

Conjectures and Refutations

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I
My aim in this lecture is to stress the significance of one particular aspect of science—its need to grow, or, if you like, its need to progress. I do not have in mind here the practical or social significance of this need. What I wish to discuss is rather its intellectual significance. I assert that continued growth is essential to the rational and empirical character of scientific knowledge; that if science ceases to grow it must lose that character. It is the way of its growth which makes science rational and empirical; the way, that is, in which scientists discriminate between available theories and choose the better one or (in the absence of a satisfactory theory) the way they give reasons for rejecting all the available theories, thereby suggesting some of the conditions with which a satisfactory theory should comply.

You will have noticed from this formulation that it is not the accumulation of observations which I have in mind when I speak of the growth of scientific knowledge, but the repeated overthrow of scientific theories and their replacement by better or more satisfactory ones. This, incidentally, is a procedure which might be found worthy of attention even by those who see the most important aspect of the growth of scientific knowledge in new experiments and in new observations. For our critical examination of our theories leads us to attempts to test and to overthrow them; and these lead us further to experiments and observations of a kind which nobody would ever have dreamt of without the stimulus and guidance both of our theories and of our criticism of them. For indeed, the most interesting experiments and observations were carefully designed by us in order to *test* our theories, especially our new theories.

In this paper, then, I wish to stress the significance of this aspect of science and to solve some of the problems, old as well as new, which are connected with the notions of scientific progress and of discrimination among competing theories. The new problems I wish to discuss are mainly those connected with the notions of objective truth, and of getting nearer to the truth— notions which seem to me of great help in analysing the growth of knowledge.

Although I shall confine my discussion to the growth of knowledge in science, my remarks are applicable without much change, I believe, to the growth of pre-scientific knowledge also—that is to say, to the general way in which men, and even animals, acquire new factual knowledge about the world. The method of learning by trial and error—of learning from our mistakes—seems to be fundamentally the same whether it is practised by lower or by higher animals, by chimpanzees or by men of science. My interest is not merely in the theory of scientific knowledge, but rather in the theory of knowledge in general. Yet the study of the growth of scientific knowledge is, I believe, the most fruitful way of studying the growth of knowledge in general. For the growth of scientific knowledge may be said to be the growth of ordinary human knowledge *writ large* (as I have pointed out in the 1958 Preface to my *Logic of Scientific Discovery*).

But is there any danger that our need to progress will go unsatisfied, and that the growth of scientific knowledge will come to an end? In particular, is there any danger that the advance of science will come to an end because science has completed its task? I hardly think so, thanks to the infinity of our ignorance. Among the real dangers to the progress of science is not the likelihood of

its being completed, but such things as lack of imagination (sometimes a consequence of lack of real interest); or a misplaced faith in formalization and precision...; or authoritarianism in one or another of its many forms.

Since I have used the word "progress" several times, I had better make quite sure, at this point, that I am not mistaken for a believer in a historical law of progress. Indeed I have before now struck various blows against the belief in a law of progress, and I hold that even science is not subject to the operation of anything resembling such a law. The history of science, like the history of all human ideas, is a history of irresponsible dreams, of obstinacy, and of error. But science is one of the very few human activities—perhaps the only one—in which errors are systematically criticized and fairly often, in time, corrected. This is why we can say that, in science, we often learn from our mistakes, and why we can speak clearly and sensibly about making progress there. In most other fields of human endeavour there is change, but rarely progress (unless we adopt a very narrow view of our possible aims in life); for almost every gain is balanced, or more than balanced, by some loss. And in most fields we do not even know how to evaluate change.

Within the field of science we have, however, a *criterion of progress*: even before a theory has ever undergone an empirical test we may be able to say whether, provided it passes certain specified tests, it would be an improvement on other theories with which we are acquainted. This is my first thesis.

To put it a little differently, I assert that we *know* what a good scientific theory should be like, and—even before it has been tested—what kind of theory would be better still, provided it passes certain crucial tests. And it is this (meta-scientific) knowledge which makes it possible to speak of progress in science, and of a rational choice between theories.

II

Thus it is my first thesis that we can know of a theory, even before it has been tested, that if it passes certain tests it will be better than some other theory.

My first thesis implies that we have a criterion of relative *potential* satisfactoriness, or of *potential* progressiveness, which can be applied to a theory even before we know whether or not it will turn out, by the passing of some crucial tests, to be satisfactory *in fact*.

