A POPPERIAN EVALUATION OF EINSTEIN'S THEORY-PLUS-METHOD

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Este artigo apresenta uma análise de vários testes experimentais das teorias de Einstein, descrevendo sua avaliação popperiana e discutindo a reação de Einstein a esses testes. Mostra-se que várias refutações relevantes das teorias de Einstein não foram aceitas por Einstein como significativas, e que portanto Einstein não seguiu as regras metodológicas de Popper. Isto é considerado um forte argumento contra o critério de demarcação de Popper.

This paper presents an analysis of several experimental tests of Einstein's theories, together with their Popperian evaluation and a discussion of Einstein's reaction to these tests. It is shown that several relevant refutations of Einstein's theories were not accepted by Einstein as significant, and that therefore Einstein did not follow Popper's methodological rules. This is regarded as a strong case against Popper's criterion of demarcation.

1. *Introduction*

This work is an attempt to apply Popper's criterion of demarcation to some episodes in the history of physics. The choice of Einstein's work as the subject of this epistemological analysis may be justified in the face of Popper's special respect – and perhaps reverence – for this scientist. The aim of this exercise is not exactly to criticize Einstein's work, but to challenge Popper's views by exhibiting its contrast with the scientific practice of a great physicist[1].

The structure of this paper is the following: I first discuss whether and how Popper's views on science can be rationally criticized, according to his own suggestions, and how a historical analysis (and particularly the study of Einstein's work) can provide arguments for or against these ideas (Section 2); then, after a brief description of Popper's criterion of demarcation and methodological rules concerning the test of scientific theories (Section 3), I discuss the history of one fundamental test of general relativity – the gravitational red-shift effect (Section 4); after this first case-study I propose a generalization of Einstein's scientific behaviour (Section 5) and corroborate it by means of the study of further historical evidence (Sections

[1] In a former paper (Martins, 1981) I have similary contrasted Einstein's practice with Bridgman's operational point of view, showing that they are not compatible.

6 and 7). It is shown that Einstein's scientific behaviour should be classified as non-scientific according to Popper's criterion, and the relevance of this result as regards the acceptance or rejection of Popper's criterion of demarcation is discussed (Section 8).

2. *Popper and Einstein*

The most famous of Popper's contributions to philosophy is his criterion of demarcation between science and non-science. The original aim of this criterion was to provide a clear epistemological distinction between the empirical sciences such as physics, and pseudo-sciences such as astrology, psychoanalysis and marxism[2]. In *The Logic of Scientific Discovery*[3] Popper describes his refutationist concept of science and contrasts it with the neopositivistic inductive view of science – which he holds to be useless and contradictory. Although in later works Popper modified his views, we may say that the core of Popper's epistemology can be found in the *Logic*, and it is this early presentation of the refutationist criterion that will be used throughout this paper.

Popper's work is certainly one of the most important contributions of the 20th century to epistemology, and deserves to be studied and understood by scientists and philosophers. But it is not sufficient to study it. Besides, it is necessary to evaluate it and decide whether we should accept and use it or not. After all, Popper himself states that his criterion of demarcation must be regarded as "a proposal for an agreement or convention" (*Logic*, §4). How can we criticize him? Many authors discuss the inner consistency of Popper's ideas, or try to contrast them with different points of view. I shall here try to provide a critical discussion of Popper's criterion of demarcation using his own point of view, and bring to light a few historical facts that he seemingly did not know, but which maybe could shake his confidence in his own system. In order to start our analysis, let us study how Popper was led to his criterion.

Popper's choice of his criterion of demarcation was guided by "value judgements and predilections" (*Logic*, §4), and he knew that acceptance or rejection of his epistemology would also depend on value issues, being therefore, he thought, beyond rational argument (*Logic*, §4). Someone might be unable to accept even Popper's most basic points if he has a different general idea about the aims of science. One may, for instance, want to provide another definition of scientific knowledge that includes marxism, but not astrology; or a criterion that stresses utility over all other aspects; or one might even deny the possibility of any clear demarcation between science and non-science. From any of these and several dif-

[2] See Karl Popper's *Autobiography* (Popper, 1974a), p. 27-29.

[3] Popper (1961). Throughout this paper, Popper's *The Logic of Scientific Discovery* is always referred to in the text simply as *Logic*. All references to this book are to section numbers instead of pages, in order to make it easier to compare with different editions of the *Logic*.

ferent points of view it may be easy to reject Popper's criterion of demarcation at once. But let us not follow this kind of external approach. Let us try to follow Popper's path, and accept his own battlefield and his choice of weapons.

If we agree with Popper's general outlook, how can we decide whether his epistemology is acceptable or not? According to Popper, we must analyze the logical consequences of his proposal, and test its power to elucidate the problems of the theory of knowledge (*Logic*, §4). Which problem are these? When Popper first formulated his criterion of demarcation, his main concern was to understand the growth of scientific knowledge and to explain how experience can contribute to this evolution[4]. There was, of course, a proposed answer: scientific knowledge grows by inductive procedures. But Popper was not satisfied with this answer and tried to give a new interpretation to scientific evolution. According to Popper, experience does not allow us to *produce* new laws or theories, but it does allow us to *refute* previous laws and theories. And it is through successive refutations that science grows and changes. After Popper framed this general idea, he chose to call science only those systems that admitted refutation when confronted with experience. And he thought that this could provide a nice demarcation between what he thought was good science (such as physics) and what he deemed unscientific.

But how can we know whether Popper's epistemology does provide a fair solution for his epistemological problems? We must have some standard at hand; and it is not difficult to find out the standards used by Popper.

Popper believed that there is a steady scientific evolution, and that scientists somehow know how to contribute to the growth of knowledge. For this reason he suggested that we should study the historical reports about the discussions concerning the acceptance or rejection of scientific theories, such as Newton's or Maxwell's or Einstein's[5]. By observing how scientists decide to accept or to reject scientific theories, it would be possible to learn about the method of science.

But how can the history of science be relevant to epistemology? We must distinguish between what scientists *do* and what they *ought to do*. The philosopher's job is not to describe what scientists do, but what they ought to do – and an *ought* cannot be derived from an *is*. Hence we certainly cannot derive an epistemology from the history of science[6]. Nevertheless, if we have some available historical episodes in the evolution of science that we accept as paradigmatic – that is, exemplary – we may use these instances as a test of some proposed epistemology. If this epistemology cannot account for the success of these exemplary episodes, or if it even

[4] Popper's *Logic* (1961), Preface to the English edition (1958).

[5] Popper (1961), Preface to the English edition.

[6] See, for instance, Hanson (1962). See also Martins (1984).

prohibits the methods used in paradigmatic scientific cases, then this epistemology is not satisfactory.

