[More recent literature of a similar orientation includes Professor Herbert Dingle's Science at the Crossroads http://russamos.narod.ru/dingle/Preface.html and the work of Professor Dr Paul Marmet, eg. with C. Couture, 'Relativistic Deflection of Light Near the Sun Using Radio Signals and Visible Light' http://www.newtonphysics.on.ca/ECLIPSE/Eclipse.html]

What is wrong with relativity?

[fn. The substance of lectures given to the Royal Institute of Philosophy, University College Chemical and Physical Society, The Institute of Science Technicians, etc.]

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Genuine physicists—that is to say, physicists who make observations and experiments as well as theories—have always felt uneasy about 'relativity'. As Bridgman said, "if anything physical comes out of mathematics it must have been put in in another form". The problem was, he said, to find out where the physics got into the theory (Bridgman 1927).

The uneasiness was increased when it was clear that distinguished scientists like C. G. Darwin and Paul Langevin could be completely misled. Darwin wrote a fatherly letter to *Nature* (Darwin 1957) describing the simple way in which he explained 'relativity' to his friends: the simplicity, however, was due to the fact that, with the exception of a quoted formula, there was no relativity theory in it at all. Langevin, likewise, gave a supposedly 'relativistic' proof of the results of an optical experiment by Sagnac, but as his countryman Andre Metz said, although "assez élégant", it was not relativity (Metz 1952).

There were other disturbing features: the fact that Einstein never wrote a definitive account of his theory; that his first derivation of the Lorentz transformation equations contained velocities of light of $c — v$, $c + v$ and $(c^2 — v^2)^{1/2}$, quite contrary to his second postulate that the velocity of light was independent of the motion of the source; and that his first attempt to prove the formula $E = m_0c^2$, suggested by Poincaré, was fallacious because he assumed what he wanted to prove, as was shown by Ives (Ives 1952).

It is not surprising, therefore, that genuine physicists were not impressed: they tended to agree with Rutherford. After Wilhelm Wien had tried to impress him with the splendours of relativity, without success, and exclaimed in despair "No Anglo-Saxon can understand relativity!", Rutherford guffawed and replied "No! they've got too much sense !"[fn. Quoted from the Rutherford Memorial Lecture to the Physical Society 1954 by P.M.S. Blackett (*Yearbook of the Physical Society* (*1955*))] Let us see how sensible they were.

First of all, a little history. There is no need to repeat the accounts, now given in many textbooks, of the unsuccessful attempts to detect the aether. The simplest hypothesis, namely that the aether did not exist and that we were thus left with action-at-a-distance or ballistic transmission, was held to be unacceptable. Instead, Poincaré preferred to raise this failure to a 'principle'—the principle of relativity—saying: "The laws of physical phenomena must be the same for a 'fixed' observer as for an observer who has a uniform motion of translation relative to him, so that we have not, and cannot possibly have, any means of discerning whether we are, or are not, carried along by s uch a motion." As a result there would perhaps be "a whole new mechanics, where, the inertia increasing with the velocity, the velocity of light would become a limit that could not be exceeded" (Poincaré 1904).

In the next year, 1905, Einstein re-stated Poincaré's principle of relativity and added the postulate that the velocity of light is independent of the velocity of its source. From the principle and the postulate he derived the Lorentz transformation equations, but in an unsatisfactory way as we have seen. Another curious feature of this now famous paper (Einstein 1905) is the absence of any reference to Poincaré or anyone else: as Max Born says, "It gives you the impression of quite a new venture. But that is, of course, as I have tried to explain, not true (Born 1956).

