Einstein and the Evolution of Twentieth-Century Physics*

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ABSTRACT. The history of science is discussed in the context of a continuous sequence of evolutionary, rather than revolutionary, paradigm shifts stressing the notion that threads of truth persist throughout the different periods of Kuhn's "normal science". Interpreting "paradigm shift" as continuously differing ways of looking at concepts in science, it is contended that the evolutionary model would be in agreement with Kuhn's theory. It is argued that one of the important signs of progress of scientific comprehension is that resolutions of questions must generate new questions. In this context, the role of authority in science is discussed in relation to scientific progress, and it is demonstrated with the "phenomenon of Einstein". By phenomenon of Einstein I refer to how the physics community regards Einstein as one of the most significant leaders of twentieth century physics yet rejects all his physics directions during most of his professional career. The paper concludes stating the importance of real freedom in scientific research, without authoritarian control, for true progress.

RESUME. On examine l'histoire de la science comme une séquence ininterrompue d'évolutions, plutôt que de révolutions, de paradigmes, en soulignant comme le fil de la vérité traverse les différentes périodes de la "science normale" de Kuhn. On soutient qu'en interprétant "changement de paradigmes" comme un changement continu de la façon dont on examine les concepts dans la science, le modèle évolutionniste serait en accord avec la théorie de Kuhn. On soutient aussi que l'un des signes importants de progrès de la compréhension scientifique est que chaque solution doit engendrer de nouveaux problèmes. Dans ce contexte, l'on discute le rôle de

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l'autorité dans la science par rapport au progrès scientifique, et on le met en évidence à l'aide du "phénomène Einstein" : la communauté scientifique le considère comme le leader le plus important de la physique du vingtième siècle mais rejette la plupart de ses idées pendant toute sa carrière. La conclusion de l'article insiste que la liberté dans la recherche scientifique, sans aucune forme de contrôle autoritaire, est indispensable pour un vrai progrès.

1. THE HISTORY OF SCIENCE

The history of science, according to Kuhn evolves in discrete stages where current paradigms are abruptly replaced with new paradigms at particular times in history. This "paradigm shift" is then said to constitute the "scientific revolution".

The full meaning of Kuhn's theory of the history of science is, of course, contingent on this meaning of the word "paradigm" [1]. On the one hand, it could refer to an ongoing set of concepts in science. On the other hand, it may refer to a particular way of looking at a given set of concepts. That is, the "paradigm shift" may refer to a radical change –a total replacement of particular scientific concepts. In physics, for example, such a radical change would be the replacement of Newton's action-at-a-distance concept of force between discrete quantities of matter, with Einstein's concept of fields of force propagating at a finite speed between nonsingular modes of a matter continuum field. Or the "paradigm shift" could refer to a change in the way of interpreting particular scientific concepts, such as the change of the interpretation of the inertia of matter in terms of an Aristotelian impetus to the Newtonian view in terms of the intrinsic resistance of matter to a change in its state of motion, as caused by external forces. This would be a nonradical interpretation.

In the discussion to follow I will assume the "radical" meaning of paradigm shift whenever I use the term "scientific revolution". Although Kuhn's meaning may be closer to the nonradical interpretation, I believe that it is the majority of the scientific community who understand by "scientific revolution" the radical change in the former sense.

This model of the history of science is analogous to the triadic theory of history (sometimes attributed to Hegel) whereby there is a shift from "thesis" –normal societal rules of government and behavior (the paradigms of the "normal science" period)– to "antithesis" –the breaking of normal rules of society (the scientific conflicts regarding the conventionally accepted rules and their predictions of ongoing science)– to the *irrational* "synthesis" –the political revolution, occurring discretely in time, overturning all the previous rules of society and government (the "scientific revolution").

