THE RELATIVITY QUESTION

by

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PREFACE

This essay describes some of my involvement in a scientific debate on Einstein's special theory of relativity. Much of this involvement has been as a supporter of the late Professor Herbert Dingle in his lonely crusade against the special theory and against what he believed to be the dogmatic adherence of the scientific community to that theory; not, it should be emphasized, against Einstein, whom he admired and respected and was proud to have known, but against his theory.

Professor Dingle told his own story of his crusade, mainly in his book *Science at the Crossroads* which is frequently cited in the present book. The purpose of the present work is to augment that story by describing events that took place after the publication of that book, to give an assessment of the present situation, and to present some arguments in support of Professor Dingle's thesis. Although a decade has now elapsed since Professor Dingle died, the story is still relevant because the questions that he raised have not been satisfactorily answered.

As far as I am aware, this is the only reasonably comprehensive account of Professor Dingle's crusade against special relativity, by anyone other than himself. Even then, much of the story is told in Professor Dingle's own words, in the form of letters written by him to various people, copies of which he sent to me in the hope that they would eventually be published. There are also some letters that were jointly written by him and a collaborator, Mr. Mark Haymon, and some letters that were written by Mr. Haymon himself. Replies to many of these letters are also included, and most of the correspondence is presented without detailed comment from me. If the presentation of the correspondence seems somewhat one-sided, part of the reason is that some of those to whom letters were written by Professor Dingle and Mr. Haymon did not reply, and some of those who did reply would not give me permission to publish their letters.

Since I am neither a physicist nor an expert on relativity, readers may wonder what justification I have for writing about the relativity debate. I suggest that it is possible to detect faults in a weakly-argued case, or in a poorly-conducted debate, without being an expert on the subject being debated. It is not necessary to be an expert on relativity to perceive the ineptitude of many of the arguments used in defending the special theory, or the inconsistencies among the defenders' arguments, or the scientific community's blindness to both. One does not need to be an expert on relativity to notice the ''hit-and-run'' tactics adopted by various relativists: those who publish statements supporting the orthodox point of view or scoffing at critics of the theory, and who when challenged retreat into silence or claim that the subject has already been debated enough and should not be re-opened. I do not need to be an expert on relativity to know when a journal editor's stated reason for rejecting a paper is completely unrelated to the merits of the paper being rejected. I do not need any expert knowledge to experience a feeling of disgust when a leading scientific journal, which had for years shown great reluctance to publish any more of the debate, allowed one of Professor Dingle's critics to use Professor Dingle's own obituary notice to present a rebuttal of his argument, when he was unable to answer back.

I also present some of my own criticisms of the special theory itself. That does not mean that I claim to be in the same intellectual class as the originator of the theory. I suggest that, just as it is possible to detect flaws in the design of a building without being an architect, so it is possible to detect flaws in a physical theory without being a physicist. I know, also, that some physicists claim that the only way to overthrow a theory is to produce a better theory to supersede the old one. I do not accept that claim; one does not necessarily expect those who recommend the demolition of an obsolete and possibly unsafe building to have to design a new building to replace it.

In my account of the debate I follow Professor Dingle's example in quoting the exact words of various participants in the debate. Since this sometimes involves the use of unpublished letters, I would like to make a statement about the publication and quoting of correspondence. In all cases in which letters written by others are reproduced or paraphrased, I have tried to observe the principle of fair dealing. In many cases in which I felt that correspondents might be sensitive to the appearance of their exact words, I have asked permission to publish their letters. In some cases, however, mainly letters of rejection from editors of journals, I have quoted short letters verbatim without asking permission; I have done this because I believe that the accurate presentation of that evidence is more important, from the ethical point of view, than the protection of the writer's copyright. Whenever permission to reproduce a letter has been sought and refused, I have respected the writer's wishes and have not reproduced the letter. However, even if permission to reproduce a letter has been refused, I do not believe that a person has the right to expect that the existence of a letter and the general nature of its contents can remain secret, unless the letter has been marked confidential. Accordingly, when a letter has seemed important to the story but permission to publish it has been refused, I have paraphrased it or given some indication of its contents, unless the letter is marked confidential or restricted in some similar way; in some cases, when the exact wording of a minor letter did not seem important, I have simply paraphrased it without going to the trouble of asking permission.

In any case, since many of the letters in question were written to Professor Dingle, I should point out that much of the relevant correspondence is publicly available, since copies of letters that were in the possession of an eminent Canadian scientist, who had been one of Professor Dingle's students, are now in the Manuscripts Division of the Public Archives of Canada in Ottawa. Also, I understand that Professor Dingle's private papers were given to Imperial College, London, where they are presumably available for consultation by scholars.

I would like to acknowledge the co-operation of Professor Dingle and Mr. Haymon in providing copies of their correspondence with various persons, and for their kind permission to reproduce that correspondence. Other writers who kindly gave permission for letters to be reproduced are acknowledged in the text.

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Toronto
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CHAPTER 1

INTRODUCTION

> Changes of view are continually forced upon us by our attempts to understand reality. But it always remains for the future to decide whether we chose the only possible way out and whether or not a better solution of our difficulties could have been found.
> Albert Einstein and Leopold Infeld: *The Evolution of Physics.*

It would be difficult to exaggerate the eminence of Albert Einstein as a scientist, or the importance attached by the scientific community to the special and general theories of relativity, which he conceived during the early years of the twentieth century and on which his eminence is largely based. Few people have written more extensively on these theories, or over a longer period, than the late Professor Herbert Dingle. It is therefore an event of some significance that, about forty years after his first acquaintance with the subject, Professor Dingle came to the conclusion that the special theory of relativity, though mathematically consistent, is physically impossible.

During the last twenty years of his life, from about 1958, Professor Dingle devoted most of his scientific activity to an attempt to persuade the scientific community that the special theory of relativity was untenable; after more than a decade of frustration, he told part of that story in his book *Science at the Crossroads*[1], published in 1972. Although the scientific community remained almost unanimous in its conviction that Dingle was wrong, it also remained remarkably incoherent and inconsistent in its responses to his criticisms, and one of the main purposes of this book is to draw attention to some of the inconsistencies. It is very striking that scientists, who do not appear to have even noticed the glaring faults and inconsistencies in arguments that have been used in defence of the theory, remain firmly convinced that there is no inconsistency in the theory itself, and the inevitable question arises: if scientists are blind to the faults in the arguments, how can they be so sure that they are not also blind to a fault in the theory itself?

Another of the main purposes of this book is to continue the story of Professor Dingle's involvement in the relativity debate beyond the activities described in his own writings. In a sense, therefore, this book is a sequel to *Science at the Crossroads;* although I hope that interested readers who have not already done so will read Dingle's book, I have tried to make this book self-contained, so that it can be understood without having read the earlier book.

As Professor Dingle repeatedly claimed, the understanding of his criticisms of the theory does not depend on difficult mathematical ideas, but rather on fundamental concepts which require clear thinking rather than advanced scientific knowledge. I think it is fair to suggest that Einstein himself would have been in sympathy with that claim (whether or not he would have agreed with the criticism), since he believed, according to

1. H. Dingle, *Science at the Crossroads,* Martin Brian & O'Keeffe, London (1972).

Infeld[2], that the fundamental ideas in physics can all be represented in words. The present book, in the same spirit, attempts to present the appropriate information and arguments, including some of my own arguments, in non-mathematical language.

It is naturally with some trepidation that I attempt to follow Professor Dingle in bringing his story up to date by presenting this account of his thesis and of some of the responses to it, since I cannot hope to match the eloquence, wit and style of his own writings. Perhaps I may excuse my presumptuousness by quoting the following sentence from his last book *The Mind of Emily Brontë*[3]: "To disinter from a mass of diverse writing a common substratum demands penetration of a far higher order, and the only ground on which I claim justification for attempting the task is the absence of competitors."

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2. L. Infeld, *Quest: The Evolution of a Scientist,* Doubleday, Doran & Co. (1941).

3. H. Dingle, *The Mind of Emily Brontë,* Martin Brian & O'Keeffe, London (1974).

CHAPTER 2

HISTORICAL BACKGROUND

I felt very strongly that science is too scientific to be left to the scientists. They are often swayed too strongly by their emotions to take a properly detached view, and can cause untold harm to the future development of science.
John Taylor: *The Listener,* 7 October, 1971.

The special theory of relativity, the theory with which this book is largely concerned, originated in a paper published by Albert Einstein in 1905, an English translation of which is included in a well-known collection of papers on relativity[1]. The first papers on the general theory appeared about a decade later.

The early part of Herbert Dingle's scientific career was contemporaneous with the growth of both scientific and public interest in relativity. Born in London on 2 August 1890, he received his B.Sc. degree from the Imperial College of Science and Technology, London, in 1918. Subsequently he was successively Demonstrator, Lecturer, Reader and Professor of Natural Philosophy at Imperial College, during the period 1918-1946; he then became Professor of the History and Philosophy of Science at University College, London, a position which he occupied until becoming Professor Emeritus in 1955. He died in Hull, England, on 4 September 1978.

Professor Dingle was a student of relativity during the years in which the theory was making its greatest impact. His first book on the subject was published in 1922[2], and he continued to publish his writings on relativity for well over half a century. One of his principal concerns, in his long study of relativity, was the prediction of the special theory that a moving clock would run slow, relative to a stationary clock. We shall have occasion to discuss this change of relative clock rates in more detail, later in this book; for the present, let us consider briefly the development of Dingle's ideas on this subject.

One of the early sources of Dingle's scepticism was the famous clock paradox. This refers to a prediction, made by Einstein in his original paper on special relativity, that, if two identical clocks were initially together, and if one of them went on a journey and later returned to the other clock, the one that had gone on the journey would show a shorter time interval between separation and reunion than the one that had not. According to some scientists, this prediction violated the principle of relativity, according to which the motion could with equal validity be ascribed to either clock; these scientists argued that both clocks must therefore show the same interval between separation and reunion.

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1. H. A. Lorentz, A. Einstein, H. Minkowski, and H. Weyl, *The Principle of Relativity,* Methuen (1923).

2. H. Dingle, *Relativity for All,* Methuen (1922).

The clock paradox is closely related to the twin paradox, in which the two clocks are replaced by a pair of twins. If one twin went away on a very long high-speed journey into space, then, according to the usual interpretation of the special theory, on his return he would have aged less than his twin who had stayed at home. Discussions of this phenomenon are frequently embellished with picturesque and amusing details: for example, the twins could be separated at birth, and the traveller could return aged one year to find that his ''twin'' had become an old man.

On the basis of the orthodox interpretation of the theory, special relativity could also be used to justify fantastic absurdities such as the case of Gilbert and Sullivan's character Iolanthe, who at the age of seventeen was the mother of a son aged twenty-four. If the banishment to which she was subjected had entailed a sufficiently long and high-speed journey after the birth of her son, the relative ages involved would have been no problem -- to an orthodox relativist.

Although Dingle seems to have never believed in the orthodox interpretation of the special theory on this point, namely that the asymmetrical ageing would occur, it was not until 1955 that he published a paper expressing his scepticism. This led to a vigorous discussion both in the scientific literature and in more popular writings. Since there has been such an enormous amount of published discussion about the clock paradox and the twin paradox, we shall not attempt to discuss them further here; an interesting survey of the discussion can be found in a book by L. Marder[3].

Professor Dingle's scepticism about the clock paradox eventually led him to the conclusion that the special theory contains a fatal contradiction. Clearly, if the special theory is wrong, the clock paradox, which arose from the theory, becomes much less important. It is unfortunate that, because of the prominence of the clock-paradox controversy in the late fifties, it is this controversy that is linked with Dingle in many people's minds. Many writers continued to criticize his arguments as if he was still arguing against the orthodox resolution of the paradox, despite explicit statements to the contrary in his book. In fact, it was his attempt to convince the scientific world that the special theory was wrong that occupied much of his time and energy during the last twenty years of his life, and it is that problem with which this book is mainly concerned. Although we are not greatly concerned with the clock paradox, there is one strong similarity between that controversy and the controversy over the validity of the theory, namely the diversity of the replies that have been made in defending the orthodox point of view. This diversity, in the case of the clock paradox, was described by Cullwick in the following words[4]:

> On one thing Professor Dingle's critics are all agreed, that he is wrong. They do not all agree, however, on the nature of his error. Some give arguments which are no more than illustrations of the obvious fact that the reciprocal Lorentz transformation is algebraically consistent; some claim that the problem requires the General Theory of Relativity; and some appear to regard the matter as settled by their knowledge of four-dimensional space-time. Some argue with patience, while others thinly disguise their irritation.

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3. L. Marder, *Time and the Space-Traveller,* Allen & Unwin (1971).

4. E. G. Cullwick, ''The Riddle of Relativity,'' *Bulletin of the Institute of Physics* **10** pp. 52-57 (March 1959).

After mentioning some of the diverse opinions on the subject, Cullwick continued as follows:

> One is reminded a little of the battle of Arsuf, in the Third Crusade, when, led by Richard, the crusaders routed the infidel with much blood and satisfaction and then started to slay each other.

While Cullwick's comparison might have been appropriate in the case of the controversy on the clock paradox, it is not such a good comparison to the controversy on the validity of the special theory. In the latter controversy the different defenders of the theory are, indeed, inconsistent with one another in their arguments, as in the former case. They do not, however, argue among themselves; they simply present their own arguments and take no notice of the contrary ones. They are like blind men investigating an elephant, each asserting with confident certitude that the object of study is a tree, a rope, a snake, or whatever, all ignoring the assertions of the others, and unanimous only in their scornful denunciation of the person who says that it is an elephant.

As I shall show, there is a great diversity among the replies that have been made to Dingle's claim that there is a contradiction in the special theory; despite the fact that some of Dingle's critics contradict each other, some contradict Einstein, and some even contradict themselves, few scientists seem to be concerned about the contradictions, and Dingle's critics still seem to be unanimous on only one thing -- that Dingle is wrong.

To illustrate some of the above-mentioned problems and attitudes, let us consider an example chosen from among the various inconsistent responses that have been made to Professor Dingle's thesis, in order to show that there is indeed an unresolved problem. This example is reasonably typical of many of the other inconsistencies, in that it is perfectly obvious to anyone who understands the English language, scientist or not.

In *The Listener* dated 11 November 1971, there appeared an article[5] by John Taylor, Professor of Mathematics in King's College, London, in which he claimed that a certain experiment, commonly known as the Hafele-Keating experiment, which had then been recently conducted, supported Einstein's special theory of relativity. Professor Dingle rebutted this claim in a published letter, and further correspondence continued to be published. In a letter which appeared on 25 November[6] M. A. Jaswon, Professor of Mathematics at City University, London, attempted to defend the theory against Professor Dingle's arguments, but conceded that the experiment in question had "no relevance whatever for the special theory". Although that statement was inconsistent with Professor Taylor's article, Taylor published another letter on 9 December[7], which continued to attack Professor Dingle but took no notice whatever of the inconsistency.

If scientists had been concerned with the pursuit of truth, rather than with the discrediting of a heretic, one would have thought that some attempt would have been made to resolve the obvious inconsistency between the statements of those two defenders of the theory; yet, as any reader can verify, the published correspondence showed no

5. J. Taylor, "Views," *The Listener* **86** pp. 642-643 (11 November 1971).

6. M. A. Jaswon, "Travelling Clocks," *The Listener* **86** p. 724 (25 November 1971).

7. J. Taylor, "Travelling Clocks," *The Listener* **86** p. 804 (9 December 1971).

attempt to resolve the inconsistency.

It should be strongly emphasized that the inconsistency between the statements of Professors Taylor and Jaswon does not arise from the inscrutability of nature, but from conflicting interpretations of a man-made theory which scientists claim to understand. If two scientists, both writing about the same theory, make statements that are inconsistent with one another, then one or other of the following conclusions is inevitable:

(1) One of the scientists has made an error.

(2) The inconsistency between the statements arises from an inconsistency that is inherent in the theory.

If neither scientist admits to having made an error, and no other scientist points out an error, then the scientific community should adopt conclusion (2) and admit that Dingle was right in saying that there is an inconsistency in the special theory.

Before leaving this topic, let us consider the last paragraph of Professor Taylor's original article in *The Listener* dated 11 November 1971, which refers to the Hafele-Keating experiment as follows[8]:

> The experiment has worked. It didn't really need doing, since Einstein's theory had already been tested under far more extreme conditions. But such a test had to be performed, if only to lay the doubting Thomases to rest. *Requiescant in pace.*

It seems strange that a scientist should state that an experiment ''didn't really need doing'', implying that its result could have been known (rather than merely predicted) in advance. An experiment, by definition, carries no guarantee of any particular outcome. Taylor's statement is, in my opinion, completely unscientific, but is reasonably typical of the complacent certainty of their own rightness which is a feature of the attitude of so many relativists.

Another very interesting feature of Professor Taylor's letter in *The Listener* dated 9 December 1971 is the way it ends, in the following words: ''I am sure Professor Dingle doesn't wish to come under the latter heading in the proverb: 'Those that can, create; those that can't, criticise.' '' The inappropriateness of that remark may be judged by the fact that, at that time, Professor Dingle's published writings on relativity had spanned a period of almost fifty years, and that he wrote his first book on the subject several years before Professor Taylor was born.

In order to illustrate the great difficulty of getting members of the scientific community to debate the merits of the arguments against the special theory, I shall now recount a small sequel to the above-mentioned correspondence in *The Listener.* In October 1983 I published an article[9] in which I drew attention to various inconsistencies in the arguments by which the special theory had been defended, including the inconsistency between the positions taken by Taylor and Jaswon in the correspondence that

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8. J. Taylor, ''Views,'' *The Listener* **86** pp. 642-643 (11 November 1971).

9. I. McCausland, ''Problems in Special Relativity,'' *Wireless World* **89, No. 1573** pp. 63-65 (October 1983).

had appeared in *The Listener*. I sent copies of the article to various eminent professors whose arguments I had criticized in the article, and Professor Taylor was kind enough to write to me about it, in a letter dated 8 November 1983. Although he would not give me permission to reproduce his letter, the issue is much too important to allow the letter to be completely suppressed, so I shall indicate in general terms what he wrote.

Professor Taylor wrote that he might not have known about Professor Jaswon's letter when he wrote his letter to *The Listener*. That is very easy to answer. The crucial letter from Professor Taylor, which appeared in the 9 December 1971 issue, referred to a letter of Dingle's that had appeared in the 2 December issue; Professor Jaswon's letter appeared in the 25 November issue. Since it seems unlikely that a competent scientist would take part in a published correspondence on a controversial subject without reading all the correspondence up to that point, I think it is safe to reject the possibility that Professor Taylor had not seen Professor Jaswon's letter when he wrote his own letter.

Professor Taylor also told me that he felt that I had a good point that there was indeed something to be cleared up about the issue. His initial feeling was that he was not right in saying that the Hafele-Keating experiment justified special relativity but that Dingle was still wrong in his claim of an inconsistency. He agreed with me that one of the other relativists had been rather unconvincing in what he wrote, but said that it is convincing enough when properly explained. He made some comments on the relevance of the general theory and the special theory to some of the problems in question, but said that he would have to look at that more closely to be sure.

This letter was very significant to me, in that it was the first letter I had ever received from a relativist admitting that there was any flaw in the relativists' case. I wrote to Professor Taylor on November 18, acknowledging his letter and making some other comments on points that he had raised; I refrained from making any suggestion that he publish some statement along the lines of his letter, because I thought that such a course of action was so obvious that there was no need to belabour the point. However, when there was no sign that Professor Taylor planned to publish anything about the subject, I wrote to him on April 3, 1984, expressing the hope that he planned to publish a statement similar to that made in his letter to me. I suggested that, if he did not wish to write a new statement, perhaps he might be willing to give me permission to publish his letter. Professor Taylor replied, in a letter dated 9th April, saying that he had not had, nor would he have within the next few months, time to consider the matter in any more detail, and until he had done so he would not feel that he had done the problem (or himself) sufficient justice. He asked me not to quote him on anything in his letter of November 8th. I replied by letter dated April 23rd, in the following words:

> Thank you for your letter dated 9th April. Although I am grateful for the promptness of your reply, I am somewhat disappointed that you have not yet had time to consider in any more detail the questions raised in my October article and in your letter dated 8th November, 1983.
>
> While I do not claim the right to suggest what your priorities ought to be in connection with this matter, I also do not wish to make any commitment that would indefinitely preclude my making use of your November letter in some way short of actually publishing it or making verbatim quotations from it. I hope, therefore, that you can give me some estimate of the date by which you will have been able to re-assess your earlier statements on the matters in question.

Professor Taylor replied by letter dated 4th May, 1984. He told me about his various commitments to his research students and the associated research programme, and to his Department and College, which forced other matters to have lower priority; that was why he was unable to devote time to the relativity question at that time. He also repeated that he was unable to give me permission to quote from his letter, which was of the form of ideas on work in progess.

Up to the end of August 1988, more than four years later, I have heard nothing further from Professor Taylor, nor am I aware that he has published anything further on the subject. I have observed, however, that he has had time to publish at least one other new item, namely a book review that appeared in the 30 January 1986 issue of *New Scientist.* Since the reassessment of the relativity question seems to have been pushed even further down on his list of priorities, I do not believe that I have an ethical obligation to keep silent any longer about Professor Taylor's letters to me; I do not think that it is reasonable to expect that, in a struggle with the Goliath of relativity, I should allow the opposition to place further restrictions on the ammunition that I am allowed to use. Readers may judge for themselves the difficulty of getting relativists to admit publicly that there are any flaws in the published defences of special relativity.

CHAPTER 3

DINGLE'S CRITICISMS OF THE SPECIAL THEORY

How wonderful that we have met with a paradox. Now we have some hope of making progress.
Niels Bohr: Quoted by R. Moore in *Niels Bohr.*

When Bohr visited Moscow, Lev Landau, also a Nobel prize winner, asked him, "How is it that Copenhagen is such a famous centre of theoretical physics and trains such brilliant people?"
Bohr answered: "Truly, I don't know. Perhaps only because we are not afraid to ask silly questions in order to clear up what we don't understand."
Leopold Infeld: *Niels Bohr and Einstein* (in *Why I Left Canada*)

The great questions are those an intelligent child asks and, getting no answer, stops asking.
George Wald: Quoted by Arthur Koestler in *The Ghost in the Machine.*

As was mentioned in Chapter 2, Professor Dingle's conviction that there is a fatal flaw in special relativity arose from the scepticism that had been aroused by the clock paradox. As he pointed out in his book[1], a paradox arises when, from the same premises P, two apparently contradictory conclusions, X and Y, seem inescapably to follow. Such a paradox can be resolved if and only if one of the following things can be shown:

(1) the conclusions are not really contradictory,

(2) conclusion X does not follow,

(3) conclusion Y does not follow,

or

(4) the premises P contain a contradiction.

Suppose, for example, that there are two identical clocks A and B, initially together and mutually synchronized. Suppose that A moves away from B at uniform speed, and later turns around and returns to B at the same speed.

In terms of the notation above, assume that the premises P are the axioms and definitions on which the special theory of relativity is based, conclusion X (symmetrical ageing) is that the readings of A and B are equal at the reunion of the two clocks, and conclusion Y (asymmetrical ageing) is that the readings of A and B are unequal at their reunion. Clearly X and Y are contradictory, ruling out possibility (1) of the first

1. H. Dingle, *Science at the Crossroads,* Martin Brian & O'Keeffe, London (1972).

paragraph. As Dingle pointed out, Einstein in his original paper accepted conclusion Y but did not disprove conclusion X. It should be emphasized that all additional proofs of either X or Y do nothing to resolve the paradox, because any such proof does not disprove the other result; since Dingle was unable to disprove Y to his complete satisfaction, he was eventually forced to consider the possibility that the paradox could only be resolved by finding a contradiction inherent in P. Once he had found what he believed to be a contradiction in P, he tried to find ways of expressing the contradiction in such a way as to avoid the accelerations which are inevitable in any experiment in which two clocks, or twins, separate and later reunite.

By the time of the publication of *Science at the Crossroads* in 1972, Dingle had refined his thesis in such a way that it could be expressed in two ways, The Argument and The Question. The Argument is presented on page 45 of *Science at the Crossroads,* in the following words:

THE ARGUMENT

> According to the special theory of relativity, two similar clocks, A and B, which are in uniform relative motion and in which no other differences exist of which the theory takes any account, work at different rates. The situation is therefore entirely symmetrical, from which it follows that if A works faster than B, B must work faster than A. Since this is impossible, the theory must be false.

The Question might be worded very briefly as follows: *Which of two clocks in uniform relative motion does the special theory require to work more slowly?* However, in order to present the story satisfactorily we should consider The Question in its extended form, as it is presented on pages 45-46 of *Science at the Crossroads:*

THE QUESTION

> According to the special relativity theory, as expounded by Einstein in his original paper, two similar, regularly-running clocks, A and B, in uniform relative motion, must work at different rates. In mathematical terms, the intervals, *dt and dt′,* which they record between the same two events are related by the Lorentz transformation, according to which $dt \neq dt'$. Hence one clock must work steadily at a slower rate than the other. The theory, however, provides no indication of which clock that is, and the question inevitably arises: How is the slower-working clock distinguished? The supposition that the theory merely requires each clock to *appear* to work more slowly from the point of view of the other is ruled out not only by its many applications and by the fact that the theory would then be useless in practice, but also by Einstein's own examples, of which it is sufficient to cite the one best known and most often claimed to have been indirectly established by experiment, viz. 'Thence' [i.e. from the theory he had just expounded, which takes no account of possible effects of acceleration, gravitation, or any difference at all between the clocks except their state of uniform motion] 'we conclude that a balance-clock at the equator must go more slowly, by a very small amount, than a precisely similar clock situated at one of the poles under otherwise identical conditions.' Applied to this example, the question is: what entitled Einstein to conclude *from his theory* that the equatorial, and not the polar, clock worked more slowly?

In the intervening period, between Dingle's first suspicion that there was a contradiction in the theory, and his final refined form of his thesis in The Argument and The Question, Dingle made various attempts to bring his criticisms to the attention of the scientific community. His first paper to present a contradiction appeared in December

1958[2], and three further papers were published in 1960. Although some private correspondence ensued, little or no public notice seems to have been taken of these papers.

In 1961, in a book written jointly with Viscount Samuel[3], Dingle again presented his criticism, and also described some of the difficulties that he had encountered in attempting to have his criticism published by The Royal Society, the Physical Society, *The Philosophical Magazine,* and *Nature.* For example, in one of the more striking examples of the attitude of a scientific journal, Dingle described how *The Philosophical Magazine* sent back a critical paper by return mail, with a statement that subjects of a polemical nature were not suited to that journal! After describing that rejection, Dingle went on to say; "One of the leading scientific journals will not publish anything of a polemical nature, which can only mean that, in science itself, it will not publish any criticism of orthodox views. Accept them, and your paper will be considered for publication; question them, and it will not." According to Dingle[4], no reviewer of the Samuel/Dingle book even mentioned that the question of the validity of special relativity had been raised. Another fairly lengthy presentation of Dingle's thesis, in an Introduction to an English translation of Bergson's *Durée et Simultanéité* [5], also failed to attract any significant attention in the scientific community.

After several more years of attempting to obtain an answer to his criticism of special relativity, Dingle became so convinced of the moral shortcomings of the scientific community, in its reluctance to meet or to answer his criticisms of the special theory, that he eventually published *Science at the Crossroads* in an attempt to draw the attention of the scientific community and the general public to what he considered to be a highly unsatisfactory state of affairs. In the Introduction to that book he summed up its theme in the following words:

> I can present the matter most briefly by saying that a proof that Einstein's special theory of relativity is false has been advanced; and ignored, evaded, suppressed and, indeed, treated in every possible way except that of answering it, by the whole scientific world (the world *of physical* science, that is; the theory has no place at present in the biological and psychological sciences). Since this theory is basic to practically all physical experiments, the consequences if it is false, modern atomic experiments being what they are, may be immeasurably calamitous.

In the next chapter, we shall summarize some of the main points of Professor Dingle's book *Science at the Crossroads,* in preparation for the continuation of the story of the controversy.

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2. H. Dingle, "The Interpretation of the Special Relativity Theory," *Bulletin of the Institute of Physics,* pp. 314-316 (December 1958).

3. H. Samuel and H. Dingle, *A Threefold Cord,* Allen and Unwin (1961).

4. H. Dingle, *Science at the Crossroads,* Martin Brian & O'Keeffe, London (1972).

5. H. Bergson, *Duration and Simultaneity,* Bobbs-Merrill Co. Inc. (1965). (Translated by L. Jacobson, with an Introduction by Herbert Dingle.)

CHAPTER 4

"SCIENCE AT THE CROSSROADS"

There is no more reason to suppose that Einstein's relativity is anything final, than Newton's *Principia.* The danger is dogmatic thought; it plays the devil with religion, and science is not immune from it.
Dialogues of Alfred North Whitehead

Herbert Dingle's book *Science at the Crossroads*[1] was published in 1972; it describes in great detail the history of the controversy up to that time, and the ways in which some members of the scientific community had responded to his criticisms of special relativity. Although I believe that it is necessary to read the book if one is to acquire a thorough understanding of the controversy, the following very brief sketch of the book is included here with the sole purpose of making the remainder of the present narrative intelligible to those who have not yet read *Science at the Crossroads.*

One of the things that Professor Dingle emphasized very strongly in his book was the fact that it was the validity of the special theory of relativity that was at stake, not the much less important problem of the resolution of the clock paradox or twin paradox. In view of the fact that so many of Professor Dingle's critics later wrote as if Dingle was still arguing about the clock paradox, I think it is pertinent to quote Dingle's explicitly-stated position on that subject, as expressed in a letter published in *The Times* of London in January 1972, in reply to a letter from Professor R.A. Lyttleton. The letter is reproduced in the Preface of Dingle's book (pp. 11-12), where it ought to have been read by all critics of the book; the following excerpt states his position quite clearly:

> Regarding the immeasurably less important clock paradox, Lyttleton is again wrong in saying that I have denied asymmetrical ageing for many years. Fifteen years ago, when I believed special relativity true, I indeed thought it impossible, but I soon discovered my error, and for more than 13 years have held the question open. . . . Despite the mu-mesons and their kind, I think asymmetrical ageing extremely unlikely, but that is an opinion; the falsity of the special relativity theory (not necessarily of the relativity of motion) I regard as proved.

The main body of the text of *Science at the Crossroads* is divided into two parts, called *The Moral Issue* and *The Intellectual Issue.* In *The Moral Issue* Dingle presented a factual narrative, documented by many quotations from his interlocutors, describing the responses of various named members of the scientific community to his attempts to obtain an answer to his Question. Although it would be superfluous to repeat the details here, some of the highlights of the story should be mentioned.

The first eminent scientist who attempted to answer Dingle's criticism was Professor Max Born. Although, as we shall see in Chapter 6, his reply[2] is highly unsatisfactory, it seems to have been accepted almost without criticism by the scientific community.

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1. H. Dingle, *Science at the Crossroads,* Martin Brian & O'Keeffe, London (1972).

2. M. Born, "Special Theory of Relativity," *Nature* **197** p. 1287 (1963).

Dingle described how he made a second attempt to have his criticism published by the Royal Society (the first attempt having already been described in *A Threefold Cord*[3], mentioned in Chapter 3). This new paper was rejected on the recommendation of two referees. Although one of the referees stated that the paper contained an elementary fallacy, Dingle was unable to obtain from the Royal Society a statement of what the alleged fallacy was. He later attempted to publish in *Nature* a letter asking the Royal Society to state the fallacy, but his letter was refused publication.

In 1968, after a lengthy private correspondence with Professor Dingle, Professor J.L. Synge published in *Nature* a letter[4] which stated his own views on the contradiction which Dingle had claimed to exist in the theory. Although Dingle sent in a reply to *Nature,* the Editor did not publish it. As a result, Dingle was later taken to task, in a debate in the correspondence columns of *The Listener* in 1969, for apparently failing to reply to Synge. (This debate in *The Listener* is not, of course, the one mentioned in Chapter 2, which took place in 1971-72.)

After the above-mentioned debate in *The Listener* had finished, Professor Dingle sent a copy of the whole *Listener* correspondence to Mr. John Maddox, then Editor of *Nature.* Mr. Maddox wrote to Dingle on 24 November 1969, stating that he proposed to write a leading article summarising the position, and that he would publish it "before the end of the year". It did not appear before the end of that year, and in response to an enquiry the Editor wrote to Dingle on 21 January 1970 to say that the article was "almost ready". Towards the end of March another enquirer, Lord Soper, wrote to Mr. Maddox, and was told that it would be "a week or two" before the article was ready; when Lord Soper enquired again on 6 July, he received no reply. The promised leading article was never published; we shall later examine the reasons subsequently given by the Editor for its non-appearance.

Professor Dingle's book also contains an interesting historical survey of the development of relativity theory, and the relationship between Einstein's special theory and Lorentz's theory, the latter theory being quite different from Einstein's in that it assumes a stationary ether, such that clocks moving through the ether would *actually* run slower than clocks that remained at rest. Dingle pointed out that the two theories are often confused with one another, and stated that all the experimental evidence that is taken to support Einstein's special theory could, with equal validity, be taken to support Lorentz's quite different theory.

Dingle also pointed out, both in his book and elsewhere, that the experimental evidence that is taken to support the special theory depends on circular arguments, since it relies on the validity of Maxwell's electromagnetic theory to infer certain intermediate results such as the velocities of certain elementary particles. We shall discuss these points in more detail later.

One of the most prominent features of Professor Dingle's book is his repeated warning that, if the special theory of relativity were in fact inconsistent, experiments based on the assumption that the theory is correct might lead to calamitous results. Since he was

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3. H. Samuel and H. Dingle, *A Threefold Cord,* Allen and Unwin (1961).

4. J. L. Synge, "Special Theory of Relativity," *Nature* **219** p. 793 (1968).

not able to name what kind of calamity might ensue, or to specify the probability of such an event, many readers of his book remained unconvinced that there were in fact any serious risks. I was myself sceptical about the seriousness of the problem, but became more convinced of Dingle's view after reading an account of the thalidomide tragedy. I wrote a article at the time[5], in which I drew a comparison between the story of Professor Dingle's crusade and the story of the thalidomide tragedy. The thalidomide problem was worsened by the fact that influence was brought to bear on scientists and on editors of journals to prevent or to delay publication of critical articles by informed scientists, in a situation where each month's delay in dealing with the problem may have meant the birth of fifty to one hundred deformed children.

There is now another equally striking example of a tragedy that could have been prevented if warnings had been heeded in time: I refer to the accident of the space shuttle "Challenger" on 28 January 1986. The accident was caused by the failure of a major part, a failure that was both predictable and predicted, because warnings of disaster were ignored by those in charge of the project who were eager to get on with the job of launching the shuttle.

Professor Dingle continued to express his concern about the possibility of calamitous occurrences that might occur from the neglect of informed criticisms of special relativity. Some of these expressions of concern are found in portions of his correspondence quoted in Chapters 7 to 10 of the present book. Before proceeding to that subject, let us consider some of the interesting reactions to *Science at the Crossroads*.