Popper himself tells us that this is his ultimate criterion:

> "The great scientists, such as Galileo, Kepler, Newton, Einstein and Bohr (to confine myself to a few of the dead) represent to me a simple but impressive view of science. Obviously, no such list, however much extended, would *define* scientist or science *in extenso*. But it suggests for me an oversimplification, one from which we can, I think, learn a lot. It is the working of great scientists which I have in my mind as my paradigm for science."
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> "... This, then, for me is science. I do not try to define it, for very good reasons. I only wish to draw a simple picture of the kind of men I have in mind, and of their activities. And the picture will be an oversimplification: these are men of bold ideas, but highly critical of their own ideas; they try to find whether their ideas are right by trying first to find whether they are not perhaps wrong. They work with bold conjectures and severe attempts at refuting their own conjectures.
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> "My criterion of demarcation between science and nonscience is a simple logical analysis of this picture. How good or bad it is will be shown by its fertility"[7].

Popper accepts that a set of "great scientists" represents the best of scientific activity, and they provide his standard of good scientific practice. A sound epistemology must agree and account for the practice of these paradigmatic men of science. And as Popper states, his own criterion of demarcation was born from his concern with these exemplary scientists.

The study of Popper's *Autobiography* shows that it was one particular "great scientist" who was first taken as his standard and from whose practice Popper's criterion of demarcation was born. This model scientist was Einstein. Popper states that it was in 1919 that he learned about Einstein; "and this became a dominant influence on my thinking – in the long run perhaps the most important influence of all"[8]. In that year (1919) all the educated world learned about Einstein, since this was the year of the famous eclipse observations that confirmed Einstein's prediction of the bending of light rays passing near the sun[9]. Popper took at once a great interest in Einstein's work. And he soon found something remarkable in one of Einstein's books:

> "... What impressed me most was Einstein's own clear statement that he would regard his theory as untenable if it should fail in certain tests. Thus he wrote, for example: 'If the redshift of spectral lines due to the gravitational potential should not exist, then the general theory of relativity will be untenable"[10].

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[7] Popper's "Replies to my critics" (1974b), p. 978.

[8] Popper (1974a), p. 28.

[9] Eddington (1919).

[10] Popper (1974a), p. 29.

Popper admired in Einstein an attitude that he felt was "the true scientific attitude"[11], and this led him to the basic idea of his criterion of demarcation:

> "Thus I arrived, by the end of 1919, at the conclusion that the scientific attitude was the critical attitude, which did not look for verifications but for crucial tests; tests which could *refute* the theory tested, though they could never establish it"[12].

At another place Popper repeats that he was most impressed by Einstein's attitude because Einstein declared that the tests of the predictions from his theory were crucial: "if they did not agree with his precise theoretical calculations, he would regard his theory as refuted"[13]. At this very page Popper states that "it was this theory [Einstein's] which led me into the philosophy of science"[14].

The above quotations are taken from relatively recent writings, and it sometimes occurs that a retrospective description distorts the past. But in this case we may safely believe that Einstein's influence was really a decisive one. In Appendix *1 of the *Logic* – originally a paper published in *Erkenntnis* in 1933 – Popper expresses his view on demarcation as a generalization of Einstein's ideas:

> "Varying and generalizing a well-known remark of Einstein's, one might therefore characterize the empirical sciences as follows: In so far as a scientific statement speaks about reality, it must be falsifiable: and in so far as it is not falsifiable, it does not speak about reality"[15].

This was Popper's first published description of his criterion of demarcation, and we see that it was indeed deeply linked to Einstein's conceptions.

Einstein not only provided the first hint towards Popper's criterion of demarcation but remained forever the ultimate standard of a good scientist for Popper. This may be easily felt if we glance at the large number of references to Einstein's work in all books by Popper. Still more than this: Popper seems to believe that Einstein stands at the very top of the biological evolutionary sequence: "... the method of (dogmatic) trial and (critical) error elimination, ... was the mode of discovery of all organisms from the

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[11] Popper (1974a), p. 29.

[12] Popper (1974a), p. 29.

[13] Popper (1974b), p. 979.

[14] Popper (1974b), p. 979.

[15] *Logic* (1961), Appendix *i, p. 314.

amoeba to Einstein"[16]. "This is why I say that we (from the amoeba to Einstein) learn from our mistakes"[17]. Popper believes that all organisms – and particulary "we", men – may be included in a series that begins with the amoeba and ends in Einstein. That is: nobody (and no animal) surpasses Einstein.

But let us not discuss Einstein's divinization. The relevant point is this: Popper accepts that there are some model scientists; that their actual methods are the best methods for increasing our scientific knowledge; and that a sound epistemology must conform to their paradigmatic scientific procedures. This suggests one possible way for discussing the acceptability of Popper's criterion of demarcation – a way that Popper himself would seemingly accept as valid: we may re-examine the actual methods of paradigmatic scientists and compare the consequences of Popper's criterion to this relevant set of historical data. Let me propose the following test for Popper's criterion of demarcation: if the historical detailed analysis of paradigmatic scientists (and specially Einstein's) shows that they regularly take scientific decisions in a way incompatible with Popper's methodological rules, we shall agree that Popper's epistemology is unsound.

I think that Popper would agree with this kind of criticism, since in a reply to Lakatos he says:

> "... In my lectures, when discussing this matter, I usually asserted that I would be willing to give up my theory of scientific method if certain conditions were to be realized.
> "Surprisingly, Professor Lakatos asks 'Under what conditions would you give up your demarcation criterion?'... as if I had never dealt with this question. But here I can give a new reply. I shall give up my theory if Professor Lakatos succeeds in showing that Newton's theory is no more falsifiable by 'observable states of affairs' than is Freud's "[18].

Therefore Popper accepts some analysis of the history of science as relevant and able to decide whether his proposal should be given up. Unfortunately, it is not easy to study the very instance proposed by Popper himself, since Newton has long been dead, and documentation concerning his attitudes is not good enough to compare with Freud's attitude. But we do have a fairly good documentation about Einstein, and in this case I think that it is possible to provide relevant and testable historical generalizations.

It might seem silly to look for a refutation of Popper's ideas exactly where he found their support. Popper was certainly well acquainted with Einstein's books, and he frequently quotes Einstein's opinions in support of

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[16] Popper (1974a), p. 41.

[17] Popper (1974b), p. 1147.

[18] Popper (1974b), p. 1010.

his criterion. But Popper was not a historian of science, and he had an excessive confidence in Einstein's own description of his methods and attitudes. It is now well known that Einstein's ideas were not coherent, and that by quoting Einstein it is possible to defend any epistemological doctrine that we may choose[19]. Hence we should not take for granted that Einstein's attitude was exactly what Popper said it was or even what Einstein said it was. Incidentally I may quote Einstein in support of my approach in this paper:

> "If you wish to learn from the theoretical physicist anything about the methods which he uses, I would give you the following piece of advice: Don't listen to his words, examine his achievements"[20].

In order not to be misunderstood, let me stress that in this paper I shall present historical data unknown to Popper; I do not simply re-interpret the episodes that were studied by Popper. And these new historical facts, it seems to me, will exhibit the incongruence between Popper's epistemology and Einstein's practice.

3. *Popper's criterion and methodology*

Although Popper's criterion of demarcation is well known, I must briefly describe it here, for the sake of completeness.

