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THREE VIEWS CONCERNING HUMAN KNOWLEDGE

Karl Popper

1. THE SCIENCE OF GALILEO AND ITS MOST RECENT BETRAYAL

ONCE upon a time there was a famous scientist whose name was Galileo Galilei. He was tried by the Inquisition, and forced to recant his teaching. This caused a great stir; and for well over two hundred and fifty years the case continued to arouse indignation and excitement–long after public opinion had won its victory, and the Church had become tolerant of science.

But this is by now a very old story, and I fear it has lost its interest. For Galilean science has no enemies left, it seems: its life hereafter is secure. The victory won long ago was final, and all is quiet on this front. So we take a detached view of the affair nowadays, having learned at last to think historically and to understand both sides of a dispute. And nobody cares to listen to the bore who can't forget an old grievance.

What, after all, was this old case about? It was about the status of the Copernican 'System of the World' which, besides other things, explained the diurnal motion of the sun as only apparent, and as due to the rotation of our own earth. [i] The Church was very ready to admit that the new system was simpler than the old one: that it was a more convenient *instrument* for astronomical calculations, and for predictions. Pope Gregory's reform of the calendar made full practical use of it. There was no objection to Galileo's teaching the mathematical theory, so long as he made it clear that its value was *instrumental* only; that it was nothing but a 'supposition', as Cardinal Bellarmino put it;[ii] or a 'mathematical hypothesis'-a kind of mathematical trick, 'invented and assumed in order to abbreviate and ease the calculations', [iii] In other words there were no objections so long as Galileo was ready to fall into line with Andreas Osiander who had said in his preface to Copernicus' *De revolutionibus:* 'There is no need for these hypotheses to be true, or even to be at all like the truth; rather, one thing is sufficient for them-that they should yield calculations which agree with the observations.'

Galileo himself, of course, was very ready to stress the superiority of the Copernican system as an *instrument of calculation.* But at the same time he conjectured, and even believed, that it was *a true description of the world;* and for him (as for the Church) this was by far the most important aspect of the matter. He had indeed some good reasons for believing in the truth of the theory. He had seen in his telescope that Jupiter and his moons formed a miniature model of the Copernican solar system (according to which the planets were moons of the sun). Moreover, if Copernicus was right the inner planets (and they alone) should, when observed from the earth, show phases like the moon; and Galileo had seen in his telescope the phases of Venus.

The Church was unwilling to contemplate the truth of a New System of the World which seemed to contradict a passage in the Old Testament. But this was hardly its main reason. A deeper reason was clear1y stated by Bishop Berkeley, about a hundred years later, in his criticism of Newton.

In Berkeley's time the Copernican System of the World had developed into Newton's Theory of gravity, and Berkeley saw in it a serious competitor to religion. He saw that a decline of religious faith and religious authority would result from the new science unless its interpretation by the 'free-thinkers' could be refuted; for they saw in its success a proof of *the power of the human intellect, unaided by divine revelation, to uncover the secrets of our world-the* reality hidden behind its appearance. This, Berkeley felt, was to misinterpret the new science. He analysed Newton's theory with complete candour and great philosophical acumen; and a critical survey of Newton's concepts convinced him that this theory could not possibly be anything but a 'mathematical hypothesis', that is, a convenient *instrument* for the calculation and prediction of phenomena or appearances; that it could not possibly be taken as a true description of anything real [iv].

Berkeley's criticism was hardly noticed by the physicists; but it was taken up by philosophers, sceptical as well as religious. As a weapon it turned out to be a boomerang. In Hume's hands it became a threat to all belief-to all knowledge, whether human or revealed. In the hands of Kant, who firmly believed both in God and in the truth of Newtonian science, it developed into the doctrine that theoretical knowledge of God is impossible, and that Newtonian science must pay for the admission of its claim to truth by the renunciation of its claim to have discovered the real world behind the world of appearance: it was a true science of nature, but *nature* was precisely the world of mere phenomena, the world as it appeared to our assimilating minds. Later certain Pragmatists based their whole philosophy upon the view that the idea of 'pure' knowledge was a mistake; that there could be no knowledge in any other sense but in the sense of *instrumental* knowledge; that knowledge was power, and that truth was usefulness.

Physicists (with a few brilliant exceptions[v]) kept aloof from all these philosophical debates, which remained completely inconclusive. Faithful to the tradition created by Galileo they devoted themselves to the search for truth, as he had understood it.

Or so they did until very recently. For all this is now past history. Today the view of physical science founded by Osiander, Cardinal Bellarmino, and Bishop Berkeley,[vi] has won the battle without another shot being fired. Without any further debate over the philosophical issue, without producing any new argument, the *instrumentalist view* (as I shall call it) has become an accepted dogma. It may well now be called the 'official view' of physical theory since it is accepted by most of our leading theorists of physics (although neither by Einstein nor by Schrödinger). And it has been me part of the current teaching of physics.

2. THE ISSUE AT STAKE

All this looks like a great victory of philosophical critical thought over the 'naïve realism' of the physicists. But I doubt whether this interpretation is right.

Few if any of the physicists who have now accepted the instrumentalist view of Cardinal Bellarmino and Bishop Berkeley realize that they have accepted a philosophical theory. Nor do they realize that they have broken with the Galilean tradition. On the contrary, most of them think that they have kept clear of philosophy; and most of them no longer care anyway. What they now care about, as physicists, is *(a) mastery of the mathematical formalism,* i.e. of the instrument, and *(b) its applications;* and they care for nothing else. And they think that by thus excluding everything else they have finally got rid of all philosophical nonsense. This very attitude of being tough and not standing any nonsense prevents them from considering seriously the philosophical arguments for and against the Galilean view of science (though they will no doubt have heard of Mach[vii]). Thus the victory of the instrumentalist philosophy is hardly due to the soundness of its arguments.

How then did it come about? As far as I can see, through the coincidence of two factors, *(a)* difficulties in the interpretation of the formalism of the Quantum Theory, and *(b)* the spectacular practical success of its applications.

