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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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].

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.
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> 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.
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> 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.
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> 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?''.
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> 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.