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5. I. McCausland, "Life at the Crossroads," *The New-Church Magazine* **94, No. 672** pp. 53-56 (April-June 1975).

CHAPTER 5

REACTION TO THE BOOK

> If I had before me a fly and an elephant, having never seen more than one such magnitude of either kind; and if the fly were to endeavour to persuade me that he was larger than the elephant, I might by possibility be placed in a difficulty. The apparently little creature might use such arguments about the effect of distance, and might appeal to such laws of sight and hearing as I, if unlearned in those things, might be unable wholly to reject. But if there were a thousand flies, all buzzing, to appearance, about the great creature; and, to a fly, declaring, each one for himself, that he was bigger than the quadruped; and all giving different and frequently contradictory reasons; and each one despising and opposing the reasons of the others -- I should feel quite at my ease. I should certainly say, My little friends, the case of each one of you is destroyed by the rest. I intend to show flies in the swarm, with a few larger animals, for reasons to be given.
> Augustus de Morgan: *A Budget of Paradoxes*

In view of the fact that one of the most striking passages of *Science at the Crossroads* is Dingle's account of the failure of the Editor of *Nature* to publish a promised leading article, it is interesting to note that one of the earliest published comments on the book was an anonymous leading article in *Nature*[1]. This article is worthy of study in some detail.

Let us begin by quoting the first sentence and the last two sentences of the leading article, which are:

> Everybody is fond of Professor Herbert Dingle, as well as of the clock paradox in special relativity which he has single-handedly nurtured since the early 1930s.

> And is there any hope that he will now be satisfied with the demonstration that moving clocks run at different speeds from clocks at rest which has been provided in the past few months by the experiments in which Hafele and Keating have flown caesium clocks in different directions around the world *(Science,* **177,** 166; 1972, see also *Nature,* **238,** 244; 1972)? It will be sad to see the clock paradox disappear, but this work is the last nail in the coffin.

The writer of the article seems not to have noticed Dingle's statement that he had for years held an open mind on the subject of asymmetrical ageing, or his attempt to make clear that the scientific issue was not what is normally associated with the expression "clock paradox"; as mentioned in Chapter 4, these statements are found in the Preface of *Science at the Crossroads*. Furthermore, the expressions "single-handedly" and "since the early 1930s" in the first sentence of the article are both totally inaccurate.

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1. "Dingle's Answer", *Nature* **239** p. 242 (September 29 1972).

In another part of the article there is quoted a passage from pages 45-46 of *Science at the Crossroads* (part of the paragraph that we quoted in Chapter 3, under the heading ''The Question''); the last sentence of the quoted passage appears in the article as follows:

> ''The supposition that the theory merely requires each clock to appear to work more slowly from the point of view of the other is ruled out merely by its many applications and by the fact that the theory would then be useless in practice but also by Einstein's own examples. . . .''

Immediately after the above sentence, which is only a partial quotation of Dingle's original (the ellipsis being as it appeared in the leading article) and which also contains a minor inaccuracy (the second ''merely''), the article continues by referring to that sentence as follows:

> The trouble, of course, is that in the last of these sentences, Dingle is denying the central principle of relativity. And why should he not accept that each of two clocks in uniform relative motion should appear to run slow from the other's point of view? That, according to the relativists, is what the real world is like.

If Dingle is ''denying the central principle of relativity'', as the article suggests, he does it by referring (in the part that the author of the article replaced by the ellipsis) to Einstein's prediction from the special theory that a clock at the equator would work (not just *seem* to work) more slowly than a clock at one of the poles (see my quotation of The Question in Chapter 3, where the full sentence can be found). Now, if Einstein deduced from the theory that an equatorial clock would *actually* work more slowly than a polar clock, not merely *appear* to work more slowly, and if that deduction denies the central principle of relativity as the author of the editorial article suggests, then that is evidence in support of the presence of an inconsistency in the theory. If a validly-deduced conclusion of an argument is inconsistent with one of the premises of the argument, then the inconsistency must be in the premises.

In the last passage quoted above, the writer of the article implies that the theory only requires one clock to *appear* to run slow from the other's point of view; it is therefore difficult to know what is meant by the following reference to Dingle in the penultimate sentence of the article: ''And is there any hope that he will now be satisfied with the demonstration that *moving clocks run at different speeds from clocks at rest* . . .'' [Italics mine]. Clearly, if a moving clock runs at a different speed from a clock at rest, it must run either faster or slower; the writer of the editorial article is therefore ''denying the central principle of relativity'' in exactly the same sense as that in which he accuses Dingle of denying it.

Regarding Einstein's statement that a clock at the equator would work more slowly than a clock at a pole, the article has this to say:

> It seems now to be accepted that Einstein's original argument was uncharacteristically loose. The point of the illustration is that a clock at the pole of rotation may be taken to be in an inertial frame which is nearly (but not quite) properly defined by the direction of the Earth's motion around the sun. The clock at the equator is in another. Einstein's lack of clarity concerns the inertial frame of the observer of the two clocks.

It is difficult to know what all this means, and it seems unkind to Einstein that the author of such a vague statement accuses *him* of looseness of statement and lack of clarity. If the writer of the above statement is suggesting that the equatorial clock does not *really* work slower than the polar one but only *appears* to some observer to do so, then he must reject Einstein's prediction that a clock that goes around in a closed path must actually show a different reading from one that stayed behind. It is interesting to compare the above quotation with what other reviewers of Dingle's book have written; their comments on the same matter will be discussed later.

One of the most interesting features of the leading article we are discussing is the way in which it handles Dingle's reference to the other leading article, the one that was promised but never published. Here is what the published article says about the one that was not published:

> Professor Dingle goes on to complain that a promised leading article rounding off the correspondence has never appeared, apparently oblivious of the way in which his own scorn for prospective contestants and his promises to "bring discredit on the journal" may have discouraged the judicious summing-up for which he asked.

This quotation gives the impression that Dingle had asked for the leading article to be written, and also implies that, because of his alleged promise to "bring discredit on the journal", he is himself responsible for its non-appearance. Both of these suggestions are in fact false, as was later shown in a published exchange of letters between Professor Dingle and Mr. Maddox in the correspondence columns of *Nature*[2,3], where it was made clear that the article had been spontaneously promised by Mr. Maddox at the time he was Editor, and also that the letter in which Dingle allegedly promised to "bring discredit on the journal" was written six months *before* Mr. Maddox promised to publish the leading article.

Although it is not in the chronological sequence of events, it is perhaps appropriate at this point to mention that the exchange of letters mentioned above resulted indirectly from an article called "The Dingle Affair: An Unresolved Scientific Controversy", which I wrote in 1974 and which was to have been published in *Science Forum,* a Canadian journal of science and technology (now defunct), in February 1975. On being shown a copy of the manuscript, Mr. Maddox was able to raise doubts in the mind of the Editor of *Science Forum* about the authenticity of my article, and it was not published as planned. Without going into the details of that situation, it is sufficient to say that there was only one item of factual information in my article that was not supported by information that had already been published prior to that time: this was my statement that Dingle's alleged promise to "bring discredit on the journal" could not have been the real reason for the non-appearance of the promised leading article, because the letter in which the promise had allegedly been made had been written six months before Maddox made the promise to publish the leading article. The authenticity of my statement has now been established in the Dingle-Maddox exchange of letters mentioned above, but was dismissed there by Mr. Maddox as a small point whose relevance is debatable.

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2. H. Dingle, "Integrity in Science," *Nature* **255** pp. 519-520 (also Vol. 256, p. 162) (1975).

3. J. Maddox, "Integrity in Science," *Nature* **255** p. 520 (1975).

It is also interesting to note that Mr. Maddox's reply[4] again mentioned Dingle's promise to "bring discredit" on *Nature,* even though it had by then been established that what Dingle had written was a plea to the Editor of *Nature* not to make it necessary for him to *reflect,* not *bring,* discredit[5]; putting the words "bring discredit" in quotation marks in the letter[6] merely makes this a quotation from the original misquotation in the editorial article[7].

Returning to the chronological sequence, the next significant reaction in *Nature* after the above-mentioned editorial article (apart from a limerick, under the brilliantly original heading "Dingle Jingle"[8], which readers may assess for themselves) was a review of Dingle's book by Professor J.M. Ziman[9]. There are many features of this review that are worthy of study.

For example, after quoting Dingle's question (as we have quoted it in Chapter 3, up to the words "How is the slower-working clock distinguished?"), Ziman says "This is a perfectly reasonable question to which science should indeed give an answer." He also states explicitly that the answer is simple, and states that the answer is: "the fastest working clock between any two events is one that travels between them by free fall". In view of the fact that the question asked which of *two* clocks worked slower, not which of *all* clocks, Ziman's answer is comparable to answering the question "Which flies slower, a Boeing 707 or a 747?" by replying "The fastest airliner is the Concorde." Whether the statement is true or not, it is simply not an answer to the question that was asked.

Like the writer of the editorial article[10], Ziman also falls into the trap of confusing the clock-paradox controversy with Dingle's claim that there is a contradiction in the theory. For example, commenting on Dingle's claim that there has been an inadequate public reply to his objections, Ziman writes: "But here, again, he is grossly unfair. The clock paradox, and its resolution, was discussed in detail by Einstein himself, and by many later scholars." Since Einstein was dead before Dingle ever claimed that there was a contradiction in the theory, it can scarcely be claimed that Einstein refuted Dingle's objections to the theory. After citing some books (none of which discuss the claimed contradiction to any significant extent) and mentioning how thoroughly they have been studied, Ziman says that this is as much of an answer as Dingle can reasonably expect, and then goes on to say: "The fact that he, one man in a thousand, thinks differently is scarcely a major flaw in the scientific consensus." This raises the interesting idea of knowledge by consensus, which I discuss in Chapter 19.

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4. J. Maddox, "Integrity in Science," *Nature* **255** p. 520 (1975).

5. H. Dingle, "Integrity in Science," *Nature* **255** pp. 519-520 (also Vol. 256, p. 162) (1975).

6. J. Maddox, "Integrity in Science," *Nature* **255** p. 520 (1975).

7. "Dingle's Answer", *Nature* **239** p. 242 (September 29 1972).

8. J. Letts, "Dingle Jingle," *Nature* **240** p. 59 (1972).

9. J. Ziman, "Science in an Eccentric Mirror," *Nature* **241** pp. 143-144 (1973).

10. "Dingle's Answer", *Nature* **239** p. 242 (September 29 1972).

In the last paragraph of the review, Ziman described the book as "dishonest". The apology that was later published[11] may serve as a confirmation that Dingle's narrative is factually accurate, and it is in the truth of its factual statements that, according to Dingle[12], the whole significance of his book lies.

Another interesting review of Dingle's book was written by Roxburgh[13]. Although this review is more sympathetic towards Dingle's point of view than some others, it contains a rather extraordinary attempt to refute Dingle's argument. After quoting "The Argument" (see Chapter 3), Roxburgh remarks that Dingle does not even discuss what he means by "faster", and then goes on to say:

> Secondly, why is it impossible for A to go faster than B and B to go faster than A? This depends on the definition of faster. To illustrate this, consider the following two statements:
>
> The Moon is bigger than the Sun.
> The Sun is bigger than the Moon.
>
> Are these statements mutually contradictory? This depends on the meaning of bigger. For terrestrial beings the first statement is true, for Martians the second is true. The relative size depends upon the position of the observer. So it is with time and clocks.

If it is important to define "faster", it is also important to use other words precisely, yet it is clear from the quotation that Roxburgh does not literally mean "is" in the two contrasted statements, in which case any similarity between his argument and Dingle's disappears. Or, if he does intend his words to be taken literally, then he, as a terrestrial being, is defending special relativity by asserting that the moon is bigger than the sun. Although we are terrestrial beings, we know that the sun is bigger than the moon, and, what is more, we know it from observations that have been made from the earth.

Clearly, any two contradictory statements can be reconciled if one is at liberty to disregard the literal meanings of one or both of the statements and re-interpret them in such a way as to avoid the contradiction; it is scarcely surprising that Roxburgh is able to avoid finding a contradiction in the theory.

Roxburgh does agree with Dingle to the extent that he says that Lorentz's theory of absolute space, and clock rates dependent on absolute motion, has not been disproved, and he states that the Lorentz and Einstein theories are "observationally indistinguishable".

Another interesting attempt to answer Dingle's question about the equatorial and polar clocks has been made by G.J. Whitrow[14], in the following statement:

> For a supporter of relativity, the essential difference between the two clocks is that relative

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11. "Professor Herbert Dingle: An Apology", *Nature* **243** p. 315 (1973).

12. H. Dingle, "Dingle's Answer," *Nature* **243** p. 366 (June 8, 1973).

13. I. Roxburgh, "Is Special Relativity Right or Wrong?," *New Scientist* **55, No. 813** p. 602 (28 September 1972).

14. G.J. Whitrow, Review of "Science at the Crossroads", *British Journal for the Philosophy of Science* **26** pp. 358-362 (1975).

> to the centre of the Earth (which for the purpose concerned can be regarded as the origin of an inertial frame) the clock at the equator describes a circle and so cannot be associated with an inertial frame, whereas the polar clock is at rest and can be associated with an inertial frame for a period of time during which the curvature of the Earth's orbit can be neglected.

If, as Whitrow suggests, the equatorial clock cannot be associated with an inertial frame, then it is beyond the scope of the special theory which, as Einstein pointed out[15], applies only to inertial frames. It is therefore not valid to infer *any* conclusion from the special theory about the relative rates of the two clocks.

Although it is not directly related to the scientific world's reactions to Professor Dingle's book, it is interesting to note what another scientist has written on this subject. A reviewer, identified only by the initials J.P.S., wrote as follows[16] in a review in *Physics in Canada* of L. Landau and Yu. Rumer's book *What is the Theory of Relativity:*

> Occasionally, however, in their effort to simplify, the authors make some incorrect statements. The most glaring example is in the discussion of the twin paradox, where they have one twin travelling on a large circular railway track, ignoring the fact that the frame of reference is not an inertial one, so is beyond the scope of special relativity.

The twin moving along a circular track has the same status in relation to special relativity as the equatorial clock in Einstein's original paper on special relativity[17]. If the circular railway track is a glaring error, why does not someone say that Einstein made a glaring error too?

Furthermore, I suggest that Whitrow is quite wrong in suggesting that there is a difference in kind between the paths of the two clocks. If the equatorial clock cannot be associated with an inertial frame because it moves in a circle, then the polar clock, which follows an elliptical path around the sun, cannot be associated with an inertial frame either; the crucial property that both paths have in common is that they both depart from straight-line uniform motion.

At least one reviewer, G. Stadlen[18], did admit the possibility that Einstein might have made a minor error in his statement about the polar and equatorial clocks; here is what he said:

> But the relative motion involved in this case, being circular, is non-uniform. I submit, therefore, that Einstein was wrong in saying that his prediction followed from the special theory, which deals only with the effects of uniform motion. This is not to say that the prediction was invalid. For Einstein was, intuitively, anticipating his later general theory, according to which the equatorial clock runs slower because of the centripetal force exerted

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15. A. Einstein and L. Infeld, *The Evolution of Physics,* Cambridge University Press (1938).

16. J.P.S., "Relativity Theory for Everyman," *Physics in Canada* **39, No 2** pp. 52-53 (March 1983). (Reviews of three books, by Landau and Rumer, Lilley, and Evett.)

17. H. A. Lorentz, A. Einstein, H. Minkowski, and H. Weyl, *The Principle of Relativity,* Methuen (1923).

18. G. Stadlen, "Dingle's Challenge," *The Listener* **88, No. 2270** pp. 411-412 (28 September 1972).

upon it.

This answer is inconsistent with at least two of the previous answers: it disagrees with Whitrow about whether the result follows from the special theory, and it disagrees with the *Nature* editorial article about whether the slower working is real or merely dependent on the motion of the observer. Furthermore, the fact that the predicted slowing follows from the general theory does not make Einstein's prediction *from the special theory* valid; it is a well known fact of logic that the truth of the conclusion of an argument does not guarantee the validity of the argument. If Einstein's prediction did not follow from the special theory, then his inclusion of that prediction was irrational and, therefore, not valid. Also, the suggestion that Einstein was so easily able to anticipate his general theory, which took him about another decade to develop, is rather unconvincing.

Stadlen's review is interesting in that it is about the only one even to mention Dingle's claim that the experimental evidence in favour of special relativity rests on circular arguments, or his claim that all observers will agree on whether a pair of relatively-stationary clocks are synchronized with one another. Both of these points are highly significant, and will be discussed in more detail later.

Another interesting comment appears in a review by Kilmister[19]; referring to Dingle's choice of Einstein's original paper on special relativity as the canonical text, he writes:

> This is a good basis for a debate, but suppose that, on one page, Einstein had made a stupid blunder; is this thereby incorporated for ever in the theory?

Is Kilmister suggesting that Einstein did make such a blunder, or is he not? If he is suggesting that Einstein did, what is the blunder? Without such clarification, Kilmister's statement is a red herring and contributes precisely nothing to the debate.

Although it appeared about a decade before the appearance of Dingle's book, another criticism of Dingle's arguments seems to be relevant to this discussion. Bronowski[20] argued, in a manner similar to Whitrow's argument discussed above, that the difference in rates of the two clocks is justified by the fact that the equatorial clock is not in an inertial frame. He was actually discussing an analogous experiment involving a rotating disc, but he related it directly to Einstein's prediction about the equatorial and polar clocks, and to the prediction of asymmetrical ageing in the clock paradox or twin paradox experiment. This is how he justified the conclusion that it was the equatorial clock that worked more slowly:

> Relativity only postulates that observers moving in *inertial* systems cannot tell which of them is moving. By contrast, an observer who moves in an *accelerated* system can tell that he has moved, simply by carrying an accelerometer (or a bucket of water).

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19. C.W. Kilmister, Review of "Science at the Crossroads", *The Observatory* **93, No. 1995** p. 154 (1973 August).

20. J. Bronowski, "Dr. Bronowski Replies to Professor Dingle," *New Scientist* **11, No. 250** p. 542 (31 August 1961).

> In the Harwell experiment, the rotating disc is not an inertial system. That is, a point on the circumference is not in uniform motion in a straight line; it is in constant acceleration, which an observer at the point could detect by carrying an accelerometer.
> [Italics in the original.]

Dr. Bronowski's argument suffers from the same flaw as Professor Whitrow's: if a clock at the circumference of the rotating disc is in constant acceleration, it is not valid to infer *any* conclusion from the special theory about the rate of that clock. Unfortunately the Editor of *New Scientist* discontinued the correspondence after Bronowski's letter, adding that "Professor Dingle wishes it to be known that he does not accept the arguments in Dr. Bronowski's letter." However, in a later comment on the twin paradox, involving twins Peter and Paul, Dingle referred to Bronowski's justification of the asymmetrical ageing as follows[21]:

> Nevertheless during a recent controversy many physicists (for example, J. Bronowski, in *The New Scientist,* Aug. 31, 1961) have continued to maintain that Paul's acceleration on reversal prevents the application of the special theory to the problem. Curiously enough, however, they do not therefore refrain from applying it but regard themselves as entitled to use its equations with a meaning of their own in place of that which the relativity postulate gives them. The result -- need it be said? -- is that asymmetrical ageing is "proved" to follow from Einstein's special theory. The reader must be left to appraise this procedure for himself.

In case any reader may think that an eminent scientist like Bronowski would not make such an obvious error, I will now give a similar example from elsewhere in his writings. In one of his books[22], Bronowski describes the well-known "Buffon's needle" experiment, in which the value of π is estimated, using probability theory, by tossing a needle many times onto a horizontal surface on which there is a grid of equally-spaced parallel straight lines. The estimate is based on the length of the needle, the spacing between the lines, the total number of tosses, and the number of tosses for which the needle falls on a line. Bronowski, just after warning against relying on probabilistic deductions based on too few data, tells his readers about an Italian mathematician who supposedly achieved an estimate for π which was correct to the sixth decimal place, based on an experiment involving "well over 3,000" throws of the needle. The number of throws in the experiment he mentions is not nearly enough for the accuracy claimed, since one more throw, *whatever its outcome,* would make the error in the estimate much larger than the hundred thousandth part of one per cent that Bronowski states that it is; even though Bronowski explicitly warns against this type of error in the previous paragraph of his book, he presents the experimental result with obvious approval. The published experimental result was probably a minor hoax; interested readers can find a detailed discussion in a very interesting book by O'Beirne[23].

In his book *The Relativity Explosion*[24], the well-known writer Martin Gardner also

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21. H. Bergson, *Duration and Simultaneity,* Bobbs-Merrill Co. Inc. (1965). (Translated by L. Jacobson, with an Introduction by Herbert Dingle.)

22. J. Bronowski, *The Common Sense of Science,* William Heinemann (1951).

23. T.H. O'Beirne, *Puzzles and Paradoxes,* Oxford University Press (1965).

24. M. Gardner, *The Relativity Explosion,* Vintage Books (1976).

misrepresents Dingle's arguments; like so many others, Gardner misses the point that Dingle was not arguing about the asymmetrical ageing involved in the orthodox resolution of the clock paradox. The following sentence shows the misrepresentation: "No physicist except Professor Dingle doubts that the astronaut's clock, when he returns, will be slightly out of phase with a nuclear clock that stayed at home." Although Gardner appends to that sentence a footnote which refers to *Science at the Crossroads* (and which also admits that Dingle is not quite alone in his beliefs), the views attributed to him in the sentence quoted are completely contrary to those expressed by him in his book. Gardner also claims that Dingle believed that *all* of relativity is wrong, both the special and general theories; whatever Gardner's authority may be for making that claim, that is certainly not stated as Dingle's belief in *Science at the Crossroads*.

Of all the various misstatements about Herbert Dingle, one of the most startling appeared in a belated review of *Science at the Crossroads* that appeared in 1976 in *The British Journal for the History of Science*[25], claiming that he had died in 1974. In this instance the reviewer and the journal were unable to avoid admitting the cogency of Professor Dingle's subsequent rebuttal[26].

In the next chapter we shall continue the story of the response of the scientific community to *Science at the Crossroads*, starting with publications that appeared soon after Professor Ziman's review in *Nature*[27].

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25. L. Pyenson, Review of "Science at the Crossroads", *British Journal for the History of Science* **9** pp. 336-337 (1976).

26. H. Dingle, *British Journal for the History of Science* **10** p. 94 (1977).

27. J. Ziman, "Science in an Eccentric Mirror," *Nature* **241** pp. 143-144 (1973).

CHAPTER 6

THE DEBATE CONTINUES

> In those days we believed in the triumph of reason, of the 'brain'. We had yet to learn that it is not the brain which controls human beings but the spinal cord -- seat of the instincts and of blind passions. Even scientists are no exception to this.
> Max Born: *The Born-Einstein Letters.*

Continuing the story of the controversy, let us start by noting some of the correspondence that appeared in *Nature* after Ziman's review[1], which is discussed in the previous chapter.

G.F.R. Ellis[2] described Ziman's review as being "admirable", and agreed that the answer to Dingle's Question was "the fastest working clock between any two events is one that travels between them by free fall". H.L. Armstrong pointed out that none of the critics appeared to have faced Dingle's claim that "all of the alleged experimental verifications involve circular arguments in their interpretation"[3], and also criticized Ellis's answer because the question was "which of the two . . ."; not "which of all possible . . .", adding: "Suppose that neither of the clocks was in free fall."[4]

Dingle[5] also published a criticism of the Ziman-Ellis answer, saying "Neither of the events need be at either of the clocks concerned, so the statement, 'the fastest working clock between any two events is one that travels between them by free fall', is futile." Unfortunately, in the same letter, in trying to reformulate his Question, Dingle made the situation somewhat more confused by writing as follows, referring to time intervals measured by two clocks A and B:

> My question is: how does the theory indicate which clock gives the larger interval? If A has velocity 0 and B velocity v, the Lorentz transformation makes that clock A; if B has velocity 0 and A velocity v, it makes that clock B.

I believe that this statement is too general, because it refers to the intervals between two events "occurring at any ascertainable positions at any times", whereas Dingle had claimed elsewhere[6] that the result depended on the pair of events chosen. To be more specific, if the clocks A and B mentioned above are taken to correspond to clocks A and

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1. J. Ziman, "Science in an Eccentric Mirror," *Nature* **241** pp. 143-144 (1973).
2. G.F.R. Ellis, "Special Relativity Again," *Nature* **242** p. 143 (1973).
3. H.L. Armstrong, "In Defence of Dingle," *Nature* **242** p. 214 (1973).
4. H.L. Armstrong, "Freely Falling Clocks," *Nature* **244** p. 26 (1973).
5. H. Dingle, "Dingle's Question," *Nature* **242** p. 423 (April 6 1973).
6. H. Dingle, "Relativity and Electromagnetism: An Epistemological Appraisal," *Philosophy of Science* **27, No. 3** pp. 233-253 (July 1960).

B respectively in an earlier paper[7], then the time intervals measured by the two clocks, between the events E_0 and E_2 described in that earlier paper, do not seem to correspond to the statement quoted above. I believe that, in his letter[8], Dingle was making a paraphrase of the claim by various advocates of the theory that "a moving clock runs slow", and inadvertently made a somewhat more sweeping statement than was justified.

Unfortunately the three replies to this letter that were published[9-11] concentrated on the question of the events, and therefore did not answer the original question. One of these replies (by Stedman) included a comment on the Ziman-Ellis answer to Dingle's question, referring to a paper[12] which pointed out that it is possible for two clocks to travel between the same pair of events by different free-fall paths; since the Ziman-Ellis answer gives no way of distinguishing between these, it does not even answer the question "which of all possible clocks . . .", much less the original question.

Dingle subsequently published yet another formulation of his question, which appeared in the August 31 1973 issue of *Nature*[13]. After some months had elapsed without further published answers, he submitted another letter, dated 30th January 1974, to *Nature,* but the Editor would not publish it. It was later published elsewhere[14]. The letter of refusal from the Editor (Dr. David Davies) has already been published in full[15]; let us study the following excerpt from it:

> Many scientists, Born, McCrea, Ziman and Roxburgh amongst them, have done you the courtesy of discussing your question, and yet I see no demonstration by you of why their answers are not acceptable. Instead, they are accused of "evasive comments" and "intricate mathematics" -- even when there is barely a mathematical symbol around. A simple question does not necessarily yield a simple answer; as a scientist you know that as well as I.

It is interesting to compare the last sentence of the above quotation with the explicit statement of one of the Editor's chosen authorities, J.M. Ziman, that the answer to Dingle's Question is simple. Ziman's and Roxburgh's attempts to answer Dingle were discussed in the previous chapter; let us now see what Born and McCrea have said.

Born's reply to Dingle is discussed on pages 42-43 of *Science at the Crossraods,* where it is pointed out that Born claimed that Dingle should have asked a different

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7. H. Dingle, "The Case Against Special Relativity," *Nature* **216** pp. 119-122 (1967).
8. H. Dingle, "Dingle's Question," *Nature* **242** p. 423 (April 6 1973).
9. R. Jacob, "Another Answer to Dingle's Question," *Nature* **244** p. 27 (1973).
10. M. Whippman, "Whippman's Answer," *Nature* **244** p. 27 (1973).
11. G.E. Stedman, "Stedman's Answer," *Nature* **244** p. 27 (1973).
12. B.R. Holstein and A.R. Swift, "The Relativity Twins in Free Fall," *American Journal of Physics* **40** pp. 746-750 (1972).
13. H. Dingle, "Dingle's Question," *Nature* **244** pp. 567-568 (August 31 1973).
14. "30th January, 1974: Dingle's Letter to 'Nature' ", *The New-Church Magazine* **93** pp. 121-123 (October-December 1974).
15. "22nd February, 1974: Letter to Dingle from Editor of 'Nature' (David Davies)", *The New-Church Magazine* **93** pp. 123-124 (October-December 1974).

question from the one he actually asked, and that Born changed the wording of the question and then answered the new question. At least one supporter of the theory, Dr. L. Marder, was critical of Born's attempt to answer Dingle, and made the following comments about it[16]:

> In a sense, it was a pity that Born then took up the challenge, because a satisfactory reply to Dingle needed more time than Born wished to devote to the matter. His brief reply, in *Nature,* consisted largely of a 'correction' to Dingle's question (hardly likely to produce the desired effect) and a partially explained space-time diagram.

In addition to changing Dingle's question, Born also made a serious logical error when he made the following claim:

> The simple fact that all relations between space co-ordinates and time expressed by the Lorentz transformation can be represented geometrically by Minkowski diagrams should suffice to show that there can be no logical contradiction in the theory.

That statement is illogical because the Lorentz transformation is only a part of the special theory of relativity, and it is not valid to claim that consistency (or any other property) of part of the theory is a sufficient condition for the whole theory to be free of logical contradiction. (Further discussion of the relationship between the transformation and the theory can be found in Chapter 13.)

The disturbing feature of this situation is that, during the many years since Born's illogical claim was made, not a single supporter of the theory, as far as I am aware, has published a word of protest at this illogical claim; furthermore, the Editor of *Nature* actually upheld Born's reply as an example showing that further discussion was unnecessary.

Born himself, according to Dingle[17], refused even to read Dingle's reply, claiming that his own argument was irrefutable. In view of the fact that Dingle had issued a challenge to the integrity of scientists, one might have hoped for a more open-minded attitude from Born, who wrote elsewhere[18] that "the belief that there is only one truth and that oneself is in possession of it, seems to me the deepest root of all that is evil in the world."

In May 1980 I sent to Professor Sir Karl Popper a copy of a brief note which I had then recently published, a correspondence item in the *Canadian Electrical Engineering Journal*[19] which also criticized Born's note. Popper pointed out, quite correctly, that the wording of one of my sentences was unsatisfactory, as I had written there, referring to Born's sentence that I quoted above "That sentence contains an elementary logical fallacy, in that it claims a property of part of the theory (The Lorentz transformation) to be a sufficient condition for the validity of the whole theory." Popper pointed out that Born

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16. L. Marder, *Time and the Space-Traveller,* Allen & Unwin (1971).

17. H. Dingle, *Science at the Crossroads,* Martin Brian & O'Keeffe, London (1972).

18. M. Born, *My Life and My Views,* Charles Scribner's Sons (1968).

19. I. McCausland, "Science on the Defensive," *Canadian Electrical Engineering Journal* **5, No. 2** pp. 3-4 (April 1980).

had not claimed that the theory was *valid,* but only that it was free of logical contradiction. In a letter to Popper, dated August 12, 1980, I conceded that the word "consistency" would have been better than the word "validity" in the sentence I wrote, and expressed my criticism of Born in the following argument, which I still believe to be valid:

> Born's argument involves two propositions, which may be expressed as follows:
> (1) The Lorentz transformation possesses property X (the nature of which is not in dispute).
> (2) Einstein's special theory of relativity is free of logical contradiction.
>
> Born's argument states that (1) is a sufficient condition for (2). I believe that this argument is logically fallacious, because the transformation does not contain the theory, nor is it identical to the theory. The theory contains the transformation, but not vice versa.

In a letter of reply, dated 2 September, 1980, Popper agreed that the Lorentz transformation is not the whole of Einstein's theory, but would not agree with my argument as a whole. In the same letter he told me that he had known both Dingle and Born, that Dingle was a minor light at his best, but that Born was a very great man, both in his tremendous achievements as a physicist and as a moral being. When I replied on September 22 I sent Popper a copy of a very glowing tribute to Dingle that Popper had written as a letter on the occasion of Dingle's seventieth birthday in 1960; the text of that letter can be found in a note published by Haymon[20]. Popper seemed rather surprised at being reminded of that letter; he made some comments on it in an attempt (which I did not find very convincing) to reconcile it with the comment he had made in his letter to me. I shall not discuss his comments, partly because some of them were made in confidence, and partly because the relative eminence of Dingle and Born, as physicists or anything else, is not relevant to the question of the validity of special relativity.

In the same letter to me as his comments on his letter of tribute to Dingle, Popper told me that I had made several mistakes (which he did not identify further) in my comments on Born, but that Born had made no mistake; he also said that he had shown this "to anybody's satisfaction who is not as stubborn as Dingle", and made some other comments which appeared to mean that he was unwilling to discuss the matter further. I wrote one more letter to him but, receiving no reply, gave up. The situation, then, is that this eminent professor of logic, while agreeing that the Lorentz transformation is not the whole of the special theory, continued to uphold Born's claim that a property of the Lorentz transformation is a sufficient condition for the whole theory to be free of logical contradiction. This seems to me to be a very strange situation.

Let us now turn to McCrea's answer to Dingle's criticisms. Professor W.H. McCrea was one of Dingle's most prominent critics during the debate on the clock paradox, at which time Professor Dingle believed the theory to be valid, and he has also attempted to refute Dingle's claim that the theory contains a contradiction. In one of his attempts to refute Dingle's argument, McCrea wrote as follows[21]:

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20. M. Haymon, "Herbert Dingle, 1890-1978," *Journal of the British Astronomical Association* **89** p. 394 (1979).

21. W.H. McCrea, "Why the Special Theory of Relativity is Correct," *Nature* **216** pp. 122-124 (1967).

> About the first thing that relativity theory does is to deny any operational meaning to the notion of simultaneity at two different places. Naturally, this fundamental feature in the theory is not affected in the slightest by any arbitrary conventions we may adopt for the synchronization of clocks. The latter is merely a particular way of putting the readings of two relatively stationary clocks in 1-1 correspondence with each other.

This seems to be a very strange argument. In fact, one section of Einstein's original paper on special relativity [22] carries the title "Definition of Simultaneity", in the course of which he writes:

> Thus with the help of certain imaginary physical experiments we have settled what is to be understood by synchronous stationary clocks located at different places, and have evidently obtained a definition of "simultaneous," or "synchronous," and of "time."

It is on this definition, as well as on the two postulates which are stated later in the paper, that the theory is based. One need only glance at almost any book on special relativity to see how much use is made of sets of synchronized clocks in deriving many of the results of the theory. As Dingle pointed out in his reply to McCrea[23], if one wishes to be free to choose another definition one must first repudiate the theory and then start again from scratch.

In a later attempt to refute Dingle, McCrea, referring to a request by Dingle for the false step in his argument to be pointed out, replied as follows[24]:

> The false step is that Dingle regards the situation treated by relativity as the symmetric comparison of one single clock with another identical single clock (in relative motion). This is not the situation. Actually many colleagues have pointed this out, or given an equivalent answer.

But, as the reader is aware, Einstein stated explicitly that a (single) clock at the equator would work more slowly than an identical (single) clock at one of the poles. Unfortunately McCrea did not identify any of the "many colleagues" whom he claimed to support his argument, but it is clear that Ziman, for example, does not; he stated that Dingle's question, about which *of two clocks in uniform relative motion* the theory required to work slower than the other, was "a perfectly reasonable question to which science should indeed give an answer".

From our discussion of the replies of Born, McCrea, Ziman, and Roxburgh, the reader may judge the cogency of the reasons given by the Editor of *Nature,* in the letter in which he upheld the replies of these scientists as if they were authoritative, for refusing to publish a letter from Dingle asking for an answer to his arguments.

