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Induction and Scientific Realism: Einstein Versus van Fraassen Part Three: Einstein, Aim-oriented Empiricism and the Discovery of Special and General Relativity*

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According to Popper, Einstein is a falsificationist. Thus Popper declares: 'Einstein *consciously seeks error elimination*. He tries to kill his theories: he is *consciously critical* of his theories' (Popper [1972], p. 25). And elsewhere Popper declares: 'what I have done is mainly to make explicit certain points which are implicit in the work of Einstein' (Whitrow [1973], p. 23). Paul Feyerabend, on the other hand, holds Einstein to be a methodological 'opportunist or cynic' or, in other words, a methodological anarchist (Feyerabend [1978], p. 213, note; see also p. 18, and pp. 56–7 and note). For Arthur Fine, Einstein adopts a view close to the natural ontological attitude (NOA). Fine writes: 'In its antimetaphysical aspect, NOA is at one with Einstein's motivational realism' (Fine [1986], p. 9). As far as I know, van Fraassen has not yet claimed that Einstein is a constructive empiricist but, amazingly, the claim has been made on his behalf by Fine, who writes:

> Indeed it would not be too far off if we summarized Einstein's views this way: 'Science aims to give us theories which are empirically adequate; and acceptance of a theory involves as belief only that it is empirically adequate' [a straight quote from van Fraassen's *Scientific Image*] . . . My argument, then, is that if we understand Einstein in the way that he asks us to, his own realist-sounding language maps out a position closer to constructive empiricism than to either 'metaphysical realism' or 'scientific realism'. (Fine 1986, p. 108.)

The temptation to see one's own view in Einstein's thought is, it seems, all but irresistible. Do not I also give way to this temptation in attributing the views I have defended in parts one and two of this paper to Einstein?

I must confess that I did not arrive at these views as a result of reading

* I am grateful to Harvey Brown for critical comments concerning the first draft of this section of the paper.

Einstein. I developed conjectural essentialism in order to make sense of the idea that it is possible that something *exists* which is responsible for order in the universe despite Hume's arguments to the contrary, and I developed aim-oriented empiricism during the course of criticizing Popper, and as the key to the solution to the problem of induction (see Maxwell [1968, 1972a, 1974, 1979]). At first I was convinced that standard empiricism had such a dogmatic stranglehold on science that it would be quite impossible for any scientist to uphold aim-oriented empiricism.[1] But it then began to dawn on me that Einstein, in developing special and general relativity, had made essential use of aim-oriented empiricism—his success owing much to his exploitation of the view in scientific practice.[2] I then discovered that Einstein had actually advocated key tenets of aim-oriented empiricism in an increasingly explicit way as the years went by—but had been ignored and misunderstood because of the powerfully prevailing influence of standard empiricism.[3] Here are my reasons for holding this view. (I have not, incidentally, found conjectural essentialism in Einstein's thought.)

Einstein invented aim-oriented empiricism in scientific practice in order to overcome a severe scientific crisis. The crisis was the demise of classical physics as a result of Planck's 1900 quantum theory of blackbody radiation. Initially, it was only Einstein who understood just how grave, how wholesale, the crisis was. In his 'Autobiographical Notes' he puts the matter like this.

> it [became] clear to me as long ago as shortly after 1900, i.e. shortly after Planck's trailblazing work, that neither mechanics nor electrodynamics could (except in limiting cases) claim exact validity. By and by I despaired of the possibility of discovering the true laws by means of constructive efforts based on known facts. The longer and the more despairingly I tried, the more I came to the conviction that only the dicovery of a universal formal principle could lead us to assured results. The example I saw before me

1 For the distinction between aim-oriented empiricism and standard empiricism see section 2 of part one of this paper.

2 It may be asked how it is possible for Einstein to be the first to exploit aim-oriented empiricism explicitly in scientific practice if what I have argued in part one of this paper is correct, and aim-oriented empiricism is inherent in *all* of science. The answer is straightforward. Actual scientific practice is massively influenced by the long-standing conviction of the scientific community that science ought to proceed in accordance with standard empiricism. The result is that scientific practice is a mixture of aim-oriented empiricism and standard empiricism. Aim-oriented empiricism is implemented in a surreptitious, hypocritical fashion, overlaid by the conviction that science ought to proceed in accordance with standard empiricism. As a result, physicalism and more specific metaphysical blueprints are not acknowledged within the intellectual domain of scientific knowledge, and this sabotages the possibility of putting the rational method of discovery of aim-oriented empiricism into sustained scientific practice. Explicit scientific exploitation of aim-oriented empiricism is frustrated if not prohibited. (See Maxwell [1976b and 1984] for further discussion of this point.) Einstein's great lucidity about fundamental matters led him to put aim-oriented empiricism into scientific practice unconstrained by hypocritical allegiance to standard empiricism.

3 Gerald Holton comes the closest to interpreting Einstein in the way that I do. One difference, of course, is that Holton espouses his 'themata' conception of science and not aim-oriented empiricism: see Holton [1973].

was thermodynamics. The general principle was there given in the theorem: the laws of nature are such that it is impossible to construct a *perpetuum mobile* (of the first and second kind). How, then, could such a universal principle be found? (Einstein [1949], pp. 51–3.)

This, I claim, is the beginning of the explicit employment of aim-oriented empiricism in scientific practice. It is to this that Einstein owed his extraordinary success in discovering special and general relativity. Soon after 1900, Einstein found himself bereft of guidelines as to how to proceed because Planck's 'trailblazing' result cast into doubt the whole of classical physics. Ordinarily a theoretical physicist can proceed by applying, extending, modifying or reinterpreting existing established physical theory. This is how classical physics had developed so far, after Newton. Einstein, however, found himself in what seemed an unprecedented situation. Existing physical theory—especially Newtonian mechanics and Maxwell–Lorentzian electrodynamics—must be fundamentally wrong, given Planck's result. A fundamentally new kind of theory was needed to stand in their stead. But, in order to discover this new theory, it would be useless to try to extend or modify existing physical theories, in the ordinary manner, since it was just these theories which were fundamentally wrong. In order to proceed Einstein was obliged to invent a new method of discovery for theoretical physics—a rational method capable of leading to the discovery of fundamentally new kinds of theories.

Within the framework of standard empiricism there can be no such rational method of discovery. If the *only* way in which theories can be rationally assessed in physics is by means of empirical success and failure, there can be no rational method for the invention of good, radically new physical theories which are incompatible with existing theories.

Popper [1959, 1963], Kuhn [1962] and Lakatos [1970], all of whom defend versions of standard empiricism, not suprisingly all deny the possibility of there being a rational method of discovery of fundamentally new theories or paradigms—theories whose invention and acceptance constitute a 'scientific revolution'. Kuhn even denies that there can be rational *assessment* of a revolutionary new theory (with respect to its predecessor). The problem of how to proceed when confronted by wholesale scientific crisis, the breakdown of all existing theoretical knowledge, which Popper, Kuhn and Lakatos failed to solve in principle in the 1930s, 1960s and 1970s, Einstein had already solved in successful scientific practice by the year 1905. He solved it by inventing special relativity.

What, then, is Einstein's new rational method of discovery, which led to the discovery of special and general relativity? It can be put, quite simply, like this. Choose two of the most fundamental physical theories, T_1 and T_2 say, which are a part of 'scientific knowledge' but which *contradict* each other. Discard everything about T_1 and T_2 that does not seem relevant to the contradiction until two mutually contradictory principles, P_1 and P_2, are arrived at, P_1 from

T_1 and P_2 from T_2, thus arriving, it is hoped, at the *nub* of the contradiction between T_1 and T_2. Modify P_1 or P_2 (or both) or relevant background assumptions so as to resolve the contradiction into a new unified principle, P_3 (a synthesis of a transformed P_1 and P_2). Take P_3 as the basis for a new theory T_3, which unifies T_1 and T_2.

In order for this method of discovery to be a rational one to adopt, one crucial assumption must be made: the universe has some kind of discoverable unified structure, of which our present fundamental physical theories give us limited, approximate (and incompatible) glimpses. Given the truth of this assumption, we have rational grounds for holding that the method can lead to success. If the assumption is false, we have no such grounds. As we shall see, Einstein seems only to have fully understood this point after the discovery of general relativity.

As far as the discovery of special relativity is concerned, Einstein used the above method in the following way. The two fundamental physical theories that he takes as his starting point (T_1 and T_2) are Newtonian mechanics (NM) and Maxwellian electrodynamics (ME). These two theories are incompatible, fundamentally because, given their most natural interpretation, NM is about forces-at-a-distance between point-particles with mass, whereas ME is about one entity, the continuous electromagnetic field. More specifically, however, there is the following contradiction. NM asserts that forces affect *accelerations*, not *velocities*. Dynamic laws (laws concerning forces and their affects), formulated within the framework of NM, do not pick out any special *velocity* any more than they pick out some special *place* or *time*. ME does, however, pick out a special velocity: the velocity of light, the velocity that, according to ME, vibrations in the field strengths of the electromagnetic field travel through space.

