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On Dingle's Controversy about the Clock Paradox and the Evolution of Ideas in Science†

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In his recent book, *Science at the Crossroads*, H. Dingle (1972) reviewed his extended controversy on the clock paradox of special relativity theory. For reasons that I will discuss presently, I do not agree with Dingle's conclusion that the theory of relativity actually entails a paradox, and therefore I do not agree that it is logically invalid. However, I do believe that it should be advisable to pay close attention to what Dingle says in his book, especially where he also discusses the present-day attitudes of dogmatism and anti-rationalism that he has experienced from the physics community, following from this controversy. If he is right about this, then it seems to me to be equally serious for the community of physicists to consider, along with the particular physics problem that he debates. For Dingle's remarks on the sociological problem imply that indeed science is 'at the crossroads', and that a continuation of such attitudes could lead to a severe slowing up of genuine progress in physics, if not stagnation.

I should like to address myself, in this paper, to both of the problems posed by Dingle—the logical-physical problem of relativity theory, and the sociological problem. First, I will present what I believe to be the actual answer to Dingle's criticism, from the view of the theory of relativity—which (in contrast with the opinion of most of my colleagues) I believe to be a bona fide technical criticism that must be answered in a technical manner. I will then discuss my view of the nature of the evolution of ideas in science, attempting to place present-day attitudes in historical perspective, and indicating what I feel to be suggested by the history of science in regard to a methodology that would be in the best interests of its future progress.

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I should like to preface may remarks by mentioning the phenomenon, quite curious to me, that Dingle's conclusion–that the theory of relativity is invalid–has aroused a great deal of emotion, making many physicists quite angry! An equally emotional reaction ensued from my findings on this problem (Sachs, 1971, 1972), but not because I came to Dingle's conclusion that the theory of relativity is logically refuted. On the contrary, I claim that by fully exploiting the conceptual basis of the theory of relativity, one must conclude that this theory, *per se*, does not predict asymmetric ageing, from any frame of reference, and therefore that there is no logical paradox. The anger I seem to have stirred is rather due to a disagreement that my colleagues have with my claim of no logical paradox as a consequence of the meaning and implied use of the space and time parameters, according to the theory itself! This is an interpretation that is in contrast with the classical view–which it appears to me my colleagues are evoking in the name of the theory of relativity–although with the modification that the classical (ontological) lengths and times are deformable, in a way similar to Lorentz' interpretation of his transformations to fit the experimental results of Michelson and Morley. But such a view of space and time is not the view of Einstein's theory of relativity!

The comments I will make below, in the second part of this paper, on scientific revolutions, may equally arouse anger. But it is not my purpose to do so. I am interested only in objectively pursuing what it is that is scientifically valid and what is not–for the sake of science itself, and for the sake of suggesting a methodology that could be conducive to progress.

1. *An Answer to Dingle's Question on Special Relativity*

In his book, Dingle (1972) rejects the validity of special relativity theory because of his failure to find an answer to a question that arises as follows: A and B are physically identical clocks, synchronised and initially relatively at rest. Suppose now that clock A should be put into a different inertial frame in uniform motion at the velocity v relative to the clock B, whose frame of reference we will call 'stationary' for this description. Then according to the common consensus among physicists, the hands of the clock A in this state of motion, relative to the hands of the clock B, should be slow, according to the Lorentz transformation formula, $T_A = T_B(1 - (v/c)^2)^{1/2}$. But according to the axiomatic basis of this theory–the principle of relativity–'motion' is strictly a subjective entity in the description of physical phenomena. Thus, it should not matter if the comparison of the clock readings is made from the frame of reference of clock A or from that of clock B. In the latter description, it would be A's reference frame that would be called 'stationary', and B's frame of reference would be said to be in motion relative to A, at the speed of $-v$ cm/sec. In the latter case, the clock readings should then relate to each other according to the Lorentz transformation formula, $T_B = T_A(1 - (v/c)^2)^{1/2}$, which would then be interpreted to mean that the hands of clock A should be fast compared with the reading of clock B.