It may be appropriate to describe here a minor sequel to the events described in this chapter. I rewrote my article *The Dingle Affair,* which was mentioned in Chapter 5, and

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22. H. A. Lorentz, A. Einstein, H. Minkowski, and H. Weyl, *The Principle of Relativity,* Methuen (1923).

23. H. Dingle, "The Case Against the Special Theory of Relativity," *Nature* **217** pp. 19-20 (1968).

24. W.H. McCrea, "Definitions and Realities," *The Listener* **82** p. 315 (1969).

published it privately as a booklet in 1977. In it I quoted the sentence referring to Born, McCrea, Ziman and Roxburgh, from the aforementioned letter written by Dr. David Davies to Professor Dingle, and I pointed out some of the unsatisfactory features of their arguments. Having sent a copy of the booklet to Dr. Davies, I later wrote to him on July 18, 1977; the following is an excerpt from my letter:

> I would be glad to know, for example, whether you still believe that the sentence I quoted from your letter is valid as an argument in support of your refusal to publish the item in question. If not, I would be glad to know whether you have a new statement that you might now make in place of that sentence. If you do not make a positive response to one or other of the above queries, then may I ask whether you would now be willing to expose the subject again in *Nature?* .

Dr Davies replied on July 26; the entire text of his letter is as follows:

> I have no particular plans to set this hare in motion again in *Nature*. I cannot think of anything that needs to be said which hasn't already been said.

I leave it to the reader to decide from this reply whether Dr. Davies continued to believe in the validity of the sentence in question.

CHAPTER 7

THE ROYAL SOCIETY

The great communion of science is not unlike a religion, or a Church, in our modern society. The doctrines of observational accuracy, rational theory and experimental verification shall be our Trinity, with the President of the Royal Society as our Pope and the Nobel laureates as our patron saints. With the Science Research Council as a College of Cardinals, with laboratory directors as abbots, with the great accelerators and radio telescopes as our cathedrals, the model is complete.
But, alas, we have no martyrs. Since that equivocal episode of poor old Galileo, it has been a wonderful success story, a primitive sect waxing mighty until made one with the State. Without conflict, without blood, without the opposition of the temporal to the spiritual power, we have been incorporated in the Establishment.
J.M. Ziman: *Impact of Science on Society,* Vol. 21, 1971

And the trouble is that man, by a series of enormous technological advances made in very recent times, has acquired almost unlimited power, at a time when his social progress gives no guarantee that this power will be wisely used.
Lord Todd: Presidential Address, British Association, 1970.

The purpose of this chapter is to place on record a correspondence between Professor Dingle and Lord Todd, soon after the latter had been elected President of the Royal Society. Since Professor Dingle sent to Lord Todd a copy of some correspondence between one of his collaborators, Mr. Mark Haymon, and the Editor of *Nature,* that correspondence is recorded first. Mr. Haymon, a London lawyer who had become interested in Professor Dingle's crusade after the publication of *Science at the Crossroads,* wrote a letter to the Editor of *Nature* in December 1975. The following is the text of that letter, the publications referred to in the first sentence being Dingle's two notes, both entitled "Integrity in Science", appearing in the issues of *Nature* dated June 12 and July 17, 1975:

INTEGRITY IN SCIENCE -- DINGLE'S QUESTION

Five months have passed since Professor Dingle issued in your columns the latest of his appeals for an answer to a patently simple question described by Professor Ziman (*Nature* January 12 1973) as a "perfectly reasonable question to which science should indeed give an answer." According to his book, *Science at the Crossroads,* reviewed by Professor Ziman in that issue of *Nature,* Professor Dingle's appeals have now been made over a period of nearly two decades. Yet no answer has appeared although not only does the honour and credit of "science" depend on the provision of an answer but also, in view of the nature of modern physical experiments, possibly the safety of the whole population.

The question is too plainly simple for any normally intelligent person, scientist or not, to mistake an evasion of it for an answer, though he may be unqualified to have judged the soundness of an answer had one emerged. The question is simply this. The special relativity theory (which, according to a *Nature* editorial, pervades the whole of modern physics) says that if two similar clocks (or persons) move uniformly at different speeds, they work (or age) at different rates, the slower-moving having the faster rate. But it says also that since all standards of rest are equally valid, either clock may rightly be called the slower-moving. The theory therefore seems inevitably to require each clock to work uniformly faster than the other, which is plainly absurd. The obvious question, then, is: how does the theory distinguish the actual slower worker from the other? The facts cited in Dingle's letter in *Nature* of August 31, 1973 prove conclusively that the theory does claim the rate-difference to be actual, as Ziman's comment also must imply; so unless a distinguishing feature exists (and clearly nothing can justify a non-disclosure of it if it does) Dingle seems unanswerable when he says that the theory crumbles and the imposing edifice of modern physics, with all that it houses, rests on sand: what becomes of such structures was foretold long ago. Nevertheless, the long-awaited statement of the distinguishing feature continues to be withheld.

In his letter to *Nature* of June 12 1975, Dingle gives two forms of answer to his question which quite obviously admits of no third form. It cannot be answered by experiment: it does not ask what happens but what the theory requires to happen. Therefore any physicist who understands and accepts the theory must at once be able, and has the duty, to justify his use of it by completing the unfinished sentence in Dingle's answer (1) (viz. -- the slower-working clock is that which . . .). Yet, during 17 years of application of the challenged theory, not one has done so.

I have reason to believe I voice the mis-givings of many at this inexplicable failure of ''science'' to fulfil what Ziman declares to be its duty, a failure accentuated by the fact that the latest purported answer, by Mr. Maddox in *Nature,* June 12, 1975, agrees with every previous ''answer'', including Ziman's, only in being one of a succession of unrelated obscurities, none of which meets the question asked. Mr. Maddox's statement requires us to believe that a theory is important precisely because it requires each of two identical clocks to work at the same time faster than the other. Your readers, who believe themselves intelligent enough to deserve a stronger reason for rejecting commonsense, are entitled to request from those, whoever they may be, who direct the course of experiments in atomic energy establishments, universities and elsewhere, in whose integrity we are all compelled to trust, an early and long overdue published authoritative choice between Dingle's two forms of answer to his question. Persistence in failure to meet this request can now leave no doubt in anyone's mind of the present moral state of ''science'' and must lead inevitably to the use of all proper means of protection against such an abuse of the unrestricted freedom of experiment which physical scientists now enjoy.

I trust, Sir, that by early publication of this letter, you will enable *Nature* to take an honourable part in regaining for ''science'' the respect it is steadily losing.

The Editor of *Nature,* Dr. David Davies, replied to Mr. Haymon on 19th December 1975 as follows:

I cannot see anything new in your letter of 17th December which makes a compelling case for us to publish it in *Nature.* I think Dingle's question is so well known to scientists, that the continued repetition of the same material profits no-one.

On January 9, 1976, Professor Dingle sent a long letter to Lord Todd, then recently elected as President of the Royal Society; as already mentioned, he enclosed with the letter a copy of Mr. Haymon's letter to the Editor of *Nature,* and also the Editor's reply. The following is the text of Professor Dingle's letter to Lord Todd:

In sending my respectful congratulations on your election to the high office which you now hold, I venture to bring to your notice a situation of the existence of which I have no doubt you are aware but of the details and basic significance of which it is very unlikely that you should be. May I say at once that I write with no personal aim. In my 86th year I have no ambitions of any kind in such a matter, and wish nothing more fervently than to be able with a quiet conscience to retire from the whole affair and spend my short remaining time on more peaceful and appropriate subjects of meditation. The present position, however, is such that I cannot do so, for my unique knowledge and experience of the whole controversy and its implications make it impossible for me honourably to withdraw so long as I might still in some measure help to prevent an outcome which, unless the scientific community can be awakened in time to the moral state into which it has lapsed (mainly unconsciously, I am sure) must sooner or later be disastrous in more than one respect.

To be as brief as possible I introduce the subject by enclosing a copy of a very recent correspondence between Mr. M. Haymon, one of a large and growing number of educated but scientifically lay members of the public (he is a man of standing in the legal world who, I may say, was personally unknown to me until after my book, *Science at the Crossroads* (1972), was published and whose concern with the matter therefore arose quite independently) and the editor of *Nature,* together with some of the recent correspondence in *Nature* which was its immediate cause. A copy of my book relating the previous history has already been sent to the Royal Society. Mr. Haymon's correspondence encloses the kernel of the matter in as small a nutshell as possible; its essence is that a crucial question, on the answer to which depends the validity of the most fundamental theory in modern science -- a question which is simple enough to be fully understood by any normal person and has been acknowledged by a *Nature* reviewer as one which ''science should indeed answer'' -- remains still, after many years, unanswered, while research proceeds as though it had never been asked. This not only violates the basic element of the moral code of science (as expressed, for instance, by the late Sir Henry Dale, whose words are quoted on the first page of Chapter 1 of my book and form, so to speak, its theme-song), but also betrays the trust which, under present conditions, the entire public is compelled to place in the integrity of scientists, whose detailed operations are necessarily far beyond general understanding. The reason given by the leading scientific journal (and I may say that no other is more open to legitimate public questioning) for refusing to allow the educated public to ask ''science'' to fulfil its acknowledged obligation is that there is ''nothing new'' in the request, since the ''question is well known to scientists'': the fact that it has not been answered is apparently insignificant. Clearly, nothing new *can* be given except an answer to the question, and since none of those from whom a genuine answer might properly be expected is willing to give one, and the scientific press is the only medium through which the public can ask it to do so, scientific activity, however apparently irresponsible, is now wholly free from legitimate public questioning. This, I know from personal experience, was very far from Dale's motive in striving for freedom for science, and it would, I am convinced, have horrified him if he could have foreseen that the freedom, when obtained, would have been used to liberate scientists from the duty of meeting informed criticiam.

I know, from my own embarrassingly large correspondence, how widespread is the dissatisfaction at this situation among educated people of all types (I am not speaking of cranks, of course, of whom there is never any lack on all sides of a question), and should a disaster occur in physical research, though the cause might be undiscoverable, the outcry at this indefensible neglect of a much repeated warning would be such that ''science'' would find it impossible to live it down, and the demand for restrictions to be placed on its activities would be irresistible, and fully justified. None of us wishes this, of course, but unless evidence is quickly forthcoming that scientists do realize their responsibilities and are prepared to meet them whatever the consequences to their theories, it is certainly what would happen.

My long intercourse with leading scientists all over the world leaves me in no doubt of the actual nature of the situation. I do not for one moment believe that, with negligible exceptions, there is any deliberate malevolence or *conscious* violation of the moral ideals of science. It is simply that physicists have, unawares, allowed their trust in special relativity to escape the control of reason and become a blind slavery to dogma, for the defence of which any means is held permissible. The best of them (e.g. Blackett, Lovell, Lonsdale, . . . , as quoted in my book) acknowledged that they did not understand the theory (I am sure they did, but mistook their perception of its impossibility for failure to grasp it), but the more mathematical, to whom the experimenters look for guidance, cannot rise to the moral height (greater, of course, in their case) of confessing lack of understanding, even if, which is at least doubtful, they are fully aware of it. The result is that they have become unable to look *at* my question as it is, in all its simplicity, but automatically see *through* it to the inevitable consequence of the only possible answer -- that special relativity is wrong -- and this it has become impossible for them to believe. They are therefore convinced that there *must* be an answer to my question that does not destroy special relativity, but as they cannot see it they either remain silent or produce some irrelevant statement abstruse enough to reduce the non-specialist to silence.

The phenomenon is familiar enough to students of the history of science. Prejudices which, after they have been superseded through the advance of knowledge, are so obviously such that it is difficult to understand how they could ever have been thought other, are nevertheless quite unrecognised while their day lasts. Many of those who rejected Galileo's clearly fatal criticism of Ptolemaic astronomy, and Harvey's of Galen's physiology, for example, were neither knaves nor fools, but were among the wisest and most honourable of their time; but they were simply unable to look at what Galileo and Harvey had to show because their field of vision was fully occupied by their preconceptions, and anything obscuring those was simply an obstacle not to be examined but removed, so as to restore clear sight of the "truth". The parallel with the present case is unmistakable -- with the all-important difference that then one could afford to wait for time to set things right, while the consequences of modern experiments based on an illusion might be unspeakably calamitous.

One of the strongest pieces of evidence for this diagnosis is the variety of the evasions, to say nothing of their character, of my question which have been offered as answers to it -- a fact which has astonished the non-specialists in the subject. A Canadian physicist, Armstrong, for instance *(Nature,* July 6, 1973) appealed in vain for the authorities to agree on so simple a matter instead of each offering a different solution (his letter managed to squeeze into publication during the interval between the editors; others, writing similarly at other times, have been less fortunate), and even in the brief correspondence herewith enclosed, it may be seen how the "answers" of Ziman, Maddox, Kilmister and Synge are totally unrelated to one another. Indeed, the frequency with which those who show me my "error" privately condole with me on the ineptitude of the published replies is the one touch of humour in an otherwise wholly grim situation.

I will not weary you with a number of examples of the *character* of the "answers", but restrict myself to the one most pertinent in this context -- that of Ziman in his *Nature* review of my book (Jan. 12, 1973) where, as Haymon has noted, he acknowledged the responsibility of "science" to answer my question. After a long dissertation on non-euclidean geometry, unintelligible to all but very specialised readers, he concluded: "the answer to Dingle's 'question' is simple: the fastest working clock between any two events is one that travels between them by free fall." Remembering that the question, correctly paraphrased by Haymon, was: which of two specified clocks (which could not possibly both travel between *any* two events) worked the faster, one sees that this is equivalent to a historian, asked who lived the longer, Julius Caesar or Napoleon, replying "The longest-living man was Methuselah." This, in its irrelevance (though in nothing else), is typical of *all* the answers so far given.

Ziman is neither a knave nor a fool. He admits that my question is "perfectly reasonable", and science must indeed answer it, and then produces an "answer" that makes it difficult to believe that he cannot be one or the other. I see no explanation but that his eyes are blind to the actual question and capable of seeing only that special relativity must be saved somehow, and therefore something must be said that will pass for an answer, any means being justified for so necessary an end. Naturally, the intelligent non-scientist, like Haymon, to whom the question is perfectly intelligible, sees that Ziman's ''answer'' is a clear evasion, and draws his own conclusions about Ziman and physicists generally who have nothing better to offer -- conclusions which *Nature's* treatment of the ''question'' seems amply to confirm. The experimental physicists, however, having written off special relativity as beyond their comprehension, accept whatever they are told by those who they suppose do understand it, and proceed happily with their work. Whatever the truth about special relativity may be, it is inescapably plain that those who ''take it for granted'' (Max Born) and apply it in the operation of the most dangerous instruments in the world, are in a state of complete mental confusion about it. The result, if this continues, is inevitable; the only question is how soon and at what cost.

This, then, is the state of affairs which, with all deference, I submit for your consideration. I know, of course, that formally the function of the Royal Society is not to adjudicate on particular scientific questions, but the now existing *de facto* situation is very different from the *de jure* one. When the Royal Society was founded, science had next to no impact on public life: to-day it is the dominant material influence in civilisation, and the Royal Society is, in this country, its chief embodiment. If the public seeks an assurance that ''science'' still preserves in practice the moral code so clearly proclaimed by Dale, it is to the Royal Society that it must naturally look for that assurance; it has no other recourse, and even for that the press is not now an available medium. And if (I should say when, but I formally leave it hypothetical) the normal course of research, whether harmlessly or otherwise, makes it impossible any longer to maintain the validity of special relativity, the responsibility for the attitude of ''science'' to this criticism of the theory, of which overwhelming evidence is already on record, and which is epitomised in *Nature's* reply to Haymon as clearly as anything could be, will inevitably fall mainly on the Royal Society. It would, of course, be presumptuous of me even to suggest what course that body should take, but I should fail in my own duty if I did not lay the position before you as clearly as I can, so that, if possible, past failures may be redeemed within the scientific community before I yield to the pressure that is being brought to bear on me from various quarters at home and overseas to seek the co-operation of extra-scientific agencies primarily concerned with public welfare and the preservation of integrity in public institutions. That is a course which I should be most reluctant to take; I would far rather that the situation were rectified through the spontaneous obedience of science to its own moral precepts (''science . . . not tolerating any lapse from precision or neglect of any anomaly, fearing only prejudice and preconception'' -- H.H. Dale) than through external pressure generated in part by considerations of physical safety; but if *Nature's* reply to Haymon's letter, the significance of which it is impossible for any intelligent open-minded person to miss, remains the last word of ''science'' on the subject, it will be impossible for me to reject it.

The following is the text of Lord Todd's reply, dated 3 February 1976, which he has kindly given me permission to publish:

First of all let me thank you for your very kind congratulations on my election to the presidency.

I do, of course, understand your concern about the problem you outline in your letter and as Sir Henry Dale was my father-in-law I am well aware of his views on science. At the same time I agree with my predecessor, Sir Alan Hodgkin, who wrote to you on an earlier occasion pointing out that it is not for the Royal Society to adjudicate in any scientific disputes and I feel, therefore, that there is little I or the Society can usefully contribute towards solving your problem.

Professor Dingle wrote again to Lord Todd, in a letter dated February 13, 1976, as follows:

> Thank you very much for your reply of Feb. 3 to my letter of Jan. 8. Though the content is disappointing, I do appreciate the literal accuracy of what you say, and it is not for me to question it, though perhaps I should make clear that I was not asking the Royal Society "to adjudicate in a scientific dispute" (a scientific matter) but to deal with a situation in which an admittedly legitimate and important scientific question had received no answer but only a large variety of incompatible evasions (a moral matter). Clearly a dispute cannot arise until a possibly disputable answer exists, and I merely hoped for the Society's assistance in the effort to obtain one.
>
> Your letter, however, imposes on me the duty of approaching you with a further inquiry which I hoped could be avoided. I cannot ignore the incalculably dangerous potentialities of a situation in which "the world cannot afford to lose such a contribution [that of science described by Sir Henry Dale] to the moral framework of its civilisation", and yet no-one with any influence or authority in the scientific community -- the Royal Society, the scientific press, any individual scientist -- will acknowledge an obligation to maintain that contribution or even to take any steps at all to see that it is maintained and not transformed into a worldwide menace. That this situation actually exists is shown conclusively by the fact indicated above -- that, to state it more explicitly, a very simple question, fully within the understanding of any normal mature person, which is clearly, and in fact has been admitted by a recognised authority to be, "a perfectly reasonable question to which science [unfortunately left as an unapproachable abstraction] should indeed give an answer", and to which it is obvious, not only to me but to a large number of highly intelligent persons, including both professionals and laymen in science (and, I have no doubt, to everyone who understands the English language), that an answer must be expressible, and can be seen to be genuinely an answer only when so expressed, in one of the two single-sentence forms given in my letter in *Nature* of June 12 last -- a question, moreover, on the right answer to which depend the effects of the whole future course of physical research and so the possible safety of the whole population -- has for many years been consistently brushed aside and still remains unanswered. I am bound, therefore, to ask your guidance, as President of the publicly supported and acknowledged leading organisation of "science" in this country, as to the means by which the public may receive its due assurance that this menace to its safety either does not exist or will at once be removed.
>
> I want to stress that I do not make this request in any spirit of resentment or provocation or anything of that kind, but because I have no honourable alternative. The facts, however seemingly incredible, are on open record and are indisputable, and the duty they place on any conscientious citizen who is aware of them and their necessary implications, is equally clear and is compulsory. I cannot believe that, despite the limitations of its formal commitments, the Royal Society should be, or is, indifferent to the effects of "science" on public welfare, and it is on my faith that it is not so indifferent that I base my justification for asking you to instruct me as to the agency which, when there are, as now, widely shared grounds for suspecting that the activities of scientists are avoidably endangering public safety, bears the responsibility for allaying those suspicions, and so may, with your approval, in the present instance rightly be asked to exact a genuine answer to my question, which the Royal Society finds itself prohibited from trying to elicit.

Having received no reply to his letter of February 13, Professor Dingle wrote again on March 22 1976 as follows:

I trust you will not think me impatient if I ask for an early reply to my letter of February 13. At my age I am finding that, in more than one respect, the physical effects of a few weeks are equivalent to those of several months not so long ago, and my responsibility in this matter is such that I have no longer the right to let secondary considerations (I have been doing so for nearly two decades) threaten my ability to discharge it before it becomes too late. As the matter now stands, the whole organisation of science in this country, like individual scientists, either ignores or denies the obligation to heed public questioning of the moral integrity and possible social effects of scientific activities, and the Royal Society in particular gives neither help nor guidance to those legitimately seeking reassurance, notwithstanding clear unrefuted evidence of the need for it.

In such circumstances it would be inexcusable for me to refrain from using whatever channels might be most effective, by whatever means, and leaving nothing unrevealed, in order to give the widest possible publicity to the reality and dire implications of the situation, thus exposing the scientific community to charges against which it would have no acceptable defence. I assure you that such a course would be utterly repugnant to me, and I should not take it if any worthy alternative existed, but, for reasons already stated which I need not repeat, unless a genuine answer to my crucial question is quickly forthcoming, before the inevitable operation of the laws of nature robs both question and answer of all significance, there is no such alternative, and I should be culpable in the extreme if I allowed personal revulsion to prevent my performance of such a duty.

I cannot stress too strongly that this is not, except incidentally, a problem within science, but one concerning the basic function of science itself. The issue is not between special relativity and a theory of mine -- I have none (see chapter 10 of my book): it is whether challenges to accepted theories shall be met or evaded, whether the inflexible purpose of science shall be to seek or to avoid the discovery of truth, whatever the truth might be. The ultimate outcome is certain -- truth is inescapable -- but on the result of the present action, which, in addition to its own intrinsic importance, symbolises the whole conflict, depends the honour of scientists, the survival or otherwise of special relativity, and therefore the whole future course of physical research, and so the possible safety of the population. If the Royal Society is not the arena for such a conflict, then I am bound to ask your advice as to where it should take place.

I will do anything within reason and within my power to produce the right answer in the least obtrusive way. I am willing to go anywhere accessible to me to discuss the matter with anyone considered competent to pronounce on the requirements of the theory, and if he will show that my question is not reasonable and fundamentally important (Mr. Haymon's statement of it will suffice), or will undertake to publish in *Nature* his own completion of the unfinished sentence in my letter there of June 12 last or his discovery of anything in the divers existing reactions to the question that makes it possible for him to provide a completion acceptable to him, I will publicly withdraw what I have written on the subject and acknowledge that I have been mistaken. If by any means the truth of the matter, whatever it may be, is brought clearly to light, I will claim no priority for anything concerned with it, but be most thankful to be relieved of a great responsibility and to be able to lapse into obscurity. I will take any other course within my power that might be proposed, regardless of its personal effect on me, that will rightly restore confidence in the moral integrity and sense of social responsibility of scientists. But if the matter remains in its present state, I must, with the assistance of others, do whatever will most effectively provide a remedy, however otherwise undesirable.

May I hope that you will make this unnecessary by soon advising me of the proper course open to members of the public, when they have reasonable grounds for fearing that the activities of scientists do not accord with their moral and social obligations or with their own professed ethical principles, in order that such fears may be authoritatively and convincingly removed?

Having received no reply to the above letter either, Professor Dingle sent the following letter to *The Times* (London) on April 13, 1976.

SCIENCE AND THE PUBLIC

The purpose of this letter is not to discuss a scientific problem, but to make it known that no means exist to ensure that scientists fulfil their moral and social responsibilities. In a current case, after many inquirers had failed with leading scientists and the scientific press, the Royal Society was asked the *general* question how members of the public could obtain reassurance when genuine misgiving arose: the only reply obtainable was: "There is little the Society can usefully contribute towards solving your problem." What it did contribute was nothing.

Since this is an actual, not hypothetical, case, and, with its momentous implications, is understandable by all, its description is needed for a true appreciation of what might otherwise be hard to credit. The special relativity theory says that if two similar clocks (or persons) move at different uniform speeds, (1) the swifter works (or ages) more slowly than the other; (2) all standards of rest are equally valid, so either clock may rightly be held the swifter. Hence, unless the theory indicates some other distinguishing feature, it must require each clock to work more slowly than the other, and since this is impossible the theory must then be false. Although many have repeatedly asked the question "what is this feature?", it remains unanswered, yet the theory continues, in the words of an outstanding authority, to be "taken for granted, the whole of atomic physics is merged with it." Everyone knows what might occur if atomic experiments are wrongly planned. The influence of the theory in the world of ideas -- philosophy, religion, etc. -- is well known and profound.

That the question is not misconceived is sufficiently shown by the verdict of *Nature's* chosen reviewer (who supports the theory); he wrote: "This is a perfectly reasonable question to which science should indeed give an answer", thus dismissing the illusion that the effect postulated is not objectively real. Nevertheless, no answer has come, and what now transpires is the impossibility of preventing "science" from freely flouting its obligations.

The various moral and social results of this can here only be adumbrated. An Open University teacher, who has failed to get his inquiries met, is expected to teach what he cannot honestly accept. A wider and subtler consequence is shown by the latest published defence of the theory against this menace (not by a practising scientist, but disowned by none; their avoidance of the menace is absolute) -- that the theory is important "precisely because" it modifies commonsense *(Nature,* June 12, 1975). Protests against this preposterous claim are refused publication. Yet the assertion does express an actual and most dangerous threat to the trust in the "saving commonsense" which is the life-blood of democracy. It stems from a confusion of two meanings of the word -- spontaneous unreasoned feeling, like that of the Earth's flatness and immobility, which may delude; and the stark rational necessity that bans this behaviour of undistinguished clocks, which cannot. The fallibility of the former is tacitly foisted on the other; the public thereby unwittingly becomes prone to accept any sophistry calling itself "science"; and no agency exists through which this or any abuse of the freedom granted to science can be challenged.

It is necessary that this widely-felt concern shall be openly voiced, so that if the situation is here falsely portrayed, the perversion may be convincingly exposed: if it is not, its withholding from public knowledge would be indefensible.

The above letter was not published in *The Times*. On July 13, 1976, after some preliminary correspondence which need not be recorded in detail, Professor Dingle submitted a letter to *Nature,* the text of which is as follows:

> In a fairly recent review in *Nature,* Professor J. Ziman wrote concerning a scientific question: "This is a perfectly reasonable question to which science should indeed give an answer." "Science", of course, is an abstraction incapable of "answering" anything, so, unless the statement is meaningless, it implies the existence of some concrete agency which has the authority, and on which rests the duty, to answer such questions. In view of the extent to which public life is now dependent on the activities of scientists, the necessity for this is obvious, yet, despite widespread inquiry, the identity of such an agency is undiscoverable. I think (and inquiry confirms this) most people regard the Royal Society, so far as this country is concerned, as a body open to reasonable public questioning on scientific matters affecting public welfare, but that is not so. It has denied such responsibility, and direct inquiry has failed to elicit from it the naming of any other body to which the public can appeal for enlightenment when it has reason to believe that the practice of scientists does not accord with the ethical principles they profess or with the regard they owe to the demands of public welfare and safety. The purpose of this letter is to make this fact generally known, and to ask whatever agency may nevertheless exist, to which the public has the right of appeal in such matters, to reveal its identity as a matter of plain and urgent necessity.
>
> One important misunderstanding must be at once removed. It has been claimed that no "spokesman" for science can possibly exist since no human authority can pronounce on matters on which, according to the basic tenets of science, nature and the laws of reason are the sole arbiters. That, of course, is unquestionable, but the issue here is quite other. It concerns the fact that no agency exists for enlightening the public, not on the course of nature but on the beliefs and voluntary acts of scientists themselves -- no agency for providing information which scientists should possess, and claim to possess, and which the public has a right, as Ziman's statement unequivocally implies, to have imparted to it. No one would expect any specialised organisation to pronounce dogmatically on the effects of alcohol on the human body, but it is the obvious duty of the producers of a beverage to answer the question whether it contains alcohol or not. The questions here referred to, of which that commented on by Ziman is one, are of the latter type. That question concerns the requirements of a theory (i.e. the contents of the theory, like those of the beverage, not its potentialities or validity) which there have been innumerable purported explanations for the public, and so one which it goes without saying the public is entitled to understand, but none of them happens to contain the answer to this question. It is emphatically not "Is the theory true or false?" but "What does the theory say on this point?".
>
> This, however, is but one example -- though perhaps the most important now -- of the general anomaly that although there are admittedly questions which the public has a right to ask of "science", which so deeply affects its whole conditions of life, "science" has no obligation to answer them, and the public no court of appeal if it fails to do so. I hope, Sir, that as a leading medium of communication between science and the public, *Nature* will recognise the disclosure of this little known, but most important, fact, as an essential part of its function.

The above letter was not published in *Nature*. In the next few chapters we shall describe some further attempts by Professor Dingle and Mr. Haymon to obtain an answer to Dingle's Question.

CHAPTER 8

CORRESPONDENCE IN "THE ECONOMIST"

As Einstein himself once said, he succeeded in good part because he kept asking himself questions concerning space and time which only children wonder about.
Gerald Holton: *The Scientific Imagination.*

Between February and May 1977 there appeared a brief but interesting correspondence in *The Economist* on the subject of relativity. The correspondence followed the publication of an anonymous article "Einstein challenged" in the February 5 issue[1], which began with the following sentence:

> Just months after the Viking spaceship brought empirical proof of his theories, a scattered minority of the world's scientists are saying Einstein got his theory of relativity wrong.

Because two of the subsequently published letters were later sent to members of the Council for Science and Society, as is described in the next chapter, they are reproduced below. The first of these two letters was sent to *The Economist* by Professor Dingle on February 17, and was published in part in *The Economist* dated March 5[2]. The following is the full text of the letter; the parts enclosed in square brackets were *not* published:

> May I congratulate the writer of the article, "Einstein challenged" (February 5th), on the general accuracy of his statement of the current controversy, which surpasses that usual in such accounts? [I do not wish to initiate in your columns a discussion appropriate to a scientific journal, but the implications of this controversy are of universal and ethical concern, and since I have been named as a representative of the challengers, may I state my position in terms less open to possible, even if unjustified, misinterpretation?]
>
> The article rightly distinguished the special from the general relativity theory (misleading terms, since, even if the general theory could be proved right, the special theory, with its metaphysical notions of "time-dilation" etc., might still be wrong), and my question concerns only the special theory, on which the Viking observations have no bearing. [I stress the word "question" because that makes the main issue a moral, not a technical, one. I have asked physicists a question, the only possible straightforward answer to which seems to me to destroy the special theory, and it has long remained unanswered, while numerous independent letters to the leading scientific journals from interested and perplexed persons, appealing for an answer, have remained unpublished.]
>
> The point is this. The theory requires that if two similar undisturbed clocks (or persons) travel at different uniform speeds, (1) the faster-moving one works (or ages) more slowly than the other; (2) either may be held the faster-moving, since all standards of rest are equally valid. Hence, unless the theory specifies some distinguishing mark, it requires each to age more slowly than the other, which it needs no specialised knowledge to see is impossible. What, then, according to the theory, is this distinguishing mark? Only a statement, not proof of its accuracy, is asked for: one sentence, of the form: "the theory

1. "Einstein Challenged", *The Economist,* pp. 78-79 (February 5, 1977).

2. H. Dingle, "Einstein Re-challenged," *The Economist,* p. 6 (March 5, 1977).

requires the slower worker (or ager) to be that which . . ." would suffice, yet it is steadfastly withheld. As your article rightly indicates, the theory requires the retardation to be actual, and not merely an appearance of each from the other.

The sub-atomic particle experiment mentioned by your author (quite excusably, since it is frequently brought into the discussion) is irrelevant, for not only was the phenomenon predicted on other grounds, and experimentally supported, before Einstein's theory was born, but also it is at once evident that it does not answer my question -- why are the more massive particles, and not the others, the "moving" ones (unless, indeed, the Earth is fixed and Copernicus was misled)?

[It is hard to exaggerate the seriousness of this affair, from the points of view of both scientific morality and public safety. For example, an Open University tutor, asked by his students what is the truth of the matter, must either tell them what he believes false, or endanger their chance of success in their examination. He cannot get his appeal to the relativity specialists for enlightenment published. What should he tell his students? The publicly-subsidised Royal Society, which is dedicated to the unremitting pursuit of truth in scientific matters, denies responsibility for saying or doing anything at all in the matter, and will not reply to an inquiry as to where the responsibility lies. Meanwhile, experiments fraught with the possibility of incalculable danger if the theory is wrong, continue, and requests for their justification are stifled at birth. I suggest, Sir, that your readers are entitled to full knowledge of this, to enable them to form their own estimate of the present moral state of the scientific community and its possible consequences.]

The other letter with which we are concerned, from Mr. Mark Haymon, appeared in the issue dated April 16[3]; it refers to Dingle's letter, and to a letter from Dr. L. Essen, which appeared in the issue dated March 19[4]. The published text of Mr. Haymon's letter is as follows:

When two such authorities as Professor Dingle and Dr Essen assert that their criticisms of basic scientific assumptions having serious public implications are consistently ignored, no conscientious citizen can help feeling apprehensive. The treatment of Dr Essen's criticism may well demand technical language -- though it should still be available for those learned in this matter -- but Mr Dingle's question is plain to everyone, and everyone can see that a straightforward answer must be expressible by the sentence he asks for. Any suspicion of a hidden obscurity in it is dispelled by the frank admission of Professor John Ziman that "that is a perfectly reasonable question to which science should indeed give an answer" (Nature, January 12, 1973). Nevertheless, neither Mr Ziman nor any other representative of science can be seen to have given anywhere an answer not plainly beside the point.

May I, as a member of the inevitably concerned public, appeal to science, either to complete Mr Dingle's unfinished sentence, or to acknowledge that the theory in question is unsound and should therefore be discarded?

The next chapter is devoted to the correspondence between Professor Dingle and Mr. Haymon and the Council for Science and Society, in which part of the correspondence in *The Economist* was sent to members of the Council.

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3. M. Haymon, "Einstein," *The Economist*, p. 6 (April 16, 1977).

4. L. Essen, "Einstein," *The Economist*, p. 4 (March 19, 1977).

CHAPTER 9

THE COUNCIL FOR SCIENCE AND SOCIETY

It is impossible for each one of us to be continually aware of all that is going on around us, so that we can immediately decide the significance of every new paper that is published. The job of making such judgements must therefore be delegated to the best and wisest amongst us, who speak, not with their own personal voices, but on behalf of the whole community of Science.
J.M. Ziman: *Public Knowledge*

In May 1977, Professor Dingle and Mr. Mark Haymon sent a letter to each member of the Council for Science and Society, a body concerned with the social consequences of science and technology. Prior to sending that letter, Mr. Haymon corresponded briefly with Professor J.M. Ziman, the Chairman of that Council. Since a copy of that correspondence was included in the Dingle/Haymon letter to the members of the Council, part of the correspondence is reproduced below: Professor Ziman's letters are not included here because he refused to give his permission to include them.