Both points are absolutely fundamental to the two theories. It is fundamental to the whole structure of NM that forces affect accelerations, not velocities (there thus being no role for absolute velocity within the theory). And it is fundamental to ME that influences should spread through the field at some fixed, *finite* velocity: for it is this which creates the need for a field theory in the first place. (Because gravitational influences, in Newton's theory of gravitation, spread at infinite velocity, instantaneous physical states can be specified in terms of point-particles. When influences travel at some *finite* velocity, as in ME, this can no longer be done, as momentum and energy associated with variations in the force travelling at finite velocity through space will not be specified.)

One way in which the clash between NM and ME may be resolved is to interpret ME as a theory which presupposes the existence of the aether, states of the electromagnetic field being states of the aether. In this case, it is reasonable to hold that light has a constant velocity with respect to the aether, and the clash with NM disappears (the constancy of the velocity of light being as unproblematic as the constancy of the velocity of sound with respect to air). In his 1905 paper expounding special relativity, Einstein gave two reasons for rejecting this approach. First, it introduces an implausible asymmetry in the explanation of electromagnetic induction, implausible because of the symmetry in the phenomena to be explained. The theoretical explanation for the current in a conductor moving near a magnet at rest is strikingly different from the explanation of the current if the conductor is at rest and the magnet moves, even though all that matters is the relative motion as far as the effect is concerned. Second, it runs into empirical difficulties in that all attempts to detect the motion of the earth relative to the 'light medium'—the aether—have failed. Einstein concluded that 'the phenomena of electrodynamics as well as of mechanics possess no properties corresponding to the idea of absolute rest' (Einstein [1905]; translated in Einstein *et al.* [1952], p. 37).

Einstein was of course well aware that the null result of the Michelson–Morley experiment does not decisively demolish the aether; he knew of Lorentz's efforts to employ the FitzGerald contraction hypothesis to develop a version of electrodynamics which both presupposes the aether and is compatible with observation. In a paper published in 1907, however, Einstein remarked of the FitzGerald–Lorentz approach (surely with some justice) that it is 'ad hoc' and 'artificial' (Holton [1973], p. 334)—although, as Grünbaum and Zahar remind us, this approach is not as grossly *ad hoc* as some have supposed (Grünbaum [1963], pp. 386–94; Zahar [1973]).

We know that during the decade before 1905, Einstein took the aether hypothesis sufficiently seriously to wonder how motion through the aether might be detected (Paris [1982], pp. 130–2). Nevertheless, it seems that, early on, Einstein was drawn to what may be called the 'Faraday interpretation' of electromagnetism, according to which, instead of seeking to interpret electromagnetism in terms of some more fundamental kind of aetherial matter, one should, on the contrary, seek to understand matter in terms of electromagnetism, which is to be regarded as fundamental (the whole idea of the aether being a mistake). This is implicit in the 'paradox' that Einstein discovered when sixteen, and which he later saw as the germ from which special relativity grew. In his 'Autobiographical Notes', Einstein describes the paradox thus:

> If I pursue a beam of light with the velocity c . . . I should observe such a beam of light as a spatially oscillatory electromagnetic field at rest. However, there seems to be no such thing, whether on the basis of experience or according to Maxwell's equations. From the very beginning it appeared to me intuitively clear that, judged from the standpoint of such an observer, everything would have to happen according to the same laws as for an observer who, relative to the earth, was at rest. For how, otherwise, should the first observer know, i.e. be able to determine, that he is in a state of fast uniform motion? (Einstein [1949], p. 53.)

This only makes intuitive sense as a paradox insofar as electromagnetism is being conceived of in the absence of the aether.

As I indicated above (p. 18), and as I have argued elsewhere (Maxwell [1979], pp. 647–8 and [1988], p. 42), there are strong aim-oriented empiricist, quasi-*a priori* grounds for favouring the Faraday interpretation of electrodynamics over the aether interpretation, the view that the aether is required to make electrodynamics intelligible being a sort of metaphysical blunder. And there is an additional consideration. According to aim-oriented empiricism, the acceptability of the aether hypothesis is to be judged in terms of its heuristic and methodological power. But as ME was developed, up to 1905, especially in the hands of Lorentz, the role of the aether seemed to become increasingly tenuous. This, according to aim-oriented empiricism, counts against the aether approach. We may thus detect, in Einstein's adoption of the Faraday interpretation of electrodynamics, and his rejection of the aether interpretation, an instinctive allegiance to aim-oriented empiricism.

There is, however, another approach to resolving the clash between NM and ME. It is possible that the velocity of light is constant with respect to the source. Einstein tried this approach; he tells us that he abandoned it because of the complications to which it led (Shankland [1963]). Evidence against this hypothesis only began to come in later, in 1913, with observations of double stars.

Granted, then, that the above two approaches to resolving the clash between NM and ME are to be rejected, we are left with the following situation: ME appears to be committed to the existence of a fundamental, *absolute* velocity—the velocity of light—just that which NM rules out. We have here, then, two good candidates for P_1 and P_2, extracted from T_1 (NM) and T_2 (ME) in order to highlight the clash between the two theories, namely:

P_1: The laws of nature have the same form with respect to all inertial (non-accelerating) reference frames.
P_2: It is a law of nature that light travels with constant velocity c (in a vacuum).

P_1 and P_2 together form, it would seem, a horrible contradiction. In order for P_1 and P_2 to be compatible it would be necessary for a beam of light to have the same velocity c with respect to *all* inertial reference frames, even though these are moving with all possible velocities with respect to each other.

Astonishingly, Einstein discovered how to make this apparently blatant absurdity entirely consistent. What we need to do is to modify our ideas about time and space, so that light *does* have the same velocity c in all reference frames. The basic postulates of special relativity are just P_1 and P_2: the many consequences of the theory arise from demanding that P_1 and P_2 be consistent.

More precisely, Einstein took P_1 as one of his basic postulates, but modified P_2 to become:

$P_2^{\ddagger}$: It is a law of nature that the velocity of light is a constant c in some 'resting' reference frame, and is independent of the velocity of the source.

P_2 is then derived from P_1 and $P_2^{\ddagger}$. It is entirely understandable that Einstein took $P_2^{\ddagger}$ as his axiom rather than P_2 interpreted to mean: It is law of nature that light has constant velocity c in all inertial reference frames. To adopt this latter postulate is to assume as comprehensible that which only becomes comprehensible with the development of the theory. $P_2^{\ddagger}$ is not initially incomprehensible in this way; on the contrary, $P_2^{\ddagger}$ is a basic tenet of the Lorentzian approach, of the aether approach widely held at the time.

How, then, is the contradiction between P_1 and P_2 to be resolved? Ordinarily we assume that the rate of clocks, and the length of rods, are unaffected by uniform motion, temporal and spatial distances being frame-independent and absolute. Suppose we have two reference frames, R_1 and R_2, with parallel axes, and with origins that coincide at $t_1 = t_2 = 0$, the origin of R_2 travelling along the x axis of R_1 with velocity v in the +ve direction, the coordinates of an event P being (x_1, y_1, z_1, t_1) and (x_2, y_2, z_2, t_2) in R_1 and R_2 respectively. In effect, we ordinarily assume that the coordinates are related by the 'Galilean' transformations:

$$x_2 = x_1 - vt_1; \qquad y_2 = y_1; \qquad z_2 = z_1; \qquad t_2 = t_1.$$

We assume, that is, that length and time are unaffected by motion, and that if a pulse of light which has velocity c along the x axis in the +ve direction in R_1, then its velocity in R_2 is $c - v$.

What Einstein realized was that if rates of clocks and lengths of rods *are* affected by relative motion, so that $x_2 = x_1 - vt_1$ and $t_2 = t_1$ are both *false*, then it is entirely possible that any given pulse of light has the same velocity c in *both* R_1 *and* R_2—indeed, the same velocity c in *all* reference frames.

It turns out that the thesis that light does have the same velocity in all reference frames—which is implied by P_1 plus P_2^*—sufices to fix uniquely just how the coordinates of R_1 and R_2 are related. All that we need, in addition, is that the relationship is *symmetric* (which may be said to be inherent in P_1 in any case), *linear* and *isotropic*. With these assumptions it is not hard to show that the coordinates of R_1 and R_2 are related by the following equations, the 'Lorentz' transformations:

$$x_2 = \frac{x_1 - vt_1}{\sqrt{1 - v^2/c^2}}; \qquad y_2 = y_1; \qquad z_2 = z_1; \qquad t_2 = \frac{t_1 - vx_1/c^2}{\sqrt{1 - v^2/c^2}}$$

According to these equations, all but uniquely determined by P_1 plus P_2^*, relative motion contracts rods and makes clocks go slow, but in such a way that the velocity of light is c in *all* inertial frames. The miracle of reconciling P_1 and P_2 has been achieved.