Irrespective of the comparison with a history of society, I do not believe that this account is an accurate representation of the history of science. A closer look at the evolving scientific concepts that underlie the behavior of matter reveals that while this Hegelian account of the history of science seems to be valid "at at distance", a study from the perspective of the scientific ideas per se reveals that the view of "irrational changes" of ideas –the "revolution" (i.e., ideas totally disconnected from past ideas)– is illusory. I believe that rather than "scientific revolution", the history of science indeed proceeds in terms of evolutionary stages. My distinction between "revolution" and "evolution" is in the difference between *discrete change* and *continuous change*. With discrete change, one set of ideas is fully replaced with another set of ideas, totally independent of the original ones. With *continuous change*, there is an invariant thread of ideas that persists throughout all periods of history. Of course, not all the ideas of any particular period of history remain, but some do. In my view, it is this persisting set that entails the progress of our understanding of the real world during any particular period of history, although we can never predict which scientific ideas will remain and which will not.

It appears from his writing that Kuhn interprets a scientific revolution in terms of a superposition of continuous change and discrete change of ideas. My view, in contrast, is based on the idea that the *essential* evolution of scientific ideas is purely continuous, where the apparently discrete changes are only so because of the perspective taken to them, subjectively. From my reading of the history of science, our understanding of the real, material world has evolved sometimes slowly and, at other times, rapidly. However, it has always been continuous, whereby some of the ideas of science were rejected and others retained, although usually altered sufficiently to make way for a new view. In this way, some of the earlier ideas evolved *continuously* into later ideas in science and philosophy.

A notable example is the shift in the role of statistics in physics from the late nineteenth century to the middle of the twentieth century. In the analyses of Boltzmann in statistical mechanics and the kinetic theory of gases, there was a need to determine mathematical functions that would *weight* the various physical properties of the atoms and molecules of matter that make up a macroscopic solid or gas. But no claim was made that underlying these statistical analyses there was not a totally predetermined representation of the individual microscopic elements that constitute the macroscopic quantity of matter. Rather, the questions asked in a statistical analysis of matter by Boltzmann and his colleagues were in a *different context* than the questions regarding the foundational context, in terms of the laws of predetermined trajectories of particles of matter and their mutual interactions. Because the mathematical problem of determining the latter explicitly was untenable, a great deal of attention was focused on the statistical aspects of the description of matter, in Boltzmann's analysis, before the appearance of the quantum revolution.

Thus statistical analyses and forms of probability calculi in analyzing matter were in the mode of thinking of physicists in the 1920s, when quantum mechanics was discovered. The (apparently) discrete paradigm shift was then the assertion (by Bohr, Born, and Heisenberg, initially) that the statistical context *is* the foundational context. To accomplish this new (revolutionary) ontological outlook, it was found, interestingly, that the type of statistics implicit in quantum mechanics had to be more general than the preceding statistical theories. Quantum mechanics, as a theory of elementary matter, not only entails the calculus of probabilities of the states of an ensemble, but it also necessarily incorporates the probabilities of transition between these states –an extension that one does not have in the classical formulations in the standard earlier approaches to statistics per se.

With this generalized form of a probability calculus to underlie the formulation of a theory of microscopic matter, it is then claimed that there is nothing more to say about matter, at a foundamental level, other than that concerning the statistical context.

2. THE SIGNIFICANT QUESTION

Research in science and philosophy –the inquiries toward truths in these respective domains of knowledge– comprises two parts. One part consists of asking questions and the other part is the attempt to answer them. Certainly, asking questions is important, but more important is the asking of a *significant* question, that is, a question the answer to which could lead to genuine progress in our comprehension of the world. But how does one know at the outset whether a particular question is significant ? It is possible to ask an insignificant question in science or philosophy without realizing the degree of insignificance it has toward furthering our understanding of objective knowledge. One may then spend a lifetime in attempting to answer an insignificant question. Thus the philosophy of science should give us some criteria whereby one could determine if a particular question in science or philosophy is indeed significant.

One criterion that has occurred to me in this regard in this : If the attempt to answer a question leads to a partial resolution as well as one or more new questions, then the question is more likely significant than if the original question did not lead to new questions. If new questions did not follow, then we would appear to have a *complete* understanding of the subject. In my view, the complete understanding of any subject about the real, objective world has no bounds, i.e., it is infinite. On the other hand, since as human inquirers we can never attain infinite knowledge, we can then never have a *complete* understanding of any particular subject regarding the real world. [1] But it is only when our understanding is complete that we have no more questions. Thus I conclude that it is only if a question in science or philosophy leads to more questions that it is likely to be significant, and it should then be pursued further.

