

Objective Knowledge

A Realist View of Logic, Physics, and History (1966)

Karl Popper

MAN, some modern philosophers tell us, is alienated from his world: he is a stranger and afraid in a world he never made. Perhaps he is; yet so are animals, and even plants. They too were born, long ago, into a physico-chemical world, a world they never made. But although they did not make their world, these living things changed it beyond all recognition and, indeed, remade the small corner of the universe into which they were born. Perhaps the greatest of these changes was made by the plants. They radically transformed the chemical composition of the earth's whole atmosphere. Next in magnitude are perhaps the achievements of some marine animals which built coral reefs and islands and mountain ranges of limestone. Last came man, who for a long time did not change his environment in any remarkable way, apart from contributing, by deforestation, to the spread of the desert. Of course, he did build a few pyramids; but only during the last century or so, did he begin to compete with the reef-building corals. Still more recently he began to undo the work of the plants by slightly, though significantly, raising the carbon dioxide content of the atmosphere.

Thus, we have not made our world. So far we have not even changed it much, compared with the changes achieved by animals and plants. Yet we have created a new kind of product or artefact which promises in time to work changes in our corner of the world as great as those worked by our predecessors, the oxygen-producing plants, or the island-building corals. These new products, which are decidedly of our own making, are our myths, our ideas, and especially our scientific theories: theories about the world we live in.

I suggest that we may look upon these myths, these ideas and theories, as some of the most characteristic products of human activity. Like tools, they are organs evolving outside our skins. They are exosomatic artefacts. Thus we may count among these characteristic products especially what is called 'human knowledge'; where we take the word 'knowledge' in the objective or impersonal sense, in which it may be said to be contained in a book; or stored in a library; or taught in a university.

When referring to human knowledge, I shall usually have this objective sense of the word 'knowledge' in mind. This allows us to think of knowledge produced by men as analogous to the honey produced by bees: the honey is made by bees, stored by bees, and consumed by bees; and the individual bee which consumes honey will not, in general, consume only the bit it has produced itself: honey is also consumed by the drones which have not produced any at all (not to mention that stored treasure of honey which the bees may lose to bears or beekeepers). It is

also interesting to note that, in order to keep up its powers to produce more honey, each working bee has to consume honey, some of it usually produced by other bees.

All this holds, by and large, with slight differences, for oxygen-producing plants and for theory-producing men: we, too, are not only producers but consumers of theories; and we have to consume other people's theories, and sometimes perhaps our own, if we are to go on producing.

'To consume' means here, first of all, 'to digest', as in the case of the bees. But it means more: our consumption of theories, whether those produced by other people or by ourselves, also means criticising them, changing them, and often even demolishing them, in order to replace them by better ones.

All these are operations which are necessary for the *growth of our knowledge*; and I again mean here, of course, knowledge in the objective sense.

I suggest that it looks at present as if it is this growth of human knowledge, the growth of our theories, which turns out, *human history* into a chapter so radically new in the history of the universe, and also in the history of life on earth.

All three of these histories-the history of the universe, the history of life on earth, and the history of man and of the growth of his knowledge-are, of course, themselves chapters of our knowledge. Consequently, the last of these chapters-that is, the history of knowledge-will consist of knowledge about knowledge. It will have to contain, at least implicitly, theories about theories, and especially theories about the way in which theories grow.

I shall, therefore, before going any further into my topic, present a general tetradic schema which I have found more and more useful as a description of the growth of theories. It is as follows:

$$P_1 \gg TT \gg EE \gg P_2.$$

Here '*P*' stands for 'problem'; '*TT*' stands for 'tentative theory'; and '*EE*' stands for '(attempted) error-elimination', especially by way of critical discussion. My tetradic schema is an attempt to show that the result of criticism, or of error-elimination, applied to a tentative theory, is as a rule the emergence of a new problem; or, indeed, of several new problems. Problems, after they have been solved and their solutions properly

examined, tend to beget problem-children: new problems, often of greater depth and ever greater fertility than the old ones. 'This can be seen especially in the physical sciences; and I suggest that we can best gauge the progress made in any science by the distance in depth and expectedness between P_1 and P_2 : the best tentative theories (and all theories are tentative are those which give rise to the deepest and most unexpected problems.

My tetradic schema can be elaborated in various ways; for example, by writing it as follows:

» TT_a » EE_a » P_{2a}

P_1 » TT_b » EE_b » P_{2b}

» TT_n » EE_n » P_{2n}

In this form the schema would indicate that, if we can, we should propose many theories as attempts to solve some given problem, and that we should critically examine each of our tentative solutions. We then find that each gives rise to new problems; and we may follow up those which promise the most novel and most interesting new problem: if the new problem, P_{2b} , say, turns out to be merely the old P , in disguise, then we say that our theory only manages to *shift the problem* a little; and in some cases we may take this as a decisive objection to the tentative theory, TT_b .

This shows that error-elimination is only *part* of our critical discussion: our critical discussion of the competing tentative theories may compare them, and assess them, from many different points of view. The decisive point is, *of course*, always: how well does our theory solve its problems; that is, P_1 .

At any rate, one of the things we wish to achieve is to learn something new. According to our schema, progressiveness is one of the things we demand of a good tentative theory: and it is brought out by the critical discussion of it: the theory is progressive if our discussion shows that *it has really made a deference to the problem we wanted to solve*; that is, if the newly emerging problems are different from the old ones.

If the newly emerging problems are different then we can hope to learn a great many new things when we proceed to solve them in turn.

Thus my tetradic schema can be used to describe the emergence of new problems and, consequently, the emergence of new solutions-that is, new

theories; and I even want to present it as an attempt to make sense of the admittedly vague idea of emergence-as an attempt to speak of emergence in a rational manner. I should like to mention that it can be applied not only to the emergence of new scientific problems and, consequently, new scientific theories, but to the emergence of new forms of behaviour, and even new forms of living organisms.

Let me give you an example. P_1 may be, say, a certain problem concerning the survival of a species, such as the problem of reproduction, of producing offspring. According to Darwin, this survival problem has found a good solution if the species survives; any other tentative solution will be eliminated by the disappearance of both the solution and the species.

According to my schema, the attempted error-elimination, that is, the struggle for survival-will bring out the inherent weakness of each of the proposed solutions in the form of a *new problem*. For example, the new problem may be that the parent organisms and their offspring are threatening to suffocate one another. This new problem may in turn be solved; for example, the organisms may develop a method of scattering or disseminating their offspring; or else the new problem may be solved by the establishment of a common economy, comprising several organisms. Perhaps the transition from unicellular to multicellular organisms proceeded in this way.

However this may be, my schema shows that there may be more than Darwin's alternative, '*survive or perish*', inherent in the process of error-elimination: error-elimination may bring out new emerging problems, specifically related to the old problem and to the tentative solution.

In what follows I shall use my schema, sometimes only implicitly; and I shall refer to emergence, assuming that my schema makes this idea sufficiently respectable within what I hope will be a rational discussion. I propose to deal with some aspects of the growth of knowledge under four headings:

1. Realism and Pluralism: Reduction versus Emergence.
2. Pluralism and Emergence in History.
3. Realism and Subjectivism in Physics.
4. Realism in Logic.