Special relativity has a number of startling implications. One is that *mass*, along with the speed of clocks and length of objects, is affected by uniform motion, so that $m_2 = m_1/\sqrt{1 - v^2/c^2}$, where m_1 is mass of object in rest frame R_1, with respect to which the object is at rest, and m_2 is mass of object in R_2. Another—the most famous of all—is that *mass* is a form of *energy*, in accordance with $E = mc^2$.

From the standpoint of aim-oriented empiricism, special relativity is doubly significant. First, the way in which Einstein *discovered* special relativity exemplifies the method of discovery of aim-oriented empiricism, to the extent that Einstein used the method I have indicated above, namely: create a new theory as the outcome of resolving a clash between two existing theories—thus creating greater conceptual and theoretical unification. Secondly, and quite strikingly special relativity itself exemplifies aim-oriented empiricism, and in an important sense cannot be adequately understood within the framework of standard empricism. For, as Einstein himself remarks, two pages on from the quotation given above from his 'Autobiographical Notes':

> The universal principle of the special theory of relativity is contained in the postulate: The laws of physics are invariant with respect to the Lorentz-tranformations . . . This is a restricting principle for natural laws, comparable to the restricting principle of the non-existence of the *perpetuum mobile* which underlies thermodynamics. (Einstein [1949], p. 57.)

Special relativity is thus a law of laws, a meta law, a guiding principle, a heuristic and methodological rule to be employed in discovering and assessing physical theories—above all, for Einstein of 1905, to be employed as a heuristic tool for the discovery of the new theory to unify classical mechanics and electrodynamics. (When viewed from this perspective, what Einstein did in creating special relativity was to take a basic restricting principle of Newtonian mechanics, namely Galilean invariance—the pre-relativistic way to interpret P_1—and modify this to make it compatible with P_2, thus forming a new restrictive principle, P_3, *i.e.* Lorentz invariance.) As a heuristic and methodological principle, special relativity has amply fulfilled Einstein's hopes for it. It played a vital role in the discovery of de Broglie's wave theory of matter, the so-called Klein–Gordon equation (first discovered by Schrödinger), the Dirac equation of the electron, quantum electrodynamics, quantum electroweak theory, and quantum chromodynamics. In a modified form, it played a crucial role in the discovery of general relativity; and it continues to be relevant to superstring theory. Here, then, is a heuristic and methodological principle of enormous fruitfulness for all of theoretical physics, which can be formulated as the demand that acceptable theories must be Lorentz invariant. This demand—equivalent to the demand that space–time be Minkowskian (in the formulation of theories)—is not *merely* a methodological principle for, as we have seen, it has substantial physics in it. Special relativity is capable of being falsified and, from the standpoint of general relativity, it is false. All this is very hard to make sense of, or do justice to, within the confines of any version of standard empiricism, precisely because standard empiricism *rejects* the idea that methodological principles have physics, or metaphysics, built into them—there being, within standard empiricism, no (level 2) *metamethodological* framework within which rival (level 1) *methodological* principles can be rationally assessed. From the standpoint of aim-oriented empiricism, there is no difference in principle between an *ordinary* methodological rule such as *position invariance* (acceptable laws and theories must be invariant with respect to change of position in space), and full *Lorentz invariance*. To both there correspond substantial *physical* or *metaphysical* principles, namely: 'space selects out no special position' or 'space–time selects out no special inertial reference frame'. Both may be false, and both therefore require critical scrutiny as science develops, in accordance with aim-oriented empiricism, and not dogmatic acceptance or rejection, as required by standard empiricism, with its *fixed* set of methodological principles (which no one yet has been able to formulate!). Standard empiricism differentiates sharply between the status of *position* and *Lorentz* invariance—only the former qualifying as a methodological rule of physics, the latter belonging exclusively to the *content* of physics, as a physical theory. But this does violence to Einstein's achievement; it does violence to the new way of doing physics inspired by Einstein, which precisely *exploits* the fruitful interplay between new theories and new heuristic and methodological principles (along the lines stipulated by aim-oriented empiricism).

Aim-oriented empiricism is even more explicit in Einstein's discovery of general relativity. Einstein exploits the same method of discovery. As before, there are two fundamental conflicting theories, namely: Newton's theory of gravitation (T_1) and special relativity (T_2). These conflict because whereas Newton's theory implies that gravitational influences travel instantaneously, special relativity implies that such influences cannot travel faster than light. As before, Einstein searches for new *principles* which will guide him to a new unifying theory. His first step is to notice that there is a principle implicit in Newton's theory of gravitation (P_1) which, if generalized (P_1*), makes it possible to generalize and *improve* the principle of relativity basic to special relativity (P_2). This latter principle seemed unsatisfactory to Einstein because of its restriction to some arbitrarily selected set of inertial reference frames all in uniform motion with respect to each other. Much more satisfactory would be a *general* principle of relativity (P_2*) which asserts that the laws of nature have the same form in *all* reference frames, however they may be moving or accelerating with respect to each other. But this general principle of relativity seems impossible to implement. It is one thing to say, given a train moving uniformly through a station, that there are two equivalent descriptions: (1) train moving, platform at rest; and (2) train at rest, platform moving (in opposite direction). It is quite another to say, given that the train crashes into the buffers at the end of the station, that there are two equivalent descriptions:

(1) train de-accelerates, platform remains unaccelerated; and (2) train remains unaccelerated, platform de-accelerates. These are not equivalent descriptions: in the first, it is people in the train that suffer from violent de-acceleration, whereas in the second it is people on the platform that suffer. But consider now the following remarkable feature of Newton's law of gravitation (P_1): in a uniform gravitational field all objects accelerate equally, whatever their mass (essentially because inertial and gravitational mass are equal). Generalize this to form the *principle of equivalence* (P_1*): no local phenomenon distinguishes between (a) uniform acceleration, and (b) being at rest in a uniform gravitational field. Whatever effect a gravitational field has on some phenomenon, it is the same as the effect that the equivalent acceleration would have in the absence of gravitation. This immediately has two consequences. First, it allows us to hold that *all* frames, however accelerating, are equivalent, as long as, in moving from one frame to another accelerating with respect to the first, we can invoke an additional, compensating gravitational field. Thus, in the case of the crashing train we have: (1) train de-accelerates, platform remains stationary; (2) train remains stationary, platform de-accelerates, and a gravitational field exists momentarily to compensate precisely for this de-acceleration. In *both* cases, it is the people in the train who suffer, according to the first description, because of de-acceleration, according to the second, because of the sudden gravitational field (and no compensating de-acceleration, as on the platform). The generalized principle of equivalence (P_1*) makes it possible, in this way, to hold the generalized principle of relativity (P_2*). The second consequence of the generalized principle of equivalence (P_1*) is that, if correct, it enables us to discover the effects that uniform gravitational fields have on phenomena; all we need to do is to consider the effects of uniform *accleration* and put these equal to the effects of the corresponding gravitational field in the absence of acceleration. The principle of equivalence (P_1*) thus has great potential heuristic power for the discovery of the new theory of gravitation, to replace Newtonian theory.

According to special relativity, acceleration affects geometry. Consider a flat, rapidly rotating disk. A rigid rod, of length L at the centre of the disk will, according to special relativity, only have length $L\sqrt{1-v^2/c^2}$ at the circumference, given that it is aligned with the motion of rotation which, at the circumference, has the value v. The geometry of the disk, as determined by the rod, will be non-Euclidean.[4] Uniform circular motion is accelerated motion. But if acceleration affects geometry, so, too, by the principle of equivalence, must gravitation. We have the possibility that gravitation *is* the (non-Euclidean) curvature of space–time—a possibility which, if true, would bring about a tremendous conceptual unification in the foundations of physics

[4] For a more detailed discussion of the role played by the rotating disk in the genesis of general relativity, see Stachel [1980].

(namely the unification of gravitation and space–time geometry). Postulate therefore that gravitation is indeed the curvature of space–time. The presence of matter curves space–time; and matter moves along geodesics in this curved space–time. Curved space–time can always be reduced to flat Minkowskian space–time in any infinitesimal region by an appropriate choice of coordinate system, in accordance with the principle of equivalence given its final local formulation (P_1**). What remains to be done is to formulate the precise way in which energy–momentum affects the Riemannian curvature of space–time.[5] The field equations of general relativity are the simplest possible solution to this problem. Indeed, granted that the equations involve derivatives no higher than the second, the field equations are detemined uniquely to be:

$$R_{ab} - 1/2 g_{ab} R = 8\pi G T_{ab}$$

Here R_{ab} is the Ricci tensor of the metric g_{ab} (the Ricci tensor being derivable from the Riemannian curvature tensor by contraction), R is the Ricci scalar (formed from R_{ab} by contraction), T_{ab} is the energy–momentum tensor, and G is Newton's constant of gravitation.[6] We have arrived at T_3, which reduces to special relativity (T_2) in the absence of gravitation, and which approaches Newtonian theory (T_1) in the limit as gravitational fields become weak and velocities become low in comparison with the velocity of light.

Does Einstein really put aim-oriented empiricism into practice in developing the special and general theories of relativity, in the way I have just sketched? Is there, here, a genuine method of discovery, given that Einstein failed for over thirty years to develop a satisfactory unified field theory? A few comments are in order.

