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DOES ORTHODOX QUANTUM THEORY UNDERMINE, OR SUPPORT, SCIENTIFIC REALISM?

BY NICHOLAS MAXWELL

I

It is usually taken for granted that orthodox quantum theory poses a serious problem for scientific realism, in that the theory is empirically extraordinarily successful, and yet has instrumentalism built into it. This paper stands this view on its head. I shall show that orthodox quantum theory suffers from a number of severe (if not always noticed) defects precisely because of its inbuilt instrumentalism. This defective character of orthodox quantum theory thus undermines instrumentalism, and supports scientific realism. I go on to consider whether there is here the basis for a general argument against instrumentalism.

The natural answer to give to the question posed in the title is that orthodox quantum theory (OQT) *undermines* scientific realism, or at least poses a serious problem for the view. Both realists and anti-realists (or instrumentalists) tend to agree on this point. A realist like Karl Popper finds it necessary to oppose the orthodox or Copenhagen interpretation of quantum theory on the grounds that it is incompatible with scientific realism (Popper 1982). By contrast, an instrumentalist like Bas van Fraassen (1980) is free to welcome OQT precisely because of its instrumentalist, anti-realist implications. Both would agree that OQT, as it exists at present, supports instrumentalism rather than realism.

Richard Miller has recently put the point like this:

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> Modern quantum physics has a unique status in debates over scientific realism. It is the one well-established field of natural science that is widely thought to require an anti-realist interpretation for reasons internal to the field itself. Of course, if arguments for a general anti-realist view of science are right, then quantum physics, along with every other field, ought to be seen in an anti-realist way. What is striking – and frightening for people with strong realist inclinations – is that the internal content of quantum physics itself seems to have dramatically anti-realist implications (Miller 1987, p. 515).

In this paper I shall turn the tables on this generally accepted view. Far from undermining realism, OQT provides powerful arguments in support of scientific realism.

II

What do I mean by 'scientific realism'? There are four ingredients to the version of scientific realism I appeal to (and wish to uphold) in this paper.

(a) It is legitimate for science to aim at improving knowledge of aspects of the world that are in principle unobservable – aspects that include entities like electrons and quarks (if they exist), and whatever exists inside black holes.

(b) It is legitimate, in general, to interpret fundamental physical theories 'realistically', as asserting the existence of entities, whether observable or unobservable, that the theories are ostensibly about. Thus, for example, it is proper (but not obligatory) to interpret Newtonian theory (NT) as a theory about *point-particles*, hypothetical physical entities which have mass and gravitational charge but which, at any instant, occupy no more space than a geometrical point. Given this 'realistic' interpretation, NT asserts the existence of point-particles, and specifies laws regulating their interaction. Likewise, it is proper to interpret Maxwellian electrodynamics as a theory about the *electromagnetic field*, a hypothetical physical entity continuously spread out in space, the state of which at any point at any instant (relative to any reference frame) can be specified in terms of the values of the electric and magnetic vectors at that point. Maxwell's equations specify how the field evolves in space and time. Given this 'realistic' interpretation, the theory asserts that the electromagnetic field, as characterized by the theory, does actually exist in physical space and time.

(c) We should not expect physical entities *precisely* like those postulated by fundamental physical theories to exist – until we have formulated an empirically successful, unified 'theory of everything' – a serious candidate for providing us with the truth about the nature of

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physical reality. Until such a theory has been formulated, we should expect our theories, however empirically successful, to be only approximately correct and thus, strictly speaking, *false*. In order to make it possible that our current theories, despite being false, are nevertheless about actually existing entities, we need to *define* the entities that our theories are about in a very imprecise way. If we define the electron to be an entity that obeys the laws of classical electrodynamics, the electron in this precise sense does not exist, granted that classical electrodynamics is false. If however we define the electron to be an entity which has such and such a charge and rest mass (within such and such limits), nothing else being built into its definition, then it is quite likely that electrons in this imprecise sense do actually exist. The existence of electrons in this imprecise sense does not require classical electrodynamics, or quantum electrodynamics, to be precisely true.

(d) In order to attain the goal of formulating a true, unified 'theory of everything' that specifies the precise nature of the fundamental physical entities of which everything is composed, we need to proceed by putting forward theories that make precise assertions about the nature of unobservable physical entities – even though these theories will subsequently turn out to be false, and the precise entities that the theories postulate will turn out to be non-existent. In order to acquire knowledge of physical reality, in other words, we need to put forward and refute a succession of precise conjectures about the nature of physical reality, even if these conjectures are at best only approximately correct about some limited aspect of physical reality.

This, then, is the version of scientific realism that I espouse here.

III

OQT appears to pose a serious problem for scientific realism, in this sense, essentially because OQT evades, and does not solve, the quantum wave/particle problem.

