

*Herbert Dingle*

*Science at the Crossroads*

*A Rational Scrutiny  
of the Clock Paradox  
in Einstein's Relativity*

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# *Contents*

## **FOREWORD FROM THE EDITOR**

*What we need to know to read Science at the Crossroad*  
*The first interpretation of relativity by Dingle*  
*Note to the 2018 electronic edition*

## **SCIENCE AT THE CROSSROADS - ORIGINAL TITLE PAGE**

*Acknowledgements*

## **PREFACE**

## **INTRODUCTION**

## **PART ONE - THE MORAL ISSUE**

- 1 - The Basic Principles of Science*
- 2 - The Origin of the Controversy*
- 3 - Reactions to Criticism*
- 4 - Attitude of the Press*
- 5 - Attitude of the 'Elder Statesmen'*

## **PART TWO - THE INTELLECTUAL ISSUE**

- 6 - Four Outstanding Errors*
- 7 - Einstein's Theory in its Original Context*
- 8 - Non-Einsteinian Relativity*
- 9 - The 'Clock (or Twin) Paradox'*
- 10 - The Present Position*

## **CONCLUSION**

## **APPENDIX**

- I - Don't Bring back the Ether*
- II - The Case Against Special Relativity*
- III - Why the Special Theory of Relativity is Correct*
- IV - The Case Against the Special Theory of Relativity*

## **EDITOR'S AFTERWORD**

*What we do not find in Dingle's testimony*  
*The implications of mathematization: an open problem*

## **BACK COVER**

*Herbert Dingle*

## ***Foreword from the Editor***

*Science at the Crossroads* was published by Herbert Dingle, an Astrophysicist, Philosopher of Science, President of the Royal Astronomical Society, and Professor at University College London, in 1972 at the end of a gruelling controversy with the English scientific world about the clock paradox in Einstein's special relativity, and is still a seminal text, which allows us to understand and evaluate, with Cartesian clarity, the problem of that paradox for all those readers, who find themselves in a widespread mental condition of having studied Einstein's special relativity or received a teaching of it, and yet of not feeling comfortable with it because the attempt to understand the logical connection of the parts with the whole did not succeed. It is a state of mind that many have experienced, and that is usually admitted without difficulty.

### ***What we need to know to read Science at the Crossroad***

Dingle's text, as everyone will ascertain reading it, is a masterpiece of clarity, where the expression flows easily and the subject is treated with completeness and consistency, making its reading a very pleasant experience. However, the readers must already know the terms of the open problem that is discussed in *Science at the Crossroads*. Readers do not need to know the solution, because the book was written for those who know the content and the mathematical part of special relativity (which, as is known, does not present any particular difficulty, unlike general relativity), but find themselves uncomfortable with the overall logical connection. Dingle's book is not for beginners, although the notions necessary to understand it are only those of elementary high-school physics. Indeed, the first part, in which we read an astonishing account of the polemics that opposed Dingle to the British physicists (his colleagues), does not require any prerequisite to be understood, but it would not say anything to those who have not personally experienced the problem of understanding special relativity. The second part of the book discusses in detail many aspects of special relativity, but was written for those who already know the elements of the theory.

So, let us recap the basics needed for reading *Science at the Crossroads*.

A book of mine is available (*Relativity from Lorentz to Einstein*<sup>1</sup>), that I recommend as a preliminary reading to all those who do not possess the prerequisites mentioned below. After reading it, Dingle's book will become fully intelligible. Here the summary of the prerequisites will be contained in a few pages.

We all know, if we know the basics of relativity, that in the early nineteenth century light and electromagnetic phenomena were interpreted as wave-like phenomena, and that a hypothesis was made, that everywhere in space light signals or electromagnetic waves propagate, there must exist a propagation medium of this wave, which was called ether, a classical term, belonging to the pre-scientific culture. But the nineteenth-century electromagnetic ether was neither a coarse concept, nor out of date; at first, the hypothesis arose only from the need to reduce the phenomenon of light to a familiar interpretation scheme, but then the concept of ether was hypothetically determined in very precise ways, according to various models, in order to attribute to it the physical characteristics that would be consistent with the properties of electromagnetic waves tested experimentally.