In 1906 Planck worked out the 'new mechanics' predicted by Poincaré, obtaining the well-known formula

$$F = \frac{d}{dt}\left\{\frac{mv}{(1 — v^2/\ c^2)^{1/2}}\right\}$$

and the corresponding expressions for momentum and energy. In the next year he derived and used the mass-energy relation (Planck 1906, 1907). In 1909, G. N. Lewis drew attention to the formula for the kinetic energy

$$\frac{m_0 c^2}{(1 — v^2/\ c^2)^{1/2}} — m_0 c^2$$

and suggested that the last term should be interpreted as the energy of the particle at rest (Lewis 1909). Thus gradually arose the formula $E = mc^2$, suggested without general proof by Poincaré in 1900.

It will be seen that, contrary to popular belief, Einstein played only a minor part in arriving at the main ideas and in the derivation of useful formulae in the restricted, or special, theory of relativity, and Whittaker cal led it the relativity theory of Poincaré and Lorentz, pointing out that it had its origin in the theory of aether and electrons (Whittaker 1953). A recent careful investigation by Keswani confirms this opinion; he summarizes Poincaré's contribution as follows:

"As far back as 1895, Poincaré, the innovator, had conjectured that it is impossible to detect absolute motion. In 1900 he introduced 'The principle of relative motion' which he later called by the equivalent terms 'The law of relativity' and 'The principle of relativity' in his book *Science and Hypothesis* published in 1902. He further asserted in this book that there is no absolute time and that we have no intuition of the 'simultaneity' of two 'events' [mark the words] occurring at two different places. In a lecture given in 1904, Poincaré reiterated the principle of relativity, described the method of synchronization of clocks with light signals, urged a more satisfactory theory of the electrodynamics of moving bodies based on Lorentz's ideas and predicted a new mechanics characterized by the rule that the velocity of light cannot be surpassed. This was followed in June 1905 by a mathematical paper entitled 'Sur la dynamique de 1'electron', in which the connection between relativity (impossibility of detecting absolute motion) and the Lorentz transformation, given by Lorentz a year earlier, was recognized. [fn. Gravitational waves with velocity *c* and the velocity addition formula should be included (Keswani 1966).]

In point of fact, therefore, Poincaré was not only the first to enunciate the principle, but he also discovered in Lorentz's work the necessary mathematical formulation of the principle. All this happened before Einstein's paper appeared" (Keswani 1965).

Einstein's attempt to derive the Lorentz transformation equations from the principle of relativity and the postulate that the velocity of light is independent of that of the source would (if it had not involved a contradiction) have made Lorentz transformations independent of any particular assumption about the construction of matter (as it had not been in Lorentz's derivation). This feature, of course, was pleasing to the mathematically minded, and Pauli considered it an advance. Einstein said that the Lorentz transformations were "the real basis of the special relativity theory" (Einstein 1935), and this makes it clear that he had converted a theory which, in Lorentz's hands at any rate, was a physical theory (involving, for instance, contraction of matter when moving with respect to the aether) into something that is not a physical theory in the ordinary sense, but the physical interpretation of a set of algebraic transformations derived from a principle which turns out to be a rule about laws, together with a postulate which is, or could be, just the algebraic expression of a fact—the independence of the velocity of light of that of the source (experiments already done appear to confirm it but more direct evidence is needed). We see, then, that 'relativity' is not an ordinary physical the ory: it is what Synge calls a "cuckoo process"; that is to say. Nature's laws must be found first, and then they can, perhaps, be adapted to comply with the overall 'principle'.

"The eggs are laid, not on the bare ground to be hatched in the clear light of Greek logic, but in the nest of another bird, where they are warmed by the body of a foster mother, which, in the case of relativity, is Newton's physics of the 19th century" (Synge 1956).

The special theory of relativity is therefore founded on two postulates

(a) a law about laws (Poincaré's principle of relativity).

(b) an algebraic representation of what is, or could be, a fact (velocity of light constant, independent of the velocity of the source) and its application to the physical universe is

(c) a cuckoo process.