Popper's criterion of demarcation between science and non-science allows us to distinguish between scientific and non-scientific sets of theory-plus-method (it cannot be applied to a bare theory) (*Logic*, § 20). A theory-plus-method is scientific only if the *theory* is empirical (or falsifiable); and a theory is falsifiable if and only if the class of its potential falsifiers is not empty (*Logic*, § 22). A theory-plus-method is scientific only if the *method* forbids conventionalist strategies that would make refutation impossible (*Logic*, § 6, § 20).

According to Popper's views, given an empirical theory, scientists should try to submit it to empirical tests. From the theory and initial conditions they must derive what *ought not to occur*; the statements describing what observable kind of occurence is impossible according to the theory (plus initial conditions) are the potential falsifiers of the theory. These potential falsifiers must be law-like, that is, general (universal) statements. Then, some potential falsifier is assumed as a hypothesis and submitted to an empirical test. If the potential falsifier is refuted, then the

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[19] "... everyone – from the extreme positivist to the critical realist – can find some part of Einstein's work to nail to his mast as a battle flag against the others" (Holton, 1960, p. 632).

[20] Einstein (1934).

theory has been corroborated. If the potential falsifier is tested and is not refuted, it is corroborated, and the theory is refuted.

Sometimes a corroboration of a potential falsifier need not be regarded as a refutation of the theory. It is valid to use "ad hoc" auxiliary hypotheses *if they increase the testability of the system and lead to new testable predictions* (*Logic*, § 20). It is also valid to reject the result of experiments if they conflict with other experiments: the empirical refutation of a theory must present a *reproducible effect* incompatible with the theory, and not just a single or a heap of disjointed observations (*Logic*, § 8, § 22, § 30).

Although the acceptance or rejection of theories is time-dependent, Popper states that in general refutation is irreversible:

> "... In general we regard an inter-subjectively testable falsification as final (provided it is well tested)... A corroborative appraisal made at a later date – that is, an appraisal made after new basic statements have been added to those already accepted – can replace a positive degree of corroboration by a negative one, but not *vice-versa*". (*Logic*, § 82).

On the other side, if a theory is tested and is not refuted, then it is corroborated, and this should increase our confidence in the theory. It is not important to predict a *possible* outcome and to confirm it; rather, the only relevant empirical research is the search for effects that are *forbidden* by a theory.

Notice that in this refutationist view of science no accumulation of corroborations of a theory allows us to disregard the power of a refutation. Previous successes of a theory do not make it immune to refutation by new experiments. Also, empirical results that were not produced in attempts to *refute* the theory cannot be regarded as "confirmations" of this theory. They should not increase our confidence in the theory.

This is, briefly stated, Popper's description of a scientific methodology compatible with his epistemological views. According to Popper's views, even if a theory is empirical, if the persons who use it do not follow these methodological rules, the theory-plus-method is not scientific. We cannot evaluate, for instance, the epistemological status of the theory of relativity as a set of theoretical statements: it is only possible to classify as scientific or non-scientific some specific *use* of this theory.

Popper's methodological rules are not intended to describe how scientists *behave*. They are not testable historical laws. If a person does not follow this rule, the rule is not refuted; but the rule then allows us to conclude that the theory-plus-method of this person is not scientific, according to Popper's criterion.

4. *The gravitational red-shift*

Let us now turn to the analysis of Einstein's relativistic theory-plus-method. We have seen that Popper was strongly impressed by Einstein's statement, published in 1918: "If the red-shift of spectral lines due to the gravitational potential should not exist, then the general theory of relativity will be untenable"[21].This seems to accord with Popper's methodology. Einstein proposed a new theory, and with the aid of initial conditions he was able to compute an effect: the thin dark lines (Fraunhofer's lines) in the spectrum of the Sun's light might be displaced by about 0.01 Å to the red side of the spectrum, when compared to a similar spectrum produced on the Earth. Now, what kind of observable effect would be incompatible with the theory? If, within observational errors, the displacement of the lines is observed to be ***regularly different*** from the prediction (if the displacement is to the blue side, or if it is null, or if it is ten times greater or smaller than the prediction, and so on); and if there is no ***testable corroborated*** auxiliary hypothesis that could account for the discrepancy; then the theory must be described as refuted. This is what we derive from Popper's norms. How did Einstein behave?

Einstein's prediction was published in 1911[22]. In his paper Einstein deduces the theory of the effect, uses known data to compute its numerical value, and describes a few previous experimental results by Evershed that seemingly exhibited a slight red-shift of the lines in the solar spectrum. These data were not produced as a test of Einstein's theory, and therefore, according to Popper, they do not corroborate Einstein's theory, and should not be used as positive evidence by anyone. Nevertheless, Einstein used it, as scientists usually do.

Let us see what happened after 1911. Several astronomers tried to detect Einstein's effect. They soon noticed that there was a completely irregular shift of most lines in the solar spectrum: some lines were shifted to the red side, others were shifted to the blue side, and a few were not shifted; the displacements were sometimes numerically greater, sometimes equal and sometimes smaller than the value predicted by Einstein. It soon became clear that there were many superposed influences at play[23].

After these preliminary observations, according to Popper's methodology, Einstein's theory would be described as corroborated, since it was submitted to empirical test and was not refuted (no ***regular*** effect incompatible with Einstein's theory was found). Actually, the confidence of physicists in Einstein's prediction decreased after these observations, because instead of looking for something forbidden by the theory, they were really trying to observe the predicted effect – and there was a great disappointement because a regular effect could not be found.

The next important step was to devise some way of selecting suitable spectral lines such that only the gravitational effect would show itself.

[21] Einstein (1918), translated in Einstein (1920), p. 132.

[22] Einstein (1911). A former prediction was published by Einstein four years before (Einstein, 1907), but it did not call much attention.

[23] Freundlich (1914).

The most widely accepted auxiliary hypothesis used to account for the observed irregularities in the shift of the lines was that the spectrum of several elements was changed by pressure. Since pressure conditions on the Sun may be widely different from those on the Earth, the gravitational effect cannot be unambiguously tested. But this auxiliary hypothesis was testable itself, and it was possible to use the available knowledge about the Sun and about several well studied spectra to choose for observation some kind of element or molecule that should not exhibit pressure effects. The spectra of metals, for instance, seemed unsuitable; but there were some good alternatives.

Using the available knowledge, in 1917, St. John[24] chose to observe the spectral lines around the wavelength 3883 Å corresponding to cyanogen, since current knowledge suggested that there should be no pressure effect on these lines. The predicted relativistic effect was a wavelength change of 0.008 Å. For a set of 25 sharp lines the observed effect was always 0.000 Å; for a set of 18 not so sharp lines the results were somehow irregular, varying from 0.0013 to 0.0037 Å, but always much smaller than the predicted shift of 0.008 Å. Other spectral lines studied by St. John also lead to results contrary to the relativistic prediction. The astronomer concludes:

> "The general conclusion from the investigation is that within the limits of error the measurements show no evidence of an effect of the order deduced from the equivalence relativity principle"[25].

Therefore, in 1917, St. John's observations were against Einstein's theory. Was the theory refuted?