(a) In 1927 Niels Bohr, one of the greatest thinkers in the field of atomic physics, introduced the so-called *principle of complementarity* into atomic physics, which amounted to a 'renunciation' of the attempt to interpret atomic theory as a description of anything. Bohr pointed out that we could avoid certain contradictions (which threatened to arise between the formalism and its various interpretations) only by reminding ourselves that the formalism as such was self-consistent, and that each single case of its application (or each kind of case) remained consistent with it. The contradictions only arose through the attempt to comprise within *one* interpretation the formalism together with more than one case, or kind of case, of its experimental application. But, as Bohr pointed out, any two of these conflicting applications were physically incapable of ever being combined in one experiment. Thus the result of *every single* experiment was consistent with the theory, and unambiguously laid down by it. This, he said, was all we could get. The claim to get more, and even the hope of ever getting more, we must renounce; physics remains consistent only if we do not try to interpret, or to understand, its theories beyond *(a)* mastering the formalism, and *(b)* relating them to each of their actually realizable cases of application separately.[viii]

Thus the instrumentalist philosophy was used here ad *hoc* in order to provide an escape for the theory from certain contradictions *by* which it was, threatened. It was used in a defensive mood-to rescue the existing theory; and the principle or complementarity has (I believe for this reason) remained completely sterile within physics. In twenty-seven years it has produced nothing except some philosophical discussions, and some arguments for the confounding or critics (especially Einstein).

I do not believe that physicists would have accepted such an ad *hoc* principle had they understood that it was *ad hoc,* or that it was a philosophical principle-part or Bellarmino's and Berkeley's instrumentalist philosophy of physics. But they remembered Bohr's earlier and extremely fruitful ‘principle of correspondence’ and hoped (in vain) for similar results.

(b) Instead or results due to the principle of complementarity other and more practical results of atomic theory were obtained, some or them with a big bang. No doubt physicists were perfectly right in interpreting these successful applications as corroborating their theories. But strangely enough they took them as confirming the instrumentalist creed.

Now this was an obvious mistake. The instrumentalist view asserts that theories are *nothing but* instruments, while the Galilean view was that they are not only instruments but also-and mainly-descriptions of the world, or of certain aspects or the world. It is clear that in this disagreement even a proof showing that theories are instruments (assuming it possible to ‘prove’ such a ting) could not seriously be claimed to support either of the two parties to the debate, since both were agreed on this point.

If I am right, or even roughly right, in *my* account of the situation, then philosophers, even instrumentalist philosophers, have no reason to take pride in their victory. On the contrary, they should examine their arguments again. For at least in the eyes of those who like myself do not accept the instrumentalist view, there is much at stake in this issue.

The issue, as I see it, is this.

One of the most important ingredients of our western civilization is what I may call the ‘rationalist tradition’ which we have inherited from the Greeks. It is the tradition of critical discussion-not for its own sake, but in the interests of the search for truth. Greek science, like Greek philosophy, was one of the products of this tradition, and of the urge to understand the world in which we live; and the tradition founded *by* Galileo was its renaissance.

Within this rationalist tradition science is valued, admittedly, for its pratical achievements; but it is even more highly valued for its informative content, and for its ability to free our minds from old beliefs, old prejudices, and old certainties, and to offer us in their stead new conjectures and daring hypotheses. Science is valued for its liberalizing influence-as one of the greatest of the forces that make for human freedom.

According to the view of science which I am trying to defend here, this is due to the fact that scientists have dared (since Thales, Democritus, Plato's *Timaeus,* and

Aristarchus) to create myths, or conjectures, or theories, which are in striking contrast to the everyday world of common experience, yet able to explain some aspects of this world of common experience. Galileo pays homage to Aristarchus and Copernicus precisely because they dared to go beyond this known world of our senses: 'I cannot', he writes, 'express strongly enough my unbounded admiration for the greatness of mind of these men who conceived [the heliocentric system] and held it to be true . . . ,in violent opposition to the evidence of their own senses. . . .' This is Galileo's testimony to the liberalizing force of science. Such theories would be important even if they were no more than exercises for our imagination. But they are more than this, as can be seen from the fact that we submit them to severe tests by trying to deduce from them some of the regularities of the known world of common experience-i.e. by trying to *explain* these regularities. And these attempts to *explain the known by the unknown* (as I have described them elsewhere) have immeasurably extended the realm of the known. They have added to the facts of our everyday world the invisible air, the antipodes, the circulation of the blood, the worlds of the te1escope and the microscope, of electricity, and of tracer atoms showing us in detail the movements of matter within living bodies. All these things are far from being mere instruments: they are witness to the intellectual conquest of our world by our minds.

But there is another way of looking at these matters. For some, science is still nothing but glorified plumbing, glorified gadget-making-'mechanics'; very useful, but a danger to true culture, threatening us with the domination of the near-illiterate (of Shakespeare's 'mechanicals'). It should never be mentioned in the same breath as literature or the arts or philosophy. Its professed discoveries are mere mechanical inventions, its theories are instruments-gadgets again, or perhaps super-gadgets. It cannot and does not reveal to us new worlds behind our everyday world of appearance; for the physical world is just surface: it has no depth. *The world is just what it appears to be. Only the scientific theories are not what they appear to be.* A scientific theory neither explains nor describes the world; it is nothing but an instrument.

I do not present this as a complete picture of modern instrumentalism, although it is a fair sketch, I think, of part of its original philosophical background. Today a much more important part of it is, I am well aware, the rise and self-assertion of the modern 'mechanic' or engineer. Still, I believe that the issue should be seen to lie between a critical and adventurous rationalism-the spirit of discovery-and a narrow and defensive creed according to which we cannot and need not learn or understand more about our world than we know already. A creed, moreover, which is incompatible with the appreciation of science as one of the greatest achievements of the human spirit.

Such are the reasons why I shall try, in this paper, to uphold at least part of the Galilean view of science against the instrumentalist view. But I cannot uphold all of it. There is a part of it which I believe the instrumentalists were right to attack. I mean the view that in science we can aim at, and obtain, *an ultimate explanation by essences.* It is in its opposition to this Aristotelian view (which I have called 'essentialism') that the strength and the philosophical interest of instrumentalism lies. Thus I shall have to discuss and criticize two views of human *knowledge-essentialism* and *instrumentalism.* And I shall oppose to them what I shall call *the third view-what* remains of Galileo's view after the elimination of essentialism, or more precisely, after allowance has been made for what was justified in the instrumentalist attack.

3. THE FIRST VIEW: ULTIMATE EXPLANATION BY ESSENCES

Essentialism, the first of the three views of scientific theory to be discussed, is part of the Galilean philosophy of science. Within this philosophy three elements or doctrines which concern us here may be distinguished. Essentialism (our 'first view') is that part of the Galilean philosophy which I do not wish to uphold. It consists of a combination of the doctrines (2) and (3). These are the three doctrines:

1. *The scientist aims at finding a true theory or description of the world* (and especially of its regularities or 'laws'), *which shall also be an explanation of the observable facts*. (This means that a description of these facts must be deducible from the theory in conjunction with certain statements, the so-called 'initial conditions'.)

   This is a doctrine I wish to uphold. It is to form part of our 'third view'.