On 20th April, 1977, Mr. Haymon sent the following letter to Professor Ziman:

> I hope you will excuse the liberty I am taking in writing to you on a matter which seems to me so serious as to demand early public attention by scientists. I enclose a copy of a current discussion in *The Economist,* and I bring it to your notice for two reasons: first because you have on various occasions clearly stressed the need for strict integrity, and open demonstration of integrity, both in scientific research and in the now very intimate relations of science with public welfare; and secondly because, since you have not resorted to the dismissal of Dingle's question as misguided, irrelevant, a hoax, and so on, but declared it to be a perfectly reasonable question which science must answer, I have confidence that you will be both able and willing to enlighten me on a point which remains obscure, despite efforts which have been made to elucidate it.
>
> My problem is this. "Science" is of course an abstraction, and an answer to Dingle's question can come only from a person or organisation with authority to represent it: who or what is that person or organisation? I know of course that there is no ultimate human authority about the course of nature, but Dingle's question is not about that but about the requirements of a man-made theory, on which scientists can choose to act or not, and it is therefore quite legitimate, as you have said, for those whose lives are affected by their actions to require them to answer it. I understand from Professor Dingle that he has failed to ascertain where the responsibility lies. He has been informed by the President of the Royal Society that it is not a function of that body to provide an answer to his question, but he cannot obtain from any source a statement of who or what is responsible, in spite of the obviously vital importance of such a matter. The numerous contributions, in *The Economist* and elsewhere over the years, from persons mostly unknown in this field, are clearly quite beside the point, and in any case carry no weight.
>
> In your review you did indeed give an answer, as the culmination of a treatment which I am quite unqualified to follow; but as, I hope, a reasonably intelligent and conscientious citizen, it seems to me that that answer, though I cannot question its truth, does not meet the question asked. Dingle asks: "how is the slower-working clock (of the two specified) distinguished?", and you reply: "the fastest working clock between any two events is one that travels between them by free fall". I must confess, with great respect,

that my feeling, on comparing this answer with the question, is equivalent to that of one who, having asked a historian who lived the longer, Julius Caesar or Napoleon, received the reply: "The longest living man was Methuselah".

My object in writing to you now, however, is not to ask for a more obviously direct answer to this question (though, of course, that would be most welcome), but to inquire from what agency the public has the right to require an answer, since I -- and indeed any member of the public with a sense of responsibility -- cannot help agreeing with you that "science" must indeed supply it. Continued neglect of such a duty in the present conditions of civilisation would be a betrayal of responsibility, with possible consequences too dire to contemplate.

Professor Ziman replied to Mr. Haymon on 22nd April 1977, but unfortunately he has refused to grant me permission to publish the letter. He told Mr. Haymon that the President of the Royal Society was perfectly right in informing Professor Dingle that it was not the function of that body to answer any particular question in the name of "science". He said that the job of the scientific community is to make its research results available to all those who wish to use them but that it is the responsibility of the user to make up his own mind according to the evidence available to him. He expressed the conviction that the conventional theory was correct, and said that, on the particular matter of the clock paradox, Professor Dingle was quite wrong in supposing that his views had not been given adequate attention.

Mr. Haymon wrote again to Professor Ziman on 3 May 1977; the following is the text of his letter:

Your prompt reply to my letter of April 20 is very much appreciated but disappointing because it fails to answer the questions I asked and diverts attention to quite different matters. I believe my letter clearly expresses my meaning, and if I now write in terms which otherwise might appear provocative, I hope you will believe that I have no intention of being so, but express myself more bluntly than I should have wished solely in order to prevent further possiblitiy of misunderstanding.

You stated in your review of Dingle's book that his question, faithfully re-expressed in *The Economist,* was "a perfectly reasonable question to which science should indeed give an answer". That was more than four years ago, and no answer has yet come from anyone. What I want to know is: to whom, or to what body, have I the right, as a member of the public whose welfare depends on the integrity of scientists, to apply for an answer? On that you give me no information whatever. You say only that "it is the responsibility of the user [of scientific pronouncements] to make up his own mind". I can only interpret this as a claim that the "user" has no responsibility to the public but is responsible only to himself, and that a member of the public may not ask him to justify actions on which the safety of the whole population depends. Do you accept that or not? If not, will you please give me the name and address of the agency from which I can request an answer to the perfectly reasonable question which you have said "science" must indeed answer. I trust that this makes my meaning quite clear.

Secondly (in the context of my letter, though primarily in a more basic sense, since the former question could not have arisen had this one been answered), there is Dingle's question itself, viz.: what distinguishes the slower-working of the two clocks specified? On this you say not a word. Instead you divert attention to the so-called "clock paradox", which is quite outside the question, as Dingle's book, which you reviewed, explicitly states (p. 184). I know of no treatment of this "paradox" (many of them I do not pretend to understand) in which the solution offered does not regard the fact that one clock changes its motion as the crux of the matter, and in Dingle's question neither clock does so. It is no answer to a question to say that others have answered a different one. I therefore ask you now to reject evasions and complete the sentence: ''the slower-working of the two clocks specified in Dingle's question is that which . . .'' It is plain to the meanest intelligence that only thus can the ''perfectly reasonable'' question asked be truly regarded as answered, and that if it cannot be so answered, the theory must be wrong, with possible consequences unimaginable.

By a remarkable coincidence, a few days after I wrote, a letter appeared in *The Times,* written by you as Chairman of ''the Council for Science and Society'', whose ''particular concern'' is ''to study the social consequences of science and technology''. I therefore repeat with enhanced justification my request for what can be clearly seen to be genuinely an answer to Dingle's question -- an answer which you would be willing to have transmitted to the Minister for Science (as it will be if necessary, with the whole correspondence) as the response of your Council to a request from a concerned member of the public for an answer to what its Chairman has acknowledged to be a question which science must indeed answer.

I repeat that I greatly regret feeling compelled to write in these terms. I assure you that I do so entirely without rancour, and with the sole object of establishing that strict integrity still prevails in the scientific world; but the issues involved in this situation are far too serious to risk misunderstanding by mincing words. I hope that, having the same ultimate desire you will allow no lesser considerations, whatever abandonment of prejudices it may entail, to stand in the way of its attainment.

Professor Ziman wrote again to Mr. Haymon on 6th May 1977; once again, he has refused to grant permission to publish the letter. He told Mr. Haymon that a great many very able scientists had given time and trouble to Dingle's question and had presented their views on it, in a number of widely published documents, and that there is not and has never been any higher authority than the general consensus of such answers. He asked Mr. Haymon to think of all the political and intellectual consequences of bringing back a formal dogmatism of the kind that he suggested that Haymon was hinting at.

Mr. Haymon replied to Professor Ziman on 31st May, 1977; on the same date, Professor Dingle and Mr. Haymon sent their letter to members of the Council for Science and Society. The text of Mr. Haymon's letter to Professor Ziman is as follows:

It is useless to continue. I have twice asked you, in the plainest terms, two questions, one of which you had already described as a perfectly reasonable one, which science must indeed answer, and the other necessarily arising out of the failure of ''science'' to do so. You have answered neither, but made statements that cannot be mistaken for anything but sheer evasion.

The obvious inference is that you cannot answer Dingle's question, or you would have done so instead of referring me to ''a number of widely published documents'', none of which you name. Furthermore, unless you or some qualified agency does so, no honest, intelligent reader of Dingle's letter in *The Economist* can doubt that scientists consider themselves no longer bound by the moral obligations generally believed to govern their actions, as expressed for instance by Sir Henry Dale and quoted in Dingle's book, or recognise any responsibility for safeguarding the public against the probability of immeasurable disaster arising from their uncontrolled activities. In these circumstances Professor Dingle and I have no honourable alternative to submitting the matter to the highest authorities available -- the Church on the moral side and the Government on the social side. Accordingly we propose to submit the facts, including any correspondence with you, to the

Archbishop of Canterbury and the Minister for Science and Education, for their consideration of the bearing of their respective functions on the situation as now clearly revealed.

It has been disclosed, however, that the Council for Science and Society of which you are Chairman, though apparently under no legal obligation to safeguard the interests of the public against infringements by scientists of their ethical principles, has a moral concern for such end, and has expressed a hope "to become recognised as a responsible body in the area where science and social ethics interact". Accordingly, we are first submitting the facts of the matter to the members of that Council individually; and I ask you, as its Chairman, to inform me, within 28 days of the date of this letter, what reply the Council, as a body with this knowledge at its disposal, accepts as being adequate to meet Dingle's question in terms as readily intelligible to the normal educated person as is the question itself, so that appeal to the highest authorities, if that still remains necessary, can be based on the statement of the most responsible representative of "science" in relation to this question that we have been able to discover.

I need hardly say that we hope most earnestly that the Council will give a reply to Dingle's question that will be so clearly a straightforward answer as to make the further action indicated unnecessary, but anything short of this will make it compulsory, and will further make necessary the use of all proper means available for making widely known to the general public "the way in which science works", according to your view, in contrast to the generally assumed way of submitting unreservedly to the demands of truth.

The following is the text of the letter dated 31st May, 1977, from Professor Dingle and Mr. Haymon, to members of the Council for Science and Society:

We are sending you, as a member of the Council for Science and Society, copies of a portion of a current correspondence in *The Economist* (omitting for the sake of brevity, all that is not immediately necessary for the present purpose; it can, of course, be looked up if desired) and of a correspondence thereon between one of us and the Chairman of your Council, Professor Ziman. While we both write with equal recognition of our duty as citizens, we are complementary in the sense that one of us, as a lawyer and a non-scientist, is satisfied that he, with many others, fully understands the moral and social implications of the situation, notwithstanding his inability to discuss its technical aspects -- in short, that he can distinguish an answer to a question from an evasion of it, and can appreciate the imperative public need that an answer shall be given to this question, though he may not have been able to judge of its validity if it had existed -- while the scientific qualifications of the other on this particular subject, as he reluctantly but necessarily had to set them out in a recent book, show that they are not exceeded, even if equalled, by those of any other living scientist. We are therefore prepared to answer inquiries on any aspect of the situation which you might feel it necessary to make.

The enclosures, we think, need little addition in order to make you fully aware of the situation. We would sum it up by saying that we are primarily actuated by the conviction that, especially in an age in which science has such an enormous effect on the whole material course of civilisation, moral integrity should be the first consideration of scientists and the evidence shows all too plainly that it has largely, even if unconsciously, been lost. We think it not inappropriate to quote the words of the late Sir Henry Dale, referred to in the Ziman correspondence, since they might not be otherwise available to you; it was largely through the efforts of Sir Henry, when President of the Royal Society, that scientists were granted the freedom of research which they now enjoy. He wrote:

> And science, we should insist, better than any other discipline, can hold up to its students and followers an ideal of patient devotion to the search for objective truth, with vision unclouded by personal or political motive, not tolerating any lapse from

> precision or neglect of any anomaly, fearing only prejudice and preconception, accepting nature's answers humbly and with courage, and giving them to the world with an unflinching fidelity. The world cannot afford to lose such a contribution to the moral framework of its civilisation.

> We ask you to compare this with Professor Ziman's last letter, adding only that there is no "consensus of such answers" to "Dingle's question" as he indicates since there has been none at all. Many have "presented their views" on matters not unrelated to it, and these show no "general consensus", but the widest diversity. The "formal dogmatism", which Ziman associates with one of us is, as you will see, nothing other than a plain request to be informed what concrete agency the public is entitled to approach for an answer to the particular question to which he has said "science should indeed give an answer".

> We shall be very pleased to supply any further information that you might wish to have in order to enable you to understand the position fully and to reach a right conclusion on the matter. As you are probably aware, the theory whose tenability depends on a satisfactory answer being given to Dingle's question is, by general consent, in the words of the late Max Born, "taken for granted, the whole of atomic physics is merged with it". Such is the seriousness of the issue involved.

Several members of the Council replied to the above letter. The replies ranged from constructive to vitriolic, and I shall now consider two of the more constructive ones; I would also have included the most vitriolic one, but its writer would not give me permission to publish it or even excerpts from it. Let us look first at the letter written by Dr. John Habgood, then Bishop of Durham, now (1988) Archbishop of York, to Mr. Haymon on 2nd June 1977. Dr. Habgood had earlier published a review[1] of Professor Dingle's book *Science at the Crossroads*. Dr. Habgood has kindly given his permission for publication of his letter, the text of which is as follows:

> I received your letter and enclosures about Professor Dingle's question in my capacity as a member of the council for Science and Society.

> I note that in your letter of May the 31st, addressed to Professor Ziman, you state that your final recourse must be to the Archbishop of Canterbury and the Minister of State for Science and Education, both of whom, I suspect, would find themselves somewhat nonplussed to be confronted with questions well outside their technical competence.

> Professor Ziman is perfectly right in saying that science does not operate in a hierarchical mode, and that it is therefore useless to look for final authorities to make binding pronouncements.

> Several years ago I conducted a correspondence with Professor Dingle, following a review I wrote about his book *Science at the Crossroads,* and it seems to me, on subsequent reflection, that there are two basic points at which the blockage in communication has taken place:

> (1) Professor Dingle treats his quotation from Sir Henry Dale as if it were holy writ. In my view the quotation presents a highly idealised view of science, which bears little relationship to what actually goes on. Much recent writing on the nature of science bears increasing testimony to the confused character of the whole enterprise and the large number of

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1. J. Habgood, "Evidence or Theory?," *Frontier* **16, No. 1** pp. 55-56 (February 1973).

loose ends which are constantly being left as science progresses. To say this is not to invalidate the enterprise as a whole, but it is to put science in the same league as most other human activities. Karl Popper's intellectual biography *Unended Quest,* gives a much truer picture of what it is all about than Sir Henry Dale's quotation.

(2) There is an underlying assumption in your letters that simple questions ought to have simple answers. I would have thought that it is overwhelmingly apparent in every aspect of life, not least in your own field of the Law, that this is a false assumption. It frequently happens that the simplest questions are the hardest to answer and that those who demand an answer in a particular form are therefore doomed to disappointment.

I make these two general points without wishing to prejudge the merits of Professor Dingle's scientific case, because it seems to me from your replies to Professor Ziman that you have not appreciated their significance.

Mr Haymon replied to Dr. Habgood, in a letter dated 22 June, 1977, in the following words:

I much appreciate your prompt reply to our inquiry, but I must confess that it fills me, and also Professor Dingle, with astonishment. Can you really mean that Sir Henry Dale's intensely felt statement, made, as Professor Dingle who knew him well recalls, at a critical time for science, and on the basis of which, scientific research was granted the freedom which it now enjoys, should not be ''treated as holy writ'' (i.e. presumably as something demanding strict acceptance and effort, however imperfect, to follow) but merely as something to be compared with ''what actually goes on'', and to be ignored if it does not conform to that? We agree with you completely that what actually goes on now ''bears little relationship'' to Dale's ideal, but we are quite unable to share your evident satisfaction that such a distinction has developed. It is Professor Dingle's conviction, and mine also, that in the context in which Dale's statement was spoken and afterwards printed, and in the present context, it is either the equivalent of holy writ, and should be honoured as such, or else a shameful lie, which should be openly disowned: any other course would be plain cowardice -- and worse. May I ask you a question? I ask it with all respect, but with all seriousness too -- not as a rhetorical question, but one to which I think a clear answer is necessary, to help remove all misunderstanding. Would you say of I Cor. 13 that it presents ''a highly idealised view'', which bears little relationship to ''what actually goes on'', and hold that what actually goes on ''gives a much truer picture of what it is all about'' then St. Paul's quotation?

Here are the bare facts of the situation that concerns us; every one of them can be substantiated beyond possibility of question:-

(1) The Chairman of your Council, Professor Ziman, has declared Professor Dingle's question to be a perfectly reasonable one, which science must indeed answer.

(2) Notwithstanding persistent efforts over many years to obtain the answer, it has not been forthcoming, though there have been numerous comments which are plainly evasive (Ziman's own, already cited, is a typical one) and many of which are mutually contradictory.

(3) No one has questioned that the theory does require the two things with which Dingle credits it in his *Economist* letter, nor could anyone regarded as an authority on the theory do so, as he must know, without laying himself open to direct refutation from both Einstein's writings and his own previous statements, with which Dingle is well acquainted. That being understood, it is obvious to any intelligent person, scientist or not, that an

answer is essential if the theory is to survive.

(4) The theory is, in the true words of one of the highest authorities, "taken for granted, the whole of atomic physics is merged with it". Any other "authority" would agree with this.

(5) The enormous potential danger of modern atomic research must, sooner or later, become actual if the theory underlying it fails.

It is in the light of these facts that one must view the satisfaction of a Bishop of the Anglican Church with things as they are, and draw his own conclusions.

Is this really your last word on the subject, and if the matter has to be referred to the Archbishop of Canterbury, are you content that he should draw *his* conclusions from it concerning the contribution which one of his bishops has made to the establishment of acceptable relations between science and society? Neither Professor Dingle nor I has any fear that he will be incapable of seeing the moral implications of such a situation, despite their technical setting.

I regret very much that, in so serious a situation, you should have resorted to the glib generalisation that "it frequently happens that the simplest questions are the hardest to answer". Can you tell me of any question at all on any subject that cannot be so dismissed? If Peter had (much more excusably) replied thus to the question in Mark, 8, 29, would your Church have been built on rock or on sand? Can you really think your generalisation a worthy comment on this particular situation? But, whether you do or not, you must allow me to say that you are quite wrong in thinking that "there is an underlying assumption in [my] letters that simple questions ought to have simple answers". (Ziman described his obviously spurious answer, quoted in my correspondence with him as "simple", but let that pass). It is quite immaterial whether the answer to Dingle's question is easy or hard; those who use a theory in practice should certainly understand it, and therefore [be] able to answer a reasonable question about what its contents are. (This question, I repeat, is about a *theory,* not about the course of nature). The sole relevant fact is that no one has done so, yet the theory continues to be used as though the question had never been asked. Professor Dingle is convinced that this is because an answer to this question is not merely hard, but impossible, so I am not likely to have assumed it simple. It needs no specialised knowledge to see that if he is right the theory must be wrong; that perception at least is simple. And in that case our peril is extreme, and that is why it is imperative that a Council concerned with the relations between science and society can properly be expected to see that an answer which is genuinely an answer, whatever that answer may be, is provided, or else acknowledged to be impossible.

Won't you reconsider the matter, and give me a reply which you consider, on reflection, to be wholly consistent with the ethics of Christianity? If so, I, and I am sure Professor Dingle also, would most gladly regard it as completely superseding all that you have already written.

Dr. Habgood replied to Mr. Haymon on 23rd June, 1977, in the following words:

I am sorry my letter of the 2nd of June has caused you and Professor Dingle some astonishment, but I have no wish to retract what I wrote, and would be entirely happy for you to send it to the Archbishop of Canterbury if that is what you wish.

My concern was to try to correct evident misunderstandings in your own letter about the kind of enterprise which science is. The fact that it falls short of Sir Henry Dale's ideal is not necessarily a consequence of the wickedness or stupidity of scientists, but results from the fact that the whole enterprise by its very nature is much more muddled and tentative and much less objective than many people believe.

This appraisal of science is now so overwhelmingly documented that I am surprised that Professor Dingle does not see the point. I would simply refer you to T.S. Kuhn's book *The Structure of Scientific Revolutions* to illustrate what I mean.

Let me repeat, this is not a moral matter, but a straightforward series of observations about how science actually works. It is not, therefore, comparable to the parallel you allege from I. Corinthians 13. You impute to me "evident satisfaction" about this situation, but I do not see what the evidence for this is in my letter. I am simply trying to be objective and realistic.

I do not agree, either, with your dismissal of my statement that the simplest questions are often the hardest to answer, as a glib generalisation. If more people appreciated the truth of it the world would be a saner place. It does not mean, as you take it to mean, that no simple questions can receive simple answers, but it does mean that one cannot prescribe in advance the kind of answer which would be counted as satisfactory to what may appear to be a simple question. Yet, if I read him rightly, that is what Professor Dingle is asking for in his letter to *The Economist.*

Mr. Haymon replied to Dr. Habgood on 4 July, 1977, in the following words:

Thank you for your reply which, however, does not decrease our astonishment. I will only say that it was Ziman, not Dingle, who described the question as "a perfectly reasonable question, which science indeed must answer" (which whatever "science" may stand for, certainly means that it must be answered). It was also Ziman who described his own "answer" (which you will have seen quoted in my letter to him of April 20,) as "simple", and it is, in fact, the only comment declaring itself to be an "answer", simple or complex, that has appeared from anyone. If you regard it as satisfactory because of "the kind of enterprise which science is", I can only express my conviction that nature will not do so, and that it is her view that will prevail. I remain simple-minded enough to believe that a genuine answer to "a perfectly reasonable question" of the form "what is the distinction between A and B?" must be expressible in the form "the distinction between A and B is . . .", however complex the continuation may be.

You are surprised that Dingle does not see the point about the nature of science which you ascribe to Kuhn and earlier to Popper. In view of the fact that Kuhn attended Dingle's lectures at UCL before he wrote his book, and of the enclosed correspondence between Popper and Dingle (which is sent in confidence and for return, as he does not wish self-advertisement and consents to my sending it to you now only because he thinks you might be large-minded enough to consider the possibility that you might be mistaken), I venture to think that you are not fully informed in this matter. Perhaps, if you had read Dingle's books, *Through Science to Philosophy* and *The Scientific Adventure* -- both written before he had seen any reason to question the special theory of relativity, which nevertheless he did come to criticise a few years before Popper, a very old friend, wrote -- you might have taken a different view of both the intellectual and moral sides of the matter. When you say that Dale's statement that science, as he describes it, makes "a contribution to the moral framework of civilisation which the world cannot afford to lose" is "not a moral matter", I can only say that I have grossly misunderstood the Anglican Church.

In the text of the above letter, the words "the enclosed correspondence between Popper and Dingle" refer to Popper's letter to Dingle on the occasion of Dingle's seventieth birthday, and Dingle's reply. As was mentioned in Chapter 6, the text of the former letter has been published by Haymon[2].

Although it is not part of the story of the Council for Science and Society, I think it is relevant to include here some correspondence between Dr. Habgood and myself in 1980; I had just published a short correspondence item[3] and I sent him a copy with the following covering letter dated May 5, 1980:

> I am taking the liberty of sending you a copy of a correspondence item published in the April issue of *Canadian Electrical Engineering Journal.*
>
> In your review of *Science at the Crossroads,* published in *Frontier* in February 1973, you wrote that you had little doubt in your own mind that Dingle was wrong and his critics right. I hope that the enclosed item may raise a little more doubt in your mind. At least, I hope it may suggest to you that his critics cannot *all* be right.
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> I have also read with interest some correspondence (sent to me by Professor Dingle) between you and Mr. Mark Haymon, about three years ago, in which you referred to T.S. Kuhn's book *The Structure of Scientific Revolutions* as giving a view of science contrasting with Sir Henry Dale's view. Indeed, the following quotation from the Postscript of Kuhn's book does bring out the contrast: "Most anomalies are resolved by normal means; most proposals for new theories do prove to be wrong. If all members of a community responded to each anomaly as a source of crisis or embraced each new theory advanced by a colleague, science would cease."
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> However, Kuhn went on to say, *in the next sentence:* "If, on the other hand, no one reacted to anomalies or to brand-new theories in high-risk ways, there would be few or no revolutions." I think it is also pertinent to add that one reviewer of Kuhn's book (C.C. Gillispie, in *Science,* 1962) wrote: "I do not think that it is overstating Kuhn's argument to say that he regards the revolutions of which he writes as the only creative episodes in science."
>
> I would respectfully invite you to consider whether the present situation, as I have attempted to assess it in the accompanying letter, is satisfactory, and what action might be taken to make it more nearly satisfactory if it is not.

Dr. Habgood replied on 14th May, 1980. The following is the text of his letter, which he has been kind enough to give me permission to publish:

> Thank you for your letter about Professor Dingle.
>
> I am interested that someone is still taking up the cudgels on his behalf, and I read your published letter with appreciation. I am not myself a physicist, and I do not pretend to be able to evaluate arguments on the subject of Relativity.
>
> As far as I remember, the factors which led me to make the judgment which you quote from my review of Dingle's book in 1973 were:
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> (1) a distinct impression of obsessionalism, which was confirmed by speaking to those who actually knew Professor Dingle:
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> (2) a belief that science and scientific theories are a great deal less tidy than his charges

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2. M. Haymon, "Herbert Dingle, 1890-1978," *Journal of the British Astronomical Association* **89** p. 394 (1979).

3. I. McCausland, "Science on the Defensive," *Canadian Electrical Engineering Journal* **5, No. 2** pp. 3-4 (April 1980).

against the scientific establishment seemed to assume.

I grant you that a man who feels thwarted at every turn may well begin to feel obsessional, and of course I take your point that sometimes a lone campaigner is proved to be right and established ideas are overthrown. In the end, though, an ordinary layman in these matters, like myself, has to rely on a general impression of where the truth is likely to be.

I replied by letter dated May 28, 1980; the following is the text of my letter:

Thank you very much for your letter of 14th May. I would just like to make one or two comments, which I will relate to the numbered items in your letter.

(1) If a person sets out to overthrow a firmly-established theory or belief, he is unlikely to get very far without persistence and determination, and it seems to me that one's assessment of whether a person is obsessed depends to some extent on one's degree of sympathy with the crusade in question. I can understand that some of Dingle's opponents may have disliked some of his methods, but I suggest that scientific truth should be based on the evidence, not on the person who presents the evidence or on the way he presents it. I might add that I corresponded with Dingle for several years, and met him on two occasions. The second occasion was within three weeks of his death; at that time his mind was still alert and his sense of humour was still active.

(2) I would grant that scientific theories are not always "tidy", but it seems to me that obvious untidiness should be attended to. If science is really the pursuit of truth, it seems to me that some attempt should be made to clear up the gross contradiction between two of Dingle's critics, in the correspondence in *The Listener* which relates to references 4-6 of my published letter. Yet the published correspondence concentrated almost entirely on attempting to refute Dingle, and no attempt was made to resolve the contradiction.

It was very kind of you to write, and I hope you will keep in mind my suggestion that this problem still needs some attention by scientists.

Another interesting letter from a member of the Council was the one written by Profesor H.L. (now Sir Hans) Kornberg, FRS, to Mr. Haymon on 4 June 1977; Professor Kornberg has kindly given permission to reproduce his letter, the text of which is as follows:

Thank you for sending me the letter of 31 May signed by Professor Dingle and yourself, which you sent to me as a member of the Council for Science and Society. It may be that, as a Biochemist, I am not sufficiently familiar with the point at issue in Professor Dingle's letter but I must confess that I do not see that the debate between you and Professor Ziman involves any point of moral integrity. I wonder whether this would not become more apparent if, say, the exchange of letters between you were translated into Latin or French or some other language less flexible than English? Perhaps I can very briefly summarise my views as follows:

The central theme of Professor Dingle's letter in the *Economist* is that (1) the faster moving clock ages more slowly than the other and that (2) either may be held the faster moving. I submit that this is a semantic trap. In order to state that one clock travels faster than the other, it must be judged so to move in relation to some frame of reference: otherwise one is left with the Orwellian paradox that all clocks are fast but some are faster than others. If now Professor Dingle's argument (2) holds and the frame of reference is itself moving in the same direction as that of the previously faster clock, so that the other is now the faster, then I presume that with reference to that new point of rest Einstein's special theory would still apply. This clearly is a prediction that can be tested by physicists and I assume that that is what Professor Ziman meant when he wrote that "science must answer this''.

Professor Ziman's statement is thus equivalent to writing, in my own subject, that ''cancer is a serious problem and science must find an answer to it''. By this, I do not mean that there is an answer that is already known to people and that some body of scientists can be persuaded to reveal. Nor is it akin to the problem in your field of law where a matter can be decided by being tested in the Courts and a decision reached which is binding on the future. The history of science is the history of man's attempts to relate his experimental observations to each other and to the world around him and thereby to make a model that enables him to test his theories further, in order to see whether they are wrong or whether they make sense. I thus believe that Professor Ziman was not being evasive or morally irresponsible in not giving you the name of a non-existent super academy of science that can hand out judgements as if they were matters of law. There really is a difference between a scientific answer achieved through experimental inquiry and a scientific *definition* -- such as, for example, the definition of the length of a metre or the boiling point of water.

I hope that you will believe me when I assure you that there is no concrete agency the public is entitled to approach for an answer to the particular question that you pose. And, as long as science remains an attempt to organise knowledge on the basis of empirical inquiry, I hope that no-one will ever attempt to erect such an agency. It is only in Nazi Germany or in the Soviet Union at the time of Lysenko that one saw the consequences of providing ''official'' answers to scientific problems!

Mr. Haymon replied to Professor Kornberg as follows, in a letter dated 22 June, 1977:

Thank you very much for your full and careful reply to our letter. I am much indebted to you for expressing your position so clearly, for it enables me, I hope, to put the moral side of our concern -- the only side, of course, on which I am entitled to speak and on which Professor Dingle and I agree completely -- in terms which will remove the misconception which our letter, to our great regret, evidently allowed you to entertain.

What has become known as ''Dingle's question'' is totally different from the question, ''What is the answer to the cancer problem?'' That is a secret of nature: Dingle's question, on the other hand, is ''What does *the theory* say about . . .'', and that is one which any student might reasonably expect his teacher to be able to answer definitively. The theory might be right or wrong; the answer is exactly the same in either case.

In my field, to which you refer, the difference is between such a question as ''Did X commit murder?'', which can be answered only on the basis of the available evidence and never with *absolute* certainty, and the question ''Does the law allow murder?'' which a judge should be able to answer categorically. If he refused to answer it, there is a concrete authority (viz. Parliament) to which the public would be able to appeal for his dismissal.

Now whatever Professor Ziman meant by calling Dingle's question a reasonable one which science must answer, it is perfectly clear to me, and I think to any intelligent person, that since it is a question akin to ''Does the law allow murder?'', and since also it is one, on the answer or impossibility of answer to which may depend not one man's imprisonment but possibly incalculable disaster to everyone, it is emphatically one which *must* be squarely faced and not left to the arbitrary caprice of anyone to consider or ignore as he pleases.

Professor Dingle assures me that there is no ''semantic trap'' in his question. It means exactly what it appears to mean to any ordinary reader, and Ziman himself called it ''perfectly reasonable'', without any qualification. And whatever the shadowy ''science'', which he says must answer it, may mean, he certainly meant that it must be answered.

> Einstein's own statement (in the generally accepted translation), in his original paper, included, for example, the deduction that "a balance-clock at the equator must go more slowly, by a very small amount, than a precisely similar clock situated at one of the poles under otherwise identical conditions." It is impossible to give this more than one meaning, however you change the frame of reference or observer or anything else. In fact, no one has questioned that the two requirements of the theory stated in Dingle's *Economist* letter are valid as they stand: all informed physicists know, says Dingle, that if they denied this they would be forced not only to contradict Einstein but to eat their own former words: he knows the literature pretty well. The "question", therefore, can be seen by anyone, scientist or not, to arise inevitably.
>
> I hope this makes it clear to you why, although making no claim myself to assess the validity of the theory as an account of what occurs in nature, I can, as an intelligent man, see quite clearly that, since highly dangerous experiments are being based on it, those who conduct them should understand the theory and therefore be able to answer a question about it. I can also see quite clearly that if, as a member of the public, I am denied an opportunity of seeking reassurance concerning the menace with which the activities of "science" appear to threaten the population, through ignorance of the agency to which my inquiry should be addressed, then contrary to your picture of the situation, I am exactly in the position of the Soviet citizen, except that the agency that silences him is called the State, and in the "free" world it is called Science. Dingle and I are not merely not asking for the equivalent of the Soviet organisation; we are protesting against the fact that in this matter it is here already, and we are asking for the assistance of your Council in removing it.
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> May I express the hope that you will use your influence in the Council to the same end? I think that if you contemplate what would be the attitude of the public to the scientific community if a large-scale disaster occurred at an atomic energy establishment, destroying all evidence as to its cause, and the protracted ignoring of Dingle's warning then became common knowledge, as it undoubtedly would, you will realise the importance of treating that warning seriously and honestly now, even on grounds of expediency, let alone more fundamental moral grounds.

The response of the Council for Science and Society itself, in reply to the Dingle/Haymon letter to members of the Council, was transmitted to Mr. Haymon by Professor Ziman in a letter dated 11th October 1977. The letter, which Professor Ziman has also refused to give me permission to publish, stated that the correspondence had been discussed at a meeting of the council in June, that it had been decided to make no response in the name of the Council, though individual members might reply to Dingle and Haymon as they wished, and that Professor Ziman himself did not propose to make any further response to their communications. The letter did not explain why the result of a discussion carried on in June was not transmitted to Professor Dingle and Mr. Haymon until October.

CHAPTER 10

THE STATE AND THE CHURCH

Science today is too much a closed world, and I believe the scientific establishment has an obligation to society to let more light in.
Shirley Williams: *The Times* (London) February 27, 1971.

The purpose of this chapter is to place on record some letters that Professor Dingle and Mr. Haymon wrote to the Minister of State for Science and Education and to the Archbishop of Canterbury, after their correspondence with the Council for Science and Society described in the previous chapter.

In November 1977, Professor Dingle and Mr. Haymon wrote to The Right Honourable Mrs. Shirley V.T.B. Williams, Minister of State for Education and Science. The following is the text of their letter:

> We wish to bring to your notice, as Secretary of State for Education and Science, a feature of public affairs which we believe to be a potential source of great danger. In brief, it is simply this. Research in physical science is now under the unrestricted control of a comparatively extremely few members of the public, whose activities are beyond the understanding of the great majority, and yet are such that their consequences are fraught with the possibility of dire and widespread disaster. This we recognise to be inevitable: the course of civilisation is such that henceforward it must proceed at great risk. Inevitably, therefore, implicit trust must be reposed in the integrity of scientific workers, and if there is good reason to suppose that this falls short of the very high standard required, we believe that the public should have some means of claiming protection from the possible catastrophe with which it is thereby threatened. In fact, however, as things stand at present, it has none.
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> It would naturally be supposed -- we ourselves took it for granted until compelled by inescapable facts to acknowledge our mistake -- that this is a purely hypothetical danger, the organisation of scientific research being such that it was an essential part ot its constitution that breaches of the ethical principles of science, if suspected, would at once be corrected if the suspicion should be found to be justified. That, however, is not so. The Royal Society, for example, as one of us (H.D.) has been informed by four successive Presidents, has no responsibility for the action or inaction of its Fellows in these matters, however evident the danger might be, and it has proved impossible to obtain from the Society any indication of any other body to which appeal can be made in any circumstances at all. Again, it might be thought that, even so, the likelihood that any significant number of reputable scientists should violate the basic ethical principles of their calling, or, if they did, that there would not be a sufficient number of others to counteract their activities, is so small as to be negligible. But that also is erroneous; such an event is not merely likely but inescapably actual.
>
> Space, of course, is not available in a letter fully to substantiate these statements. Evidence that they are true is given in a book by one of us -- *Science at the Crossroads,* by Herbert Dingle (Martin Brian & O'Keeffe, 1972) -- which has never been refuted; the only attempt to challenge them resulted in an apology from the journal and writer of the review in which it appeared (see *Nature,* June 8, 1973). We can, however, relate a very recent occurrence which we think will serve to show that the situation we have depicted, despite its apparent impossibility, is nevertheless actual.

