SCIENCE

At the Crossroads

HERBERT DINGLE

Professor Emeritus of History

And Philosophy of Science,

University of London

MARTIN BRIAN

& O'KEEFFE

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Preface

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, 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.
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> 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 Poincare (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 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 rustics' 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 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 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 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.

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Introduction

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 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 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 emphasised 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 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' (sec 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'etre* 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, 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.

* * *

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.

PART ONE

The Moral Issue

1

The Basic Principles of Science

On the nature and definition of Science there has long been, and will doubtless continue to be, much disputation, but on one characteristic at least of its practice, agreement is general — its unqualified devotion to the discovery of truth at whatever cost to its expectations and tentative assumptions. Its conception of 'truth', of course, may be limited — this again is a matter of controversy — but never qualified by compromise or expectancy of any kind. Within its own intellectual sphere, however that may be conceived, its disinterestedness has been regarded as absolute, and it has often been held up as a model for other human activities — political, theological, and what not — in which throughout history has been only too evident the influence of prejudice and partisanship, from which science alone has kept itself free. Of the many expressions of this idea which may be found in the literature of the last few centuries, coming from both scientists and non-scientists, I select as a paradigm the following typical statement by the late Sir Henry Dale, O. M., a former President of the Royal Society, and one of the outstanding scientists and most universally respected representatives of his calling in this century:

> And science, we should insist, better than any other discipline, can hold up to its students and followers an ideal of patient devotion to the search for objective truth, with vision unclouded by personal or political motive, not tolerating any lapse from precision or neglect of any anomaly, fearing only prejudice and preconception, accepting nature's answers humbly and with courage, and giving them to the world with an unflinching fidelity. The world cannot afford to lose such a contribution to the moral framework of its civilisation.[1]

It is not, of course, to be supposed that every scientist has on every occasion lived up to the counsel of perfection which this statement represents; far from it, although it is true that, on the whole, the history of science compares very favourably indeed with the history of most, if not all, other human activities. Nevertheless, there are examples enough of prejudices and preconceptions, on the part of both individual scientists and scientific organisations — it is sufficient to mention the general dismissal during the eighteenth century of authentic evidence for the reality of falls of meteorites, on the sole ground that such things could not happen: for belief in the inviolability of laws of nature was substituted belief in the inviolability of the existing conception of what those laws were. It would be a gross error to imagine that scientists, as a class, are inherently more honest in their thinking and actions than men in other classes — a fact evident enough when we compare their extra-scientific activities with those of others. They are human, all too human, neither better nor worse on the whole than politicians and theologians, than historians and business men, than artists and artisans. What makes scientists behave so much more consistently in accordance with their ideals is not a unique 'original virtue' but the nature of their job.

This, as I have said, is, in its fundamental essence, a matter of dispute and academic discussion, but, speaking in general terms— which is not to say false terms so far as they go — it may be said that the aim of science is to discover what actually exists in nature and to express the relations between natural phenomena in rational form, 'i.e. in statements which, when established by sufficient evidence and found to hold good over a sufficiently wide range of experience, we call laws of nature, and when less completely supported but still possessing some measure of plausibility, we call theories or hypotheses. The evidence is never complete, and experience is never exhaustive, so all these statements are subject to change, but, however tenaciously scientists may wish to retain those which they have learned to trust, there is a finality about both experience and reason that ultimately overrides all opposition and forces the scientist to acknowledge the error of his preconceptions, however reluctant he may be to do so. The historian may defend or condemn the execution of Charles I; the theologian may assert or deny justification by faith; and nothing that any of them can do can finally refute his opponent. But the astronomer who asserts the existence of seven planets and denies the possibility of more, is silenced when an eighth is discovered; the experience on which he relics for the reality of the seven must have the same validity with regard to the eighth, and he has no option but to yield. The mathematician, in whose calculations leading to what he has asserted to be a proof of a theory the accidental omission of a factor 2 is discovered, must likewise acknowledge his error: no matter how strong his belief in the theory may be, the demands of reason which he has trusted to establish it now demand its abandonment. Scientists *must* be honest in the long run because the nature of their occupation makes them so: experience and reason are irresistible.