Consider the famous two-slit experiment. Photons, or electrons (or even atoms), having a precise momentum, are directed at a two-slitted screen, and are detected at a second screen beyond (see figure 1). In appropriate circumstances, interference bands are detected at the second screen, a result that can be readily explained if it is assumed that each quantum system is an extended wave-like entity with wavelength $\lambda = h/p$, where h is Planck's constant and p is the momentum to be associated with the quantum systems in the direction of flight. It is all but impossible to see how this experimental result (along with countless others) can be explained in any other way except by supposing that the

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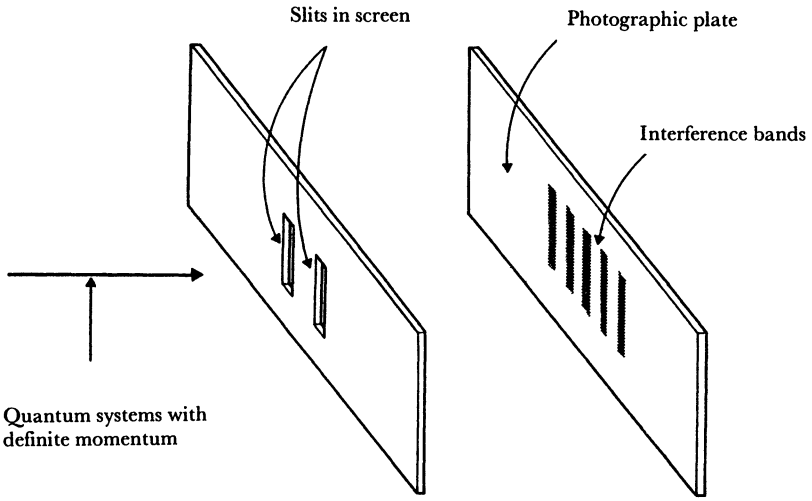

Figure 1 The Two-Slit Experiment

quantum systems are extended wave-like entities. But this very experiment (along with countless others) seems to establish equally conclusively that individual quantum systems cannot possibly be extended wave-like systems, in that each system is detected as a minute dot on the second screen. The wave-like aspects of the quantum systems determine the *probability* of a particle-like detection. And, quite generally, the wave-like aspects of quantum systems are always detected experimentally as a statistical feature of a great number of particle-like detections. In these circumstances, the urgent and fundamental question arises: what sort of physical entities are photons, electrons, atoms and other quantum systems, given that they possess these ostensibly contradictory wave-like and particle-like aspects? A realist interpretation (or version) of quantum theory (QT) requires a solution to this basic wave/particle problem, since otherwise QT fails to specify consistently what sort of entities quantum systems are.

OQT was developed, quite explicitly, by Heisenberg, Bohr, Born, Dirac and others, in such a way that the theory did not need to provide an answer to this fundamental problem. OQT was developed, not as a theory about quantum systems *per se*, but rather as a theory about the results of performing measurements on quantum systems. Just because there is no solution to the wave/particle problem, the ψ-function of OQT, which specifies the 'quantum state' of a system, cannot be interpreted as specifying the actual physical state of the system; rather it is to be interpreted as containing probabilistic information about what

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would be the results of performing measurements of various observables of the system (or ensemble of systems), such as position, momentum, energy. Thus $|\psi|^2 dV$ gives the probability of finding the system in volume element dV if a position measurement is made; it does not tell us anything about the position of the system in the absence of measurement. Quantum observables arise in the context of measurement, and cannot be thought of as being possessed by quantum systems in the absence of measurement.

In a little more detail, non-relativistic OQT includes the following two basic postulates:

(1) $$i\hbar \frac{\partial \psi}{\partial t} = -\frac{\hbar^2}{2m} \nabla^2 \psi + V\psi$$

(2) If a measurement of observable A is performed on a system in a state ψ, then the probability of obtaining a value between a_r and $a_{r+dr} = |(\alpha_r, \psi)|^2 dr$, where a_r and α_r are eigenvalues and eigenvectors of the Hermitian operator $\hat{A}$ corresponding to the observable A.

(1) is Schrödinger's famous time-dependent equation: it specifies the deterministic fashion in which the quantum state ψ evolves in time when not subject to measurement. (2) specifies how, in general, probabilistic predictions about measurement are to be extracted from the state vector ψ.

It is sometimes claimed that quantum field theory successfully solves the quantum wave/particle problem. This is not the case. Orthodox quantum field theory is just as restricted to making predictions about *measurements* as non-relativistic quantum theory is.

Thus, quite generally, OQT is a theory that is restricted to making predictions about the results of performing measurements on quantum systems: it fails to specify how the actual physical states of quantum systems evolve in physical space and time in the absence of measurement. OQT has instrumentalism built into it, and cannot be interpreted realistically, due to the lack of a solution to the key wave/particle problem. OQT can of course be interpreted realistically in the very weak sense indicated in point (c) above in part II; but it cannot be interpreted realistically in the strong sense indicated in (d), and as required by (a) and (b).