3. EXTERNAL CONTROLS IN SCIENCE

I believe that the foregoing is precisely how our understanding in science has evolved, *continuously*, from the earliest investigations of ancient times to the present.

There have been unfortunate periods of dogmatism in the history of the human race when "authorities" have declared that all the significant questions had already been asked and answered, that there is nothing new to be learned that would have fundamental significance. Of course, even in these periods of dogmatism there was usually agreement that new *facts* could still be discovered. But it was often assumed that the *fundamental concepts* that underlie these facts had been discovered, once and for all.

[1] Galileo made the following comment in his *Dialogues Concerning Two Chief World Systems*, S. Drake, translator (CA, 1970) : There is not a single effect in Nature, not even the least that exists, that the most ingenious theorists can ever arrive at a complete understanding of it.

A well-known example of dogmatism of this sort was the Inquisition, instituted by the Christian Church, in the Middle Ages. In Spain and the Middle East the intellectually exciting developments of Islamic and Jewish science, mathematics, and philosophy were truly stagnated. Later, in Western Europe, the Copernican revolution in science was severely pressured by the Inquisition to stop its free thinking and experimentation in science –most notably the attack of the Christian Church on Galileo [2].

Other such unfortunate examples of external controls on science and philosophy were in the twentieth century in countries dominated by the Nazi and Communist regimes. In this regard, it is indeed ironic that such totalitarian governments nevertheless encourage technological development that is generally a by-product of the foundational areas of science, while at the same time stagnating the free flow of ideas in foundational science itself.

In my view, there seems to be a great deal of misunderstanding, even in this day and in the free world, of the difference between technology per se and science per se. This is not only among the government and industrial leaders and the public, but also among the scholars in the sciences and the humanities. To say that in the final analysis fundamental science is motivated by technological development would be like making the erroneous claim that Galileo's fundamental studies of motion represented *his* aim toward the development of an intercontinental ballistic missile. [2]

4. INTERNAL CONTROLS IN SCIENCE

Another structure within society that controls thought in science is, surprisingly (to outsiders), the institution of science itself –the *profession* that is in the business of discovering new ideas in science. This, then, brings us back to the subject of Kuhn's analysis of the history of scientific change. The restriction on creative studies in science by the establishment of science itself is more subtle and, in the long run, more effective in halting true progress in our comprehension of the physical universe, more so than the political controls of totalitarian governments.

[2] In fact, Galileo did have other personal motives, in addition to his search for scientific truth, for its own sake, such as his prejudices because of his appeal for patrons [3]. Nevertheless, this may have been necessary at least in part in order to finance his research program but with an underlying motivation that was still pursuant of objective scientific truth.

Of course, this is because such controls are internal rather than external. This is a situation that is indeed "antiscience within science". [3]

On this subject it is usually faisely assumed by the public that anything a "scientist" does is "science". This assertion is clearly false, for science is a thing in itself –the pursuit of new, objective knowledge that increases our comprehension of the physical world in any of its domains. On the other hand, a "scientist" is a human being, capable of pursuing many avenues of activity in addition to science per se. For example, the scientist may exercise his or her scientific expertise in a field of technological development, a field that in itself is not science. A well-known example is the unified efforts by the majority of the world's fundamental scientists during World War II, on both sides, to develop war machinery : the nuclear bomb, missiles, etc. But these efforts were not related directly to fundamental science per se ; they were strictly in the field of technology.

As a human being, the scientist is also capable of exercising prejudices regarding the directions of scientific research. These prejudices are sometimes based on emotional reactions and could indeed be detrimental to progress in fundamental science. The sociological aspects of the history of science reveals this continual opposition to needed changes in the ideas of science, opposition that often has come from the leaders of the scientific establishment itself and many of its followers.

