DOES THE SCIENTIFIC PAPER ACCURATELY MIRROR THE VERY GROUNDS OF SCIENTIFIC ASSESSMENT?

Mircea FLONTA*

* Department of Philosophy, Faculty of Philosophy, University of Bucharest, Bd. M. Kogălniceanu, 64, Bucharest 70609, Romania.

BIBLID [ISSN 0495-4548 (1996) Vol. 11: No 27; p. 19-31]

ABSTRACT: This paper presents a prevalent representation about the objectivity and impartiality of scientific knowledge that emerges from the structure and style of the standard research paper. This representation is critically examined considering some rather untypical scientific papers reporting controversies between researchers in a certain field of experimental science. The role of personal preconceptions and intellectual prejudices in the assessment of scientific theories is emphasized by reference to Einstein's grounds for his general theory of relativity.

Keywords: research paper, scientific assessment, impersonal character of scientific knowledge, scientific controversy, personal preconceptions of the scientist.

The basic modern scientific literature consists of papers published in scientific journals. These papers communicate and discuss the results of research in established fields of scientific activity. It is a prevalent point of view that research papers are a faithful mirror of scientific thought and of scientific life. And it is suggested that they are a truthful source for learning about the grounds of the scientific assessment.

In the following, I shall develop a two-step approach. In the first step, an image of scientific rationality, particularly a current representation about the grounds of scientific assessment, will be summarily sketched. This is the image that results mainly from research papers in specialized areas of theoretical and experimental science. In a second step, a critical examination of this image will be undertaken. Evidence which supports this examination can be found in some unconventional discussions and controversies between scientists, in scientific correspondence and in other personal confessions and recollections of well-known research workers.

The appearance of scientific journals is strongly-built with the setting up of scientific institutions, of learned societies that bring together different groups of researchers. One of main aims of these associations has been to reinforce and to secure the autonomy of scientific research, to free it entirely from the tutelage of theology and tradition natural philosophy. In this emancipation effort the impersonal character of scientific knowledge, its independence from *a priori* subjective biases and beliefs is particularly stressed. It has been suggested that the search for objective knowledge imposes an increasing detachment from cultural or personal options, options which can separate and don't bring together the researchers into a consensus that will be apt to promote impersonal knowledge. Unwritten rules of scientific publications contributed largely to confer authority to this widespread image about the excellency of scientific knowledge. The changes that scientific papers underwent in time, regarding their structure and style can be considered as a major expression of a popular representation about the impersonal and superpersonal character of scientific knowledge. A main idea that seems to lie behind the evolution of the structure and style of scientific research paper seems to be that it should be freed from any information, considerations and discussions which are not absolutely necessary for understanding and verifying the results reported within.

In our century the style of scientific publications became more and more concise and impersonal. The austerity of the scientific expression has progressively increased. An irresistible propensity towards uniformity in structure and style becomes obvious. Intentions to avoid too daring hypotheses -judged as rash or premature- and to suppress affective reaction to one's or somebody else's research results reinforced this propensity. The famous German mathematician Carl Friedrich Gauss already used to say that papers presenting new scientific discoveries should contain none of its "scaffolding" with many elements of personal nature -elements which were active in making possible these discoveries. When presented on paper, they should look like a finished building. More than half a century ago, Erwin Schrödinger gives a magnificent characterization of this image of scientific knowledge that emanates from the standard research paper:

> Here emerges the ideal: any personal, subjective element must be discarded; the aim is exclusively the finding of pure objective truth, a truth that can be tested by anyone with the same result, independently of his temper. We often hear that not only the individual, but also the human race must be eliminated as a subject of research. Any anthropomorphism must be discarded so that at least in this area man shouldn't be the measure of all things, in the way sophists demanded. (E. Schrödinger, 1932, 26 -my translation).