IV

So far it looks as if the widely held view that OQT poses a problem for scientific realism is correct: for here is this empirically extraordinarily

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successful theory of OQT which must be interpreted instrumentalistically and cannot, it seems, be interpreted realistically. What dramatically turns the tables on this conclusion is the following consideration. *Precisely because OQT fails to solve the wave/particle problem, and thus cannot be interpreted realistically, OQT suffers from the following six devastating defects*:

(i) It is imprecise.
(ii) It is ambiguous.
(iii) It is *ad hoc*.
(iv) It is non-explanatory.
(v) It is restricted in scope.
(vi) It obstructs unification with general relativity.

I take these six defects in turn (for earlier expositions of some of these points see Maxwell 1972b, 1976a, 1982, 1988; Bell 1973, 1987).

(i) As a result of being restricted to making predictions about the results of *measurements* – due to the lack of a solution for the wave/particle problem – OQT is very seriously *imprecise*. The crucial point here is that the notion of 'measurement' is inherently imprecise. Precisely what physical conditions must obtain for a measurement to take place? What are the precise quantum mechanical conditions – the necessary and sufficient physical conditions – for a measurement to occur? OQT can provide only more or less vague answers to these questions. It does not help to say that measurement takes place when a physical process is irreversible, macroscopic, or approximately describable in classical terms: these conditions are all as imprecise as the notion of measurement itself is.

(ii) OQT is *ambiguous* concerning the crucial and fundamental question as to whether the laws of the quantum domain are deterministic or probabilistic. Looked at in one way, the theory asserts that they are deterministic, in that the fundamental dynamical equation, Schrödinger's equation, is deterministic. In principle, it may be applied to any evolving system, including one that incorporates measurement, as long as there is a *second* measuring instrument to record the result. Looked at in another way, the theory asserts that the basic laws are probabilistic, in that the theory does appear to assert that measuring processes are inherently *probabilistic* in character. Neither way of looking at the theory is satisfactory, however. The first makes it all but impossible to understand how *probabilistic* predictions emerge from the theory. The second associates probabilistic transitions exclusively with *measurement*. But if probabilistic transitions really do occur in nature, they surely do not occur only when we dub some process a 'measurement'.

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(iii) The theory is grossly *ad hoc.* OQT on its own can only issue in *conditional* predictions about what the result *would* be *if* such and such a measurement were to be performed. In order to obtain actual, *unconditional* predictions, some part of classical physics must be added to the quantum postulates of OQT, for a description of the measurement process. OQT, lacking its own physical ontology, has to depend on classical physics to supply the existence of *something* – namely measuring apparatus. Without this, quantum theory would not be about *anything*! Thus the theory that makes actual physical predictions, about actual physical states of affairs, consists of purely quantum mechanical postulates *plus* some part of classical physics applied to measurement. But *this* theory, quantum plus classical postulates, is severely *ad hoc* in that it consists of conceptually incoherent parts.

(iv) Despite its immense empirical success, OQT is seriously defective from the standpoint of enabling us to *explain and understand* quantum phenomena. There are four reasons for holding this to be the case. (a) No version of QT can enable us fully to explain and understand the quantum domain if it fails to solve the most puzzling feature of all of the quantum domain – the nature of quantum objects, given their apparently contradictory wave-like and particle-like properties. (b) A basic task for QT is to predict and explain complex macro-phenomena in terms of elementary micro-phenomena – so that macro-phenomena can be explained and understood as the outcome of interactions between vast numbers of micro-systems. But this OQT cannot do because it lacks a consistent model for micro-systems, a consistent micro-ontology. The theory can only specify states of micro-systems relative to prior classical descriptions of macro-systems – preparation and measuring devices. Descriptions of micro-states presuppose, as a matter of conceptual necessity, descriptions of macro-states. That which is to be explained must be presupposed! Hence the theory cannot conceivably explain macro-phenomena as arising solely as a result of interactions between large numbers of micro-systems. (c) QT has the task of explaining the (approximate) empirical success of classical physics from purely quantum mechanical postulates. But this, again, OQT cannot do. In any physical application, the theory must presuppose (some part of) classical physics for an account of preparation and measurement devices. Once again, just that which is to be explained must be presupposed. (d) In order to be explanatory, a theory must not be *ad hoc.* But we have already seen that OQT is very severely *ad hoc.* Therefore it is non-explanatory.

(v) OQT is seriously *restricted in scope.* It is standard practice these days to apply QT to states of the cosmos soon after the Big Bang, in

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physical conditions which preclude the very possibility of the existence of anything like preparation and measurement devices. OQT cannot be applied in this way. Only a version of QT which has its own physical ontology could be so applied. Again, some attempts to understand early states of the universe seek to apply QT to the cosmos as a whole (thus creating quantum cosmology). OQT cannot be employed in this way, it being conceptually impossible that the cosmos as a whole should be subject to preparation and measurement.

(vi) OQT obstructs unification with general relativity (GR). For, according to the orthodox version of the theory, a system only has a quantum state in so far as it is subject to preparation and measurement devices which are external to the system in question. In order to quantize GR, space-time itself would need to be given quantum states. In order to do this, according to OQT, it would be necessary to postulate preparation and measurement devices external to space-time. The existence of such devices is somewhat problematic!