There is, however, one striking difference between the refutation of a hypothesis by experience and by reason which we must acknowledge, though it may be left to the psychologist to explain. Experience — i.e. observation or experiment — usually carries much greater conviction than reason, though ultimately they have equal authority. When a hypothesis is used to predict a certain experimental result, and the relevant experiment when performed yields the opposite result, there is generally no further discussion; the hypothesis is dismissed, or at least changed. But when the reasoning involved in a hypothesis is disputed (the example just given of an accidental mathematical error is of course a very special case, though it differs only in degree from reasoning processes much less obviously erroneous) there is usually no such general agreement on the truth of the matter, although, according to the strict principles of science, there should be. Newton, albeit with an ill grace, acknowledged an error in his reasoning concerning falling bodies which was detected by Hooke; he did not insist on an experimental test. But there has frequently been less readiness to abandon a cherished idea on rational than on experimental or observational grounds. In the nineteenth-century controversy on the age of the Earth between the geologists and the physicists, both sides had all the available evidence before them, and the difference in their conclusions arose wholly from the reasoning which they applied to it. We can see now, not only why equally intelligent reasoners reached widely different conclusions from the same evidence, but also that a stricter regard for the difference between necessary and probable conclusions from the evidence would have enabled a disinterested adjudicator to form a single judgement even then. It was, of course, a matter which could not be tested by experiment; had it been so, the dispute would have ended. In the event, only further experience, not then available but the possibility of which might have been anticipated, led to an agreed conclusion.

This greater degree of conviction which experience provides has had an important consequence in the progress of science. It has led to a relaxing of the demand that scientific hypotheses shall be strictly rational and a greater reliance on the ultimate verdict of experiment. This is not merely a development of the chief characteristic of the modern scientific movement that began in the seventeenth century and was marked by an exchange of interest solely in *a priori* reasoning prevalent in the Middle Ages for an interest primarily in experience. The pioneers of that movement — Galileo and Newton in particular — indeed insisted on the primacy of experience, but they relied no less than the Schoolmen on faithful obedience to the demands of reason in their ordering of experience and their deductions from what it revealed. Galileo has been criticised for his reasoning from 'thought experiments', and not only were these 'experiments', which were a novelty at that time, but also they involved rational thought and permitted nothing that violated the strict rules of reasoning. Newton, though he declared, in a famous phrase, that he did not make hypotheses, and in fact did make numerous experiments, nevertheless also laid down 'Rules of Reasoning in Philosophy', and did not hesitate to use what many today would regard as hypotheses. The scientific movement of the seventeenth century was a blend of experience and reasoning, in which both were essential but the reasoning was confined to what was derived from experience, and everything that was derived from 'principles' that had no justification except that they seemed necessary or good to those who adopted them, was firmly eschewed. But what gradually developed later, as a result of the greater degree of conviction that an experimental result brought with it, was a permissiveness in the framing of hypotheses, arising from the certainty that, if they were wrong, experiment would inevitably reveal that fact, and there was always a chance that, however improbable they might seem, they might turn out to be right.

There is much that can be said in defence of this — or at least there was — so long as the hypotheses are recognised for what they are — namely, a means of arriving at truth and not truth itself. Anything imaginable *might* be true — there are more things in heaven and earth than are dreamed of in our philosophy — and a few more dreams, which are not accepted as reality until waking experience confirms them, can do no harm, apart from a possible waste of time and money in a good cause, and might lead to the discovery of truths that would otherwise remain hidden. This indeed has happened not once or twice in the history of science. But it is attended by two dangers. The first, which was evident many years ago, is that the dreams shall be *substituted* for reality and accepted as true, not only before experience verifies them but wholly in their own right, regardless of whether experience verifies them or not. The second danger, which is relatively new, and demands far more urgent attention, is one reason why this book has had to be written: it lies in the fact that the experimental testings of the hypotheses of modern physics are attended by such possibly catastrophic results if the hypotheses are wrong, that the preliminary confirmation, that they are not *necessarily* wrong through violating the laws of reasoning, becomes imperative. Anything *imaginable* might be true: what is *not* imaginable — such as that Hitler both is and is not dead, or, to take a requirement of the hypothesis with which this book is chiefly concerned, that one clock can work steadily both faster and slower than another — *cannot* be true, and experiments based on the assumption that it is, are bound to lead, sooner or later, to anomalous results.