According to this analysis, then, it appeared that the theory of relativity implies a logical paradox—that clock A will run both slow and fast compared with clock B. If the theory of special relativity is indeed a bona fide law of nature, then it must be logically consistent. This requires the prediction that *at all times*, one of the clocks can be only slow, or only fast, compared with the other, or that the clocks remain synchronised, irrespective of their relative motion.

Rejecting the latter possibility within his interpretation of the framework of the theory of relativity, Dingle then asks: Which is the clock that runs slow compared with the other? Not seeing a satisfactory answer to his question, Dingle then concluded that the paradox is necessarily implicit in the theory of special relativity, and therefore that the theory is false. But the scientific community was understandably reluctant to accept Dingle's conclusion, for a few reasons. A primary one is the enormous success that the mathematical formulation of the theory had in predicting and explaining observable effects in modern physics. Still, I must agree with Dingle that mathematical successes, while necessary, are certainly not sufficient to establish the validity of a law of nature. For a theoretical structure to be valid as a law of nature, it must also be logically consistent.

From my studies of this problem (Sachs, 1971) I do not believe that the theory of relativity is logically inconsistent. The reason for Dingle's conclusion, as I see it, is a false interpretation of the inequality of time scales in relatively moving inertial frames, $t \neq t'$, that was presented in the early stages of development of this theory. At that time, Einstein identified the abstract time parameter, t, *directly*, with a physical duration of a material mechanism. But the way in which the time dilation arose in the theory of relativity in the first place had nothing to do, directly, with the evolution of a physical mechanism. It rather appeared as an abstract parameter contraction, whose only purpose was to facilitate an objective description of a law of nature, the Maxwell field theory of electromagnetism—the first discovered law exhibiting special relativistic covariance. The basic axiom of this theory—the principle of relativity—requires that it should be the law of nature that is the cause-effect relation. The solutions of the equations that represent the laws of nature, rather than the independent parameters, such as t, then relate to the predictions of physical effects.

The explicit statement of the principle of relativity is: *all laws of nature, as expressed in any particular coordinate frame, must be in one-to-one correspondence with the expressions of the same laws in any other relatively moving coordinate frame, as determined from the given particular coordinate frame.* The only logical role of the transformations, such as $t \rightarrow t'$, between relatively moving frames of reference, is to ensure that such objectivity in the forms of the laws of nature will be maintained.

To predict any physical effect in a moving frame of reference, such as ageing (which might be the unwinding of the steel spring of a clock, biological ageing, the decay of mu mesons, and so on) one must then follow this procedure: First, determine the correct transformations of the temporal and spatial parameters, between the respective frames of reference. If the relative motion is uniform, these are the Lorentz transformations. Next, insert the transformed coordinates into the laws of nature, as expressed in the moving frame, relative to the frame that is called stationary. Finally, these equations, as described in the relatively moving frame of reference, must be solved, and the solutions used in a prescribed way, in accordance with the structure of the theory, to predict the physical effects that are appropriate to that particular law of nature—such as the effect of the unwinding of the spring of a clock. Of course, one always has the freedom to calibrate a time scale t, that he sets up in his own frame of reference, as a standard to correspond with the evolution of some physical process in this frame. But unlike classical mechanics, where $t = t'$, so that this standard would remain independent of the frame of reference, the correspondence as a standard would not generally carry over in relativity physics to the description of the physically evolving process in the moving frame, since here $t \neq t'$, *and the Lorentz transformation relation between* t *and* t' *is not a physical cause–effect relation.* To determine the actual evolution of the physical process in the moving frame, according to relativity theory, one must proceed with the method outlined above, utilising the explicit laws of nature. Thus, according to the logical structure of the theory of relativity, the inequality of time scales in relatively moving frames of reference does not refer to any asymmetry in the evolutions of physical processes in the respective coordinate frames, such as the unwindings of springs of clocks that are in relative motion.