It is important to note that these six defects, (i)–(vi), are all consequences of the inherently *instrumentalistic* character of OQT, in turn a consequence of the failure of the theory to solve the wave/particle problem. If a satisfactory solution to the wave/particle problem had been available, then it would not have been necessary to develop QT as a theory exclusively about performing *measurements* on quantum systems; QT could have been developed as a theory ostensibly about quantum systems *per se*, evolving and interacting in physical space and time. Just this is what we find in classical physics. Fundamental dynamical theories of classical physics do ostensibly specify the nature of the physical objects to which they apply. Leaving aside general philosophical objections to scientific realism, no special difficulty arises in interpreting NT realistically, as a theory about point-particles with mass and gravitational charge; no special difficulty arises in interpreting classical electrodynamics realistically, as a theory about the electromagnetic field. As a result, these theories can make physical predictions, even testable predictions, without needing to incorporate the notion of 'measurement' in their basic postulates, in the manner of OQT. It is because OQT fails to solve the wave/particle problem, and thus fails to supply its own (ostensible) physical ontology, that it must be interpreted as a theory about the results of performing *measurements* on quantum systems, which in turn renders the theory imprecise, ambiguous, *ad hoc*, non-explanatory, restricted in scope and resistant to unification.

These six defects of OQT are, I submit, devastating. Taken together, they suffice to establish that OQT, despite its immense empirical success, is not an acceptable physical theory. Furthermore,

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instrumentalists ought to accept this conclusion. Instrumentalists may perhaps reject the idea that science seeks to *explain* phenomena in any strong sense. Perhaps they may think it merely tries to *predict* phenomena by means of confirmed (or empirically adequate) theories. Instrumentalists may thus hold that (iv) above is not really a defect at all. But all instrumentalists ought surely to hold that physics seeks to develop (empirically adequate) theories that are precise, non-ambiguous, non-*ad hoc*, not artificially restricted in scope, and not resistant to unification. Thus for instrumentalists, almost as much as for realists, OQT ought to be regarded as an unacceptable physical theory. It must be emphasized that it is not QT as such that is being characterized as unacceptable, but only QT *given its orthodox interpretation.*

In brief, OQT, because of its inherent instrumentalism, is a profoundly defective physical theory, even to the extent of being unacceptable. Einstein was entirely correct when he remarked 'one simply cannot get around the assumption of reality – if only one is honest. Most . . . [physicists] simply do not see what sort of risky game they are playing with reality – reality as something independent of what is experimentally established' (Einstein 1950).

V

In order to rid instrumentalistic OQT of its defects a version of QT needs to be developed which is open to a *realistic* interpretation. Thus quantum theory, if anything, supports realism rather than instrumentalism. This much has been established so far.

The above argument can however be strengthened to provide a *general* argument against developing inherently instrumentalistic physical theories. The decisive point is this. *Any* fundamental physical theory that is inherently instrumentalistic in the way in which OQT is will suffer from the same defects. A possible exception to this point concerns defect (ii), namely ambiguity. OQT is ambiguous as to whether the laws of the quantum domain are deterministic or probabilistic because of the specific way in which OQT restricts probabilistic transitions to *measurement.* This might not be a feature of *any* inherently instrumentalistic fundamental physical theory. Such a theory will however be (i) imprecise, (iii) *ad hoc*, (v) restricted in scope, and (vi) resistant to unification. The relevant arguments of part IV apply quite generally to *any* theory which is restricted to predicting the results of performing measurements on systems described by the theory. We have here, then, a rather strong general argument against the

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legitimacy of developing inherently instrumentalistic fundamental theories within physics.

VI

Can the argument be generalized further, so that it becomes an argument against instrumentalism as such?

In order to discuss this question, let us consider any fundamental physical theory T, which poses no special problem to being interpreted realistically: unlike OQT, T can be, on the face of it, regarded as being about unobservable physical entities; it specifies how these entities evolve and interact in physical space and time in the absence of measurement. Furthermore, even though the concept of measurement does not figure in the basic concepts of the theory, T is empirically testable.

We need now to consider two different formulations of the theory, namely T interpreted realistically, T_R, and T interpreted instrumentalistically, T_I. Whereas T_R – granted scientific realism – can be interpreted as a theory about unobservable physical entities, T_I must be interpreted as a theory that is exclusively about *observable phenomena*. Instrumentalism demands that any such theory as T be interpreted to assert T_I and no more. Do the above arguments which I have levelled against the acceptability of OQT apply with equal force against T_I?

If T_I has been formulated in such a way that it too, like OQT, is a theory about the results of performing *measurements* on systems, the measuring process being described by some theory other than T_I itself, then T_I will have all the defects that OQT has. In this case, the above arguments apply with equal force to T_I. It seems, however, that T_I need not be similar to OQT in this way. We can imagine that T_I consists of all consequences of T that happen to be about observable states of affairs. The peculiar, and peculiarly undesirable, features of OQT are absent. In this case the above arguments are no longer applicable.

Nevertheless an argument that is somewhat analogous to the above arguments deployed against OQT can be directed against the scientific acceptability of T_I.