This criterion of relative potential satisfactoriness (which I formulated some time ago, and which, incidentally, allows us to grade theories according to their degree of relative potential satisfactoriness) is extremely simple and intuitive. It characterizes as preferable the theory which tells us more; that is to say, the theory which contains the greater amount of empirical information or *content*, which is logically stronger; which has the greater explanatory and predictive power; and which can therefore be *more severely tested* by comparing predicted facts with observations. In short, we prefer an interesting, daring, and highly informative theory to a trivial one.

All these properties which, it thus appears, we desire in a theory can be shown to amount to one and the same thing: to a higher degree of empirical *content* or of testability....

IX

Like many other philosophers I am at times inclined to classify philosophers as belonging to two main groups—those with whom I disagree, and those who agree with me. I also call them the verificationists or the justificationist philosophers of knowledge (or of belief), and the falsificationists or fallibilists or critical philosophers of knowledge (or of conjectures). I may mention in passing a third group with whom I also disagree. They may be called the disappointed justificationists—the irrationalists and sceptics.

The members of the first group—the verificationists or justificationists—hold, roughly speaking, that whatever cannot be supported by positive reasons is unworthy of being believed, or even of being taken into serious consideration.

On the other hand, the members of the second group—the falsificationists or fallibilists—say, roughly speaking, that what cannot (at present) in principle be overthrown by criticism is (at present)

unworthy of being seriously considered; while what can in principle be so overthrown and yet resists all our critical efforts to do so may quite possibly be false, but is at any rate not unworthy of being seriously considered and perhaps even of being believed—though only tentatively.

Verificationists, I admit, are eager to uphold that most important tradition of rationalism—the fight of reason against superstition and arbitrary authority. For they demand that we should accept a belief *only if it can be justified by positive evidence*; that is to say, *shown* to be true, or, at least, to be highly probable. In other words, they demand that we should accept a belief only if it can be *verified*, or probabilistically *confirmed*.

Falsificationists (the group of fallibilists to which I belong) believe—as most irrationalists also believe—that they have discovered logical arguments which show that the programme of the first group cannot be carried out: that we can never give positive reasons which justify the belief that a theory is true. But, unlike irrationalists, we falsificationists believe that we have also discovered a way to realize the old ideal of distinguishing rational science from various forms of superstition, in spite of the breakdown of the original inductivist or justificationist programme. We hold that this ideal can be realized, very simply, by recognizing that the rationality of science lies not in its habit of appealing to empirical evidence in support of its dogmas—astrologers do so too—but solely in the *critical approach*—in an attitude which, of course, involves the critical use, among other arguments, of empirical evidence (especially in refutations). For us, therefore, science has nothing to do with the quest for certainty or probability or reliability. We are not interested in establishing scientific theories as secure, or certain, or probable. Conscious of our fallibility we are only interested in criticizing them and testing them, in the hope of finding out where we are mistaken; of learning from our mistakes; and, if we are lucky, of proceeding to better theories.

Considering their views about the positive or negative function of argument in science, the first group—the justificationists—may be also

nicknamed the “positivists” and the second—the group to which I belong—the critics or the “negativists.” These are, of course, mere nicknames. Yet they may perhaps suggest some of the reasons why some people believe that only the positivists or verificationists are seriously interested in truth and in the search for truth, while we, the critics or negativists, are flippant about the search for truth, and addicted to barren and destructive criticism and to the propounding of views which are clearly paradoxical.

This mistaken picture of our views seems to result largely from the adoption of a justificationist programme, and of the mistaken subjectivist approach to truth which I have described.

For the fact is that we too see science as the search for truth, and that, at least since Tarski, we are no longer afraid to say so. Indeed, it is only with respect to this aim, the discovery of truth, that we can say that though we are fallible, we hope to learn from our mistakes. It is only the idea of truth which allows us to speak sensibly of mistakes and of rational criticism, and which makes rational discussion possible—that is to say, critical discussion in search of mistakes with the serious purpose of eliminating as many of these mistakes as we can, in order to get nearer to the truth. Thus the very idea of error—and of fallibility—involves the idea of an objective truth as the standard of which we may fall short. (It is in this sense that the idea of truth is a *regulative* idea.)

Thus we accept the idea that the task of science is the search for truth, that is, for true theories (even though as Xenophanes pointed out we may never get them, or know them *as true* if we get them). Yet we also stress that *truth is not the only aim of science*. We want more than mere truth: what we look for is *interesting truth*—truth which is hard to come by. And in the natural sciences (as distinct from mathematics) what we look for is truth which has a high degree of explanatory power, which implies that it is logically improbable.