Over the course of just a century, the theory of electrical and magnetic phenomena had immensely evolved, passing from Galvani's experiments with his frogs, which he mistakenly believed to be generators of electricity, to Voltaic piles and from there to electrical technology that in the first years of the twentieth century allowed the operation of electric trams and locomotives, electrical distribution networks in cities, telegraphs, telephones, Marconi's radio stations, and so on.

**( ... *End of the Foreword Preview* ... )**

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<sup>1</sup> Palazzi, Alberto, *Relativity from Lorentz to Einstein. A Guide for Beginners, Perplexed and Experimental Scientists*, il glifo ebooks, 2018. In Italian: *La relatività da Lorentz a Einstein. Una guida per principianti, perplessi e scienziati sperimentali*, il glifo ebooks, 2017.

### ***Note to the 2018 electronic edition***

*Science at the crossroads* was scanned on the basis of a 1972 paper copy and carefully controlled, in order to preserve and make available this text, until now very difficult to find. Since the 1972 paper edition is extremely rare, this electronic edition has been edited carefully in order to make it the main source of Herbert Dingle's original text and ensure its availability to readers.

The page numbers of the original edition have been preserved in [square brackets].

Footnotes have been numbered continuously in a unique series, since notes numeration in the original text followed an unpractical criterion. The numbers of each note in the original text has also been preserved in [square brackets]. Some footnotes had no number, but a symbol [+], [\*], also preserved.

# *Science at the Crossroads - Original title page*

*Science at the Crossroads*

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*Practical Applications of Spectrum Analysis*, 1950

*The Scientific Adventure*, 1952

*The Sources of Eddington's Philosophy*, 1954

*Life and Work of Sir Norman Lockyer (Part Author)*, 1954

*A Threefold Cord (with late Viscount Samuel)*, 1961

*A Century of Science (Editor)*, 1951

## ***Acknowledgements***

I should like to thank Mr Frank W. Cousins F.R.A.S. and Mr Ian Kiek for their invaluable help during the course of publication of this book. I should also acknowledge and thank the Editor of *Nature* and Professor W. H. McCrea for permission to quote from its pages and to thank the Editors of *The Times* and the *Listener* for quotations used from correspondence concerning the issues raised in this book.

H. D.

## *Preface*

[9] This book was written during the first half of 1971. Before arrangements for its publication had been completed, however, an independent controversy sprang up in the *Listener*, in which reference was made to the correspondence in that journal which is discussed in the following pages (83-87). This seemed to afford a possibility of achieving the desired end without the necessity of revealing the much fuller story told here: accordingly I withheld the typescript and gave, in the *Listener* of 23 September 1971, a brief account of the sequel to the former controversy. The result was another long series of letters, extending from the issue of 30 September 1971 to that of 13 January 1972, which inspired, among other things, an article by Mr Bernard Levin in *The Times* of 21 December 1971, which itself led to a brief correspondence in *The Times*.

The general interest thus brought to light, as I know from my subsequent correspondence from various parts of the world, was great and widespread, but the one essential desideratum of the whole exercise — plain evidence, through an answer to, or acceptance of, a very simple refutation of the immeasurably important special relativity theory, that the obligation to preserve strict integrity in science continues to be honoured — was still not forthcoming. Physical research, both theoretical and practical, still proceeds as though special relativity were unquestioned. There remains, therefore, no alternative to publication of the facts here recorded.