Let us now examine the way in which the impersonal character of scientific knowledge is reflected in the content and structure of standard research papers reporting a new discovery. The impression of nearly superhuman objectivity derives largely from the suggestion that scientific ideas are to be assessed, accepted or rejected, solely by using the criteria leading to a consensus: logical coherence, consistence with experimental (and observational) data. Consequently, scientific ideas don't retain any trace of those beliefs, convictions and biases of the individual research worker or of the group which was active in producing them. In exceptional controversial situations it is agreed that the prevailing judgement should belong to a restricted circle of highly competent scientists, with great authority in the field, the authority of men who are recognized to be particularly skilled in applying the presumed generally accepted criteria of scientific assessment. Their verdict is naturally subject to the scrutiny of the whole scientific community. And it is expected that any scientist deserving this title will accept the verdict irrespective of his personal inclinations and expectations. For what characterizes the true scientist is precisely his capacity of freeing himself from personal assumptions and representations which are not susceptible to be agreed upon by the research community. It is suggested that since one deals with qualified scientists who are genuinely interested in searching for truth, any misunderstanding or difference of opinion will be in the end overcome. All that is required is good will, patience and perseverance in order to isolate and remove misunderstandings and to produce arguments that will have an equal impact upon all those who are both honest and competent. No wonder that scientists like to stress the difference between scientific and common thinking, the latter being ridden by prejudice and preconceived ideas. However, more than any general considerations, the standard research paper conveys the popular image of the true scientist, a person characterized by a sceptical reserve towards any ideas and assumptions which are insufficiently tested, who has learned through a severe censorship of his subjective biases to conduct his mind according solely to the imperatives of an impersonal logic and experience. It is a person who tries to understand the world through cold reasoning, experimentation and again reasoning. In this process any moral and emotional ground for preferring a conclusion would be out of place.

The concern for reaching consensus is thus a dominant concern in writing current research papers. It manifests itself strongly in the patterns and stereotypes used in writing these papers, especially in research papers reporting experimental results. These usually contain several sections, with different extent: *introduction, methods and materials, research results, discussion* and *conclusion*. The introduction sketches the research area within which the work done by author (authors) is placed; the features of each previous approach and way of attack of the research topic as well as the main results obtained so far are presented; emphasis is placed upon the accepted results that represent the starting point and the support of the reported work; issues that are still open or controversial are mentioned; one draws attention to the questions and aspects that need to be further investigated; at this point the general objectives of the current research are formulated. In the next section the required details regarding materials, research instruments, equipment and installations used are presented together with the experimental procedures; one also presents the precautions taken to ensure the adequate functioning of the equipment and installations and to control the conditions in which the presented experiments took place. The research results are presented in a systematic manner; written texts, tables, figures and other means are used; estimations of the accuracy of quantitative data and correlations obtained through experiments are presented. In the final section of the paper, the main aims of the research are reminded and the results obtained are discussed in relation with present knowledge in the research field, with hypotheses and theories that have circulation in the field; the novelty of the reported research results is underlined as well as the significance of these results with respect to controversial or open issues in the field; data and assumptions which are confirmed or refuted by the current research are stressed; questions to which the reported results offer no answer or a doubtful one are shown; issues that were not sufficiently clarified are mentioned and recommendations for future research are made. These topics are present in all research papers, though their relative weight and place in each text may differ.

Both the general structure and the style of scientific research papers suggest insistently that the presented results are in no way dependent of personal beliefs and biases of the author (authors), but have on objective, universal value. Presentation and discussion of data are done in such a manner that it be best seen that the results are *recorded facts*; they are imposed, it is suggested, by controlled observations and measurements; consequently, only the overthrow -by new research results- of premises so far accepted as certain, or the discovery of a systematic error in the functioning of the experimental devices could invalidate the reported results. The impression of detachment and critical reserve which is proper to a research exclusively oriented by an impersonal search for the truth is underlined by cautious, careful formulations abundant in the text. Typical expressions like "reported experiments could not point out certain correlation", "we could not exclude certain assumption", "certain hypothesis was not confirmed", "the obtained data make (more) plausible certain connection" are to be found widely. Thus the competent reader gets the idea that the author (authors) states only what can be firmly based on controlled facts. The word *speculative* is usually used disparagingly for hypotheses and assumptions that cannot be subject to the control of experimental facts. The experimental devices and procedures are presented in such a manner that one is left to believe that any qualified researcher, using the indicated materials and devices and applying the described procedures, will be able to reproduce the reported results. On the whole, the typical structure of experimental research papers and the characteristic style of such texts have the function of convincing the readers that the author (authors) states only things which are firmly based on controllable and reproducible facts and that they consider such facts as the only legitimate source and support for their scientific statements. There is nothing to suggest that the outstanding authority of certain scientist, the author's (authors') professional status, his more direct contact with certain experimental practice, subjective biases or other factors, variable from person to person, from one group to another, could have had any influence on the presented results. Whenever the author (authors) proposes amending or revising some largely accepted ideas and suppositions, he suggests that he does so only because he was able to discover facts that compel him to do so. The implicit suggestion is that, apart from some generally accepted ideas, all the assumptions and the research conclusions were explicitly scrutinized and presented in such a manner that they could be critically scrutinized by any competent colleague. It is easy to notice that while writing a standard research paper all the steps were taken to meet necessary and sufficient conditions for a wide consensus with respect to the scientific value and significance of the reported results.