2. *The scientist can succeed* in *finally establishing the truth of such theories beyond all reasonable doubt.*

   This second doctrine, I think, needs correction. All the scientist can do, in my opinion, is to test his theories, and to eliminate all those that do not stand up to the most severe tests he can design. But he can never be quite sure whether new tests (or even a new theoretical discussion) may not lead him to modify, or to discard, his theory. In this sense all theories are, and remain hypotheses: they are conjecture *(doxa)* as opposed to indubitable knowledge *(episteme).*

3. *The best, the truly scientific theories, describe the 'essences' or the 'essential natures' of things-the realities which lie behind the appearances.* Such theories are neither in need nor susceptible of further explanation: they are *ultimate explanations,* and to find them is the ultimate aim of the scientist.

   This third doctrine (in connection with the second) is the one I have called 'essentialism'. I believe that like the second doctrine it is mistaken.

Now what the instrumentalist philosophers of science, from Berkeley to Mach, Duhem, and Poincaré, have in common is this. They all assert that explanation is not an aim of physical science, since physical science cannot discover 'the hidden essences of things'. The argument shows that what they have in mind is what I call *ultimate* explanation[ix]. Some of them, such as Mach and Berkeley, hold this view because they do not believe that there is such a thing as an essence of anything physical: Mach, because he does not believe in essences at all; Berkeley, because he believes only in spiritual essences, and thinks that the only essential explanation of the world is Gad. Duhem seems to think (on lines reminiscent of Kant[15]) that there are essences but that they are undiscoverable by human science (though we may, somehow, move towards them); like Berkeley he thinks that they can be revealed by religion. But all these philosophers agree that (ultimate) scientific explanation is impossible. And from the absence of a hidden essence which scientific theories could describe they conclude that these theories (which clearly do not describe our ordinary world of common experience) describe nothing at all. Thus they are mere instruments.[16] And what may appear as the growth of theoretical knowledge is merely the improvement of instruments.

The instrumentalist philosophers therefore reject the third doctrine, i.e. the doctrine of essences. (I reject it too, but for somewhat different reasons.) At the same time they reject, and are bound to reject, the second doctrine; for if a theory is an instrument, then it cannot be true (but only convenient, simple, economical, powerful, etc.). They even frequently call the theories 'hypotheses'; but they do not, of course, mean by this what I mean: that a theory is *conjectured to be true,* that it is a descriptive though possibly a false statement; although they do mean to say that theories are uncertain: 'And as to the usefulness of hypotheses', Osiander writes (at the end of his preface), 'nobody should expect anything certain to emerge from astronomy, for nothing of the kind can ever come out of it.' Now I fully agree that there is no certainty about theories (which may always be refuted); and I even agree that they are instruments, although I do not agree that this is the reason why there can be no certainty about theories. (The correct reason, I believe, is simply that our tests can never be exhaustive.) There is thus a considerable amount of agreement between my instrumentalist opponents and myse1f over the second and third doctrines. But over the first doctrine there is complete disagreement.

To this disagreement I shall return later. In the present section I sha11 try to criticize (3), the essentialist doctrine of science, on lines somewhat different from the arguments of the instrumentalism which I cannot accept. For its argument that there can be no 'hidden essences' is based upon its conviction that t*here can be nothing hidden* (or that if anything is hidden it can be only known by divine revelation). From what I said in the last section it will be clear that I cannot accept an argument that leads to the rejection of the claim of science to have discovered the rotation of the earth, or atomic nuclei, or cosmic radiation, or the 'radio stars'.

I therefore readily concede to essentialism that much is hidden from us, and, that much of what is hidden may be discovered. (I disagree profoundly with the spirit of Wittgenstein's dictum, 'The riddle does not exist'.) And I do not even intend to criticize those who try to understand the 'essence of the world'. The essentialist doctrine I am contesting is solely t*he doctrine that science aims at ultimate explanation;* that is to say, an explanation which (essentially, or by its very nature) cannot be further explained, and which is in no need of any further explanation.

Thus my criticism or essentialism does not aim at establishing the non-existence of essences; it merely aims at showing the obscurantist character of role played by the idea of essences in the Galilean philosophy or science (down to Maxwell, who was inclined to believe in them but whose work destroyed this belief). In other words my criticism tries to show that whether essences exist or not the belief in them does not help us in any way and indeed is likely to hamper us; so that there is no reason why the scientist should *assume* their existence.

This, I think, can be best shown with the help of a simple example – the *Newtonian theory of gravity.*

The essentialist interpretation of Newtonian theory is due to Roger Cotes. According to him Newton discovered that every particle of matter was endowed with *gravity,* i.e. with an inherent power or force to attract other matter. It was also endowed with *inertia – an* inherent power to resist a change in its state of motion (or to retain the direction and velocity of its motion). Since both gravity and inertia inhere in each particle of matter it follows that both must be strictly proportional to the amount of matter in a body, and therefore to each other; hence the law of proportionality of inert and gravitating mass. Since gravity radiates from each particle we obtain the square law of attraction. In other words, Newton's laws of motion simply describe in mathematical language the state of affairs due to the inherent properties of matter: they describe the *essential nature of matter.*

Since Newton's theory described in this way the essential nature of matter, he could explain the behaviour of matter with its help, by mathematical deduction. But Newton's theory, in its turn, is neither capable of, nor in need of, further explanation, according to Cotes – at least not within physics. (The only possible further explanation was that God has endowed matter with these essential properties.)

This essentialist view of Newton's theory was on the whole the accepted view until the last decades of the nineteenth century. That it was obscurantist is clear: *it prevented fruitful questions from being raised,* such as, 'What is the cause of gravity?' or more fully, 'Can we perhaps explain gravity by deducing Newton's theory, or a good approximation of it, from a more general theory (which should be independently testable)?'

Now it is illuminating to see that Newton himself had not considered *gravity* as an essential property of matter (although he considered *inertia* to be essential, and also, with Descartes, *extension).* It appears that he had taken over from Descartes the view that the essence of a thing must be a true or absolute property of the thing (i.e. a property which does not depend on the existence of other things) such as extension, or the power to resist a change in its state of motion, and not a relational property, i.e. a property which, like gravity, determines the relations (interactions in space) between one body and other bodies. Accordingly, he strongly felt the incompleteness of this theory, and the need to explain gravity. 'That gravity', he wrote, 'should be innate, inherent, and essential to matter, that one body may act upon another at a distance... is to me so great an absurdity that I believe no man who has in philosophical matters a competent faculty of thinking can ever fall into it.'