The full aim-oriented empiricist method of discovery involves the tackling of at least four kinds of problems: (1) conflicts between experimental results and theory; (2) conflicts between well-established fundamental theories; (3) conflicts between such theories and the best available blueprint for physics; and (4) conflicts inherent in the best blueprint itself (or between rival blueprints). It could be argued that Einstein only exploits a small part of this method of discovery, in that he is primarily concerned with type (2) problems (and type (1) problems where relevant). But this is, I think, wrong for a number of reasons.

First, the metaphysical thesis that the basic laws of nature have a *unified* structure is an implicit or explicit assumption in all of Einstein's deliberations.

Second, in developing special and general relativity it is precisely the pre-existing metaphysical blueprints of classical physics which Einstein is led to transform—basic assumptions about the nature of space, time, energy, mass,

[5] For a discussion of this part of Einstein's creation of general relativity, see Pais ([1982], Chs. 11 and 12), or, even better, Norton [1984].

[6] Good expositions of general relativity are to be found in Friedman [1983], Schutz [1988] and Misner *et al.* [1973].

force. In developing new *principles*—such as the principle of Lorentz invariance or the principle of equivalence—Einstein is, at one and the same time, modifying pre-existing blueprint ideas (Newtonian space–time being transformed into Minkowskian space–time which is in turn transformed into the Riemannian space–time of general relativity).

Third, lurking behind the type (2) problems which concern Einstein (involving clashes between *theories*) there are type (4) *blueprint* problems. Consider the type (2) problem that led to special relativity—the clash between Newtonian mechanics and Maxwellian electrodynamics or, more specifically, the clash between Galilean invariance and the thesis that the constancy of the velocity of light is a law of nature. Around 1900, as we have seen, there was an obvious solution to this problem: interpret electrodynamics in terms of the aether, regard the constancy of the velocity of light as being relative to the aether, and expect Galilean invariance to break down for high velocities with respect to the aether. This amounts, of course, to adopting a blueprint for physics—the aether blueprint. In formulating the problem in the way in which he did, Einstein is in effect *rejecting* this aether blueprint; he is adopting Faraday's view that the field is fundamental, and does not require an underlying aether to make it comprehensible. As I have argued above and elsewhere (Maxwell [1979], pp. 647–8, [1988], p. 42), there are good reasons for preferring what may be termed the Faraday blueprint to the aether blueprint. The important point, however, is that in formulating his type (2) problem in the way in which he did (crucial for the development of special relativity), Einstein is in effect interpreting Newton's and Maxwell's theories to be two equally fundamental, rival theories, each with its rival *blueprint*, namely, the Newtonian (or Boscovichean) blueprint of point-particles surrounded by spherically symmetrical, rigid fields, and the Faraday field blueprint with variations in the field being transmitted at some *finite* velocity. There is, in short, a type (4) blueprint problem inherent in the type (2) problem which led Einstein to special relativity. This type (4) problem may be formulated, not as a problem about how to reconcile, or choose between, two rival blueprints, but rather as the problem of how to resolve the clash that results from attempting to unify the two blueprints in such a way as to accommodate charged point-particles *and* a field.

Fourth, there are grounds for holding that Einstein's fundamental problem soon after 1900 was the type (4) *blueprint* problem I have just indicated—the problem of understanding how charged point-particles can interact with the field, or the problem of unifying point-particle and field. It is a striking fact that Einstein's three great papers of 1905 can all be interpreted as exploring aspects of this fundamental problem. We have just seen that this is true of the paper introducing special relativity. It is also true of the Brownian motion paper, concerned to establish the existence of atoms—the existence of the particle-like aspect of reality. And it is true above all of the paper which put forward the idea that light has a particle-like aspect in accordance with $E = nh\nu$ (where E is the energy and ν the frequency of the light, h is Planck's constant and n is some integer, the number of light quanta present), this 'heuristic' hypothesis of light quanta then being used to explain the photoelectric effect. Here the classical particle/field problem is intensified to an extraordinary extent in that the field itself is revealed to have a particle-like aspect.[7]

As it happens, Einstein himself makes clear in his 'Autobiographical Notes' (Einstein [1949]) that he held the classical particle/field problem to be of fundamental importance. Having explained that theories are to be critically assessed from the two distinct standpoints of empirical success and 'inner perfection' (unity or comprehensibility)—which in itself commits Einstein to aim-oriented empiricism (see below)—he goes on to assess critically Newtonian mechanics and Maxwellian electrodynamics from the standpoint of inner perfection. We have here, incidentally, an adjunct to, and refinement of, Einstein's method of discovery: *one* theory is here taken at a time, and is assessed from the standpoint of 'inner perfection'—from the standpoint, that is, of the capacity of the theory to provide a 'perfect' blueprint for all of physics as far as the *form* of the theory is concerned. (In indicating the 'inner perfection' *defects* of a theory one in effect indicates, at least in general terms, what would constitute a 'perfect' theory; one indicates, that is, a blueprint.) Einstein discusses five 'inner perfection' defects in Newtonian mechanics, namely: (1) arbitrariness in the determination of inertial reference frames from an infinity of alternatives, and inadequacy of introducing absolute space (with respect to which all bodies have absolute acceleration as a solution to this problem); (2) two distinct basic laws (and not *one*), namely: (a) the law of motion ($F = ma$), and (b) the expression for force or potential energy ($F = Gm_1m_2/d^2$); (3) arbitrariness of (b) given (a), there being endlessly many equally good possibilities for (b) given (a); (4) the possibility of the force law being determined by the structure of space (the form of the force law being suggestively simple when viewed in geometrical terms), and yet the *failure* to exploit this possibility; (5) the *ad hoc* character of the equality of inertial and gravitational mass; and (6) unnaturalness of energy being split into two forms, kinetic and potential (see Einstein [1949], pp. 27–31). As far as electrodynamics is concerned, Einstein discerns one basic defect associated with interpreting the field equations as applying to matter and, in the case of the vacuum, to the aether. Einstein argues (perhaps not altogether accurately) that this defect was overcome by Lorentz in reinterpreting the field equations to hold essentially only for the vacuum, with matter, in the form of charged particles, being the source of the field. Einstein then remarks: 'If one views this phase of the development of theory critically, one is struck by the dualism which lies in the fact that the material point in Newton's sense and the field as continuum

[7] Lucid summaries of these papers are to be found in Lanczos [1974].

are used as elementary concepts side by side' (Einstein [1949], p. 37). Einstein explains why attempts to overcome this basic defect by eliminating the point-particle do not succeed; and he concludes: 'Accordingly, the revolution begun by the introduction of the field was by no means finished. Then it happened that, around the turn of the century . . . a *second fundamental crisis* set in' (my italics)—namely the crisis engendered by the first step towards quantum theory, Planck's quantum explanation of his empirical radiation law. If this is the *second* fundamental crisis, then the *first* is particle/field dualism of classical physics. As it happens, the two crises are intimately interrelated, since Planck's law and quantum theory deal with the *interaction* of field and matter.

There are good grounds, then, for holding that Einstein was concerned with problems from type (2) to type (4), as defined above, and type (1) problems where relevant, a type (4) problem of special concern to Einstein being the problem of how to unify point-particles and field.

But did Einstein really invent an authentic method of discovery in view of his failure, during the last thirty years of his life, to discover the unified field theory he so ardently sought?

One reply can be made immediately: the method of discovery, indicated above, though rational, is also non-mechanical and fallible. The failure of the method to lead to a good fundamental new theory over a period of thirty years—even in the hands of Einstein—does not prove that the method is inauthentic.

But there is a much more important reply to be made. Einstein did not use his method of discovery in seeking to formulate his unified field theory. Or rather Einstein *misapplied* this method, in a quite elementary way.

After around 1930, the two fundamental theories that stand in most glaring contradiction with each other are general relativity and quantum theory. In order to implement Einstein's rational method of discovery from about 1930, the first step to take is to extract basic principles, P_1 and P_2, from general relativity and quantum theory respectively, which *contradict* each other—this even perhaps being the *nub* of the contradiction between the two theories. The task then is to modify P_1 and P_2 (or something else) to form P_3, a new principle which guides us to a new unified theory T_3, unifying general relativity and quantum theory.[8]

Einstein did none of this. Instead he took as his two theories general relativity and classical electrodynamics and sought to unify these two theories, to form a theory which applied to all phenomena, including quantum phenomena. One may well have doubts as to whether these two theories really

[8] Elsewhere, I have suggested that the way to implement this method of discovery is to take, as P_1, the general idea of deterministic dynamic space-time geometry (from general relativity), and to take, as P_2, the general idea of ontological probabilism (from quantum theory), the task then being to create unified probabilistic dynamic geometry, P_3: (see Maxwell [1985], pp. 40–1).

do fundamentally contradict each other—even though the theories are clearly *two* distinct theories and not *one* unified theory. They are at least both field theories; they both incorporate Lorentz invariance, at least locally; and they are both classical and deterministic. What is dramatically apparent is that the fundamental contradiction of theoretical physics after 1930 concerns, not the clash between classical general relativity and classical electrodynamics, but rather the clash between general relativity and quantum theory. (One can add that it is perverse to continue to take the unification of gravitation and electromagnetism as *the* unification to strive for, sufficient to create the comprehensive unified field theory, after the discovery of the strong and weak forces in addition to the forces of gravitation and electromagnetism.)