The scientific research paper has been examined as a literary genre (J. Gusfield, 1976). Words and expressions are used in a manner meant to suggest the impersonal, universal significance of the resulting conclusions. Recurrent expressions like "tests indicated...", "recent research shows...", "data confirm that...", "based on the results of performed tests, we conclude that...", "these findings compel us to recognise the necessity to amend (to revise) the supposition that..." are intended to convey an impression of sovereign impartiality, to awake the feeling that the scientists have succeeded to free their minds from any preconception or subjective bias. There is a strong suggestion that using the same materials and devices and applying the same methods and procedures any qualified researcher would reach the same results. The author of a paper of this kind speaks to his reader saying: 'I will give you the complete information I have about the facts. We will reason together and reach a consensus through facts and judgment. As a rational being you must come to the same conclusions as myself. Seen as an individuality who can thing and judge unlike other persons equally interested and qualified to seek the truth, the author disappears from the standard scientific texts.

A closer scrutiny at some scientific controversies in experimental science offers a suitable starting point and support for a critical examination of the popular representation about the impersonal character of scientific knowledge, a representation so much strengthened by the structure and style of standard research papers. In the following, I will consider some rather untypical scientific papers, containing reports on scientific controversies, with a detailed presentation of the pertinent arguments to each side. My evidence is taken from a collection of papers under the title "*Water in the Cell*". It contains a paper by G. N. Ling, critical comments on his paper by many reviewers and some polemical answers which express Ling's reactions to these critical comments (G.N. Ling, 1988).

In his paper Ling deals with the quantitative proportions of water, proteins and potassium and sodium ions (K^+ and Na^+) in living matter. He maintains that the till now widely accepted theory about these proportions, i.e. the membrane pump theory, has been refuted by verifiable experimental facts. In order to explain the proportions just mentioned, the author advances his own alternative theory, the association-induction (A.J.) theory.

The membrane pump theory introduces some hypothetical pumps, located in the cells membrane, and suggests that the function of these pumps is to maintain the quantitative balance between potassium and sodium ions. The presumed pumps do this job by controlling the elimination of intracellular Na^+ and the absorption of extracellular K^+. The free energy needed for the functioning of the pumps -the theory maintains- is generated by the hydrolysis of some highly energetic substances whose prototype is ATP, catalysed by ATP-ases, which in turn are activated by potassium and natrium. Ling presents facts and arguments that speak against this theory. Thus, experiments performed by him in the '50's showed that the frog muscle preserved the concentrations of K^+ and Na^+ even when the chemical reactions that were supposed to activate the membrane pumps were suppressed. These experimental results were confirmed by other researchers. Based on these findings, Ling formulated the hypothesis that the cell has other metabolic energy sources. This hypothesis is supported also by an energy balance, published by Ling in 1962, a balance who shows a large discrepancy between the energy available according to the membrane pump theory and the energy required to maintain potassium ions in the cell. No objections were formulated against these calculations at that time, though their results were obviously incompatible with the membrane pump theory. Even more, Ling calculations were confirmed by two papers published in 1965 and 1973. Three attempts at proving that the energy required for preserving the potassium ions in the cell is less than that given in Ling's energy balance were refuted experimentally. Additionally, it is fair to assume that supplementary pumps are also required in order to preserve other soluble chemical compounds in the cell. Therefore, the incapacity of the membrane pump theory to account for the required amount of energy appears even more acute. The development of the technology of preserving living cells at low temperatures, namely at temperatures at which chemical reactions stop almost completely proves -according to Ling- that preserving life does not depend on the activity of membrane pumps and generally on continuous chemical processes. The association-induction (A.J.) theory, advanced by Ling in 1962 as an alternative to the membrane pump theory, explained life preservation under such conditions not by some uninterrupted chemical activity, but as a result of the presence of a special energy state, called the *living state*. The assumption is that the living cell components -proteins, water and K^+- are strongly associated and maintained together by a high level of free energy. When death occurs, the free energy becomes very low, entropy increases and therefore the interactions between the cell components become extremely relaxed. According to the A.J. theory, ATP is an active agent, with high electric potential that polarizes, assembles and brings to a state of high energy and low entropy proteins, water and ions, as well as other chemical components that through association form the living cell. The new theory explains not only the constancy of the potassium ions in the cell, but also the constancy of interactions between proteins and water.