This basis of the theory explains a great deal that has mystified many physicists and engineers. They could not understand how Einstein could sometimes speak as though the aether was superfluous (Einstein 1905) and at other times say "space without aether is unthinkable" (Einstein 1922). This was due, of course, to not starting with physical terms—matter its motion, and its interactions (force). A physical theory which included radiation would have to *start* by stating whether an aether, action-at-a-distance, or ballistic transmission of force was being postulated It explains, also, how mass and inertial force get into the special theory which is founded on a geometrization of *uniform* velocities, for it is well known that inertial forces do not appear when the velocities are uniform. Formulae which purport to give the relation between measurements in one state of uniform velocity and those made in another state of uniform motion cannot logically throw any light on what happens during the *change* from one state to the other. This is only possible by using the cuckoo process—assuming Newton's second law and the conservation of momentum, and then modifying them. It also makes clear how Einstein could call Tolman's account of theory (Tolman 1934) definitive, and also praise Bergmann's treatment (Bergmann 1942), when the former author thought length contractions real and in principle observable, whereas the latter seems to have thought it only an appearance.

The fact that Einstein asserted that the Lorentz transformation equations were the basis of the special theory, and these are, of course, purely mathematical, means that, in so far as the theory is considered to have any physical implications, these implications must be the result of the *interpretation* of mathematical expressions in physical terms. But in this process there can be no guarantee that contradictions will not arise, and, in fact, serious contradictions have have arisen which have marred the special theory. Half a century of argumentation has not removed them, and the device of calling them only apparent contradictions (paradoxes) has not succeeded in preventing the special theory of relativity from becoming untenable as a physical theory.

The most outstanding contradiction is what the relativists call the clock paradox. We have two clocks, A and B, exactly similar in every way, moving relatively to one another with uniform velocity along a line joining them. If their own interaction is ignored and they are far removed from other matter, they continue to move with uniform velocity, and so each clock can be considered as being the origin of a set of inertial axes. The Lorentz transformations show that the clock which is treated as moving goes slow. The principle of relativity, however, asserts that, as A and B both provide inertial frames, they are equivalent for the description of Nature, and all mechanical phenomena take the same course of development in each. Referred to A, B goes slow; referred to B, A goes slow. It is not possible for each of two clocks to go slower than the other. There is thus a contradiction between the Lorentz transformations and the principle.

This contradiction can be seen clearly in a diagram which prevents the confusion which arises when the expression 'as se en from' is allowed to enter the argument (e.g. 'the time at B as seen from A'). In figure 1, two long lines of clocks are passing close to one another with uniform velocity *V*. At a given instant, two clocks opposite one another, A and B, are set to read the same time. All the A series of clocks are then synchronized with A by the method of reflected light signals, suggested by Poincaré and accepted by Einstein and other relativists. In a similar way all the B series of clocks are synchronized with B.