Indeed, the temporal and spatial coordinates, as used in the theory of relatively, are not more than the 'relative' (i.e. subjective) elements of a language, used to facilitate and expression of (objective) laws of nature. The role of the transformations in this theory is not any different, in principle, than the role of language translations, for example in comparing the sentences: 'le ciel est bleu' and 'the sky is blue'—*to express the same physical fact.* But the correspondence between the words and the syntax of one sentence and the other is not in itself a physical cause–effect relation. In the same sense, the Lorentz transformations are not physical cause–effect relations. Hence, in the example mentioned above, the relation of the contracted time scale T_A to the time scale T_B is not a cause–effect relation that implies a physical effect—the slowing down of the unwinding of the spring of the clock A compared with the spring of the clock B. The Lorentz transformation is only used if one wishes to determine the physical properties of a mechanism that is in motion relative to his own coordinate frame. To do so, it requires him to use a contracted time scale, as a parametric set of 'words' in the language of the law of nature that predicts the actual unwinding of the spring of the clock that is in motion relative to his own frame of reference.

It follows from this analysis that so long as there is no extra force entering the system, to physically act on the moving matter, but not to act on the matter in the observer's frame of reference (or vice versa), then there can be no asymmmetric ageing—according to the logical structure of the theory of relativity itself! There is no paradox because there is no asymmetric ageing predicted *from any frame of reference.* Thus, if physically identical clocks A

and B are initially synchronised, they should maintain their synchronisation at all future times during the relative motion.

When one measures the difference in frequencies from identical oscillators that are in relative motion (i.e. the relativistic Doppler effect) it is because 'frequency' is a number of cycles per 'second', and the 'second' is here a frame-dependent quantity in the parametric representation of the measurement. But the ageing, in this case, rather refers to the *number* of cycles—that might be marked off on a tape during some interval—and this quantity is invariant. Similarly, there are other frame-dependent quantities, such as the *rate* of unstable particle decay, or the measures of the separate components of the energy-momentum of a swiftly moving elementary particle. All of these types of observations have well-verified the predictions of special relativity theory. But none of these quantities are physical ageing, *per se.* The different values for these measured quantities in different relatively moving frames of reference are due strictly to the fact that the measurements are made from a moving platform, relative to the described matter or radiation—they are not intrinsic physical changes of matter in motion, such as the unwindings of springs or the contractions of rigid rods, as claimed by the majority of physicists today (Sachs, 1969).

In a recent mathematical study of this problem (Sachs, 1971) I have found from a general functional analysis, using the most general (unambiguous) expression for the differential metric in four-space, with a Riemannian geometry, and without using any approximations or special models, that the total ageing of a physical mechanism is predicted to be independent of the path that might be traced out in space-time. This result, obtained with a general relativistic formulation (also incorporating any conclusions of special relativity theory alone, as this limit is asymptotically contained) then implies that the theory of relativity, in itself, does not predict the asymmetric ageing effect.

The result was obtained by showing that the path integral, $\oint_{s_1}^{s_2} ds$, between any two space-time points, s_1 and s_2, taken over the path C, is independent of this particular path. That is, for any two paths in space-time, C and C', $\oint_{s_1}^{s_2} ds = \oint'^{s_2}_{s_1} ds$. Thus, one starts out by making a calibration of the abstract time scale, ds, as a standard corresponding with the physically evolving process of some mechanism in the proper frame of reference. If then the physical mechanism that takes the alternate path C' is identical with the physical mechanism that takes the path C, between the same space-time points, s_1 and s_2, then the equality of the path integrals—as exact geodesics in space-time—must correspond to the equality of the physical ageing of the respective physically identical mechanisms. In this analysis, there is no comparison made from a given frame to other relatively moving frames. One only compares the proper times for two different paths in space-time, using the feature of general relativity theory that a body will move along a geodesic path, as determined by the solutions of the general form (unapproximated) of the metrical field equations.