It is interesting to see that Newton condemned here, in anticipation, the bulk of his followers. To them, one is tempted to remark, the properties of which they had learned in school appeared to be essential (and even self-evident), although to Newton, with his Cartesian background, the same properties had appeared to be in need of explanation (and indeed to be almost paradoxical).

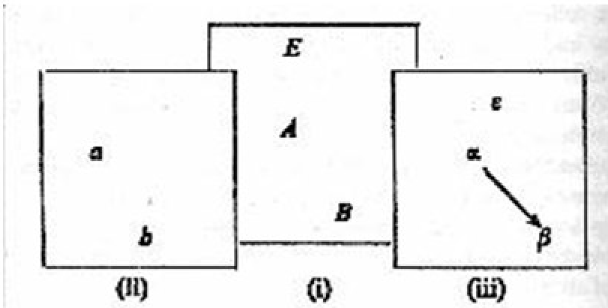
Yet Newton himself was an essentialist. He had tried hard to find an acceptable ultimate explanation of gravity by trying to deduce the square law from the assumption of a mechanical push-the only kind of causal action which Decartes had permitted, since on1y push could be explained by the essential property of all bodies, extension. But he failed. Had he succeeded we can be certain that he would have thought that his problem was finally solved – that he found the ultimate explanation of gravity. But here he would have been wrong, The question, 'Why can bodies push one another ?' *can* be asked)as Leibniz first saw), and it is even an extremely fruitful question. (We now believe that they push one another because of certain repulsive electric forces.) But Cartesian and Newtonian essentialism, especially if Newton had been successful in his attempted explanation of gravity, might have prevented this question from ever being raised.

These examples, I think, make it clear that the belief in essences (whether true or false) is liable to create obstacles to thought – to the posing of new and fruitful problems. Moreover, it cannot be part of science (for even if we should, by a lucky chance, hit upon a theory describing essences, we could never be sure of it), But a creed which is likely to lead to obscurantism is certainly not one of those extra-scientific beliefs (such as a faith in the power of critical discussion) which a scientist need accept.

This concludes my criticism of essentialism.

4. THE SECOND VIEW: THEORIES AS INSTRUMENTS

The instrumentalist view has great attractions. It is modest, and it is very simple, especially if compared with essentialism. In essentialism we must distinguish between (i) the universe of essential reality, (ii) the universe of observable phenomena, and (iii) the universe of descriptive language or of symbolic representation. I will take each of these to be represented by a square.



The function of a theory may here be described as follows. *a, b* are phenomena; *A, B* are the corresponding realities behind these appearances; and *α, β* the descriptions or symbolic representations of these realities. *E* are the essential properties of *A, B,* and ε is the theory describing *E.* Now from ε and *α* we can deduce *β;* this means that we can explain, with the help of our theory, why *a* leads to, or is the cause of, *b.*

A representation of instrumentalism can be obtained from this schema simply by omitting (i), i.e. the universe of the realities behind the various appearances. *α* then directly describes *a,* and *β* directly describes *b;* and ε describes nothing – it is merely an instrument which helps us to deduce *β* from *α.* (This may be expressed by saying – as Schlick did, following Wittgenstein – that a universal law or a theory is not a proper statement but rather 'a rule, or a set of instructions, for the derivation of singular statements from other singular statements').

This is the instrumentalist view. In order to understand it better we may again take Newtonian dynamics as an example. *a* and *b* may be taken to be two positions of two spots of light (or two positions of the planet Mars); *α* and *β* are the corresponding formulae of the formalism; and ε is the theory strengthened by a general description of the solar system (or by a 'model' of the solar system). Nothing corresponds to ε in the world (in the universe ii): there simply are no such things as attractive forces, for example. Newtonian forces are not entities which determine the acceleration of bodies: they are nothing but mathematical tools whose function is to allow us to deduce *β* from *α.*

No doubt we have here an attractive simplification, a radical application of Ockham's razor. But although this simplicity has converted many to instrumentalism (for example Mach) it is by no means the strongest argument in its favour.

Berkeley's strongest argument for instrumentalism was based upon his nominalistic philosophy of language. According to this philosophy the expression 'force of attraction' must be a meaningless expression, since forces of attraction can never be observed. What can be observed are movements, not their hidden alleged 'causes'. This is sufficient, on Berkeley's view of language, to show that Newton's theory cannot have any informative or descriptive content.

Now this argument of Berkeley's may perhaps be criticized because of the intolerably narrow theory of meaning which it implies. For if consistently applied it amounts to the thesis that all dispositional words are without meaning. Not only would Newtonian 'attractive forces' be without meaning, but also such ordinary dispositional words and expressions as 'breakable' (as opposed to 'broken'), or 'capable of conducting electricity' (as opposed to 'conducting electricity'). These are not names of anything observable, and they would therefore have to be treated on a par with Newtonian forces. But it would be awkward to classify all these expressions as meaningless, and from t*he point of view of instrumentalism* it is quite unnecessary to do so: all that is needed is an analysis of the meaning of dispositional terms and dispositional statements. This will reveal that they have meaning. But from the point of view of instrumentalism they do not have a descriptive meaning (like non-dispositional terms and statements). Their function is not to report events, or occurrences, or 'incidents', in the world, or to describe facts. Rather, their meaning exhausts itself in the permission or licence which they give us to draw inferences or to argue *from* some matters of fact to other matters of fact. Non-dispositional statements which describe observable matters of fact ('this leg is broken') have cash value, as it were; dispositional statements, to which belong the laws of science, are not like cash, but rather like legal '*instruments'* creating rights to cash.

One need only proceed one step further in the same direction, it appears, in order to arrive at an instrumentalist argument which it is extremely difficult, if not impossible, to criticize; for our whole question-whether science is descriptive or instrumental – is here exposed as a pseudo-problem.

The step in question consists, simply, in not only allowing meaning – an instrumental meaning – to dispositional terms, but also a kind of *descriptive meaning,* Dispositional words such as 'breakable', it may be said, certainly describe something; *for* to say of a thing that it is breakable is to describe it as a thing that can be broken. But to say of a thing that it is breakable, or soluble, is to describe it in a different way, and by a different method, from saying that it is broken or dissolved; otherwise we should not use the suffix 'able'. The difference is just this – that we describe, by using dispositional words, what may happen to a thing (in certain circumstances). Accordingly, dispositional descriptions *are* descriptions, but they have nevertheless a purely instrumental function. In their case, knowledge *is* power (the power to foresee). When Galileo said of the earth 'and yet, it moves', then he uttered, no doubt, a descriptive statement. But the function or meaning of this statement turns out nevertheless to be purely instrumental: it exhausts itself in the I help it renders in deducing certain non-dispositional statements.