Imagine that all physically significant consequences of T are represented in the form of points organized in the form of a pyramid, with T at the apex, it being possible to construct lines running across the pyramid so that propositions corresponding to points on the line imply propositions corresponding to all points below the line (see figure 2). Let us suppose that such a line, L, cutting across the pyramid, divides up consequences of T, and of T_R, into (a) points above the line which represent consequences of T_R about unobservable states of affairs, and

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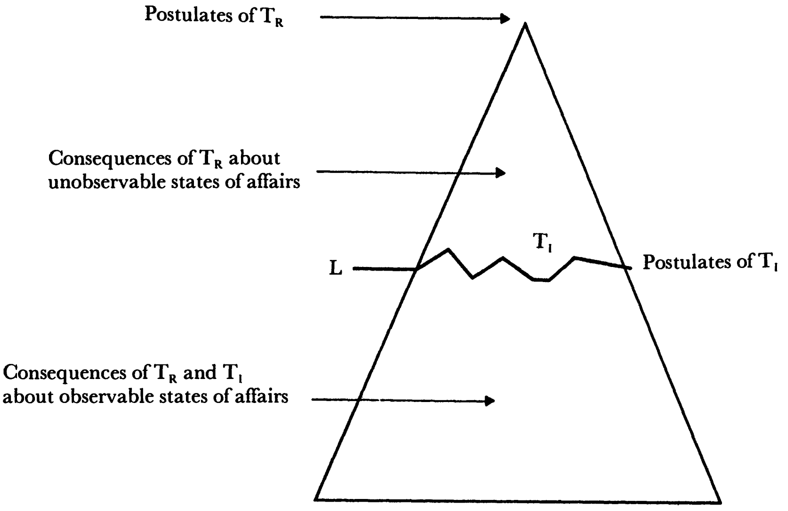

Figure 2 The Consequences of Theory T

(b) points on and below the line which represent consequences of T_R about observable states of affairs.

We may assume that T, or T_R, is an acceptable physical theory on both empirical and non-empirical grounds; in particular, it is precise, non-*ad hoc*, unified. T_I, by contrast, must be *imprecise*, *grossly ad hoc*, and *grossly disunified.*

T_I is *imprecise* because the line dividing off the observable from the unobservable is imprecisely specified. In terms of figure 2, the basic *physical* postulates of T_I, represented by points lying on L, are imprecisely specified.

T_I is *grossly ad hoc* because its physical postulates, represented by points on L in figure 2, are both diverse, and large in number, there being, perhaps, infinitely many of them. Granted scientific realism, all these postulates of T_I can be derived from the very few basic postulates of T_R. Granted instrumentalism, however, this cannot be done. For, granted instrumentalism, statements below L provide descriptive knowledge of the physically actual (because observable), but statements above L provide no such descriptive knowledge. Therefore statements below L cannot be derived from statements above L. More specifically, laws below L are to be interpreted physically as specifying regularities of physically possible states of affairs, some of which may be presumed to be actual (for otherwise scientific knowledge of laws would be merely true *vacuously*). Laws above L, however, cannot be interpreted physically in this way. For this requires laws above L to be

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interpreted as being about physically possible states of affairs, some of which may be presumed to be actual, and this violates instrumentalism. It follows at once that the basic postulates of T, or indeed any set of consequences of T above L, cannot suffice to imply physically interpreted statements of laws below L. Given instrumentalism, the least *ad hoc* formulation of the physical theory T_I possible takes some large subset of points on L as its basic physical postulates. *This* theory, T_I, is grossly *ad hoc* when compared with T realistically interpreted, i.e., T_R. Only dishonesty about what can be derived from what can enable us to pretend that T_I is even remotely as good a theory as T_R.

And, for similar reasons, whereas T_R is, by hypothesis, a unified theory, T_I is *grossly disunified.*

The conclusion is inescapable: physical theories that are precise, non-*ad hoc* and unified when interpreted as being about unobservable entities necessarily become hopelessly imprecise, *ad hoc* and disunified when interpreted instrumentalistically. Instrumentalism is untenable. And this argument against instrumentalism is somewhat similar to, but not the same as, the argument developed above against inherently instrumentalistic OQT.

VII

Objections to the above argument may be raised at a number of points.

First, it may be objected – in flat contradiction to what was argued for in part IV – that OQT has *no* serious defect as a result of its inbuilt instrumentalism. It may be that, in some technical sense, OQT is imprecise, *ad hoc*, ambiguous, non-explanatory, restricted in scope and resistant to unification. It may even be that these features exist, and only exist, because OQT evades rather than solves the wave/particle problem. These features cannot, however, be held to constitute *rational*, *scientific* grounds for rejecting OQT, or for finding it fundamentally defective. The reason for this is quite simple: in the end, in science, evidence alone determines what theory is accepted, what rejected. As long as OQT continues to meet with astonishing empirical success, non-empirical reasons for rejecting OQT do not deserve a moment's consideration.