The reason that there are different geodesics in this problem is that there are different source terms of the metric tensor field g^{ab} for the respective paths C and C' of the relatively moving clocks. In Einstein's equations, these would correspond to the energy momentum tensor $T_{ab}^{(1)}$ for, say, the clock that stays on Earth, and $T_{ab}^{(2)}$ for the clock that might depart in a rocket ship, later to return. The derivation of the geodesic equation is independent of the explicit form of T_{ab}. But the solutions of the geodesic equation depend on g_{ab}, and therefore they depend implicitly on T_{ab}. Thus, two separate metric tensor solutions, $g_{ab}^{(1)}, g_{ab}^{(2)}$, are due to two distinct source terms, $T_{ab}^{(1)}$ and $T_{ab}^{(2)}$. The reason that the clock will move along one of these geodesics, say the one determined by $T_{ab}^{(1)}$, is that, in accordance with the definition of this tensor, each point along the geodesic path corresponds to minimal energy-momentum for the body. That is, in accordance with the theory of general relativity, we are describing each of these motions—the one along C and the one along C'—as the free motion of a body in a space-time whose geometry is completely determined by T_{ab} for this body.

The equality of the path integrals does not mean that so long as the clocks are not brought together in the same inertial frame, relatively at rest, that they would be out of synchornisation, and then, suddenly and mysteriously, their synchornisation would return when they would come back together. The result implies that *so long as there is no extra force acting on one of the clocks and not the other*, the two clocks would not be out of synchronisation at any stage of their respective histories in space-time. In the clock problem, there are no extra forces evoked in the prediction of asymmetric ageing. It is only the Lorentz transformation that is evoked to predict the effect, or to claim (as Dingle does) that while the clocks are in relative motion, the paradoxical conclusion must follow from the theory that one of these clocks would be both slow and fast compared with the other. These are illogical conclusions because the Lorentz transformation (or the transformations of general relativity theory) are not cause–effect relations. That is, a physical effect is claimed here without a logically connected physical cause.

To sum up, my analysis on this problem indicates, on the basis of a rigorous mathematical formulation of the theory of general relativity (whose results incorporate those of special relativity), as well as an analysis of the logical structure of the theory, that there is no prediction of asymmetric ageing, and there is no logical paradox in the physical predictions of the theory.

I believe that Einstein's identification of the Lorentz transformation with a physical cause–effect relation, and the subsequent conclusion about asymmetric ageing, was a flaw, not in the theory of relativity itself, as Dingle believes, but rather a flaw in the reasoning that Einstein used in this particular study—leading him to an inconsistency with the meaning of space and time, according to his own theory. If Einstein (and almost the entire physics community) had not been so in error, then I would have to agree with Dingle's conclusion about the invalidity of the theory; for his criticism—that the logical paradox would be implicit in the theory of special relativity—would then remain unanswered. *Note added in proof.* I do not believe that Einstein fully maintained his initial interpretation in the later years when he was developing his theory further. In his 'Autobiographical Notes', in *Albert Einstein–Philosopher –Scientist* (Library Living Philosophers, Evanston, ed. P.A. Schilpp, p. 59), Einstein made the following remark: 'Speaking strictly, measuring rods and clocks would have to be represented as solutions of the basic equations (objects consisting of moving atomic configurations) not, as it were, as theoretically self-sufficient entities.' But he then went on to excuse the earlier analysis with what appears to me as a logical *non-sequitur,* as expressed in the following: 'However, the procedure justifies itself because it was clear from the very beginning that the postulates of the theory are not strong enough to deduce from them sufficiently complete equations for physical events sufficiently free from arbitrariness.'

