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# I.—SENSATIONALISM

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SENSATIONALISM is the traditionally important doctrine according to which all our knowledge of the world comes to us through the senses. My aim is to systematize the traditional arguments against sensationalism, to show their incompleteness, and to supplement them with some modern arguments. Round the turn of the century a new version of sensationalism was proposed by Duhem and Meyerson and it is therefore not surprising that only modern criticism meets it. This version was constructed by people who had accepted the traditional arguments against the traditional versions of sensationalism. I shall now show that it is the last possible version, so that criticizing it may be considered to be criticizing sensationalism altogether.

There are two traditional divisions of sensationalism, yielding four possible sensationalist schools of thought. The first division is that between the sensationalists who think that informative theoretical knowledge is possible—the inductivists—and the sensationalists who think that informative theoretical knowledge is not possible—the conventionalists. The second division is that between naive and sophisticated sensationalists: the naive sensationalists, but not the sophisticated sensationalists, assert that all well attested reports of observation are entirely reliable. Both of these divisions of sensationalism, it should be noticed, are exclusive and exhaustive. It is essential for the criticism of sensationalism, if it is to be complete, to be the criticism of all the four versions, or classes of versions, mentioned above. The traditional criticism of sensationalism was the criticism of inductivism and of naive sensationalism: it left room for sophisticated conventionalism, which was soon constructed with incredible ingenuity by Duhem and Meyerson. Yet ingenious as it is, sophisticated conventionalism too has to be rejected, and with this sensationalism is completely superseded.

SENSATIONALISM

| | inductivism | conventionalism |
|---|---|---|
| Naive | Telesio | Poincaré |
| Sophisticated | Bacon | Duhem |

For the sake of simplicity the discussion of the present essay will be confined to our knowledge of the external world. The problem of our knowledge of ourselves will be avoided in order to avoid reference to any psychological theory except perception theory, and in order to avoid exegeses of the classical sensationalist text. The very famous dictum ' nothing is in the mind which has not been previously in the senses ' may be understood to refer solely to our knowledge of the external world. It is therefore understood here in this restricted sense, and criticized together with the four philosophical schools of thought which endorse it.

### (1) *Sensationalism versus theoretical knowledge*

According to sensationalism all knowledge of the world comes through the senses. This obviously entails that knowledge consists exclusively of observational reports and statements derivable from them. It is therefore inconsistent with the view that there exists theoretical knowledge about the world, since theoretical knowledge of the world is (analytically) that knowledge which is not derivable from observational reports alone.

Sensationalists are well aware of this criticism ; they view the problem of how to answer it as an integral part, if not the core, of the problem of induction. They vacillate between two alternative answers to this criticism, inductivism and conventionalism.

The inductivist view is that theoretical knowledge is ' indirectly ' derived from the senses, being based on observational reports by induction. Now, if induction were a purely deductive process, then theoretical knowledge would be implicit in the observational reports ; which is not the case. Therefore whether or not theoretical knowledge is gained by the process of induction, and whatever process induction may be, if theoretical knowledge exists then sensationalism is false.

Now, the inductivist may claim that although all factual knowledge is derived from the senses, a particular piece of knowledge may at first have to be a conjecture, and only then it has to be verified, or become a result of observation. Conjectures of a given kind are capable of certification by observation.

This theory—verificationism—is vague and dangerous. It is vague in not telling us whether or not the verified conjecture follows from the reports which have verified it and whether or not the verifying reports could be secured without any prior conjecture. Only when one adds that the conjecture does follow from the reports which in principle could be secured without any previous conjecture—only in this case is verificationism sensationalist. And in this case verification leads to no theoretical knowledge. Verificationism is dangerous because it raises false hopes. It raises false hopes because it can provide no assurance or guarantee that our conjectures will be verified. Sensationalism requires that the guarantee be verified ; this is the infinite regress argument already discovered by Hume. Moreover, the guarantee will make verifications of some conjectures unnecessary and thus it will conflict with sensationalism.

Take first the simplest case of one conjecture and a guarantee that we shall be able to verify that conjecture. As we have a guarantee that this conjecture is true, we need not verify it. Let us now replace the full guarantee by a partial one. Now, a partial guarantee is not helpful if it is consistent with utter failure ; hence it must be a perfect guarantee for partial success. This will be the guarantee that some conjectures of a given kind will be verified. This guarantee was presented by Keynes who has labelled it ' the principle of limited variety '. This principle is consistent with the case of refuting all the members of the given set of conjectures except one. Thus, it is quite possible that after a certain amount of failure the perfect guarantee for partial success becomes a perfect guarantee for full success. And this, as I have argued, renders the process of verification an unnecessary tedium, and is thus in conflict with sensationalism. Furthermore, any guarantee would render at least some questions of fact decidable without observations, namely, such questions of fact as the ones concerning the possibility and nature of human knowledge. Obviously, in this case we cannot claim that the view that human knowledge is possible is based on experience without begging the question.

The next retreat would be an attempt to replace the notion of verification of a conjecture by that of its confirmation, namely, the verification of some of its consequences. However, the problem reappears. We can have no guarantee for any verification whatever, not even for a verification of a weak consequence of a conjecture. If we had, then, again, we could construct a possible case in which the guarantee will render observation unnecessary. Moreover, the margin between the verification of a conjecture and its mere confirmation, namely the unverified consequences of an accepted hypothesis, would be the non-sensational element of human knowledge.

Thus, sensationalism is incompatible with the view that informative theoretical knowledge exists, no matter how it was acquired, and what its status is. As Hume has already shown, inductivism (be it correct or not) fails to reconcile sensationalism with the view that theoretical knowledge exists.