Thus the attempt to show that theories have a descriptive meaning *besides* their instrumental meaning is misconceived, according to this argument; and the whole problem-the issue between Galileo and the Church-turns out to be a pseudo-problem.

In support of the view that Galileo suffered for the sake of a pseudo-problem it has been asserted that in the light of a logically more advanced system of physics Galileo's problem has in fact dissolved into nothing. Einstein's general principle, one often hears, makes it quite clear that it is meaningless to speak of absolute motion, even in the case of rotation; for we can freely choose whatever system we wish to be (relatively) at rest. Thus Galileo's problem vanishes. Moreover, it vanishes precisely for the reasons given above. Astronomical knowledge can be nothing but knowledge of how the stars behave; thus it cannot be anything but the power to describe and predict our observations; and since these must be independent of our free choice of a co-ordinate system, we now see more clearly why Galileo's problem could not possibly be real.

I shall not criticize instrumentalism in this section, or reply to its arguments, except the very last one – the argument from general relativity. This argument is based on a mistake. From the point of view of general relativity, there is very good sense-even an absolute sense-in saying that the earth rotates: *it rotates in precisely that sense in which a bicycle wheel rotates.* It rotates, that is to say, with respect to *any* chosen local inertial system. Indeed relativity describes the solar system in such a way that from this description we can deduce that *any* observer situated on *any* sufficiently distant freely moving physical body (such as our moon, or another planet, or a star outside the system) would see the earth rotating, and could deduce, from this observation, that for its inhabitants there would be an apparent diurnal motion of the sun. But it is clear that this is precisely the sense of the words 'it moves' which was at issue; for part of the issue was whether the solar system was a system like that of Jupiter and his moons, only bigger; and whether it would look like this system, if seen from outside. On all these questions Einstein unambiguously supports Galileo.

My argument should not be interpreted as an admission that the whole question can be reduced to one of observations, or of possible observations. Admittedly both Galileo and Einstein intend, among other things, to deduce what an observer, or a possible observer, would see. But this is not their main problem. Both investigate physical systems and their movements. It is only the instrumentalist philosopher who asserts that what they discussed, or , 'really meant' to discuss, were not physical systems but *only* the results of possible observations; and that their so-called 'physical systems', which appeared to be their objects of study, were *in reality* only instruments for predicting observations.

5. CRITICISM OF THE INSTRUMENTALIST VIEW

Berkeley's argument, we have seen, depends upon the adoption of a certain philosophy of language, convincing perhaps at first, but not necessarily true. Moreover, it hinges on the *problem of meaning,* notorious for its vagueness and hardly offering hope of a solution. The position becomes even more hopeless if we consider some more recent development of Berkeley's arguments, as sketched in the preceding section. I shall try, therefore, to force a clear decision on our problem by a different approach – by way of an analysis of science rather than an analysis of language.

My proposed criticism of the instrumentalist view of scientific theories can be summarized as follows.

Instrumentalism can be formulated as the thesis that scientific theories – the theories of the so-called 'pure' sciences – are nothing but computation rules (or inference rules); of the same character, fundamentally, as the computation rules of the so-called 'applied' sciences. (One might even formulate it as the thesis that 'pure' science is a misnomer, and that all science is applied'.)

Now my reply to instrumentalism consists in showing that there are profound differences between 'pure' theories and technological computation rules, and that instrumentalism can give a perfect description of these rules but it is quite unable to account for the difference between them and the theories. Thus instrumentalism collapses.

The analysis of the many functional differences between computation rules (for navigation, say) and scientific theories (such as Newton's) is a very interesting task, but a short list of results must suffice here. The logical relations which may hold between theories and computation rules are not symmetrical; and they are different from those which may hold between various theories, and also from those which may hold between various computation rules. The way in which computation rules are *tried out* is different from the way in which theories are *tested;* and the skill which the application of computation rules demands is quite different from that needed for their (theoretical) discussion, and for the (theoretical) determination of the limits of their applicability. These are only a few hints, but they may be enough to indicate the direction and the force of the argument.

I am now going to explain one of these points a little more fully, because it gives rise to an argument somewhat similar to the one I have used against essentialism. What I wish to discuss is the fact that theories are tested by *attempts to refute them* (attempts from which we learn a great deal), while there is nothing strict1y corresponding to this in the case of technological rules of computation or calculation.

A theory is tested not merely by applying it, or by trying it out, but by applying it to very special cases – cases for which it yields results different from those we should have expected without that theory, or in the light of other theories. In other words we try to select for our tests those crucial cases in which we should expect the theory to fail if it is not true. Such cases are 'crucial' in Bacon's sense; they indicate the cross-roads between *two* (or more) theories. For to say that without the theory in question we should have expected a different result implies that our expectation was the result of some other (perhaps an older) theory, however dimly we may have been aware of this fact. But while Bacon believed that a crucial experiment may establish or verify a theory, we shall have to say that it can at most refute or falsify a theory. It is an attempt to refute it; and if it does not succeed in refuting the theory in question – if, rather, the theory is successful with its unexpected prediction – then we say that it is corroborated by the experiment. (It is the better corroborated the less expected, or the less probable, the result of the experiment has been.)

Against the view here developed one might be tempted to object (following Duhem) that in every test it is not only the theory under investigation which is involved, but also the whole system of our theories and assumptions – in fact, more or less the whole of our knowledge – so that we can never be certain which of all these assumptions is refuted. But this criticism overlooks the fact that if we take each of the two theories (between which the crucial experiment is to decide) *together* with all this background knowledge, as indeed we must, then we decide between two systems which differ *only* over the two theories which are at stake. It further overlooks the fact that we do not assert the refutation of the theory as such, but of the theory *together* with that background knowledge; parts of which, if other crucial experiments can be designed, may indeed one day be rejected as responsible for the failure. (Thus we may even characterize a *theory under investigation* as that part of a vast system for which we have, however vaguely, an alternative in mind, and for which we try to design crucial tests.)

Now nothing sufficiently similar to such tests exists in the case of instruments or rules of computation. An instrument may break down, to be sure, or it may become outmoded. But it hardly makes sense to say that we submit an instrument to the severest tests we can design in order to reject it if it does not stand up to them: every air frame, for example, can be 'tested to destruction', but this severe test is undertaken not in order to reject every frame when it is destroyed but to obtain information about the frame (i.e. to test a theory about it), so that it may be used *within the limits of its applicability* (or safety).

For instrumental purposes of practical application a theory may continue to be used *even after its refutation,* within the limits of its applicability: an atronomer who believes that Newton's theory has turned out to be false will not hesitate to apply its formalism within the limits of its applicability.