It is impossible in a brief space satisfactorily to summarise the whole of this latest phase of the matter, nor is it necessary, for the journals concerned may be consulted by interested readers, and on the one vital point no progress is made; the criticism remains unanswered and unaccepted, and its implications are unchanged. It will, however, serve to authenticate this statement, and at the same time introduce the reader at once to the central source of the book, [10] if I reproduce the final letters, in *The Times* of 8 and 26 January 1972, respectively — the first from Professor R. A. Lyttleton, F.R.S., of St John's College, Cambridge, and the second my reply — and simply add that Professor Lyttleton has not responded, either privately or publicly, to my appeal to him for the one

brief statement that would settle the whole matter. Lyttleton wrote as follows:

My old friend Dr. Dingle seems at last to have found in Bernard Levin (article, December 21) a kindred spirit to champion him in his lone verbal onslaughts against what he regards as a certain pernicious claim of modern physics.

In brief, what Dingle has steadfastly maintained these many years against all comers is this: That if Peter and Paul are identical twins, and Paul goes on a journey leaving Peter to stay at home, then when Paul returns he will still be exactly the same age as his brother.

The truth of this seems so self-evident as to be beyond need of discussion by any sane people. But the trouble is that it is false, and physical theory shows inescapably that Paul will arrive back having aged less than Peter. For ordinary everyday speeds the difference is negligibly small, and it rises to importance only when velocities begin to become comparable with that of light, but such speeds are now common in much of physics.

The kinematics and mechanics (of special relativity) that hold for high-speed motions had their inception in the inspired genius of Poincaré (Henri) and Einstein and others of their day, and the suggestion that such men, never mind modern exponents of theoretical physics, do not know what they are talking about is on a par with claiming that Vardon and Taylor and Hagen knew nothing of golf. But this so-called ‘clock paradox’ (it is not really a paradox at all) is built for friend Dingle, since the man-in-the-street does not have to deal with relativistic particles such as mu-mesons, or the design of synchrotrons, and so along with Mr. Levin can remain absolutely certain that Dingle must be right wielding his prolix pen ‘while words of learned, length and thundering sound, amaze the gazing rustics gathered round.’

Dr Dingle’s attitude is of a golfing enthusiastic that has read the great masters, but finding himself unable to break 100 (never mind break 70) concludes it is *they* that must be wrong [11] somewhere; and what is more, that it is their bounded duty to interrupt their careers to prove to his satisfaction that they are right.

If your energetic Bernard would spend a little time learning up this branch of physics, which is not really all that difficult, he can easily discover for himself who is right and who is wrong, but he will discover also that it is not possible to convince our dear Dingle, ‘For e’en though vanquished, he can argue still,’ — and will!

My reply was this:

My old (in affection, not alas in wisdom) friend Professor Lyttleton (January 8) has got everything wrong — even the point at issue. I have carefully avoided the ‘clock’, or ‘twin’, paradox (in which Paul, after space-travelling, rejoins Peter), knowing from experience that Paul’s reversal of motion can be misused ad lib, to meet any need. In the present discussion Paul moves on, undeviating, into the intense inane.

Suppose clocks A and B move along the same straight line at uniform speeds differing by 161,000 miles a second: we call A ‘stationary’ and B ‘moving’, but that is merely nominal. At the instant at which B passes A both read noon. Then, according to special relativity, at the instants when B reads 1 and 2 o’clock, A reads 2 and 4 o’clock respectively. Of course, A is not at B to allow a direct comparison, but Einstein’s theory is based on a particular process for finding a clock-reading for a distant event, and it demands these values. Einstein himself made just this calculation, but using general symbols instead of these numerical values, and concluded that since B recorded a smaller interval than A between the same events, it was working more slowly.

But if he had similarly calculated the reading of B (still ‘moving’) for the readings 1 and 2 o’clock of A (still ‘stationary’) he would have got 2 and 4 o’clock respectively, and must have reached the opposite conclusion: he did not do this, so missed the contradiction. I invite Ray to fault these calculations, or convince your ‘gazing rushes’ that each of two clocks can work faster than the other. I do hope he will not disappoint them.

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 [12] discovered my error, and for more than 13 years have held the question open. Had we but world enough and time, or wings as swift as meditation or the thoughts of love (since I too like invoking the English, and even the Irish, poets), we could indeed make a direct test: as it is, we must await a valid determination of the true relation between the velocity of light and that of its source. 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.