According to Popper's rules, yes. St. John's results exhibited an empirical regularity; they were distinctly different from Einstein's prediction; and the "ad hoc" auxiliary hypothesis concerning pressure effect could not be used for the studied lines.

But one year *after* the publication of St. John's paper – that had a strong repercussion – Einstein could still write the sentence that was to generate Popper's epistemology: "If the red-shift of spectral lines due to the gravitational potential should not exist, then the general theory of relativity will be untenable." Einstein, in 1918, still believed that his theory had not been refuted.

The observational work did not stop after St. John's paper. The situation in 1920 (one year after Popper's conversion to refutationism) was clearly described by Eddington – the greatest apostle of relativity in England at that time: "The expected shift of the spectral lines on the sun, compared with the corresponding terrestrial lines, has been looked for; but it has not been found. . . The chief investigators St. John,

[24] St. John (1917).

[25] St. John (1917).

Schwarzschild, Evershed, and Grebe and Bachen, seem to be agreed that the observed displacement is at any rate less than that predicted by the theory"[26].

Although the observed values did not agree with the prediction, they were somehow irregular, and the meaning of these fluctuations was not completely clear. Eddington remarked that the best set of observations seemed to him to be St. John's study of the lines of cyanogen. These lines exhibited no irregularity, and the most dependable set of these lines showed a shift of exactly 0.000 Å. Eddington did not conclude that the theory had been refuted; but he was careful enough to discuss in the following pages how the theory should be changed if the gravitational red-shift did not exist.

In the next year there appeared in *Nature* a series of papers on the theoretical and experimental aspects of relativity. St. John was invited to describe the state-of-the-art concerning the gravitational red-shift of spectral lines. He reported that there was still no agreement between observation and theory[27]. St. John's observations on the cyanogen lines, to which Eddington ascribed a high value, had been questioned in the meanwhile, and new "ad hoc" auxiliary hypotheses were proposed to dismiss these observations as inconclusive. But recent astronomical observations by Adams, Grebe, Bachen and St. John refuted one astronomical auxiliary hypothesis, while laboratory work refuted another hypothesis. So, the cyanogen results had been corroborated by new tests, and St. John could say that "in respect to the observations at Mount Wilson on the lines of the cyanogen band at wavelength 3883, I have as yet found no grounds for considering them seriously in error"[28]. Since the falsifying hypothesis had been tested and corroborated, the theory of relativity should be regarded as refuted.

As there were at this time strong doubts concerning the very existence of the gravitational red-shift, theoreticians began to discuss whether the theory of relativity did really imply this effect. There was one simple way open: it was possible to re-interpret the relationship between the observed wavelengths and the theoretical wavelengths. This had been suggested by Eddington, and was soon accepted by several relativists. As St. John pointed out in 1921, the observations of the solar red-shift could eventually be used to adjust the theory one way or the other, instead of refuting or corroborating it[29]. Therefore theoreticians were now using one of the four conventionalist stratagems, described by Popper (*Logic*, §19) as invalid means of protecting a theory against refutation: to change the meaning of observational terms.

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[26] Eddington (1920), p. 129-130.

[27] St. John (1921).

[28] St. John (1921).

[29] St. John (1921).

How was Einstein behaving at this time? In 1921 he was very happy, receiving his Nobel Prize and delivering a talk on the theory of relativity where he does not mention the difficulties met by his prediction of the gravitational red-shift[30]. The eclipse test had turned up for his theory, and that was enough for him. He never doubted that the red-shift observations would finally agree with his theory[31], and for this reason he was not very disturbed by the negative results.

What would Popper say about this? "We shall not continue to accord a positive degree of corroboration to a theory which has been falsified by an intersubjectively testable experiment based upon a falsifying hypothesis" (*Logic*, §82). Physicists should have turned to the study of alternative theories, since general relativity had been refuted. But Einstein and the relativists did not follow these rules.

After a few years of negative attempts, astronomers were at last able to find the searched for gravitational red-shift in the Sun, and later in other stars[32]. It seems that the eclipse results were convincing enough to make astronomers willingly find an agreement between observations and prediction of the red-shift[33]. Once positive instances of Einstein's prediction were found, the negative outcomes were forgotten; they are not discussed in modern textbooks[34]. This shows that the scientific community afforded a greater value to the positive instances than to the negative instances. This clashes with Popper's rules that tell us to give more value to refutations.

Was Popper ever aware of the true history of the gravitational red-shift? Probably not. He states that the *first* test of Einstein's general relativity was the eclipse observation of 1919[35]. This was indeed the first *successful* test; but the red-shift search was certainly earlier than this.

5. *A testable hypothesis concerning Einstein's attitude*

In the red-shift episode it is clear that Einstein and the scientific community did not follow Popper's rules. What would Popper say if someone told him about this actual history? He would perhaps be sad and shocked at

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30 Einstein (1921).

31 Einstein wrote on May 28, 1921, to Besso: "The displacement of the spectral lines towards the red has now been verified... at no moment did I doubt the fact that it would turn out this way" (Speziali, 1972, p. 163-4). On this particular point, and also on the whole subject of the red-shift test, one way consult the wonderful paper by Earman and Glymour (1980). I only came across their article when mine had already been written, and hence I could not use it. Earman and Glymour's detailed description agrees in all basic points with my own analysis.

32 Since there was much confusion around the study of solar lines, the independent study of the relativistic red-shift in the spectrum of a dwarf star (Adams, 1925) was a much stronger argument for Einstein's prediction.

33 See Earman and Glymour (1980).

34 See, for instance, Tonnelat (1964).

35 Popper (1974b), p. 980.

Einstein's failure to follow a strict refutationist method; but this would not entail a refutation of Popper's ideas. Even an Einstein may sometimes misbehave. Popper could defend himself in the following way: in the empirical sciences sometimes an experimental anomaly exhibits results incompatible with some law or theory, but irregular effects are not a sufficient refutation of a law (*Logic*, § 8); analogously Einstein's misbehaviour need not be regarded as sufficient evidence against the accordance of the behaviour of paradigmatic scientists with Popper's methodology. Only if Einstein's (or other paradigmatic scientists') behaviour is regularly opposed to Popper's rules shall we take this as sufficiently strong evidence against Popper's programme.

Let me therefore propose the following generalization of Einstein's behaviour described above: "Whenever some experimental test led to results in disaccord with Einstein's theories, he always maintained his belief that instead of being a refutation of his theories these results were due to perturbing influences, or lack of experimental skill, etc., and that future experimental work would some day agree with his predictions". Now, this is a testable historical general statement. If it is accepted, then we shall be led to the conclusion that Popper's methodology is in disagreement with the systematic practice of his most admired and respected exemplary scientist.

Now, I do seriously propose that Einstein never accepted as a refutation of his theories the negative outcomes of careful experiments. I cannot prove this general statement, of course. But, using Popper's methodology, I shall provide the instructions for testing this statement.