Reviewers of Ling's paper respect his criticism of the membrane pump theory and express their scepticism regarding the new association-induction theory. One of the reviewers (H. E. Rorschach) holds that the description given by Ling to the structure of polarized water is not supported by the experimental facts; some of Ling's statements go far beyond the present state of our knowledge in the field. To this assertion Ling gives a noteworthy answer:

> That a theory is not entirely proven yet and thus ahead of what we truly know is entirely in keeping with the time sequence of the Scientific Method where understanding *follows* full experimental confirmation of the predictions of the theory (G. N. Ling, 1988, 908).

Another reviewer (G. Roomans) considers that proving the possibility of preserving living cells at low temperature does not in itself invalidate the membrane pump theory. His argument is that present- day methods succeed in preserving living cells at low temperature just trough changing by a large amount the ions contents of the cell. According to the A.J. theory, such changes should modify the energy state of the cell proteins. To this, Ling replies that such a decrease of temperature makes impossible the chemical reactions on which pumps' functioning depends and therefore life cannot be preserved by this mechanism. Consequently, preserving life at low temperatures is not compatible with the membrane pump theory since the possibility of 'defreezing' the cell should be interpreted as creating life, a conclusion obviously unacceptable (G. N. Ling, 1988, 910). The third reviewer (J. Z. Nagy) strongly opposes the idea that the membrane pump theory was refuted by experimental facts. He maintains that all the arguments advanced by Ling are based on his own experiments on frog muscles exposed at low temperatures that were performed over 30 years ago and published in 1962. To maintain that these experiments were rigorously controlled and they represent scientific facts is, in Nagy's opinion, a great exaggeration. Ling mentions only two confirmations, one of these contained in the Appendix to an unpublished Ph. D. thesis from 1965. Nagy stresses that the membrane pump theory could be refuted only by a set of extensive and carefully conducted tests. The data that support the energetic balance of Ling cannot offer a basis for a well-grounded assessment of this theory. On the other hand, experiments at the temperature 0°C cannot serve the purpose of an energy calculation since in hibernation all physiological processes are extremely slow. Besides, Ling didn't point out errors in other totally different energy calculations which tend to support the membrane pump theory. Nagy concludes:

> In the present form, his claims about the energetic balance of the sodium transport in the frog muscle remain only declarations without any support, and what is even worse, due to the missing explanations, one cannot even make any attempt to control them. *It is not acceptable to make reference to a work written 25 years ago, and blame all the existing cell physiology knowledge in this respect without a careful analysis and comparison of the more recent experimental data* (G. N. Ling, 1988, 911).

Nagy notices also that Ling does not examine other possibilities of mitigating controlled experimental data with the membrane pump theory and adds that "it cannot be the task of the reviewer to list all the possible mechanisms and explanations which are just omitted by the author" (G. N. Ling, 1988, 912).