The first danger — the substitution of imagination for experience — was, as I say, realised long ago; this is how I exemplified it in a book published in 1931:[2]

> I will give three quotations from representative scientists, covering the period from Newton to the present time and separated by roughly equal intervals. The first is from Newton himself (1687): 'I frame no hypotheses. For whatever is not deduc'd from the phaenomena, is to be called an hypothesis; and hypotheses, whether metaphysical or physical, whether of occult qualities or mechanical, have no place in experimental philosophy.' The second is from Laplace, referring to his famous 'nebular hypothesis' (1796): 'I will suggest an hypothesis which appears to me to result with a great degree of probability, from the preceding phenomena, which, however, I present with that diffidence, which ought always to attach to whatever is not the result of observation and computation.' The third is from Eddington (1926); 'Care is taken to provide "macroscopic" equations for the human scale of appreciation of phenomena as well as "microscopic" equations for the microbe. But there is a difference in the attitude of the physicist towards these results; for him the macroscopic equations — the large-scale results — are just useful tools for scientific and practical progress; the microscopic view contains the real truth as to what is actually occurring.' The course of development is from a categorical rejection of hypotheses of any kind whatever, through a diffident presentation of one which results 'with a great degree of probability' from phenomena, to the confident assertion that a hypothesis contains 'real truth' and phenomena are just 'useful tools.' The question of the validity of this process is the most vital question, both for the philosophy of Science and for the application of scientific ideas to other departments of thought, at the present time.

Since that was written the process has gone even further. Not only are hypotheses held to contain the 'real truth'; it is now claimed that *any* (mathematical) hypothesis is *necessarily true.* In a recent paper, two physicists, 0.Bilaniuk and E.C.G. Sudarshan, write:3 'There is an unwritten precept in modern physics... which states that in physics "anything which is not prohibited is compulsory". Guided by this sort of argument we have made a number of remarkable discoveries, from neutrinos to radio galaxies.' 'We', of course, means scientists in general, and it is evident from the context that 'prohibited' means *mathematically* impossible. The statement that neutrinos and radio galaxies, or anything else, were so discovered is, of course, nonsense, but the statement is taken seriously and has instigated experiments directed towards the observation of 'tachyons' — hypothetical particles that travel faster than light — and stimulated a serious discussion in *Nature on* whether an effect can precede its cause. The relation between mathematics and physics is discussed in Chapter 6; in the meantime, this is sufficient to indicate how far we have gone along the path that started with the recognition that hypotheses might assist in the discovery of phenomena: first phenomena became 'useful tools' for the creation of hypotheses, and now hypotheses themselves are enthroned as necessary phenomena.

But it is the second danger that calls for immediate attention and, as indicated in the Introduction, has made this book necessary: it arises from the fact that modern physical experiments are such that the unexpected results which they produce might be catastrophic. Ironically enough, it is the very safeguard against the first — the certainty that experiment will ultimately show up the falsity of bad reasoning — that constitutes the essence of the second. We can contemplate with equanimity a temporary disregarding of truth, for we know that truth is great and will prevail, but the means by which its triumph is achieved may now ensure that there shall be no one left to care whether it prevail or not. When Rutherford's early experiments with atoms produced a result quite impossible if atoms were as he had conceived them, he declared that he was as surprised as if he had fired a bullet at a piece of tissue paper and it had rebounded and hit him. Similar misconceptions today, when chain reactions may occur that were not possible in Rutherford's experiments, may cause unimaginably great disasters, and the necessity that the hypotheses on which modern physical experiments are planned shall be scrutinised with the utmost care and freedom from prejudice is thus paramount. In fact, as later chapters will show, it is ignored. All unconsciously, scientists have allowed themselves to relapse into the mental state which science is usually regarded as having displaced — that of imagining how nature ought to behave and then assuming that she does so, instead of examining nature with an open mind and then expressing her observed behaviour in rational terms.

The factor that has made this possible, if one may use metaphorical terms to express the idea more vividly, is the exchange by reason of the cloak of Aristotelian logic for that of mathematics. Both begin with so-called 'axioms' which are conceived in the mind without reference to experience, and their implications are developed into extended systems of thought which necessarily follow from the axioms but may or may not correspond to what can be observed in nature. For example, it was a mediaeval axiom that all celestial bodies moved in circles or in orbits that could be analysed into circular movements. This had nothing to do with observation: it was assumed before any regard was paid to observation of the actual movements of the bodies, and when those movements were observed it was regarded as a necessity to analyse them into circles of which their obviously quite different paths were the resultants. The essence of the scientific approach, applied to this particular example, consisted in taking the *observed* movements as the starting point, and expressing them in the simplest terms, without restriction to any preconceived notions of what those terms should be.