Sensationalists have realized (since the Middle Ages) that sensationalism implies that we cannot have theoretical concepts, that all our concepts are either those derived from observations, or their combinations ; that even in our wildest imagination we cannot fancy anything but new combinations of old observational material, so that all concepts are, like the concept 'sphinx' (to take Bacon's example), merely combinations of observational concepts. Indeed, this is Hume's starting point. Einstein's and Russell's favourite argument against it is the essentially Kantian idea that mathematical concepts go very far beyond any past experience, and that some of these concepts are employed very fruitfully in science.

Yet this very criticism gives the cue to the alternative sensationalist view, namely conventionalism. Conventionalism gives great scope to the imagination, and views both mathematics and theoretical science as admirable structures produced by the imagination. But in admitting that theories go beyond experience conventionalism empties theories of all factual or empirical content. It denies that theories are empirical or factual or informative. It claims that a theory is not informative knowledge but our way of looking at particular facts, our way of classifying particular observed facts. Like mathematics, theoretical science is merely an empty structure to store information in, a way of saying things, a language. Nothing in reality strictly corresponds to abstract or imagined theoretical concepts like ' space curvature ' or ' atom '. These words are no more than shorthand symbols with no independent meaning (their meanings are given by implicit definitions), and statements containing them impart no more information than the information procured by sensations alone.

Although conventionalism is a much clearer and more coherent view than inductivism, it was traditionally viewed by men of science as a deafeatist position, because the aim of science, it was felt, was not just to replace an unordered or an arbitrarily ordered heap of information by an elegantly ordered yet not richer stock of information. The intuitively accepted view behind the scientific tradition between 1600 and 1900 was that there exists a hidden reality (*i.e.* hidden from the senses) and that the aim of science is the search for it, to wit, the attempt to discover the laws of nature and not the laws of elegant and concise languages. Although hardly any of the classical natural philosophers adhered to conventionalism consistently and persistently, many of them used it as a second best alternative to inductivism. They used conventionalism as a temporary refuge for they wished to retain their sensationalism, which they viewed as the basis of empiricism, as the ground for the validity of empirical science. But this view is mistaken.

## (2) *Sensationalism versus empiricism*

If we assume that theoretical knowledge is possible, then we may inquire into the grounds for its validity. The two traditional answers to the problem of the grounds for the validity of theoretical knowledge are apriorism and empiricism. Empiricism is traditionally based on sensationalist assumptions ; but the converse is not true : conventionalism is sensationalist and yet it is neither apriorist nor empiricist. By entailing the denial of the existence of informative theoretical knowledge, conventionalism avoids giving rise to the problem to which empiricism and apriorism are the traditional alternative solutions. Yet, it being a sensationalist view, traditional empiricists prefer conventionalism to apriorism, and were even ready to use it as a temporary refuge when their empiricism was beaten, hoping that with the increase of the amount of factual information they would be able to return to their empiricism in order to find informative theoretical knowledge about the world.

Traditional natural philosophers have always emphasized the significance of empirical theoretical knowledge. The most often quoted passage of Bacon's was his parable of the ant, the spider, and the bee: the empiric or sceptic who has only reports of observed facts is like the ant which only collects; the reasoner or apriorist who has only theories is like the spider which only spins out its own material; the interpreter of nature, the true empirical theoretical philosopher, is like the bee which both collects and adds something of its own to the collected material. (This parable, says Russell, is unfair to the ant. If so, it is also unfair to the spider.) In another famous passage Bacon speaks of science as the wedding of the intellect and the senses.

These metaphors conceal a problem. Admittedly the contribution of the senses is empirical. But what is the contribution of the intellect? Is it not the case that the contribution of the intellect is non-empirical? Is not the idea of empirical theoretical science self-defeating?

Bacon must already have been aware of this problem, for he gave an answer to it (in his Preface to *The Great Instauration*). His answer is this. Just as by sensing the rays of light our eyes see things, so, by analogy, by sensing things our intellect sees the laws of nature. This answer is a traditional mystical or intuitionist view which assumes the existence of a mental eye that sees or intuits laws with complete assuredness just as the eye of the flesh sees things with complete assuredness. (The traditional mystic formula is that of the unity of the knower and the known with knowledge; it occurs in a crucial passage of Bacon, in his *Novum Organum*, II, Aph. 19.) No wonder that no later empiricist shared this view with Bacon. The problem remains, then: how is empirical theoretical knowledge possible?

The obvious substitute for Bacon's answer is the view that the senses provide the material and the intellect the order. But this answer is the denial of empiricism. It is either conventionalist, if the order which our mind provides is claimed to be merely ours, or apriorist, if that order is claimed to coincide with the order of the world. (Kant seems to have vacillated between these two claims and preferred to leave the choice between them undecided. He stressed that the order is provided by us, but left open the question of whether this order of ours coincides with the order of the world or not.)

Another answer is this: when the senses make their own contribution they stimulate the intellect to make its own contribution. But this is not to the point: the apriorists themselves, since Bruno and Descartes, have always asserted that the senses may stimulate the intellect; they only denied that the senses are the source of knowledge; they declared that the intellect makes a contribution, and that we can see the independence of the validity, or the self-evidence, of this contribution. Clearly, they would argue, if any part of the contribution of the intellect is independent of the senses to any extent apriorism is not excluded, while if the contribution of the intellect depends entirely on perceptions it cannot add to the information which can be provided by the senses.

This may explain the fact that quite a few great thinkers became apriorists. I think it is cheap to ridicule eminent apriorists (Descartes is the traditional scapegoat) whenever the validity of your own brand of empiricism is challenged. The function of the repeated sneer at apriorism is to drive home the idea that any deviation from narrow sensationalism leads towards apriorism. This idea does not solve our problem, however, but rather sharpens it. For we can state the dilemma in this way: if we do not go beyond sense experience we have no theoretical knowledge of the world, while if we do go beyond it the margin is not contained in sense experience, and is, thus, *a priori*.