Berkeley - Quine - Carnap

1. Realism and Pluralism: Reduction versus Emergence

Man produces not only scientific theories but many other ideas - for example, religious or poetical myths or, say, plots for stories.

What is the characteristic difference between a scientific theory and a work of fiction? It is not, I hold, that the theory is possibly true while the descriptions in the story are not true, although truth and falsity have something to do with it. The difference is, I suggest, that the theory and the story are embedded in different critical traditions. They are meant to be "judged by quite different traditional standards (even though these standards may have something in common).

What characterises the theory is that it is offered as a solution to a scientific problem; that is, either a problem that has arisen before, in the critical discussion of earlier tentative theories, or (perhaps) a problem discovered by the author of the theory now offered, but discovered within the realm of the problems and solutions belonging to the scientific tradition.

However, I am not leaving it at that. For the scientific tradition in its turn is, or was until recently, characterised by what may be called *scientific realism*. That is to say, it was inspired by the ideal of finding *true solutions* to its problems: solutions which corresponded to the facts.

This regulative ideal of finding theories which correspond to the facts is what makes the scientific tradition a realist tradition: it distinguishes between the world of our theories and the world of facts to which these theories belong.

Moreover, the natural sciences with their critical methods of problem solving, and some of the social sciences too, especially history and economics, have represented for quite a long time our best efforts in problem solving and fact finding (by fact finding I mean, of course, the discovery of statements or theories which correspond to facts). Thus these sciences contain, by and large, the best statements and theories from the point of view of truth; that is, those giving the best description of the world of facts, or of what one calls 'reality'.

Now let us look at certain relations that hold between some of these sciences.

Take physics and chemistry for example; sciences which make assertions about all physical things and physical states, including living organisms.

Physics and chemistry are not very different, and there seems to be no great difference in the kind of things to which they apply, except that chemistry, as it is usually understood, becomes inapplicable at very high temperatures and also, perhaps, at very low ones. It therefore would not be very surprising if the hopes, held for a long time, that chemistry can be reduced to physics, were to come true, as indeed they seem to be doing.

Here we have a real paradigm case of a '*reduction*'; by a *reduction* I mean, of course, that all the findings of chemistry can be fully explained by (that is to say, deduced from) the principles of physics.

Although such a reduction would not be very surprising, it would be a very great scientific success. It would not only be an exercise in unification, but a real advance in understanding the world.

Let us assume that this reduction has been carried out completely. This might give us some hope that we may also reduce one day all the biological sciences to physics.

Now this would be a spectacular success, far greater than the reduction of chemistry to physics. Why? Because the kind of things to which physics and chemistry apply are really very similar from the start. Only think how difficult it would be to say whether the atomic theory is a physical or a chemical theory. In fact, for a long time it was both; and it is this common bond which provides the link which may lead, or perhaps has led, to their unification.

With living organisms the situation is different. They are, no doubt, subject to all kinds of physical and biological laws. Yet there appears to be some *prima facie* difference between living organisms and non-living things. Admittedly, we learn from science that there are transitory or intermediate stages, and also intermediate systems; and this gives us hope that a reduction might be achieved one day. Moreover, it seems not at all improbable that recent tentative theories about the origin of life on earth might be successfully put to the test, and that we might be able to create primitive living organisms artificially.

But even this would not necessarily mean a complete reduction. This is shown by the fact that chemists were able to create all sorts of chemicals, inorganic and organic, before understanding even their chemical composition, to say nothing about their physical structure. Thus even the control of chemical processes by purely physical means is not as such equivalent to a reduction of chemistry to physics. Reduction means much

more. It means *theoretical* understanding: the *theoretical* penetration of the new field by the old field.

Thus we might find a recipe for creating some primitive forms of life from non-living matter without understanding, theoretically, what we were doing. Admittedly, this would be a tremendous encouragement to all those who seek for a reduction, and rightly so. But the way to a reduction might still be long; and we could not know whether it was not even impassable: there may be no theoretical reduction of biology to physics, just as there seems to be neither a theoretical reduction of mechanics to electrodynamics, nor a theoretical reduction the other way round.

If the situation is such that, on the one hand, living organisms may originate by a natural process from non-living systems, and that, on the other hand, there is no complete theoretical understanding of life possible in physical terms, then we might speak of life as an *emergent* property of physical bodies, or of matter.

Now I want to make it quite clear that as a rationalist I wish and hope to understand the world and that I wish and hope for a reduction. At the same time, I think it quite likely that there may be no reduction possible; it is conceivable that life is an *emergent* property of physical bodies.

My point here is that those believers in reduction who, for some philosophical or other reason, adopt *a priori* the dogmatic position that reduction must be possible, in a way destroy their triumph should reduction ever be achieved. For what will then be achieved ought to have been achieved all the time; so their triumph will be only the uninteresting one of having been proved right by events.

Only those who assert that the question cannot be settled *a priori* can claim that any successful reduction would be a tremendous discovery.

I have dwelt on this point so long because it has some bearing on the position of the next rung of the ladder—the emergence of consciousness.

There are philosophers, called 'radical behaviourists' or 'physicalists', who think that they have *a priori* reasons, such as *Ockham's razor*, for asserting that our introspection of mental states or events, and our reports about mental states or events, are simply introspections and reports about ourselves qua physical systems: they are reports about physical states of these systems.

Two philosophers expected here this morning have defended such a view with brilliant arguments. They are Herbert Feigl and Willard Van Orman Quine. I should like to make a few critical remarks about their views.

Quine says, with a reference to Carnap and Feigl, that if theoretical progress can be 'achieved by ... positing distinctive mental states ... behind physical behaviour, surely as much ... could be achieved by positing..... certain correlative physiological states and events instead.... Lack of a detailed physiological explanation of the states is scarcely an objection to acknowledging them as states of human bodies.... The bodily states exist anyway; why add the others?'

Let me point out that Quine speaks here as a realist: 'The bodily states exist anyway', he says. Nevertheless, from the point of view I am adopting here, he is not what I should call a 'scientific realist': he does not wait to see whether science will achieve a reduction here, as perhaps it may one day; instead he applies Ockham's razor, pointing out that mental *entities* are not necessary for the theory.

But who knows what Ockham or anybody else might mean here by necessity? If mental entities or, better, mental states should exist - and I myself do not doubt that they do exist, then positing mental states is necessary for any true explanation of them; and should they one day be reduced to physical states, then this will be a tremendous success. But there will be no success at all if we reject their existence by merely noting that we can explain things without them, by the simple method of confining ourselves to physical things and their behaviour.

To sum up my argument in brief: philosophical speculations of a materialistic or physicalistic character are very interesting, and may even be able to point the way to a successful scientific education. But they should be frankly tentative theories (as think Feigl's theories are). Some physicalists do not, however, consider their theories as tentative, but as proposals to express everything in a physicalistic language; and they think these proposals have much in their favour because they are undoubtedly convenient: inconvenient problems such as the body-mind problem do indeed, most conveniently, disappear. So these physicalists think that there can be no doubt that these problems should be eliminated as pseudo-problems.