Why did Einstein so crudely and wilfully *misapply* his rational method of discovery? The answer is straightforward: because of his abhorrence of quantum theory given its orthodox interpretation (OQT). Einstein was absolutely correct to find OQT fundamentally defective from the crucial standpoint of 'inner perfection'. As I shall argue in a moment, Einstein's attitude towards OQT exemplifies yet again his (sound) commitment in scientific practice to aim-oriented empiricism and scientific realism. Where Einstein went *wrong* was to conclude that quantum theory was therefore entirely devoid of heuristic value—that it 'offers no useful point of departure for future development' (Einstein [1949], p. 87).

What is striking about this is that it is actually a vital feature of Einstein's method of discovery that one deals with theories that are intrinsically *defective*. The defects are clues as to how the theory may be fruitfully modified. As we have seen above, Einstein indicates a number of fundamental defects inherent in Newtonian mechanics and Maxwellian electrodynamics. Einstein even knew, by 1901, as a result of Planck's work, that both theories are fundamentally incorrect. This did not stop him taking these theories as 'points of departure'. Indeed, it is the *defects* in the theories, as perceived by Einstein, which make his method of discovery so successful: for it is these defects which indicate how the theories are to be modified to overcome the contradictions between them. For Einstein to argue, after 1930, that the *defective* character of quantum theory ensures that the theory cannot form a proper point of departure does violence to the very heart of Einstein's own earlier method of discovery, used in the discovery of special and general relativity with such striking success.

Why did Einstein fail to recognize the fairly obvious point just made? In essence, because his abhorrence of OQT was so intense, so profound, that it was emotionally impossible for him to work seriously with the theory. He did not want to contribute to what he interpreted as a sickness which had entered physics, and which he regarded as symptomatic of the basic sickness of our times. In a sense, Einstein turned his back on quantum theory, and devoted himself to the task of unifying general relativity and classical electromagnetism as a kind of moral protest against the tenor of our times.

In order to substantiate this point I must now break off my discussion of Einstein's successes and failures in implementing aim-oriented empiricism so that I can consider in a little more detail the question of Einstein's attitude to OQT.

His mature attitude can be summarized like this. From the standpoint of empirical criteria, OQT must be judged to be an immense success. From the equally important standpoint of criteria having to do with 'inner perfection', with unification, OQT must be judged to be a disaster. This is because the theory cannot be interpreted to be about some hypothetical *reality*. It was not so much the lack of *determinism* that came to worry Einstein as the lack of *realism*. In his 'Autobiographical Notes' he puts it like this.

Physics is an attempt conceptually to grasp reality as it is thought independently of its being observed. In this sense one speaks of 'physical reality'. In pre-quantum physics there was no doubt as to how this was to be understood. In Newton's theory reality was determined by a material point in space and time; in Maxwell's theory, by the field in space and time. (Einstein [1949], pp. 82–3.)

Einstein goes on to point out that as far as OQT is concerned, there is no quantum equivalent to the classical material point or field. OQT makes probabilistic predictions about the results of performing measurements on an ensemble of similarly prepared systems, but cannot be interpreted as specifying the physical state of the individual system as it evolves in space and time independent of measurement. As Einstein puts it in volume 2 of the same book, in his 'Reply to Criticisms':

What does not satisfy me . . . [about OQT], from the standpoint of principle, is its attitude towards that which appears to me to be the programmatic aim of all physics: the complete description of any (individual) real situation (as it supposedly exists irrespective of any act of observation or substantiation). (Einstein [1949], p. 667.)

In a letter to Schrödinger in 1950, Einstein expresses himself even more emphatically.

You are the only contemporary physicist, besides Laue, who sees that one cannot get around the assumption of reality—if only one is honest. Most of them simply do not see what sort of risky game they are playing with reality—reality as something independent of what is experimentally established. They somehow believe that the quantum theory provides a description of reality, and even a *complete* description; this interpretation is, however, refuted, most elegantly by your system of radioactive atom + Geiger counter + amplifier + charge of gun powder + cat in a box, in which the Ψ-function of the system contains the cat both alive and blown to bits. Is the state of the cat to be created only when a physicist investigates the situation at some definite time? Nobody really doubts that the presence or absence of the cat is something independent of the act of observation. But then the description by means of the Ψ-function is certainly incomplete, and there must be a more complete description. If one wants to consider the quantum theory as final (in principle), then one must believe that a more complete description would be useless because there would be no laws for it. If that were so then physics could only claim the interest of shopkeepers and engineers; the whole thing would be a wretched bungle. (See Przibram [1986], p. 39.)

Einstein's opposition to OQT—arising from the lack of realism of the theory—was implacable, even vehement. It was this, after all, which had led to the great rupture between mainstream theoretical physics and Einstein's own work. From 1905 to 1926 Einstein was at the centre of developments in theoretical physics. But from 1926 onwards the ways parted, essentially because Einstein was not able to bring himself to contribute to the development of OQT (confining himself to critical analysis of it). Robert Shankland, who met Einstein a number of times during the years 1950–4, has remarked on the uncharacteristic vehemence of Einstein's opposition to OQT.

His well-known scepticism on this subject [of quantum mechanics] was clearly evident and his comments on both the subject itself and its leading proponents were often highly critical and even emotional, in contrast to his restrained and quiet explanations of relativity. (French [1979], p. 39.)

Something of the strength of Einstein's opposition to OQT also emerges from a correspondence which he had with Born on the subject. Einstein makes it quite clear that he finds OQT unacceptable because of its lack of realism. Born persists in a stance of somewhat patronizing incomprehension, Einstein rather sharply writes that he does not wish to continue the discussion, and Pauli is obliged to step in and tick Born off for misunderstanding Einstein, even though he agrees with Born that Einstein's position amounts to asking 'how many angels are able to sit on the point of a needle' (see Born [1971], pp. 199–229).

The strongest statement of Einstein against OQT that I have come across is quoted by Fine ([1986], p. 1): 'This theory [the present quantum theory] reminds me a little of the system of delusions of an exceedingly intelligent paranoiac, concocted of incoherent elements of thoughts.'

These quotations establish beyond all possible doubt that Einstein was committed to full-blooded scientific realism, at least as far as the basic aim of physics is concerned.

Einstein is absolutely correct to hold that OQT cannot be interpreted realistically. As he points out in his letter to Schrödinger, if one attempts to interpret the Ψ-function of OQT as providing a complete description of reality, one is led to the (apparently) absurd conclusion that Schrödinger's cat persists as a superposition of being alive and being dead until we open the box and look. And similarly, we would have to conclude that the outcome of any quantum measurement is not some definite state of the apparatus but rather a superposition of macroscopically distinct states—the superposition only collapsing miraculously when we look. The simplest way to demonstrate the impossibility of interpreting OQT realistically, however, arises from the following consideration. If we interpret the Ψ-function as describing quantum reality directly, and exclude *measurement* from the basic postulates of the theory, we are left with a theory that is fully deterministic, since quantum states, corresponding to Ψ-functions, evolve deterministically in accordance with Schrödinger's equation. Such a version of OQT fails to make contact with the most basic feature of the quantum world—its probabilistic character. In short, just as Einstein declares, OQT must be regarded as a theory which makes probabilistic predictions about the results of performing *measurements* on systems, but which does not specify the actual physical state of the individual system in the absence of measurement.

How did this extraordinary state of affairs arise? Essentially because, as quantum theory (QT) developed with the work of Bohr, Heisenberg, Schrödinger, Born and others, no solution was found to the quantum wave/particle problem. As we have seen, this problem was first discovered by Einstein with his invention of light quanta—or 'photons' as they subsequently came to be called. The problem was further intensified in 1923 when de Broglie proposed that *electrons*, up till then believed to be particles, have a wave-like aspect associated with them, as was subsequently confirmed experimentally by Davisson and Germer. In order to develop QT as a realistic theory, it would have been necessary to solve the quantum wave/particle problem in such a way that it is possible to specify, consistently and precisely, what sort of physical entitites photons and electrons are as they evolve in space and time independently of measurement. This did not happen. Instead, Heisenberg invented matrix mechanics in 1925 intending, from the outset, that the theory should predict the outcome of measurements but should remain silent about what exists physically in the absence of measurement. Schrödinger invented wave mechanics in 1926 with the hope that the wave aspect of quantum entities would turn out to be fundamental. This hope was dashed when it became clear that the Ψ-function could not be regarded as describing quantum reality directly, but had to be interpreted as containing probabilistic information about the results of performing *measurements* on an ensemble of similarly prepared systems—as Born was the first to point out.