This is the logic which led thinkers to abandon empiricism in favour of either apriorism or conventionalism. For according to both these views our present theoretical knowledge necessarily transcends our experience; they differ only as to the question of whether this knowledge is informative (apriorism) or not (conventionalism).

Yet empiricist philosophers who have studied the problem of knowledge have usually stuck to their sensationalism in spite of this refutation. Perhaps they hoped that somewhere a logical error had been committed in the refutation of sensationalist empiricism. They were unable to refute any step of the criticism, but they had a strong argument in favour of the view that a logical error could be found in the criticism. The argument is this. In our ordinary behaviour we show that we consider theoretical science as informative, for we normally rely on theoretical information. Moreover, we show that this information is indeed connected with experience, for if theoretical information clashes with the information gained by experience we prefer the latter; we accept theoretical information only when it is strongly supported by experience. Furthermore, we gain theoretical knowledge or at least theoretical hints from certain important experiments, like that of Michelson and Morley. In brief, we know that the error is there, since we know that we gain theoretical knowledge from experience.

I readily accept this last contention, although it is in no way imperative to accept it. I also admit that this contention—we gain theoretical knowledge from experience—is the core of empiricism and amounts to the rejection of both conventionalism and apriorism. None the less, I deny that there exists an error in the refutation of sensationalism. For, the refuted thesis, sensationalism, is the contention that we learn about reality only from sense experience, while the thesis of empiricism is the contention that we learn about reality only from experience. Hence, either the identification of all experience with sense experience is an error, or else empiricism is inconsistent : only if we get rid of this identification we may retain empiricism. I shall now discuss the different ways in which experience was identified with sense experience and argue that this identification leads to the surprising conclusion that we cannot describe our experiences, that we never know what experience is!

### (3) *Sense-experience versus experience*

The identification of experience with sense experience has been done in a naive way and in a more sophisticated way. The naive identification of experience with sense experience is a version of naive realism. It is simply the claim that we see things as they are. It was admirably criticized by many modern philosophers from Galileo and Kant to Einstein and Russell. An elegant argument against it is, perhaps, Schroedinger's argument (in *Nature and the Greeks*). We see the sun as being not much bigger than a cathedral. Assuming that the sun is as big as we see it, and accepting very simple, and intuitively quite obvious, trigonometrical theorems, we can calculate the distance between the eastern and western positions of the sun and find it to be no more than one day's walking distance.

This argument does not convince the adherents of naive sensationalism. Naive sensationalism carries great force with it. Even if we do not see all things precisely as they are, we all admit that we can see this table tolerably well (arguments from perspective notwithstanding). We admit that endless speculations and disputations will not be useful to determine some question of fact which can easily be determined by plain observation and experiment. Surely that much we all admit, and it is, I suppose, the core of what the naive sensationalist wishes to assert.

Many historians of physics and a few physicists claim that the medieval scholars were apriorists and use against them the above naive sensationalist criticism which is no more than putting to ridicule those who never rely on their eyes but prefer *a priori* reasoning to plain observation. Undoubtedly, it is often preferable to rely on one's eyes than on one's reasoning ; and undoubtedly naive sensationalism justifies this preference. Yet naive sensationalism is plainly false, and this (correct) preference of observation over reasoning needs a better explanation or justification. We do not rely on the eye of the mind because it may mislead us, and to be fair we should not rely on the eye of the flesh as it may mislead us too. Why then do we sometimes rely on our senses but never on our intellect? When the sensationalist becomes aware of the fact that experience can mislead us, instead of ceasing to rely on experience he claims in a sophisticated manner that there must exist some kind of experience—pure experience, as he calls it—which cannot mislead us.

There exist strong versions of sophisticated sensationalism. They specify which, or what kind of, experiences cannot mislead us. The weak version of sophisticated sensationalism is the mere assertion of the existence of some kind of reliable experience. There exist two historical examples of strong versions of sophisticated sensationalism, Bacon's and Locke's. Bacon's position is that the theoretical element of experience is what misleads us, not the sensationalist element of experience. Once we get rid of all our prejudices, of all of our preconceived ideas, we can experience things as they are (in the manner assumed by the naive sensationalist).[1] Locke's view is that the reliable experiences are the elements of individual sensations, which were later called sense-data; they are pure sensations like the sensations of sounds or the sight of coloured patches. Bacon's doctrine can be shown to be inconsistent, for it is itself a preconceived idea. Locke's view has been experimentally refuted. The identification of patches was shown to be not independent of our knowledge of geometry and perspective, the identification of a colour shown to be not independent of our language, and even the ability to distinguish between two similar sounds depends on theoretical instruction. Thus, although sophisticated sensationalism as such—the view that there exist pure sensations—is irrefutable, its two more substantial versions—Bacon's and Locke's—are logically or empirically refutable, and they were refuted.

No one doubts that sensations are a necessary part of any experience. My contention is that we cannot specify a type of experience which must be fully reliable; that in particular we are not, and cannot be, aware of pure sensations; that there exists no immediate or direct sense experience, just as there exists no immediate or direct experience of the electric signals

[1] If you substitute 'class-prejudice' for 'prejudice' in the statement above you get Marx's view, and if you substitute 'neuroses' instead you get Freud's view.

which, according to modern neuro-physiology, sensations consist of. It is a fact of experience that when describing or reporting scientific experiments we very rarely describe or report our sensations.