First, look into the scientific literature for tests of Einstein's theories (such as special and general relativity)[36]; second, select from the list those tests that were described by their authors as yielding a result against Einstein's theories; third, study these tests and the reaction of the scientific community relatively to the experiments, and see whether these results were soon dismissed by (Popperian) allowed methodological procedures (repetition of the experiments, use of testable auxiliary hypotheses plus attempts to test these new hypotheses – *Logic*, § 20); fourth, select those tests that were not dismissed at once by the scientific community using Popperian valid methodological procedures: let us call these the "relevant refutations of Einstein's theories"; fifth, search Einstein's writings and other sources and try to find out Einstein's early reaction to these tests. If the result of this search shows that Einstein did regularly accept that his theories had been refuted by the relevant refutations of Einstein's theories, then I shall accept that my historical generalization is wrong.

[36] To "look into the scientific literature" does not mean to consult the traditional text-book accounts, but to use the available bibliographical instruments such as *Physics Abstracts, physicalische Berichte, Bulletin sygnaletique*, etc., in order to unearth the many experimental tests willingly forgotten by the scientific community.

Notice that it is irrelevant whether the relevant refutations of Einstein's theories have after five or ten or fifty years been superseded by new experimental work, and shown to be wrong. In the framework of Popper's methodology a refutation must be accepted as a refutation until new knowledge allows us to dismiss it; anyone might coherently maintain a belief in the future refutation of the refutations, but this is not in accord with Popper's rules.

6. *The experiments of Fizeau and Kaufmann*

The earliest experimental evidence against Einstein's theory of relativity was produced much *before* Einstein was born. In 1859, Fizeau devised a method for measuring a change in the index of refraction of glass due to the motion of the Earth through the ether[37]. He tested the method and observed an effect due to the motion of the Earth, in good agreement with his predictions. The work was presented to the French Academy of Sciences, published at another place in full version, and soon translated into German, due to its importance[38]. There soon appeared a paper by the astronomer Faye[39], who showed that Fizeau's theory could be improved by the use of available astronomical knowledge and that after a natural change was introduced, the agreement between theory and experiment increased.

Fizeau was a highly respected experimental physicist, and nobody took these experiments as wrong due to incompetence. While describing Fizeau's experiment, Lorentz says: "No objection could be presented, it seems to me, to the conclusion of this sage: that near the surface of the earth the ether is not at rest relative to it [the earth], but I think that it has not been demonstrated by those experiments that the speed of the ether is exactly equal to the speed of the earth"[40]. Hence, although Lorentz was not sure about the *quantitative* aspects of Fizeau's experiment, he agreed that Fizeau detected an effect due to the motion of the Earth through the ether. The same confidence in Fizeau's skill is shown in a paper by Lodge, where, after describing Michelson's failure to detect the motion of the earth, he adds:

> "There is already one experiment, which I have never seen criticized either way, tending in a sense precisely contrary to Michelson's. Fizeau observed the polarization produced by a pile of plates, and considered that he had proved that the azimuth of the plane of polarization varied with the direction of orbital motion of the Earth, and hence that the

[37] Fizeau (1859). Do not confound this polarization experiment with Fizeau's previous experiment on the ether drag by moving transparent bodies, which is not in disagreement with the theory of relativity.

[38] Fizeau (1859), Fizeau (1960).

[39] Faye (1859).

[40] Lorentz (1887), p. 204.

ether was streaming past them... The experiment seems to me extremely difficult, but to be well worthy of repetition by other observers"[41].

But these experiments were not reproduced before 1905. Therefore, when Einstein proposed his theory of special relativity grounded on the impossibility of detecting absolute motion through the ether, this very basic idea was not in accord with available experimental knowledge. It is true that at that time some physicists, including Lorentz, had a strong suspicion that Fizeau's results were wrong[42], but they neither repeated the experiment nor proposed testable auxiliary hypotheses to explain away the results obtained by Fizeau. So, according to Popper's rules, it was necessary to accept the experimental result, and it could be viewed as strong evidence against Einstein's theory. Einstein certainly read about Fizeau's experiment in one of Lorentz's papers[43], but this experiment did not shake his belief in the principle of relativity. He simply *ignored* Fizeau's results. Einstein was either behaving in a completely irrational way, or was hoping that future experiments would become compatible with the principle of relativity. This agrees with my proposed historical generalization of Einstein's behaviour.

A second interesting refutation of Einstein's theory of special relativity was provided by Kaufmann's experiments on the mass of fast electrons. This is a valuable historical episode that has already deserved a detailed description[44], and only a brief outline will be presented here.

During the period from 1901 to 1906 Kaufmann published the results of a series of careful experiments designed to measure the variation of the mass of electrons with their speed. The results of these researches were then compared to three different theoretical predictions: Abraham's, Bucherer's and Lorentz's. Einstein's theory, published in 1905, led to the same result as Lorentz's for this effect. What was the result of this test?

"The results above speak against the correctness of Lorentz's and also consequently of Einstein's fundamental hypothesis. If one considers this hypothesis as thereby refuted, then the attempt to base the whole of physics, including electrodynamics and optics, upon the principle of relative motion is also a failure"[45].

On the other side, the experimental results were compatible with both Abraham's and Bucherer's theories, and were not able to decide which

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[41] Lodge (1893), p. 750.

[42] Lorentz (1895).

[43] Miller (1981), p. 87.

[44] Cushing (1981).

[45] Kaufmann (1905).

was to be preferred. This conclusion was accepted, for instance, by Max Planck, who in 1906 declared:

> "To be sure, this question (of the acceptability of the principle of relativity) appears to be already settled by the recent important experiments of Kaufmann, and in the negative, so that further investigation remains to be done"[46].

Kaufmann's data were later reanalyzed by Planck, who hoped to find something wrong in the statistical analysis. But Planck concluded that the results favoured Abraham's theory, not Lorentz's[47]. And Lorentz himself, in a series of lectures delivered at Columbia University in 1906, accepted that his theory had been refuted:

> "Kaufmann, who, as early as 1901, had deduced from his research on this subject that the value of m/e increases most markedly, so that the mass of an electron may be considered as wholly electromagnetic, has repeated his experiments with the utmost care and for the express purpose of testing my assumption. His new numbers agree within the limits of experimental errors with the formulae given by Abraham, but not so with the second of the equations (313) [$m'' = (1 - \beta^2)^{-1/2} m_0$, that is, Lorentz's prediction for the mass-velocity relation], so that they are decidedly unfavourable to the idea of a contraction, such as I attempted to work out"[48].

Lorentz then duly remarks that some different approaches, such as those proposed by Bucherer and Langevin, could be more successful than his own theory. Lorentz therefore acted according to Popper's rules, accepting the refutation of this theory.

In 1907 Einstein discusses these experiments[49]. He agrees that Kaufmann's analysis is correct and that the difference between his theory and the experimental results cannot be easily explained away as a statistical effect:

> "The existing discrepancies are... systematic and beyond the limits of error of Kaufmann's experiment. That Kaufmann's calculations are error-free follows from the fact that Planck, using another method of calculation, was led to results which completely agree with those of Kaufmann"[50].

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[46] Planck (1906).

[47] Planck (1907).