We may sometimes be disappointed to find that the range of applicability of an instrument is smaller than we expected at first; but this does not make us discard the instrument *qua* instrument – whether it is a theory or anything else. On the other hand a disappointment of this kind means that we have obtained new *information* through refuting a *theory* – *that* theory which implied that the instrument was applicable over a wider range.

Instruments, even theories *in so for as they are instruments,* cannot be refuted, as we have seen. The instrumentalist interpretation will therefore be unable to account for real tests, which are attempted refutations, and will not get beyond the assertion that *different theories have different ranges of application*. But then it cannot possibly account for scientific progress. Instead of saying (as I should) that Newton's theory was falsified by crucial experiments which failed to falsify Einstein's, and that Einstein's theory is therefore better than Newton's, the consistent instrumentalist will have to say, with reference to his 'new' point of view, like Heisenberg: 'It follows that we do not say longer: Newton's mechanics is false. . . . Rather, we now use the following - formulation: Classical mechanics ... is everywhere exactly "right" where its concepts can be applied.'

Since 'right' here means 'applicable', this assertion merely amounts to saying, 'Classical mechanics is applicable where its concepts can be applied' – which is not saying much. But be this as it may, the point is that *by neglecting falsification, and stressing application, instrumentalism proves to be as obscurantist philosophy as essentialism.* For it is only in searching for refutations that science can hope to learn and to advance. It is only in considering how its various theories stand up to tests that it can distinguish between better and worse theories and so find a criterion of progress.

Thus a mere instrument for prediction cannot be falsified. What may appear to us at first as its falsification turns out to be no more than a rider cautioning us about its limited applicability. This is why the instrumentalist view may be used *ad hoc* for rescuing a physical theory which is threatened by contradictions, as was done by Bohr (if I am right in my interpretation, given in section ii, of his principle of complementarity). If theories are mere instruments of prediction we need not discard any particular theory even though we believe that no consistent physical interpretation of its formalism exists.

Summing up we may say that instrumentalism is unable to account for the importance to pure science of testing severely even the most remote implications of its theories, since it is unable to account for the pure scientist's interest in truth and falsity. In contrast to the highly critical attitude requisite in the pure scientist, the attitude of instrumentalism (like that of applied science) is one of complacency at the success of applications. Thus it may well be responsible for the recent stagnation in quantum theory. (This was written before the refutation of parity.)

6. THE THIRD VIEW: CONJECTURES, TRUTH, AND REALITY

Neither Bacon nor Berkeley believed that the earth rotates, but nowadays everybody believes it, including the physicists. Instrumentalism is embraced by Bohr and Heisenberg only as a way out of the special difficulties which have arisen in quantum theory.

The motive is hardly sufficient. It is always difficult to interpret the latest theories, and they sometimes perplex even their own creators, as happened with Newton. Maxwell at first inclined towards an essentialist interpretation of his theory: a theory which ultimately contributed more than any other to the decline of essentialism. And Einstein inclined at first to an instrumentalist interpretation of relativity, giving a kind of operational analysis of the concept of simultaneity which contributed more to the present vogue for instrumentalism than anything else; but he later repented.

I trust that physicists will soon come to realize that the principle of complementarity is *ad hoc,* and (what is more important) that its only function is to avoid criticism and to prevent the discussion of physical interpretations; though criticism and discussion are urgently needed for reforming any theory. They will then no longer believe that instrumentalism is forced upon them by the structure of contemporary physical theory.

Anyway, instrumentalism is, as I have tried to show, no more acceptable than essentialism. Nor is there any need to accept either of them, for there is a third view.

This 'third view' is not very startling or even surprising, I think. It preserves the Galilean doctrine that the scientist aims at a true description of the world, or of some of its aspects, and at a flue explanation of observable facts; and it combines this doctrine with the non-Galilean view that though this remains the aim of the scientist, he can never know for certain whether his findings are true, although he may sometimes establish with reasonable certainty that a theory is false.

One may formulate this 'third view' of scientific theories briefly by saying that they are *genuine conjectures – highly* informative guesses about the world which although not verifiable (i.e. capable of being shown to be true) can be submitted to severe critical tests. They are serious attempts to discover the truth. In this respect scientific hypotheses are exactly like Goldbach's famous conjecture in the theory of numbers. Goldbach thought that it might possibly bee true; and it may well be true in fact, even though *we do not know, and may perhaps never know, whether it is true or not.*

I shall confine myself to mentioning only a few aspects of my 'third view', and only such aspects as distinguish it from essentialism and instrumentalism; and I shall take essentialism first.

Essentialism looks upon our ordinary world as mere appearance behind which it discovers the real world. This view has to be discarded once we become conscious of the fact that the world of each of our theories may be explained, in its turn, by further worlds which are described by further theories – theories of a higher level of abstraction, of universality, and of testability. The doctrine of an *essential or ultimate reality* collapses together with that of ultimate explanation.

Since according to our third view the new scientific theories are, like the old ones, genuine conjectures, they are genuine attempts to describe these further worlds. Thus we are led to take all these worlds, including our ordinary world, as equally real; or better, perhaps, as equally real aspects or layers of the real world. (If looking through a microscope we change its magnification, then we may see various completely different aspects or layers of the same thing, all equally real.) It is thus mistaken to say that my piano, as I know it, us real, while its alleged molecules and atoms are mere 'logical constructions' (or whatever else may be indicative of their unreality); just as it is mistaken to say that atomic theory shows that the piano of my everyday world is an appearance only – a doctrine which is clearly unsatisfactory once we see that the atoms in their turn may perhaps be explained as disturbances, or structures of disturbances, in a quantized field of forces (or perhaps of probabilities). All these conjectures are equal in their claims to describe reality, although some of them are more conjectural than others.

Thus we shall not, for example, describe only the so-called 'primary qualities' of a body (such as its geometrical shape) as real, and contrast them as the essentialists once did, with its unreal and mere1y apparent 'secondary qualities' (such as colour). For the extension and even the shape of a body have since become *objects of explanation* in terms of theories of a higher level; of theories describing a further and deeper layer of reality – forces, and fields of forces – which are related to the primary qualities in the same way as these were believed by the essentialists to be related to the secondary ones; and the secondary qualities, such as colours, are just as real as the primary ones - though our colour experiences have to be distinguished from the colour properties of the physical things, exactly as our geometrical-shape-experiences have to be distinguished from the geometrical-shape-properties of the physical things. From our point of view both kinds of qualities are equally real – that is, conjectured to be real; and so are forces, and fields or forces, in spite of their undoubted hypothetical or conjectural character.