And furthermore, this 'standard empiricist' assumption (that evidence is the only rational, objective, scientific ground for choice of theory) is central to instrumentalism. Any argument which just assumes that the 'standard empiricist' thesis is wrong, as the above argument against the acceptability of OQT in part IV does, is bound to fail against instrumentalism, since what needs to be established is just

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that non-empirical grounds play a vital role in theory-choice *in addition* to empirical grounds.

Second, there can be no rational scientific grounds for judging OQT to be *false*, granted that it has met with great empirical success with no serious set-backs. To do so would be to judge the scientific acceptability of OQT on grounds other than the purely empirical.

Third, granted that OQT is empirically highly successful, and that there is no serious, equally successful rival version of QT, and granted, further, that OQT is inherently instrumentalistic in character, the standard view that OQT supports instrumentalism and undermines scientific realism must be deemed to be correct.

Fourth, the argument against instrumentalism developed in part VI backfires against scientific realism. It has been admitted in part II that realistically interpreted physical theories, however empirically successful, are in fact, strictly speaking, false (points (c) and (d)). It is just the statements of T_R that are represented as points above L in figure 2 that we should expect to be false. Thus, for example, given either Bohr's or Schrödinger's quantum theory of the atom, it is clear that almost all the earlier theoretical ideas about the nature of atoms are false. (For a critique of scientific realism along these lines, see Laudan 1981.) As a predictive and explanatory theory, then, T_R is only superior to T_I in that, whereas T_R uses *false* statements about unobservables, T_I uses statements about observables that may well be *true*! In brief, T_R is only genuinely predictive and explanatory in so far as it surreptitiously exploits T_I.

I take these objections in turn.

The first objection would be valid if the version of instrumentalism that it appeals to were viable. But it is not. No conception of science can be viable which holds that empirical considerations *alone* determine choice of theory in science, all non-empirical considerations – such as theoretical precision, non-*ad hoc*ness, non-ambiguity, explanatoriness, simplicity and unity – having no role whatsoever. Given any accepted, empirically successful theory T (QT let us say, or GR), we shall inevitably have something like the following circumstances. There will be phenomena A which T successfully predicts; there will be phenomena B to which T applies but has not yet successfully predicted (because here the equations of T have not yet been solved); there will be phenomena C where T appears to make predictions different from what is observed, T ostensibly being refuted; and there will be phenomena D which lie outside the predictive scope of current physical theories. If there are no non-empirical constraints on what is to count as an acceptable theory in physics, then we can easily construct endlessly

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many rival theories to T, all of which will be empirically more successful than T. We have, for example, T*, made up of two basic postulates: (i) for phenomena A, everything occurs as T predicts; (ii) for phenomena B, C and D, everything occurs in accordance with laws L_B, L_C and L_D respectively. Here, L_B, L_C and L_D simply amount to a listing of all the phenomena of B, C and D – taking a phenomenon as a repeatable effect, and thus equivalent to an experimental or observational law. In comparison with T, T* is grossly imprecise, *ad hoc*, non-explanatory, disunified, complicated, horrible, consisting as it does of thousands of conceptually unrelated, distinct postulates. But if all such considerations are ignored, T* ought to be accepted and T ought to be rejected on empirical grounds since T* (a) successfully predicts everything T predicts, (b) is not refuted where T is apparently refuted, and (c) successfully predicts phenomena T does not predict at all. But this does not happen in science; nor ought it to happen. (If it did, scientific progress would come to an end.) Empirically successful theories like T* are not even formulated in science, let alone seriously considered, not because of empirical considerations, but because of *non-empirical* considerations: such theories are much too grotesquely *ad hoc*, complicated and disunified even to qualify as 'theories' at all. Thus versions of instrumentalism which hold that empirical considerations alone determine choice of theory are indefensible.

It does not help at all, it should be noted, to argue with Popper (1959) that if T_2 is more falsifiable (i.e., has greater empirical content) than T_1, then T_2 ought to be preferred to T_1 even if exclusively empirical considerations are neutral between the two theories, for of course T* is more falsifiable (has more empirical content) than T.

Nor does it help to argue that T is more acceptable than T* *on exclusively empirical grounds* because precise, non-*ad hoc*, explanatory, simple, unified theories are inherently more verifiable than imprecise, *ad hoc*, non-explanatory, complex, disunified theories are. The moment one adopts this sort of view, one must accept that, in so far as OQT is imprecise, *ad hoc*, non-explanatory and disunified, it is *not* acceptable on empirical grounds. The inherently instrumentalistic character of OQT becomes seriously damaging to the acceptability of the theory.