In his answer to Nagy's review, Ling stresses that the association-induction theory is able to explain the distribution of Na^+ and K^+ in the living cell, as that of other soluble elements at cell temperature at which the possibility of conserving life was proved. By contrast, the membrane pump theory can explain the low level of Na^+ and the high level of K^+ only at some of these temperatures, say at 25°C, but not at 0°C. On the other hand, Ling thinks that it is suitable to bear in mind that truth is ageless: the value of an energy balance published more than 25 years ago remains the same as long it has not been invalidated by more recent experimental results. It is true that different energy balances were published before 1962 but they referred to experiments carried out in different conditions. These experiments were not, however, aimed at testing the membrane pump theory. Ling claims that the advocates of the membrane pump theory do not seem in general inclined to take seriously into account experimental data which are not compatible with the theory. For example, a review of the membrane pump theory, published in 1975 by two researchers from the Physiology Laboratory of Cambridge University, mentions 245 papers in favour of this theory, covering a period of over 20 years, but no reference is made to any of Ling's or other scientists' experimental results which seem to offer strong grounds for rejecting the theory. Ling notices:

> This practice of citing only favourable evidence and not citing unfavourable ones is against the ethical rules of science and strikes at the very root of science which has not other purpose than the seeking of the truth. (G. N. Ling, 1988, 912)

Rejecting Nagy's reproaches that he didn't examine other possibilities of mitigating experimental facts with the membrane pump theory, Ling states that he had proved the lack of validity of all so far published attempts of bringing the membrane pump theory to terms with the existing experimental data and that no reply to his conclusions was published till now. And Ling declares himself ready to examine any such attempt when published.

We can remark that the whole discussion taking place between Ling and the reviewers of his paper didn't contribute to bring closely together their views. This is a significant remark. In the final part of his review, Nagy says that he remains open to new ideas and explanations in the field of cell physiology, but these "must be based on solid facts and not on fantasies which are far from biological reality." (G. N. Ling, 1988, 912). Ling strongly rejects this insinuation. Such a deep disagreement between competent scientists, on a matter that can be subject to experimental validation, cannot pass without attracting special attention. A quick survey of the arguments and conterarguments put forward in this discussion shows that each side believes that his position is supported by an evidence that must be compulsory for all the competent researchers in the field. How can the complete lack of receptivity for such an evidence be explained? And isn't such a persistent and systematic disagreement on matters considered to be under the control of facts a provocation for the popular representation about the impersonal character of scientific knowledge, a representation that is so strongly suggested by the structure and style of the standard research paper?

It is evident that each side considers his own views as conclusions based exclusively on controllable facts and compulsory deductions. In other words, these views are presented as objectively grounded and thus entitled to claim for universal recognition. In this sense, the claims of the sides taking part in the controversy are mutually exclusive. The only acceptable conclusion seems to be that one of the sides has violated, willingly or not, generally acknowledged requirements and unwritten rules of the "scientific method". The reactions of the two sides seem in our case to confirm this assumption. The typical accusations are that the basic statements of the other side are not satisfactorily supported by the facts, that the data which support the main conclusions are not controllable, that published research results in the field have not been carefully and impartially examined and were rejected in a manner that does not fulfil the requirements of a truly scientific criticism, that basic experimental data that support the general claims of one of the sides in the discussion were not confirmed by unbiased researchers, that supporters of a theory are not ready to consider experimental data seriously and with the required detachment when they seem to be inconsistent with the principles of their favourite theory. Consequently, each side accuses the other of infringing some generally accepted requirements of objective research, an "ethical rule of science."

It seems however difficult to avoid the impression that such an explanation of a persistent disagreement between experienced and well-known researchers in a consolidated domain of modern experimental science is too easy and at hand. The sides taking part in the controversy finally accuse each other of lack of willingness to abandon a theory proved to be incompatible with experimental controlled data. This is a conclusion not stated as such, but nevertheless clearly suggested. In the above described case, as in many other cases, it seems to be not only a painful one, but an implausible and therefore difficult conclusion to accept. We should seek for other explanations before reaching the extreme conclusion, namely to question the scientific probity of researchers with an excellent reputation.