Although in one sense of the word 'real', all these various levels are equally real, there is another yet closely related sense in which we might say that the higher and more conjectural levels are the *more real* ones-in spite of the fact that they are more conjectural. They are, according to our theories, more real (more stable in intention, more permanent) in the sense in which a table, or a tree, or a star, is more real than any of its aspects.

But is not just this conjectural or hypothetical character of our theories the reason why we should not ascribe reality to the worlds described by them? Should we not (even if we find Berkeley's 'to be is to be perceived' too narrow) *call only* those *states of affairs 'real' which are described by true state*ments, rather than by conjectures which may turn out to be false? With these questions we turn to the discussion of the instrumentalist doctrine, which with its assertion that theories are mere instruments intends to deny the claim that anything like a real world is described by them.

I accept the view (implicit in the classical or correspondence theory of truth) that we should call a state of affairs 'real' if, and only if, the statement describing it is true. But it would be a grave mistake to conclude from this that the uncertainty of a theory, i.e. its hypothetical or conjectural character, diminishes in any way its implicit *claim* to describe something real. For every statement s is equivalent to a statement claiming that s is true. And as to s being a conjecture, we must remember that, first of all, a conjecture *may* be true, and thus describe a real state of affairs. Secondly, if it is false, then it contradicts some real state of affairs (described by its true negation).

Moreover, if we test our conjecture, and succeed in falsifying it, we see very clearly that there was a reality – something with which it could clash.

Our falsifications thus indicate the points where we have touched reality, as it were. And our latest and best theory is always an attempt to incorporate all the falsifications ever found in the field, by explaining them in the simplest way; and this means (as I have tried to show in *The Logic of Scientific Discovery,* sections 31 to 46) in the most testable way.

Admittedly, if we do not know how to test a theory we may be doubtful whether there is anything at all of the kind (or level) described by it; and if we positively know that it cannot be tested, then our doubts will grow; we may suspect that it is a mere myth, or a fairy-tale. *But if a theory is testable, then it implies that events of a certain kind cannot happen; and so it asserts something about reality.* (This is why we demand that the more conjectural a theory is, the higher should be its degree of testability.) Testable conjectures or guesses, at any rate, are thus conjectures or guesses about reality; from their uncertain or conjectural character it only follows that our knowledge concerning the reality they describe is uncertain or conjectural. And although only that is certainly real which can be known with certainty, it is a mistake to think that only that is real which is known to be certainly real. We are not omniscient and, no doubt, much is real that is unknown to us all. It is thus indeed the old Berkeleian mistake (in the form 'to be is to be known') which still underlies instrumentalism.

Theories are our own inventions, our own ideas; they are not forced upon us, but are our self – made instruments of thought: this has been clearly seen by the idealist. But some of these theories of ours can clash with reality; and when they do, we know that there is a reality; that there is something to remind us of the fact that our ideas may be mistaken. And this is why the realist is right.

Thus I agree with essentialism in its view that *science is capable of real discoveries,* and even in its view that in discovering new worlds our intellect triumphs over our sense experience. But I do not fall into the mistake of Parmenides – of denying reality to all that is colourful, varied, individual, indeterminate, and indescribable in our world.

Since I believe that science can make real discoveries I take my stand with Galileo against instrumentalism. I admit that our discoveries are conjectural. But this is even true of geographical explorations. Columbus' conjectures as to what he had discovered were in fact mistaken; and Peary could only conjecture – on the basis of theories – that he had reached the Pole. But these elements of conjecture do not make their discoveries less real, or less significant.

There is an important distinction which we can make between two kinds of scientific prediction, and which instrumentalism cannot make; a distinction which is connected with the problem of scientific discovery. I have in mind the distinction between the prediction of *events of a kind which is known,* such as eclipses or thunderstorms on the one hand and, on the other hand, the prediction of *new kinds of events* (which the physicist calls 'new effects') such as the prediction which led to the discovery of wireless waves, or of zero-point energy, or to the artificial building up of new elements not previously found in nature.

It seems to me clear that instrumentalism can account only for the first kind of prediction: if theories are instruments for prediction, then we must assume that their purpose must be determined in advance, as with other instruments. Predictions of the second kind can be fully understood only as discoveries.

It is my belief that our discoveries are guided by theory, in these as in most other cases, rather than that theories are the result of discoveries 'due to observation'; for observation itself tends to be guided by theory. Even geographical discoveries (Columbus, Franklin, the two Nordenskjölds, Nansen, Wegener, and Heyerdahl's Kon-Tiki expedition) are often undertaken with the aim of testing a theory. Not to be content with offering predictions, but to create new situations for new kinds of tests: this is a function of theories which instrumentalism can hardly explain without surrendering its main tenets.

But perhaps the most interesting contrast between the 'third view' and instrumentalism arises in connection with the latter's denial of the descriptive function of abstract words, and of disposition-words. This doctrine, by the way, exhibits an essentialist strain within instrumentalism – the belief that events or occurrences or 'incidents' (which are directly observable) must be, in a sense, more real than dispositions (which are not).

The 'third view' of this matter is different. I hold that most observations are more or less indirect, and that it is doubtful whether the distinction between directly observable incidents and whatever is only indirectly observable leads us anywhere. I cannot but think that it is a mistake to denounce Newtonian forces (the 'causes of acceleration') as occult, and to try to discard them (as has been suggested) in favour of accelerations. For accelerations cannot be observed any more directly than forces; and they are *just as dispositional:* the statement that a body's velocity is accelerated tells us that the body's velocity in the next second from now will exceed its present velocity.

In my opinion *all universals are dispositional.* If 'breakable' is dispositional, so is 'broken', considering for example how a doctor decides whether a bone is broken or not. Nor should we call a glass 'broken' if the pieces would fuse the moment they were put together: the criterion of being broken is behaviour *under certain conditions.* Similar1y, 'red' is dispositional: a thing is red if it is able to reflect a certain kind of light – if it 'looks red' in certain situations. But even 'looking red' is dispositional. It describes the disposition of a thing to make onlookers agree that it looks red.

No doubt there are *degrees* of dispositional character: 'able to conduct electricity' is dispositional in a higher degree than 'conducting electricity now' which is still very highly dispositional. These degrees correspond fairly closely to those of the conjectural or hypothetical character of theories. But there is no point in denying reality to dispositions, not even if we deny reality to all universals and to all states of affairs, including incidents, and confine ourselves to using that sense of the word 'real' which, from the point of view of ordinary usage, is the narrowest and safest: to call only physical bodies 'real', and only those which are neither too small nor too big nor too distant to be easily seen and handled.