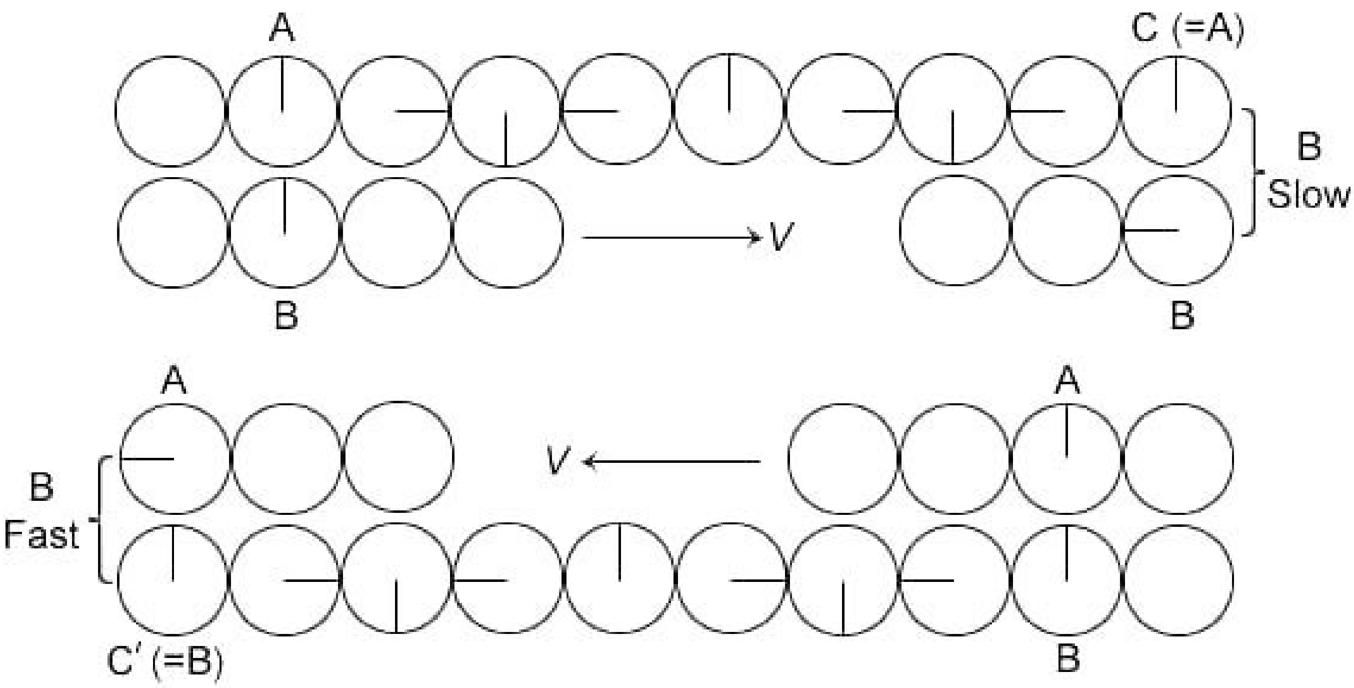

Figure 1.

In the upper diagram the A clocks are taken to be at rest and the B clocks to be moving to the right. After a time interval, the clock B has travelled a distance *d*, say: its reading is then compared with that of the clock C momentarily opposite to it. C, however, has been synchronized with A so that the comparison is in effect a comparison of B with A. According to the Lorentz transformations, the moving clock B goes slow, and its reading, therefore, behind that of C(= A) as shown. In the lower diagram the B clocks are taken to be at rest and the A clocks to be moving to the left. When A has travelled the distance d, its reading is compared with that of clock C', momentarily opposite to it. But, as before, C' has been synchronized with B so that we have , in effect, another comparison of B with A, and this time A's clock goes slow, so that B's reading is in advance of A's as shown. The two comparisons should yield the same result according to the principle of relativity. It is obvious that they do not.

A more intriguing instance of this so-called 'time dilation' is the well-known 'twin paradox', where one of two twins goes for a journey and returns to find himself younger than his brother who remained behind. This case allows more scope for muddled thinking because acceleration can be brought into the discussion. Einstein maintained the greater youthfulness of the travelling twin, and admitted that it contradicts the principle of relativity, saying that acceleration must be the cause (Einstein 1918). In this he has been followed by relativists in a long controversy in many journals, much of which ably sustains the character of earlier speculations which Born describes as "monstrous" (Born 1956).

Surely there are three conclusive reasons why acceleration can have nothing to do with the time dilation calculated:

(i) By taking a sufficiently long journey the effects of acceleration at the start, turn-round and end could be made negligible compared with the uniform velocity time dilation which is proportional to the duration of the journey.

(ii) If there is no uniform time dilation, and the effect, if any, is due to acceleration, then the use of a formula depending only on the steady velocity and its duration cannot be justified.