This fact a sophisticated inductivist will readily admit, and yet he will claim in describing our scientific experiments we do report our sensations, even though indirectly. Here again our inductivist runs against our dilemma, and again he refuses to consider it, being sure that we learn from experience and that experience must be, ultimately, sense experience. Rather than resolving the dilemma he tries to purify a given report of a scientific experiment of its theoretical element and reduce the scientific report to a report about past sensations. Yet when trying to do this he soon uses theories and statements of objective facts rather than reports about sensations. He will justify his use of statements of objective facts by claiming that they were once constructed out of pure-sense-elements—thus assuming what he has set out to prove. He will also justify his speaking of facts by defending naive sensationalism. He will then retreat from naive sensationalism to a sophisticated one, and so on. It is a historical fact that very few thinkers ever tried to show how a given piece of scientific information can be decomposed into, and recomposed from, sense-perceptions ; such attempts, notably Laplace's, Mach's and Russell's, were complete failures because of their authors' vacillation between naive and sophisticated views, as well as between inductivist and conventionalist views.

This argument leads inductivists to two characteristic reactions. The one is to try again. The other is to dismiss the whole debate as too sophisticated. In order to show that it is not unnecessarily over-sophisticated I shall take an example of the inductivist's muddled approach to experimental errors.

It is a well-known fact that John Dalton reported having observed the atomic weight of oxygen to be, on the average, near to but slightly above 6·5 and decided that it is actually 7. Obviously, he could not get the result which we have today, namely 16, because he thought that water contains oxygen and hydrogen in equal proportions and not, as we think today, in the ratio of one to two ; but better experiments, it is alleged, might have led him to the result 8 rather than to 7. It is therefore unanimously accepted by modern historians of science that Dalton was a bad observer, Dr. Thomas Thomson's personal testimony to the contrary notwithstanding. It is difficult to imagine that a bad observer was the inventor and improver of experimental techniques in weighing gases. If the historians who condemn Dalton were serious about the whole matter they would have tried to repeat Dalton's experiments as his contemporaries did. In this case they would undoubtedly get the same result as Dalton's, just as Dalton's contemporaries did before Davy discovered a better method which yielded the result 7.5.

It is obvious to me that Dalton's result is respectable and yet untrue. He who doubts it will have to apply the same doubt to the results of all nineteenth-century chemical experiments. The best and most precise experiments concerning the atomic weight of chlorine then gave 35·5 as a result, and they were equally mistaken; the atomic weight of chlorine is much nearer to either 35 or 37 than to 35·5. The naive and sophisticated inductivists alike must fail to explain all this. In order to explain why our predecessors accepted and we reject 35·5 as the atomic weight of chlorine different theories have to be referred to. It transpires, then, that contrary to all we were taught in chemistry classes and in history of science classes and in philosophy degree courses, it was the factual report which has been declared to be false in the light of modern theories. But before trying to defend this startling conclusion I wish to discuss the sophisticated conventionalists' explanation of this situation. For it is the great advantage of sophisticated conventionalist that he handles this situation with great ease.

### (4) *Sensationalism versus common sense*

The criticisms of naive sensationalism and of inductivism which I have presented so far do not cause the slightest difficulty to the sophisticated conventionalist. He does not claim that theoretical knowledge is derived from experience but neither does he claim that theoretical knowledge is informative. He therefore can easily reconcile the existence of uninformative theoretical knowledge with sensationalism. He does not claim any observation report is purely sensational, so that he can stick to his reliance on the senses in spite of all the alterations which the observation reports undergo. The major defect of this position seems to lie in the fact that it sounds just too defeatist a position, defeatist both regarding theory and regarding observation. But this is an error: defeatism regarding theory is quite sufficient. Sophisticated conventionalists can argue that even though we cannot separate the sensational element in any observation statement, this element is invariant regarding any translation of a report from one language to another. The nineteenth-century observation report 'the atomic weight of chlorine is 35·5' is not discarded by modern chemists but is translated by them into the twentieth-century language; the translation reads: 'the average atomic weight of terrestrial chlorine is 35·5.' The translation of the report preserves its sensational element. The sensational element has not been rejected, only the theoretical element has been replaced. The twentieth-century report 'the atomic weight of chlorine is 35 or 37' does not contradict the nineteenth-century report 'the atomic weight of chlorine is 35·5': they are cast in different languages, and forgetting this fact we rashly conclude that they contradict each other. Before we can find out whether they are in contradiction or not we must state them both in one and the same language. Now we cannot easily translate the twentieth-century report into the nineteenth-century language, because the later language is better—more elegant—than the older language. So it is more convenient to translate the nineteenth-century report into the twentieth-century language. As we have seen, the translation shows the two reports to be perfectly compatible with each other.

It is essential for this mode of thought that it is both conventionalist—in viewing theoretical science as a mere system of languages—and sophisticated. Had we been able to state one observational report with no theoretical overtones, then the problem which the sophisticated conventionalist has solved would have arisen in a very different manner and his solution to it would be obviously unacceptable. And if we wish to conclude that it is impossible to have purely observational reports, we must assume that even though we can translate a report into many languages without losing or altering its sensational content, we shall never be able to isolate this sensational element entirely. No doubt, had we constructed all the possible languages, and had we then stated one report in all these languages, the 'conjunction' of all these many statements of this one observation report should give us a fair idea of the observation as such. But this is merely a thought experiment: there can be infinitely many languages, or theoretical systems, for any finite set of observation reports to be expressible in.

This discussion seems to me to clarify a number of points. First, it explains why sophisticated conventionalism never was popular: it is somewhat too sophisticated. Second, it explains the modern search after pure observation reports. Any sensationalist alternative to Duhem's and Meyerson's doctrine must contain the claim that we can isolate sense impressions from the theoretical element with which it amalgamates when presented in a scientific report. Yet in order to be convincing one must indicate how this can be done. Now Popper has argued (1935) that, since universal names are dispositional, reports containing them contain predictions, and are thus no pure reports. *E.g.* the report ‘ here is a glass of water ’ contains predictions since the glass is breakable or else we would not call it ‘ glass ’, and water is decomposable, etc. Hence the immense literature concerning dispositions and dispositional terms which has followed Carnap's study (1936) of the relations between dispositions and pure observations.