We can begin to see some of the reasons for Einstein's vehement rejection of OQT as a satisfactory theory (despite its immense empirical success). It was Einstein after all who, in a sense, invented quantum theory. Planck introduced the idea that the energy E of an oscillator of frequency ν is quantized in accordance with $E=nh\nu$ as a calculational device, not as a new hypothesis incompatible with classical physics. Planck's aim was to deduce his empirical law of blackbody radiation from the basic postulates of classical physics. He was dismayed to discover that the quantization of energy *contradicted* classical physics, and he spent the next fifteen years or so trying to remove this defect from his derivation. It was Einstein, and Einstein alone, who appreciated that

Planck's work spelt the downfall of classical physics, a new beginning being required. In this sense, Einstein initiated quantum theory with his paradoxical 'heuristic' hypothesis that light consists of discrete quanta with energy $E = h\nu$, even though light also undeniably has a continuous wave-like character. For Einstein around 1905, the fundamental task of the new theory, needed to replace classical physics, would be to solve the riddle of the nature of quantum reality in view of its ostensibly contradictory particle and field aspects. No wonder Einstein was dismayed when the new theory was developed deliberately to *evade* and not to *solve* this basic quantum riddle.

But there is more than this to Einstein's opposition to OQT. As I have stressed above, the failure to solve the quantum wave/particle problem ensures that OQT cannot be interpreted realistically, which in turn ensures that OQT must be interpreted as making (probabilistic) predictions about the results of performing measurements. But this in turn has a variety of disastrous—though rarely noticed—consequences. For it means that OQT only issues in actual physical predictions if some part of *classical physics* (CP) is adjoined to OQT for a treatment of measurement. OQT alone can only issue in *conditional* predictions of the type: *if* a measurement of observable A is made, the outcome will be one or other of the values ($a_1 \ldots a_n$) with probabilities ($p_1 \ldots p_n$), with $\sum_{r=1}^{n} p_r = 1$. And even this goes too far: strictly speaking, according to OQT, a quantum mechanical state Ψ can only be attributed to a system in so far as the system has been subjected to some preparation procedure—which must be specified by means of CP. Thus OQT, devoid of CP, has no physical content whatsoever. It is only OQT + CP which has physical content. But OQT + CP, considered as a fundamental theory of physics, is a disaster. It is (i) grossly *ad hoc* or aberrant, in that it consists of two conceptually incoherent parts, OQT and CP. It is (ii) *imprecise*, because the circumstances in which CP is to be applied are only specified in terms of measurement, and the notion of measurement cannot be made precise (Maxwell [1972b]). It is (iii) *ambiguous* because the theory does not decide unambiguously between probabilism and determinism. It is (iv) *non-explanatory*, not only because of the *ad hoc* character of the theory, but also because the theory is obliged to presuppose some part of what it is intended to explain, namely CP. The theory is (v) *severely restricted in scope* in that it cannot be applied to conditions which exclude the possibility of measurement, such as early states of the universe. It (vi) *excludes the possibility of quantum gravity and quantum cosmology*, since these would require measuring instruments, described in terms of CP, to exist outside space–time and beyond the cosmos, and clearly this is not possible. (These are points I have developed over a number of years: see Maxwell [1972b, 1973a, 1973b, 1975, 1976a, 1982], and especially [1988], pp. 1–8.)

These six gross defects—especially (i) to (iv)—ensure that OQT + CP is unacceptable as a fundamental physical theory. OQT + CP cannot justifiably be held to be part of theoretical scientific *knowledge*. (OQT + CP encompasses a great deal of *empirical* knowledge, but cannot be said to be an acceptable theory, constituting *theoretical* knowledge.) OQT + CP is as unacceptable as the absurd, empirically successful but grossly *aberrant* theories considered in part one of this essay. In practice this point is beyond dispute. The vast majority of physicists, from soon after 1926 down to the present day, have regarded OQT as an entirely acceptable part of scientific knowledge: they have been able to do this because they have been able to pretend that OQT + CP is really just OQT. In almost all the textbooks and physical journals quantum theory is treated *as if* its postulates are purely quantum mechanical ones. As a result, OQT appears to be thoroughly *non-ad hoc*, precise and explanatory, as conceptually coherent and unified as any classical theory. But all this is an illusion. It is the outcome of pretending that the *physical* theory—the theory that has *physical content*—is OQT rather than OQT + CP. No such thing is possible. OQT, devoid of CP, has no physical content whatsoever. Only an all-pervasive intellectual dishonesty makes it possible to pretend that OQT alone has physical content (or that OQT + CP is really, somehow, just OQT).

All this demonstrates just how sound Einstein's instincts were when he judged OQT to be an unacceptable theory. How unerringly correct Einstein was to declare that Bohr and company 'do not see what sort of risky game they are playing with reality'; and how sound his comparison is between OQT and 'the system of delusions of an exceedingly intelligent paranoiac, concocted of incoherent elements of thoughts'—namely QT *and* CP!

It is important to appreciate that the above six defects of OQT, even though consequences of the impossibility of interpreting OQT realistically, are not defects which only realists will recognize. Any physicist, whether realist or instrumentalist, aim-oriented empiricist or standard empiricist ought in practice, to regard the above defects sufficient grounds for finding OQT unacceptable. We have here, in effect, an additional general argument against instrumentalism and for realism. Any fundamental physical theory, and not just OQT, which is interpreted instrumentalistically as predicting only the (observable) outcomes of measurements will be, in the same way, unacceptably (i) *ad hoc*, (ii) imprecise, and (iv) non-explanatory. In other words, theoretical unity implies realism; anti-realism, built into a physical theory (as it is built into OQT) must inevitably, at some point, lead to unacceptable *ad hocness* or aberrance (see Maxwell [1993]).

Even though it is not essential to be an aim-oriented empiricist in order to find OQT unacceptable, it helps. For aim-oriented empiricism provides a clear and cogent *raison d'être* for finding OQT unacceptable even though the theory has met with such outstanding empirical success. Standard empiricism, on the other hand, can provide no such *raison d'être*. If scientific theories ought in the end to be judged solely on the basis of empirical success and failure, then there can be no rational grounds for rejecting OQT, given its immense, its unprecedented empirical success.

It is in just this way that most of Einstein's contemporaries tended to view his rejection of OQT: as the outcome of unscientific, metaphysical prejudice, or even as an indication of 'senility' (as Einstein himself put it). Even Abraham Pais, so knowledgeable about, and so sympathetic towards Einstein, nevertheless regards Einstein's objections to OQT as 'unfounded' (Pais [1982], p. 464).

Einstein's attitude towards OQT, so strikingly at odds with most of his contemporaries, provides further evidence in support of my contention that aim-oriented empiricism is implicit in Einstein's scientific work. If Einstein had assessed OQT in purely standard empiricist terms, he could have had no rational grounds for rejecting OQT—no grounds even for rejecting OQT as a 'point of departure' (since this is an *epistemological* judgement, to the effect that OQT is fundamentally *false*). From the standpoint of standard empiricism, Einstein's implacable opposition to OQT is just plain irrational prejudice. From the standpoint of aim-oriented empiricism, however, Einstein's rejection of OQT emerges as entirely well-founded, scientific, rational and objective. OQT is entirely acceptable from the standpoint of empirical considerations, but unacceptable from the equally important standpoint of theoretical unity, comprehensibility. The scandal is that the majority of contemporary physicists do not see this obvious point.

What is irrational, in other words, is not Einstein's rejection of OQT, but the majority *acceptance* of OQT, the general *blindness* to its gross defects. Einstein, I believe, held this to be the result of the fact that too many physicists put fame before understanding the universe. Einstein felt that, given a choice between winning a Nobel prize and improving our understanding of the universe, too many physicists would choose the former over the latter. This, for Einstein, amounted to a betrayal of the soul of theoretical physics, the pursuit of a corrupt goal, fame (not for Einstein so very different from the pursuit of power), in preference to the pursuit of the noble goal of improving understanding. And this in turn was, for Einstein, I believe, characteristic of a general sickness of our age: the pursuit of shallow or corrupt goals in life in preference to goals of genuine value.[9] Here is the source of Einstein's inability to contribute to OQT after 1926. It is in this sense that Einstein's pursuit of his unified field theory is a kind of moral protest; this was the clearest way in which he could express his conviction as to what physics ought to be, at its best.

Einstein did not get everything right about OQT. He assumed that the

[9] 'Perfection of means and confusion of goals seem—in my opinion—to characterize our age' (Einstein [1973], p. 337). One can regard this state of affairs as the result of the failure of our age to develop and implement a kind of rational inquiry designed to help us improve our goals, informed by aim-oriented rationalism and the philosophy of wisdom, themselves the outcome of generalizing Einstein's way of doing physics: see Maxwell [1976b and 1984].

ostensibly highly non-local features of OQT—which seem to contradict special relativity—do not correspond to reality. Here he was wrong.