(iii) There is, in principle, no need for acceleration. Twin A can get his velocity *V* before synchronizing his clock with that of twin B as he passes. He need not turn round: he could be passed by C who has a velocity *V* in the opposite direction, and who adjusts *his* clock to that of A as he passes. When C later passes B they can compare clock readings. As far as the theoretical experiment is concerned, C's clock can be considered to be A's clock returning without acceleration since, by hypothesis, all the clocks have the same rate when at rest together and change with motion in the same way independently of direction. [fn. I am indebted to Lord Halsbury for pointing this out to me.] One more contradiction, this time in statics, may be mentioned: this is the lever with two equal arms at right angles and pivoted at the corner. It is kept in equilibrium by two equal forces producing equal and opposite couples. According to the Lorentz transformation equations referred to a system moving with respect to the lever system, the couples are no longer equal so the lever should be seen to rotate, which is, of course, absurd. Tolman tried to overcome this by saying that there was a flow of energy entering one lever arm and passing out through the pivot, just stopping the rotation! Overlooking the fact that energy is a metrical term and not anything physical (Brown 1965, 1966), there would presumably be some heating in the process which is not considered. Statics provides insuperable difficulties for the physical interpretation of Lorentz transforma tion equations and this part of mechanics is avoided in the textbooks—in fact, Einstein omits statics in his definition: "The purpose of mechanics is to describe how bodies change their position in space with time" (Einstein 1920, P. 9).

The three examples which have been dealt with above show clearly that the difficulties are not paradoxes) but genuine contradictions which follow inevitably from the principle of relativity and the physical interpretations of the Lorentz transformations. The special theory of relativity is therefore untenable as a physical theory.

Turning now to the general theory of relativity, Einstein tells us in his autobiography (Einstein 1959) how, at the age of 12, he began to doubt Bible stories. "The consequence was a positively fanatic (orgy of) free-thinking coupled with the impression that youth is intentionally being deceived by the State through lies; it was a crushing impression. Suspicion against every kind of authority grew out of this experience, a sceptical attitude tow ards the convictions which were alive in any specific social environment—an attitude which has never again left me."

This sceptical attitude towards prevailing convictions possibly explains why Einstein was not satisfied with the relativity theory of Poincaré and Lorentz which stopped short of including accelerating systems, thus still leaving something apparently 'absolute'. He still seemed to be affected by this word 'absolute', but it is difficult to see what it could mean except with regard either to the Sensorium of God (Newton) or an aether pervading all space. He pushed on, therefore, with an attempt to show that natural laws must be expressed by equations which are covariant under a group of *continuous* coordinate transformations. This group, which Einstein took as the algebraic expression of a general principle of relativity, included, as a subgroup, the Lorentz transformations which Poincaré had taken as the algebraic expression of the restricted principle.

To overcome the physical difficulty that acceleration produces forces (inertial) whereas uniform velocity does not, Einstein was led to assert that these forces cannot be distinguished from ordinary gravitational force, and are therefore not an absolute test of acceleration. This contention Einstein called the principle of equivalence. In trying to support this contention, he imagined a large closed chest which was first at rest on the surface of a large body like the Earth, and then later removed to a great distance from other matter where it was pulled by a rope until its acceleration was *g*. No experiment made inside could, he claimed, detect the difference in the two cases. But in this he was mistaken, as I have shown (Brown 1960).

In the first case, if two simple pendulums were suspended with their threads a foot apart, the threads would not be parallel but point towards the centre of mass of the Earth (or a point somewhat nearer allowing for their mutual attraction). The angle between them would, in principle, be detectable by the Mount Palo mar telescope. When accelerated by a rope, the threads would be parallel if it were not for the small mutual attraction. If now, the threads were moved so as to be further apart, the angle between them would *increase* in the first case, but in the second case the threads would become more parallel so that the angle would therefore *decrease*. The principle of equivalence is therefore untenable. It is gratifying to find one theoretician who states that the principle is false (Synge I960): "In Einstein's theory there is a gravitational field or there is none, according as the Riemann tensor does or does not vanish. This is an absolute property: it has nothing to do with the observer's world-line."