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[3] To exemplify such an attitude, consider the publication policies of contemporary physics journals. In an editorial statement written by S.A. Goudsmit in one of the leading U.S. physics journals [Phys. Rev. D **8**, 357 (1973)], he said : We occasionally receive a manuscript for which it is extremely difficult and sometimes impossible to find a suitable referee who is willing to read it. *This indicates that such papers are of little interest to our normal readership and should not be published in our journals.* The subject matter of these papers usually concerns a fundamental aspect of theoretical physics... The author proposes new theories, but their specific assumptions are usually hidden behind lengthy arguments... Some of these papers may have an important bearing on the philosophy of physics. However, since there exist excellent journals publishing articles on the foundations and on the philosophy of science, *we shall no longer accept papers of this type for the Physical Review.* [my emphasis] Note that the papers the editor is referring to are in the area of theoretical physics, not in philosophy of science per se. Thus such papers certainly belong in a physics journal rather than a journal of the philosophy of science. What he is saying then, as editorial policy, is that this physics journal will not accept such papers on theoretical physics only because they deviate from some preestablished norm on what theoretical physics is really about. In my view, this is a clear example of "antiscience within science".

Of course, there is literature on this aspect of the sociology of science, and writers, such as Kuhn, delve into its analysis. But there is a point I should like to add here in regard to the responses of the scientific community, particularly that of the contemporary physics community. In my view, the present-day physicist is generally *not* dogmatic about the ideas of physics. That is, I don't believe that there is *intellectual dogmatism* in the attitudes of the present-day physics community. Instead, the physicist is dogmatic with reference to the authorities who are to be endowed with full understanding and judgment regarding the ongoing research directions and future paths of inquiry.

To demonstrate this state of affairs in physics, a brief overview of research activity in this field reveals that gestures from its leadership toward any sort of change in the ideas of the day would lead the great majority of physicists, as well as the granting agencies, enthusiastically into those new directions of investigation. This would happen no matter how worthy or unworthy these particular ideas may be in regard to objective science, e.g., making predictions whether or not in agreement with experimental facts. A well-known example in present-day theoretical physics is the great deal of attention focused on the "string theories" of elementary particle physics. Worthy as this (currently speculative) theory may become in the future, it has still not predicted any new experimental facts in agreement with any observation. This is a complicated view of elementary particles in terms of an invented ten-dimensional spacetime, which still relies on the mathematical features of a renormalizable quantum field theory ; that is, its results still depend on the process of substracting infinites away to yield finite answers, though the latter is one aspect of modern-day quantum field theory that the string theory is trying to eliminate. In spite of its lack of concrete results thus far, after many years of research, it is an approach that has caught the attention of a sizable fraction of theoretical physicists.

Reciprocally, even with the strongest need for scientific change, based on inconsistencies between theory and experiment or demonstrated internal logical and mathematical inconsistencies, suggestions for a research program in a new direction to at least partially resolve these difficulties, though coming from a scientist who is not a member of its leadership, would normally be unrecognized by the scientific establishment. The reason is the blind faith of the majority of scientists in their leaders, not unlike the role of the high priests of past societies to their flocks [4].

5. THE "PHENOMENON OF EINSTEIN"

To demonstrate this point of the history of science, consider the "phenomenon of Einstein". The outside world looks upon Einstein as a *father figure* in modern physics. But this is certainly not the case in the community of physicists, in the approaches they have been taking during most of Einstein's life and to the present time.

To illustrate this dichotomy, I recall the following dialogue. I asked an active theoretical/experimental physicist the following question : "Who do you believe is the most significant physicist of the twentieth century ?" He responded immediatly, "Einstein". I then asked him, "Then why is it that you don't believe or trust any of the physics he followed in the last 40 years of his life (the latter three quarters of his professional career) ?" He responded just as quickly, "Because he was wrong !" I responded with this question : "If you believe that Einstein was wrong about the physics directions he took during most of his professional career, especially *after* he had developed the first quarter of his research experience, then why is it that you also believe that he was the most significant physicist of the twentieth century ?" There were no explicit answers to this question, except to repeat the sentence, "Well, everybody knows that Einstein was wrong !" This reaction to Einstein is typical of the community of physicists today. It is the sociopsychological paradox that I call the "phenomenon of Einstein".

If, on the one hand, the scientific approach that Einstein opposed – the Copenhagen school– had been conclusively proven from the stands of experimental confirmation *and* theoretical consistency, and on the other hand, he did not have an alternative approach to explain the outstanding experimental facts of the day, then it may seem reasonable to say that while Einstein did significant research in his earlier days, he had nothing to contribute in his later days. The physics community might then have had some justification for respecting Einstein's earlier contributions while not trusting his later judgment. However, in spite of the optimistic claims of the leaders of the physics community, this was not the case.