[48] Lorentz (1915), p. 212-3.

[49] Einstein (1907).

[50] Einstein (1907).

Einstein acknowledges that the theories of Abraham and of Bucherer provide a better fit to the data, but offers his opinion that "they have a small probability of being correct since they produce complicated expressions for the mass of a moving electron"[51].

Notice that Einstein presents as argument against Abraham and Bucherer their "complicated expressions". Lorentz's (and his own) formula was "simpler". It might seem that Einstein is here using another of Popper's methodological rules: to prefer the simplest theory. Actually he is once more violating Popper's ideas. The three equations (Lorentz's, Abraham's, Brucherer's) were, according to Popper's concept of simplicity, equally simple: all were on the same footing as regards refutability; all had the same number of adjustable parameters; the only difference between these equations is this: it is easier to make mathematical computations using Lorentz's formula than using the others'. But to dismiss the other equations for this reason is foolish, and has no relation to Popper's methodology.

It is also interesting to point out that in this paper Einstein used a small trick to impress the public: Kaufmann had presented, in one of his papers, a graph showing the relation between the experimental results and the three theories. In this graph, it was easily seen that the agreement with the Lorentz-Einstein equation was worse than in the case of the other theories. Einstein reproduced Abraham's graph, but he omitted the results of Bucherer's and Abraham's theories; he only retained the experimental data and the predictions according to his own theory, and claimed that the concordance was satisfactory[52]. This does not seem a very honest procedure.

After some time, new experiments began to support the Lorentz-Einstein equation. But only in 1914 and 1915 were obtained definite and precise experimental results that favoured the Lorentz-Einstein formula and rejected Abraham's theory[53]. In the meanwhile, Einstein not only went on with special relativity, but also began to work on the general theory of relativity. He did not bother about the refutation of his theory, and did not search for another theory compatible with the experimental data. He did not follow Popper's rules.

7. *Miller's measurements of the ether-drift*

Perhaps the two historical instances discussed in the previous sections are not very impressive: after some time, both Fizeau's and Kaufmann's results were dismissed as wrong, and superseded by new experiments. But let us go on and study the next relevant refutation of Einstein's theories.

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[51] Einstein (1907).

[52] Miller (1981), p. 344.

[53] See Neuman (1914) and Guye and Lavanchy (1915). However, it is important to remark that even in 1957 Faragó and Jánossy (1957) were able to show that there was up to that time no decisive experimental refutation of Abraham's theory.

It is well known that the most important single experiment in favour of Einstein's theory of relativity is the famous interferometer experiment of Michelson and Morley. In 1881 Michelson published his first paper on this subject[54], where he reported that in an experimental search for an effect due to the motion of the Earth through the ether, he found a result ten times smaller than that predicted by the ether theory of Fresnel. The experiment was improved and in 1887 Michelson and Morley reported[55] that the observed effect was 16 times smaller than that predicted by the ether theory of Fresnel. In 1905 Morley and Miller repeated the experiment, with similar results[56]. Those experiments could be interpreted either as favouring Stokes' ether theory, or as favouring the principle of relativity. From 1921 to 1926 Miller performed a very large series of new experiments to decide which interpretation was correct[57].

According to Stokes' theory the outcome of the experiment could be different if performed at the top of a high mountain; according to Einstein's theory, the place was irrelevant. The result of an enormous number of carefull repetitions of the experiment, at the Mount Wilson astronomical observatory, was against Einstein's theory: there was an observable effect that could not be explained away. After collecting his data, Miller developed a detailed quantitative theory of the ether effect, that was able to account for all details of the observations with a single new hypothesis. The theoretical and experimental results were published in a series of remarkable papers, from 1925 to 1934.

How did the scientific community react? Several possible systematic errors in Miller's experiments were suggested: problems related to temperature effects, small deflections of the stone used as a base for the experimental arrangements, and so on. All the suggestions were tested[58], and Miller refuted all the "ad hoc" auxiliary hypotheses. Several physicists tried to repeat the experiment[59]; but the new experiments were not comparable with Miller's, since they did not reproduce the relevant experimental condition: it was necessary to perform the measurements in a room where the walls would not interfere with the motion of the ether. Miller worked in a small cabin with open windows at all sides, thus providing an easy path for the ether. All other experiments were performed between heavy closed

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[54] Michelson (1881).

[55] Michelson and Morley (1887).

[56] Morley and Miller (1905).

[57] Miller (1922), Miller (1925), Miller (1926), Miller (1933), Miller (1934a). It may be relevant to add that Miller was not an obscure crank. When he published his 1926 paper, he was the President of the American Physical Society. It is evident that he was highly respected by the scientific community at this time.

[58] Miller (1926), Miller (1933).

[59] Kennedy (1926), Piccard and Stahel (1926), Illingworth (1927), Piccard and Stahel (1928), Michelson, Pease and Pearson (1929), Joos (1930), Joos (1931).

walls, and were therefore irrelevant[60]. Miller's experiments have never been reproduced.

After Miller's 1933 paper[61], where all possible objections are answered, and the strong coherence of his data displayed, the scientific community reacted favourably to Miller. In searching the relevant literature I have found *no* article in the next 20 years criticizing Miller's experiments. On the contrary: there were favourable comments. In 1933, Hill[62] described Miller's results and remarked: "... the general consistency of the observations with the predicted results as regards variation with time of day and of year is very striking. Clearly such a situation leaves the accepted theory [Einstein's theory of relativity] at rather severe odds with the experiment". He concludes that the theory must undergo some change. In 1934 Drysdale describes Miller's work and states that it refuted the theory of relativity[63]. In the same year Carwile proposed a small change in Miller's experiment in order to obtain a better evaluation of the speed of the Earth relatively to the ether[64]. In the following year Cartmel analyzed the theory of the interferometer used by Miller and proposed a new formula that led to a better agreement with Miller's data and that explained away the negative results of Kennedy and Illingworth[65].

Parallel to Miller's experiments, a respectable French astronomer, Esclangon, had also observed optical effects that he ascribed to the motion of the Earth relatively to the ether[66]. Although Esclangon's experiments used an arrangement completely different from Miller's, it was soon claimed by Carvallo that the two effects were closely related, and in 1934 Carvallo showed that the two independent experiments led to concordant results concerning the direction and speed of the motion of the Earth[67].

There were also attempts to show that Miller's results were compatible with the theory of relativity. Rosen[68] introduced a small change in the equations of the general theory of relativity and used this new theory to account for Miller's results[69]; his ideas amounted to the introduction of a kind of gravitational ether:

> "Thus we see that it is possible to account for Miller's observation of an "ether-drift", together with the value he finds for the drag-coefficient, on the basis of a flatspace theory of gravitation, as being due to the

60 Miller (1933); Miller (1934b).
61 Miller (1933).
62 Hill (1933).
63 Drysdale (1934).
64 Carwile (1934).
65 Cartmel (1935).
66 Esclangon (1926a), Esclangon (1927).
67 Carvallo (1932), Carvallo (1934).
68 Rosen (1940a).
69 Rosen (1940b).