To this I would reply that by the same method we could have eliminated *a priori* all chemical states and problems connected with them: we could have said that they were obviously physical, and that there was no need to specify them in detail: that all we needed to do was to postulate the

existence of some physical state correlative to each chemical state. : I think it is clear that the general adoption of such a proposal would have led to the attitude of not looking for the detailed reduction of chemistry to physics. No doubt, it would have dissolved the analogue of the body-mind problem-the problem of the relation of physics to chemistry; but the solution would have been linguistic; and as a consequence we should not have learned anything about the real world.

All this leads me to assert that realism should be at least tentatively pluralistic, and that realists should subscribe to the following pluralistic postulate:

We must beware of solving, or dissolving, factual problems

linguistically; that is, by the all too simple method of refusing to talk about them. On the contrary, we must be pluralists, at least to start with: we should first emphasise the difficulties, even if they look insoluble, as the body-mind problem may look to some.

If we can then reduce or eliminate some entities by way of scientific reduction, let us do so by all means, and be proud of the gain in understanding.

So I would say: let us work out in every case the arguments for emergence in detail, *at any rate before attempting reduction*.

To sum up and sharpen the considerations advanced in this section:

The reduction of chemistry to physics, apparently now well on the way, may be described as a paradigm case of a genuine scientific reduction which satisfies all the requirements of a good scientific explanation.

'Good' or 'scientific' reduction is a process in which we learn much that is of great importance: we learn to understand and to explain the theories about the field to be reduced (in this case chemistry) and we learn a great deal about the power of the reducing theories (in this case physics).

It is conceivable, although not yet certain, that the reduction of chemistry to physics will be completely successful. It is also conceivable, though less likely, that we may one day have *good reductions* of biology, including physiology, to physics, and of psychology to physiology, and thus to physics.

I call *bad reduction or ad hoc* reduction the method of reduction by merely linguistic devices; for example, the method of physicalism which suggests that we postulate *ad hoc* the existence of physiological states to explain behaviour which we previously explained by postulating (though not by postulating *ad hoc*) mental states. Or in other words, by the linguistic device of saying that I report on a *physiological* state of mine when I report that I now feel that I understand the Schrödinger equation.

This second kind of reduction or the use of Ockham's razor is bad, because it prevents us from seeing the problem. In ,the picturesque as well as hard-hitting terminology of Imre Lakatos, it is a disastrous case of a '*degenerating problem shift*'; and it may prevent either a good reduction, or the study of emergence, or both.

In order to avoid this disastrous method we must in each case try to learn as much as possible about the field which we hope to reduce. It may be that the field resists reduction; and we may even possess arguments to show why the field cannot be reduced. In this case we may have an example of genuine emergence.

If I may perhaps end my comments on the degenerating of behaviourism (especially linguistic behaviourism) with the following remark.

Behaviourists and materialists are anti-idealists: and they are, rightly, opponents of Berkeley's '*esse = percipi*' or

to be = to be observable.

According to them, 'to be' is 'to be material', 'to behave as a body in space and time'. Nevertheless, it may be said that they do adhere, unconsciously, to Berkeley's equation, although they put it in a slightly different verbal form:

to be = to be observed

or perhaps

to be = to be perceived.

For they say that only those things exist which can be observed. They do not realise that *all observation involves interpretation in the light of theories*, and that what they call 'observable' is what is observable in the light of pretty old-fashioned and primitive theories. Though I am all for common sense, I am also for enlarging the realm of common sense by

learning from science. At any rate, *it is not science but dubious philosophy (or outdated science) which leads to idealism, phenomenism, positivism; or to materialism and behaviourism, or to any other of anti-pluralism.*

Peirce - Heisenberg - Hegel - Ilyenkov - de Saussure

2. Pluralism and emergence in History

I shall not speak about the history of the universe, but only say a few words about the history of life on earth.

It seems that a very promising start has recently been made towards reconstructing the conditions under which life *emerged* on earth; and I think we may, perhaps, expect some major success soon. But while sanguine about emergence, even experimental emergence, I feel very sceptically inclined about reduction. This is due to certain thoughts of mine about the evolution of life.

It seems to me that evolutionary processes or major evolutionary changes are as unpredictable as historical processes or major historical changes. I hold this view because I am strongly inclined towards an indeterminist view of the world, somewhat more radical than Heisenberg's: my indeterminism includes the thesis that even classical physics is indeterministic, and is thus more like that of Charles Sanders Peirce, or that of Alfred Landé. And I think that evolution proceeds largely probabilistically, under constantly changing conditions or problem situations, and that every tentative solution, whether more successful or less successful or even completely unsuccessful, creates a new problem situation. This seems to me to prevent a complete reduction as well as a complete understanding of the processes of life, although it does not prevent constant and far-reaching progress towards such understanding. (This argument should not be taken to be like Bohr's application of his idea of complementarity to living organisms - an argument which seems to me very weak indeed.)

But I want to speak in this section mainly about human history, about the story of mankind. This, as I have indicated, is very largely the history of our knowledge - of our theories about the world - and, of course, of the repercussions of these products, which are of our own making, upon ourselves and our further productions.

It is obvious that one can adopt a physicalist or materialist attitude towards these theoretical products of ours; and it might be suspected that

my emphasis upon the objective sense of knowledge - my emphasis upon theories as contained in books collected in libraries and as taught in universities-indicates that I sympathise with the physicalist or materialist interpretation of theories; I mean an interpretation which sees language as consisting of physical objects - noises, or printed letters-and which sees ourselves as conditioned, or dispositioned, to react to these noises or letters with certain characteristic kinds of physical behaviour.

But nothing is further from my intention than to encourage *ad hoc* reductions of this kind. Admittedly, if forced to choose between any subjectivist or personalist view of human knowledge and the materialist or physicalist view I have just tried to sketch, I should choose the latter; but this is emphatically *not* the alternative.

The history of ideas teaches us very clearly that ideas emerge in logical or, if the term is preferred, in dialectical contexts. My various schemata such as

$$P_1 \gg TT \gg EE \gg P_2$$

may indeed be looked upon as improvements and rationalisations of the Hegelian dialectical schema: they are rationalisations because they operate entirely within the classical logical organon of rational criticism, which is based upon the so-called law of contradiction; that is to say, upon the demand that contradictions, whenever we discover them, must be eliminated. Critical error-elimination on the scientific level proceeds by way of a conscious search for contradictions.

Thus history, and especially the history of ideas, teaches us that if we want to understand history, we must understand ideas and their objective logical (or dialectical) relationships.

I do not believe that anybody who has ever seriously gone into any chapter of the history of ideas will think that a reduction of these ideas could ever be successful. But I take it as my task here not so much to argue against the possibility of any reduction, as to argue for the recognition of emergent entities, and for the need to recognise and describe these emergent entia before one can seriously think about their possible elimination by way of reduction.

One of my main arguments for the emergent character of theories I have given elsewhere. My argument depends upon the conjecture that there is such a thing as a genuine growth of scientific knowledge; or in practical terms, that tomorrow, or a year hence, we may propose and test important

theories of which nobody has seriously thought so far. If there is growth of knowledge in this sense, then it cannot be predictable by scientific means. For he who could so predict today by scientific means our discoveries of tomorrow could make them today; which would mean that there would be an end to the growth of knowledge.

On the other hand, unpredictability in principle has always been considered as the salient point of emergence; and it seems to me that my argument shows at any rate that the growth of knowledge must be unpredictable in principle.