2. *Comments on Scientific Revolutions*

The magnitude of Einstein's error in his conclusion about asymmetric ageing, during the very early stages of relativity theory, is, in my mind, infinitesimal in comparison with the greatness of his discoveries, not only the theory of relativity (special and general), but in all significant aspects of twentieth-century physics. It is indeed a strange commentary on the physics community of the contemporary period, that while it has dogmatically upheld Einstein's original comments about asymmetric ageing, he was almost universally opposed on (what seems to me) the much more important aspect of modern physics having to do with a direction he advised (at least) some should investigate, to resolve the problem of matter in the domain of quantum physics. Indeed, the approach of the *Copenhagen school*, which Einstein vigorously opposed for logical as well as intuitive and aesthetic reasons, enlisted almost the entire physics community—even though (to this date) the Bohr–Heisenberg view has not been successfully extended so as to yield a demonstrably (mathematically and logically) consistent relativistic quantum field theory. Neither have most of the contemporary theoretical physicists, who are called 'relativists', followed Einstein's interpretation of the theory of relativity, which is a view that necessarily leads toward a general theory of matter, based on the unified field approach.

An interesting and significant current controversy among historians and philosophers of science on the structure of scientific revolutions was instigated by recent studies of Kuhn (1970). Some of this controversy is presented in the volume edited by Lakatos & Musgrave (1970). I believe that in addition to the contribution of this controversy to an increased understanding of the history of science, it can also be beneficial to those who are directly engaged in scientific research—especially at times when judgement must be exercised on choices of alternate paths of inquiry to explain particular phenomena. Thus, I wish to add a few comments here from the point of view of a physical scientist, rather than a philosopher or historian of science.

As I read Kuhn, he sees the history of science to be somewhat analogous to an electron's history in a linear accelerator. For most of its journey, the electron coasts at constant speeds, in the different sections of wave guide, analogous to Kuhn's 'normal science' periods. During these periods of the electron's motion, there are fluctuations from the constant speed, analogous to the conflicts that build up in the history of science, during the 'normal science' periods. But the electronic equipment is designed to force the electron to accurately restore and maintain its constant speed, analogous to the pressures of the scientific community to maintain the ongoing paradigms in science. Then, over relatively short times, analogous to the periods of scientific revolution, the electron is greatly agitated as it passes through an electrostatic potential, before being sent on its way in the next section of wave guide, with increased energy.

Applying the Hegelian dialectical theory of history to science, one might then view the 'normal science' period as 'thesis', the build-up of conflicts in this period as 'antithesis', and the new paradigms arising with the scientific revolution (i.e. just after the period of agitation) as 'synthesis'. This view would contend, with Feyerabend (1970) that no conceptual residue of the earlier periods of 'normal science' would carry over into the succeeding periods of 'normal science'–the earlier ideas should be totally eliminated by the succeeding scientific revolutions.

On the other hand, the application of Hegel's philosophy to the history of science can be questioned. Indeed, I believe that one can show evidence that as far as the abstract concepts of theoretical physics are concerned, there are threads of scientific knowledge that persist throughout the different scientific revolutions, analogous to the persisting electron, following through all of the sections of accelerating regions of the linear accelerator. A well-known example of a persistent concept in physics is that of the inertia of matter, even though the concept has been continually modified since the earliest times of antiquity.

To obtain a full understanding of scientific revolutions, I believe that one must add to Kuhn's view some essential human factors; in particular, the attitudes of scientists toward new ideas, in the different periods that lead up to scientific revolutions.

Just after a period of great agitation (the revolution) most scientists are in a liberal frame of mind. They are still aware of the progress that had so recently been achieved from the open-minded attitudes of those who had participated in the revolution. As time goes on, during the next generation of scientists, in the first part of the coasting period a large fraction of scientists becomes increasingly conservative, as they correspondingly develop increased emotional security with the ideas they have come to believe in. During this 'normal science' period, there develops a healthy balance between the conservative and the liberal elements in the scientific community. But then, in the latter part of the coasting period, a large portion of the conservative element solidifies into a reactionary element–i.e. a group who are unwilling to hear about new ideas, even within an existing theoretical framework that may exhibit ever-increasing conflicts.