> motion of the earth through the gravitational field of all the matter (and energy) in the universe. This field, the resultant of a large number of small contributions, is nearly static and serves to single out a frame of reference for determining 'absolute' motion"[70].

Another relativist, Synge, tried to explain Miller's results (and the different results obtained by others) through a detailed study of the accelerated motion undergone by the apparatus while moving attached to the Earth[71]. So, even in 1952 there was a general belief that Miller's data were good, and that they should be accounted for by a theoretical analysis.

It is interesting to remark that Popper was aware of Miller's experiments, and refers to them in the *Logic* (§ 8). But Popper does not provide a correct historical description of this case, for he says: "Since later tests again gave negative results it is customary to regard these latter as decisive, and to explain Miller's divergent results as 'due to unknown sources of error' ". (*Logic*, § 8) But, as noticed above, Miller's experiments have never been properly reproduced.

It is important to notice that Popper refers to Miller's experiments as an instance of the possibility of irregular and irreproducible eventual observations. Anyone who might read Miller's papers will perceive that this is not a fair description of his 200 000 singular observations that led to 12 800 concordant measurements of the speed to the Earth relative to the ether[72]. Miller's effect was as regular and seemingly real as any physical effect. Whether or not different observers would also obtain similar results in similar conditions *we do not know*, because this has never been tried. Since the experiments were not repeated, to ascribe Miller's results to "unknown sources of error" is obviously invalid from the point of view of Popper's methodology; it is just a conventionalist trick.

What was Einstein's early reaction to Miller's work? According to Shankland[73], when Miller began his experiments in 1921, Einstein was visiting the United States of America for the first time, and went to see Miller. He stimulated Miller to go on and keep improving his experiments, because he hoped that as the measurements were continued the anomalies would disappear, or a simple explanation would be found to account for the results without damage to relativity. That is: Einstein did not regard Miller's results as a refutation of the theory of relativity.

It is very interesting to study how Einstein referred to Miller's experiments many years after their performance. There is a very informative

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70 Rosen (1940b).

71 Gardner (1952); Synge (1952a), Synge (1952b); Ditchburn and Heavens (1952).

72 Miller (1933).

73 Shankland et al. (1955); Shankland (1963).

paper written by Shankland which describes five meetings he had with Einstein in the period 1950-1954[74]. Shankland was one of Miller's assistants while performing the ether-drift experiments[75], and it seems that Miller believed that Shankland would continue his work; before his death, Miller handed Shankland all his unpublished laboratory notes on the experiment, and asked him to use them[76]. It is sad to see that Shankland only used them to try to find something wrong in Miller's statistical analysis. And it was in the period when Shankland was developing this analysis that he visited Einstein and talked to him (among several subjects) about Miller's experiments.

Before discussing these conversations, it is important to emphasize that in this period the general outlook of the theory of relativity was receiving a strong blow from Dirac's proposal of a new ether theory[77]. The development of quantum theory had at this time led to a picture of the "vacuum" that was altogether different from the idea of a mere void space: the vacuum was now regarded as the seat of strong processes, such as the spontaneous virtual creation and annihilation of all sorts of particles and antiparticles. Considering this state of affairs, Dirac proposed the reintroduction of the ether and of absolute time in physics. The relativists reacted, and there was, for instance, a weak reply by Infeld[78], who tried to show that it was not necessary to follow Dirac; but most authors agreed with Dirac's proposal[79]. Ives, a well known anti-Einsteinian, took this occasion as a fine opportunity for asserting that the ether should never have been dismissed[80].

Now let us come back to the conversations. When Shankland first told Einstein that he wanted to talk about Miller's observations, Einstein replied: "Do you really think there is something in them?" Then, when Shankland replied "that I felt confident that a thorough analysis of the observations might show that they were consistent with a null result, he was all interest and excitement"[81]. It is clear from this description that Einstein was not much interested and excited before Shankland told him this. But what could be the reason for this excitement? If Einstein *knew* that Miller was wrong, or if he thought that someone had already proved this, he would not be so interested in Shankland's work. As Shankland told him about his conjectures, Einstein became very animated, and exclaimed several times: "This is very beautiful!" Actually, as later recognized

[74] Shankland (1963).
[75] Miller (1933), p. 242.
[76] Shankland et al. (1955); Shankland (1963).
[77] Dirac (1950); Dirac (1951a); Dirac (1951b); Dirac (1951c); Dirac (1952a); Dirac (1952b); Dirac (1953).
[78] Infeld (1952).
[79] Stern (1952); Datzeff (1957).
[80] Ives (1953).
[81] Shankland (1963), p. 51.

by Shankland himself, his first conjecture, that Miller's mathematical analysis was wrong, turned out to be invalid.

As remarked above, Shankland had been one of Miller's assistants (although he never refers to this in his articles). He was doubtless very well acquainted with Miller's experimental procedure, and could not at first imagine how a spurious effect could arise. At last, Shankland supposed that it might be due to Miller's method of treating the data. This shows us very clearly that Shankland himself did not at first suspect any *experimental* error (systematic influence) in Miller's observations. Einstein aparently agreed with this opinion, and he also remarked that "he considered Miller an excellent experimenter and thought his data must be good"[82]! This is the reason why Einstein was so happy with Shankland's idea that there was an error in the mathematical analysis: everybody agreed that Miller was a very good experimental physicist.

Einstein also told Shankland that "Lorentz had studied Miller's work for many years and could not find the trouble"; "... Lorentz could never explain Miller's result and felt that it could not be ignored, although Einstein was not sure whether Lorentz really believed Miller's results"[83].

Although Einstein seemed to trust Miller's experimental skill, and said that so did other outstanding physicists (such as Lorentz) he nevertheless rejected his results. It is important to stress that he always retained this opinion, and that it was not a mere result of Shankland's analysis. Einstein himself told Shankland that several years before their talk, there had been a meeting at the Institute for Advanced Study (Princeton) where Miller's results were discussed, and Einstein commented: "God is hard on us, but He is not malicious! Surely there is an explanation for Miller's results"[84]. It is obvious that he was still waiting for some "ad hoc" hypothesis that would explain away Miller's experiments. If there was no proper repetition nor alternative explanation of Miller's results, why did Einstein not regard them as a refutation of the theory of relativity?

In Shankland's report we may find two main reasons: Einstein stated that the empirical results (the direction of motion of the Earth relative to the ether) were irregular, and that the ether theory behind the experiment was incompatible with another experimental result (the astronomical aberration): (1) "He repeated several times during our talk that since the phases found by Miller ("which fix the direction in space") were not consistent, this was the strongest argument against the drift reported by Miller"[85]; (2) "He added that the idea that a bigger "ether wind" would exist on a

[82] Shankland (1963), p. 51.
[83] Shankland (1963), p. 51.
[84] Shankland (1963), p. 57.
[85] Shankland (1963), p. 51.

mountain had always been impossible to bring into a picture with the facts of astronomical aberration"; "He also reminded me that any "drag" would be inconsistent with aberration"[86].