But there are other arguments for the emergent character of theories, or of knowledge in the objective sense. I shall only mention an argument or two against the very popular and very naive view that theories can be reduced to the mental states of those who produce them, or of those who understand them. (Whether or not these mental states themselves may then perhaps be reduced to physical states in turn will not be further discussed.)

The idea that a theory in its objective or logical sense may be reduced to the mental states of those who hold the theory takes, as a rule, the form that the theory just *is* a thought. But this is a trivial mistake: it is the failure to distinguish between two senses of the word 'thought'. In its subjective sense, the word 'thought' describes a mental experience or a mental process. But two mental experiences or processes, though they may stand in causal relations to each other, cannot stand in logical relations to each other.

Thus, if I say that certain ideas of the Buddha agree with certain ideas of Schopenhauer, or that they contradict certain ideas of Nietzsche, then I am not speaking about the mental thought-processes of these people or about their interrelations. If I say, however, that Nietzsche was influenced by certain ideas of Schopenhauer, then I do mean that certain thought processes of Nietzsche's were causally influenced by his reading of Schopenhauer. So we have actually these two different worlds, the world of *thought-processes*, and the world of the *products* of thought-processes. While the former may stand in *causal* relationships, the latter stand in *logical* relationships.

The fact that certain theories are incompatible is a logical fact, and holds quite independently of whether or not anybody has noticed or understood this incompatibility. These purely objective logical relationships are characteristic of the entities which I have called theories, or knowledge, in the objective sense.

This may also be seen from the fact that the person who produces a theory may very often not understand it. Thus it might be argued without paradox that Erwin Schrödinger did not fully understand the Schrödinger equation, at any rate not until Max Born gave his statistical interpretation of it; or that Kepler's area law was not properly understood by Kepler, who seems to have disliked it.

In fact, understanding a theory is something like an infinite task, so that we may well say that a theory is never fully understood, even though some people may understand some theories extremely well.

Understanding a theory has, indeed, much in common with understanding a human personality. We may know or understand a man's system of dispositions pretty well; that is to say, we may be able to predict how he would act in a number of different situations. But since there are infinitely many possible situations, of infinite variety, a full understanding of a man's dispositions does not seem to be possible. Theories are similar: a full understanding of a theory would mean understanding all its logical consequences. But these are infinite in a non-trivial sense: there are infinitely many situations of infinite variety to which the theory might be applicable; that is to say, upon which some of its logical consequences may bear; and many of these situations have never been thought of; their possibility may not yet have been discovered. But this means that nobody, neither its creator nor anybody who has tried to grasp it, can have a full understanding of all the possibilities inherent in a theory; which shows again that the theory, in its logical sense, is something objective and something objectively existing - an object that we can study, something that we try to grasp. It is no more paradoxical to say that theories or ideas are our products and yet not fully understood by us than to say that our children are our products and yet not fully understood by us, or that honey is a product of the bee, yet not fully understood by any bee.

Thus, the study of the history of our theories or ideas-and a good case could be made for the view that all human history is largely a history of our theories or ideas-should make us all pluralists. For what exist, for the historian, are people in physical, social, mental, and ideological problem situations; people producing ideas by which they try to solve these problems, ideas which they try to grasp, to criticise, to develop.

The student of the history of ideas will find that ideas have a kind of life (this is a metaphor, of course); that they can be misunderstood, rejected, and forgotten; that they can reassert themselves, and come to life again. Without metaphor, however, we can say that they are not identical with

any man's thought, or belief; that they can exist even if universally misunderstood, and rejected.

All this may be reminiscent of Plato and Hegel. But there are great differences here. Plato's 'ideas' were eternal, unchanging conceptions or notions; Hegel's were dialectically self-changing conceptions or notions. The ideas which I find most important are not conceptions or notions at all. They correspond not to words but to statements or propositions.

In opposition to Plato and Hegel I consider *tentative theories* about the world-that is, hypotheses together with their logical consequences-as the most important citizens of the world of ideas; and I do not think (as Plato did) that their strangely non-temporal character makes them eternal and thereby *more real* than things that are generated and are subject to change, and to decay. On the contrary, a thing that can change and perish should for this very reason be accepted as *prima facie* real; and even an illusion is, *qua* illusion, a real illusion.

This is important in connection with the problem of time, and of change.

A historian cannot, I think, accept the doctrine that time and change are illusions; a doctrine upheld by some great physicists and philosophers such as Parmenides, Weyl, and Schrödinger. Nothing is more real than an event, an occurrence; and every event involves some change.

That the pluralistic universe in which the historian lives, with its individual men living individual lives, trying to solve their problems, producing children, and ideas about them, hoping and fearing and deceiving themselves and others, but always theorising, and often seeking not only happiness but also truth-that this pluralistic universe should be successfully 'reduced' to one or another kind of monism-this seems to me not only unlikely, but impossible. But this is not my point here. My point is that only after recognising the plurality of what there is in this world can we seriously begin to apply Ockham's razor. To invert a beautiful formulation of Quine's, only if Plato's beard is sufficiently tough, and tangled by many entities, can it be worth our while to use Ockham's razor. That the razor's edge will be dulled in being used for this tough job is only to be expected. The job will no doubt be painful. But it is all in a day's work.

Bohr - Einstein - Heisenberg - Quine - Bridgman - Ilyenkov - Wittgenstein

3. Realism and Subjectivism in Physics

There are two important fields in modern physics in which physicists have allowed subjectivism not only to enter, but to play an essential role: Boltzmann's theory of the subjectivity of the direction of time, and Heisenberg's interpretation of the indeterminacy formulae as determining a lower limit to the effect of the observer's interference with the observed object.

There was also another intrusion of the subject, or of the observer, when Einstein brought in the observer in a number of imaginary thought experiments intended to elucidate relativity; but this is a field from which the observer was exorcised, slowly but steadily, by Einstein himself.

I shall not discuss this point further, nor shall I discuss the subjective theory of time which, in trying to tell us that time and change are human illusions, forgets that they are very real illusions which have in no way been reduced to anything else (and which, I conjecture, are not amenable to reduction). I shall not discuss all this because I have done so only recently. I merely want to say a few words about the Heisenberg formulae and their interpretation.

These formulae are usually derived in a fairly complicated manner; there is, for example, an interesting derivation by Weyl and another rather complicated one by Born.

Yet in fact the Heisenberg formula for energy depends neither on wave mechanics nor on Heisenberg's matrix mechanics; nor do we need the commutation relations (which according to Hills are insufficient for the derivation of the formulae). It simply does not depend on the revolutionary new quantum mechanics of 1925-6, but follows directly from Planck's old quantum postulate of 1900:

$$E = hf.$$

From this we get immediately

$$(2) DE = h Df.$$

By using the principle of harmonic resolving power,

$$(3) Df \text{ approx} = 1/Dt,$$

we obtain from (2) and (3)

$$(4) DE \text{ approx} = h / Dt,$$

which leads at once to

$$(5) \Delta E \cdot \Delta t \approx h;$$

that is to say, a form of Heisenberg's so-called *indeterminacy formulae*.

