 'positivist' until it covers anybody who takes any interest in natural science, so that it can be applied even to opponents 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 sheer blunder can be checked by anybody who is prepared to read my early *Logik der Forschung*.

It is, however, worth mentioning that one of the victims of the two misnomers, 'positivism' and '*Der Positivismusstreit*' is Dr Alfred Schmidt, who describes himself as a 'collaborator of many years' standing (*langjähriger Mitarbeiter*) of Professors Adorno and Horkheimer. In a letter to a newspaper *Die Zeit*,¹⁰ written to 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 unaware of the fact that my alleged positivism, which was used to give the book *Der Positivismusstreit* its name, consisted of a fight against all this, which he describes (fairly correctly) as 'positivism'. I have always fought 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 positivistic epistemology is inadequate even in its analysis of the natural sciences, which, in fact, are not 'careful generalizations from observation', as is usually believed, but are essentially speculative and daring. Moreover, I have taught, for more than thirty-eight years,¹¹ 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 I can only suppose that these two did not know, originally, that I held such views. (In fact, I suspect that they were no more interested in my views than I am in theirs.)

The suggestion that anybody interested in natural science is to be condemned as a positivist would make positivists not only of Marx and Engels, but also of Lenin – the man who introduced the equation of 'positivism' and 'reaction'.

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 well as I could in my *Open Society*. Hans Albert too has said many incisive things on this topic, both in his replies to Habermas in *Der Positivismusstreit* and in his important book *Traktat über kritische Vernunft*.¹²

NOTES

- 1 H. Maus and F. Fürstenberg, eds, *Der Positivismusstreit in der deutschen Soziologie*, Luchterhand, Berlin, 1969.
- 2 'What is Dialectic?', *Mind*, XLIX, 1940, pp. 403ff. Reprinted in *Conjectures and Refutations*.
- 3 The Vienna Circle consisted of men of originality and of the highest intellectual and moral standards. Not all of them were positivists, even if we mean by this term no more than a condemnation of speculative thought, although most of them were. I have always been in favour of criticizable speculative thought and, of course, of its criticism.
- 4 The quotation is from Mannheim. It is discussed more fully in my *Open Society*, volume II, p. 215.
- 5 *The Poverty of Historicism*, p. 155.
- 6 Cp. my *Conjectures and Refutations*, especially chapter 4.
- 7 Karl Marx, *Capital*, volume II, 1872, 'Nachwort'. (In some later editions this is described as 'Preface to second edition'. The usual translation is not 'mystifying' but 'mystified'. To me this sounds like a Germanism.)
- 8 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'.
- 9 In the original publication of this article in *Archives européennes de sociologie* the three columns contained, respectively, the original German, a paraphrase into simple German of what seemed to be being

asserted, and a translation of this paraphrase into English.

10 12 June 1970, p. 45.

11 See my *Logic of Scientific Discovery*, new appendix *x.

12 Hans Albert, *Traktat über kritische Vernunft*, J.C.B. Mohr, Tübingen, 1969.

ADDENDUM 1974: THE FRANKFURT SCHOOL

I first heard of the Frankfurt School in the 1930s but decided then, on the basis of some experimental reading, against conscientiously reading its output.

In 1960, as recounted in my 'Reason or Revolution?', I was asked to open a discussion at a conference in Tübingen, and was told that Adorno would reply to my paper. This led me to another attempt at reading the publications of the Frankfurt School and especially Adorno's books.

Most of Adorno's works may be divided into three groups. First, there are his essays on music, literature, or culture. These I found little to my taste. To me they read like imitations of Karl Kraus, the Viennese writer – bad imitations, because they lacked Kraus' sense of humour. I had known, and heartily disliked, this kind of writing in my days in Vienna. I used to think of it as cultural snobbery, practised by a clique which regarded itself as a cultural *élite*. These essays, incidentally, are characterized by their social irrelevance.

Then there was a second group of books, on epistemology or philosophy. And these seemed just the sort of thing one calls in English 'mumbo-jumbo' (or in German *Hokuspokus*).

Of course, Adorno was a Hegelian as well as a Marxist. And I am opposed to both: to Marxism and especially to Hegelianism.

As to Marx, I have great respect for him as a thinker and as a fighter for a better world, though I disagree with him on many points of decisive importance. I have criticized his theories at considerable length. He is not always particularly easy to understand, but he always tries his best to be understandable. For he has something to say, and he wants people to understand him. But as to Adorno, I can neither agree nor disagree with most of his philosophy. In spite of all efforts to understand his philosophy, it seems to me that all of it, or almost all, is just words. He has nothing whatever to say, and he says it in Hegelian language.

But there is a third group of his writings. The essays which belong

In December 1973 I was asked by the BBC if I would agree to be interviewed on the subject of the so-called 'Frankfurt School' for use in a programme on their work to be broadcast in January 1974. I prepared a brief paper (which was not, in fact, broadcast in this form because I was given only five minutes in which to speak) containing a few critical remarks I make in section I of my paper 'Reason or Revolution' about my attitude towards the Frankfurt School.

to this third group are mainly complaints about the times we live in. But some of them are interesting and even moving. They give direct expression to his fears: to his anxiety, as he calls it himself, and to his deep depression. Adorno was a pessimist. After Hitler came to power – an event which, he says, surprised him as a politician – he despaired of mankind, and he surrendered his belief in the Marxist gospel of salvation. It is a voice of utmost despair which sounds from these essays – a tragic and pitiful voice.