The principle of equivalence is made plausible by the use of the expression 'gravitational field', overlooking the fact that this is a useful conception but cannot be demonstrated. All we can do is place a test particle at the point in question and measure the force on *it*. This might be action-at-a-distance. As soon as the term 'field' is dropped and we talk about the gravitational force between bodies at rest we realize that the force is centripetal, whereas the force of inertia is not. This is an important difference obscured by the use of the word 'field'. Relativists now admit that the principle of equivalence only holds at a point; but then, of course, we have left physics for geometry—experiments cannot be made at a point.

This contact with the physical world having gone we are left in the general theory only with the principle of covariance—that the *laws* of physics must be expressed in a form independent of the coordinate system, and the mathematical development of this condition which Einstein did with Grassman and others. Unfortunately, given sufficient ingenuit y, almost any law of physics can be expressed in covariant form, so that the principle imposes no necessary restriction on the nature of these laws. The principle is therefore barren, and Einstein had to regard it as merely of heuristic significance (by considering only the *simplest* laws in accord with it (Einstein 1959, p. 39)). Also the number of problems which can be completely formulated, let alone solved, is extremely small. Some relativists look on it rather as an encumbrance (Fock 1959).

The three consequences stemming from Einstein's theory of gravitation, that are usually brought forward as supporting it, are also not impressive. The movement of the perihelion of Mercury was known before and can be explained in various ways (Whittaker 1953). The 'bending of light' round the Sun had been suggested before, and the much advertised confirmation in the eclipse of 1919 involved assuming Einstein's law of 'bending' to obtain the 'scale constants', with the help of which the results were derived which were supposed to prove it. The deflections of stars that moved transversely or in the opposite direction to that predicted were omitted. The mean deviation and its direction varied from plate to plate during the eclipse, suggesting refraction in a turbulent diffuse 'atmosphere'. Nevertheless a mean value was obtained "in exact accord with the requirements of the Einstein theory" (*Lick Observatory Bulletin* 1922, No. 346). Later attempts have given different values. This must be one of the most extraordinary self-deceptions in the whole history ot science (see Poor 1930). The gravitational red shift of light now appears to be confirmed, but this follows from Mach's hypothesis. [fn. Einstein and others call it Mach's principle, but it is not a principle—it is a physical hypothesis.] that inertial forces are due to interaction with the distant bodies of the Universe [fn. Newton considered this possibility (see Brown 1943)], and does not require 'relativity' as the author has shown (Brown 1955).

We see, then, that the general theory is based physically on a fallacy (principle of equivalence) and on a principle that is barren (covariance) and which is also, mathematically, almost intractable. Genuine physicists may well agree with Fock that it is not a major contribution to physics.

The whole subject of 'relativity' is extremely interesting looked at from the point of view of scientific method. Western science long ago involved the rejection of the view that Nature's ways can be found by just taking thought, or by t he adoption of principles based on reason alone, or beauty, or simplicity. The idea of perfection in the heavens, as we know, held back astronomy with epicycles and caused sunspots to be explained away.

Newtonian method consists in first establishing the facts by careful observation and experiment, and then proceeding to attempt an explanation of them in physical terms—matter, motion and force—then from such a theory to derive, by logic and mathematics, various principles (e.g. conservation of momentum) as well as further consequences which can be put to experimental test. Natural science is concerned with causes: logic and mathematics are only tools. Newton made this clear when, after giving the first satisfactory explanation of the tides, he said: "Thus I have explained the causes of the motion of the . . . sea. Now it is fit to subjoin something concerning the quantity of those motions." But relativists now assert that "The dignity of pure theoretical speculation has been rehabilitated . . . based on a process of the mind with its own justification" (shades of Descartes!). Relativity "has saved science from narrow experimentalism, it has emphasized the part which beauty and simplicity must play in the formulation of theories of the physical world" (Mercier 1955).