As to the second objection, it is important to appreciate that throughout science – throughout pure and applied research and standard technological applications of science – potential laws and theories are persistently assumed to be *false* because of their *ad hoc*, complex, non-explanatory, disunified character. This point is indeed implicit in the above reply to the first objection. Consider the task of

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designing and building a bridge, of size and type B, in some specific locality L. A number of scientific laws and theories will be used to calculate the strength and durability of the bridge. None of these laws and theories will have been applied in conditions precisely the same as those of this bridge B in locality L. All these laws and theories can be modified, in grossly *ad hoc* ways, so that, as far as empirical consequences are concerned, the new laws and theories *only* differ from the old in B-type and L-type circumstances. Before the bridge is built, purely *empirical* considerations favour neither the old nor the new laws and theories. These 'new' laws and theories will, however, quite properly, be dismissed out of hand as being obviously *false*, simply because of their grotesquely *ad hoc* character. In a similar way, a grotesquely *ad hoc* theory, such as NT + KL, taken to assert 'all bodies *except for those of the solar system* interact in accordance with NT, and the bodies of the solar system move in accordance with Kepler's laws (KL)' would be dismissed out of hand as being obviously false, just because of its *ad hoc* character, even in the absence of any evidence against the theory.

All this applies with equal force to OQT, made up as the theory is of two conceptually quite distinct parts: quantum postulates applied to quantum systems, and some part of classical physics applied to measuring instruments (QT + CP). This theory, QT + CP, is much too grossly *ad hoc* to be true. If, during the last sixty years or so, most physicists have not held OQT to be obviously false in this way, and for these reasons, that is because most physicists have not appreciated just how grossly *ad hoc* OQT is. In order to appreciate this fully, it is essential to appreciate that OQT draws upon some part of classical physics for a treatment of measurement, not out of practical convenience only, but as a matter of conceptual necessity, because of the inherently instrumentalistic character of OQT – so that a realistic version of QT would be free of this defect. Only in this light does one appreciate that OQT really does have the *ad hoc* structure QT + CP, being *ad hoc* in the same sort of way as NT + KL. General acceptance of instrumentalism among physicists during the last sixty years or so has *blinded* these physicists to the grossly *ad hoc* character of OQT, preventing them from appreciating just how defective, just how unacceptable the theory is. Viewed from the perspective of scientific realism, it is obvious that OQT is defective; viewed from the perspective of instrumentalism, this is much less obvious – since, from this standpoint, it is not obvious that, if quantum theory abandons realism, it also, inevitably, becomes unacceptably defective, on grounds that all instrumentalists ought to endorse.

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The above demolition of the first and and second objections automatically demolishes the third objection as well.

The fourth objection exposes a central difficulty facing scientific realism: in order to overcome it, orthodox assumptions about the nature of science must be modified. In order to rebut the objection it suffices to show that it is scientifically reasonable to hold that a succession of good, accepted, but *false* physical theories $T_1 \ldots T_n$, which apparently contradict each other sharply about the physical nature of postulated fundamental physical entities $E_1 \ldots E_n$, nevertheless can be interpreted as providing progressively *improving* knowledge of the basic physical entities of the universe, the theories as a result providing progressively improving predictions and explanations of phenomena.

This can be shown as follows. The first point to appreciate is that, even though, at one level, $T_1 \ldots T_n$ contradict one another sharply about the entities they postulate, nevertheless it may well be possible to extract from $T_1 \ldots T_n$ somewhat less precise statements, $T_1(E) \ldots T_n(E)$, let us say, about unobservable entities, such that these statements are all compatible, and, furthermore, $T_n(E)$ logically implies $T_{n-1}(E)$, and so on down to $T_1(E)$, but not *vice versa.* In these circumstances, $T_1 \ldots T_{n-1}$ can be regarded as progressively specifying more and more about the nature of entities postulated by T_n.

What makes this possible is that, given any realistically interpreted theory, there will always be infinitely many different ways of interpreting the theory realistically, depending on how precisely or imprecisely the basic entity of the theory is specified. Thus, for example, NT can be interpreted as being about point-particles which obey all the laws of NT; or about point-particles which obey *some* centrally directed, spherically symmetrical, infinitely rigid, distance-dependent force laws; or about point-particles which obey forces that are repulsive as well as attractive; which have different strengths as one goes from one kind of particle to another; which are not spherically symmetrical; not centrally directed; not infinitely rigid. We have here increasingly imprecisely specified entities for NT, and therefore entities that are increasingly likely to exist.

Assume, now, that physicalism is true. Assume, that is, that the world is made up of a very few different sorts of fundamental physical entities E, which interact in accordance with a precisely determining unified pattern of physical law (perhaps probabilistic). Let T_t be the corresponding as-yet-to-be-discovered true, unified, physical 'theory of everything'. We can now say that the series $T_1 \ldots T_n$ constitutes progress towards the truth T_t, if and only if T_n can be 'derived' from T_t, and T_{n-1} can be 'derived' from T_n, and so on down to T_1, but not *vice*

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versa, so that no T_{r+1} can be 'derived' from any T_r for $r = 1 \ldots n-1$, and T_t cannot be 'derived' from T_n.

