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Editor-in-Chief: Dr. Ray Scott Percival
E-mail: tcr-editors@www.eeng.dcu.ie
Web: http://www.eeng.dcu.ie/~tkpw/tcr/tcr-home

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Einstein, Popper and the Theory of Relativity

Christoph von Mettenheim

Contents

Introduction

Der Aberglaub´, in dem wir aufgewachsen,
Verliert, auch wenn wir ihn erkennen, darum
Doch seine Macht nicht über uns. - Es sind
Nicht alle frei, die ihrer Ketten spotten.

G.E. Lessing

(1) Karl Popper has found beautiful words for his admiration of the period of enlightenment, which he considered one of the most inspired in European history[1]. In my country this period is connected not only with the name of the great philosopher and scientist Immanuel Kant, but also with that of the poet Gotthold Ephraim Lessing, one of the early outstanding figures in German literature. Having acquired from Karl Popper a taste for using mottoes I would like to put this paper under a motto taken from Lessing's play "Nathan der Weise"—"Nathan the Wise". This is a beautiful play, staging a dramatic background from the time of the crusades for a strong and very touching plea for tolerance between the three great religions, the Jewish religion, Christianity, and Islam, and Lessing puts into the mouth of one of the main figures of the play, the young templar, words which might, in a clumsy attempt to preserve the metre, perhaps be translated as follows:

The superstition in which we were brought up
will, though we have seen through it, thereby
not lose its power over us. - Not all
are free who mock at their own fetters.

(2) Of this last passage, "Not all are free who mock at their own fetters"—"Es sind nicht alle frei, die ihrer Ketten spotten", I feel reminded time and again when reading Karl Popper's books, because I keep discovering new implications of ideas of his which I have accepted as true many years ago without, apparently, realising their full importance. This used to worry me a lot, but I have ceased to do so after reading, in *Unended Quest*, that it took Popper himself years to discover the bearing which his demarcation criterion of falsifiability had on the problem of induction (Popper 1976, p. 52).

(3) I now believe this deficiency must be a normal result of the fact that we are human beings, and not computers. But it does mean, I think, that we

[1] I have in mind his speech on "Kant's Critique and Cosmology" at the 150th. anniversary of Kant's death (Popper 1965, pp. 175 ff., now also contained in the introduction to the German translation).

must retain a very critical attitude even towards those of our own views which seem to be the clearest because they may be remnants of old superstitions which we believe to have left behind.

Part I

(4) Einstein's theory of relativity is a striking example for this observation. Most of you will know of Popper's admiration for Einstein, and how he was inspired by the theory of relativity (and by its high refutability in contrast to the irrefutability of psychoanalysis) more than by anything else to develop the criterion of falsifiability as a demarcation between science and metaphysics (Popper 1976, pp. 37–38). And in connection with this criterion he often quoted—and amplified—Einstein's words from *Geometrie und Erfahrung* (Einstein 1921):

> Insofar as the expressions of mathematics refer to reality they are not certain, and insofar as they are certain they do not refer to reality.

(5) Going by this sentence it would appear that there could hardly have been a more perfect understanding on methodology than that between Einstein and Popper. And they even agreed in this, that they both believed the theory of relativity *not* to be the final truth. Einstein spent the last thirty years of his life in search of a general field theory. And from Popper's *Realism and the Aim of Science* (Popper 1983, p. xxviii) it can be seen that he considered the issue between Einstein and Lorentz, which is none other than the issue between relativistic physics and non-relativistic physics, to be still open.

(6) Most amazing about this is that they both held the key to the problem in their hands. Let us take a look at the general theory of relativity and at Einstein's concept of "curved space". It is reputed to be a difficult concept, but Einstein's deduction is simple enough to be understood by everybody. In one of his famous *Gedankenexperimente* he assumes a large box, or lift, being accelerated through space at a constant rate outside any other field of gravitation. He then first discusses the situation of a generation of physicists being born and living in that box, without being able to "look out of the window", and finds that, due to the constant rate of acceleration, they will be under the impression of living in a gravitational field. Permanent acceleration will permanently press them to the floor of the box. He then lets a ray of light travelling horizontally (parallel to the bottom, or x-axis) fall into the box through a hole in its side, and finds that, due to the finite velocity of light, and, again, due to the constant rate of acceleration of the box, this ray, which had been travelling in a straight line outside the box, will assume the shape of a parabolic curve inside the box. From the combination of these two considerations he infers that the scientists living in that box will ascribe the deflection of light to the influence of the gravitational field. And his further inference is that, since we, the human race, are living in the gravitational field of the earth, and the sun, without being able to "look out of the window", we must also assume that the lines of light will be curved in this field. Going by his own explanation, this, and nothing else, lies at the bottom of his famous concept of "curved space".