Firstly, the physics that Einstein (as well as a few notable colleagues, such as Planck, de Broglie, and Schrödinger) opposed was not well established in many respects, as I will discuss below. Secondly, Einstein did have an alternative approach to a fundamental law of matter based, primarily, on the continuous, deterministic field concept in general relativity –though it wasn't fully worked out yet. (Planck, de Broglie, and Schrödinger also had alternative approaches, each opposed to the Copenhagen school).

Then why was it that Einstein's alternative approach was automatically rejected by the physics community (as were the other approaches, particularly Schrödinger's, who also had a specific program for the quantum formalism, different from that of Bohr and his collaborators) ? In my view, it was that a "school of thought" in science was set up –the "Copenhagen school"– that gave physicists such a strong psychological crutch that it effectively became the *only* school in physics. Why was it, then, that Einstein (or the others) did not have a following in the form of an alternative school in physics ? I believe that this is an interesting question for social psychologists and historians of science to investigate. But the fact that there never was an "Einstein school" (nor any other "school" in twentieth-century physics) was, in my view, the main cause of the "Einstein phenomenon", as I understand the history of contemporary physics.

Exploring this dialogue further, most physicists disagree with features from Einstein's later work, from the last 40 years of his life. In my view, there were two points of his physics and one of his epistemology that most of the leadership of physics have never accepted. First, in his physics Einstein rejected the Copenhagen interpretation of the quantum theory as a fundamental theory of matter. Secondly, he devoted a great deal of his research effort to his view of a unified field theory. This was an approach to a fundamental explanation of the basic nature of matter in terms of the *continuous field concept*, in the context of the theory of general relativity –applied to *all* domains of physics. On rigorous grounds of logical and mathematical structure, the basis of the quantum theory and that of general relativity (according to Einstein's meaning of the latter) are incompatible [5]. The physics community has overwhelmingly accepted the quantum view, thus rejecting the basic view of general relativity as fundamental toward an explanation for the behavior of elementary matter.

Only in the relatively recent past have physicists started to talk about "unification" of the forces in nature. But this has not been in the sense that Einstein (and, originally, Faraday) meant. The latter view is that all natural forces must follow from a common field theory, as manifestations revealing themselves, seemingly separately, only because of the manner of observation of these physical effects –for example, the revelation of a purely electric force (or a purely magnetic force) due to the lack of motion (or the motion) of an observer relative to the charged matter that is observed– though objectively following from a single, unified electromagnetic field of force. The "unification" of present-day elementary particle physics is instead, as in the current attempt to formulate GUT (grand unified theory), based on a phenomenological approach in which one generalizes the quantum descriptions without abandoning any of its pluralistic bases, i.e., its logic is *inductive*. This is a view of theoretical physics as descriptive rather than explanatory, that is, it does not derive unification from first principles *deductively*, as Einstein's approach requires, as in the derivation of the unification of electricity and magnetism from the underlying principle of special relativity.

The physics establishment has never accepted the validity of Einstein's idea of a unified field theory. Even though elementary particle physicists today use the words "unified theory" along with the jargon of particle physics, *these names are only meant in the context of quantum mechanics*, rather than Einstein's context of a nonsingular, continuous field theory based on the principle of general covariance. Even so, it is indeed curious to me that many elementary particle physicists today, who discuss unification of the forces of physics in this way, still refer to Einstein, rather than Bohr and/or Heisenberg, as inspirational for their approach. Much closer to the ideas they are in fact pursuing is the Copenhagen school. That is, Einstein's disagreement with present-day particle physicists is due to their investigations being in the context of the quantum theory and its interpretation in terms of the Copenhagen school, while his approach was in a *different context* –that of general relativity as explanatory toward the behavior of matter in any domain– from elementary particle physics to cosmology.

Then what is the difference between these contexts ? The answer lies in the difference between the epistemological approaches of Einstein and the Copenhagen school. Einstein's philosophy of physics is in terms of *realism* –the idea that there is a real world, independent of any particular observer's view of it. Einstein expressed his realistic view in many places and in different modes of expression. For example, he said the following in 1952 : There is something like a "real state" of a physical system which does exist objectively, independently of every observation or measurement" [6].