Here, to say that T_1 can be 'derived' from T_2 is to say that a theory structurally and empirically equivalent to T_1 emerges from T_2, as quantities which T_2 asserts to be different from zero tend to zero. It is in this sense that KL can be 'derived' from NT, and NT in turn can be 'derived' from GR. Consider, for example, the 'derivation' of KL from NT. This can be done in three steps. (i) Restrict NT to N-body systems confined to some finite volume. (ii) Keep the mass of one body constant (the 'sun') and let the masses of the other bodies (the 'planets') tend to zero. NT implies that, in the limit, paths traced out by the planets are Keplerian ellipses. (iii) Re-interpret laws obtained in (ii) so they apply to solar-type systems, with the mass of one body, the sun, being very much larger than the masses of the other bodies, there being sufficient distance between the other bodies for gravitational attraction between them to be small. The result contains KL. It is step (iii) which ensures that the outcome is incompatible with the premises of the 'derivation', namely NT.

'Derivations' of this type, which are genuinely predictive and explanatory, exist throughout physics. Whenever, from T_2, we can 'derive' a theory T_1, in the above sort of way, we can also derive, in a *logically valid* way, an approximation statement of the form: 'entities E_2, postulated by T_2, evolve *as if* they are entities E_1, postulated by T_1, in circumstances C_2, to such and such an approximation A_2'. Thus, granted that T_1 can be 'derived' from T_2, and T_2 in turn can be 'derived' from T_3, right up to T_n and T_t, we can indicate what it is that the rth theory, T_r, asserts about the basic entities E of the universe. It is: 'E evolves as if it is E_r, postulated by T_r, in circumstances C_r, to an approximation A_r'. This assertion is derivable from T_t in a logically valid way; it is thus *true* of the basic physical entities E of the universe: it is the true realistic core of the false theory T_r. Furthermore, the predictive and explanatory power of T_r has much to do with the fact that the above realistic assertion of T_r is *true* (being logically derivable from T_t).

For this argument to work, physicalism must be an integral part of current scientific knowledge. This is indeed the case according to the *aim-oriented empiricist* conception of science depicted in figure 3, and defended at length elsewhere (see Maxwell 1972a, 1974, 1976b, 1977, 1979, 1980, 1984, 1993; Kneller 1978, pp. 80–95). It is hard to see how there can be an adequate response to the above fourth objection if the idea that physicalism is a part of scientific knowledge is rejected.

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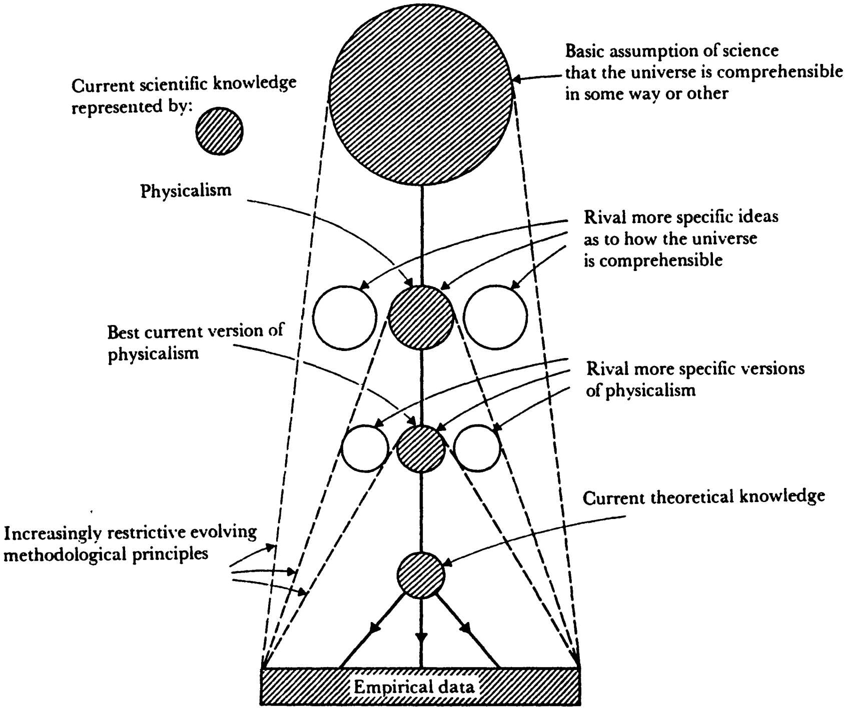

Figure 3 Aim-Oriented Empiricism

VIII

Granted scientific realism – and granted, especially, the argument of this paper – the real challenge becomes to develop a fully realistic version of QT free of the defects of OQT, and experimentally distinguishable from, and more successful than, OQT. One approach to this task is to adopt the view that the quantum domain is fundamentally *probabilistic* in character, quantum entities (such as electrons and atoms) being quite different from anything encountered in classical physics because of the intrinsically *probabilistic* way in which they evolve and interact (Maxwell 1976a, 1982, 1988). The task then becomes to specify the precise quantum mechanical conditions under which probabilistic transitions occur (Maxwell 1972b). Several suggestions along these lines have been made in recent years (Maxwell

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1982, 1988; Bedford and Wang 1975; Bussey 1984; Penrose 1985; Ghirardi *et al.* 1986). As a result of accepting instrumentalism, most physicists have, until recently, been blind to the need to develop a fully realistic version of QT free of the defects of OQT. Acceptance of instrumentalism by physicists adversely affects physics itself.

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