(7) Now, I fear that some of you may not believe this. The lift-example is taken from the book "Über die spezielle und die allgemeine Relativitätstheorie" in which Einstein tried to explain the theory of relativity to non-physicists in a popular way (Einstein 1917, chapter 20). It seems unfair to hold this attempt at a simple visualisation of a difficult scientific theory against him. But I assure you that I have looked through Einstein's papers very carefully, especially through the early ones, and I have found many complicated discussions of the implications *resulting from* curved space, and also many *adaptations* of the theory to empirical results which surprised me because the experiments in question might well have been taken as refutations of the theory. I have also found the arguments used in the lift-example formulated in a more scientific shape, referring not to lifts, or boxes, but to systems of coordinates, and planets, and ellipses (Einstein 1916). But the gist of the argument always remained the same. And nowhere have I found anything like a *critical* discussion of the fundamental premise that light will be deflected in the gravitational field. The discussion of this premise did not take place in Einstein's papers; nor have I found it anywhere else.

(8) You may still not feel reassured because I am only a lawyer, and admittedly know very little about physics, and even less about mathematics. But perhaps a quotation from Einstein's own text will convince you. In "Die Grundlage der allgemeinen Relativitätstheorie" (Einstein 1916)—and this is not a simplified book, but one of his original publications in *Annalen der Physik*—he explicitly says (the translation is mine, but the words are his):

> For it is possible *'to create'* [he puts this in quotation marks] a gravitational field by merely changing the system of coordinates ... it is easy to see that the way of light relative to K' [K' is the system of coordinates that is being accelerated, i.e. the "lift"] will generally be curved if light moves in a straight line and with constant velocity relative to K.

(9) This, I believe, shows quite clearly that, at that time, he considered the deflection of light as a necessary *implication* of acceleration, resulting from a purely mathematical operation, viz. the "changing of the system of coordinates". To him it was the result of a mathematical equation. In fact he *inferred* this deflection from two very simple premises: (1) the constant velocity of light, and (2) an identification of acceleration with gravitation[2].

(10) Let us now look at this argument with Karl Popper's eyes.

(11) According to Popper's "Logic of Scientific Discovery" Einstein's reasoning is not only *very* strong, but in fact *too strong.* For it is, of course, true that the ray of light falling into the accelerating box will form a parabolic curve. But this is true not only for light, but also for *any* other ray travelling at constant speed, and entering the box at an angle of 90°. The shape of the parabolic curve will, of course, vary depending on the velocity of the ray and the acceleration of the box, but if one of two intersecting systems of coordinates is moving at constant velocity while the other is being accelerated the function of the intersection must necessarily be a parabolic curve. This is not only true for rays of light; it is even true for rays, or lines, that exist only in our imagination, or for a box without top or bottom, and therefore *without gravitational field.* It is independent of any physical properties of those rays, and has nothing to do with gravitation, but simply consists of a geometrical description of two bodies, or systems of coordinates, moving at speeds relative to each other, when one of them is being accelerated and the other is not. It is the result of a *valid* mathematical inference which can therefore *never* be refuted. It is a simple truism of analytical geometry and therefore *belongs to mathematics, but not to physics.* So it falls victim to Einstein's own criterion: because it is certain it does not refer to reality. And if it does not refer to reality, then it does not describe a physical property of light, or of space. And in Popper's terminology the theory of curved space is non-empirical because it cannot be refuted by any conceivable experiment, or physical property of light[3]. Therefore we may *not* infer from this theory that space is curved *in reality*, and that light will be deflected in the gravitational field.

[2]Dr. Elie Zahar has kindly pointed out to me that my description does not take into account the Riemann-Christoffel curvature of space which has nothing to do with the choice of the coordinates. I admit that I am making use of a simplification which is, however, necessary because otherwise my arguments, too, would become unintelligible. The following text will show that, as long as the concept of curved space—in whatever version—is based not on *empirical assumptions*, but on *mathematical inference*, my arguments against it remain the same.

[3]The same argument will apply to *any amendment* of this theory as, for example, by the Riemann-Christoffel curvature of space, as long as it is *purely* mathematical and does not introduce new empirical (falsifiable) information.