For it is clear, first of all, that we do not merely want truth—we want more truth, and new truth. We are not content with “twice two equals four,” even though it is true: we do not resort to reciting the multiplication table if we are faced with a

difficult problem in topology or in physics. Mere truth is not enough; what we look for are *answers to our problems*. The point has been well put by the German humorist and poet Busch, of Max-and-Moritz fame, in a little nursery rhyme—I mean a rhyme for the epistemological nursery:

*Twice two equals four: 'tis true,
But too empty, and too trite.
What I look for is a clue
To some matters not so light.*

Only if it is an answer to a problem—a difficult, a fertile problem, a problem of some depth—does a truth, or a conjecture about the truth, become relevant to science. This is so in pure mathematics, and it is so in the natural sciences. And in the latter, we have something like a logical measure of the depth or significance of the problem in the increase of logical improbability or explanatory power of the proposed new answer, as compared with the best theory or conjecture previously proposed in the field. This logical measure is essentially the same thing which I have described above as the logical criterion of potential satisfactoriness and of progress.

My description of this situation might tempt some people to say that truth does not, after all, play a very big role with us negativists even as a regulative principle. There can be no doubt, they will say, that negativists (like myself) much prefer an attempt to solve an interesting problem by a bold conjecture, *even if it soon turns out to be false*, to any recital of a sequence of true but uninteresting assertions. Thus it does not seem, after all, as if we negativists had much use for the idea of truth. Our ideas of scientific progress and of attempted problem-solving do not seem very closely related to it.

This, I believe, would give quite a mistaken impression of the attitude of our group. Call us negativists, or what you like: but you should realize that we are as much interested in truth as anybody—for example, as the members of a court of justice. When the judge tells a witness that he should speak “The truth, the *whole truth*, and nothing but the truth,” then what he looks for is as much of the *relevant truth* as the witness may

be able to offer. A witness who likes to wander off into irrelevancies is unsatisfactory as a witness, even though these irrelevancies may be truisms, and thus part of “the whole truth.” It is quite obvious that what the judge—or anybody else—wants when he asks for “the whole truth” is as much *interesting and relevant* true information as can be got; and many perfectly candid witnesses have failed to disclose some important information simply because they were unaware of its relevance to the case.

Thus when we stress, with Busch, that we are not interested in mere truth but in interesting and relevant truth, then, I contend, we only emphasize a point which everybody accepts. And if we are interested in bold conjectures, even if these should soon turn out to be false, then this interest is due to our methodological conviction that only with the help of such bold conjectures can we hope to discover interesting and relevant truth.

There is a point here which, I suggest, it is the particular task of the logician to analyse. “Interests,” or “relevance,” in the sense here intended, can be *objectively* analysed; it is relative to our problems; and it depends on the explanatory power, and thus on the content or improbability, of the information. The measures alluded to earlier ... are precisely such measures as take account of some *relative content* of the information—its content relative to a hypothesis or to a problem.

I can therefore gladly admit that falsificationists like myself much prefer an attempt to solve an interesting problem by a bold conjecture, *even (and especially) if it soon turns out to be false*, to any recital of a sequence of irrelevant truisms. We prefer this because we believe that this is the way in which we can learn from our mistakes; and that in finding that our conjecture was false, we shall have learnt much about the truth, and shall have got nearer to the truth.

I therefore hold that both ideas—the idea of truth, in the sense of correspondence with the facts and the idea of content (which may be measured by the same measure as testability)—play about equally important roles in our considerations, and that both can shed much light on the idea of progress in science....

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... What is the general problem situation in which the scientist finds himself? He has before him a scientific problem: he wants to find a new theory capable of explaining certain experimental facts; facts which the earlier theories successfully explained; others which they could not explain; and some by which they were actually falsified. The new theory should also resolve, if possible, some theoretical difficulties (such as how to dispense with certain *ad hoc* hypotheses, or how to unify two theories). Now if he manages to produce a theory which is a solution to all these problems, his achievement will be very great.

Yet it is not enough. I have been asked, "What more do you want?" My answer is that there are many more things which I want; or rather, which I think are required by the logic of the general problem situation in which the scientist finds himself; by the task of getting nearer to the truth. I shall confine myself here to the discussion of three such requirements.