The disadvantages of systems of theoretical speculation based on a process of the mind with its own justification—well understood by Bacon and the early founders of the Royal Society—are very evident in 'relativity'. Uncomfortable facts have to be forced into the system by specious reasoning, as in the case of the right-angled lever mentioned above, or ignored altogether, as in the case of Römer's one-way determination of the velocity of light. This method is not mentioned in books by relativists although it is a famous determination, being the first historically, and known to Newton in his later years. Römer's method is worth examining in detail because it nullifies Einstein's contention, repeated by Eddington and others, that we only know the out-and-return velocity, not the one-way velocity, so that the time of arrival of a signal at a distant point is never known from observation but can only be a convention. Römer measured the durations of the eclipse of one of Jupiter's satellites. These time periods increased when the Earth was moving away from Jupiter, and decreased again when the Earth was moving towards it. A knowledge of the size of the Earth's orbit, and thus the distances moved during the eclipses, allowed a calculation of the velocity of light which had only travelled one way. Modern photometric observations at Harvard University yield an excellent value which remains constant with the varying changes of direction as Jupiter moves round in its orbit.

Now the timing of the eclipses on the Earth's surface is not open to criticism, since measurement of time is *defined* for observers on the Earth. But relativists might say that the assumption of uniform rotation of the satellite, based on Newton's laws, and the use of astronomical triangulation applied to moving bodies (which is necessary to determine the Earth's orbit) both involve knowledge of the one-way velocity of light, and that this is constant—which is just what we are trying to determine.

Although the high accuracy of astronomical observations and the general agreement with theory over long periods of time is a sufficient proof that the velocity of light does not fluctuate, the best way to avoid this criticism is to notice that the experiment could be carried out *in principle* (we are only concerned with the relativist assertion that is impossible in principle) on the surface of the Earth.

The periodic eclipses could be replaced by a flashing beacon B (figure 2) controlled so as to flash at whatever are defined to be equal intervals, and this equality can be judged from the distant point A with one clock. The observer is carried round on the edge of a circular rotating table (corresponding to the Earth's motion in its orbit), and makes a mark on the stationary surrounding rim every time he sees a flash (this could be done automatically).

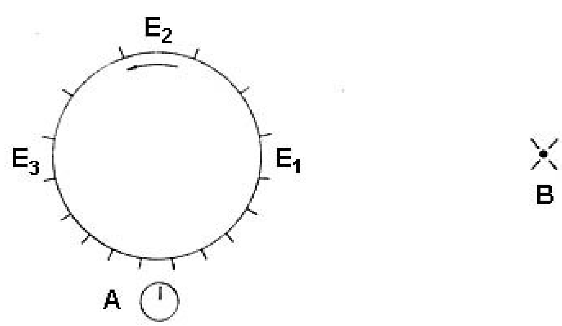

Figure 2.

These marks get further apart between E_1, and E_2, corresponding to the increase in eclipse time periods in the Jupiter case. The clock A, at rest with respect to the beacon, the centre of th e table and the stationary rim, makes marks on the table edge, the distances between which can be used as a test of uniform rotation, and also serve to convert the distances between the stationary rim marks into time intervals. The distance E_1E_3 is measured with the metre bar. The one-way velocity is calculated, as in t he astronomical case, from the data. In this way we can avoid using the properties of light in order to determine the length E_1E_3, and there is only one clock. With modern techniques this method might possibly be used to test the effect of movement of the source on the velocity of light.