The subject of the concern in question is the special theory of relativity, which, by universal consent, is fundamental to the whole of atomic physics. In *Science at the Crossroads* and elsewhere, evidence has been given that this theory leads to an impossibility and is therefore untenable, and that this fact has been evaded or ignored, with the result that current physical experiments are being conducted on a false basis, which must inevitably imply a high probability of their becoming catastrophic. On February 5 last an anonymous article on the theory appeared in *The Economist,* which gave rise to correspondence in which each of us took part. Copies of our letters are enclosed, and one of them contains, in a compact and we believe generally intelligible form, a comment on the theory, culminating in a question on the possibility of an answer to which clearly depends the validity of the theory. No answer appeared. Accordingly, one of us (M.H.) wrote to Professor J.M. Ziman (who had written in a review in *Nature* of *Science at the Crossroads,* "This is a perfectly reasonable question to which science should indeed give an answer") asking him to what concrete body a member of the public might look for the answer which he had said "science" must give, but which had not been given. A copy of the ensuing correspondence is enclosed. Towards the end, as will be seen, it transpired that Professor Ziman was Chairman of the Council for Science and Society, a voluntarily formed body of persons distinguished in various fields, which hoped, "on the strength of its work, to become recognised as a responsible body in the area where science and social ethics interact." It seemed likely that this body, though with no legal obligation, might serve as a Court of Appeal to which the public might apply in the circumstances we have described. Accordingly, since M.H. had failed to obtain replies from Professor Ziman to his questions addressed to him personally, we sent, on May 31, a joint letter to each member of the Council, and asked Professor Ziman, as its Chairman, to inform us of the reaction to the situation of the Council as a whole; a copy of our letter, which contains a statement of our credentials, is enclosed. We heard nothing from Professor Ziman, despite intermediate inquiries, until he wrote, on October 11, that the Council had decided in June "to make no response", but "individual members might reply to your letters as they wished". We had indeed received six letters from individual members -- out of about 40 -- varying widely in character, which we naturally assumed to have been written with a view to enabling the writers to contribute rightly to the Council's deliberations, whereas it now appears that the Council had already decided to make "no response".

We find it impossible to interpret this as other than a confirmation of the reality of the extremely improbable situation which we described earlier. Here is a question, admitted by the Chairman of the Council (and indeed, no one with even an elementary knowledge of the theory could possibly deny it) to be a perfectly reasonable one which science must indeed answer. It is equally evident that on the possibility of an answer depends the validity of what all agree to be the most fundamental theory of modern physics ("At present special relativity is taken for granted, the whole of atomic physics is merged with it", wrote the late Professor Max Born). The question has, nevertheless, long remained unanswered, and the momentous implications of that fact are presented for comment to a Council which aims to be a responsible body in the area where science and social ethics interact. What could such a Council be expected to say? If an answer has been given and overlooked, nothing would have been easier and more fitting than to give a reference to it. If indeed no answer has been given, nothing would have been more clearly called for than a move to obtain one. Yet the Council did neither of these things, but "decided to make no response".

We, who have no doubt at all that no answer is possible, can also have no doubt that that is the reason for the Council's behaviour. But, whatever the reason may be, the fact itself irresistibly compels the conclusion that there has been a most serious moral lapse in integrity among scientists which requires immediate attention, and on this we are writing to the Archbishop of Canterbury, but the aspect of the matter which we think we have an obligation to submit to you is the protection of the public from the physical danger to which such an attitude on the part of scientists subjects it. It is, of course, impossible, in our ignorance, to be more specific, but that is unnecessary, as the implications of the present position are clear beyond possibility of doubt. Unless the question posed in the *Economist* letter is answerable, atomic experiments are proceeding on the basis of a false theory; they must therefore issue sooner or later in an unexpected result; and, their character being what it is, the probability of that result being disastrous is so high that a decision on the question so crucial for the theory is an imperative duty. That is the situation which we are compelled respectfully to lay before you, as the member of the Government ultimately responsible for the safety of the public in this field. There can be no doubt whatever that, should a disaster occur at an atomic energy establishment, even if, as would probably be the case, all means of discovering its cause were destroyed, the total disregard of the warnings already given in *Science at the Crossroads* and elsewhere of the possibility of such an event would produce a most unwelcome public reaction on those ultimately responsible for public safety. Our object in laying this situation before you is solely that of forestalling such an eventuality.

We should, however, in view of a misunderstanding which one or two letters from members of the Council have revealed, add that nothing is further from our intention than a request for control of research in pure science such as that which has been imposed in totalitarian countries. Our appeal to you is based solely on the desire to safeguard the public from the menace undeniably threatened by the present attitude of scientists to their moral obligations; it is entirely free from the desire to advocate any particular means of achieving that end.

Professor Dingle and Mr. Haymon received a reply dated December 1977, signed by a member of the Science Branch of the Department of Education and Science; the following is the text of the letter:

I am replying to your letter of November 1977 addressed to the Secretary of State.

The primary matter at issue appears to be the way in which scientific questions are resolved and whether somebody should make a reply on behalf of the scientific establishment to the questions which you have raised about the special theory of relativity. Our advice is that this is not the normal method of resolving scientific issues, and that the institution of any "authority" other than the general consensus of informed people would be a retrograde step. We are therefore not persuaded that it would be appropriate to intervene in any way in the normal process of scientific discussion in this case.

As stated above, Professor Dingle and Mr. Haymon also wrote to the Archbishop of Canterbury, Dr. Donald Coggan. The following is the text of their letter, dated 16 November 1977:

We are venturing to lay before you a concern which we feel is of such importance that we are not justified any longer in trusting to the efforts of individuals, many and various though these be, to deal with, but must submit it to the highest quarters. We approach you on the general ground of your leading position in the Christian community in this country, and on the particular ground of your recent address to the nation, which invites us, as individuals who accept wholly your insistence on the primacy of moral considerations in the conduct of human affairs, to write to you for assistance in a situation in which everything we have been able to do has proved impotent. We recognise the need for brevity, and shall leave much unsaid, but, even so, the subject of our concern is so much at variance with what is commonly taken for granted as to be at first sight incredible, and its implications are so momentous that a statement of some length is unavoidable. We shall be as brief as possible, and therefore state at once the essence of the whole matter, trusting to the evidence that will follow to make credible, and we hope convincing, what must at first seem illusory.

The conclusion that has been forced on us is that the scientific world as a whole (there are many exceptions, but they are without influence on the actual prosecution of physical research) has allowed its sense of moral obligation to lapse, and has allowed its commitment to a temporarily successful theory to prevail over its duty to submit all theories to the test of reason and experience. Criticism is not faced and answered; it is evaded, suppressed, ignored, as the case may be, and atomic research, with its immeasurable great potential for disaster if things go wrong, proceeds as though the theory that lies at its foundation -- the special theory of relativity -- were finally established truth. Unless physicists can be awakened to the state into which they have unconsciously lapsed, the outcome is inevitable; it is only a matter of time before it declares itself.

We would emphasise particularly that our concern here is not with the question whether the theory is right or wrong. That is a scientific question of the normal type, which can be profitably discussed only by scientists. What we are concerned with is the fact that an apparently fatal criticism of it is not answered but ignored, and that is a moral failure which violates not only the ethical principles of science, but also the duty of scientists not to subject to unnecessary risks the lives and welfare of the public which is forced to trust in their integrity. The actuality of that moral failure is now, we are convinced, beyond question.

Before proceeding -- necessarily only partially, but we hope adequately -- to the evidence for this, we should give some indication of the nature of our qualification to form a judgment on this matter. One of us (H.D.) writes as a scientist, specially concerned with the theory in question. He has set out in a book *(Science at the Crossroads,* Martin Brian & O'Keeffe, 1972) the story of his efforts, now extending over nearly 20 years, to recall scientists to a sense of their moral obligations. It became necessary in so doing to state his relevant scientific qualifications, which can be seen to be such that they are not exceeded by those of anyone now living: this is not a boast, but a simple statement of fact which it would be wrong in this context to leave unsaid through false modesty. The criticism of the theory here in question originated with him, though many others now accept it, and others again have made independent criticisms of the theory. The other (M.H.) is not a scientist but a lawyer interested in science, who has become convinced of the decline in standards of integrity among scientists through the facts stated in *Science at the Crossroads* and through his own intercourse with scientists and with the scientific press, which have made it impossible for him to doubt that dogma is now firmly enthroned where reason is believed to reign. It should be understood, therefore, that although we write jointly because the essence of our concern resides in its moral significance, the responsibility for any statement we make requiring scientific sanction rests on H.D. alone.

The bulk of the evidence for our view of the situation is contained in *Science at the Crossroads,* and we restrict ourselves here to an account of one more recent example, which indeed epitomises the whole story. An unsigned article on the theory appeared in *The Economist* of February 5 last, which gave rise to correspondence in which each of us took part; a copy of our letters is among the enclosed papers. H.D.'s contains a very brief, but quite rigorous, statement of a requirement of the theory, culminating in a question on the answer to which obviously depends the tenability of the theory. This question had long been vainly asked, and is the central theme of the scientific part of *Science at the Crossroads.* In reviewing that book in *Nature,* Professor Ziman wrote: "This is a perfectly reasonable question to which science should indeed give an answer", but his own "answer" obviously referred to a quite different question, and no other even purporting to be an answer has appeared anywhere. Accordingly, one of us (M.H.) wrote to Professor Ziman for enlightenment, first in his personal capacity as a scientist and reviewer of H.D.'s book, and later as Chairman of the Council for Science and Society, which, as you are probably aware, is a body of men and women, distinguished in various fields, which "hopes, on the strength of its work, to become recognised as a responsible body in the area where science and social ethics interact". A copy of this correspondence is enclosed. It culminated in our sending to each member of the Council a letter, of which we enclose a copy, and we asked Professor Ziman to inform us of the Council's reaction to the situation. To this he made no reply, despite successive requests for one, until October 11, when he wrote briefly (a copy of his letter is included in the enclosed correspondence) to say that the Council had discussed the matter in June, and had "decided to make no response", but "individual members might reply to your letters as they wished".

Why we were not informed until October of a decision reached in June we do not know. We did, in fact, receive letters from six individual members (out of about 40), varying widely in character, to which of course we replied under the impression that the writers wished to obtain a clearer view of the situation in order to enable them to contribute most effectively to the Council's consideration of the matter, whereas it now transpires that the Council had already decided to do nothing. We do not think it necessary to send the correspondence with these individuals (although, of course, it is available if desired), except that with the Bishop of Durham, who was one of the six mentioned, because although, we presume, he does not serve on the Council as an official representative of the Anglican Church, nevertheless his position there is bound to suggest that his views on the moral aspect of the matter are consonant with those of the body of which you are the temporal head.

We do not share Dr. Habgood's opinion that you will have difficulty in distinguishing a moral from a technical issue, but his expression of that opinion makes it necessary for us to indicate the distinction we see in this particular case. The question whether a legitimate criticism of a theory should be answered or evaded is, we believe, a question of morals, and when, on the choice between these treatments may depend the survival and welfare of trusting members of the public, an additional moral factor enters. On the other hand, the question whether a particular answer to the criticism is valid or not we regard as a technical question. We cannot stress too strongly that we are here concerned only with the former -- indeed, the technical issue cannot arise, for there has been no answer. We recognise fully how incredible it must appear that leading scientists should so violate the basic ethical principles of their calling, yet the fact is there -- plain, undeniable -- and its implications are unspeakably ominous. In this last episode -- only one of many -- a Council whose concern is with the interaction of science and social ethics, when presented with a question which is obviously, and which its Chairman has explicitly declared to be, one which science must indeed answer, decides to make "no response". If there has been an answer, nothing would have been easier and more fitting than to give a reference to it; if not, nothing would have been more clearly called for than a move to obtain one. Yet the decision of the Council is "to make no response". Incredible, on any honourable basis, yet manifestly true. The question, it should be noted, is not about the course of nature, on which nothing but tentative answers can ever be given, but about the requirements of a man-made theory, and therefore answerable categorically by anyone who understands the theory. Such is the situation in which we stand, which we respectfully submit for your consideration. Incidentally (though, in the ultimate scale of values, far from incidental), the importance, for all philosophical and theological problems in which the nature of time is involved, of reaching the actual truth about the conceptions of special relativity, whatever it may be, can scarcely be exaggerated, but this consideration is unlikely to influence the majority of scientific workers.

One of us (H.D.) would willingly meet anyone whom you might delegate to look into and report to you on the matter, and to answer any questions within his capacity which such a delegate might wish to put. His own consultations, both by personal conversation and by letter, with individual scientists, have invariably led nowhere, and what they have revealed seems to us to go some way towards reducing the incredibility of the facts. The response of those, among the leading scientists whose work depends on the validity of the theory, who would have been most expected, from personal knowledge as well as reputation, to be alive to the moral aspect, has been uniformly that they do not understand the theory. To the late Dame Kathleen Lonsdale it was "esoteric nonsense". Sir Bernard Lovell has "never been one of those who pretended to understand either the theory of relativity or its implications". The late Lord Blackett, when President of the Royal Society, wrote: "I am no relativist, that is except in the sense that all we practical high energy physicists are, and have not the time nor the ability to discuss fundamentals".

This last remark seems to us to throw much light on the matter. "The sense in which all high energy physicists" accept the theory is, as Blackett quite honestly said, one of blind trust, which prohibits them from considering any criticism of it (Blackett made this remark in refusing to submit such a criticism to the Royal Society for consideration). But in fact it is fantastic to suppose that they have not the ability to understand the theory. The special, unlike the general, relativity theory is far simpler than much traditional physics, and is fully within the comprehension of far less able scientists than these. But their understanding seems to show them that the theory requires what is absurd. This they cannot believe, so complete is the mastery which prejudice has acquired over reason in this matter, so they conclude that they must have misunderstood it. They cannot see where, so they abandon as hopeless the attempt to "correct" their perfectly valid perception, and blindly trust the mathematicians who tell them that, despite appearances, the theory is quite sound.

All approaches to the mathematicians have failed; they cannot see beyond the mathematics, which is simple and impeccable, whereas the impossibility of the theory lies in its physical interpretation of the mathematics. It is as though, because 2 - 5 = -3, one can conclude that it is possible to get five loaves of bread out of two by leaving -3 behind in the cupboard. The arithmetic is unchallengeable, but its application to loaves is invalid, though its application to bank balances is quite in order if the Manager permits overdrafts; you can actually get £5 from your balance of £2.

Long experience shows that this is a precise analogue of the situation in physics today. There are phenomena to which the special relativity mathematics is applicable, and its success there has misled practical physicists into believing that its applicability is universal. But the simple consideration underlying H.D.'s letter in *The Economist* shows that the application to clocks and human beings of equations that hold for purely conceptual entities like muons is no more valid than the application to loaves of an equation that holds for bank balances, unless an answer can be given to the question asked in that letter. That question, instead of being answered is pushed aside: the mathematicians cannot see beyond the mathematics, and the physicists, who can, cannot believe what they plainly see. Only, we believe, through a recall of physicists to their moral obligation to face, and either accept or refute, legitimate criticism can dire disaster, sooner or later, be forestalled; otherwise, *après nous le déluge*.

We are fully conscious of the imperfections of this letter. It is hardly possible, even for much more skilled expositors, to convey, in a brief statement, the reality of a situation so at variance with general belief concerning the strict integrity of scientific workers. Nevertheless, we hope that we have been able to show that there does exist an imperative need for a more positive reaction than the Council for Science and Society has made to the actual facts, a need for the consciences of scientists to be stabbed broad awake to the reality of the state into which they have lapsed, and, with all respect, we submit for your judgment what appears to us to be an outstandingly important example of the basic disease of modern civilisation -- the neglect of the moral factor.

The Archbishop replied in a letter addressed to Professor Dingle, dated 28th November 1977; the following is the text of the letter, which Lord Coggan has kindly given me permission to publish:

> I am afraid I find the letter which you and Mr. Haymon sent me on the 16th November very difficult to deal with. My problem is that you are asking me to make a serious judgment in a field in which I am not competent. While I am, of course, deeply concerned about truth, and while I naturally look for integrity from scientists, I do not know enough about the subject of your letter to make the condemnatory moral judjment which you seem to me to be seeking.
>
> On the other hand, I imagine that those who work at Science, like those who work at Theology, are subject to certain self-correcting mechanisms which ensure that through widespread publication and public criticism errors, inconsistencies, and at times deceptions, are ultimately exposed in their true colours. There is no alternative to this rather slow process, and the appeal to truth and integrity cannot hasten it, when all those involved believe that they are acting with the best of intentions.
>
> I am sorry to have to disappoint you but it would, I feel, be dishonest of me if I tried to go further.

Professor Dingle wrote the following letter to Dr. Coggan on December 19, 1977. (The separation of the letter into two parts by a row of dots is from the original.)

> Thank you for your kind letter. But I most deeply regret a quite false impression which our appeal to you has given. Nothing was further from our thoughts than to ask you to make a "condemnatory moral judgment". We had taken it as axiomatic (for reasons which will appear, this letter is mine alone, but I have no doubt that here I speak for Mr. Haymon also) that the Church existed, not to condemn the world, but that the world through it might be saved, and it was because we have inescapable evidence of its urgent need for salvation -- in this matter both morally and physically -- that we sought the support of the Christian Church to that end, and to that end alone. I am not clear where our expression went wrong, but evidently it did, and I can only apologise and ask your forgiveness.
>
> How far the remainder of your letter is affected by this misunderstanding I do not know, but as it might well be so I venture, with all respect, to make a few comments which I hope will not be out of place. It is true that in science there are "certain self-correcting mechanisms", but (and here I can speak with some confidence since this has been the chief study of my life) those who work at it are not "subject to" them: ultimately they have the power to control them, and unless, by a voluntary act, they put them rightly into operation, the mechanisms either will not work or will work disastrously. The first principle of the whole process is a *moral* one -- that expressed by Sir Henry Dale (and of course by many others on various occasions over a long period, but his statement was the most appropriate here because it was made at a time when he was fighting for, and succeeded in obtaining, the unrestricted freedom of fundamental research which scientists now possess), that scientists shall always be open to examine informed criticism without prejudice, to accept or reject it according to the demands of reason and experience alone, and *then* to put into action the automatic mechanisms as those arbiters require. In particular, this primary obligation requires that no theory shall be held as true that is contrary to reason or experience (it is recognised that, although no theory can be proved finally to be true, it is possible for a theory to be proved finally to be false).

It is that first principle, not the subsequent procedure, that is here being violated, and that error is not self-correcting. It is in a sense self-revealing, but, under modern conditions, only too probably too late for correction, and its innocent victims would be those who have granted scientists the freedom which they now abuse -- that is, the public generally. Nothing can remove that danger but a recall of scientists to their moral commitment.

We did not, therefore, ask you to make a serious judgment in a field in which you are not competent. The nature of the scientific problem concerned is quite immaterial; the preliminary moral obligation takes precedence over *all* scientific activity, and one does not need to know anything at all about the subject to which the automatic mechanism is applied in any particular case, in order to determine whether that obligation has been fulfilled or ignored. We think the evidence, of which we have given a small but, we hope, sufficient part, shows conclusively that here it is being ignored, and we wrote jointly in order to emphasise the fact that I, who was universally recognised as an authority on the theory before I discovered a flaw in it, and Mr. Haymon, a lawyer of distinction who makes no pretension to be an authority on it, are equally competent to judge that evidence. We hoped that your influence would be available to recall scientists to a sense of their unfulfilled moral duty before it is too late.

. . . .

I hope that this shows the nature of our concern more clearly, and will enable you to decide whether it is a matter to which the Christian Church should give its attention or not. But now I venture, in all humility and all earnestness, to seek your guidance in its personal aspect. I have a special responsibility here. The controversy, in its present form at least, originated with me. It is I who condensed it from a conflict of views to a single crucial question, on the answer to which the whole issue depends, and I have therefore an obligation not to remain passively indifferent so long as that question remains unanswered. I am well on in my 88th year, and have experienced some 20 years of frustration. There is nothing I would more gladly do, if I could do so with a clear conscience, than to drop the whole thing, let come what may, and turn my thoughts to more congenial things. I have no personal fears or ambitions connected with it, and if disaster comes it is others on whom the blame, and others again on whom the suffering, will fall. I have already done more than enough to be immune from possibility of censure, and could now await the inevitable outcome, whether I am then alive or dead, without personal apprehension of any kind. But to do that would be to forfeit my self-respect and peace of mind, and these I am not prepared to sacrifice. I must do something, but what I cannot see. What would you advise one who accepts the ethical demands of Christianity, and tries to meet their requirements, to do in these circumstances?

If it were a matter of my holding a different view from the majority on a scientific point, I would be ready, however strongly I felt my view to be right, to recognise my fallibility and leave the issue to time to determine. But it is not that. What I cannot escape is not my own possibly mistaken ideas, but the certainty of *objective facts accepted by all.* Here they are:

> I have asked a question about a theory, which is obviously and has been admitted by *Nature's* antagonistic reviewer of my book to be a perfectly reasonable one which must be answered. It is also obvious that on the answer depends the validity of the most fundamental theory in modern physics, of which the late Max Born said -- and everyone would agree -- that this theory is now "taken for granted, the whole of atomic physics is merged with it". It is undeniable that modern atomic experiments,

> if based on a false theory, must sooner or later have unforeseeable results, with a high probability of these results being immeasurably disastrous. My question is undeniably answerable categorically, for it is not about the course of nature but about the requirements of a human theory of which an authentic record is available: a parallel in Theology would be a question not about God, which might well be unanswerable, but about what Aquinas said of God in a particular context, which could be answered conclusively by reference to his works. The question here is similarly answerable, yet it has not been answered by anyone; nevertheless the theory continues to be applied, both theoretically and practically, as though the question had not been asked. There is a radical difference between shirking a legitimate question and dissenting from a proposed answer to it -- the difference between a moral shortcoming and an intellectual conviction -- and moral integrity in atomic science is now more than ever an imperative.

All this is pure objective fact. Not a single item in it has been, or could be, questioned by anyone, whatever his scientific views, or even if he has none. I cannot shut my eyes to these facts, or give them any interpretation other than that which reason relentlessly demands, however strong the impulse to think that interpretation, like the reports of Hitler's atrocities (though very different of course), too unlikely to be possibly true. The statement contains no reference at all to the technical aspect of the matter, and every sentence means exactly what any normal reader would naturally take it to mean. I could add the conclusion, of which I have not the slightest doubt, that the reason why my question remains unanswered is that the only possible answer proves the theory to be wrong, and physicists have lost the power to believe that -- the cardinal sin in science -- and so remain silent. They look, not *at* the question but *through* it, see the inevitable answer waiting on the other side, find that incredible, and look away. How else can the universal silence of normally honest men be accounted for? But that, indubitable as my experience convinces me that it is, is still a conjecture, and I hold myself open to reject it if a better explanation can be given: what I cannot do, and would not if I could, is to shut my eyes to the above indented passage, which is all universally accepted fact, and an imperative challenge to any Christian who is aware of it.

What, in these circumstances, can the one who first put his hand to the plough and cannot look back -- what can he do? I cannot, like Dr. Habgood, infer that because the question has not been answered, it must therefore be one of those apparently simple but really impossibly difficult ones which we know to exist. It was I who framed the question, and I know that it is not, and that no recognised authority has risked exposing his incompetence by claiming that it is -- the Chairman of his Council has acknowledged as much. My brief letter in *The Economist,* which we sent you, means exactly what everyone would naturally take it to mean, and I cannot foster the delusion that it veils a hidden mystery. To lay that flattering unction to my soul would be, for me, to take refuge in a lie, and that I will not do. But what *can* I do? What has proved impossibly difficult is not to answer the question -- I can do that easily enough -- but just to get it answered, and the necessary implications of the answer accepted, by those in authority. Their resistance is impregnable.

That is my problem: I cannot evade it. I have made personal appeals to the leading physicists concerned, who either say they do not understand the theory they use, or ignore the question and give quite irrelevant reasons why they believe the theory must be true. I have written a book setting out the whole matter: the press has either failed to review it, or the reviewers have evaded the essence of the matter and concentrated on trivial details. The Royal Society denies responsibility for doing anything at all, and will not tell me where the responsibility lies. The Council for Science and Society decides to make "no response". The Government Department of Education and Science replies that it is "not persuaded that it would be appropriate for it to intervene in any way''.

What possibilities remain? There are many persons in several parts of the world -- not cranks, though of course there is no lack of those, but men of standing and integrity in many walks of life, scientific and non-scientific -- Mr. Haymon, a lawyer of distinction, is one of a numerous band -- who, through reading my book, see clearly the terrible potentialities of a situation in which unrestricted power is given to those so blind to the moral obligations which their privileges impose on them. Their own very considerable efforts in their respective countries have met with the same frustration, and they look to me, as the one who opened their eyes to the realities of the situation, not to relax mine. I cannot fail them, for no one is so familiar with the whole course of the controversy or has the knowledge and experience of the whole thing that I have. But what can I do? If you assure me that the Christian Church has no responsibility for using its influence to extract the all-important answer, I must of course accept your judgment, but can it give practical guidance to one whose responsibility *cannot* be shed, and who appeals to it in his extremity? If so, I should be inexpressibly grateful to receive it.

Having received no reply to his letter of December 19, Professor Dingle wrote again to Dr. Coggan on March 20, 1978, as follows:

Forgive me if I seem unduly importunate in sending a copy of the letter which I sent to you on December 19 last. I have had no acknowledgment of its receipt, and therefore am bound to take into account the possibility that it went astray. The subject concerned -- the attitude of the scientific community to the validity or otherwise of its most fundamental theory -- will, of course, become basic material for scientific history when, as must happen, the truth later emerges, so complete records are being kept, by those who may be expected to outlive me, of all its phases, in order that the account which will be written will be unquestionably authentic. It is therefore incumbent on me (in any case, but especially in view of the fact that the Chair which I held was that of History and Philosophy of Science) to determine beyond doubt whether the attitude of the Anglican Church to the moral aspect of that question is truly represented by the absence of a reply to my letter, or whether the letter has not been received. May I therefore respectfully ask for an assurnace on this point?

In reply, Professor Dingle received a letter dated 21st March 1978 from a Lay Assistant to the Archbishop. The letter assured him that his letters of the 19th December and the 20th March had been received by the Archbishop, but stated that the Archbishop had written to him personally on the 28th November and did not feel able to continue the correspondence any further.

CHAPTER 11

THE TWIN PARADOX REVISITED

> Not to publish what ought to belong to the consensus is a crime against Science as such, and can only be justified by the demands of a social system with other ends.
> J.M. Ziman: *Public Knowledge*

This chapter tells the story of Professor Dingle's last major attempt to publish a paper on the relativity debate, a paper which he hoped would clear the matter up once and for all. Although, as is clear from the correspondence quoted here, he also hoped that the present story would be made authentic by reproduction of both sides of the correspondence, I am unable to comply with his wish because I have been refused permission to publish those letters that were written by Dr. Sharrock of the editorial staff of *Nature*.

In *Nature* dated 28 July 1977 there appeared, in the "News and Views" section, an article by Tom Wilkie[1] entitled "The Twin Paradox Revisited". The article, which referred to the accurate determination of the muon lifetime, as reported in another article in the same issue, also pointed out that the resolution of the twin paradox depended on the fact that one twin had to undergo acceleration in order to return to the other twin. Subsequently, Professor Dingle submitted a letter to *Nature,* which was published in the correspondence columns of the 22 September 1977 issue[2]. The text of the letter is as follows:

> Tom Wilkie's remark (28 July, page 295) that the resolution of the twin paradox lies in the fact that the equivalence of the twins is destroyed by the necessity for the 'moving' one to reverse his motion continues to ignore the fact, pointed out in my letter in *Nature* of 31 August, 1973, that, if that were so, the equivalence would be maintained during the first half of the journey, before reversal occurs. Hence, in the example there cited, Paul, the 'moving' twin, would reach the distant planet a teenage boy, and the reversal and return would restore him to babyhood. It is not customary in scientific research to ignore such obviously remarkable and important points. Will Mr. Wilkie therefore kindly say whether he accepts this necessary consequence of his suggested resolution of the paradox, or agrees with me that it makes such a resolution impossible?

Immediately below Professor Dingle's letter there appeared Dr. Wilkie's reply, in the following words:

> I stand by every word of my article and do not accept that the above is a necessary consequence. I am writing to Professor Dingle privately on this matter.

Professor Dingle carried on some private correspondence with Tom Wilkie, and after it had terminated he wrote a new paper which contained his summing up of the controversy on the twin paradox. He attempted to have that paper published in *Nature,* but was unable to have it accepted. The following is the story of his attempt to have it

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1. T. Wilkie, "The Twin Paradox Revisited," *Nature* **268** pp. 295-296 (1977).

2. H. Dingle, "The Twin Paradox," *Nature* **269** p. 284 (22 September 1977).

published in *Nature*.

Professor Dingle sent the manuscript to Dr. David Davies, Editor of *Nature,* on February 8, 1978. His covering letter refers to a correspondence between Dr. Sharrock of *Nature* and Mr. Haymon, in which Dr. Sharrock gave some reasons for believing that *Nature* was right in publishing Professor Dingle's letter but that it would have been wrong to publish a lengthy reply from Dr. Wilkie. The following is the text of Professor Dingle's covering letter:

> I have had considerable correspondence with Dr. Wilkie concerning the question I asked him in *Nature* of Sept. 22, which has now reached a natural termination, and I have invited him to write to *Nature* giving his considered reply to my question, since nothing now remains to prevent his doing so with full understanding of its meaning. He appears unwilling, however, and has not replied to my intimation that, failing such action on his part, it would clearly be necessary for me to point out the implications of this episode. I hope you will agree that it would be impossible for me honourably to allow a vital question, considered worthy of public presentation, to pass into oblivion by default, and I accordingly submit the enclosed article for publication.
>
> But in addition to this relative detail, I hope also that you will agree that a general summing-up of this apparently interminable controversy is long overdue, since nothing now remains to be said on either side, and the ultimate point at issue -- the status of the special relativity theory -- is so fundamental that the continued failure of physicists to agree on what it requires in a particular case has become intolerable. I write quite objectively when I say that I do not think there is anyone now living in such an advantageous position as mine to give such a summing-up. I have had direct experience of the whole controversy from the beginning, and for many years had no doubt of the soundness of the theory until compelled by a number of considerations to change my view. I have known and discussed the question personally with practically all the most distinguished of those who have contributed to the theory in general and the twin paradox in particular (including Einstein himself, of course), and I do not think there is any aspect of the problem with which I am not thoroughly familiar. This I consider lays on me the duty to make the necessary summing-up, and I hope you will agree that the necessity of commenting on the Wilkie incident provides a natural occasion for it. I have been much pleased by the responsible attitude revealed by Mr. Sharrock in his correspondence with Mr. Haymon, and although of course I do not agree with his view of special relativity, I welcome his remark that it would be "a disservice to science and morally unjustifiable to suppress dissenting views" irrespective of the length necessary for their expression, and have no doubt that the same would hold good concerning an informed summing-up of the position in such a confused problem as this. I hope that you will agree that, whatever may be thought of this comment on "the party line", as he aptly calls it, it is necessary that it should be expressed, so that, if "the party" does not accept it, it shall have an opportunity of showing where it fails.
>
> All this I write on wholly objective grounds, but now I would ask a personal favour. If you agree with what I have said, could you deal with the matter expeditiously, notwithstanding that in normal circumstances the pressure of many other subjects of importance might necessitate some delay? I ask this because, being well advanced in my 88th year, my remaining time must necessarily be rather brief, and the various effects of age are, of course, steadily increasing the strain of whatever I undertake. Whatever happens, this cannot be the end of the matter, and I wish to be as fit as possible for whatever the immediate future may demand. If you would take this into consideration I should be most grateful.

Dr. Stuart Sharrock, Physical Sciences Editor of *Nature,* replied to Professor Dingle on 4 April. Unfortunately, when I requested permission to publish this letter and others that followed from Dr. Sharrock, permission was refused by Macmillan Journals Limited who own the copyright in the letters. I am therefore giving a brief summary of the letter, in enough detail to allow the reader to follow the rest of the correspondence.

Dr. Sharrock apologized for taking so long to reply, because of the deep and serious consideration required. He mentioned some criteria that have to be satisfied by manuscripts submitted for publication as letters or articles in *Nature.* A criterion of prime importance is originality of content, and Dr. Sharrock appeared to feel that that criterion was not satisfied in the case of the article in question. Although Professor Dingle's article might have been a candidate for publication as a review article, there were further criteria applicable to review articles on controversial problems. Although the controversy on special relativity was sufficiently serious and fundamental, the only reason for repeating the arguments would be that some advance had been made to clarify the situation, and Sharrock stated that no such advance had been made. He expressed the opinion that repetition of published dissenting arguments is harmful to the dissenting viewpoint, and also stated that a wrong theory could only be proved wrong by experiment.

Dr. Sharrock also mentioned his concern with the problem of suppression; he expressed his pleasure that *Nature* had published certain specified letters of Dingle's in 1973, 1975 and 1977, and stated that he felt that it would not be correct to repeat those arguments.

Professor Dingle replied to Dr. Sharrock on 11 April, in the following words:

> Thank you very much for the care you have taken in commenting on my article on the twin paradox. I am sorry to report, however, that since writing it I have suffered a slight stroke, the chief residual effect of which is an inability to use my eyes for reading or writing. However, a friend, who is a fairly close neighbour (Dr. John Bradley), author of 'Mach's Philosophy of Science', Athlone Press, 1971, has kindly agreed to act in this matter as my secretary.
>
> Two points immediately stand out. First, your letter does not say whether you have reached a decision concerning publication. Some passages could be read as a reason for rejection, and others as a reason for publication and the typescript has not been returned, as is usual with rejected ones. I am going to assume therefore that this is an interim letter, the question of publication being still under consideration.
>
> Second, you nowhere mention the Wilkie aspect of the matter, which started the article off (this strengthens the probability that the question of publication is still under consideration), and since this originated wholly with *Nature,* and its publication seems to violate the reasons you give for rejecting material, I am puzzled about this. You will remember that Wilkie's dismissal of the twin paradox was an unwarranted intrusion into a description of an irrelevant experiment; this intrusion was far from new, added nothing to what had many times been published, yet was selected by *The Times* account [''Science Report'', August 1, 1977] as though it were new and the main subject of the paper. Having thus begun the matter, you were clearly committed to a reasonable comment on it which, of course, you fulfilled by publishing my question. But that meant that you were equally committed to publishing a reply from Wilkie or a protest from me that he had failed to give one. Many have been astonished at his merely saying that he would reply privately, and if nothing else appears, the natural inference would be that he had silenced me, whereas in

fact I think the reverse is the case. I am sure you would not wish such a false impression to be given, but regarding the summing-up (I would revise the title to "The twin paradox of relativity -- a summing-up") do you know of any publication anywhere of a summing-up by anyone, let alone one with my experience of the matter; or of any reference to the uniqueness of the so-called paradox as arising from theory alone, and not from observation; or any treatment of Einstein's own account by anyone else? If so, I should be glad to know of it. If not, I think there is no possibility of regarding the article as a repetition in its most outstanding features.