There are two sorts of conflicts that arise, which (all would agree) play the role of the seeds of the scientific revolution that is to come. These are: (1) conflicts that are generated by experimental facts that seem to have no explanation within the existing paradigms, and (2) conflicts concerned with a breakdown of logical consistency–for example, when two theories that were consistent schemes to describe supposedly mutually exclusive phenomena, but together have logically dichotomous axioms, must be unified to explain some new data. A well-known example of the latter in present-day physics is the attempt to unify the quantum theory with the theory of relativity, in order to explain the data relating to elementary particle physics.

Such conflicts can remain for a very long time, in principle indefinitely, without a scientific revolution taking place. For before the revolution can start, the period of conflict must lead to a period of controversy. That is to say, the leaders of the scientific community must be willing to (1) admit the existence of the conflicts, (2) rationally discuss and debate them, and (3) open their minds to the possibility of rejecting 'established knowledge', and accepting new notions in its place.

Kuhn contends that when the weight of these conflicts becomes sufficiently heavy, it must necessarily produce such controversy, and hence a scientific revolution. This conclusion would follow from the tacit assumption that the primary values and motivating drives of scientists are based on altruism, objectivity, and a search for scientific truth for the sake of knowledge alone. I believe that there is some truth in this judgement. But it is not wholely true. From my view, as a theoretical physicist, rather than an historian of science, I would contend that during the 'normal science' period, the leaders of the scientific community, as well as most of their followers, acquire vested interests and a strongly emotional attachment to the ongoing paradigms about the way the world is, in their view. A state of dogmatism is then reached in which it is literally impossible for most of them to give up these ideas—in spite of any quantity of experimental and/or logical inconsistencies that may pile up. Still, in the history of science, there has always appeared, during these periods of dogmatism, a very small number of scientists who do not find this so impossible. (Similar observations to these were expressed earlier by Max Planck, in his autobiography.) Thus, the heretics, who are very few in number, are absolutely necessary for the changes to take place that are *essential* for progress in scientific knowledge.

I believe that a significant question is the following: Why are there so few people involved in thinking about new *fundamental* ideas in science that can lead to progress? I do not think that the answer lies in Kuhn's theory of scientific revolutions (Kuhn, 1970), nor do I see the answer in the writings of his critics (Lakatos & Musgrave, 1970). For Kuhn, as well as his critics, address themselves to the scientists' strength of commitment to *ideas*—an intellectual commitment. (Such commitment has been discussed in detail by Polanyi (1958).) Since this would be based on a logical structure of ideas, there may in this case be hope of convincing many of the need for a scientific revolution in the face of inconsistencies. But in contrast with Kuhn's thesis, I do not believe that scientists are in fact committed to *ideas.* I am quite certain, based on my own experience within the scientific community, that *under the proper circumstances* most scientists would be willing and able, at any time within the 'normal science' period, to accept as reasonable, and to pursue new ideas that could resolve existing conflicts—even if it should mean a full-blown scientific revolution!

They do not do so for one primary reason, in my view. It is that, rather than committing themselves to ideas, scientists commit themselves to people—those whom they set up as leaders of their profession. Most scientists, quite early in their careers, invest their leaders with all of the necessary wisdom concerning the fundamental ideas that underlie their respective sciences. They then feel obligated only to work within a given theoretical framework (Kuhn's 'paradigm'). Indeed many important and subtle new results have come from this activity, shedding new light and understanding on the nature of the existing ideas. Still, one does not question the basic tenets of these existing paradigms.

A commitment to individual people, rather than to ideas, seems to me a much more unhealthy situation because it is an emotional rather than a rational commitment. But I believe that it is this commitment that explains the unwillingness of the majority of scientists to consider giving up the 'truth' of the ideas they had been nurtured on—unless some of their leaders should be willing to give up these same ideas!