Einstein's ontology was, of course, not the type that entails the Newtonian model of matter in terms of a system of material atoms tracing out their predetermined trajectories. Rather, his reality was in terms of the continuous field concept. This is an abstract form of realism – "abstract" because the reality of the world was to be taken in terms of not directly perceivable continuous fields, distributed in space and time *everywhere*, rather than being localized things of the classical atomistic theories. [4]

Schrödinger, who came close to Einstein's philosophy of physics, called the variables of Einstein's field theory "matter fields". These are the continuous variables that characterize the real material world most primitively, viewed as a closed system rather than an open system of separable things. It then follows that such matter fields necessarily solve nonlinear differential, inhomogeneous equations as the most basic forms of the laws of nature.

The epistemological approach of the Copenhagen school, led primarily by Bohr and Heisenberg (though they differed on points), was in essence that of *logical positivism.* This is an approach to knowledge based on the assertion of the *principle of verifiability* –the idea that the only meaningful statements about matter (verbal or mathematical) must be directly verifiable in observations. The Copenhagen school then interprets "directly verifiable" as relating to measurements by a macroapparatus of the physical properties of micromatter. Because of the way this theory of matter has developed, the formal expression of quantum

[4] A different view of Einstein's realism in terms of a reinterpretation of the wave function of quantum mechanics to represent a statistical ensemble of elementary particles (electrons, quarks, etc.) –with a "fuzzy" image in reality– is discussed in A. Fine, *The Shaky Game* (Chicago, 1986). In my view, there is a similarity here with the version of realism attributed to Einstein's outlook by the followers of the hidden-variable theories. These views are incorrect in that they presume that Einstein's ontology was in terms of localizable (or almost localizable) particles of matter to comprise a material system –whether fundamentally statistical or not. But this is not at all true, for Einstein's "reality" was actually in terms of continuous, nonsingular fields, rather than localized "things". These were fields to represent a *closed system* ; thus they are not separable. The nonseparability of the constituent fields of the closed system is indeed more crucial to a characterization of this system than is the feature of localization.
To emphasize his adherence to the continuous field concept, Einstein wrote the following to D. Bohm in 1953 (Einstein, Archives, Jewish National and University Library, Jerusalem, Call N̊4, 1576:8-053) : "When one is starting from the correct elementary concepts, if, for example, it is not correct that reality can be described as a continuous field, then all my efforts are futile, even though the constructed laws are the greatest simplicity thinkable".

mechanics is that of a theory of measurement ; its solutions do not represent the dynamics of the measurement itself, but rather they relate to the states of a system before and after a measurement is carried out. These solutions, the "probability amplitudes", are complex functions the absolute squares of which relate to the probabilities of measuring the material system in one state or another.

Quantum mechanics is a probability calculus primarily because of the loss, in principle, of causality in the way of representing the measurement process. This follows from the claim of this theory that there can be no reciprocity between the action of the measuring apparatus on the measured micromatter and the reaction, in turn, of the apparatus to the micromatter. Thus it follows that the laws of micromatter, according to the Copenhagen school, are particular *acausal* sorts of measurement rules that are laws of probability.

We see, then, that the context of quantum mechanics –the view accepted today by the physics community– and the context of Einstein's theory of general relativity, as a general theory of matter, are indeed mutually exclusive. It has been established at the present stage of physics that the quantum theory, extended fully to a "relativistic quantum field theory", has serious logical/mathematical difficulties [7]. Then why does the physics establishment prefer the Copenhagen context to Einstein's context ? Why is this so in spite of the facts that 1) the quantum theory has not yet (objectively) proven itself in a sufficiently conclusive way, free of logical and mathematical inconsistencies, and 2) many physicists nevertheless respect Einstein's physical intuition enough to refer to him as the most significant physicist of the twentieth century ?