In precisely the same way we obtain the Heisenberg formula for position and momentum from Duane's principle (whose analogy to Planck's principle has recently been stressed by Alfred Landé). It may be written

$$(6) \Delta p_i \approx h / \Delta q_i$$

According to Landé this may be interpreted as follows: a body (such as a grid or a crystal) endowed with the space-periodicity Δq_i is entitled to change its momentum p_i in multiples of $\Delta p_i \approx h / \Delta q_i$.

From (6) we obtain at once

$$(7) \Delta p_i \cdot \Delta q_i \approx h,$$

which is another form of Heisenberg's indeterminacy formulae.

Considering that Planck's theory is a statistical theory, the Heisenberg formulae can be most naturally interpreted as statistical *scatter relations*, as I proposed more than thirty years ago. That is, they say nothing about the possible precision of measurements, nor anything about limits to our knowledge. But if they are scatter relations, they tell us something about the limits to the homogeneity of quantum-physical states, and therefore, though indirectly, about predictability.

For example, the formula $\Delta p_i \cdot \Delta q_i \approx h$ (which can be obtained from Duane's principle just as $\Delta E \cdot \Delta T \approx h$ can be obtained from Planck's principle) tells us, simply, that if we determine the coordinate x of a system (say, an electron) then, upon repetition of the experiment, the momentum will scatter.

Now how can such an assertion be tested? By making a long series of experiments with a fixed shutter opening Δx and by measuring, in every single case, the momentum P_x . If these momenta scatter as predicted, then the formula has survived the test. But this shows that in order to test the scatter relations, we have actually measured, in every case, p_x with a precision far greater than Δp_x ; for otherwise we could not speak of Δp_x , as the scatter of p_x .

Experiments of the kind described are carried out every day in all physical laboratories. But they refute Heisenberg's indeterminacy interpretation, since measurements (though not the predictions based upon them) are more precise than this interpretation permits.

Heisenberg himself noted that such measurements are possible, but he said that it was 'a matter of personal belief' or personal taste' whether or not we attach any meaning to them; and ever since this remark they have been universally disregarded as meaningless. But they are not meaningless, for they have a definite function: they are tests of the very formulae in question; that is, of the indeterminacy formulae qua scatter relations.

There is, therefore, no reason whatever to accept either Heisenberg's or Bohr's subjectivist interpretation of quantum mechanics. Quantum mechanics is a statistical theory because the problems it tries to solve - spectral intensities, for example - are statistical problems. There is, therefore, no need here for any philosophical defence of its non-causal character.

The irreducibility of statistical theories to deterministic theories (rather than the incompatibility of these two kinds of theories) should, however, be established. Arguments to this effect have been offered by Landé, and very different ones by myself.

To sum up, there is no reason whatsoever to doubt the realistic and objectivistic character of all physics. The role played by the observing subject in modern physics is in no way different from the role he played in Newton's dynamics or in Maxwell's theory of the electric field: the observer is, essentially, the man who tests the theory. For this, he needs a lot of other theories, competing theories and auxiliary theories. All this shows that we are not so much observers as thinkers.

Schlick - Brouwer - Quine - Carnap - Dewey - Hegel - Wittgenstein

4. Realism in Logic

I am opposed to looking upon logic as a kind of game. I know about so-called alternative systems of logic and I have actually invented one myself, but alternative systems of logic can be discussed from very different points of view. One might think that it is a matter of choice or convention which logic one adopts. I disagree with this view.

My theory is briefly this. I look upon logic as the theory of deduction or of derivability, or whatever one chooses to call it. Derivability or deduction involves, essentially, *the transmission of truth and the retransmission of falsity*: in a valid inference truth is transmitted from the premises to the conclusion. This can be used especially in so-called 'proofs'. But falsity is also retransmitted from the conclusion to (at least) one of the premises, and this is used in disproofs or refutations, and especially in *critical discussions*.

We have premises and a conclusion; and if we show that the conclusion is false, and assume that the inference is valid, we know that at least one of our premises must be false. This is how logic is constantly used in critical discussion, for in a critical discussion we attempt to show that something is not in order with some assertion. We attempt to show it; and we may not succeed: criticism may be validly answered by counter-criticism.

What I should wish to assert is (1) that criticism is a most important methodological device; and (2) that if you answer criticism by saying, 'I do not like your logic: your logic may be all right for you, but I prefer a different logic, and according to my logic this criticism is not valid', then you may undermine the method of critical discussion.

Now I should distinguish between two main uses of logic, namely (1) its use in the demonstrative sciences-that is to say, the mathematical sciences-and (2) its use in the empirical sciences.

In the demonstrative sciences logic is used in the main for proofs-for the transmission of truth-while in the empirical sciences it is almost exclusively used critically-for the retransmission of falsity. Of course, applied mathematics comes in too, in which we implicitly make use of the proofs of pure mathematics, but the role of mathematics in the empirical sciences is somewhat dubious in several respects. (There exists a wonderful article by Schwartz to this effect.)

Thus in the empirical sciences logic is mainly used for criticism; that is, for refutation. (Remember my schema $P_1 \gg TT \gg EE \gg P_2$.)

Now, what I wish to assert is this. If we want to use logic in a critical context, then we should use a very strong logic, the strongest logic, so to speak, which is at our disposal; for we want our criticism to be *severe*. In order that the criticism should be severe we must use the full apparatus; we must use all the guns we have. Every shot is important. It doesn't matter if we are over-critical: if we are, we shall be answered by counter-criticism.

Thus we should (in the empirical sciences) use the full or classical or two-valued logic. If we do not use it but retreat into the use of some weaker logic - say, the intuitionist logic, or some three-valued logic (as Reichenbach suggested in connection with quantum theory)-then, I assert, we are not critical enough; it is a sign that something is rotten in the state of Denmark (which in this case is the quantum theory in its Copenhagen interpretation, as I indicated earlier).

Now let us look, by contrast, at proofs. Every mathematician knows that considerable interest lies in proving a theorem with the help of a *minimum apparatus*. A proof which uses stronger means than necessary is mathematically unsatisfactory, and it is always interesting to find the weakest assumptions or minimum means which have to be used in a proof. In other words, we want the proof not only to be sufficient - that is to say valid-but we want it if possible to be necessary, in the sense that a minimum of assumptions have been used in the proof. This, I admit, is a somewhat sophisticated view. In unsophisticated mathematics we are happy and grateful if we can prove anything, but in more sophisticated mathematics we really want to know what is *necessary* for proving a theorem.

So if one can prove mathematical theorems with methods weaker than the full battery of classical logic, then this is extremely interesting from a mathematical point of view. Thus in proof theory we are interested in weakening if possible our classical logic, and we can, for example, introduce intuitionist logic or some other weaker logic such as positive logic, and investigate how far we can get without using the whole battery.