Well-known examples throughout the history of science of leaders in science to whom the bulk of the scientific community attributed near omniscience were Aristotle, Newton and Bohr. It is an interesting commentary on the sociological implications of the different philosophic stands that the approach advocated by all three of these scholars was essentially that of positivism, with the strong feature of dogmatism embedded in its epistemology. On the other hand, scholars who have advocated the philosophic stand of abstract realism, such as Einstein in the contemporary period, took the anti-dogmatic attitude toward ideas in science, and had almost no following among scientists! Once again, it seems to me, it is the emotional factor of added security in the rigid, dogmatic stand that has drawn the majority of followers, rather than the lack of ease that is necessarily generated from the flexible stand of continually searching for scientific truth, implied by the anti-dogmatic philosophy. Nevertheless, I would contend that the latter can be the only real approach of science that could lead to genuine progress in our understanding.

In addition to Kuhn's theory of the structure of scientific revolutions, there is also advice that follows from his analysis, on the methodology that should be followed by the researcher. It is that one should do scientific work by trying to dogmatically hold onto the older ideas as long as possible, even in the face of glaring conflicts—just as the electronic equipment of the linear accelerator forces the electron to move at constant speeds in the succeeding sections of wave guide. I do not believe that there can be any effect of this approach in scientific methodology, other than to retard progress, create regress, or (if the leadership in science is sufficiently strong) it may lead to a state of 'suspended animation'.

I should rather advise the community of scientists, as well as the members of any other intellectual endeavour, that the healthiest attitude in the field of ideas is one of *anarchy*, where each investigator is probing the truths of nature on his own, trusting *first* his own intuition as to the choice of reasonable paths of inquiry, and appealing only to pure reason, rather than to the words of authoritative individuals, for the acceptability of scientific discoveries. Recall Galileo's comment that 'the humble reasoning of a single individual is worth more than the words of a hundred authorities'. If the approach of intellectual anarchism should lead to the abolishment of the high priest status in science, then I feel confident that science would accelerate its progress. The only strict rule that I would maintain is that each scientist is willing, *on his own,* to criticise, scrutinise, and to reject well-established scientific concepts, if he can find technically superior replacements—and that he is willing to subject his own findings to the scrutiny and criticism of any or all of his peers, and to respect their opinions so long as they are technically competent.

Many disagree with *intellectual anarchy* in science. They argue that most scientists are not capable of judging what is and what is not a technically superior replacement for an older theory. Their idea is that scientists are trained to think in terms of a particular set of ideas, and that therefore it is impossible for most of them to break out of this way of thinking, and the language that it entails. They would then uphold the view of the scientific establishment, with its leaders pointing the way. It is possible that one must use this type of argument in purely subjective creative fields, such as music and art, because certain rare individuals happen to be gifted, from birth, with talents not shared by the majority. But science is not a subjectively oriented presentation of truth. It is, by definition, clear-cut and objective, with language and rules that are agreed upon by all at the outset. Thus, any well-trained scientist, who has learned his mathematics and the method of evaluating the data from physical experimentation, should have the ability to determine if a proposed new scientific theory is or is not technically superior to the theory it is claiming to supersede.

I do not believe that the present-day situation, where scientists rely on the judgements of their leaders in regard to the fundamental ideas, is a matter of our being at a primitive stage of intelligence, with all of the 'genius' attributed to the leaders. Nor do I agree with many of my colleagues who claim that this is simply 'the way things are with scientists'. I think that it is rather a matter of *conditioning*, and that we are indeed capable of conditioning ourselves into the more mature state where we must rely on our own opinions about scientific theories. I believe that if such an intellectual climate could be generated within the scientific community, it would certainly be in the best interests of scientific progress in our fundamental understanding of the real world.

In this sense, Dingle has been an intellectual anarchist. Even though I do not agree with his conclusion about relativity theory, because I do not accept his interpretation of the Lorentz transformations as consistent with the theory of relativity, his criticism and prolonged debate on this question—because he did not receive a logical answer—have contributed significantly to the true spirit of scientific inquiry.

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