My suggestion in this sense is to consider seriously the role played by researchers' personal but nonarbitrary propensities, preferences and intellectual prejudices in the formulation and assessment of new scientific ideas. These are strong intuitions which shape the profile of a scientific individuality and cannot enforce the consensus of a larger community of researchers in the field. So far, intuitions of this kind aren't able to guide towards an important and generally recognized scientific discovery, they seem to be nothing more than personal idiosyncrasies. Such preconceptions, which can hardly be analysed because they are seldom explicitly stated, could be responsible for deep disagreements between scientists of an excellent reputation, disagreements that cannot be overcome even when patience is present for removing possible misunderstandings and there is willingness to listen to and to examine attentively the opponent's arguments, as in the case reported above. One can only speculate about the sources of the preconceptions of a creative scientist: irrepressible biases deeply rooted in the core of a strong intellectual personality, particularities of education and of professional experience, the fascination exerted by some exemplar works and achievements in the history of science. Scientists often aren't aware of their own preconceptions and show little understanding for the preconceptions of their colleagues[1].

Impressive evidence on the role which can be played by the scientists' personal biases and *a priori* orientations not only as a guide of scientific inspiration but also as grounds in the assessment of new theoretical hypotheses are Einstein's reiterated attempts to legitimate his general theory of relativity, a theory which met sceptical reserve in many scientific circles. This case shows also a strong contrast between the grounds for accepting or rejecting a theory presented in standard scientific writings and the grounds for scientific assessment revealed by informal accounts and confessions of creative scientists as expressed in interviews, private conversations, correspondence, memoirs or writings of philosophical flavour.

The well-known French physicist and historian of science Pierre Duhem maintained near a hundred years ago that the agreement with experience is the unique criterion for the truth of a physical theory. This remained the widely accepted representation about the grounds of scientific assessment in Einstein's time. Einstein himself, in his standard expositions of the theory, refers to astronomical observations as grounds for trust in its truth (Einstein, 1917; Einstein, 1955). He speaks about a "confirmation with a remarkable approximation" of the prediction about the deflection of the light rays, in the gravitation field of the sun. Einstein's interest in a more conclusive empirical confirmation of his theory was obvious[2]. For he was convinced that this is the most accessible way to reach a wide recognition of the new theory in the whole scientific community. On the other hand, he was aware that many empirically oriented physicists had a very reticent attitude towards his theory because they saw in the actual astronomical observations an unconvincing support for it. There is abundant evidence in this respect. A reserved attitude was shared by eminent scientists which delimit themselves clearly from groups like the members of the famous *Arbeitgemeinschaft deutscher Naturforscher zur Erhaltung reiner Wissenschaft* whose resistance against this theory was mainly politically and racially motivated. So Max von Lane, Walter Nernst and Heinrich Rubens, all admirers and friends of Einstein, expressed publicly in a letter to the Berliner newspaper *Tägliche Rundschau* their disapproval of the antirelativistic campaign organised by this *Gemeinschaft*. However, an attitude of precaution towards the theory is evident in this letter. Its authors stated that the empirical verification of the theory remains a task for future research and add that irrespectively of the validation or rejection of the general relativity theory, Einstein's scientific contributions secure him a lasting place in the history of nature research. This is a position in full harmony with the decision of the Nobel Prize Committee to award the prize to Einstein not for his relativity theory but for his work in quantum physics. Doubtless, the Nobel Committee was concerned to signalize that the decision to award the prize to Einstein should not be interpreted as a scientific recognition of his general relativity theory[3]. And a great scientist like Walter Nernst, with much sympathy for Einstein, doesn't hesitate to make a much stronger remark with respect to one of the first discoveries made by the author of the relativity theory on the kinetic theory of molecules, published in 1905: "This discovery of Einstein is more important than his relativity theory because the relativity theory is philosophy and this is the single important scientific discovery" (M. Born, L. Infeld, 1967, 49 -my translation).