Belief in principles because of their mathematical elegance, or cogency, leads also to a distortion of physics, its purpose and its history. Most of the discussion about observers and their imagined measurements is remote from anything that physicists do. Having to call force a fiction, which it cannot be by definition, since we have a special set of deep-seated nerves for detecting it, and asserting that it can be removed by a mere transformation of axes illustrate distortions of physics which are common. Even distortion of mathematics occurs in Einstein's later attempt to derive the Lorentz transformation equations from the principle of relativity together w ith algebraic expression of the constancy of the velocity of light. In this proof he is forced, as Essen has pointed out (Essen 1962), to use the same symbol for two different quantities, and later he derives a dimensionally impossible equation by putting a length equal to unity (Einstein 1920). [fn.Relativists seem to be rather shaky on dimensions: has not Eddington told us that the mass of the Sun is 1·47 km and have we not been favoured with a revelation from Ireland that 1° centigrade = $3{\cdot}804 \times 10^{-76}$ seconds (Synge 1960)?] It is difficult not to repeat Keswani's comments on Einstein's first (1905) proof: "The steps taken have a curiously compensating effect and apparently the demonstration was driven towards the result" (Keswani 1965). The distortion of the purpose of physics has already been exemplified by Einstein's definition of mechanics which leaves out statics. "The object of physics is to predict the results of given experiments concerning stated events", says McCrea (McCrea 1952) but the business of physicists is with "the causes of sensible effects", as Newton said—*causes*, not just rules and predictions. The distortions of the history of physics are too common to be worth detailed mention—many papers and broadcast lectures begin with a travesty of Newton's views.

Einstein's own part in the development of 'relativity' is particularly instructive from the point of view of scientific method. The early adolescent suspicion of all authority, and consequently of anything called 'absolute'—resulting in the desire to prove all frames of reference equal—led to proofs having to be forced and contrary facts ignored. As so often happens in other spheres, some frames turned out to be more equal than others (inertial frames). The attempt to extend 'equality' to accelerated axes led to invoking a principle (equivalence) whose application gradually shrank to a mathematical point, and to a postulate (covariance) which turned out to be barren. His final years devoted to trying to obtain a unitary mathematical treatment of gravitation and electrodynamics ended in failure. It is difficult to think of a more convincing demonstration of the dire effects of abandoning Newtonian method.

What then remains of the *theory*? The Lorentz transformations have proved not to be the necessary formulation of the principle of relativity, as Poincaré believed, since physical interpretations of them have contradicted the principle. When applied, perspicaciously, to Newtonian physics they produce formulae which are certainly superior to the 'classical' ones at high speeds. But the Lorentz transformation equations were first derived and used by Voigt in 1887 in connection with elasticity, and later, again by Lorentz in connection with the electron theory of matter, and do not depend on 'relativity' for their derivation. [fn. They can be derived without the principle (see Capildeo 1967)] The placing of the Lorentz term $(1-v^2/c^2)^{1/2}$ under *m* the mass, following Poincaré's prediction of a velocity *c* that cannot be exceeded by matter, has been supported by experiments with accelerators (relative to the machine). Once again, however, interpretations of algebra are not a substitute for genuine physical theory: the interaction of a particle with distant matter (force of inertia), tending to infinity when *v* approaches *c*, is not the only physical interpretation it may be that interaction with nearby matter (the accelerating force) may tend to zero when *v* approaches *c*. This hypothesis, for example, avoids the supposition of an enormous amount of matter in the Universe for which there is no evidence (Brown 1955, 1957, 1958, 1963).

The general theory has been well summed up by Fock: "It is... incorrect to call Einstein's theory of gravitation a 'General theory of relativity' all the more since 'The general principle of relativity' is impossible under any physical condition."

The general covariance of equations has quite a different meaning from the physical principle of relativity, it is merely a formal property of the equations which allows one to write them down without prejudging the question of what coordinate system to use. The solution of equations written in generally covariant form involves four arbitrary functions; but the indeterminacy arising from this has no fu ndamental importance and does not express any kind of 'general relativity'. From a practical point of view such an indeterminacy even represents something of a disadvantage" (Fock 1959).

It is still too soon to attempt a final judgment on 'relativity', but certainly we can say that 'relativity' has not provided convincing justification for adopting a new scientific method which involves "processes of the mind which are their own justification", and rejecting Newton's continual plea for more experiments as "narrow experimentalism". Nor does it justify substituting the derivation of physical theory, by interpretation of an algebraic representation of a postulated general principle, for the derivation of general principles from the algebraic representation of a physical theory.

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