I think, incidentally, that the term 'intuitionist logic' is a misnomer. It is just a name for a very interesting and somewhat weakened form of classical logic invented by Brouwer and formalised by Heyting. I certainly do not want to say anything in favour of the philosophical theory called intuitionism though I should like to say something in favour of the Brouwer-Heyting logic. But I trust it will not be supposed that I am in any sense defending the authority of intuition in philosophy or logic or anywhere else. Leaving aside for the moment Brouwerian logic, one might say that intuitionism is the doctrine that intuitions are not only important but generally *reliable*. As against this I think that intuitions are very important but that as a rule they do not stand up to criticism. So I am not an intuitionist. However, Brouwerian or so-called 'intuitionist logic' is, from the standpoint of the present discussion, important because it is just a part, a genuine part, and thus a weakened form, of classical logic; that is to say, every inference which is valid from the point of view of

intuitionist logic is also valid from the point of view of classical logic, while the opposite is not the case: we have inferences which may be validly drawn in classical logic but which are not valid in intuitionist logic. Thus if I can prove a theorem (so far proved only by classical means) with intuitionist logic, I have made a real mathematical discovery; for mathematical discoveries do not consist only in finding new proofs of new theorems, but they consist also in finding new proofs of old theorems; and a new proof of a theorem will be especially interesting if it uses weaker means than the old proof. A proof using stronger means one can always have for the asking, *a fortiori*; yet finding a weaker proof is a real mathematical achievement.

So intuitionistic logic is a very interesting approach to mathematics because it tries to prove as many mathematical theorems as possible with reduced logical means.

Intuitionistic logic has a further advantage: one can show that in it the so-called 'law of excluded middle' is not demonstrable (although it is a well-formed formula of the system) One can also show that if in any system whatsoever some well-formed formula is not demonstrable, then the system must be consistent. Generally speaking, the weaker the logical means we use, the less is the danger of inconsistency the danger that a contradiction is derivable. So intuitionist logic can also be looked upon as an attempt to make more certain that our arguments are consistent and that we do not get into hidden inconsistencies or paradoxes or antinomies. How safe such a weakened logic is, as such, is a question into which I do not want to enter now; but obviously it is at least a little safer than the full classical logic. I do not suppose it is always safe, but that is not my point. My point is this. If you wish to prove, or to establish something, you should use weak means. But for disestablishing it-that is to say, for criticising it-we may use strong means. Of course someone might say, 'Look here, I can refute you even with weak means; I do not even need to use the whole of intuitionist logic.' Still, that is not very important. The main thing is that for the rationalist any criticism is welcome-though he may reply to it by criticising the criticism.

Now this rationalist view is a realist view of logic. First, because it looks upon logic partly in connection with the methodology of the natural sciences which, I have tried to argue, is a realistic affair. Secondly, and this is a very special point, because it looks upon logical inference as truth transmitting or falsity re-transmitting; that is to say, it is concerned with the idea of truth.

I would assert that not the least important of the achievements of Alfred Tarski is that by introducing two ideas into logic, he has actually made logic very much a realistic affair. The first is Tarski's idea (partly anticipated by Bolzano) that logical consequence is truth transmission. The second, I would say, is the rehabilitation of the correspondence theory of truth, the rehabilitation of the idea that truth is simply correspondence with the facts.

I think I may differ here a little from Quine, because I think that this idea of Tarski's ought to be interpreted as destructive of relativism, and because I think that Tarski's claim that his theory of truth is an 'absolutistic' theory of truth is correct. In order to explain this point, I will recount a very old story with a slightly new point to it. The old story is the story of the three main theories of truth. The new point is the elimination of the word 'truth' from the story, and with it, of the appearance that we are dealing here with words, or verbal definitions. However, for this elimination some preparatory discussion is needed.

Of the three main theories of truth, the oldest was the correspondence theory, the theory that truth is correspondence with the facts, or to put it more precisely, that a statement is true if (and only if) it corresponds to the facts, or if it adequately describes the facts. This is the theory which I think Tarski has rehabilitated. The second theory is the so-called coherence theory: a statement is regarded as true if (and only if) it coheres with the rest of our knowledge. The third theory is that truth is pragmatic utility or pragmatic usefulness.

Now, the coherence theory has all sorts of versions of which I shall mention just two. According to the first, truth is coherence with our beliefs, or more precisely, a given statement is true if it coheres with the rest of our beliefs. This I find a bit disconcerting because I do not want to put beliefs into logic, for well-known reasons. (If Peter believes p , and if p and q are interdeducible, we might say that Peter is logically bound to believe q . Yet he may not know that p and q are interdeducible, and he may in fact disbelieve q .)

According to the second version of the coherence theory a certain given statement, of which we do not know whether it is true or not, is to be accepted as true if (and only if) it coheres with the statements we have previously accepted. This version has the effect of making our knowledge utterly conservative: 'entrenched' knowledge can hardly be overthrown.

The theory of pragmatic utility is especially concerned with the problem of theories in the natural sciences such as physics. It says that we should

accept a physical theory as true if it turns out in tests, and other applications, to be pragmatically useful, or successful.

I propose now to use something like a trick. My trick consists in this. I shall very soon, until very near the end of this paper, stop referring to *truth*. I shall not any longer ask, 'What is truth?' There are several reasons. My main reason is that I believe that 'What is?' or 'What are?' questions or, in other words, all verbal or definitional questions, should be eliminated. 'What is?' or 'What are?' questions I regard as pseudo-questions; they do not all seem to be so pseudo, but I do think they all are pseudo-questions. Questions such as, 'What is life?' or 'What is matter?' or 'What is mind?' or 'What is logic?' I think should not be asked. They are typically unfruitful questions.

So I think we should also discard the question, 'What is truth?' My first reason (just mentioned) for discarding the question 'What is truth?' one may call 'anti-essentialism'. My second reason is even more important. It is that we should altogether avoid, like the plague, discussing the meaning of words. Discussing the meaning of words is a favourite game of philosophy, past and present: philosophers seem to be addicted to the idea that words and their meaning are important, and are the special concern of philosophy.

I will for your convenience present again here-on the next page-a table which I have used before.

On the left we have *words or concepts and their meanings*, and on the right we have *statements or propositions or theories and their truth*.

IDEAS <i>that is</i>	
DESIGNATIONS <i>or</i> TERMS <i>or</i> CONCEPTS	STATEMENTS <i>or</i> PROPOSITIONS <i>or</i> THEORIES
<i>may be formulated in</i>	
WORDS	ASSERTIONS
<i>which may be</i>	
MEANINGFUL	TRUE
<i>and their</i>	
MEANING	TRUTH
<i>May be reduced, by way of</i>	

DEFINITIONS	DERIVATIONS
<i>to that of</i>	
UNDEFINED CONCEPTS	PRIMITIVE PROPOSITIONS
<i>The attempt to establish (rather than reduce) by these means their</i>	
MEANING	TRUTH
<i>leads to an infinite regress</i>	

Now I have been taught by the experience of a lifetime in this field that one should always try to get away from the left side of the table and to keep to the right side. One should always keep to assertions, to theories, and the question of their truth. One should never get involved in verbal questions or questions of meaning, and never get interested in words. If challenged by the question of whether a word one uses really means this or perhaps that, then one should say: 'I don't know, and I am not interested in meanings; and if you wish, I will gladly accept your terminology.' This never does any harm. One should never quarrel about words, and never get involved in questions of terminology. One should always keep away from discussing concepts. What we are really interested in, our real problems, are factual problems, or in other words, problems of theories and their truth. We are interested in theories and how they stand up to critical discussion; and our critical discussion is controlled by our interest in truth.

Having said this, I intend now to stop using the word 'truth'.

Our problem is no longer: Is truth correspondence? Is truth coherence? Is truth usefulness? This being so, how can we formulate our real problem?