The standard answer given by physicists is that the empirical facts support the rules of quantum mechanics. However, it is well known that the philosophy of science teaches that while an alleged theory in science must *necessarily* predict the empirical facts correctly, this is *not sufficient* to claim the truth of the theory. The proposed theory must also be logically and mathematically consistent. To this stage in the history of physics the quantum theory has not yet succeeded in satisfying these criteria. One of the primary reasons for this failure is that the formal expression of quantum mechanics has never been fully unified with the mathematical requirements of the theory of special relativity (minimally) –in the form of a mathematically consistent relativistic quantum field theory. This is a requirement of the quantum theory itself, that is, on is own terms. [5]

Of course, the physics community is aware of this difficulty, which has persisted since the onset of quantum mechanics in the 1920s. If this is the case, then why has such dogmatic belief in the Copenhagen interpretation persisted along with the (sometimes irrational) oppositions to competitors such as Einstein and Schrödinger ? I do not believe that the answer is that physicists are religiously committed to the Copenhagen view. What I do believe is that modern-day physicists set up their leaders, whom they choose for the various reasons, including brilliant "track records" of past works, and then follow their directions only, in physics research, irrespective of the objective facts of science.

The historians of science who follow the "standard view" say that this is the way it is with scientists. It is a law of nature about the workings of the scientific community, just as much as the Newtonian law that predicts the orbit of a comet. But this is surely not the case. The societal laws entail many more variables than the Newtonian law of a moving body. Indeed, it is not impossible that attitudes can change toward a more progressive method that entails more freedom of exploring diverse avenues, without confining ourselves to a given method of inquiry

[5] The primary technical reason that the "relativization" of the quantum theory (in the form of "relativistic quantum field theory") breaks down, yielding a formalism without any solutions, is that infinities are automatically generated in this formal expression of the theory. In later years, "renormalization" methods were discovered that subtract away these infinities with other infinites, to yield the finite solutions that were then successfully compared with data. The trouble is that such a mathematical scheme is not demonstrably mathematically consistent. This problem has never been resolved, to this time. In his later years, Dirac made the following comment on this problem ["The Early Years of Relativity", G. Holton and Y. Elkana, editors, *Albert Einstein : Historical and Cultural Perspectives* (Princeton, 1982), p. 85] : It seems clear that the present quantum mechanics is not in its final form. Some further changes will be needed, just about as drastic as the changes made in passing from Bohr's orbit theory to quantum mechanics. Some day a new quantum mechanics, a relativistic one, will be discovered, in which we will not have these infinities occurring at all. It might very well be that the new quantum mechanics will have determinism in the way that Einstein wanted. The determinism will be introduced only at the expense of abandoning some other preconceptions that physicists now hold. So, under these conditions I think it is very likely, or at any rate quite possible, that in the long run Einstein will turn out to be correct, even though for the time being physicists have to accept the Bohr probability interpretation, especially if they have examinations in front of them.

in science, and where free thinking in science is not only tolerated but encouraged. [6]

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References

[1] The different concepts of paradigm are discussed in T.S. Kuhn, *The Structure of Scientific Revolutions*, 2nd ed. (Chicago, 1970), p. 174.
[2] An accurate account of Galileo's interaction with the Inquisition is given in P. Redondi, *Galileo Heretic*, R. Rosenthal, translator (Princeton, 1987).
[3] See, for example, R.S. Westfall, Isis **76**, 11 (1985).
[4] An excellent text that discusses the societal motivations of the modern-day elementary particle physics enterprise is A. Pickering, *Constructing Quarks : A Sociological History of Particle Physics* (Chicago, 1984).
[5] This dichotomy is discussed in detail in M. Sachs, *Einstein Versus Bohr : The Continuing Controversies in Physics* (Open Court, 1988), Chap. 10.
[6] Einstein, in *Louis de Broglie Physicien et Penseur*, Albin Michel, editors (Paris, 1952).
[7] See, for example, P.A.M. Dirac, *The Principles of Quantum Mechanics* 4th ed. (Oxford, 1958), Sec. 81; R.P. Feynman and A.R. Hibbs, *Quantum Mechanics and the Path Integral* (McGraw-Hill, 1970), Chap. 9 and p. 260.

[6] On the idea of freedom of choice of the *methods* of scientific inquiry, I am in full agreement with P. Feyerabend [*Against Method* (Verno, 1975], implying, of course, that no particular method should be considered unique in pursuing scientific truth.