Such an attitude of reserve towards the general relativity theory from the part of the scientists which consider successful empirical tests as indispensable grounds for the validation of a physical theory was fully justified by the rather disappointing results of various attempts to obtain confirmations for the astronomical predictions of the general relativity theory. Measurements made by the English expedition in 1919 and by the American expedition in 1922 produced results relatively closer to the values predicted by the theory for the deflection of light in the gravitational field of the sun. In 1929 Freundlich made a new attempt to test the predictions of Einstein, with improved technical instruments and measurement methods, on the occasion of a solar eclipse in Sumatra. Unfortunately, the values obtained proved to be more remote from the predicted values than the results of previous measurements. Further measurements made by Freundlich until 1952 didn't succeed to produce conclusive results. In a survey of his work Freundlich maintains that the estimation of the measured values of the light rays' deflection is unavoidably subjective. Scientists who are *a priori* inclined to accept or to reject the theory will reach incompatible conclusions (W. Theimer, 1977, 142).

An estimation like this could be a hard blow for the proponent of a physical theory who was persuaded that a good confirmation of its quantitative predictions is the sole and supreme ground for trust in the truth of the theory. Despite some of his public declarations[4], Einstein was not such a man. The grounds of his firm belief in the excellence of the general relativity theory were of a marked personal nature. Since his youth Einstein nourished the conviction that mathematical simplicity is the very seal of truth in a physical theory. This was for Einstein a preconception, an *a priori* idea and in this sense a metaphysical belief. It guided the judgement and the scientific assessments of the author of the relativity theory during his whole scientific career and was expressed more and more categorically in his later years, especially in private communication. Einstein was inclined to estimate the general relativity theory as his most important scientific achievement because he saw in it a significant step in the direction of a deeper location of the foundations of physical science. The cardinal virtue of the theory was for Einstein the revelation of physical relations and structures endowed with the highest degree of invariance with respect to the position and the movement of the observer, structures described by equations which put themselves forward through their uncommon mathematical beauty and simplicity. These features of the theory sharply contrasted with the still more evident difficulty to produce unequivocal empirical tests in its support. It is obvious that the author of a physical theory like the general theory of relativity could be only a scientist with a profound philosophical motivation and a very strong personality. Einstein himself was perfectly conscious of what distinguished him from his colleagues[5].

It is noteworthy that Einstein had a perfect trust in the truth of his theory already in the final stage of its elaboration, before any test destined to confirm observational consequences derived from the theory. Already in this stage, in March 1914, Einstein wrote to his close friend, Michele Besso:

> I am now totally satisfied and have no doubts about the validity of the whole, matter if the observation of the eclipse will succeed or not. The grounds for the thing are only too evident (A. Einstein, 1978, 32 -my translation).

Even a considerable gap between the values predicted by the theory and the results of actual observations couldn't weaken Einstein's trust. For him the logical simplicity and the mathematical beauty of the theory represented sufficient guarantees for its truth. For truth must be simple and beautiful. Revealing for this specific attitude is a story reported by Ilse Rosenthal-Schneider (in a manuscript from 23 July 1957, quoted by the American historian of science, Gerald Holton). Ilse Rosenthal-Schneider, who studied at the time physics in Berlin, visited Einstein just in days in which he received a message from Eddington about the results of the measurements made by the English expedition. Einstein stressed in conversation that he knew in advance that the theory was right. She asked then what would he say if the gap between prediction of the theory and the astronomical observations made by the expedition had been much larger. Einstein answered: "Well, I would have been sorry for good Lord; the theory is right." Confronted with the conventional methodological conception which transpires from standard scientific papers an utterance like this one appears at least dubious. In those days and later Einstein seemed to be interested in results of new astronomical observations only if they represented confirmations of his theory. He regarded such confirmations as important not for convincing himself but for persuading the scientific opinion about the truth of his general relativity theory. On the whole, this attitude seems to illustrate a paradoxical saying of Eddington: "It is a good rule not to place confidence in the results of the observations as long as they are not confirmed by the theory." Einstein's correspondence in his latter days brings a conclusive corroboration of this credo. In a letter from January 1938 to Cornelius Lanczor he says:

> Starting with a sceptical empiricism which came more or less near to that of Mach, the gravitation problem transformed me in a faithful rationalist who looks for the only worthy source of confidence in mathematical simplicity (...) To convert oneself to rationalism means to look for the most simple mathematical form and to reach in this way the beauty. (A. Einstein, 1980, 86-87 -my translation)