Regarding the letters of mine which you cite as evidence of freedom from suppression, perhaps you do not know that I was libelled in *Nature,* and when I wrote asking that the statement should be substantiated or withdrawn, Mr. Maddox refused to publish my request and offered instead a letter from Ziman repeating the libel. For this aggravation, I could have claimed heavy damages -- I did not do so on condition that my letter of June 8, 1973, was published, as my aim was not money but the truth. A second libel by Maddox secured the publication of my letter of June 12, 1975, which, as Dr. Davies will know, was published as a result of my conversation with him in the presence of our solicitors. I need not mention the circumstances attending the letters cited but you will be able to judge of the weight of evidence concerning suppression which the publications you cite carry.*

I must tell you the following not as a threat, or in order to influence your actions in any particular direction, but simply because it is a fact which it would be culpable to withhold from you (I should like to say specifically that I write wholly in a friendly spirit with no personal rancour whatever towards anyone, but because of the duty which, independently of my will, has fallen to my lot to do what I can to restore integrity in science). Complete records are being preserved by others to be published, whether I am alive or dead, of the attitude of the scientific world to this question, and if my article is not published in *Nature* it will be published later (next year's centenary of Einstein's birth will be the obvious occasion) together with this correspondence. Arrangements for this are already in hand. I must tell you this so that you shall not write further in ignorance of the use which will certainly be made of your letters. This has to be done so that there shall be no question of *Nature's* attitude being represented by a second-hand description.

It is far from my wish to injure *Nature* in any way. If you will look back 50 years to *Nature* of November 1928, you will see that I was entrusted by Gregory to write the leading article on "The Understanding of Relativity" at the ninth anniversary of the historic meeting at Burlington House. The attitude of *Nature* to my contribution after 50 years' intimate additional experience of the subject will inevitably be interpreted by readers in their estimate of *Nature's* progress or retrogress. I need hardly say that to injure *Nature* in any way would cause me much regret, but the claims of truth, especially in such an important matter, are paramount.

I apologise for all imperfections of this letter, but you will understand the difficulty of replying to a letter over which one cannot cast his eyes to and fro. I hope, however, that I have made the essentials clear and have not made anything either ambiguous or falsely appear provocative. I have one aim only -- to present the truth without disguise, and without resentment or ill-feeling of any kind towards anyone. I know the truth will ultimately prevail, though at what cost, I cannot of course foresee. Whether I am then alive or dead is comparatively trivial.

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* And of course my reply to Synge (see my letter in *Nature* of June 12, 1975), which would have been equivalent to an admission from his lips that special relativity must be abandoned, is still lying suppressed in *Nature* office after ten years.

Having received no reply to the previous letter, Professor Dingle wrote again to Dr. Sharrock on May 15. Mr. Gerald Priestland, who is mentioned in this letter, had discussed ''Dingle's Question'' in April 1978 in one of his broadcasts as Religious Affairs Correspondent of the British Broadcasting Corporation. The following is the text of Professor Dingle's letter:

> Don't think me impatient if I ask for an early decision about publication of the article which I sent on Feb. 8 and on which I wrote, in reply to your comment on it, on April 11. From personal considerations alone I would willingly await whatever time the embarrassing situation you have inherited might seem to demand, but the state of my health does not justify me in doing so, in view of the seriousness, both moral and physical, of the issues involved. (Did you hear Gerald Priestland's recent remarks on the controversy in his weekly broadcast? It has opened many eyes to the realities of the matter). Should you reject the article, which I sincerely trust will not be so, I must now resort to the speediest means, regardless of other factors, of effectively bringing to public knowledge the attitude of the leading scientific journal to its responsibilities. I have no doubt you will agree that there is one overriding factor determining the decision of an honourable journal in such a matter, namely, would publication assist discovery of the truth, whatever it may be? I think the answer is clear. If my statement is valid it resolves this long-standing ''paradox'' once for all; if fallacious, it gives a perfect opening for those who see my error to expose it, and so establish the standard resolution once for all. I am sure you will not find this criticism, let alone an answer to it, elsewhere, so there is no risk of redundancy.
>
> What makes *Nature* at this stage the uniquely responsible medium for publishing this criticism is the obligation it has laid on itself by publishing Wilkie's dogmatic statement that the ''paradox'' had been resolved by the fact which my article treats. That makes it impossible honourably to refuse equal publicity to an informed refutation of that claim, especially since it has allowed him to take refuge in a promised ''private'' reply (which has not been given) to a patently crucial question which *Nature* published. Who, if the matter rests there, could fail to infer that *Nature* is unworthily covering up his inability to reply?
>
> I am sure you wish this sequel no more than I, but unless I hear very soon that the article will appear quickly, I shall have no alternative; I cannot incur the grave responsibility of deserting Truth now. In that case I must ask you to give me your reason for rejection, which I promise to publish verbatim, as I must avoid all possibility of misrepresenting *Nature* or being suspected of doing so. But I trust that, on reflection, you will make all this unnecessary, and that the single-minded desire to reach and make known the truth of the matter will lead you to a decision that will leave *Nature* beyond possibility of reproach.

Dr. Sharrock replied on 14th June. He again apologized for the delay in replying, saying that he had recently returned from an extended visit to the USA. He also apologized for not having returned the manuscript with his earlier letter. He stood by his decision not to publish, saying that it had been reached after much objective consideration.

Dr. Sharrock noted Professor Dingle's comments concerning publication of correspondence, saying that he understood that such correspondence was regarded as confidential, and that he assumed that permission would be sought before publication.

Dr. Sharrock also expressed the hope that Professor Dingle had fully recovered from his recent stroke.

Professor Dingle replied on June 22, as follows:

> Many thanks for your letter, which I am very glad to receive as it came just in time to prevent me from taking steps which I did not wish to take, on the assumption that no reply

to my last letter was now to be expected.

But the fact that stands out conspicuously is your continued avoidance of all reference to the Wilkie incident which started the whole thing; whatever may be said about publication of my article, nothing that you have yet said explains your abrupt closure, for no apparent justifiable reason, of a vitally important matter, started on your own initiative, at a point where it was poised for a definite solution. You cannot, of course, compel Dr. Wilkie to submit for publication his "private" reply to my published question, but to leave the matter as it is without further comment is so obviously at variance with what readers would rightly expect from an honourable journal dedicated to the discovery of the truth in scientific matters, that I cannot think you would willingly allow the only reasonable inference to be drawn from such a practice. May I therefore ask what you now propose to do to prevent it? If you do not wish to publish an editorial comment on this serious failure on Dr. Wilkie's part, what kind of comment would you accept from me, to prevent the false supposition that either Dr. Wilkie has answered my question conclusively, or that you have inexcusably failed to publish his answer? I should have thought that my article was an eminently appropriate sequel, and I am astonished at your statement that you reject it on grounds previously described to me, for on looking again at your letter of April 4, I see that the article passes all the tests there described. (1) As you agree, the subject is beyond doubt important. (2) The content of the article is completely new; I know the literature pretty well, and I am sure that you will not find it elsewhere. (3) Experimental tests of the twin paradox are clearly as yet impossible in any form, so any resolution of it must be based on its rational implications, of which my article gives a new and vital one. (4) As you are "most concerned with the question of suppression", you must surely see that to refuse publication of a new criticism of the currently accepted resolution of the paradox, in which you have found no error, cannot possibly be called anything but suppression of the plainest kind.

However, the decision on that matter rests with you, and the responsibility for it will be yours when, as must happen, the truth unmistakably emerges; my responsibility in that matter ends when I have brought its full significance to your attention, and in returning the article to you now, I am merely giving you the opportunity of looking at it again, to verify that in no respect does it fail to meet the criteria for publication which you give. But the Wilkie affair is another matter. You published Wilkie's statement in *Nature* and in *The Times* in plain violation of your own principle -- as you correctly told Mr. Haymon, so far from its being new, it was, so far as it went, the "party line". To close all reference to the matter at a point where the crucial question remains unanswered would be to expose *Nature* indefensibly to the charge of holding one set of principles for the Establishment and the opposite set for its critics; there can be no two opinions about that. I am therefore bound to ask you to let me know at once what course you regard as proper to bring this matter to a culmination in a manner worthy of a journal with the ideals and traditions of *Nature*.

Your remark concerning confidentiality compels me to say this. The whole matter -- at least on my part and I suspect also on yours -- is entirely impersonal and concerns only objective truth and public welfare. In the service of these I must regard myself as wholly unrestricted by formalities, and I do not imagine that you would say anything affecting them that you would wish to conceal from general knowledge. Indeed, it is just such apparent concealment on Dr. Wilkie's part that lies at the heart of the present concern, and my protest is against just that. I could not possibly, therefore, agree either to write or to receive anything relating to it that I should not be free to publish if general public interest required it. But I do hope that this matter can be settled wholly amicably (I see no reason why it should not) and with the minimum of publicity, and for reasons which I have given I

> am bound to act quickly. I much appreciate your expression of concern for my health. My activities are much restricted, but I am still able to perform essential functions, though with increased difficulty and little prospect of improvement. I am therefore obliged to act with less delay than I should wish.

Dr. Sharrock replied on 3 July; his letter commented on the two points raised by Professor Dingle, namely the decision on publication of the article, and the sequel to what Dingle had called the "Wilkie incident". He stated that he had read the article again and stood by the decision not to publish. Regarding the Wilkie incident, he stated that Dr. Wilkie's comment on the paper published in *Nature* had appeared in the News and Views section, that it was an explanatory article aimed at presenting the consequences of the experiment to a wider audience than would be reached by the original paper, and that it did not make any new statements but simply presented the currently accepted viewpoint of the situation. Professor Dingle had not accepted that viewpoint and had submitted a letter to that effect, which had been published; Dr. Sharrock stated that their responsibility stopped there, and that it was not their responsibility to present the detailed arguments on both sides. He said that any member of the community was able to write to either Professor Dingle or Dr. Wilkie to obtain details of the discussion, and so he did not see that there had been any question of suppression. He commented further on his remarks about confidentiality in his letter of June 14, saying that they were meant to indicate what he understood to be the accepted procedure in such matters. He stated that requesting permission before publication is also the polite way to proceed and therefore he had no doubt that Dingle would follow this procedure.

Professor Dingle replied on July 5. Unfortunately, he seemed to have been confused by the wording of Dr. Sharrock's letter, which gave him the impression that there had been another published item by Dr. Wilkie; the text of his letter is as follows:

> A phrase in your letter of July 3 comes as a complete surprise: you say "Dr. Wilkie's comment on the paper published in *Nature* appeared in our News and Views section." I am not now able to scan the periodicals, but Dr. Wilkie has told me of no such comment, none of those who keep me informed has done so, and you have made no reference to it in reply to my repeated statement to you that I submitted my article because Dr. Wilkie had not himself published anything in reply to my question, which he was obviously under an obligation to do (the article itself states this). I should be glad if you would give me the reference to the comment to which you refer, which clearly has a vital bearing on the matter.

Dr. Sharrock replied on 11 July. He wrote that he was sorry that his letter of July 3 had obviously been a little confusing; he had not been referring to a new publication of Dr. Wilkie's, but to the original article, which was not a published paper in the normal sense but was a commentary on the paper by Bailey *et al* in *Nature* **268** 301 1977.

Professor Dingle's last letter to Dr. Sharrock, dated 15 July 1978, read as follows:

> Thank you for your letter of July 11, from which it appears that the reference to Dr. Wilkie's "comment" was quite beside the point. The whole of my concern with his paper was with the reference to the twin paradox in his article of July 28, 1977, which, though it was gratuitously irrelevant, was selected for the heading of the paper and for the notice in *The Times*.
>
> It is evidently useless to continue this correspondence. It is now perfectly clear that you reject my article, in which you have found nothing wrong and the essential contents of which you have found nowhere else, and which, it is evident to anyone, either settles this

> long-standing controversy or provides a unique opportunity for anyone who can refute it to do so, solely on the dogmatic ground that you "do not consider it appropriate for publication in *Nature*". Also, I have asked you what comment you consider appropriate when one to whom you have addressed a crucial question replies that he will answer it only privately, and will not then publish his answer, and the only possible interpretation of your indirect reply is that you will do nothing and allow nothing, despite the protests you have received from readers. In view of this attitude, it would be a waste of time to write further. When this correspondence is published, readers will have no difficulty in deciding whether the present purpose of *Nature* is to assist attainment of the truth in scientific matters, whatever it may be, or to protect the established view from awkward criticism, whether that view is true or not. I am sorry that you are so short-sighted -- you cannot stop the ultimate emergence of the truth, though you may delay it, with unforeseeable consequences -- but I regret still more that a once honourable journal has sunk so low.

That is essentially the end of Professor Dingle's active involvement in the relativity debate; he died peacefully on 4th September 1978. After his death I submitted his article to the journal *Wireless World,* where it was eventually published in October 1980[3].

3. H. Dingle, "The 'Twins' Paradox of Relativity," *Wireless World* **86, No. 1537** pp. 54-56 (October 1980).

CHAPTER 12

THE QUESTION REMAINS

> The simplistic idea that science marches undeviatingly down an ever broadening highway can scarcely be sustained by the historian of ideas. As in other human affairs, there may be prejudice, rigidity, timid evasion and sometimes inability to reorient oneself rapidly to drastic changes in world view.
> Loren Eiseley: *The Firmament of Time.*

Although Herbert Dingle is no longer with us, the story has not ended because his criticisms have still not been adequately answered by the scientific community. Although there are others who are attempting to persuade the scientific establishment to debate various aspects of the special theory, I shall concentrate most of the remainder of this book on matters that seem to me to arise more or less directly from Professor Dingle's work.

In February 1979, a few months after Professor Dingle's death, *Nature* published an obituary note written by Professor G.J. Whitrow[1], which included a rebuttal of one of Dingle's arguments. It is very interesting and significant that *Nature* published the rebuttal, in spite of the fact that the editors of the journal had over and over again insisted that there was nothing further to say on the subject; readers may judge for themselves the fairness of publishing such a rebuttal when Dingle was unable to answer back. Now let us consider part of the discussion, taken from the obituary, in which Whitrow refers to Dingle's argument about two clocks A and B in the following words:

> His implicit requirement that the epochs assigned to *any* event by A and B, respectively, should always be in the same ratio would imply that by a new choice of time unit for one of these clocks it could be arranged that the times assigned to any given event by A and B would be the same. Dingle's requirement is therefore equivalent to adopting the Newtonian concept of universal time, and this is incompatible with special relativity.

Especially in view of the fact that *Nature* was so reluctant to publish anything because there was nothing new to be said, it should be pointed out that the above passage is an almost verbatim transcript of a passage from Whitrow's earlier review[2] of Dingle's book in the *British Journal for the Philosophy of Science*. Publication of this passage seems to me to be incompatible with the policy stated by the representative of *Nature* in the correspondence mentioned in the previous chapter, that the criterion of prime importance for something to be published in *Nature* is that the content must be new and the ideas or results must not have been published previously.

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1. G.J. Whitrow, "Herbert Dingle," *Nature* **277** pp. 584-585 (1979).

2. G.J. Whitrow, Review of "Science at the Crossroads", *British Journal for the Philosophy of Science* **26** pp. 358-362 (1975).

On March 20, 1979, shortly after the appearance in *Nature* of Whitrow's Obituary, I submitted to the Editor of *Nature* the following note for publication as a correspondence item:

> In his Obituary of Herbert Dingle *(Nature,* Vol. 277, pp. 584-5) G.J. Whitrow gives an argument that purports to refute Dingle's claim that special relativity is untenable because it is inconsistent. Quite apart from the propriety of using a man's obituary to rebut his argument, when he is unable to answer back, the supposed refutation has some unsatisfactory features, such as a reference to Dingle's ''implicit requirement'', later made more explicit by calling it ''Dingle's requirement''. It should be pointed out that Dingle stated, on page 77 of his book *Science at the Crossroads,* that his *only* requirement for regularly-running clocks was that ''one such clock should not be able to run steadily both faster and slower than another''.
>
> The most interesting feature of Whitrow's argument is, however, what he omits to say. Having ended the published part of his argument by saying ''this is incompatible with special relativity'', he appears to consider the next step in his argument so obvious that there is no need to say it: *Therefore Dingle is wrong.* However, the other possible conclusion is: *Therefore special relativity is wrong.* Since it is the validity of special relativity that is in doubt, Whitrow begs the question by ignoring that possible conclusion.
>
> A similar begging of the question occurs in an Editorial *(Nature,* Vol. 239, p.242) in which, after quoting a passage from Dingle's book, the writer goes on to say: ''The trouble, of course, is that in the last of these sentences, Dingle is denying the central principle of relativity.'' The passage from Dingle's book is quoted elliptically (and, incidentally, slightly inaccurately), and obscures the fact that the main point made in the sentence in question is that Einstein himself claimed that, according to the special theory, a clock at the equator would actually run more slowly than an identical clock at one of the poles. What the writer is saying may therefore be paraphrased as follows: *One of Einstein's deductions from the theory contradicts one of the postulates of the theory; hence there can be no contradiction in the theory.* (The last step in the argument is, as in Whitrow's case, apparently considered to be so obvious that it is left for the reader to infer.)
>
> Whitrow also says that ''Dingle believed that his question concerning clock-rates was not answered but evaded by his critics.'' Dingle's question was described in a review *(Nature,* Vol. 241, pp. 143-144) as ''a perfectly reasonable question to which science should indeed give an answer''; since Whitrow does not even say what that question is, and gives neither an answer nor a reference to where the answer may be found, he says nothing that would lead us to doubt what Dingle believed.

The full text of the Editor's reply, dated 28 March 1979, was as follows:

> Thank you for your letter. I am afraid I am not able to fit it into *Nature,* but have read it with interest.

The editorial article in *Nature* mentioned in the above letter to the Editor is discussed more fully in Chapter 5, where the wording of the article is quoted in more detail. The crucial point is, of course, that if Dingle deduces something from the theory that is inconsistent with some other part of the theory, then two possibilities exist: either Dingle is wrong, or the theory itself contains the inconsistency. To state or to imply that the inconsistency *shows* that Dingle is wrong is to beg the question, assuming in advance the very point that is in doubt, and both Whitrow and the writer of the editorial article have done that. In order to show that Dingle is wrong it is necessary to find a fault in his argument.

As mentioned in Chapter 11, Professor Dingle's article summing up the controversy was submitted by me to the journal *Wireless World,* in which it was published in October 1980[3]. At the Editor's suggestion, I wrote a very brief biography of Professor Dingle, which was published with the article. I was also given the opportunity to write a brief note about the controversy, and chose to write a reply to an article by Paul Davies, which had appeared in *New Scientist* in August 1980[4].

Although Dr. Wilkie had chosen not to publish a reply to Dingle's letter in *Nature,* he did publish in *Wireless World* a reply[5] to the article. The reply can be read by anyone who is interested, but what seem to me to be two of its most striking features are Dr. Wilkie's deliberate choice *not* to discuss relativity, and his repeated assertions that too much had already been written on the subject of Dingle and his criticism of special relativity. Consider, for example, the following excerpts from his article:

> Sir, too much has already been published on the Dingle question, and the time is long past to call a halt to this whole business.
>
> Most academic journals have for some years rightly viewed the matter as settled and regarded more discussion of it as a waste of paper.

and his final paragraph:

> Too much has been written on this matter already. Please, let it rest.

It is very odd that Wilkie was so insistent that the subject had already been discussed too much, after he himself had published the article entitled "The Twin Paradox Revisited"[6]. Not only did he write the article in such a way as to make it virtually certain that a rebuttal would be forthcoming (which of course it was), but he also referred explicitly to one of Dingle's publications[7], thereby adding to the literature on what he called "the Dingle question" himself. Then, when a rebuttal occurred, he said that too much had been written on the subject and asked that the matter be allowed to rest. This special pleading is an interesting way of conducting a debate.

It is interesting to read what Wilkie wrote about Einstein's statement about the equatorial and polar clocks:

> Although I do not have German enough to verify this myself, it may be that there is an error or ambiguity in one of the examples Einstein gave in the paper (comparing clocks at the north pole and the equator). Instead of regarding this, if it be true, as an interesting insight into how Einstein himself had not fully thought out the implications of relativity at that time, Professor Dingle chose to regard that paper as a canonical definition of the theory

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3. H. Dingle, "The 'Twins' Paradox of Relativity," *Wireless World* **86, No. 1537** pp. 54-56 (October 1980).
4. P. Davies, "Why Pick on Einstein?," *New Scientist* **87** pp. 463-465 (7 August 1980).
5. T.D.B. Wilkie, "Twins Paradox of Relativity," *Wireless World,* pp. 47-48 (June 1981).
6. T. Wilkie, "The Twin Paradox Revisited," *Nature* **268** pp. 295-296 (1977).
7. H. Dingle, "Integrity in Science," *Nature* **255** pp. 519-520 (also Vol. 256, p. 162) (1975).

> and used it as the spearhead of his attack. As the discoverer of a possible mistake by Einstein, Professor Dingle might have written an illuminating chapter on the history of science; as Einstein's dogged, but mistaken, critic, he has written himself into that history.

This is another addition to the interesting collection of attempts to reply to Dingle's question about the polar and equatorial clocks, some of which are discussed in Chapter 5. Wilkie did not commit himself to saying that Einstein made an error, but only hinted that he *might* have done so. Yet he wrote elsewhere in his reply, referring to his correspondence with Professor Dingle, "I emerged from our correspondence with a much deeper understanding of Special Relativity, and an unshakable conviction that Professor Dingle's criticisms are wholly without foundation." In other words, he was not sure whether Einstein was right, but he had an *unshakable conviction* that Professor Dingle's criticisms were *wholly without foundation.*

I am not convinced that there is any serious problem with the interpretation of the original German, and I am not aware that anyone else has considered it to be a problem. With my admittedly limited knowledge of German, the usual English translation of Einstein's statement about the clocks seems to match the original reasonably faithfully. The English translation of that statement seems to be prefectly clear; is it a valid inference from the special theory, or is it not?

I was glad to be able to publish a reply to Wilkie and other correspondents[8], which may be consulted by the interested reader if desired.

Some time later I published an article entitled "Problems in Special Relativity" in *Wireless World*[9]. Some discussion ensued in *Wireless World,* but unfortunately no published comments came from any of those more or less eminent relativists whom I had cited in the article, even though one of them, as I have described in Chapter 2, wrote to me privately admitting that there was indeed a problem to be cleared up.

Another small publication of mine is also worth mentioning here. In 1983 there appeared in *The Times Literary Supplement* a review[10] by Professor Sir Brian Pippard of Abraham Pais's book *"Subtle is the Lord . . ."*. Professor Pippard chose to include in his review a completely gratuitous attack on anti-relativists, as a result of which I wrote a letter to the Editor, which was published shortly thereafter[11]. The following is the text of the letter:

> It is a little difficult to know why Sir Brian Pippard devotes such a large proportion of his review of Abraham Pais's book *"Subtle is the Lord . . ."* (April 1) to an attack on anti-relativists. He does not refute their case, but sets up his own interpretations of some anti-relativist arguments in order to dismiss them, and also accuses the critics of

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8. I. McCausland, "The Twins Paradox of Relativity," *Wireless World*, pp. 73-74 (July 1981).

9. I. McCausland, "Problems in Special Relativity," *Wireless World* **89, No. 1573** pp. 63-65 (October 1983).

10. B. Pippard, "To the Heart of Matter," *The Times Literary Supplement*, p. 315 (1 April 1983).

11. I. McCausland, "Einstein," *The Times Literary Supplement*, (22 April 1983).

incompetence ("This is the step the objectors cannot take . . .").

It is clear, both from the wording of Sir Brian's argument and its context in the review, that he assumes that the twin paradox (or clock paradox) and the asymmetrical ageing of space travellers can be adequately treated within the domain of special relativity. Yet the book that he is reviewing includes the following statement (p145), referring to Einstein's original prediction of asymmetrical ageing in his paper on special relativity:

> He [Einstein] called this result a theorem and cannot be held responsible for the misnomer *clock paradox,* which is of later vintage. However, as Einstein himself explained some time later, the logic of special relativity does not suffice for the explanation of this phenomenon (which has since so often been observed in the laboratory) since frames other than inertial ones come into play.

If the result in question is a theorem of special relativity, and yet the special theory is insufficient for the explanation of the phenomenon, then these facts themselves show that the special theory is inadequate.

Sir Brian may be correct in saying that any attempt to discard the special theory would cause chaos rather than enlightenment. That is not sufficient reason to refuse to consider that possibility, and I suggest that scientists should take to heart Emerson's statement that we can take our choice between truth and repose, but that we can never have both. The abandoning of special relativity would involve a scientific revolution; like other scientific revolutions, it might cause chaos for a time, but it might also lead to an enormously stimulating period of scientific research. Scientists should not shrink from grasping such an oportunity.

No reply to my letter was published; shortly thereafter I addressed the following letter, dated May 19, 1983, to the Editor:

Sir Brian Pippard's reply to my earlier letter *(Einstein,* April 22) uses the strongest and most frequently used argument of the relativists, an argument which is singularly difficult to rebut: complete silence.

The are two reasons why silence is an effective argument in this case. Firstly, if an obscure engineer criticizes a former Cavendish Professor's views on relativity, without reply, it will be widely assumed by readers that the critic is a crank whose views may be safely ignored. Secondly, the published arguments that have been used to defend special relativity against its critics are so diverse and mutually inconsistent (as I have documented elsewhere) that it is almost impossible for a scientist to make any statement in defence of the theory without contradicting what some other defender has written; in such circumstances silence is the only effective response.

My earlier letter mentioned a contradiction; please permit me, Sir, to state it more explicitly. With reference to the phenomenon of asymmetrical ageing, generally associated with the terms *twin paradox* and *clock paradox,* consider the following pair of propositions:

1. Einstein's special theory is sufficient for the explanation of the phenomenon.

2. Einstein's special theory is not sufficient for the explanation of the phenomenon.

The first proposition represents the position taken by modern textbooks on special relativity and, as I suggested in my earlier letter, apparently supported by Sir Brian Pippard. The second proposition, according to the passage from Pais quoted in my earlier letter, represents the position taken by Einstein.

> Since the two propositions are logically contradictory, one of them must be false. I invite Sir Brian Pippard to state, clearly and explicitly, which one is false. I look forward with interest to his reply.

The Editor declined to publish this letter; that is not surprising, since it is not usual to publish one side only of a debate in such correspondence columns. However, it would be interesting to read what other scientists might reply to the question I asked in the above letter. For example, here is what Professor Whitrow wrote about the relationship of the clock paradox to the special theory[12]:

> It has long been realized by many relativists that Einstein's Special Theory of Relativity is not adequate for a complete discussion of the clock paradox if accelerations are involved. Appeal has therefore been made from time to time to Einstein's General Theory. Unfortunately, this has tended to cloud the issue still further.

If it has long been realized that the special theory is not adequate for a complete discussion of the clock paradox, why is it that contemporary books on special relativity treat the clock paradox in detail?

A similar example of a reviewer who retreated into silence when challenged occurred at about the same time as Pippard's review. The reviewer J.P.S., whom we already met in Chapter 5, in his review of A. Evett's book *Understanding the Space-Time Concepts of Special Relativity,* wrote as follows[13]:

> The first chapter of part two (ch. 8) gives the clearest, most complete discussion I have seen anywhere of the twin paradox. It is treated from no less than five different points of view, all yielding the same result: that the stay-at-home twin ages more, and should forever put aside any further controversy on this question.

I wrote a letter to the Editor, which was published in the July issue[14]; the text of the letter, in which I referred to the clock paradox rather than the twin paradox, considering those two expressions to be synonymous, is as follows:

> I would like to comment on the review by J.P.S. of three books on relativity, published in the March 1983 issue. Referring to Evett's book, the reviewer states that it gives the clearest, most complete discussion of the clock paradox that the reviewer has ever seen, and suggests that it "should forever put aside any further controversy on this question".
>
> Unfortunately Evett's discussion fails to provide the one thing necessary to resolve the paradox, namely a proof that the special theory does *not* lead to the result that the two clocks read the same when they get back together. He does not even *state* that the theory does not give that result, he merely says (p. 69) that "no acceptable way of fitting this conclusion into the rest of relativity theory has been found". Put more bluntly, this says: This result contradicts the result that is generally accepted, so it is not accepted.

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12. G.J. Whitrow, *The Natural Philosophy of Time,* Clarendon Press, Oxford (Second Edition, 1980).

13. J.P.S., "Relativity Theory for Everyman," *Physics in Canada* **39, No 2** pp. 52-53 (March 1983). (Reviews of three books, by Landau and Rumer, Lilley, and Evett.)

14. I. McCausland, *Physics in Canada* **39, No. 4**(July 1983).

> The "clock paradox" is not a paradox; it is a contradiction. The special theory leads to two contradictory results, symmetrical and asymmetrical ageing, because the special theory contains a contradiction. The contradiction cannot be removed by producing five, or five hundred, proofs that the theory predicts asymmetrical ageing. Furthermore, experimental results are of no help in removing the contradiction, because a theory which contains a contradiction can be used to predict any experimental results one wishes.
>
> Evidence of the continuing confusion surrounding the clock paradox can be found in Abraham Pais's recent book "Subtle is the Lord ...", which states (p. 145) that Einstein pointed out that the logic of special relativity is not sufficient to explain the phenomenon of asymmetrical ageing; this is inconsistent with most recent books on special relativity, including Evett's.
>
> In spite of your reviewer, the paradox persists, and will continue to persist until the special theory is examined for inconsistency.

No reply to my letter was published, although it seems reasonable to assume that J.P.S. read it. This is another example of a published boosting of the orthodox viewpoint, followed by silence when challenged.

Part of the problem with the debate on special relativity is that it is very difficult to publish articles that are critical of the theory. In Chapter 19 I document some of the examples of rejections that I have received when trying to publish articles of this kind. In view of this difficulty, I devote the next few chapters to some of the arguments against the theory that I might have published as journal articles if I had been able to do so.

CHAPTER 13

THE LORENTZ TRANSFORMATION AND THE SPECIAL THEORY

The history of theoretical physics is a record of the clothing of mathematical formulae which were right, or very nearly right, with physical interpretations which were often very badly wrong.
J.H. Jeans: *Physics and Philosophy*

There seems to be considerable confusion about the relationship between the Lorentz transformation and the special theory of relativity. Judging from the following remarks made many years ago by Eddington[1], confusion about the relationship between the two appears to have existed for a long time:

> This vagueness and inconsistency of the attitude of most physicists is largely due to a tendency to treat the mathematical development of a theory as the only part which deserves serious attention. But in physics everything depends on the insight with which the ideas are handled before they reach the mathematical stage.
>
> The consequence of this tendency is that a theory is very commonly identified with its leading mathematical formulae. We continually find special relativity theory identified with the Lorentz transformation, general relativity with the transformation to generalised co-ordinates, quantum theory with the wave equation or the commutation relations. It cannot be too strongly urged that neither relativity theory nor quantum theory are summed up in fool-proof formulae for use on all occasions. A relativist is not a man who employs Lorentz-invariant formulae (which were introduced some years before the relativity theory appeared), but one who understands in what circumstances formulae ought to have Lorentz-invariance; nor is he a man who transforms equations into generalised co-ordinates (a practice at least a century old), but one who understands in what circumstances a special system of co-ordinates would be inapplicable.

As Eddington implies, the Lorentz transformation is only a part of the special theory of relativity. This relationship is obvious from Einstein's original paper[2], in which the transformation is derived from certain basic assumptions, and is therefore implicit in the assumptions from which it is derived. However, some writers have claimed that the theory is contained in the transformation, rather than the reverse. For example, Bertrand Russell[3] wrote as follows: "Technically, the whole of the special theory is contained in the Lorentz transformation". More recently, Cullwick[4] has described the relationship as follows:

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1. A.S. Eddington, *The Philosophy of Physical Science,* Cambridge University Press (1939).
2. H. A. Lorentz, A. Einstein, H. Minkowski, and H. Weyl, *The Principle of Relativity,* Methuen (1923).
3. B. Russell, *The Analysis of Matter,* Allen and Unwin, Second Edition (1954).
4. E.G. Cullwick, "Relativity and the Ether," *Electronics and Power* **22** p. 40 (1976).

> After all, the whole of Einstein's theory is wrapped up in the Lorentz transformation, and since the one set of mathematical equations in reciprocal form suffices for both, whatever difference there may be in the underlying physical ideas, it follows that no physical deduction can be obtained from Einstein's theory that cannot also be deduced from that of Lorentz.

In the light of statements such as these, one is entitled to ask, with Essen[5], why the special theory is called Einstein's theory and not Lorentz's theory. It is clear, also, that Born was confused about the relationship between the transformation and the theory. His claim that consistency of the transformation is sufficient for the whole theory to be free of logical contradiction, which we have discussed in Chapter 6, can only be justified if the theory is contained in (or identical to) the transformation, yet Born credited the special theory to Einstein, even though he stated explicitly[6] that the transformation had been derived by Lorentz and, indeed, that it had been derived earlier by Voigt. The acceptance by the scientific community of Born's illogical claim, as shown by the fact that not a single supporter of the theory, as far as I am aware, has published a word of criticism of it, seems to me sufficient evidence that scientists are still confused by the relationship between the transformation and the theory.

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5. L. Essen, "Einstein's Special Theory of Relativity," *Proceedings of the Royal Institution of Great Britain* **45** pp. 141-160 (1972).

6. M. Born, *Einstein's Theory of Relativity,* Dover (1962).

CHAPTER 14

INERTIAL FRAMES

> We know of no rule for finding an inertial system. Given one, however, we can find an infinite number, since all co-ordinate systems moving uniformly, relative to each other, are inertial systems if one of them is.
> Albert Einstein and Leopold Infeld: *The Evolution of Physics*

Some authors have suggested that what they consider to be Professor Dingle's errors lie in his interpretation of the concept of an inertial frame. For example, here is what D.F. Lawden, Professor of Mathematical Physics at the University of Aston in Birmingham, wrote in *The Times* of London on December 24, 1971:

> The fact is that Professor Dingle's failure to appreciate the crucial significance for special relativity theory of the concept of an inertial frame has been paraded before the scientific world for so many years and has wasted so much of other people's time that the silence which now greets his message requires no sinister explanation.