Yet one should notice, perhaps, that the sophisticated view according to which we cannot separate the sense information from the theoretical element in an observation report, though unpopular amongst philosophers, has gained popularity amongst some schools of contemporary psychology. This is so, partly, I suppose, owing to the fact that psychologists cannot evade problems concerning observation reports as easily as other scientists : such troubles are their business. Partly it is due to the influence of Külpe's critical realism. The full discussion of this point is beyond the scope of the present essay ; yet this much can, and ought to, be said here. The sophisticated view according to which we cannot separate the sensational and the theoretical element in an observation report is not in itself intolerable ; it is intolerable only when we adopt it together with an inductivist or with a conventionalist attitude. For, when we adopt it together with a critical realistic attitude, we merely admit that any observation report must contain some hypothetical element ; only when we adopt it together with a conventionalist attitude it turns out that we do not quite know what we are saying. For, according to sophisticated conventionalism, only the sense element is informative, and the sense element is unisolable ; hence, according to sophisticated conventionalism, the information contained in a report is unisolable. The analogous realist attitude only entails that the certain and entirely warrantable element of a report is unisolable ; namely, that observation reports are never certain.

To make this clearer, consider an observation report stated in court. To say that the judge does not quite know what is the information he receives from a witness is very disquieting. Moreover, it is not at all difficult to imagine, or to draw out of history, a case in which a piece of evidence would condemn the accused when cast within one theoretical system, and acquit him when cast in another. To critical realists this causes no trouble, since they permit error in any observation report. But not so to the sophisticated conventionalist.

Duhem was not unaware of this difficulty, for he tried to solve it. He suggested that naive sensationalism should apply to commonsense situations (such as the one described above) and sophisticated sensationalism to science. His argument in favour of this suggestion is too involved to reproduce here. Nor need it be reproduced. For this division between science and common sense cannot be maintained, especially in the light of modern perception studies, of the Külpe school and its derivatives. Today's common sense, as Maxwell has already claimed, is yesterday's frontier of science.

The sophisticated conventionalist may attempt to answer this criticism, but the more he will do so the more he will defeat his own purpose, for he is presenting a more and more elaborate theory about science and its role in society—a theory which he must consider as informative, and which entails that it is itself uninformative. As long as we only look at scientific theories we may suggest viewing them as empty—as Duhem does. But we cannot merely suggest to a judge that he view scientific theories as empty ; we have to explain to him why he should do so by providing a theory of sorts ; and he will rightly apply this theory to itself in order to dismiss it as empty.

This discussion explains, I hope, why Poincaré, by no means an unsophisticated philosopher, preferred naive sensationalism to sophisticated sensationalism ; it is untenable to claim that we do not quite know what we say when in ordinary circumstances we state a simple and unproblematic observation report ; nor is it tenable to divorce such reports from scientific enquiry. But though Poincaré's rejection of sophisticated sensationalism is well founded in common sense, his acceptance of naive sensationalism was a serious error.

We have now come to the end of the list of traditional alternatives. Those philosophers who pin their hopes on the future success of present-day efforts to discover pure observation reports hope to erect a new inductivist epistemology or a less sophisticated conventionalism than Duhem's. The rest are faced with the choice between apriorism, sophisticated conventionalism (a Kantian vacillation between the two), or the search for a revolutionary approach. The notorious conservatism of the bulk of philosophers (plus the unpopularity of apriorism and sophisticated conventionalism) is my only explanation for the popularity of the search for pure observational reports, for observable hard and fast facts, in the face of the increasing amount of evidence from modern psychology, from modern perception theory, which shows the futility of this search. Psychologists are usually unaware of the philosophical implication of their studies, of the fact that their studies give rise to the need for a new epistemology. But then they are usually not interested in this aspect.

The claim that there are no pure (or 'neat') observation reports is central to Ryle's argument in his *Concept of Mind* (1949). By implication Ryle also rejects sophisticated conventionalism (when he denies the existence of a fundamental difference between common sense and science, pp. 288 ff.). He thus faces *the* problem of epistemology, and he is well aware of it: he sketches a programme for a new epistemology (pp. 317-318). This need for a new epistemology is rooted not in Ryle's central doctrine, in his proposed solution to the body-mind problem, but in his revolutionary perception theory. Popper, who dissents from Ryle's solution to the body-mind problem, but shares Ryle's perception theory, had outlined over a decade earlier a similar, if not the same, programme, and also proposed theories which answer the *desiderata* of that programme. It is regrettable that this logic of Ryle's argument has not been clearly seen by the general philosophical public. It is this oversight which is responsible for the popular view of Ryle's doctrine as a version of behaviourism, even though he explicitly rejects behaviourism because it is based on the naive belief in pure observation reports. It is the same oversight which is responsible for the popular identification of Ryle's psychological theory of knowledge as a set of dispositions with Mill's and Schlick's similar epistemological theory of knowledge as the proper procedure of connecting past and future events. (The two sets of theories of knowledge obviously come to solve two quite different sets of problems. The epistemological theory answers questions of status and basis of validity, the psychological theory answers questions of the seat of knowledge and its influence on behaviour. It is regrettable that Ryle's metaphors allow for the confusion of his view with Schlick's.)

Popper's new theory of the status and methods of science is opposed by many philosophers because it entails the non-existence of pure observation reports. This, as we have seen, is a very scanty ground for opposition. Others find it difficult to share his reasons for the acceptability of some observation reports in spite of their inherent uncertainty. It is this last point which I now wish to discuss in some detail.