With respect to the failure of Freundlich new measurements to meet the values predicted by the theory, Einstein expresses with unparalleled clarity his disagreement with the common view about the grounds for accepting or rejecting a physical theory with a high level of generality, in a letter to Max Born from May 1952:

> Freundlich in exchange does not move me in the slightest. Even if the deflection of light, the perihelial movement or lineshift were unknown, the gravitation's equations would be still convincing because they avoid the inertial system (this phantom which affects everything but it is not itself affected). It is really strange that human beings are normally deaf to the strongest arguments while they are always inclined to overestimate measuring accuracies. (A. Einstein, 1972, 206)

The conclusion is obvious: for Einstein the "inner perfection" of an extensive physical theory like the general relativity theory, namely its logical simplicity and mathematical beauty, says much more about its truthfulness that the confirmation of some of its empirical predictions. This is a marked personal option. Only the development of the physical theoretisation can give unequivocal indications about its relative fertility. In any case, the disclosure of preconceptions or *a priori* beliefs which sometimes set apart nature researchers who deserve the highest esteem, the revealing of their role in the assessment of theories and even of experimental results could lead to a more realistic, and a more differentiated understanding of scientific life.

Notes

1 A testimony in this sense can be found even in the controversy between Ling and the reviewers of his paper. It is Ling's passing observation:

> While I disagree to varying degree with almost all the statements Dr. Nagy expressed here, I nevertheless have sympathy for Dr. Nagy and other fundamentally innocent scientists who have been seriously let down by the eaders whom Dr. Nagy and others have trusted implicitly. (G.N. Ling, 1988, 912)

2 In a brief letter to the German culture minister in 6 December 1919, short time after the announcement of the observations made by the English expedition initiated by Sir Arthur Eddington during the solar eclipse in March 1919, Einstein requested the transfer of the astronomer Erwin Freundlich to the Postdam astronomical observatory, in order to offer him the opportunity to devote himself to the testing of the astronomical consequences of the general relativity theory (Albert Einstein in Berlin, 1979).

3 There are the significant remarks of Ph. Frank in his well-known book about Einstein:

> Toward the end of 1922, the Academy thought of a clever expedient by which it could award the prize to Einstein without having to take a stand on his relativity theory. It awarded the prize to Einstein for his work in 'quantum theory'. This work had not been so hotly debated as the theory of relativity. But in it 'facts were discovered' -that is, statements were advanced from which observable phenomena could be deduced by means of few conclusions. In the case of relativity theory this train of reasoning was much longer. The subtle distinction, however, authorized the Academy in the case of the photoelectric and photochemical law to speak of 'discovered facts' while it would not do so in the case of relativity theory. By this expedient the Academy succeeded to avoid the expression of any opinion about the controversial theory of relativity. (Ph. Frank, 1967, 202)

4 Herbert Feigl mentions that in his lectures on relativity theory held in Prague in 1920, Einstein said that, in spite of his "internal perfection", the new theory of gravitation would have been "dust and ashes" if measurement of the massive red shifts in the red spectra of light wouldn't come out quantitatively in accordance with the principles of the theory (See P. Feyerabend, 1972, 176)

5 Asked by the philosopher Hans Reichenbach how did it happen that he invented the relativity theory, Einstein replied: "Because I was firmly convinced of the general harmony of the world." The physicist Leopold Infeld, who was for some time Einstein's associate in the United States, reported a very interesting remark of the latter. To his observation that the special relativity theory could have been discovered by other scientists, Einstein's reaction was: "But the general relativity would have remained undiscovered." (M. Born, L. Infeld, 1967, 50 -my translation)

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Mircea Flonta, PhD. in Philosophy, professor for Theoretical Philosophy at the University of Bucharest. Research interests: epistemology, philosophy of language, philosophy of science, *Kant-Studien*. He has published *Necessary Truths? A monographical study about analicity, Philosophical Presuppositions in Exact Science, Cognitio - A Critical Introduction into the Problem of Knowledge, Images of Science,* and about 100 papers in romanian and international journals. Editor of *Theory and Experiment, Epistemology and the Logical Analysis of Language, Philosopher - King?*