If two particles, 1 and 2, interact at time t_1 and then separate widely then, in certain circumstances, a measurement performed on 1 at time t_2 enables one to predict with certainty what the result would be of measuring 2. A measurement of the momentum of 1 enables one to predict the precise momentum of 2; or, alternatively, a measurement of the position of 1 enables one to predict the position of 2. It is possible that 2 only acquires a precise momentum or position at time t_2, when one or other kind of measurement is performed on 1. This possibility requires that an influence of some kind travels instantaneously from 1 to 2 to inform 2 as to whether it should acquire a precise momentum or position. If we reject the existence of such instantaneous influences, then in order to explain the correlations between measurments on 1 and 2 we are obliged to hold, it seems, that these correlations are the outcome of correlations established at time t_1, when 1 and 2 interact. But this has the consequence that at time t_2 particle 2 must simultaneously have a precise position *and* momentum (since 2, by hypothesis, cannot 'know' instantaneously, at time t_2, whether particle 1, far away, is subjected to a position or momentum measurement). But, according to OQT, no system can be in a state which corresponds to having simultaneously a precise position *and* momentum. Thus OQT implies that correlations cannot be established at t_1 when 1 and 2 interact; they must be established instantaneously, at t_2, when one or other measurement is performed on 1.

That OQT does have this highly non-local character was discovered by Einstein, and was expounded in a famous paper by Einstein, Podolsky and Rosen [1935]. Because of the evident clash with special relativity, Einstein concluded that this kind of non-local prediction of OQT is *false*. Particle 2 *does* have a precise position and momentum at time t_2 irrespective of whether measurements are performed on 1 or not, and QT must be interpreted as a purely statistical theory which gives only an *incomplete* description of the evolution of the individual system.

Einstein held that the only reasonable option available was to interpret QT in this way, as an inherently incomplete, statistical theory of 'particles'. There can be no doubt that this reinforced his conviction that QT did not constitute a proper starting point for future developments—which in turn reinforced Einstein's search for a unified field theory.

Subsequent developments, due to Bohm [1957], Bell [1964], Aspect *et al.* [1982] and others, have shown that Einstein was *wrong* to dismiss the non-local predictions of OQT as not corresponding to reality: these predictions have now been experimentally confirmed!

This concludes my case for saying Einstein invented and applied aim-oriented empiricism in scientific practice in developing the special and general theories of relativity, and in critically examining quantum theory.

It may be asked: but did Einstein explicitly *advocate* aim-oriented empiricism? I turn now to a discussion of this question.

There can be, to begin with, no doubt that Einstein devoted his life to the goal of discovering the unified structure of the universe and that, for him, this constituted an entirely proper aim for science, indeed the noblest motive for pursuing scientific inquiry. Something of what the desire to understand meant to Einstein emerges from the following passage.

The most beautiful experience we can have is the mysterious. It is the fundamental emotion which stands at the cradle of true art and true science. Whoever does not know it and can no longer wonder, no longer marvel, is as good as dead, and his eyes are dimmed. It was the experience of mystery—even if mixed with fear—that engendered religion. A knowledge of the existence of something we cannot penetrate, our perceptions of the profoundest reason and the most radiant beauty, which only in their most primitive forms are accessible to our minds—it is this knowledge and this emotion that constitute true religiosity; in this sense, and in this alone, I am a deeply religious man. I cannot conceive of a God who rewards and punishes his creatures, or has a will of the kind that we expererience in ourselves. Neither can I nor would I want to conceive of an individual that survives his physical death; let feeble souls, from fear or absurd egoism, cherish such thoughts. I am satisfied with the mystery of the eternity of life and with the awareness and a glimpse of the marvelous structure of the existing world, together with the devoted striving to comprehend a portion, be it ever so tiny, of the Reason that manifests itself in nature. (Einstein [1973], p. 11.)

On one occasion in 1925 he expressed himself to the novelist Esther Salaman in the following terms:

I want to know how God created this world. I'm not interested in this-or-that phenomenon, in the spectrum of this-or-that element. I want to know His thoughts, the rest are details. (Salaman (1979], p. 22.)

That a basic aim of science is to unify all phenomena is affirmed in numerous passages, such as, from 1936 (see Einstein [1973], p. 293):

The aim of science is, on the one hand, a comprehension, as *complete* as possible, of the connection between the sense experiences in their totality, and, on the other hand the accomplishment of this aim *by use of a minimum of primary concepts and relations.* (Seeking, as far as possible, logical unity in the world picture, i.e. paucity in logical elements.)

Einstein also makes it clear that science at its best assumes that this goal of unification is realizable.

Certain it is that a conviction, akin to religious feeling, of the rationality or intelligibility of the world lies behind all scientific work of a higher order. This firm belief, a belief bound up with deep feeling in a superior mind that reveals itself in the world of experience, represents my conception of God. In common parlance this may be described as 'pantheistic' (Spinoza). (First published 1929; see Einstein [1973], p. 262.)

And on another occasion:

From the very beginning there has always been present the attempt to find a unifying theoretical basis for all [the] single sciences, consisting of a minimum of concepts and fundamental relationships, from which all the concepts and relationships of the single disciplines might be derived by logical process. This is what we mean by the search for a foundation of the whole of physics. The confident belief that this ultimate goal may be reached is the chief source of the passionate devotion which has always animated the researcher. (First published 1940; see Einstein [1973], p. 324.)

As for scientific realism, Einstein expresses himself with his usual clarity and brevity:

The belief in an external world independent of the perceiving subject is the basis of all natural science. Since, however, sense perception only gives information of this external world or of "physical reality" indirectly, we can only grasp the latter by speculative means. (First published 1931; see Einstein (1973], p. 266.)

And, on another occasion, as we have already seen:

Physics is an attempt to grasp reality as it is thought independently of its being observed.

It is all summed up succinctly in a letter to Cornelius Lanczos in 1942:

You are the only person I know who has the same attitude toward physics as I have: belief in the comprehension of reality through something basically simple and unified. (Dukas and Hoffmann [1979], p. 68.)

All this might seem more than enough to demolish decisively the views of those, like Fine and Popper, who hold that Einstein upheld some version of standard empiricism. Unfortunately it is not. In all the above quotations, Einstein can be interpreted as asserting no more than that he, and science, seek to discover, and presuppose the existence of, a unified structure to the universe, *in the context of discovery*. According to this interpretation, Einstein would hold that, *in the context of justification*, nothing must be permanently assumed about the nature of the universe, the sole aim being empirical adequacy, empirical considerations *alone* in the end deciding what is to constitute theoretical scientific knowledge.

On this issue—the crucial issue which divides off standard from aim-oriented empiricism—Einstein seems to have wavered. Consider the following passage.

Can we hope to be guided safely by experience at all when there exist theories (such as classical mechanics) which to a large extent do justice to experience, without getting to the root of the matter? I answer without hesitation that there is, in my opinion, a right way, and that we are capable of finding it. Our experience hitherto justifies us in believing that nature is the realization of the simplest conceivable mathematical ideas. I am convinced that we can discover by means of purely mathematical constructions the concepts and the laws connecting them with each other, which furnish the key to the understanding of natural phenomena. Experience may suggest the appropriate mathematical concepts, but they most certainly cannot be deduced from it. *Experience remains, of course, the sole criterion of the physical utility of a mathematical construction.* But the creative principle resides in mathematics. In a certain sense, therefore, I hold it true that pure thought can grasp reality, as the ancients dreamed. (Italics mine.) (First published 1933; see Einstein [1973], p. 274.)

This comes tantalizingly close to aim-oriented empiricism. A central tenet of aim-oriented empiricism is that we are rationally entitled to assume that the universe is knowable—there being some fallible, non-mechanical but rational method of discovery available to us—the knowability of the universe implying its comprehensibility. It is just this key element of aim-oriented empiricism which Einstein asserts here, his epistemological and methodological instincts as usual getting almost everything right. (The point is also brilliantly made in one of Einstein's most famous sayings: 'Raffiniert ist der Herrgott, aber boshaft ist er nicht'—God is sublime but not malicious.) Unfortunately, in the quotation given above, the italicized sentence provides Popper or Fine with the perfect excuse for interpreting the passage as a defence of standard empiricism. One can argue, of course, that the whole passage only really makes sense if interpreted as asserting: experience remains the *sole* criterion of physical utility *granted that we restrict our attention to simple, unified theories.* This would of course *violate* standard empiricism. But the text, as it stands, is sufficiently ambiguous to leave the matter undecided.

Consider next the following passage.

The very fact that the totality of our sense experiences is such that by means of thinking (operations with concepts, and the creation and use of definite functional relations between them, and the coordination of sense experiences to these concepts) it can be put in order, this fact is one which leaves us in awe, but which we shall never understand. One may say 'the eternal mystery of the world is its comprehensibility'. It is one of the great realizations of Immanuel Kant that the postulation of a real external world would be senseless without this comprehensibility.

In speaking here of 'comprehensibility', the expression is used in its most modest sense. It implies: the production of some sort of order among sense impressions, this order being producd by the creation of general concepts, relations between these concepts, and by definite relations of some kind between the concepts and sense experience. It is in this sense that the world of our sense experiences is comprehensible. The fact that it is comprehensible is a miracle.