It is clear from this that, notwithstanding many years of reiteration of what my letter shows to be a simple, generally intelligible — but, if valid, fatal — criticism of the most fundamental theory of modern physics, the

ultimate reaction, coming from an eminent mathematical physicist or astronomer, is simply a paraphrase of what this book will show to have been every other supposedly authoritative response during that long time — namely, first an evasion of the point by its transformation into something different, for the refutation of which justification is claimed on grounds too abstruse for general presentation; and secondly, complete silence when the transformation is exposed and an answer to the genuine, easily understandable, criticism requested. The function of this book is to provide conclusive evidence of this, and so to enlighten the public on a matter of the most profound concern to its moral and physical welfare.

It remains to summarise the necessity for this exposure, which of course is elaborated in the following pages. This necessity is twofold. First, the facts show, I think beyond question, that the traditional proud claim of Science that it acknowledges the absolute authority of experience (i.e. observation and experiment) and reason over all theories, hypotheses, prejudices, expectations or probabilities, however apparently firmly established, can no longer be upheld. The devotion to truth at all costs has gradually given place — largely unconsciously, I believe, but still undeniably — to the blind pursuit of the superficially plausible; the direction towards the most seductive, in which advance has been easiest, has been taken without regard to preservation of contact with the base, which is the truth of experience and reason; the verdict of those authorities falls on deaf ears, that of the Vardons or Hagens of physics, to question [13] which is automatically to place oneself in a class which Lyttleton's letter makes starkly clear, having now established itself as final; mathematics has been transformed from the servant of experience into its master, and instead of enabling the full implications and potentialities of the facts of experience to be realised and amplified, it has been held necessarily to symbolise truths which are in fact sheer impossibilities but are presented to the layman as discoveries which, though they appear to him absurd, are nevertheless true because mathematical inventions, which he cannot understand require them. The situation is precisely equivalent to that in which the zoologist assured the astonished spectator of the giraffe that if he understood anatomy he would know that such a creature was impossible — except that, in physical science, the layman usually believes what he is told and, unless he is

enlightened in time, will be the victim of the consequences. This phenomenon, most evident in relation to special relativity, is now common in physical science, especially in cosmology, but its culminating point lay, I think, in the acceptance of special relativity, and it is with that alone that the present discussion is concerned. It is ironical that, in the very field in which Science has claimed superiority to Theology, for example — in the abandoning of dogma and the granting of absolute freedom to criticism — the positions are now reversed. Science will not tolerate criticism of special relativity, while Theology talks freely about the death of God, religionless Christianity, and so on (on which I make no comment whatever). Unless scientists can be awakened to the situation into which they have lapsed, the future of science and civilisation is black indeed.

The second reason for the publication of this book is a practical one. Directly or indirectly — at present chiefly the latter, though none the less inseparably — special relativity is involved in all modern physical experiments, and these are known to be attended by such dangerous possibilities, should something go wrong with them, that the duty of ensuring as far as possible that this shall not happen is imperative. It is certain that, sooner or later, experiments based on false theories will have unexpected results, and these, in the experiments of the present day, may be harmless or incalculably disastrous. In these circumstances an inescapable obligation is laid on experimental physicists to subject their theories to the most stringent criticism. As this book will show, their general practice is to leave such criticism to mathematical theorists who either evade [14] or ignore it, and the possible consequences are evident and unspeakably menacing. This alone would compel the publication of the facts here revealed.

Nothing, I think, remains to be said to enable the reader to form his own estimate of the story that follows, which he requires no special knowledge to enable him to do. My duty is to make it known; its significance is for him to judge.

April 1972

## ***Introduction***

[15] This is a book which I have been trying for more than thirteen years to avoid having to write: I have at last been forced to do so because it has become impossible for its purpose to be achieved otherwise and that purpose is imperative.