Another scientist who attacked Professor Dingle's interpretation of the significance of inertial frames was Professor Kilmister, who wrote a brief communication on the subject of *Inertial Frames in Relativity* in December 1974[1]. In it he stated that "Dingle's attacks have shown the exposition of special relativity to be inadequate, though it is doubtful whether he has shown any deficiencies in the theory." He then went on to discuss the views of Professor Dingle and others, (including those whom he called Dingle's "antagonists") on inertial frames, and this prompted Professor Dingle to write the following reply:

SPECIAL RELATIVITY AND INERTIAL FRAMES

> In a discussion which I do not enter, Professor Kilmister makes two serious misstatements concerning my attitude to special relativity which it is essential to correct. First, he says: "The key to the whole matter lies in the notion of an inertial frame." True, some of my "antagonists" (those so described are too various to generalise) have intruded that phrase, causing only confusion. Briefly, but adequately, what I say is this. Einstein's special theory is based on two postulates, open to possible disproof by observations not yet made, and a definition, consistent with them, of the "time" by a clock of a distant event (or, equivalently, a definition of synchronization of separated relatively stationary clocks) which was, as he italicised, freely chosen *by definition.* From this follows the Lorentz transformation, which requires, as he proved, that the "time" of an event, according to a uniformly "moving" clock, must be earlier than that according to a "stationary" one which agreed with the other when, earlier, they were together. All (including me) agree that this is logically impeccable. But the theory also assumes (1) that these "times" will agree with the "readings" of the clocks in question when these, or others synchronized with them, are at the events, and therefore that "moving" clocks work at a slower rate than

1. C.W. Kilmister, "Inertial Frames in Relativity," *Nature* **252** pp. 439-440 (1974).

> "stationary" ones; and (2) that either clock may be called the "moving" one. Thus each clock must work faster than the other, which is impossible. Hence my "antagonists" must either state what, consistently with the theory, decides which clock *actually* works the faster, or acknowledge that this impossibility disproves the theory. Overwhelmingly the gravest aspect of this situation is that they all refrain from doing either, thus violating the ethical principles of science; which alternative is right, important though it is, is relatively trivial, as my recent book shows in detail. It will be observed that "inertial frames" have nothing to do with the matter.
>
> Secondly, Kilmister says: "The basis of Dingle's long-standing argument with the relativists seems to be his insistence on the arbitrary nature of inertial frames. Neither his antagonists nor he himself have noticed that this arbitrariness is explicitly denied." Denied by whom or what? Certainly not by Einstein, with whose theory alone the scientific, as distinct from the moral, aspect of my argument is concerned. To take but one of many examples, he wrote in *The Times* (the article is reprinted in *The World as I see it,* p. 170 of English edition): "What has nature to do with our co-ordinate systems and their state of motion? If it is necessary for the purpose of describing nature to make use of a co-ordinate system arbitrarily introduced by us, then the choice of its state of motion ought to be subject to no restriction". It is Einstein who insists on the arbitrary nature of all frames, including inertial ones, and it is his theory of 1905 of which Max Born, at its Jubilee in 1955, truly said: "Now special relativity is taken for granted, the whole of atomic physics is merged with it." That is the essence of the matter: to take *any* theory for granted is the cardinal sin in science, and it is not redeemed by explicitly denying the theory implicitly embraced.

I understand that Professor Dingle submitted the above reply to *Nature* for publication. Unfortunately I have not seen any of the correspondence pertaining to this item, so I am unable to give a reason for its failure to be published.

As evidence of the confusion surrounding the concept of inertial frames, it is perhaps appropriate to refer to Dingle's own paper on the subject in 1962[2], in which he cited "fifteen examples of explicit or implied definitions of inertial systems which have appeared in recent papers", claiming that the ideas put forward in those examples were "all directly at variance with the idea of an inertial system which Einstein regarded as fundamental to his relativity theories." In view of that article, it seems difficult to accept Professor Lawden's view that Professor Dingle failed to appreciate the significance of the concept of an inertial frame. I think it is also fair to suggest that the various statements I have quoted in Chapter 5 show that there are serious inconsistencies among the defenders of the theory in their interpretations of the significance of the concept of an inertial frame.

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2. H. Dingle, "On Inertial Reference Frames," *Science Progress* **50** pp. 568-583 (1962).

CHAPTER 15

THE ROLE OF THE OBSERVER

Laboratory results are triumphantly deduced from a mathematical transformation of the alleged measurements of a non-existent observer.
Alfred O'Rahilly: *Electromagnetic Theory.*

Merely corroborative detail, intended to give artistic verisimilitude to an otherwise bald and unconvincing narrative.
W.S. Gilbert and A. Sullivan, *The Mikado.*

Although observers play prominent roles in most presentations of special relativity, there seems to be a great deal of confusion about what an observer really is or does. For example, as Dingle[1] (pp. 138-140) has pointed out, an observer is often considered to be the same thing as a reference frame. Consider, for example, the following description by Skinner[2]:

> Furthermore, we can imagine that there are spectators at every point of the reference frame who can determine the position *(x,y,z)* and time *t* of each event coincident with its occurrence. The imaginary arrangement we have just outlined is what we mean by the phrase "an inertial reference system." Often, we shall say "(inertial) observer" instead of "inertial reference system" in order that the mental image given above be brought more vividly to mind. However, it must be remembered that such an observer is always present at every event; he is not seated at one point in space watching distant events when their light reaches his eye.

Skinner's description of the observer is repudiated by French[3], who writes:

> The last and most treacherous aspect of introducing an observer attached to a given frame of reference is that one may get the impression that this observer has some kind of bird's-eye view of the whole of his reference frame at a given instant. *This is entirely false.* A single observer is not ubiquitous; at a given instant he has awareness only of events occurring at his own location -- e.g. a burst of photons striking his retina.
> [Italics in the original.]

It is difficult to imagine two interpretations of the same word being more directly opposed to one another. It would appear that we must reject one or other of the above descriptions; alternatively, it would appear that the concept of the observer is unimportant, if each writer is free to write his own description of the properties of the observer.

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1. H. Dingle, *Science at the Crossroads,* Martin Brian & O'Keeffe, London (1972).
2. R. Skinner, *Relativity,* Blaisdell (1969).
3. A.P. French, *Special Relativity,* Norton (1968).

Consider now the following interpretation of the role of the observer, as described by Taylor and Wheeler[4]. After suggesting that recording clocks be placed at various locations in each reference frame, each capable of punching cards denoting the times of various events and the location of the clock, they go on to describe the observer as follows:

> In relativity we often speak about "the observer." Where is this observer? At one place or all over the place? *The word "observer" is a shorthand way of speaking about the whole collection of recording clocks* associated with one inertial frame of reference. No one real observer could easily do what we ask of the "ideal observer" in our analysis of relativity. So it is best to think of the observer as the man who goes around picking up the punched cards turned out by all the recording clocks in his employ.
> [Italics in the original.]

If this interpretation is correct, then it seems obvious that the whole idea of an observer becomes superfluous. It is clear that all the recorded phenomena remain unchanged if the punched cards on which they are recorded remain uncollected, and this shows that those phenomena are all purely objective, independent of any observer. The mention of an "ideal observer", to which no observer really corresponds, suggests the same fact, namely that the phenomena do not depend in any way on the motion of the observer. This independence of the phenomena and the observer is also pointed out by Arzeliès in the following two passages[5]:

> The state of motion of the observer is in itself of no relevance whatsoever. The very notion has no meaning if we consider that, in the final analysis, the observer is a spiritual being, a state of consciousness.

> . . . it must not be supposed that the state of motion of the observer has any influence on the phenomena he measures. These phenomena possess an *objective reality,* exactly in the same sense as in pre-relativistic physics.
> [Italics in the original.]

The above statements by Arzeliès are supported by Bridgman, who writes[6]:

> The function of the observer is to determine the coordinates, in the frame in which he is moving, of some system of events. These coordinates describe the coincidences of the events with the nodes of the framework; the observer observes these coincidences. But a coincidence of an event with a node in the frame S is also a coincidence with a node in the frame S', and since a coincidence is a coincidence, the observer is in a position to observe the S' coincidences as well as the S coincidences. That is, only one observer is necessary, not two. Or going still further, an observer residing in S is not necessary. A single observer, in neither frame, observing from a detached external position, could determine all the coincidences in both S and S' which are needed for the formulation of the results of the theory.

Furthermore, it should be noted that there is no mention of any observer in the

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4. E.F. Taylor and J.A. Wheeler, *Spacetime Physics,* W.H. Freeman & Co. (1966).

5. H. Arzeliès, *Relativistic Kinematics,* Pergamon (1966).

6. P.W. Bridgman, *A Sophisticate's Primer of Relativity,* Wesleyan University Press (1962).

wording of the two postulates from which Einstein derived the special theory[7], and it should therefore be possible to derive all the results contained in the special theory without reference to any observer.

Closely related to the concept of the observer is the question of whether, when the theory states that one clock works more slowly than another, the slower working is real or only an appearance. For example, in attempting to refute Dingle's argument, Stadlen[8] wrote that "What Einstein really said was that each clock would appear to run slow to an observer moving with the other". This statement may be contrasted with the following statement by Davies[9]:

> The apparently paradoxical conclusion that both clocks are running slow relative to each other sometimes causes a certain amount of confusion to the unaccustomed reader. It must not be imagined that the time dilation is an *illusion,* caused by the propagation of light signals or whatever. It is not that each observer merely *sees* the other clock running slow, it actually *is* running slow -- a real physical effect.
> [Italics in the original.]

Although Davies describes the twin-paradox experiment in support of the above assertion, it is clear from the context that the slowing he describes refers also to clocks in uniform relative motion. The quotation from Davies supports Dingle's thesis that, in the case of two clocks in uniform motion, the special theory requires each one to work slower than the other; unfortunately, however, Davies does not discuss Dingle's thesis in his book, in fact he does not even mention it. In a later article[10] he writes that Dingle "enjoyed international notoriety, and acquired a retinue of adherents, largely on the basis of his attacks on the orthodox resolution of the 'twins' paradox'." In accordance with what I have written in Chapter 4, referring to Professor Dingle's statements about the clock paradox, it is clear that Davies represents Dingle's crusade inaccurately in that statement. Furthermore, Davies does not mention in his article that he himself made the statement that I quoted above from his book, which supports Dingle's thesis.

In the published correspondence in *The Listener* that is discussed in Chapter 2, Professor Dingle attempted to clear up the question of whether the slowing down of a moving clock was a real effect or only an appearance. He referred to Einstein's prediction, in his original paper, that if there were two clocks at A and B, originally stationary relative to one another and synchronized with each other, and if the clock at A moved to B, then on its arrival at B its reading would lag behind the reading of the clock that had remained at B. He then went on to ask the following question[11]:

> Professor Taylor and others now say in effect that the theory only requires that each clock appears to an observer with the other to go slow. I therefore ask him to answer, with

7. H. A. Lorentz, A. Einstein, H. Minkowski, and H. Weyl, *The Principle of Relativity,* Methuen (1923).
8. G. Stadlen, "Dingle's Challenge," *The Listener* **88, No. 2270** pp. 411-412 (28 September 1972).
9. P.C.W. Davies, *Space and Time in the Modern Universe,* Cambridge University Press (1977).
10. P. Davies, "Why Pick on Einstein?," *New Scientist* **87** pp. 463-465 (7 August 1980).
11. H. Dingle, "Travelling Clocks," *The Listener* **86** p. 768 (2 December 1971).

a plain yes or no, the following question: does he consider that Einstein meant that when the clocks, originally at A and B, met at B, observers acompanying them throughout the process would disagree on which was giving the earlier reading?

It was perhaps unfortunate that Dingle asked Taylor about what Einstein meant, for this allowed Taylor to say[12] that the answer to the question was a job of a historian of science, and thus avoid giving the plain yes or no that Dingle had requested. He did, in a rather roundabout way, say that all observers would agree that the clock that was originally at A would have gone slow with regard to the other, but attributed the asymmetry to the acceleration to which one clock, but not the other, was subjected; he then ended his letter with some unkind remarks about Professor Dingle, some of which are quoted in Chapter 2. Mr. Bernard Levin considered Professor Taylor's answer to be so obviously evasive that he wrote an article about the controversy in *The Times* of London[13] in December 1971. A brief correspondence followed in *The Times,* one item of which was the letter from Professor D.F. Lawden, which was mentioned in Chapter 14.

Like Taylor, many other writers use the acceleration of one clock to justify the claimed asymmetrical changes in clock readings. I believe that it is pertinent to record Essen's comments on arguments of this type[14]:

> An imaginary experiment can yield only a result which is already present in some form in the data given. Since no data concerning the effects of acceleration are included, the result cannot be a consequence of acceleration.

It is interesting, also, to note Einstein's discussion of the relative motion of a magnet and a conductor, which appears in the first paragraph of his original paper on special relativity[15] and is therefore of fundamental interest in showing the kind of foundation on which the theory is based. Einstein pointed out that the *description* of the phenomena that occurred when the magnet moved was different from the *description* of the phenomena that occurred when the conductor moved, but the *phenomena* themselves were the same whichever was assumed to move; Born has described Einstein's discussion of this point in the following words[16]:

> He says that the usual formulation of the law of induction contains an asymmetry which is artificial and does not correspond to facts. According to observation the current induced depends only on the relative motion of the conducting wire and the magnet while the current theory explains the effect in quite different terms according to whether the wire is at rest and the magnet moving or vice versa.

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12. J. Taylor, "Travelling Clocks," *The Listener* **86** p. 804 (9 December 1971).

13. B. Levin, "A Plain Man's Guide to the Theory that Einstein was Wrong," *The Times*, p. 10 (December 21, 1971).

14. L. Essen, "The Clock Paradox of Relativity," *Nature* **180** pp. 1061-1062 (1957).

15. H. A. Lorentz, A. Einstein, H. Minkowski, and H. Weyl, *The Principle of Relativity,* Methuen (1923).

16. M. Born, "Physics and Relativity," *Jubilee of Relativity Theory, Proceedings*, pp. 244-260 (1956). (Helvetica Physica Acta, Supplementum IV.)

It seems very strange that, largely in order to explain an asymmetry which, in Born's words, "is artificial and does not correspond to facts", Einstein should have developed a theory which introduces an asymmetry which is real; in other words, the theory requires the *actual phenomena* to be different, depending on whether one object (the clock at A) or another object (the clock at B) moves. If the asymmetry in *phenomena* can be justified by saying that one clock has been accelerated and the other not, it would have been just as easy to justify the asymmetry in *description* in the first place by saying that the magnet has been accelerated and the wire not, or vice versa.

It should also be noted that Einstein himself treated two reference frames as being in uniform relative motion even though one had accelerated and the other had not. In his original paper[17] on special relativity he took two systems of co-ordinates in "stationary" space, and *then* supposed that a constant velocity was imparted to one of them; in spite of the fact that one had accelerated and the other had not, he continued to treat them as co-ordinate systems in uniform motion relative to each other for the purpose of deriving the supposedly symmetric results appropriate to the principle of relativity. It seems very strange that this acceleration is later invoked whenever it is necessary to explain the difference between the readings of the clocks A and B.

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17. H. A. Lorentz, A. Einstein, H. Minkowski, and H. Weyl, *The Principle of Relativity,* Methuen (1923).

CHAPTER 16

THE SYNCHRONIZATION OF CLOCKS

> The concept of simultaneity, which is derived from synchronization, is objective and independent of the observer.
> H. Arzeliès: *Relativistic Kinematics*

In his original paper on special relativity[1], Einstein defined a procedure by which it can be determined whether two relatively stationary clocks are synchronized with one another. He considered two clocks, A and B, at rest relative to one another, and considered a flash of light emitted from A and reflected back from B to A. If the reading on B at the moment of reflection is halfway between the readings of A at emission and return of the flash, the clocks are synchronized, acording to Einstein. As Dingle[2] has argued convincingly, this procedure does not depend on the observer; the following is an excerpt from his argument:

> The clocks are synchronised if the reading of the distant clock when it receives the signal is half-way between the readings of the standard clock at emission and return of the signal. It is, however, extremely common to read that, according to special relativity, clocks which are synchronised for one observer are not synchronised for a relatively moving observer.... But it should surely be obvious that the readings of the clocks when they encounter the signal cannot depend in the least on who happens to observe them; their photographs could be examined afterwards by anyone at all, and it is the relation between those readings alone that determines whether the clocks are synchronised or not.

Another way to visualize the operation of synchronization is to imagine the experiment to be done in darkness, a pulse of light being emitted from one clock and reflected back from the other; the two visible readings of the first clock, and the one visible reading of the second, determine whether the clocks are synchronized. Obviously, whatever the observer's position and state of motion, the same set of three readings will be found, and the same answer will be made to the question of whether the clocks are synchronized. It is not necessary for the observer to read his own clock, or even to possess a clock, for the observation of the three relevant time readings.

It is interesting to note that Dingle's critics have almost unanimously ignored his statement about synchronization, and, in typical fashion, those who have referred to it are inconsistent with one another. For example, Stadlen[3] agrees with Dingle that

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1. H. A. Lorentz, A. Einstein, H. Minkowski, and H. Weyl, *The Principle of Relativity,* Methuen (1923).
2. H. Dingle, *Science at the Crossroads,* Martin Brian & O'Keeffe, London (1972).
3. G. Stadlen, ''Dingle's Challenge,'' *The Listener* **88, No. 2270** pp. 411-412 (28 September 1972).

synchronization does not depend on the observer. After a brief paraphrase of the definition of synchronization, Stadlen goes on to write: "Since the reading of a clock when it emits or receives a flash of light is a public event, all observers will agree that the clocks are synchronised." This is inconsistent with an article by Hall[4], who refers to Dingle by writing: "His troubles all stem from his insistence on the postulate that 'if two clocks are synchronized, then they are synchronized absolutely and for all observers.' "

On May 12, 1978, I sent the following letter to the journal in which Hall's article had been published:

> Since Dr. Donald E. Hall, in his article "Intuition, time dilation, and the twin paradox" in the April issue of *The Physics Teacher*, states that Professor Herbert Dingle's criticism of special relativity is deserving of careful refutation, I think that Dr. Hall could perform a very useful service if he would review the argument by which Dingle concludes that "if two clocks are synchronized, then they are synchronized absolutely and for all observers", and identify the precise nature and location of what he believes to be Dingle's error.

The Associate Editor acknowledged my letter, and sent Dr. Hall a copy for his reply. Dr. Hall, after ascertaining from one of my publications[5] that I was one of the "doubters" of special relativity, wrote to the Associate Editor expressing some reluctance to review Dingle's argument, asserting that the issue had already been "beaten to death". Since he was the one who had resuscitated it, this seemed an odd reason for not wishing to review the argument. If anyone really feels that the issue raised by my letter has been beaten to death, I suggest a compilation of a list of references in which the statement of Dingle's that was quoted by Hall has been specifically discussed by other scientists; I believe that such a list would be extremely short.

After he had received Hall's letter, a copy of which I had also received, the Associate Editor wrote to me to say that he did not feel that it would be useful to present the discussion in the journal. Although I exchanged one or two further letters with Dr. Hall, I did not receive any convincing answers to my question. In any case, I am convinced by my experiences of this kind that, in order to resolve the problems discussed in this book, it is necessary for all discussion to be carried on in public, and I am disappointed that the journal that published Dr. Hall's very explicit assertion was unwilling to publish a request for an explicit justification of that assertion.

Let us now consider Einstein's argument, again from his original paper on special relativity, by which he concluded that observers moving relative to a pair of clocks would find that they were not synchronized. The argument involves a rigid rod aligned with the x axis of a stationary reference frame, and moving longitudinally along the x axis; at its ends A and B are two clocks, and along the x axis are several stationary clocks which are synchronized with one another. A flash of light is emitted from A and reflected back from B to A to test for synchronization. Except for the fact that the rod carrying the clocks at A and B is moving rather than fixed, relative to the x axis, the

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4. D.E. Hall, "Intuition, Time Dilation, and the Twin Paradox," *The Physics Teacher* **16** pp. 209-215 (1978).

5. I. McCausland, "On the Special Theory of Relativity," *Proceedings of the Institution of Electrical Engineers* **119, No. 12** pp. 1766-1767 (December 1972).

situation as described up to this point is similar to that described in the definition of synchronization.

The crucial feature of this new situation is that each of the clocks at A and B is constrained to give the same reading as the stationary clock (relative to the *x* axis) that happens to be adjacent to it at any instant. I say ''constrained'' deliberately, because it turns out from well-known results derived later in Einstein's paper that the clocks at A and B, if they were running freely, would *not* continue to give the same readings as the stationary clocks adjacent to them as they moved along, but would fall further and further behind the stationary clocks. To make them continue to show the same readings as their stationary neighbours they would have to be continually readjusted, in which case they would not be regularly-running clocks. To put it more bluntly, they would not be clocks at all, for their clock works could be removed and their readings adjusted by demons to correspond to the readings of the adjacent stationary clocks. Even more simply, the ''clocks'' could be removed altogether and replaced by mirrors which would simply *reflect* the appropriate readings.

In this experiment, the flash of light reflected from B arrives back at A, the end of the rod from which the flash was emitted. Since A has by then moved on, relative to the stationary row of clocks, the clock then opposite A is not the same clock as the one that was opposite A when the flash was emitted from A; the reading on the clock at B when it receives the flash is therefore not halfway between the two clock readings on the ''clock'' at the A end of the rod. Therefore, according to Einstein, ''observers moving with the moving rod would thus find that the two clocks were not synchronous''.

But Einstein is not using his own definition of synchronization in reaching that conclusion. The ''clocks'' at the ends A and B of the rods are not regularly-running clocks, but merely objects reflecting the readings of the stationary clocks beside them. Since the definition requires the reflected flash of light to return to the regularly-running clock from which the original flash was emitted, and since it does not do so until after it has passed the new position of end A of the moving rod, it is not valid to make any inference about synchronization of clocks from the reading of the clock at the new position of A. Einstein's conclusion is therefore unjustified.

CHAPTER 17

EXPERIMENTAL VERIFICATION OF THE SPECIAL THEORY?

> Einstein was looking for crucial experiments whose agreement with his predictions would by no means establish his theory; while a disagreement, as he was the first to stress, would show his theory to be untenable.
> This, I felt, was the true scientific attitude. It was utterly different from the dogmatic attitude which constantly claimed to find ''verifications'' for its favourite theories.
> Karl Popper: *Unended Quest.*

It is widely believed that Einstein's special theory of relativity has been confirmed by experiment. However, little or no attention has been paid to Dingle's claim that the alleged confirmation depends on circular arguments in the interpretations of the relevant experiments.

Before the appearance of the special theory, there appeared to be a conflict between Maxwell's electromagnetic theory and the idea of relativity of uniform motion, or, in other words, between Maxwell's theory and Newton's laws of motion. There were, as Oppenheimer[1] has succinctly described it, three possibilities; the following is a paraphrase of his description of these possibilities:

(1) There is a system in which electric and magnetic fields exist and obey Maxwell's equations; anything that is in motion with reference to that system may have different physical behaviour because of that motion. Accepting this possibility means rejecting the idea of the relativity of uniform motion.

or

(2) Maxwell's electromagnetic theory is wrong.

or

(3) Maxwell's theory is right, and there is relativity of uniform motion, but the Galilean equations do not describe the transformations of relativity of uniform motion.

Oppenheimer went on to mention the Michelson-Morley experiment as a key experiment in determining the right choice among the above three possibilities, part of his reference to the experiment being as follows: ''What Michelson did was to measure the time taken by light to move a moderate distance back and forth in the laboratory and to

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1. J.R. Oppenheimer, *The Flying Trapeze: Three Crises for Physicists,* Oxford University Press (1964).

see whether this was the same when parallel to the earth's motion round the sun and perpendicular to it.''

But, as Dingle has pointed out[2], this interpretation of the experiment involves a presupposition which should not be made. Consider his commentary on the Michelson-Morley experiment, in his Introduction to Bergson's book[3]:

> In this experiment, as it is invariably described, the times taken by beams of light to traverse different paths are compared, and an explanation is given in terms of the modification of these times by the motion of the apparatus. . . .
>
> But in fact no clocks at all are used. The experiment is conducted without reference to a clock or to time, so the effect, if any, of motion on clocks cannot account for the observations. We observe only interference fringes, which keep a constant position throughout. How, then, is time introduced into the description? Simply by interpreting the fringes in terms of the Maxwell-Lorentz theory which supposes that they are caused by light having a constant velocity c, a frequency n, and a wave length λ, which are related by the equation, $c = n\lambda$. c and n involve time, and so time enters the description.
>
> But the moment we recall the purpose of the experiment, we see that this is quite illegitimate. It was designed to decide between Newtonian mechanics and the Maxwell-Lorentz electromagnetic theory; we must therefore not presuppose that either of these is true. But that is exactly what has been done. When the Maxwell-Lorentz theory is presupposed, only two explanations are possible: either Newtonian mechanics is wrong or there has been some disturbing factor that has been overlooked. Einstein chose the first alternative and Lorentz the second. The simple, superficial explanation that Michelson automatically adopted, that electromagnetic theory is wrong, is ruled out by the terms in which the experiment is described; it was therefore ignored by everyone except Ritz, who died almost immediately and could therefore be forgotten.

In other words, Lorentz chose Oppenheimer's possibility (1), and Einstein chose (3); possibility (2) was not given serious consideration. Dingle went on to point out that circular arguments are used in claiming that certain experiments involving elementary particles support the special theory, because the velocities of the particles are inferred from Maxwell's electromagnetic theory, on the assumption that that theory is valid.

There has been considerable discussion on the question of whether Einstein's second postulate -- that the velocity of light is independent of the velocity of its source -- has been verified experimentally. Dingle rules out verifications that depend on the use of Maxwell's electromagnetic theory to infer the velocities of elementary particles, and doubts have been cast by others, such as Fox[4] and Moon and Spencer[5], on the validity of

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2. H. Dingle, ''A Re-examination of the Michelson-Morley Experiment,'' *Vistas in Astronomy* **9** pp. 97-100 (1967).

3. H. Bergson, *Duration and Simultaneity,* Bobbs-Merrill Co. Inc. (1965). (Translated by L. Jacobson, with an Introduction by Herbert Dingle.)

4. J.G. Fox, ''Experimental Evidence for the Second Postulate of Special Relativity,'' *American Journal of Physics* **30** pp. 297-300 (1962).

5. P. Moon and D.E. Spencer, ''Binary Stars and the Velocity of Light,'' *Journal of the Optical Society of America* **43** pp. 635-641 (1953).

deductions made from observations of binary stars. Dingle has also pointed out[6] (page 207) that observations on binary stars, usually taken to verify Einstein's second postulate, are also compatible with a quite different postulate, which he attributed to Ritz, namely that light keeps a constant velocity with respect to its own source. He also makes interesting comments[7] (page 133) on an earlier suggestion by Faraday[8], in which each elementary source of light is assumed to possess a system of "rays", extending indefinitely in all directions, the vibrations on which constitute light; this concept would also be compatible with observations on binary stars. As Dingle suggests, further study of these ideas of Ritz and Faraday might prove profitable.

As Dingle has also pointed out, the experimental evidence that is taken to support Einstein's special theory could just as well be taken to support Lorentz's theory if Einstein's theory had never been conceived. Various other scientists have stated or implied agreement with this point of view; for example, Roxburgh[9] states that the two theories are "observationally indistinguishable".

Some writers have also tried to suggest that Dingle's thesis itself can be refuted by experimental results; for example, in the *Nature* editorial article[10] discussed in Chapter 5, it is implied that the results of the Hafele-Keating experiment refute his thesis. But, as Dingle repeatedly stressed (see, for example, his Letter to *Nature)*[11] his argument is not concerned with the phenomena that actually occur, but with the phenomena that the theory *requires* to occur.

Unfortunately, almost no notice has been taken of Dingle's claim that the experimental evidence that is taken to support the special theory depends on circular arguments because the interpretation of the evidence presupposes that Maxwell's electromagnetic theory is valid. Quite apart from the desirability of seeking the truth for its own sake, the resolution of this matter might have an enormous practical significance, as was pointed out by Essen[12], in the correspondence in *The Economist* discussed in Chapter 8. After stating that the scientific establishment had accept relativity as a faith and refused to consider any criticism of it, and that in consequence rational developments of electromagnetic theory have been hindered, Essen went on to say: "There is some evidence that a new theoretical approach could break the stalemate in the development of nuclear fusion, which appears to offer the only source of energy that could prolong our civilisation far into the future."

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6. H. Dingle, *Science at the Crossroads,* Martin Brian & O'Keeffe, London (1972).
7. H. Dingle, *Science at the Crossroads,* Martin Brian & O'Keeffe, London (1972).
8. M. Faraday, "Thoughts on Ray-vibrations," *Philosophical Magazine* **28 (Series 3)** pp. 345-350 (1846).
9. I. Roxburgh, "Is Special Relativity Right or Wrong?," *New Scientist* **55, No. 813** p. 602 (28 September 1972).
10. "Dingle's Answer", *Nature* **239** p. 242 (September 29 1972).
11. "Dingle's Letter to 'Nature' ", *The New-Church Magazine* **93** pp. 121-123 (1974).
12. L. Essen, "Einstein," *The Economist,* p. 4 (March 19, 1977).

CHAPTER 18

INCONSISTENCIES IN THE SPECIAL THEORY

Still, relativity theory to this day remains controversial and suffers from internal difficulties.
Bertrand Russell: *Wisdom of the West* (1959)

In the present chapter I present some arguments supporting Dingle's claim that there is an internal inconsistency in the special theory of relativity. Specifically, I shall show that, when there are two clocks in uniform relative motion, the theory requires that each works more slowly than the other.

Consider first the predictions that would be made, concerning the relative rates of clocks in uniform relative motion, using each of the two different concepts of the observer described by Skinner[1] and French[2], as discussed in Chapter 15. Consider a clock A fixed at the origin of a reference frame S', moving along a row of clocks that are located along the x axis of a reference frame S and fixed relative to S. The clocks in this row are synchronized with one another, by the procedure defined by Einstein and mentioned in Chapter 16. As is described very clearly in many books on special relativity (such as Skinner's book), it is predicted by special relativity that, as A moves along the row, a sequence of direct comparisons between this clock and the neighbouring member of the synchronized set shows that the reading of A drops further behind the reading of its immediate neighbour as it progresses along the row. Now consider how this situation is interpreted by each kind of observer.

A ''global'' or ''distributed'' observer of the kind described by Skinner, fixed relative to S, would conclude that clock A, moving relative to S, was running slow. On the other hand, a ''local'' or ''point'' observer as described by French would, if travelling with A, conclude that the other clocks (which are moving relative to him) were running fast. (It should be remembered that, as pointed out in Chapter 16, the synchronization of the clocks does not depend on the state of motion of the observer; it can be demonstrated repeatedly to the point observer, if necessary, that the clocks along the row are synchronized with one another.) It is clear, therefore, that we can deduce that a moving clock runs slow relative to a clock carried by the observer, or that a moving clock runs fast relative to a clock carried by the observer, simply by adopting whatever definition of observer we choose. It might be argued that this inconsistency could be removed by adopting one definition of the observer, but there are others which can not be so easily removed.

Another argument showing an inconsistency is based on interpretation of pictures taken by a cine-camera. This argument also depends for its validity on Dingle's claim that synchronization of clocks is independent of the observer, a claim that has not been

1. R. Skinner, *Relativity*, Blaisdell (1969).

2. A.P. French, *Special Relativity*, Norton (1968).

satisfactorily refuted.

Consider once again the clock A situated at the origin of S', moving along a row of synchronized clocks that are stationary relative to S, exactly as described above. Now consider the situation described by McCrea[3], in which a cine-camera anywhere, in any state of motion, could take a series of pictures of the clock A, each picture showing A beside a clock of the synchronized set, a different clock in each picture. Using such a sequence of pictures, together with the fact that the clocks along the row are synchronized, the cameraman could deduce that A was running slower than the clocks in the synchronized set. On the other hand, the same camera, in the same state of motion, could take a sequence of pictures of a clock B fixed at the origin of S, as it moved along a row of synchronized clocks fixed along the x' axis of reference frame S'; using this sequence of pictures the cameraman could show that B was running slower than the clocks in that synchronized set. Hence, if both rows of clocks are present, the cameraman can take a sequence of pictures showing that A runs slower than B, or that B runs slower than A, simply by choosing which sequence of pictures he takes; the choice is quite independent of his state of motion. In other words, it is the sequence of events that the observer chooses to record that determines his results, not the velocity of the observer.

In a similar way, it is possible to collect a certain subset of all the punched cards from the recording clocks described by Taylor and Wheeler[4] (see my Chapter 15) and to show from the data punched on the cards that clock A runs slower than B; using a different subset of the punched cards, it is possible to show that B runs slower than A.

Another argument, by which it can be shown that each clock works more slowly than the other, can be established by considering Einstein's prediction, from his original paper on special relativity[5], that a clock at the equator would work more slowly than an exactly similar clock at one of the poles. First of all, it is clear that the predicted slowing in this case is an actual slowing and not merely an effect of observation; observers attached to both clocks would agree that there is a progressive retardation of the equatorial clock, as compared with the polar clock, as the process continues.

Now, taking one step backwards in the argument by which Einstein[6] deduced that the equatorial clock would work more slowly than the polar one, let us suppose that the "equatorial" clock travels at uniform linear speed along the perimeter of a square having the "polar" clock at its centre. If X denotes the clock at the centre of the square, and Y denotes the clock travelling along the square path, then, by Einstein's argument, Y would work more slowly than X. By symmetry, Y would obviously experience the same amount of lag while travelling along each of the four sides of the square; furthermore, there would be no discontinuities of Y's reading at any corner of the square. Hence Y would

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3. W.H. McCrea, "Why the Special Theory of Relativity is Correct," *Nature* **216** pp. 122-124 (1967).

4. E.F. Taylor and J.A. Wheeler, *Spacetime Physics*, W.H. Freeman & Co. (1966).

5. H. A. Lorentz, A. Einstein, H. Minkowski, and H. Weyl, *The Principle of Relativity*, Methuen (1923).

6. H. A. Lorentz, A. Einstein, H. Minkowski, and H. Weyl, *The Principle of Relativity*, Methuen (1923).

run steadily slower than X while travelling along any one side of the square.

Now consider a third clock, denoted by Z, travelling through space at uniform velocity relative to X, in such a way that for a small portion of its journey it travels beside Y along one side of the square. Since it must keep time with Y as it travels beside it, then Z must also work steadily slower than X, even though its velocity relative to X is perfectly uniform.

In other words, if Y runs slower than X, then Z runs slower than X. Now, using the principle of relativity, it can with equal validity be shown that X runs slower than Z. These results, both of which are deduced from Einstein's original presentation of the theory, are inconsistent with one another.

In other words, if Einstein's prediction is valid, that a clock at the equator would work more slowly than a clock at one of the poles, then each of two clocks in uniform motion must work more slowly than the other.

CHAPTER 19

CONSENSUS OR TRUTH?

What is Truth against an *esprit de corps?*
Benjamin Jowett.
[Quoted in *The Oxford Book of Oxford.]*

It was of astrophysicists that the great Russian theoretical physicist, L. D. Landau, said that they are 'often in error but never in doubt!'
J. C. Polkinghorne: *The Particle Play.*

Every scientist knows, from sad experience, that he and all the other experts are nearly always wrong, because they never know all the relevant facts and are incapable of exact rational thought. But that's the fun of it.
John Ziman: *The Listener,* October 6, 1960.

There seems to be a conviction among some scientists that the purpose of scientific activity is to reach a consensus. This conviction has perhaps been most explicitly expressed by Professor Ziman, for example in his books *Public Knowledge* and *Reliable Knowledge*[1,2], in both of which he states that *the goal of science is a consensus of rational opinion over the widest possible field.* In this chapter I wish to question that statement and similar ideas.

In his book *Public Knowledge*[3], Ziman refers to N.R. Campbell's book *What is Science?* in the following words (p. 30):

> In the only book that I have found where the consensus idea is seriously discussed, Norman Campbell derives the whole conventional apparatus of 'the Scientific Method' from the following definition: 'Science is the study of those judgements concerning which universal agreement can be obtained'. From this sentence, which is obviously very close to the position taken in this book, Campbell arrives very convincingly at a common-sense version of the logico-inductive scheme.