Our problem can be sharply formulated only by pointing out that the opponents of the correspondence theories all made an *assertion*. They all asserted that there cannot be such a thing as the correspondence between a statement and a fact. This is their central assertion. They say that this concept is meaningless (or that it is undefinable, which, incidentally, in my opinion does not matter, since definitions do not matter) In other words, the whole problem arises because of doubts, or scepticism, concerning correspondence: whether there is such a thing as a correspondence between a statement and a fact. It is quite clear that these doubts are serious (especially in view of the paradox of the liar).

It is also quite clear that, but for these doubts, the upholders of the coherence theory and of the theory of pragmatic usefulness would really have nothing to argue against. Nobody denies that pragmatic usefulness and such matters as predictive power are important. But should there exist something like the *correspondence of a theory to the facts*, then this would obviously be more important than mere self-consistency, and certainly also much more important than coherence with any earlier knowledge' (or 'belief'); for if a theory corresponds to the facts but does not cohere with some earlier knowledge, then this earlier knowledge should be discarded.

Similarly, if there exists something like the correspondence of theory to the facts, then it is clear that a theory which corresponds to the facts will be as a rule very useful; more useful, qua theory, than a theory which does not correspond to the facts. (On the other hand, it may be very useful for a criminal before a court of justice to cling to a theory which does not correspond to the facts; but as it is not *this* kind of usefulness which the pragmatists have in mind, their views raise a question which is very awkward for them: I mean the question, 'Useful for whom?'.)

Although I am an opponent of pragmatism as a philosophy of science, I gladly admit that pragmatism has emphasised something very important: the question whether a theory has some application, whether it has, for example, predictive power. *Praxis*, as I have put it somewhere, is invaluable for the theoretician as a spur and at the same time as a bridle: it is a spur because it suggests new problems to us, and it is a bridle because it may bring us down to earth and to reality if we get lost in over-abstract theoretical flights of our imagination. All this is to be admitted. And yet, it is clear that the pragmatist position will be superseded by a realist position if we can meaningfully say that a statement, or a theory, may or may not correspond to the facts.

Thus the correspondence theory does not deny the importance of the coherence and pragmatist theories, though it does imply that they are not good enough. On the other hand, the coherence and pragmatist theories assert the impossibility or meaninglessness of the correspondence theory.

So without ever mentioning the word 'truth' or asking, 'What does truth mean?' we can see that the central problem of this whole discussion is not the verbal problem of defining 'truth' but the following substantial problem: can there be such a thing as a statement or a theory which corresponds to the facts, or which does not correspond to the facts?

Behind the doubts concerning the possibility of speaking about correspondence, there are various strong arguments.

First of all, there are paradoxes or antinomies which arise out of this correspondence idea. Secondly, there are the countless unsuccessful attempts to say more precisely what the correspondence between a statement and a fact consists of. There is the attempt of Schlick, who said that correspondence is to be explained by a one-one relationship between the linguistic statement and the fact; that is, by uniqueness. A statement, he said, is 'true', or corresponds to the facts, if it stands to the facts of the world in a one-one relationship or in a unique relationship: non-correspondence or 'falsity' is the same as ambiguity. Of course, this is an unacceptable view, for many vague and ambiguous statements (such as 'there are a few people somewhere in America') may correspond to the facts; and vice versa, every general proposition or theory which corresponds to the facts corresponds to many facts, so that there is not a one-one relationship.

Moreover, a statement which does not correspond to the facts may be quite unambiguous. A murderer may say unambiguously, 'I have not killed him.' There is no ambiguity in this assertion; but it does not correspond to the facts. Clearly, Schlick's attempt to explain correspondence misfires. Another even worse attempt is Wittgenstein's. Wittgenstein suggested that a proposition is a picture of reality and that correspondence is a relationship very much like the one that holds between the groove on a gramophone record and the sounds which it denotes: a kind of projective relationship between facts and statements. The untenability of this view can easily be shown. One is reminded of the famous story of Livingstone being introduced by an interpreter to a Negro king whom he asked, 'How are you?'. The Negro king answered with one word, and the interpreter began to talk and talk and talk and talk, for ten minutes, translating the word to Livingstone in the form of a long story of the king's sorrows. Then Livingstone asked whether the king was in need of medical assistance, and then the king began to talk and talk and talk and talk and talk. And the interpreter translated it with one word: 'No.'

No doubt this story is invented. But it is well invented; and it illustrates the weakness of the projection theory of language, especially as a theory of the correspondence between a statement and a fact.

But this is not all. The matter is even more serious; namely, Wittgenstein, after having formulated this theory, said that it is impossible to discuss the relationship of language to reality, or to discuss language at all.

(Because language cannot be discussed by language.) This is a field in which words fail us. 'It shows itself' is his favourite expression to indicate the failure of words. Any attempt to go deeper into the relationship between language and reality or to discuss language more deeply or statements more deeply is, accordingly, bound to be meaningless. And although he says in the Preface of his book, 'the *truth* of the thoughts that are here set forth seems to me unassailable and definitive', he ends up by saying, 'Anybody who understands me eventually recognises them [the propositions of the *Tractatus*] as nonsensical.' (Because talk about language is meaningless.) No doubt this refers, apart from other things, especially to his theory of projection. His remark that his readers will see that what he says is meaningless thus confirms what the opponents of the correspondence theory have always said of the correspondence theory, namely that it is meaningless to speak about the correspondence between a statement and a fact.

So we are back at the real issue. It is this: is there or is there not a tenable correspondence theory? Can we or can we not speak meaningfully of the correspondence between a statement and a fact?

Now my assertion is that Tarski has rehabilitated the correspondence theory. This, I think, is a great achievement, and it is a great philosophical achievement. I say this because it has been denied by many philosophers (for example, by Max Black) that there is something philosophically important in Tarski's achievement.

The key to the rehabilitation of the correspondence theory is a very simple and obvious observation made by Tarski. That is, if I want to speak about correspondence between a statement *S* and a fact *F*, then I have to do so in a language in which I can speak about both: statements such as *S*, and facts such as *F*. This seems to be frightfully trivial; but it is nevertheless decisive. It means that the language in which we speak in explaining correspondence must possess the means needed *to refer* to statements, and to *describe* facts. If I have a language which has both these means at its disposal, so that it can refer to statements and describe facts, then in this language -the metalanguage-I can speak about correspondence between statements and facts without any difficulty, as we shall see.

A metalanguage is a language in which we talk about some other language. For example, a grammar of the German language, written in English, uses English as a metalanguage in order to talk about German. The language about which we talk in the *metalanguage* (in this case

English) is usually called *the 'object language'* (in this case German). The characteristic thing about a metalanguage is that it contains (metalinguistic) *names* of words and of statements of the object language, and also (metalinguistic) *predicates*, such as 'noun (of the object language)' or 'verb (of the object language)' or 'statement (of the object language)'. If a metalanguage is to suffice for our purpose it must also, as Tarski points out, contain the usual means necessary to speak about at least all those *facts* about which the object language can speak.

All this is the case if we use English as our metalanguage in order to speak about German (as the object language under investigation).

For example, we shall be able to say in the English metalanguage such things as:

The German words '*Das Gras ist grün*' form a statement of the German language.

On the other hand, we shall be able to describe in our (English) metalanguage the fact which the German statement '*Das Gras ist grün*' describes. We can describe this fact in English simply by saying that grass is green.