I am well aware that the bare summary of the matter given in this Introduction will appear so incredible that the reader will feel an almost irresistible impulse to dismiss it as illusory: that is why the evidence has to be given at such length and in such terms that doubt of its reality will be impossible; its gravity, if it is real, will need no proof. The fantastic appearance of the situation is indeed one of the reasons why it has not been rectified long since; those who could have rectified it have found it impossible to credit, and it has accordingly been allowed to persist, with the result that unless drastic action is taken, the whole community stands at a risk which is quite incalculable but might be overwhelmingly great. In introducing the matter here, therefore, I beg the reader to suspend his incredulity, which it will need the whole evidence that follows to remove, and to accept, merely as a working hypothesis at present, that what I have to say is true. Part One, which is concerned only with the *ethical* principles of science, not with technical details, is wholly comprehensible to any intelligent person, while Part Two needs a little elementary knowledge of physics, less than that possessed by any physics undergraduate, for its full comprehension, and only ordinary intelligence for a true idea of its general import.

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 [16] basic to practically all physical experiments, the consequences if it is false, modern atomic experiments being what they are, may be immeasurably calamitous. That is why the failure of physical scientists to practise what is generally understood to be their faithfully preserved fundamental ethical principle — the subordination of all theories, however plausible,

to the demands of reason and experience — compels its exposure. In the conditions of former days the falseness or otherwise of the theory could have been left to the arbitrament of experiment, which would, sooner or later, inevitably have appeared: today the possible consequences of such, equally inevitable, settlement of the question are far too dire, and nothing but the observance of strict scientific integrity, here and now, can meet the ethical demands of the case.

The reason why this has happened is largely that which will, in all probability, immediately strike the reader — namely, that the theory of relativity is believed to be so abstruse that only a very select body of specialists can be expected to understand it. In fact this is quite false; the theory itself is very simple, but it has been quite unnecessarily enveloped in a cloak of metaphysical obscurity which has really nothing whatever to do with it; the physical theory itself, indeed, is much simpler than many physical theories familiar to most educated non-scientific but interested persons in the nineteenth century; it is wholly devoid of any mystical significance. This will be explained in Part Two, where the historical reasons for the illusions concerning the theory are fully set out. But the consequences of those illusions are the vitally important matter for the general public. They are, briefly, that the great majority of physical scientists, including practically all those who conduct experiments in physics and are best known to the world as leaders in science, when pressed to answer allegedly fatal criticism of the theory, confess either that they regard the theory as nonsensical but accept it because the few mathematical specialists in the subject say they should do so, or that they do not pretend to understand the subject at all, but, again, accept the theory as fully established by others and therefore a safe basis for their experiments. The response of the comparatively few specialists to the criticism is either complete silence or a variety of evasions couched in mystical language which succeeds in convincing the experimenters that they are quite right in believing that the theory is too abstruse for their comprehension and that they may safely trust men endowed with the metaphysical and mathematical [17] talents that enable them to write confidently in such profound terms. What no one does is to answer the criticism.

It would naturally be supposed that the point at issue, even if less

esoteric than it is generally supposed to be, must still be too subtle and profound for the ordinary reader to be expected to understand it. On the contrary, it is of the most extreme simplicity. According to the theory, if you have two exactly similar clocks, A and B, and one is moving with respect to the other, they must work at different rates (a more detailed, but equally simple, statement is given on pp. 45-6, but this gives the full essence of the matter), i.e. one works more slowly than the other. But the theory also requires that you cannot distinguish which clock is the 'moving' one; it is equally true to say that A rests while B moves and that B rests while A moves. The question therefore arises: how does one determine, consistently with the theory, which clock works the more slowly? Unless this question is answerable, the theory unavoidably requires that A works more slowly than B and B more slowly than A — which it requires no super-intelligence to see is impossible. Now, clearly, a theory that requires an impossibility cannot be true, and scientific integrity requires, therefore, either that the question just posed shall be answered, or else that the theory shall be acknowledged to be false. But, as I have said, more than 13 years of continuous effort have failed to produce either response. The question is left by the experimenters to the mathematical specialists, who either ignore it or shroud it in various obscurities, while experiments involving enormous physical risk go on being performed.