I do not think that Ziman is correct in interpreting Campbell's statement as being very close to his own position. Campbell started by suggesting that science was the study of the external world, and then went on to a careful discussion of the grounds for

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1. J. Ziman, *Public Knowledge: The social dimension of science,* Cambridge University Press (1968).
2. J. Ziman, *Reliable Knowledge: An exploration of the grounds for belief in science,* Cambridge University Press (1978).
3. J. Ziman, *Public Knowledge: The social dimension of science,* Cambridge University Press (1968).

believing in the existence of an external world. As a result of that discussion he concluded that our knowledge of the external world (if there is one) must correspond to other people's knowledge of the external world, and thence he concluded that science should include only those judgments concerning which universal agreement can be obtained.

It seems to me that Campbell's statement referred to universal agreement on the observational or empirical evidence, and was not intended to suggest that the criterion of universal agreement was to apply to the whole superstructure of scientific theories that has been erected on the empirical evidence. I believe that this is borne out by a later statement in his book; after stating that scientists do differ among themselves, sometimes acrimoniously, he goes on to say[4] (p. 30):

> I do not say that all the propositions of science are universally accepted -- nothing is further from my meaning; what I say is that the judgments which science studies and on which its final propositions are based are universally accepted. Difference of opinion enters, not with the subject-matter, but with the conclusions that are based on them.

Campbell has also shown elsewhere that he did not hold the view that a consensus improved the reliability of decision-making; in another book[5] he described the action of a certain scientific group in the following words:

> The only intelligible explanation of their action is that wisdom is not always an A-magnitude [meaning, roughly speaking, a measurable quantity], additive in combination, and that a sufficiently numerous body of sufficiently eminent persons is capable of errors that none of them individually would commit.

There is an enormous body of literature written by philosophers on the subject of knowledge. Although there may not be universal agreement on a definition of knowledge, it is widely agreed, with what one writer[6] has called "a unanimity remarkable for philosophers", that *necessary* conditions for knowledge are *justified true belief.* Although Professor Ziman has written three books with the word "knowledge" in the title[7-9], I am not aware that he has given, in any of those books, any good reason for assuming that a consensus of rational opinion satisfies the necessary conditions for knowledge; for example, it is not clear how a consensus of rational opinion provides

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4. N.R. Campbell, *What is Science?*, Methuen (1921).

5. N.R. Campbell, *An Account of the Principles of Measurement and Calculation,* Longmans, Green (1928).

6. W.W. Rozeboom, "Why I Know So Much More Than You Do," *American Philosophical Quarterly* **4, No. 4** pp. 281-290 (October 1967).

7. J. Ziman, *Public Knowledge: The social dimension of science,* Cambridge University Press (1968).

8. J. Ziman, *The Force of Knowledge: The scientific dimension of society,* Cambridge University Press (1976).

9. J. Ziman, *Reliable Knowledge: An exploration of the grounds for belief in science,* Cambridge University Press (1978).

justification for the beliefs that the members of the consensus hold in common. With specific reference to the relativity debate, I suggest that two scientists cannot *both* be justified in believing that Professor Dingle is wrong if the reasons they give for their belief are incompatible with one another. Similarly, it is difficult to discern how a consensus can be a guide to scientific *truth.* Professor Ziman writes[10] (p. 92): ''Scientists may make mistakes but truth must surely triumph'', which implies that there is something that is truth, and that we have some way of knowing (presumably, other than by consensus) what it is, and when it triumphs. In a similar way, Ashby[11] reassuringly tells us in his review of *Reliable Knowledge* (entitled, interestingly, ''How do you know whether it's true?'') that ''in the end false facts and theories are discarded''. That would be encouraging, were it not for the fact that in the end we are all dead.

One of the dangers of accepting a consensus as an indicator of reliable knowledge is that a group of people may unwittingly and unconsciously delude themselves into reaching and accepting a completely erroneous conclusion; this kind of situation has been studied by Janis[12], who calls this activity ''groupthink''.

According to Janis, groupthink occurs when members of a group become more concerned with retaining the approval of the fellow members of their group than with reaching good solutions to the problems at hand. He attributes various historical fiascoes to this attitude of mind, and lists various symptoms of groupthink, among which are an illusion of invulnerability, an unquestioned belief in the rightness of their cause, characterization of the opponents as too weak or too stupid to matter, a shared illusion of unanimity, self-censorship of deviations from the consensus, and a tendency to protect the group from contrary information. Since the recognition and respect of one's colleagues are among the highest rewards of a scientist, it would not be surprising to find evidence of groupthink in science, and I believe that the attitudes of the relativists show several of the above-mentioned symptoms of groupthink.

Janis made some suggestions for counteracting groupthink. The main ones are the deliberate provision for inputs from outside the group, giving high priority to the expression of objections and doubts, inviting outside experts who are not members of the group to challenge the views of the group, and assigning at least one member to the role of Devil's advocate.

Most scientists agree that scientists make mistakes. But, as has been pointed out by Bauer[13] (p. 144), scientists who are upholding a particular point of view at a particular time are not likely to be willing to admit that they could be mistaken in that particular case; they admit the possibility of making mistakes in general, but not the possibility of

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10. J. Ziman, *Public Knowledge: The social dimension of science,* Cambridge University Press (1968).

11. E. Ashby, ''How Do You Know Whether It's True?,'' *New Scientist* **81, No. 1137** p. 104 (11 January 1979).

12. I.L. Janis, *Groupthink: Psychological studies of policy decisions and fiascoes,* Houghton Mifflin Company, Boston (Second Edition 1983).

13. H.H. Bauer, *Beyond Velikovsky: The history of a public controversy,* University of Illinois Press (1984).

being mistaken *now.*

It is interesting also to consider how flexible the idea of a consensus can be. For example, when Ziman was discussing consensus in general, he wrote as follows[14] (p. 44):

> The '5% significance level' almost implies that 1 in 20 of one's colleagues is entitled to disbelieve one! What we must seek is *overwhelming* evidence that will persuade *everybody* -- one doubt in 10,000 might be too many.

Yet, in discussing Dingle in his review[15], Ziman wrote: "The fact that he, one man in a thousand, thinks differently is scarcely a major flaw in the scientific consensus."

If one is free to choose the acceptable significance level, depending on one's predilections or whatever other criteria one wishes to use, the idea of a consensus as a goal of scientific activity becomes rather unsatisfactory.

In science, theories are not completely determined by the observational evidence, and there may occur situations in which the same phenomena can be explained by different theories; for example, as has already been pointed out in Chapter 5, Roxburgh has suggested that Einstein's theory and Lorentz's are "observationally indistinguishable". That means that the choice of one theory rather than another depends on something other than the physical observations. It seems clear, therefore, that the choice of one theory rather than another is something that is by no means clear-cut and is open to debate, even though all who might participate in such a debate agree on the empirical observations. We must also remember that, even if all agree on the observations, that does not necessarily mean that they agree on the *interpretation* of the observations. Note, for example, the many scientists who have interpreted the Michelson-Morley experiment in terms of time intervals, even though the actual observations in the experiment are not time intervals.

Because theory choice is not completely determined by the empirical observations but is subject to some element of judgment, there appears to be a need for external criticism of science, just as literature, art and music have their critics, who do not need to be able to excel in those arts themselves; possibly this function might be performed by philosophers of science. Although many scientists seem to resent criticism from outside, many of them also assume the right to criticize other fields of knowledge. For example, Ziman assumes the right to criticize other fields from outside; writing from outside the field of philosophy of science, he describes his own book[16] as "amateur philosophy" (p. xi), refers to the Philosophy of Science as being "arid and repulsive" (p. 31), and makes pejorative comments such as his reference to "those self-appointed authorities the Philosophers of Science" (p. 6).

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14. J. Ziman, *Public Knowledge: The social dimension of science,* Cambridge University Press (1968).

15. J. Ziman, "Science in an Eccentric Mirror," *Nature* **241** pp. 143-144 (1973).

16. J. Ziman, *Public Knowledge: The social dimension of science,* Cambridge University Press (1968).

In assessing Ziman's views on science, we need to inquire into what he means by "rational" when he talks about the goal of science being a "consensus of rational opinion". Suppose, for example, that there is a consensus of rational opinion that a certain theory should be accepted. It appears that this could mean either of two things: it could mean that the people forming the consensus are rational people, or it could mean that each person holding that opinion has arrived at that opinion by a rational method. If the former is meant, this suggests that there is some way of assessing the rationality of those people; since it would be completely circular to judge their rationality by their opinion on the theory in question, their rationality must presumably be judged by their opinions on other matters. Such a judgment presents serious difficulties, because it is not possible in general to guarantee that, if a person holds one rational belief, then all his beliefs are rational. If that *were* possible, it would imply that, if two rational people held any one rational belief in common, then they would hold all their rational beliefs in common. Furthermore, if we were to judge the rationality of a person's belief on the theory in question by the rationality of his or her belief on some other matter, that would lead to an infinite regress.

The other alternative is that each person holding the "rational opinion" has arrived at that opinion by the use of reason. This implies that there is some criterion by which that reasoning can be shown to be correctly performed; if that is the case, then the reliability of the knowledge so obtained depends on the correctness of the reasoning rather than on the consensus.

A very interesting example of the use of majority opinion occurs in connection with the twin paradox in James A. Coleman's book *Relativity for the Layman*[17]. In the first Pelican version of his book (1959) he claims that space travellers would not age more slowly than their stay-at-home relatives, but in the Revised Edition (1969) he takes the opposite position. In his "Preface to Revised Edition," after mentioning the "voluminous outpouring of papers" in 1957-59 on the clock paradox, he states:

> I have taken advantage of the clarification of this topic which has resulted and have completely rewritten the section dealing with it to conform to the present views of the majority of those theoretical physicists who are deemed to be most knowledgeable about relativity.

Unfortunately Professor Coleman does not say what criteria he used to deem which theoretical physicists were most knowledgeable about relativity.

Another of the very interesting features of Professor Coleman's book is the way in which various printings of the book carry a quotation from Einstein purporting to endorse the book. For example, on the outside back cover of a 1979 Pelican printing of the Revised Edition there appear the following headings and statement:

a Pelican Book
Second Edition

Of this book Albert Einstein said: 'Gives a really clear idea of the problem,

17. J.A. Coleman, *Relativity for the Layman,* Pelican Books (1959, 1969).

> especially the development of our knowledge concerning the propagation of light and the difficulties which arose from the apparently inevitable introduction of the ether. The relativity of motion judged from a cinematic point of view is vividly demonstrated and also the principle of equivalence.'

The inside front cover of a 1959 Pelican printing of the first edition also carries the same quotation from Einstein. It is interesting that the quotation is printed in support of the first edition, which makes statements about the twin paradox which are completely contrary to Einstein's own position on the subject, and the revised edition, which was first published in 1969, more than a decade after Einstein's death on 18 April 1955. It is also interesting to note that, on enquiring about the source of the quotation, I was informed by Professor Coleman that it is from a letter from Einstein to him, dated February 7, 1955, shortly before Einstein's death.

There is a very interesting paragraph in the earlier version of Coleman's book (p. 71 of the Penguin edition), referring to a journey taken by men in rocket ships:

> This is the paradox: At the end of such a rocket trip will the people on earth be older than the rocket men, or will the rocket men be older than the people on earth? Both views appear to be correct according to the Special Theory. Yet they are contradictory, and both cannot be true.

It is unfortunate that Coleman did not seem to realize that, although the possibilities could not both be true, they certainly could both be "correct according to the Special Theory". If he had applied simple reasoning to his statement he would have realized that, if both views are correct according to the special theory, then the special theory contains an inconsistency. Unfortunately, however, he eventually chose to accept the views of the majority instead of applying reason to his statement.

Part of the problem with the attempt to reach a consensus is the problem of allowing all voices to be heard. As Ziman has put it[18] (p. 116): "The whole ideology of Science, the principle of a *freely accepted* consensus, implies a society in which there is general freedom of speech and comment." [Italics in the original.]

Unfortunately there are two problems in connection with the relativity debate: one is that there is not general freedom of speech and comment; the other is that scientists are unaware that there is not. For example, Professor J.H. Fremlin, writing in *New Scientist* in 1980[19], rejected the idea that opponents of special relativity find it difficult to get a fair hearing. The argument that he gave in support of his rejection of that idea was wholly based on the fact that various critical statements had been published in the past; he presented no evidence to refute claims that various people have made that critical papers have been denied publication. I submitted an answer to *New Scientist* but it was not pub-

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18. J. Ziman, *Public Knowledge: The social dimension of science,* Cambridge University Press (1968).

19. J.H. Fremlin, "Special Theory," *New Scientist*, (25 September 1980, p.950).

lished, so I published the following answer in *Wireless World:*[20]

> Professor Fremlin stated that he would "like very much to refute the suggestion that opponents of the theory of relativity find it difficult to get a proper hearing". He might *like* to refute the suggestion, but his letter certainly does not do so. The only evidence he presents in support of his "refutation" is about things that were published, whereas the suggestion that he claims to refute is related to the fact that papers have been denied publication. There is no contradiction between the fact that some papers have been published and the fact that others have been denied publication. Unfortunately few people, except those who have direct experience, are aware of the difficulty of having *any* paper published if it is critical of relativity. Part of the problem is that almost all the evidence about papers that have been rejected is hidden from public view.

If a scientist has had a critical paper rejected by a journal, it is unlikely that the same journal or another one will be willing to publish a statement that the paper was rejected. For example, Professor Dingle has described in his book[21] how he submitted a paper to the Royal Society, only to have it rejected because it supposedly contained an elementary fallacy. When the Royal Society would not divulge to him the nature of the alleged fallacy, he attempted to publish in *Nature* a request for the Royal Society to state the fallacy, but the Editor of *Nature* told him that he would not publish the request, saying that "what other journals do is not usually our business.".

Because scientific journals do not usually tell their readers about the way in which critical papers on relativity are arbitrarily rejected, most scientists are unaware that this happens; as an anonymous writer[22] in *The Economist* put it, referring to this kind of phenomenon, "successful censorship leaves no evidence". However, in spite of the journals, Professor Dingle was eventually able to describe in books some of the ways in which his critical articles were rejected by journals, and anyone who doubts that opponents of special relativity have found it difficult to get a proper hearing should read his writings, especially his book *Science at the Crossroads*[23]. In case readers may think that Dingle's descriptions of rejections of critical papers are isolated examples which may be dismissed, I will now present a few examples from my own experience.

In January 1972 I sent a Letter entitled "On the Special Theory of Relativity" to Dr. D. ter Haar, for possible publication in *Physics Letters A*. The Letter claimed to obtain two mutually contradictory results by applying the special theory of relativity in a "travelling twins" situation in which one astronaut travelled away from another and later turned around and returned. Dr. ter Haar replied immediately, rejecting my Letter in the following words:

> I am afraid that I am unable to accept your paper. The reason is that the clock paradox is *not* a problem in special but in general relativity (for a very good discussion see C. Møller's Relativity) because the motions considered involve *accelerations* (see your p. 3 3rd line after equation (6): he "reverses his velocity").

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20. I. McCausland, "The Twins Paradox of Relativity," *Wireless World*, pp. 73-74 (July 1981).

21. H. Dingle, *Science at the Crossroads*, Martin Brian & O'Keeffe, London (1972).

22. "Banning the Book", *The Economist* **292, No. 7356**(August 25, 1984).

23. H. Dingle, *Science at the Crossroads*, Martin Brian & O'Keeffe, London (1972).

> I am therefore returning your ms. to you.

Now, it is true that some people hold the view expressed in ter Haar's letter. However, if one consults almost any contemporary book on the special theory, one finds the clock paradox treated in the context of the special theory. At best, ter Haar's reason for rejection illustrates the confusion that exists on this subject, which is also mentioned in Chapter 12.

The other examples that I present here are North American, partly to balance the fact that most of those presented by Professor Dingle were British. In October 1973 I sent a paper entitled "Einstein's Special Theory of Relativity" to Dr. P.R. Wallace, Editor of the *Canadian Journal Of Physics*. I received an acknowledgment dated October 12, 1973, signed by a secretary, which said in part: "As soon as I have heard from the referee, I shall let you know." Shortly thereafter I received the paper back with a covering letter dated October 16, 1973, signed by Dr. Wallace, which read as follows:

> We do not believe that the enclosed paper is of such a character as to be suitable for publication in the Canadian Journal of Physics. We suggest that you consider as more appropriate a journal of the character of "American Journal of Physics" or "Nature".

No mention was made of any referee.

On August 4 1976 I sent an article to the editor of *Science* with the following covering letter:

> I enclose an original and two copies of an article entitled "The Dingle Affair: An unresolved scientific controversy"; will you please consider this for possible publication in *Science*.
>
> This article is a factual account of some of the treatment that Professor Dingle's criticism of special relativity has received at the hands of the scientific community, and I believe it would be of interest to your readers.

I received the manuscript back, accompanied by the following letter from the Editor, Dr. P.H. Abelson:

> Thank you for letting us see your manuscript entitled "The Dingle Affair." Unfortunately, we now have a substantial backlog of accepted articles, and we are obligated to give them first priority for publication. Hence, we cannot handle your article at this time.
>
> Accordingly, I am returning your manuscript.

In assessing the cogency of Dr. Abelson's reason for refusing to publish my article, it may be noted that the returned manuscript was date-stamped Aug. 6 1976, presumably the date on which it was received by the journal, while his letter of rejection was dated 9 September. It seems odd that it should take a weekly journal more than a month to realize that it had a "substantial backlog of accepted articles". Obviously any article could be rejected by such a formula; the reason given is obviously unrelated to the merits of the article or the truth of its factual statements.

In May 1981 I sent a copy of a paper entitled "Some Problems in Special Relativity" to The Franklin Institute for consideration for publication in the Journal. I was encouraged to hope that my paper would receive favourable consideration by the fact that the following statement appeared in issues of the Journal showing its policy:

Papers will be considered in all traditional branches of mathematics and the physical sciences, pure and applied, as well as in interdisciplinary fields or composite sciences that combine the philosophies of two or more disciplines. No technical field to which important contributions are being made is excluded -- whether experimental or theoretical, mathematical or descriptive.

This appearance of receptivity was enhanced by a further statement that "Articles of general interest will be given special attention and consideration."

The following is the text of the letter that I received in reply, dated June 5, 1981, signed by the Managing Editor, Mrs. L.H. Falgie:

I am returning your manuscript entitled "Some Problems in Special Relativity" because it unfortunately is not suited as a contribution to the Journal of the Franklin Institute. It is more appropriate as a contribution to such publications as *Nature, Science, Physics Today, New Scientist,* etc., rather than this journal.

However, thank you for sending it to us for consideration.

The above examples are just a selection from my files, and I think that they are sufficient to show that criticisms of special relativity are not treated very seriously by scientific journals. I am glad to be able to report that the article that was rejected by the *Journal of the Franklin Institute* was eventually published under the title "Problems in Special Relativity" in 1983[24], and is mentioned in Chapter 12.

Another problem in reaching a rational consensus is that the arguments used in discussing controversial subjects are not always rational. We repeatedly find scientists using arguments having nothing whatever to do with the scientific merits of the case; a prime example is Arzeliès[25], who states that the difference between the relativists and the anti-relativists is simply that they have different mental structures, and that it is a waste of time to discuss the subject using arguments based on physics or mathematics. In the following excerpt from his pronouncement on this subject, the two groups of physicists mentioned are the relativists and the anti-relativists:

Quelle conclusion adopter? Mais tout simplement que les deux groupes possèdent vraisemblablement des structures mentales quelque peu différentes; ce qui est évident, très clair, pour les uns, est obscur et absurde pour les autres.

La discussion sort du domaine de la physique; elle relève de la psychologie expérimentale ou de la psychiatrie. Je dis cela très sérieusement, sans aucune ironie. . . .

De toute façon, continuer à discuter entre physiciens, avec des arguments de physique ou de mathématique, est une perte de temps. . . .

[Italics in the original.]

Another author who hints that anti-relativists have psychiatric problems is Marder, who uses the following quotation as an motto for his chapter entitled "The Doubters" in

24. I. McCausland, "Problems in Special Relativity," *Wireless World* **89, No. 1573** pp. 63-65 (October 1983).

25. H. Arzeliès, *Relativité Généralisée: Gravitation,* Gauthier-Villars (1961).

his book *Time and the Space-Traveller*[26]:

> There are no doubt minds which have not this predisposition to regard as substantial the things which are permanent; but we shut them up in lunatic asylums.

One of the interesting things about that quotation is that Marder attributes it to Eddington, specifically his book *The Mathematical Theory of Relativity*. Having been unable to find the sentence in question in that book, I wrote to Dr. Marder to ask for the edition and page number, but unfortunately he had made no record of those facts and was unable to supply the information. ~~I am therefore prepared to offer a small prize (a free copy of this book) to the first person who can provide the exact information required to identify the quotation precisely.~~ Note added May 2008: It is on pages 120-1.

In any case, Eddington expressed elsewhere an opinion quite contrary to the view expressed in the sentence in question. In his 1929 Swarthmore Lecture[27] Eddington wrote:

> It would be a shock to come across a university where it was the practice of the students to recite adherence to Newton's laws of motion, to Maxwell's equations and to the electromagnetic theory of light. We should not deplore it the less if our own pet theory happened to be included, or if the list were brought up to date every few years.

The study of how scientists arrive at scientific knowledge usually involves some considerations of philosophy. If scientists arive at their knowledge by consensus, however, we must also consider the sociological problems involved in the propagation of knowledge; in other words, we must study the sociology of science. As R.G.A. Dolby, a sociologist of science, has pointed out[28] (p. 14):

> . . . the dominance of each science by a relatively small number of the most expert tends to inhibit other scientists from expressing contrary views if they accept that they are less expert or would not be regarded by fellow scientists as experts on the issue in question. It may be argued, therefore, that science is very far from being democratic, and widespread apparent agreement is due to submission to the shared views of a small proportion of elite scientists. In a social system in which dissent is discouraged and in which the expert is always deferred to, the views of the elite naturally have a disproportionate influence.
>
> The discussion so far has suggested that the appearance of consensus in science is not due to the universal agreement of scientists. Rather, commentators on science have tended to pay more attention to signs of agreement than to signs of disagreement, and scientists themselves usually try to minimize visible controversy. The actual consensus is not produced by universal use of completely effective methods or rational assessment of scientific ideas, but rather by the general tendency to defer judgement about most of

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26. L. Marder, *Time and the Space-Traveller*, Allen & Unwin (1971).

27. A.S. Eddington, *Science and the Unseen World*, Allen & Unwin (1929). (The 1929 Swarthmore Lecture.)

28. R.G.A. Dolby, "Reflections on Deviant Science," *On the Margins of Science: The social construction of rejected knowledge*, pp. 9-47, University of Keele, (1979). (Sociological Review Monograph 27, Edited by R. Wallis.)

science to a relatively small proportion of elite scientists who interact fully with one another. This is the nature of the scientific orthodoxy. Each orthodox scientist builds upon the apparent consensus and his original contributions offer arguments which extend it further.

The danger of consensus as an indicator of reliable knowledge has been shown by Professor R.A. Lyttleton[29,30], who describes an effect known as the Gold Effect, after its discoverer. What happens is that an idea which has initially a moderate number of believers can, as a result of people studying the idea and holding meetings to discuss it, acquire more and more believers until the idea is solidly entrenched, even if the idea is devoid of scientific merit. This is especially likely to happen if there is a tendency for those who are interested in the idea to accept the opinions of others instead of thinking critically about the idea for themselves. I am not aware of any description of the Gold Effect published by Gold himself, but the following brief summary from the second of the above-mentioned papers by Professor Lyttleton gives the general idea:

> In essence [the Gold Effect] is that even if only random moves and selections are made in regard to an idea initially supported by a slight majority, then through lack of rigorous scientific thinking and allowing other considerations to be allowed to have influence, there will develop an increasing concentration of people believing that the idea is correct, when, in fact, it may be no more than an arithmetically brought-about illusion of truth, a simulacrum without scientific basis, a kind of lying truth.

The ideas of relativity have taken an enormous hold over the minds of both scientists and non-scientists, over a very long period of time. As early as 1928, Professor Dingle wrote a leading article in *Nature* (to which he referred in his 11 April 1978 letter to Dr. Sharrock [see Chapter 11]) on the occasion of the ninth anniversary of the meeting at the Royal Society in Burlington House, at which the British eclipse expedition announced the confirmation of Einstein's prediction from the general theory of relativity that starlight would be deflected by the gravitational field of the sun. In that article[31] he wrote as follows:

> Rarely has a scientific discovery, apparently so forbidding in character, been attended by such an outburst of interest and inquiry. The silent, matter-of-fact way in which relativity has been absorbed into the general scheme of physics stands in striking contrast to the fanfare with which it has been received by the general public. From the time of the Burlington House meeting onwards there has been a ceaseless procession of books, pamphlets, newspaper articles, lectures, pictures, even cinema films, dedicated to the task of making plain to the man in the street what relativity really means.

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29. R.A. Lyttleton, "The Gold Effect," *Lying Truths: A critical scrutiny of current beliefs and conventions*, pp. 181-198, Pergamon Press, (1979). (Compiled by Ronald Duncan and Miranda Weston-Smith.)

30. R.A. Lyttleton, "Journey to the Centre of Uncertainty," *Science and Uncertainty*, pp. 46-67, Science Reviews Ltd., (1985). (Proceedings of a Conference held under the auspices of IBM United Kingdom Ltd., London, March 1984; Edited by Sara Nash.)

31. H.D., "The Understanding of Relativity," *Nature* **122** pp. 673-675 (1928).

The enormous interest in relativity has continued ever since, reaching new heights during the celebrations in 1979 of the centenary of Einstein's birth. I suggest that the Gold Effect may have been very strongly present as a result of the enormous interest in relativity over so many years.

All things considered, consensus is a very shaky foundation for scientific knowledge. As we have seen, we have scientists who publish comments on the doubters, and then ignore published rebuttals or plead to let the matter rest or retreat behind statements that the subject has been beaten to death. We have censorship. We have appeals to authority, even to the endorsement of a book revision published a decade after the authority's death. By such means is ''scientific knowledge'' upheld and reinforced.

The inappropriateness of consensus as a goal of science has been aptly pointed out by Anthony Standen in his well-known book *Science is a Sacred Cow*[32], especially in the following passage in which he compares science with democracy:

> Science, being a matter of reproducible experiments and of theories applied to the facts by the use of reason, is entirely different [from democracy], although one would hardly think so from hearing or reading what scientists say and write. They frequently go according to ''what most experts believe --'' or ''the consensus of expert opinion --'' or ''the understanding of the leading specialists --'' When they do this, what they are doing is science in name only, just as in some countries there is democracy in name only. Genuine science does not proceed, as democracy does, by counting heads, even if they are heads of departments at universities.

32. A. Standen, *Science is a Sacred Cow*, E.P. Dutton (1950).

CHAPTER 20

AN OVERDUE SCIENTIFIC REVOLUTION?

To make a *paradigm switch* on the scale of the revolution of the Earth Sciences in the 1960s one must *unlearn* much that one thought one knew, to the extent almost of returning to the ignorance of the student or child.
John Ziman: *Reliable Knowledge.*

The findings presented in this work do not suggest a retreat from Einstein to pre-Einstein; they suggest an advance from Einstein to post-Einstein.
Dewey B. Larson: *New Light on Space and Time.*

In his article ''Overdue: another scientific revolution'', Dr. J. Tuzo Wilson[1] described some of the characteristics of scientific revolutions of the past, saying that ''history shows that earlier scientific revolutions have cast their shadows before them and have progressed through several parallel stages''. Since Dr. Wilson was actively involved in another scientific revolution, in connection with continental drift, his ideas on the growth and decay of scientific theories are worthy of some study.

Before each of the past revolutions, according to Wilson, the old theories worked well and were useful; the triumph of each theory was due to new discoveries or ideas that the previous theories could not explain. He went on to describe the features that scientific revolutions possess in common, in the following way:

> In each case the great event marking the revolution revealed contradictions and introduced problems which the old theory could not resolve, leading to a period of confusion. For the most part it was not experts but rebels, outsiders and interlopers from other fields of science who suggested the need for a revolution and produced the evidence supporting it. Thus most geologists continued to oppose the concept of continental drift long after Wegener (a meteorologist) had championed it, after physicists had provided key evidence supporting it and after many of the public had accepted it.
>
> So, far from welcoming the new ideas, the establishment, who had the most to lose, clung for as long as possible to the old, justifying their position by questioning the new data, discrediting those who advanced them, and trying to patch up the old theories. In these endeavours they frequently found themselves supporting quite illogical positions. Before each revolution was accepted, the state of the subject had become chaotic, but each solved many problems so that the ensuing periods were times of great scientific progress and material benefit.

I think that an objective examination of the facts of Dingle's criticisms of special relativity, and the responses of the scientific community, shows clearly that many of the characteristics of a scientific revolution are present.

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1. J.T. Wilson, ''Overdue: Another Scientific Revolution,'' *Nature* **265** pp. 196-197 (1977).

Professor Ziman, in his book *Reliable Knowledge*[2], made a somewhat similar point to Wilson's when he wrote:

> The experts in a particular field can become so indoctrinated and so committed to the current paradigm that their critical and imaginative powers are inhibited, and they cannot 'see beyond their own noses'. In these circumstances scientific progress may come to a halt -- knowledge may even regress -- until intellectual intruders come through the interdisciplinary frontiers and look at the field without preconceptions. Thus it was only an influx of physicists drawing new evidence from the detailed study of rock magnetism that broke down the prejudices of the geologists against the theory of continental drift.

It is interesting that Wilson and Ziman, both physicists, use an example that allows them to point out the prejudice and opposition of the geologists and the positive contributions of the physicists. It is also interesting to note that one scientist was involved in a small way in both the continental-drift revolution and the relativity controversy; I refer to Sir W.L. (Lawrence) Bragg, mentioned by Dingle[3] as one of the "elder statesmen" to whom he appealed (he was about four months older than Dingle!). Bragg has described how impressed he was on first hearing about Wegener's theory of continental drift, saying[4]:

> I was so thrilled that I wrote to Wegener for an account of his theory, got it translated, and presented it to our Manchester Literary and Philosophical Society. The local geologists were furious; words cannot describe their utter scorn of anything so ridiculous as this theory, which has now proved so abundantly to be right.

Like Wilson and Ziman, Bragg (also a physicist) pointed out the prejudice of the geologists. Unfortunately, neither Ziman nor Bragg, in their dealings with Dingle, seemed to be as willing to consider the possibility that there might also be serious prejudice and opposition to new ideas in their own field of physics.

It is the main thesis of this book that there is a case to answer, and that it has not been answered. Quite independently of whether the special theory of relativity is right or wrong, the fact remains that the arguments used in defending the theory from Professor Dingle's criticisms are so full of inconsistencies that it is impossible to take them seriously, and I do not believe that the great man who was Albert Einstein would have wished his theory to be defended, after his death, by such a collection of inept and inconsistent arguments.

Although Herbert Dingle criticized Einstein's theory, he repeatedly acknowledged Einstein's genius. Even if the special theory had to be abandoned, Einstein's place in history would still be secure, and there is no question of returning to pre-Einsteinian

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2. J. Ziman, *Reliable Knowledge: An exploration of the grounds for belief in science,* Cambridge University Press (1978).

3. H. Dingle, *Science at the Crossroads,* Martin Brian & O'Keeffe, London (1972).

4. S.-I. Akasofu, B. Fogle, and B. Haurwitz (Editors), *Sydney Chapman, Eighty: from his Friends,* University of Alaska, University of Colorado, and University Corporation for Atmospheric Research (1968). (The item by Professor Bragg is on page 50.)

physics. Unfortunately, however, many physicists appear to be somewhat anxious about what they consider to be attempts to reduce the eminence of Albert Einstein. Consider, for example, the following excerpt from a review of *The Einstein Decade, 1905-15*[5]:

> No doubt we can easily forgive the author's over-anxiety about the status of his hero, but the constant jumping-up-and-down exhibited by recent writers on Einstein is too desperate by far and too noisy. They protest too much. Surely Einstein himself, it is safe to assume, is waiting serenely and quietly for everything to take its proper place.

We also need to remember that it was Einstein's own opinion[6] that "there are no eternal theories in science."

Even so, the question of whether special relativity is right or wrong may not be the most important issue. An important question is whether science seeks the truth without prejudice. If scientists publish papers that uphold orthodox views or criticize unorthodox ones, and then suppress or ignore rebuttals or say that the subject has been debated enough, they are not likely to approach closer to the truth. If a journal refuses to publish anything more on a debatable subject because there is nothing more that needs to be said, and then uses a scientist's obituary as a means of repeating an earlier published criticism of his views on that subject, it does not show a very high regard for either fairness or truth.

Consider the following statement, which is the first sentence of an article by John Maddox on experimental tests of special relativity[7]:

> Given the zeal with which those who disbelieve the special theory of relativity go about their business, it is a comfort that experimental tests of relativity are also flourishing.

Although Mr. Maddox does not refer to any particular group of disbelievers, it is reasonable to assume that he includes those who claim that the theory is self-contradictory. It should therefore be pointed out that there is no experimental evidence that can refute that claim, because a theory that is self-contradictory can, by valid logical rules of inference, be used to predict any experimental result whatever. As Professor Popper puts it[8] (p. 92):

> . . . the importance of the requirement of consistency will be appreciated if one realizes that a self-contradictory system is uninformative. It is so because any conclusion we please can be derived from it.

As Professor Dingle repeatedly stated, and others repeatedly ignored, his Question about the special theory is not about what actually occurs in a particular experiment, but about what the theory *requires* to occur. Experimental results have no bearing whatever

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5. "That Man Again", *The Economist* **252, No. 6836** p. 81 (August 31, 1974). (Anonymous review of 'The Einstein Decade, 1905-15', by C. Lanczos).

6. A. Einstein and L. Infeld, *The Evolution of Physics,* Cambridge University Press (1938).

7. J. Maddox, "More Tests of Special Relativity," *Nature* **319** p. 533 (13 February 1986).

8. K.R. Popper, *The Logic of Scientific Discovery,* Harper Torchbooks (1968).

on the answer, and Professor Dingle's claim that there is an internal inconsistency in the special theory cannot be refuted by any experimental results whatever,

The last sentence of Mr. Maddox's article[9] is also interesting; it reads as follows:

> Much has been done already, but further efforts are needed, and not merely to keep the army of sceptics of the special theory at bay.

Since Mr. Maddox viewed Herbert Dingle's supporters in 1975 as a small misguided band[10], it is interesting to see that only a decade later he viewed the body of sceptics of the special theory as an army. Perhaps this rapid change is a sign that an overdue scientific revolution is about to occur.

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9. J. Maddox, ''More Tests of Special Relativity,'' *Nature* **319** p. 533 (13 February 1986).

10. J. Maddox, ''Integrity in Science,'' *Nature* **255** p. 520 (1975).