(12) I wonder if any of you will accept this. If you do not, I would be eager to hear a counter argument because I know of none. I have discussed this argument with physicists of my acquaintance without being contradicted. I have tried to get a public discussion by publication in serious periodicals of science, but was rejected. I then tried to get support, or contradiction, from experts and sent a paper containing this argument—and other arguments which I consider even more important, but which are not directly related to Karl Popper's theories—to well known physicists in England, France, the U.S.A. and Germany without being deemed worthy of a reply[4]. So I have never been able to put on trial my case *Popper v. Einstein.* Nobody seems to be willing to believe that a practising lawyer can have anything sensible to say on the theory of relativity; nor is anybody willing to discuss the argument on its own merits. All I can do, therefore, is take advantage of a defenseless audience and present my view at the Annual Popper Conference, and via *The Critical Rationalist*, and I am grateful to be able to do so. To me it appears clear that some aspects of Einstein's *general* theory of relativity and Popper's "Logic of Scientific Discovery" contradict each other flatly and cannot, therefore, both be right.

(13) I could end my article at this point because this was what I wanted to say. But you may ask: What about the empirical confirmations? Was not the general theory of relativity almost miraculously confirmed in Eddington's famous experiments, made during the eclipses of 1919 and 1922? Did he not observe that the light coming from distant stars was in fact deflected in the gravitational field of the sun?—And did not Einstein predict that the spectrum of light coming from other stars would be shifted towards the lower frequencies, which has since been confirmed by observation many times, in particular by the American astronomer Edwin Hubble?—Was it not even observed that there is a time shift if we circumnavigate the earth in fast aeroplanes, which was predicted, and can only be explained, by the special theory of relativity?

(14) I have tried to look into all these empirical confirmations. Some of them require more knowledge of mathematics than I have. This does not worry me too much because, as an admirer of Karl Popper, I am more interested in refutations than in confirmations, and to refute an empirical theory

[4] I am happy to be able to correct this statement now because it is no longer true: after writing first drafts of this article, but before reading it at the conference, I received a very kind reply from Dr. Thomas Angelidis, London, who spared a tremendous amount of time to go into the details of my criticism and caused me to correct some serious mistakes in earlier versions of this article. Before the conference time was too short to reach a result in our discussion, but some of the footnotes refer to it. And Dr. Elie Zahar, to whom the paper was sent by a common friend, also made some important—critical—comments. As far as possible I discuss these too in the footnotes (see already footnotes 2, 3).

you do not necessarily need mathematics. It may be sufficient to discuss the premises on which it relies. According to the *Logic of Scientific Discovery* (Popper 1980) one refutation will bring down the theory of relativity, and so many confirmations will not set it up again if this refutation is valid and can be repeated. But as far as my understanding goes I can say that *all* the "confirmations" of the theory of relativity are extremely dubious. I will explain this for the three most important ones, trying to be as brief as possible.

(15) Eddington's experiment is justly famous, not only for its results but also for its magnanimity because, while the First World War was still going on, a team of British scientists meticulously designed an experiment to put to the test the theory of a German scientist. Anti-Semitism was not so important in 1919, and Einstein was still considered to be a German citizen. Eddington *believed* in the theory of relativity and tried to *confirm* it. His experiment was carried out during the eclipse of 1919, and repeated in 1922, and Eddington was able to show that, as Einstein had predicted, the light coming from distant galaxies was in fact deflected, as by a convex lens, in the gravitational field of the sun. It seems that this was the point when all resistance against the theory of relativity which had previously existed ultimately broke down. My impression is that the theory of relativity was never again seriously challenged after that.

(16) But looking into the publications of that time will produce some surprising results. As mentioned, Eddington's experiment was carried out in 1919/1922. But Einstein had already read, in 1915, a paper before the Royal Prussian Academy of Science in which he stated that the deflection of light passing the sun was 1.7 sec. of arc relative to the passing distance from the sun (Einstein 1915). And he *adapted* his general theory of relativity to this value. Therefore the value cannot have been *predicted* by the theory, but must have been known *before*, and independent of it. The theory was designed, and adapted, so as to *explain* this particular value.

(17) *After* the publication of Eddington's experiment Einstein *still* spoke of a deflection of 1.7 sec. of arc. So the outcome of the experiment seems to have been exactly what he expected. But he now explained this value as having been caused "one half by the (Newtonian) attractive field of the sun, the other half by the geometric modification ("curving") of space caused by the sun" (Einstein 1917, p. 84f, my translation of his words).