We can now make a statement in the metalanguage about *the correspondence of a statement of the object language to the facts* as follows. We can make the assertion: *The German statement 'Das Gras ist grün' corresponds to the facts if, and only if, grass is green.* (Or: '*... only if it is a fact that grass is green.*')

This is very trivial. It is, however, important to realise the following: in our assertion, the words '*Das Gras ist grün*', put within *quotes*, function as a metalinguistic (that is, an *English*) name of a German statement; on the other hand, the English words 'grass is green' occur in our assertion above *without* any quotation marks: they do not function as a name of a statement, but simply as the description of a *fact* (or alleged fact).

This makes it possible for our assertion to express a relationship between a (German) *statement*, and a *fact*. (The *fact* is neither German nor English, although it is, of course, described or spoken about in our metalanguage, which is English: the fact is non-linguistic, it is a fact of the real world, although we need of course a language if we wish to talk about it.) And what our metalinguistic assertion asserts is that a certain (German) statement *corresponds to a certain fact* (a non-linguistic fact, a fact of the real world) under conditions which are precisely stated.

We can, of course, replace the German object language by any other—even by English. Thus we can make the metalinguistic assertion:

The English statement 'Grass is green' corresponds to the facts if, and only if, grass is green.

This looks even more trivial. But it can hardly be denied; nor can it be denied that it expresses the conditions under which a statement corresponds to the facts.

Generally speaking, let '*S*' be a (metalinguistic) name of a statement of the object language, and let '*f*' be the *abbreviation* of an expression of the metalanguage that describes the (supposed) fact *F* which *S* describes. Then we can make the following metalinguistic assertion:

A statement *S* of the object language corresponds to the facts if, and only if, *f*. (or: . . . if it is a fact that *f*.)

Note that while '*S*' is here a metalinguistic name of a statement, '*f*' is not a name, but an abbreviation of an expression of the metalanguage describing a certain fact (the fact which we can name '*F*').

We can now say that what Tarski did was to discover that in order to speak about the correspondence between a statement *S* and a fact *F*, we need a language (a metalanguage) in which we can *speak about* the statement *S* and *state* the fact *F*. (The former we speak about by using the name '*S*', the latter by using a metalinguistic expression '*f*' which *states or describes F*.)

The importance of this discovery is that it dispels all doubt about the meaningfulness of talking about the correspondence of a statement to some fact or facts.

Once this is done, we can, of course, replace the words 'corresponds to the facts' by the words 'is true'.

Tarski, apart from this, introduced a method of giving a *definition* of truth (in the sense of the correspondence theory) for any consistent *formalised system*. But this is not, I think, his main achievement. His main achievement is the rehabilitation of talk about correspondence (and truth). Incidentally, he showed under what circumstances such talk may lead to paradoxes, and how we can avoid these paradoxes; *and he also showed how in ordinary talk about truth we can, and do, avoid paradoxes.*

Once we have settled that we can use 'truth' in the sense of the correspondence of statements to facts, there is really nothing of importance to be added about the word 'truth'. There is no doubt that correspondence to the facts is what we usually call 'truth'; that in ordinary language it is correspondence that we call 'truth', rather than coherence or pragmatic usefulness. A judge who admonishes a witness to speak the truth and nothing but the truth does not admonish the witness to speak what he thinks is useful either for himself or for anybody else. The judge admonishes a witness to speak the truth and nothing but the truth but he does not say, 'All we require of you is that you do not get involved in contradictions', which he would say were he a believer in the coherence theory. But this is not what he demands of the witness.

In other words, the ordinary sense of 'truth' as it is used in courts of law is, no doubt, correspondence. But my main point is that this may be regarded as an afterthought, and as an unimportant afterthought. For if anybody should want to say, 'No, in ordinary language, "truth" is used in a different sense', I should not quarrel with him. I should suggest that we forget all about terminology: I should be prepared to use the terminology of my opponent, pointing out, however, that we have *at least* these three meanings at our disposal: this is the only thing about which I should be prepared to quarrel; but I should refuse to quarrel about words.

I should point out, though, that the correspondence theory of truth is a realistic theory; that is to say, it makes the distinction, which is a realistic distinction, between a theory and the facts which the theory describes; and it makes it possible to say that a theory is true, or false, or that it corresponds to the facts, thus relating the theory to the facts. It allows us to speak of a reality different from the theory. This is the main thing; it is the main point for the realist. The realist wants to have both a theory and the reality or the facts (don't call it 'reality' if you don't like it, just call it 'the facts') which are different from his theory *about* these facts, and which lie can somehow or other compare with the facts, in order to find out whether or not it corresponds to them. Of course, the comparison is always extremely difficult.

One last word about Tarski's theory. Its whole purpose is often misinterpreted: it is wrongly assumed that it is intended to yield a *criterion of truth*. For-coherence was so intended, and likewise pragmatic usefulness; they strengthened the traditional view that any serious theory of truth should present us with *a method of deciding* whether or not a given statement is true.

Tarski has proved many things from his definition of truth. Among other things, he has proved that in a sufficiently powerful language (and in every language in which we can formulate mathematical or physical theories) there can be no criterion of truth; that is, no criterion of correspondence: the question of whether a proposition is true is not in general decidable for the languages for which we may form the concept of truth. Thus the concept of truth plays mainly the role of a regulative idea. It helps us in our search for truth that we know there is something like truth or correspondence. It does not give us a means of finding truth, or of being sure that we have found it even if we have found it. So there is no criterion of truth, and we must not ask for a criterion of truth. We must be content with the fact that the idea of truth as correspondence to the facts has been rehabilitated. This has been done by Tarski; and I think that he has thereby rendered an immense service to the realistic outlook.

Although we have no criterion of truth, and no means of being even quite sure of the falsity of a theory, it is easier to find out that a theory is false than to find out that it is true (as I have explained in detail elsewhere). We have even good reasons to think that most of our theories—even our best theories are, strictly speaking, false; for they oversimplify or idealise the facts. Yet a false conjecture may be nearer or less near to the truth. Thus we arrive at the idea of nearness to the truth, or of a better or less good approximation to the truth; that is, at the idea of '*verisimilitude*'. I have tried to show that this idea can be rehabilitated in a way similar to Tarski's rehabilitation of the idea of truth as correspondence to the facts. In order to do so I have used mainly the two Tarskian ideas mentioned here. One is the idea of truth. The other is the idea of logical consequence; or more precisely, of the set of logical consequences of a conjecture, or the content of a conjecture.

By incorporating into logic the idea of verisimilitude or approximation to truth, we make logic even more 'realistic'. For it can now be used to speak about the way in which one theory corresponds better than another to the facts—the facts of the real world.

To sum up. As a realist I look upon logic as the *organon of criticisms* (rather than of proof) in our search for true and highly informative theories - or at least for new theories that contain more information, and correspond better to the facts, than our older theories. And I look upon criticism, in its turn, as our main instrument in promoting the growth of our knowledge about the world of facts.

Objectivity is a philosophical concept of being true independently from individual subjectivity caused by perception, emotions, or imagination. A proposition is considered to have objective truth when its truth conditions are met without bias caused by a sentient subject. Scientific objectivity refers to the ability to judge without partiality or external influence, sometimes used synonymously with neutrality.