## (5) *Explanation versus consent*

The whole literature concerning the methods of science seems to be agreed on one point, which I shall now try to criticize. It is agreed amongst philosophers that when it is said that a certain piece of information is scientifically acceptable it is meant that the piece of information in question ought to be accepted as true—to be believed. At least I have never come across any philosopher who has contested this. Popper has stressed that this acceptance must be tentative ; but even he agrees that accepting a report is, for the time that it is accepted, considering it to be true. My own alternative is that observation reports ought to be accepted as a task, as something which we should try to explain, and this does not exclude the possibility that we should explain that piece of information as based on an error. This forces us to admit, I shall argue, that the problem of observation, the problem of why an observation report was made, and what is our guarantee that it is true, belongs to science and not to philosophy.

Science deals with factual information, but not with all factual information and particularly not with information concerning miracles. Much has been written about the difference between scientifically acceptable and scientifically unacceptable information, and none of it seems to me satisfactory. Let me first state the difference and then discuss it. The bare facts of the matter seem to be these. In 1661 Boyle published an essay ' On the unsuccessful Experiment ' (in his *Certain Physiological Essays*) in which he ruled that science has nothing to do with unrepeatable experiments, that if we cannot repeat an experiment which someone claims to have performed we do not have to call him a liar or explain his claim in any other way—we can simply ignore it until it is reported to have been repeated by others. This proposal of Boyle has become a part of the scientific tradition. Although very few philosophers have discussed this situation, every physicist is well aware of it. Yet this situation should have been discussed more often, as it is problematic : the claim that any experiment is repeatable is a mere hypothesis. Boyle himself was extremely worried about this, because he thought that only factual information is certain to some degree (' morally certain '), and that factual information is therefore always to be preferred to a hypothesis with which it clashes. Yet as the rejection of a hypothesis is based on the acceptance of an observation report and the acceptance of the observation report is based on the hypothesis that it is repeatable, it follows that we reject a hypothesis not on the basis of solid facts but on the basis of another hypothesis. Is this not too arbitrary?

That the repeatability of an experiment is hypothetical can be shown by general considerations and by historical example. The general considerations are these: a description of an experiment is a description of the circumstances in which a certain event takes place, and a report of the experiment is the statement that at a certain time and place under the said circumstances the said event was indeed observed. Now many other circumstances were observed at the same time and place, of which there is no record and yet which may be, and sometimes are, essential to the success of the experiment. Thus, the success of the nineteenth-century experiments which made chemists think the atomic weight of chlorine to be 35·5 depended on circumstances which they did not notice but which we can vary today and thus approximate any result between 35 and 37 as we wish.

Boyle was aware of this difficulty. He demanded that we should report as many of the circumstances under which the experiment has been conducted as we can, and that we should vary the circumstances as much as possible. But the more circumstantial the description, the less repeatable is the described experiment; we do not know all the circumstances; we cannot vary all of them; and we cannot even report all of those which we notice. Boyle's last and posthumous publication, *Experimenta et Observationes Physicae,* is burdened with superfluous descriptions of irrelevant circumstances. Yet in his preface to it, which he probably wrote on his death-bed, he expressed the fear that negligently he had omitted some relevant circumstances, thus rendering his own experiments unrepeatable.

The cause of this insoluble problem of Boyle is, I suggest, his rule, according to which whenever a hypothesis and a report of a repeatable experiment contradict each other it is the hypothesis which has to be thrown overboard. To my knowledge nobody has contested this rule. Even Popper, the first philosopher who has stressed the utter and inescapable tentativity of all observation reports, has accepted Boyle's rule. Yet the rule has to be rejected. Here is a historical example of a case in which the rule was at first correctly broken and then mistakenly adhered to.

In 1815-16 Prout published his celebrated hypothesis, according to which the ancient philosophers' primordial matter is identical with hydrogen. According to this hypothesis the chemical atoms are not quite atoms, or indivisible, and their atomic weights must be multiples of the atomic weights of hydrogen atoms, namely whole numbers. Prout's essay is full of experimental evidence, not his own but compiled from the most up-to-date works of the leading chemists of his age. None of these results agreed with Prout's hypothesis very well, and some of them did not agree with it at all. Yet he evidently considered these results as quite encouraging.

A short time later a youngster, Jean-Servais Stas, heard about this hypothesis and, to use his own words, fell in love with it. Like Prout he hoped that with the improvement of the available experimental techniques the results of the measurements of atomic weights would converge towards the results predicted by Prout. Stas soon became the greatest expert in the field. His results did not agree with his expectation, and they broke his heart: he declared that his loyalty to science stood above his loves; consequently he gave up Prout's hypothesis. One may remember that some of the techniques by which isotopes are isolable were available to Stas. Had he insisted that the atomic weight of chlorine cannot be 35·5 he might have suggested that chlorine is a mixture of two physically different though chemically identical substances. But unlike Prout, Stas refused to stick to the hypothesis in the face of known facts in the hope that the facts will adjust themselves to theory rather than the other way round.

This example shows that we have to improve upon Boyle's rule. I suggest that Popper's theory allows for a new rule. According to Popper's view scientific theories are explanatory and testable, and the more highly explanatory and testable they are the better. This view seems to me to have gained a sufficiently wide recognition to enable me to use it without any preliminaries. My present discussion, if correct, renders Popper's theory of the empirical basis of science superfluous; this theory (Section 29 of his *Logic of Scientific Discovery*) is perhaps the subtlest and most intriguing part of his study, but it is also unsatisfactory in its very subtlety, and the cause of most of the criticisms and the misunderstandings of his views. I am glad it can be dismissed without any loss.