(18) At least this last explanation can certainly not be valid. For, as I have tried to show before, the concept of "curved space" had been based, by Einstein, *exclusively* on the deflection of light in the gravitational field. If (only) one half of the deflection actually observed can be explained by the curving of space, which had, in turn, been explained by the gravitational field, then the other half cannot be explained by the same gravitational field, this time in the "Newtonian" sense. You cannot arbitrarily "split" the effect of gravitation into one half causing curved space, and the other half causing attraction[5]. This seems to indicate that the deflection actually observed by Eddington must have been much stronger than Einstein's equations would permit[6].

(19) Nevertheless there remains the indisputable fact that the theory of relativity predicts a deflection of light by gravitation and that a deflection was actually observed. The inconsistency in Einstein's explanation does not affect this coincidence; it only puts a question mark behind the alleged correspondence of the exact values of theory and observation. But it gives additional weight to the question whether the theory of relativity can be a *true* explanation of this observation.

(20) Almost equally famous is Einstein's explanation of the redshift of light coming from distant galaxies which can be observed by the shift of the characteristic spectral lines. In special relativity Einstein explains that a clock which is being moved in a straight line will go more slowly than one that is stationary (Einstein 1917, p. 25). In general relativity he infers from this that a clock fastened on the periphery of a rotating disc will permanently work more slowly than one at rest (Einstein 1917, p. 53). And finally he reaches the general conclusion that any oscillating process, as which he regards the atom emitting light, will be decelerated in the vicinity of inert mass[7]. This implies that the spectrum of light coming from stars with great inert mass will be shifted towards the lower frequencies. That was Einstein's explanation of the redshift which had also already been observed when he wrote.

(21) But the American astronomer Edwin Hubble discovered in or before 1937 that the redshift of light coming from distant galaxies is *proportional to their distance* from earth (Hubble 1938, p. 35). According to Einstein's theory the extent of the redshift, being caused by gravitation, would be a function of inert mass. It should therefore vary *in proportion to the inert mass* of the object from which light is coming. So we should expect to observe different redshifts in heavier or less heavy bodies. But the Hubble

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[5]This argument remains valid even after taking into account the Riemann-Christoffel curvature of space (cp. footnotes 2, 3) as long as this relies on purely mathematical inferences, i.e. without introducing new *empirical* hypotheses.

[6]The same appears to have happened with his explanation of the rotation of the long axis of the Mercury-orbit. In (Einstein 1915) he reports that this implied the assumption of a curving of light in the gravitational field *twice* as strong as previously calculated by him. This was *before* Eddington's experiment!

[7]Also in (Einstein 1990, p. 91)

effect—in its common interpretation by the Doppler principle[8]—shows that the extent of the redshift is in fact graduated not in proportion to mass, but to distance. This would only conform with general relativity if we assume the stars, or galaxies, to be arranged precisely *in the order of their size* (mass), which conflicts with observation.

(22) So here we have a serious clash. We can either accept the general theory of relativity or Hubble's explanation of the redshift in the light coming from distant galaxies. And in view of the strong, and repeated, empirical confirmation of Hubble's observation I think we must decide in favour of Hubble.

(23) Another famous experiment is usually discussed in terms of the special theory of relativity[9]. The special theory of relativity rests on two fundamental premises. One is the constant vacuum spreading velocity of light, which Einstein accepted as the result of various experiments, and the other is the equivalence of all inertial systems, which he assumed because we have no reason to believe the laws of physics to be different on other stars from those here on earth. From these two premises Einstein inferred, in faultless mathematical deduction, that, if the velocity of light is constant, time itself must be variable. And he predicted that, therefore, a body in motion will age more slowly than one that is stationary.

(24) To test this theory a most interesting experiment was carried out in October 1971 by two American physicists, Hafele & Keating (1972*b*, 1972*a*). Four caesium beam atomic clocks were flown on regularly scheduled commercial jet flights around the world twice, once eastward and once westward. It was observed that the flying clocks lost time (aged more slowly) during the eastward trip, and gained time (aged faster) during the westward trip relative to the corresponding time recorded by the reference atomic time scale at the U.S. Naval Observatory, MEAN (USNO). Hafele & Keating explained this as a prediction of the theory of relativity in the following words:

> ... consider a view of the (rotating) earth as it would be perceived by *an inertial observer looking down on the North Pole from a*

[8]I doubt the correctness of this interpretation, but this does not affect the validity of the argument against the general theory of relativity since, at any rate, the Hubble effect must be explained *somehow*.