The first requirement is this. The new theory should proceed from some *simple, new, and powerful, unifying idea* about some connection or relation (such as gravitational attraction) between hitherto unconnected things (such as planets and apples) or facts (such as inertial and gravitational mass) or new "theoretical entities" (such as field and particles). This *requirement of simplicity* is a bit vague, and it seems difficult to formulate it very clearly. It seems to be intimately connected with the idea that our theories should describe the structural properties of the world—an idea which it is hard to think out fully without getting involved in an infinite regress. (This is so because any idea of a particular structure of the world—unless, indeed, we think of a purely *mathematical* structure—already presupposes a universal theory; for example, explaining the laws of chemistry by interpreting molecules as structures of atoms, or of subatomic particles, presupposes the idea of universal laws that regulate the properties and the behaviour of the atoms, or of the particles.) Yet one important ingredient in the idea of simplicity can be logically analysed. It is the idea of

testability. This leads us immediately to our second requirement.

For, secondly, we require that the new theory should be *independently testable*. That is to say, apart from explaining all the *explicanda* which the new theory was designed to explain, it must have new and testable consequences (preferably consequences of a *new kind*); it must lead to the prediction of phenomena which have not so far been observed.

This requirement seems to me indispensable since without it our new theory might be *ad hoc*; for it is always possible to produce a theory to fit any given set of explicanda. Thus our two first requirements are needed in order to restrict the range of our choice among the possible solutions (many of them uninteresting) of the problem in hand.

If our second requirement is satisfied then our new theory will represent a potential step forward, whatever the outcome of the new tests may be. For it will be better testable than the previous theory: the fact that it explains all the explicanda of the previous theory, and that, in addition, it gives rise to new tests, suffices to ensure this.

Moreover, the second requirement also ensures that our new theory will, to some extent, be fruitful as an instrument of exploration. That is to say, it will suggest to us new experiments, and even if these should at once lead to the refutation of the theory, our factual knowledge will have grown through the unexpected results of the new experiments. Moreover, they will confront us with new problems to be solved by new explanatory theories.

Yet I believe that there must be a third requirement for a good theory. It is this. We require that the theory should pass some new, and severe, tests.

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Clearly, this requirement is totally different in character from the previous two. These could be seen to be fulfilled, or not fulfilled, largely by analysing the old and the new theories logically. (They are "formal requirements." The third

requirement, on the other hand, can be found to be fulfilled, or not fulfilled, only by testing the new theory empirically. (It is a "material requirement," a requirement of *empirical success*.)

Moreover, the third requirement clearly cannot be indispensable in the same sense as are the two previous ones. For these two are indispensable for deciding whether the theory in question should be at all accepted as a serious candidate for examination by empirical tests; or in other words, whether it is an interesting and promising theory. Yet on the other hand, some of the most interesting and most admirable theories ever conceived were refuted at the very first test. And why not? The most promising theory may fail if it makes predictions of a new kind....

Refutations have often been regarded as establishing the failure of a scientist, or at least of his theory. It should be stressed that this is an inductivist error. Every refutation should be regarded as a great success; not merely a success of the scientist who refuted the theory, but also of the scientist who created the refuted theory and who thus in the first instance suggested, if only indirectly, the refuting experiment.

Even if a new theory (such as the theory of Bohr, Kramers, and Slater) should meet an early death, it should not be forgotten; rather its beauty should be remembered, and history should record our gratitude to it—for bequeathing to us new and perhaps still unexplained experimental facts and, with them, new problems; and for the services it

has thus rendered to the progress of science during its successful but short life.

All this indicates clearly that our third requirement is not indispensable: even a theory which fails to meet it can make an important contribution to science. Yet in a different sense, I hold, it is indispensable none the less. (Bohr, Kramers and Slater rightly aimed at more than making an important contribution to science.)

In the first place, I contend that further progress in science would become impossible if we did not reasonably often manage to meet the third requirement; thus if the progress of science is to continue, and its rationality not to decline, we need not only successful refutations, but also positive successes. We must, that is, manage reasonably often to produce theories that entail new predictions, especially predictions of new effects, new testable consequences, suggested by the new theory and never thought of before. Such a new prediction was that planets would under certain circumstances deviate from Kepler's laws; or that light, in spite of its zero mass, would prove to be subject to gravitational attraction (that is, Einstein's eclipse-effect). Another example is Dirac's prediction that there will be an anti-particle for every elementary particle. New predictions of these kinds must not only be produced, but they must also be reasonably often corroborated by experimental evidence, I contend, if scientific progress is to continue....