But Einstein's statements are wrong: (1) The most beautiful aspect of Miller's results is their very strong coherence – there was no inconsistency or irregularity as suggested by Einstein; (2) Einstein is probably referring to the old form of Stokes' ether theory; this theory was indeed full of theoretical and experimental problems until Lorentz corrected it[87], but afterwards it became acceptable, and it was the *corrected version*, completely in accord with astronomical aberration and all experiments, that was invoked by Miller[88]. It is very clear that Einstein did not understand this ether theory, since he also states "that there should be nearly as much "drag" at Mount Wilson as in the Case basement room due to the mass of the interferometer itself"[89]. Einstein seemingly ignored that in 1892 Lodge had shown that laboratory-sized bodies do not produce any noticeable ether drag[90].

The study of Einstein's attitude as reported by Shankland shows very clearly that he did not pay much attention to experiments at all. His ignorance about the physical structure of the Michelson-Morley apparatus[91] clearly shows that he never studied their article carefully, although this was (as said above) the single most important experiment in favour of the principle of relativity.

It is also relevant to describe Einstein's reaction to Synge's work on Miller's results. Synge was a well known relativist, but he was unable to dismiss Miller's experimental results. So, he tried to show that the observed results were a real effect, not due to the speed of the Earth, but to its *acceleration*. This would obviously save relativity, and would be an elegant solution. It was well known that in accelerated motion (such as rotation) optical experiments could give positive results, and this did not conflict with the theory of relativity. Nevertheless Einstein's reaction was very strange: "Einstein stated strongly that he felt Synge's approach could have no significance. He felt that even if Synge devised an experiment and found a positive result, it would be completely irrelevant". "He then recalled Synge and his work and asked me if he (Synge) were not somewhat of a philosopher. He again said that more experiments were not necessary, and results such as Synge might find would be 'irrelevant'. He told me not to do any experiments of this kind"[92].

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[86] Shankland (1963), p. 57, p. 52.
[87] Lorentz (1887).
[88] Miller (1933).
[89] Shankland (1963), p. 52.
[90] Lodge (1893-4).
[91] Shankland (1963), p. 48.
[92] Shankland (1963), p. 53, p. 54.

Here we see very clearly that Einstein was for some inscrutable reason convinced that Synge was wrong, and that in this case it was not necessary to pay any attention to its experimental test. This is exactly like his attitude as regards Kaufmann's corroboration of Abraham's and Bucherer's theories. It seems that Einstein relied on his own feelings completely; whether this is good or not, I do not discuss, but it is obvious that this is not what Popper requires scientists to do.

It is interesting to notice that although Shankland's first attempts to invalidate Miller's results were not fruitful, he, at last, thought that he had found a systematic error in Miller's experiments. The main problem, according to Shankland, was lack of temperature control. But Miller had given a lot of attention to this problem, and it seems to me that the experimental results cannot be ascribed to this kind of error. Shankland then produced a lengthy paper describing his criticisms, and Einstein liked it very much (Einstein would love any idea that could be used to invalidate Miller's experiments). The article was published[93], and is usually regarded by relativists as the standard refutation of Miller's experiments.

We could add some new examples of relevant refutations of Einstein's theory that were known by Einstein but that did not shake his confidence in the theory of relativity. In his most important paper[94], Miller refers to other experimental and independent refutations of the theory of relativity such as the gravitational studies of Courvoisier and Esclangon[95]. Even if Einstein had not been aware of these researches before, he must have heard about them through Miller's citations. I shall not provide a description of these interesting researches here. It is enough to say – and anyone may try to refute me – that these were relevant refutations of Einstein's theory of relativity, since Esclangon and Courvoisier were able to detect several regular measurable effects of the translational motion of the Earth through the ether (or absolute space). These experiments were never repeated; nor was any explanation of these results suggested; and nevertheless this did not bother Einstein: he still maintained his old beliefs and never wrote a word about these experiments.

It would not be difficult to add several new instances to the above cases. But this is not necessary. According to Popper's rules, I may accept my general historical hypothesis about Einstein until someone refutes me using the clearly prescribed instructions described above (Section 5).

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93 Shankland et al. (1955).

94 Miller (1933).

95 Esclangon (1926b); Courvoisier (1926-29); Courvoisier (1927).

8. *Conclusion*

Popper's criterion of demarcation itself is not an empirical or scientific law, and hence it cannot be refuted by any set of data concerning the behaviour of scientists. Nevertheless, Popper's criterion was developed as an attempt to systematize the actual rules that guide the scientific behaviour of the "great scientists". Among the paradigmatic scientists cited by Popper, the topmost example is Einstein. Using Popper's rules, we have corroborated the testable hypothesis that Einstein never accepted the "relevant refutations of his theory" (that is, good refutations, according to Popper's criteria) as refutations of his theories. This corroborated historical hypothesis refutes Popper's claim to provide a criterion that systematizes the actual scientific behaviour of the "great scientists", and this takes from Popper's criterion of demarcation the basis that Popper supposed it had. In brief: I have used Popper's methodology to show that Einstein's theory-plus-method is not scientific, according to Popper's criterion. (This does also seemingly imply that Einstein is not a scientist, although Popper did not define scientist.) It seems to me that Popper would regard this as a strong argument against his criterion, since Popper always refers to Einstein as his chief instance of paradigmatic (that is, exemplary) scientist. If the actual practice of outstanding physicists is not different from what Popper describes as unscientific, what is, after all, the difference between actual physics and psychoanalysis or marxism? Popper's criterion does not provide an answer.

It is obvious that a Popperian may defend Popper's epistemology in the following way: it is possible to describe Popper's ideas without any reference to the history of science or to particular scientists. Even if the origin of the criterion of demarcation is illegitimate, it may be an important and useful tool. But if someone wants to adopt this attitude, he must be aware of its consequences: according to Popper's criterion, the theory of relativity has been refuted; a detailed analysis will perhaps show that *all* basic physical theories have been refuted. Therefore if we want to maintain Popper's criterion, we shall perhaps be obliged to say that nowadays there are no acceptable basic physical theories. Will any epistemologist dare to say this?

Epistemology is logically independent from the history of science. But it has already been said that an epistemology that is not guided by a concern with the actual scientific practice (that is, the real history of science) is sterile[96]. Epistemologists should not close their eyes to the history or contemporary practice of scientists, or scientists will close their eyes to epistemology. There is another important rule: it is necessary to

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[96] Burian (1977).

have a good acquaintance with the (sometimes dirty) details of the history of science in order to avoid the false pictures of scientific practice that abound in current books.

Popper's views are very interesting and still deserve to be studied, although they seem to me unacceptable[97]. I have presented here only one argument against them; there are many others, of course.

Popper has described a very interesting method that is sometimes used by scientists and that had never been clearly and coherently described before his work. This method may still be used by contemporary and future scientists, and it will be used and lead sometimes to useful results. But scientists may behave in a way incompatible with Popper's rules; this does not imply that they are unscientific when they violate Popper's rules. Scientific procedures are much richer than what Popper describes as the valid scientific method.

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[97] In my opinion, *any* criterion of demarcation is unacceptable (Martins, 1980).

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