[9]Strictly speaking the experiment discussed here must be considered in terms of the general theory of relativity because it deals with circular movements. But since the authors Hafele & Keating (see footnote 10) apply the special theory "as a first approximation" I am doing the same in this article. The distinction between special and general relativity has no bearing on the arguments used in the following text as long as it is agreed that general relativity will also produce a time difference for the clock experiment. Einstein explicitly said so (Einstein 1917, p. 53).

> *great distance.* A clock that is stationary on the surface at the equator has a speed $R\Omega$ *relative to nonrotating space*, and hence runs slow relative to *hypothetical co-ordinate clocks of this space* in the ratio...

(25) I will not continue this quotation because I think the pivot of criticism is obvious: the clock of comparison actually used in the experiment was *not* in the hands of "an inertial observer looking down on the North Pole from a great distance"; it was at U.S. Naval Observatory, which is, to my knowledge, situated at Richmond, Florida, and therefore close to the equator. So it was itself rotating with the equator. If there is no preferred inertial system, as Einstein claims, then relative to *this clock* the velocity of both flying clocks, eastward and westward, was *equal*, and therefore, if the theory of relativity had been right, the time dilatation should have been equal too[10]. But in fact

[10] At this point I have been accused of misunderstanding the special theory of relativity. Both Dr. Angelidis and Dr. Elie Zahar argued that the speed should be taken "with respect to the same inertial frame" (Zahar's words), and that the east-west asymmetry would then be explained by "the *different* relative speeds involved" (Dr. Angelidis' words). Their argument, I understand, is that the speed of the planes should not be taken relative to USNO where it would be equal, but relative to the axis of the earth, where it must be added to, or subtracted from, $R\Omega$ (the rotation velocity of the equator). I think they did not get my point, and I will therefore try to clarify my view with the following *two arguments*:

1. According to Hafele & Keating "special relativity predicts that a moving standard clock will record less time compared with (real or hypothetical) coordinate clocks distributed at rest in an inertial reference space" (their words). Let us now assume three satellites, S_g, S_h, S_l, each with a clock on board. S_g is in a geostationary position, rotating with the equator and keeping its place relative to USNO and any other location on earth. S_h is in a "heliostationary" position, keeping its position between the sun and the earth (and therefore not rotating with the equator). And S_l is in a "lunarstationary" position, keeping its position between the earth and the moon, and relative to any location on the moon, and therefore rotating around the earth, but not with the equator. Why do Hafele & Keating measure the time difference (only) relative to S_h? Taking S_l would have produced something quite different, and taking S_g would have produced equal relative velocities for both trips. Is there any reason to consider only S_h to be "at rest", and the others not to be at rest? Since no observer was out in space, or on the North Pole, they could just as well have chosen S_g or S_l as their reference clock. Unless, for some reason, they "prefer" S_h. But "preferring" is not permitted by relativity.

2. Another way of demonstrating the incoherence of the Hafele & Keating experiment and the results obtained by relativity can be found in the following *Gedankenexperiment*: let a satellite, having on board a caesium beam clock sending time signals to earth, fly on a course shaping a *very long ellipse*, comparable to the course of Halley's comet relative to the sun, and circumnavigate the earth as its focal point *from east to west*, i.e. against the direction of its rotation. According to special relativity the time dilatation on this satellite should be strongest (the ageing of the clock should be at its minimum) when its velocity is highest. This would be the

the flying clocks lost time during the eastward trip and gained time during the westward trip. So we have another clash, this time between observation and special relativity. We can either believe in Einstein's theory of special relativity or in the observations made by Hafele & Keating. But we cannot believe in both.

(26) And the worst is yet to come: As Hafele & Keating explain, special relativity predicts a time difference for the clock coming back to its starting point after having circumnavigated the earth. But what is a "time difference"? This clearly depends on what meaning we give to the concept of "time".

(27) There have been endless discussions of this concept. Being a firm believer in Popper's methodological nominalism, and in his view that any discussion of "concepts" will inevitably end in "empty verbiage and barren scholasticism"[11], I have no intention of joining them. I will therefore try to cut short any discussion on the "true", or "real" meaning of the concept of "time" by stating explicitly that, in this article, I am *not* discussing the "existence" of time, or of the "reversibility" of natural events connected with the concept of the "arrow of time", or anything of that sort, but will simply use the term "time" in the everyday sense which we employ when we ask somebody: "What time is it?" We then expect an answer expressed in *units* of time, i.e. in terms of "days", "hours", "minutes", "seconds" etc.