It cannot be too strongly emphasized that this question is exactly what it appears to be, with every word and phrase bearing its ordinary, generally understood, meaning; it is not a profoundly complicated question, artificially simplified to bring it within the scope of the non-scientific reader's intelligence. It is presented here in its full scientific reality, and the ordinary reader is as fully competent to understand whether a proffered answer is in fact an answer or an evasion as is the most learned physicist or mathematician — though, of course, he may not be able to judge whether the suggested answer is true or not. For instance, the statement: 'the slower-running clock is that judged by a chosen body of experts to be the more beautiful' would be an answer, though it is not likely to be acceptable to anyone. On the other hand, the statement: 'I cast my vote for the special theory of relativity and the abandonment of Dingle's [18] concept of clocks because the latter is equivalent to

Newton's concept of absolute time, and relativistic physics appears to me to represent nature more closely than Newtonian physics does' (see p. 77 for the fuller statement from which this is taken), which is the conclusion reached by one generally considered to be among the most authoritative mathematical experts on relativity, can be seen by anyone to be no answer at all, but a clear evasion of the question. Who can gather from this how to tell which clock works the more slowly? The question is by-passed, and the reader is led into a slough of metaphysical concepts which have nothing whatever to do with it. Nevertheless, the statement serves to confirm the experimenters' conviction that the matter is beyond their understanding but has been competently dealt with by an expert authority, so they need give it no further attention.

This is typical of all responses to the criticism that have yet appeared: I choose it here because of the outstanding reputation of its author in this field and the fact that it can be expressed more briefly than most — far more briefly, for instance, than the equally evasive and far denser obscurity (given here in the Appendix) that 'convinced' the then President of the Royal Society that what he had been 'teaching' for many years but confessed he did not understand, was indeed true (see pp. 97, 100). It serves to explain why this book has become necessary — because unceasing and world-wide effort over many years has produced nothing but such evasions of a simple question needing less than six lines to answer if answer is possible, and revealing a universal attitude foreshadowing certain danger to the whole population if it is not. Any reviewer of the book can dispose at a stroke of its basic *raison d'être* by giving those six lines. By the same token, his failure to do so would speak for itself.

It is no doubt generally believed that means exist for preventing the occurrence of such a situation as this, and theoretically, of course, they do. The Royal Society is a body whose function includes the safeguarding of scientific integrity in all matters, and especially those vital to public welfare in this country (the situation is of general significance, of course, but for reasons of space I deal in this book almost wholly with Britain), and accordingly, after great difficulty in overcoming the interposed obstacles, the criticism was submitted to it for consideration. It was rejected on the basis of a report from an anonymous 'specialist' that the

fallacy invalidating it was too elementary even to be instructive. The ‘fallacy’, however, [19] was not revealed, nor was the simple but crucial question answered, but the customary paragraphs of mystical comment were supplied, and these satisfied the Society that the criticism was baseless. A letter to the leading scientific journal, *Nature*, asking, in the public interest and in accordance with the principles of the Society, that the fallacy should be published, was refused publication, on the ground that actions of the Royal Society were not open to question in *Nature*. An attempt was made to obtain a ruling of the Press Council (one of whose functions is ‘to keep under review developments likely to restrict the supply of information of public interest and importance’) on this refusal of *Nature* — not, be it noted, merely on this instance, but on the general decision of the editor that *no* action of the Royal Society, whatever its relation to the public interest, was open to questioning in the journal — but the officers of the Council would not allow the inquiry to reach it. As will be seen in this book, other scientific journals impose a similar veto; that again is part of the reason why I have been forced to use the medium of a book to acquaint the public with the position in which it stands: a body of scientists, in whose uncontrolled hands the physical safety of the whole community lies, is daily engaged in experiments of the greatest potential danger, based on principles which the experimenters confess they do not understand, and the Press is closed to any criticism, however well informed, of their activities, and to all questioning of their decisions.