For even then we should realize that 'every description uses… universals; every statement has the character of a theory, a hypothesis. The statement, "Here is a glass of water," cannot be (completely) verified by any sense-experience, because the universals which appear in it cannot be correlated with any particular sense-experience. (An "immediate experience" is *only once* "immediately given"; it is unique.) By the word "glass", for example, we denote physical bodies which exhibit a certain *law-like behaviour;* and the same holds of the world "water".'

I do not think that a language without universals could ever work; and the use of universals commits us to asserting, and thus (at least) to conjecturing, the reality of dispositions – though not of ultimate and inexplicable ones, that or essences. We may express all this by saying that the customary distinction between *'observational terms'* (or *'non-theoretical terms')* and *theoretical terms* is mistaken, since all terms are theoretical to some degree, though some are more theoretical than others; just as we said that all theories are conjectural, though some are more conjectural than others.

But if we are committed, or at least prepared, to conjecture the reality of forces, and of fields of forces, then there is no reason why we should not conjecture that a die has a definite *propensity* (or disposition) to fall on one or another of its sides; that this propensity can be changed by loading it; that propensities of this kind may change continuously; and that we may operate with fields of propensities, or of entities which determine propensities. An interpretation of probability on these lines might allow us to give a new physical interpretation to quantum theory – one which differs from the purely statistical interpretation, due to Born, while agreeing with him that probability statements can be tested only statistically.[35] And this interpretation may, perhaps, be of some little help in our efforts to resolve those grave and challenging difficulties in quantum theory which today seem to imperil the Galilean tradition.

NOTES

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[i] I emphasize here the diurnal as opposed to the annual motion of the sun because it was the theory of the diurnal motion which clashed with Joshua 10, 12.f., and because the explanation of the diurnal motion of the sun by the motion of the earth will be one of my main examples in what follows. (This explanation is, of course, much older than Copernicus even than Aristarchus-and it has been repeatedly re-discovered; for example by Oresme.)

[ii] '. . . Galileo will act prudently', wrote Cardinal BelIarmino (who had been one of the inquisitors in the case against Giordano Bruno) '. . . if he will speak hypothetically, *IX suppositione ...*: to say that we give a better account of the appearances by supposing the earth to be moving, and the sun at rest, than we could if we used eccentrics and epicycles is to speak properly; there is no danger in that, and it is all that the mathematician requires.' Cf. H. Grisar, *Galileistudien,* 1882, Appendix ix. (Although this passage makes Bellarmino one of the founding fathers of the epistemology which Osiander had suggested some time before and which I am going to call 'instrumentalism', Bellarmino-unlike Berkeley was by no means a convinced instrumentalist himself, as other passages in this letter show., He merely saw in instrumentalism one of the possible ways dealing with inconvenient scientific hypotheses. The same might well be true of Osiander. See also note 6 below.)

[iii] The quotation is from Bacon's criticism of Copernicus in the *Novum Organum,* II, 36. In the next quotation (from *De revoltitionibus)* I have translated the term *'verisimilis'* by 'like the truth'. It should certainly not be translated here by 'probable'; for the whole point here is the question whether Copernicus' system is, or is not, similar in structure to the world; that is, whether it is similar to the truth, or truth like. The question of degrees of certainty or probability does not arise. For the important *problem of truthlikeness of verisimilitude,* see also ch. 10 below, especially sections iii, x, and xiv; and *Addendum6,*

[iv] See also ch. 6, below

[v] The most important of them are Mach, Kirchhoff, Hertz, Duhem, Poincaré, Bridgman and Eddington-all instrumentalists in various ways.

[vi] Duhem, in his famous series of papers. *'Sözein to phainómena' (Ann.de philos. chrétienne, anneé* 79, tom 6, 1908, nos. 2 to 6), claimed for instrumentalism a much older and much more ilIustrious ancestry than is justified by the evidence. For the postulate that, with a our causal hypotheses, we ought to 'explain t*he observed facts', r*ather than 'do violence to them by trying to squeeze or fit them into our theories' (Aristotle, *De Caelo,* 293a25;296b6; 297a4, b24ff; *Met.* 1073b37, 1074al) has little to do with the instrumentalist thesis that our theories *cannot explain* the facts). Yet this postulate is essentially the same as that we ought to '*preserve the phenomena'* or 'save' them *([dia-]sõzein ta phainnmena).* The phrase seems to be connected with the astronomical branch of the Platonic School tradition. (See especially the most interesting passage on Aristarchus in Plutarch's *De Faeie in Orbe Lunae, 923a;* see also 933a for the 'confirmation of the cause' by the phenomena, and Cherniss' note *a* on p.168 of his edition of this work of Plutarch's; furthermore, Simplicius, commentaries on *De Caelo* where the phrase occurs e.g. on pp. 497 1.21, 506 1.10, and 488 1.23 f, of Heiberg's edition, in commentaries on *De Caelo* 293a4 and 292b10.) We may well accept Simplicius' report that Eudoxus, under Plato's influence, in order to account for the observable phenomena of planetary motion, set himself the task of evolving an abstract geometrical system of rotating spheres *to which he did not attribute any physical reality.* (There seems to be some resemblance between this programme and that of the *Epinomis,* 990-1, where the study of abstract geometry-of the

theory of the irrationals, 990d-991 b-is described as a necessary preliminary to planetary theory; another such preliminary is the study of number-i.e. the odd and the even, 990c.) Yet even this would not mean that either Plato or Eudoxus accepted an instrumentalist epistemology: they may have consciously (and wisely) confined themselves to a preliminary problem.

[vii] But they seem to have forgotten that Mach was led by his instrumentalism to fight against atomic theory-a typical example of *the obscurantism of instrumentalism* which is the topic of section 5 below.

[viii] I have explained Bohr's 'Principle of Complementarity' as I understand it after many years of effort. No doubt I shall be told that my formulation of it is unsatisfactory. But if so I am in good company; for Einstein refers to it as 'Bohr's principle of complementarity, a sharp formulation of which . . . I have been unable to attain despite much effort which I have expended on it.' Cf. *Albert Einstein: Philosopher-Scientist,* ed. by P. A. Schilpp, 1949, p. 674

[ix] The issue has been confused at times by the fact that the instrumentalist criticism of(ultimate) explanation was expressed by some with the help of the formula: the aim of science is *description rather than explanation.* But what was here meant by 'description' was the description *of the ordinary empirical world;* and what the formula expressed, indirectly, 1was that those theories which do not describe *in this sense* do not explain either, but are nothing but convenient instruments to help us in the description of ordinary phenomena.