But so far as Adorno's pessimism is philosophical, its philosophical content is nil. Adorno is consciously opposed to clarity. Somewhere he even mentions with approval that the German philosopher Max Scheler asked for 'more darkness' (*mehr Dunkel*), alluding to the last words of Goethe, who asked for 'more light' (*mehr Licht*).

It is difficult to understand how a Marxist like Adorno could support a demand for more darkness. Marx, certainly, was for enlightenment. But Adorno has published, together with Horkheimer, a book under the title *Dialectic of Enlightenment*¹ in which they try to show that the very idea of enlightenment leads, by its inner contradictions, into darkness – the darkness which we are allegedly in now. This is, of course, a Hegelian idea. Nevertheless, it remains a puzzle how a socialist, or a Marxist, or a humanist, like Adorno, can revert to such Romantic views, and prefer the maxim 'more darkness' to 'more light'. Adorno acted on his maxim by publishing intentionally obscure and even oracular writings. It can only be explained by the nineteenth-century tradition of German philosophy, and by the 'Rise of Oracular Philosophy', as I call it in my *Open Society* – the rise of the school of the so-called German Idealists. Marx himself was brought up in this tradition, but he reacted forcefully against it, and in *Capital* he made a remark about it, and about Dialectic, that I always admired. Marx said in *Capital*: 'In its mystifying form the Dialectic became the ruling fashion in Germany.'² Dialectic is still the ruling fashion in Germany. And it is still 'in its mystifying form'.

But I would like also to say a few words about Horkheimer. Compared with Adorno, his writings are lucidity itself. But Horkheimer's so-called 'Critical Theory' is empty – devoid of content. This is more or less admitted by the editor of Horkheimer's *Kritische Theorie*, when he says: 'To cast Horkheimer's conception into the form of understandable ('*eingängige*') propositions is ... almost impossible'.³ There remains only a vague and unoriginal

Marxian historicism: Horkheimer does not say anything tenable that has not been said better before. His views may be said to be objectively uninteresting, including those with which I can agree.

For I have found in Horkheimer some propositions with which I can agree. I can even agree with Horkheimer's formulation of his ultimate aims. In the second volume of his book *Kritische Theorie* he says, after rejecting Utopianism: 'Nevertheless, the idea of a future society as a community of free men ... has a content to which we ought to remain loyal through all [historical] change.'⁴ I certainly agree with this idea, the idea of a society of free men (and also with the idea of loyalty to it). It is an idea that inspired the American and the French revolutions. Unfortunately, Horkheimer has nothing of the slightest interest to say about the problem of how to get nearer to this ideal aim.

In fact, Horkheimer rejects, without argument and in defiance of historical facts, the possibility of reforming our so-called 'social system'. This amounts to saying: Let the present generation suffer and perish – for all we can do is to expose the ugliness of the world we live in, and to heap insults on our oppressors, the 'bourgeoisie'. *This is the total content of the so-called Critical Theory of the Frankfurt School.*

Marx's own condemnation of our society makes sense. For Marx's theory contains the promise of a better future. But the theory becomes vacuous and irresponsible if this promise is withdrawn, as it is by Adorno and Horkheimer. This is why Adorno found that life is not worth living. For life is really worth living only if we can work for a better world *now*, and for the immediate future.

It is a crime to exaggerate the ugliness and the baseness of the world: it is ugly, but it is also very beautiful; inhuman, and also very human. And it is threatened by great dangers. The greatest is world war. Almost as great is the population explosion. But there is much that is good in this world. For there is much good will. And there are millions of people alive today who would gladly risk their lives if they thought that they could thereby bring about a better world.

We can do much *now* to relieve suffering and, most important, to increase individual human freedom. We *must not* wait for a goddess of history or for a goddess of revolution to introduce better conditions into human affairs. History, and also a revolution, may easily fail us. It did fail the Frankfurt School, and it caused Adorno to despair. We must produce and critically try out ideas about what can and should be done now – and do it now.

To sum up with a phrase of Raymond Aron, I regard the writings of the Frankfurt School as 'opium of the intellectuals'.⁵

NOTES

- 1 Max Horkheimer and Theodor W. Adorno, *Dialectic of Enlightenment*, Herder & Herder, New York, 1972.
- 2 Karl Marx, *Capital*, volume II, 1872, 'Nachwort'.
- 3 Max Horkheimer, *Kritische Theorie*, edited by A. Schmidt, S. Fisher, Frankfurt, 1968, volume II, pp. 340f.
- 4 Horkheimer, *Kritische Theorie*, p. 166.
- 5 Raymond Aron, *L'Opium des intellectuels*, Calmann-Lévy, Paris, 1955.