(28) The advantage of using the concept of "time" in this everyday sense is that we ourselves can *define* its meaning, for example by *setting clocks*. This is an implication of Popper's methodological nominalism[12]. And if, as a matter of expedience, we define "time" as a *unique* coordinate of singular events, meaning that no set of space coordinates may have more than one time function, then there can never be two different "times" for any singular physical location. Let me explain this by an *example*. We could, quite arbitrarily, define noon time as the point when the sun reaches its zenith at, say, Greenwich. If our plane now starts from Greenwich and returns to Greenwich when the sun is in its zenith there, and the clock on board does not show 12.00 *h* sharp at its arrival then we do not have two different times at Greenwich but, according to our definition, *the clock is wrong*. And we should not blame the sun for this either, nor would it be wise to alter our definition of "time" so that it will now permit two "times" at Greenwich because this would create

point when its course is closest to the earth as its focal point. But the observation made by Hafele & Keating shows that, at this point, when circumnavigating the earth in an east-west direction, the clock on the satellite will in fact be *gaining time* (ageing faster).

[11] Popper's words, (Popper 1966, volume 2, p. 9).

[12] I am referring to chapters 3 & 11 of "The Open Society and its Enemies", (Popper 1966).

an awful muddle. Our problem is, rather, to find out *what happened to the clock*, which is an empirical problem.

(29) But relativity will, even starting from this premise of time defined by the course of the sun, predict a time difference for the plane surrounding the equator[13]. It is, as I understand it, a necessary theoretical implication of relativity that the clock starting and arriving exactly when the sun has reached its zenith at Greenwich will not show 12.00 h sharp at its arrival, but some fractions of a second less, and this although we have *defined* 12.00 h as being the moment when the sun reaches its zenith at Greenwich. So, in spite of our nominalistic definition of time permitting no more than one time function for one location, and assuming only that we can circumnavigate the earth, we can infer from relativity that,

1. time will be 12.00 h when the sun reaches its zenith at Greenwich, and
2. time will *not* be 12.00 h when the sun reaches its zenith at Greenwich.

(30) Assuming that a circumnavigation of the earth is possible, the theory is therefore *contradictory.*

(31) *If this inference is correct, then special relativity must be wrong. And if the inference is not correct it must still be wrong because, if it does not predict a time difference, it cannot account for the time difference observed by Hafele & Keating in their experiment.*

(32) So we are faced with a truly enigmatic situation. And since the mathematical formulae of special relativity are faultless this implies that one of its *empirical* premises must be given up. But special relativity rests on *only two* empirical premises. One of them is the assumption that the laws of nature (physics) are universal; giving up this would be the end of all explanation. And the other is the hypothesis of the constant spreading velocity of light. Therefore it must be this second premise which has to be given up. In other words: Special relativity does not explain, or establish, the relativity of space

[13] This is a necessary implication of any theory involving the Lorentz transformation. For the expression:

$$t' = \frac{t - \frac{v}{c^2}x}{\sqrt{1 - \frac{v^2}{c^2}}}$$

will yield $t' \neq t$ for any $c < \infty, v > 0$. The arguments related to special relativity will therefore remain valid for general relativity as long as general relativity relies on a transfer of the results obtained from an application of the Lorentz transformation in special relativity. This transfer Einstein undertook explicitly in (Einstein 1917, p. 53), when he assumed a time dilatation for a clock mounted on the periphery of a rotating disc.

and time; but *it does refute the empirical hypothesis of the constant spreading velocity of light*[14]. If we can travel around the world and come back to our starting point, then the velocity of light, if it is finite, cannot be constant. And since the constancy of the spreading velocity of light is the *fundamental* premise both of special and of general relativity this all boils down to the thesis:

If the earth is round then the theory of relativity must be wrong.

(33) It also follows from this that the time dilatation observed by Hafele & Keating was not an effect of special or of general relativity, but must have been an effect of something else, at any rate something empirical. My conjecture is that it was an effect of *ether*[15], though of a somewhat different kind than that expected by Michelson/Morley in their famous experiment (1887).