As Popper has argued, the demand for high testability leads to the demand to exclude the explanation of a series of successful repetitions of an experiment as due to chance. As he has also noticed, the demand for testability justifies the rule according to which unrepeatable experiments should be ignored, since repetitions are a kind of test. This led him to the tacit assumption, which I propose to reject, that results of repeatable experiments must be (tentatively) accepted as true. That he does make this assumption, though tacitly, can be seen in his acceptance of Fries' claim that as the acceptance of observation reports should not be dogmatic it must be justified. This is a sensationalist relic in his theory. It led him to agree with Fries that the attempt at a justification leads either to an infinite regress or to a sensationalism. His own solution to the problem is that although we do not go on for ever testing observation reports by repeating the observation, we can do so when and if challenged. Hence, says Popper, there is an element of dogmatism or conventionalism in the acceptance of the report, since it may be false, as well as a sensational element, as it is causally related to sensations, as well as an element of (potential) infinite regress, since the possibilities of testing it are inexhaustible.

All this can be ignored, I propose. We need speak neither of acceptance, nor of justification of acceptance, of any observation report. We merely have to demand that account be taken of the fact that some observation reports were made repeatedly, and that this fact be explained by some testable hypotheses. The demand to explain given observation reports by highly testable hypotheses entirely suffices. If the most testable hypothesis explains given observation reports while assuming them to be true, which is sometimes the case, we choose that hypothesis. Yet the most testable hypothesis may explain the observation reports as being the results of crude measurements, as Prout's hypothesis did; or as results of sense illusions, as many psychological hypotheses do; or as results of specific initial conditions or specific circumstances, as Einstein's relativity did; or as lies and propaganda—remember the totalitarian scientists! There is no empirical reason to reject such hypotheses on the basis of past experience; rather we go and test them by having recourse to new experiments.

To put it differently, it is not for the general theory of scientific experiment to explain why an experimental report was made, since the possible and even the actual explanations are varied. It is the task of a scientific hypothesis to do this. In particular, we must consider as false Boyle's, Fries', and Popper's view, according to which all (repeatable) scientific observation reports are explained (tentatively or not) as true, and are therefore preferred to hypotheses which conflict with them. Whenever a report is made repeatedly, a scientific hypothesis which explains why it was made is sought for. And of all those specific hypotheses which are found, that one is preferred which is more testable than the others. Thus, when Mercury was reported to deviate from its Newtonian path, a few explanations of it were offered. One explanation of the report was based on the assumption that the observation was inaccurate, *i.e.* that the report was false. Another on the assumption that the initial conditions in the vicinity of the sun are more complicated than previous observers had assumed, *i.e.* that the report was true, but that other reports were false. Both these explanations incorporated Newton's theory of gravity. Yet another explanation of the situation was Einstein's theory of gravity. And the latter was preferred and tested, as it was the most easily testable. The preference for Einstein's theory over the other two alternatives was definitely not based on the fact that the other alternatives incorporated the assumptions that some previous observation reports had been inaccurate: Einstein's theory incorporated the assumptions that practically all observation reports had been inaccurate, of course. If this were not so, practically every observation report would refute the theory which the observation came to test: hardly any observation ever fully agrees with the prediction which it comes to test.

The philosophical problem of the acceptability or otherwise of observation reports can thus be entirely ignored by non-sensationalists; no philosophical problem even corresponds to it outside sensationalism. Instead, many scientific problems correspond to it: in each field of enquiry investigators have to explain all repeated observation reports, and they may explain them as true, as approximations, or as sheer fancy. These explanations are not justifications and therefore should be suspected, and therefore should be tested. Indeed, the assumption that a new theory contradicts an older observation report is itself a suggestion of how that theory may be tested, namely by repeating the older observation with a higher degree of accuracy. This has been recently noticed by Popper (in his 'The Aims of Science'). But he did not notice, I think, that this amounts to the admission that observation reports may be accepted as false and that hence the problem of the empirical basis is thereby disposed of, which is my proposed view.

This proposal of mine severs the last connection between the philosophy of experience and sensationalism, by suggesting that philosophy should not include the attempt to discuss the causes of observation reports. Consequently my proposal sounds dangerously idealistic. I wish to argue that, on the contrary, it is the most realistic approach to experiment that has ever been proposed.

## (6) *The roots of scientific realism*

The chief objection to my view would be that it is idealistic. But it is not idealistic ; it leaves it to scientific hypotheses to say whether an observation report is true, near to the truth, or utterly false ; it leaves it to scientific hypotheses to say whether a specific observation report was stimulated by sensations emanating from things, by hallucinations, by dreams, or by the desire to achieve fame. My view may sound idealistic, but only because it trusts science to take care of realism ; which science is doing very well.

But why is science realistic? The generally accepted answer is that scientists have a metaphysical faith in the existence of things physical. Following Popper I consider this answer as true but unsatisfactory : beliefs may dictate our acceptance of the scientific discipline, but the question is whether this discipline leads to realism, or whether we must add to this discipline a disposition towards realism in order to obtain science as we know it. Clearly this disposition is inessential. It is a simple fact that whether you are a realist or not, you must admit that the method of science alone already pushes you towards handling realistic hypotheses, whether you like them or not, whether you accept them or not.

The reason for this fact is very simple. Idealism is just one way of looking at our experiences, and a way whose importance was immensely exaggerated by contrasting it with all the infinitely many alternatives to it as if it were a contrast between merely two views, idealism and realism. The reason for this exaggeration is, of course, the claim, which I endorse, that sensationalism leads one irresistibly to idealism. But once we ignore sensationalism, idealism becomes one of the very many uninteresting ways in which we may try to account for our experiences. As Lewis Carroll knew, we can say not only that the world is my dream, but also that the world is his dream.

The scientific accounts of experience, then, are realistic plainly because they all differ from one historically famous though unscientific account of our experiences—idealism—an account which leads us nowhere, and which was considered significant because of its close relation to sensationalism. As all versions of idealism are untestable, and scientific theories are highly testable, scientific theories are not idealistic, *i.e.* they are realistic. But is not our predilection for highly testable theories rooted in realism? The answer to the question as I have put it is, No. Science is realistic; more precisely, some versions of realism are scientific; but not all versions of realism are scientific. Realism alone is thus merely the rejection of idealism; it leads no more to science than to animism in any of its most primitive versions. We can be realists without wishing to explain or to test our explanations, but not *vice versa*. Let me show this by the following argument.