In my opinion, nothing can be said *a priori* concerning the manner in which the concepts are to be formed and connected, and how we are to coordinate them to sense experiences. In guiding us in the creation of such an order of sense experiences, success alone is the determining factor. All that is necessary is to fix a set of rules, since without such rules the acquisition of knowledge in the desired sense would be impossible. One may compare these rules with the rules of a game in which, while the rules themselves are arbitrary, it is their rigidity alone which makes the game possible. However, the fixation will never be final. It will have validity only for a special field of application (i.e., there are no final categories in the sense of Kant). (First published 1936; see Einstein [1973], p. 292.)

This, once again, is tantalizingly close to aim-oriented empiricism. Einstein recognizes clearly that only in a very special kind of universe—a comprehensible universe—is scientific explanation and understanding possible. He recognizes that the particular way the universe is assumed to be comprehensible at any stage in the development of science will lead to rules or principles—such as Galilean or Lorentz invariance, the principle of equivalence, conservation of momentum and energy—without which physics would be impossible. And he points out that these rules are not final: it is to be expected that they will change as science advances. All this accords beautifully with aim-oriented empiricism. What violates aim-oriented empiricism is the suggestion that there is no Kantian synthetic *a priori* proposition built into scientific knowledge. According to aim-oriented empiricism, there is just one such proposition, namely: the universe is comprehensible (in some way or other). We cannot of course *know for certain* that this proposition is true. It must remain for ever a *conjecture*—all our knowledge being conjectural in character. The decisive point is that knowledge becomes impossible if this conjecture is false, this rationally entitling us to adopt the conjecture as a permanent part of scientific knowledge. Nothing is to be gained from doubting the conjecture, and much may be gained from incorporating it into the rest of our conjectural scientific knowledge. In short, 'the universe is comprehensible' is a synthetic *a priori* statement not in the full-blooded Kantian sense that it can be known to be true of all possible experience with absolute certainty, but in the radically qualified Kantian sense that it is a conjecture about reality—about the noumenal world—which must remain permanently an integral part of conjectural human knowledge, and which is adopted as knowledge on non-empirical grounds. This crucial tenet of aim-oriented empiricism is, it seems, explicitly rejected by Einstein in the above passage—even though the whole point of the passage, ironically enough, is to affirm it, affirm, that is, that science *cannot* proceed without the assumption that the universe is comprehensible.

Einstein's ambivalent attitude to the crucial issue which separates off standard from aim-oriented empiricism gains explicit expression in the following quotation.

[The aim of science is to arrive] at a system of the greatest conceivable unity, and of the greatest poverty of concepts of the logical foundations, which is still compatible with the observations made by our senses. We do not know whether or not this ambition will ever result in a definite system. If one is asked for his opinion, he is inclined to answer no. While wrestling with the problems, however, one will never give up the hope that this greatest of all aims can really be attained to a very high degree. (First published 1936; see Einstein [1973], p. 294.)

We might interpret this to mean that when Einstein is thinking primarily as a theoretical physicist he unthinkingly takes the ultimate comprehensibility of the universe for granted—the key component of aim-oriented empiricism. When he comes to reflect philosophically about the aims and methods of his work, however, his (misconceived) philosophical conscience gets the better of him, and he lapses into standard empiricism. Einstein's scientific instincts, in short, are more enlightened than his philosophical reflections—an important point, implicit in my claim that aim-oriented empiricism arose, for Einstein, out of scientific practice, adopted in response to a severe scientific problem.

Are we to conclude, then, that Einstein did not in the end manage to free himself explicitly from the trap of standard empiricism? One point to remember is that throughout his scientific life Einstein's views on the philosophy of science evolved from something close to Machian positivism at the outset (an extreme version of standard empiricism) to a view that comes to resemble aim-oriented empiricism more and more closely towards the end of his life. Einstein himself put the matter like this, in a letter to Lanczos in 1938.

> Coming from sceptical empiricism of somewhat the kind of Mach's, I was made, by the problem of gravitation, into a believing rationalist, that is, one who seeks the only trustworthy source of truth in mathematical simplicity. The logically simple does not, of course, have to be physically true; but the physically true is logically simple, that is, it has unity at the foundation. (See Holton [1973], p. 241.)

This, to begin with, sounds like a clear enough confession of a convinced aim-oriented empiricist. As it stands, it is perhaps something of an oversimplification. In the first place, as we have seen above, elements of aim-oriented empiricist thinking can be found in Einstein's scientific work almost from the outset—from Einstein's first great creative period in 1902–5. Second, Einstein's views concerning the philosophy of science went on developing long after the creation of general relativity, right to the end of his life. Our best hope, then, of finding a clear, unambiguous formulation of aim-oriented empiricism is to look at Einstein's very last writings on philosophy of science. I provide two final quotations. The first comes from Einstein's 'Autobiographical Notes', written when he was 67. Einstein is discussing the points of view from which physical theories can be critically assessed, quite generally.

> The first point of view is obvious: the theory must not contradict empirical facts. . . . The second point of view is not concerned with the relation to the material of observation but with the premises of the theory itself, with what may briefly but vaguely be characterized as the 'naturalness' or 'logical simplicity' of the premises (of the basic concepts and of the relations between these which are taken as a basis). This point of view, an exact formulation of which meets with great difficulties, has played an important role in the selection and evaluation of theories since time immemorial. The problem here is not simply one of a kind of enumeration of the logically independent premises (if anything like this were at all unequivocally possible), but that of a kind of

reciprocal weighing of incommensurable qualities . . . Of the 'realm' of theories I need not speak here, inasmuch as we are confining ourselves to such theories whose object is the *totality* of all physical appearances. The second point of view may briefly be characterized as concerning itself with the 'inner perfection' of the theory, whereas the first point of view refers to the 'external confirmation'. The following I reckon as also belonging to the 'inner perfection' of a theory: We prize a theory more highly if, from the logical standpoint, it is not the result of an arbitrary choice among theories which, among themselves, are of equal value and analogously constructed. (Einstein [1949], pp. 21–3.)

It is surely clear from this that Einstein came quite explicitly to repudiate all versions of standard empiricism towards the end of his life. There is no suggestion here that the second requirement of 'inner perfection' or unity is somehow to be reduced to the first requirement of empirical adequacy: empirical considerations do *not*, for Einstein, *alone* determine choice of theory. Furthermore, Einstein has made it abundantly clear already that, in his view, in choosing only theories which satisfy the requirement of inner perfection, we are in effect assuming that the universe itself is comprehensible—this being a permanent presupposition of scientific knowledge upheld on non-empirical grounds. But in case there is any doubt on this score, here is a passage written in 1950 in which the thesis that there can be no knowledge without the presupposition that the universe is comprehensible is explicity affirmed.

It is of the very essence of our striving for understanding that, on the one hand, it attempts to encompass the great and complex variety of man's experience, and that on the other, it looks for simplicity and economy in the basic assumptions. The belief that these two objectives can exist side by side is, in view of the primitive state of our scientific knowledge, a matter of faith. Without such faith I could not have a strong and unshakable conviction about the independent value of knowledge. (Einstein [1973], p. 357.)

I conclude that Einstein came close to articulating aim-oriented empiricism towards the end of his life, even if he did not recognize that this position is required to solve the problem of induction, and did not appreciate that it provides a more rational conception of science than does standard empiricism—and not a less rational conception, as Einstein's references to 'faith' and 'miracle-creed' tend to suggest.

In the end, however, what really matters is the philosophy of science implicit in Einstein's scientific deeds. Einstein himself held this view. As he put it: 'If you want to find out anything from the theoretical physicists about the methods they use, I advise you to stick closely to one principle: don't listen to their words, fix your attention on their deeds' (Einstein [1973], p. 270). As we have seen above, in order to make rational sense of Einstein's scientific judgements and deeds it is essential to see them from the standpoint of aim-oriented empiricism. More important, Einstein can be said to have invented aim-oriented empiricism in scientific practice during the course of discovering the special and general theories of relativity. His success in discovering these theories owes much to the invention and exploitation of the rational method of discovery of aim-oriented empiricism. This aspect of Einstein's work transformed the whole character of subsequent theoretical physics. Einstein's contributions to theoretical physics are intimately interrelated to his contribution to the philosophy of physics: after Einstein, indeed, physics and philosophy of physics ought to form one integrated discipline—aim-oriented empiricist natural philosophy. The various versions of standard empiricism defended by Popper, van Fraassen and Fine (and most contemporary philosophers of science) all fail to do justice to this vital dimension of Einstein's contribution to science. Indeed, advocacy of standard empiricism after Einstein amounts in itself to a failure to understand an important aspect of Einstein's contribution to science.[10]

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[10] In my view, the most important implications of the new way of doing physics created by Einstein in developing special and general relativity lie in fields far beyond that of theoretical physics: see Maxwell [1976b, 1980, 1984, 1986, 1991, 1992], where I attempt to spell out these implications for science as a whole, for technological research, social inquiry, scholarship and education.

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