These, then, are the circumstances that have made this book necessary. My purpose throughout is not to indict but to inform, and let the facts bring whatever indictment is necessary. This book is the only means I have of doing so. I have written it with the greatest regret, not only because iconoclasm is not an activity in which I take any pleasure at all, but also because most of those whom I am forced to present in what is bound to appear an unfavourable light — though I still believe that they do not fully realise what they are doing — are those whose friendship I value and must inevitably run the risk of forfeiting: it is largely this consideration that has persuaded me to continue so long in an endeavour which perhaps I ought long ago to have realised was hopeless. But to continue now to withhold the certain knowledge which I possess from those whose welfare, and even existence, depend on it, would be a

betrayal of responsibility of which I am no longer willing to be guilty.

\* \* \*

[20] After the writing of this book was completed came the sad news of the death of Sir Lawrence Bragg who, as will be seen, figures prominently in one section. This raised a problem, and after reflection I have decided to leave what was written exactly as it was, without change even of tense. This seemed desirable for two reasons. First, it conforms to what I cannot too strongly emphasise — that the purpose of the book is wholly objective and what is said in it of any person relates only to the public significance of the work of that person and so is independent of whether he or she is alive or dead. Secondly, Sir Lawrence had read this Introduction and the whole passage referring to him, knowing that it would be included verbatim in the book, as it appears here down to his last letter, printed on p. 113, which was written only a few weeks before his death and now takes on an added poignancy. I know, therefore, that by leaving the passage unchanged I am saying nothing to the appearance of which he would have raised objection.

The case of Dame Kathleen Lonsdale, who died during the writing of the book, is slightly different. I should not in any case have sent her a copy of the part referring to her, knowing her well enough to be sure that there was nothing in it to which she would have taken exception.

( ... *End of Preview* ... )

## ***Back Cover***

*Science at the Crossroads* was published by Herbert Dingle in 1972 at the end of a gruelling controversy with the English scientific world about the clock paradox in Einstein's special relativity. It is still a seminal text, which allows us to understand and evaluate, with Cartesian clarity, the problem of that paradox for all those readers who find themselves in a widespread condition of having studied Einstein's special relativity or received a teaching of it, and yet of not feeling comfortable it because the attempt to understand the logical connection of the parts with the whole did not succeed. Many have experienced this state of mind, and can admit it. In this way *Science at the Crossroads* is an important source of knowledge about Einstein's special relativity and its historical background.

*Science at the Crossroads* contains a great wealth of philosophical and epistemological ideas. The essay has a classic imprint, rich with sharp and clear distinctions, and very elegant in style. We meet countless research possibilities concerning the mathematical idealism of anti-classical physics after 1920 (assuming it is still correct to call it physics rather than metaphysics, as Dingle would suggest, as a partisan of modern science empirical method). Every page written by Dingle suggests research that could be carried out in depth, starting from what Dingle observes on the origins of mathematical idealism in Maxwell himself and his displacement current postulated to ensure the continuity of a given mathematical function, to end with the extreme tendencies of this kind of thought that were manifested towards the 1970s, as for example in the case of Professor Hoyle mentioned in the last chapter, who "has plainly stated his advocacy of the process of telling nature what to do instead of looking to see what she does". Naturally, it is our task to continue Dingle's research up to the present.

The present electronic edition provides a Foreword from the editor, which informs about the basic knowledge readers are expected to have in order to fully understand *Science at the Crossroads*.

## ***Herbert Dingle***

Herbert Dingle (1890–1978) was an English physicist and philosopher

of science, who served as president of the Royal Astronomical Society from 1951 to 1953. Dingle became a professor of Natural Philosophy at Imperial College in 1938, and was a professor of History and Philosophy of Science at University College London from 1946 until his retirement in 1955. Thereafter, he held the customary title of Professor Emeritus from that institution. He was one of the founders of the British Society for the History of Science, and served as President from 1955 to 1957. He founded what later became the British Society for the Philosophy of Science as well as its journal, the *British Journal for The Philosophy of Science*.