[14] At this point I should mention that the hypothesis of the constant spreading velocity of light is also incompatible with the common interpretation of the Hubble effect as an expression of the Doppler principle. Hubble's interpretation relies on the assumption that the redshift of light from distant galaxies is caused by their (assumed) flight movement which will, according to the Doppler principle, reduce frequency, as we can all observe when we hear the horn of a passing car. But this presupposes that the velocity of the (assumed) flight movement can be *deducted* from the velocity of light coming from distant galaxies. The Doppler principle will never yield a difference of frequency without difference of velocity. But this clashes with Einstein's principle of the constant spreading velocity of light which explicitly *excludes* an addition, or subtraction, to or from the velocity of light. Therefore the common interpretation of the Hubble effect as an expression of the Doppler principle is also incompatible with the theory of relativity.

[15] This is a somewhat rash conclusion which, for time reasons, it was impossible to elaborate when actually reading the paper at the conference, and of which only a sketchy outline can be given here. It is clear that, even if the time difference observed by Hafele & Keating cannot be explained by special relativity, as I have tried to show in the text (par. 22), the refutation of special relativity will not explain it either. We need a new *empirical* hypothesis, and time dilatation can be explained if we assume the earth and its surrounding atmosphere to be a vortex-like rotating field of energy (ether) in which matter is a form of appearance (comparable to a disturbance) of that field. The clock flying eastward is then flying *with* the direction of rotation, the other one *against* it, and all the time the field is "flowing" *through* both clocks, and their time difference is accounted for by the Doppler principle which would otherwise be incompatible with the assumption of a constant spreading velocity of light (see footnote 14). The "time difference" is then, in fact, a difference of *performance*: one of the clocks has gone through more oscillations of the field than the other.

Part II

(34) I am almost certain that no readers of this article will accept this. How can anyone presume to know more about physics than Einstein? But remember that I am not competing with Einstein in questions of physics, or mathematics, but in questions of methodology, and thanks to Karl Popper methodology has made wonderful progress since Einstein's time. You may have noticed that, so far, I did not use one single mathematical formula in this text[16], and yet I have questioned all the premises from which Einstein started. And I am not only relying on my own arguments, but on those supplied by Karl Popper, most of all on his methodological nominalism[17]. So I may be wrong, but then, I think, Karl Popper would also have been wrong. At any rate—since it is impossible to reach a final result on this in one single article—I challenge every reader of this article to disprove my arguments *in writing*. As a practising lawyer I know only too well that oral discussion always has an element of chance in it, and that is why I am longing to be able to publish my views—I have a lot more to say—and get a public discussion, if only the physicists would let me. But this is a "Popperian Journal" and I have been speaking on physics already too long. Let me therefore return to the theory of scientific knowledge.

(35) I consider it as definitely established that Einstein's theory of relativity and Popper's theory of scientific knowledge are incompatible. Einstein's theory of "curved space" is non-empirical, and the empirical parts of his theory concerning time and gravitation have been empirically refuted. If I am right, then physical science now has the task of re-interpreting a tremendous number of experiments, and of finding new solutions for several important problems, most of all the problem of gravitation which has never been solved. The theory of the "Big Bang" and the theory of the "Black Holes" will both break down, and we may find that we know even less about our universe than we thought we did. What physics desperately needs, and apparently totally lacks at the moment, is a critical discussion of the premises on which the various theories rely which are at present happily co-existing despite all their contradictions. And to permit this discussion it seems that physics will first have to open its mind (and its institutions) to the possibility of error, and, instead of getting lost in mathematical formulae, find the courage to put forward new and daring ideas. And for the pupils and admirers of Karl Popper there remains the question why he himself did not see these implications—if they are as I see them.

[16]The formula in footnote 13 is needed only to make the contradiction clearer for adherents of relativity; my criticism of the theory of relativity could do without it.

[17]Here again I am referring to chapters 3 & 11 of *The Open Society and its Enemies*, (Popper 1966).

(36) The answer, I believe, has been given by Lessing: "Not all are free who mock at their own fetters." I think Karl Popper was so impressed by Einstein's boldly stating the exact conditions under which his theory would break down, and by the contrast in which this stood to the dogmatism of psychoanalysis, and so overwhelmed by the outcome of Eddington's experiment, that he never extended his own criticism to Einstein himself. He was a boy of thirteen when Einstein published the general theory of relativity, and barely seventeen years old when he heard Einstein's lecture in Vienna just after Eddington's first experiment. At that time he had not yet developed the criterion of falsifiability as a demarcation between science and metaphysics, but Eddington's experiment gave the impulse from which it was eventually to emerge. It would have been almost superhuman for Popper to apply his criticism to the very theory which had inspired his own discovery. In later years he was critical of the theory of relativity in the sense that he did not consider it as the final truth, as was Einstein himself, who spent the last thirty years of his life in search of a better theory, and once wrote that:

> ... the most beautiful lot of a scientific theory is if it shows the way to a more comprehensive theory in which it continues to live as a borderline case.