One may still feel that my attempt to ignore the general question of why observation reports are made is unrealistic; it may be unrealistic in a somewhat narrower and more naive sense than in the sense of being philosophically idealistic. One may suggest not that the whole world is my dream but merely that the scientific world is a dream. Do I allow for the possibility of a mock-science, of a situation in which some people build laboratories and state observation reports and some people try to explain them, but no one ever bothers to observe?

Let us take this possibility seriously for a moment, although it is puerile. I fear that it may have played an important role in the history of the philosophy of science even though it was never explicitly and carefully discussed (except, perhaps, by Bacon; he warned people against making reports without observing first; which, incidentally, is precisely what he himself did). Let us consider the hypothesis—call it hypothesis B—that there are very few observations and experiments going on anywhere on earth. I contend that at present almost nobody can check more than a negligible fraction of the observation reports which fill the current scientific literature, and that even in one's own field of research one must accept many reports without checking them. Thus, no one can deny hypothesis B on the basis of first-hand knowledge. But anyone can pose many awkward questions to those who accept hypothesis B; evading them will render hypothesis B unexplanatory, and attempts to answer them will render it more and more *ad hoc*, *i.e.* less and less testable. Hence, one who accepts Popper's demand for explanation and high testability, will reject hypothesis B. All the other existing approaches will make one feel very disturbed by hypothesis B once one has taken it seriously. Sensationalism forces one to take it seriously.

The whole point of the present discussion can be summed up by stressing the unreasonableness of taking hypothesis B seriously on philosophical grounds together with the reasonableness of taking it seriously as a testable explanation of a picture of the situation which we may have, say as a result of a hypothetical victory of Nazism. This is nothing but Popper's revolutionary thesis that the basis of science is social and not psychological.

But why do people observe? Why do they not simply imagine facts? My first answer is that they do, in all earnestness, try to imagine facts, but that their imagination is ludicrously less informative than the imagination of experimental investigators. Not only is the imagination of the author of *Arabian Nights* infinitely inferior to that of Jules Verne; when a cinematic version of a science-fiction novel of Verne is done nowadays, its script-writers have to improve upon his imagination—by using what men of science present, rightly or wrongly, as observed facts! Facts are stranger than fiction. Fiction is a very poor substitute for observation!

But this is only my preliminary answer. I do not wish to imply for one minute that we prefer observation to fiction because it is a better fiction than fiction; nor do I wish to belittle the significance of fiction (including that of Jules Verne) as a stimulus for observation; I only wish to argue that the fear of illusion which has ridden philosophers is rooted in an incredible overestimate of the power of our imagination. The attempt to explain an observation report, whether as a result of observation or as a result of hallucination, shows that we do not think we are so good at self-illusion: otherwise we could explain all observation reports, past and future, as a result of illusions, which is a version of idealism. It is because we do realize the limitation of our imagination that we have to ignore hypothesis B. The attempt to explain already implies that we think that we live in a world populated with humans who observe, think, and make statements, often because they think, rightly or wrongly, that they are true. In brief, we observe in order to test, though we do not always succeed. This is why I think that the problem of observation has been overrated: it has been overrated because the significance of the desire to explain or to comprehend has been underrated. The desire to explain, in its turn, has been underrated because the desire for certitude was great, and imaginative explanation is quite a different kettle of fish from certainty of any kind. As the quest for certitude or near-certitude has to be abandoned anyhow, and as the demand to present highly explanatory and highly testable theories is realistic enough, we may leave it to science to explain each observation report in the most suitable way without trying to explain, in addition, observation reports as such.

## (7) *Conclusion*

> We all start from " Naive realism ", *i.e.* the doctrine that things are what they seem. We think that grass is green, that stones are hard, and that snow is cold. But physics assures us that the greenness of grass, the hardness of stones, and the coldness of snow, are not the greenness, hardness, and coldness that we know in our own experience, but something very different. The observer, when he seems to himself to be observing a stone, is really, if physics is to be believed, observing the effects of the stone upon himself. Thus science seems to be objective, it finds itself plunged into subjectivity against its will. Naive realism leads to physics, and physics, if true, shows that naive realism is false. Therefore naive realism, if true, is false; therefore it is false.

This passage from the beginning of Russell's *An Inquiry Into Meaning and Truth* (pp. 14-15), which has aroused the admiration of Einstein, is the core of Einstein's comments on that book (in *The Philosophy of Bertrand Russell*). Einstein explains there how the desertion of naive realism led to sophisticated sensationalism and thus to idealism as the only alternative to apriorism. ' I am particularly pleased to note ', says Einstein in the conclusion of his comments, ' that, in the last chapter of the book, it finally crops out that one can, after all, not get along without " metaphysics " [*i.e.* without unwarranted realism]. The only thing to which I take exception there is the bad intellectual conscience which shines through between the lines.' This ' bad intellectual conscience ', to sum-up, is rooted in the following implicit assumptions. First, that there exists only one picture of the world which may be properly viewed as naive realism. Second, that if science explains this naive picture of the world, it ought to accept it as true—which it does not. Third, that science explains not our naive picture, which is false, but reports about our sensations, which are true. In contrast to these tacit assumptions of Russell I propose the following view. (1) All pictures of the world which science explains are realistic. (2) All of them are naive to this or that degree. (3) Yesterday's frontier of science is today's rather naive realism. (4) Science is the attempt to explain the existing picture of the world, but this attempt is not based on the adoption of this picture; rather it leads to changes of the picture. (5) As Popper has suggested, science must remain at war with itself if it is to progress.

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