(37) But I think Popper simply never felt the necessity to call the theory of relativity itself into question. He said that it took him years to discover the bearing which his demarcation criterion of falsifiability had on the problem of induction. In the case of the theory of relativity, I think, he continued to live with an inconsistency without being aware of it.

(38) But Einstein, I believe, was less of a *critical* rationalist than Popper thought. He did say:

> Insofar as the expressions of mathematics refer to reality they are not certain, and insofar as they are certain they do not refer to reality.

So he held the instrument of criticism in his hands. But the superstition in which he had been brought up had not lost its power over him. The most important "confirmations" of his theory were, in fact, observations already known before, or at least expected at, the time he wrote. The redshift of light from distant galaxies, the deflection of light in the gravitational field, and the perihelion-rotation of the course of Mercury, were all known, or at least surmised, before 1914, as can easily be seen from the fact that Einstein himself discussed them in his early papers (Einstein 1916, Einstein 1915).

They were the facts from which he *started*, and to which he *adapted* his theory by logical or mathematical inference. I am almost sure that if he had been asked he would have said that he was working by *induction* because he said so almost explicitly[18]. The power of his theory lay in the fact that it was the first to offer one solution for a whole set of different problems hitherto unconnected. But Einstein did not know what we have since learned from Karl Popper, that falsifiability of a theory is no vice, but a virtue. So even after he had published the general theory of relativity he kept adapting it to whatever new observations came his way, including Hubble's discovery (Einstein 1990, pp. 116ff.). Thus he could *never* be refuted because he would always change his theory. For a pupil of Karl Popper, Einstein's early publications on the theory of relativity are indeed quite astonishing to read because he was permanently mending his theory in view of criticism and new discoveries. But he never stooped to discussing the compatibility of his premises with Hubble's new observations. And the very fact that, in the above quotation, he spoke of the "expressions of mathematics" with respect to reality shows that he did not distinguish between mathematics and physics as sharply as Bertrand Russell, Alfred Tarski and Karl Popper have since taught us to do. Today we know, in Russell's famous words, that:

> ... pure mathematics is the subject matter in which we do not know what we are talking about, or whether what we are saying is true.

(39) But we should not forget that it was a difficult process to reach this point of knowledge, and that some of the most eminent scientists were involved in reaching it. Even today, and in spite of their teaching, the view is widely spread that the rules of logic, and the calculi of arithmetic, are "laws of thought". So we should learn to be still more critical of our own views and theories because even the best knowledge we have, or believe to have, will not automatically put right all those mistakes on which it has a bearing. Our brain may be superior to any computer because it can create new and revolutionary ideas. But it is also inferior to any computer because it may overlook inconsistencies which no computer would tolerate. If men like Einstein and Popper can fail then anyone can fail. That is why we should extend our criticism to everyone, to Einstein, and even to Popper, *and most of all*

[18] There are many examples in his texts. In (Einstein 1917, p. 13) he says: "This is where the theory of relativity sets in. By an analysis of the physical concepts of "time" and "space" [he did not put quotation marks] it appeared that, *in reality, there is no incompatibility of the relativity principle with the law of spreading velocity of light*, but that systematic application of both these laws will yield a logically faultless theory"—(his italics; my translation). I think this shows quite clearly that he *thought* he was working exclusively by logical inference.

to ourselves. And the best service we can do to the memory of Karl Popper, as *I* understood his intentions, is by keeping in mind his words that:

> … there are experts, but no authorities,

not even Einstein or Popper. Let us begin a new period of enlightenment by daring to use our *own* intelligence: *Sapere aude!* (in this context I should probably be saying "*Sapere audiemus*", but I am quoting Kant). Let us use logic and mathematics where they can be helpful, but let us also beware of being carried away by their beauty. We must keep criticizing the premises of our theories, and for this we need logic and mathematics. But beyond that we should leave them to the computers wherever possible, and use our wonderfully creative human intelligence for developing new and daring ideas—*and take the risk of being wrong!*

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Author Contact Information

Christoph von Mettenheim

Altes Badhaus,
D-75203 Stein,

Germany.

Telephone: +49-7232-4576
FAX: +49-721-495723
Web: http://www.eeng.dcu.ie/~tkpw/people/vonmett.html

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