I shall consider in more detail in Chapter 6 the relation between mathematics and physics, but the matter is so fundamental for our present considerations that some preliminary remarks on it are desirable here. It was particularly Galileo who realised that mathematics provided the most effective terms in which to express physical observations, and it was he who contributed most to the introduction of those terms into science. The book of nature, he wrote, 'is written in the mathematical language'. But there are two things that should be said about this oft-quoted aphorism. The first is that 'nature', or 'the universe', as Galileo conceived it was a much more restricted concept than that which we hold and that with which modern science is concerned. It comprised only what we study in mechanics; all other phenomena — sights, sounds, smells, etc. — belonged in his view not to the external world but to the observing subject, and it was not at all his idea that mathematics played the all-comprehensive role in science that it is nowadays often assumed to do. Secondly, a language is a medium for expressing ideas, and it is just as capable of expressing false ideas as true ones. The fact, therefore, that something can be expressed with rigorous mathematical exactitude tells you nothing at all about its truth, i.e. about its relation to nature, or to what we can experience.

The most dangerous intellectual error of modern science, with which this book is concerned, lies in the fact that this has been overlooked. Mathematics is an immensely more powerful tool than the Aristotelian syllogism, and its use as a language in which to express the facts of experience has been so successful that the idea has crept unperceived into the minds of physicists that whatever it says must be true. This is openly expressed in the statement already quoted, that everything that is not mathematically forbidden is necessarily observable. Accordingly the habit has developed of assuming that a physical theory is necessarily sound if its mathematics is impeccable: the question whether there is anything in nature corresponding to that impeccable mathematics is not regarded as a question; it is taken for granted.

The fact is, however, that mathematical truths are far more general than physical truths: that is to say, the symbols that compose a mathematical expression may, with equal mathematical correctness, correspond both to that which is observable and that which is purely imaginary or even unimaginable. If, therefore, we start with a mathematical expression, and infer that there must be something in nature corresponding to it, we do in principle just what the pre-scientific philosophers did when they assumed that nature must obey their axioms, but its immensely greater power for both good and evil makes the consequences of its misapplication immensely more serious.

There are so many instances, even in the most elementary uses of mathematics, in which its indications are obviously false, that it may seem strange that this fact is almost automatically overlooked in the more advanced uses of the tool. But there is a universal tendency, not only in science but in everyday life as well, to pay exaggerated attention to predictions that are realised and to ignore those that are not. If, on say three occasions in a week, we dream of something unusual which happens later to occur, there is a very strong pre-disposition to believe that the dreams and the occurrences are directly related, notwithstanding the thousands of instances of dreams, apparently of the same general type, that are *not* realised. In somewhat the same way, although almost all mathematical solutions of a physical problem give both true and false results, we habitually accept the former as valid and pay no attention at all to the latter, when we are working in fields of experience where our existing knowledge is sufficient to enable us to distinguish them at once. Here is an example which I gave in a broadcast talk a short time ago,[4] to which I shall revert later:

> Suppose we want to find the number of men required for a certain job under certain conditions. Every schoolboy knows such problems, and he knows that he must begin by saying: 'Let x = the number of men required.' But that substitution introduces a whole range of possibilities that the nature of the original

problem excludes. The mathematical symbol, *x,* can be positive, negative, integral, fractional, irrational, imaginary, complex, zero, infinite, and whatever else the fertile brain of the mathematician may devise. The number of men, however, must be simply positive and integral. Consequently, when you say, 'Let x = the number of men required,' you are making a quite invalid substitution, and the result of the calculation, though entirely possible for the symbol, might be quite impossible for the men.

Every elementary algebra book contains such problems that lead to quadratic equations, and these have two solutions, which might be 8 and - 3, say. We accept 8 as the answer and ignore - 3 because we know from experience that there are no such things as negative men, and the only alternative interpretation — that we could get the work done by subtracting three men from our gang — is obviously absurd. But what right have we to reject - 3? Clearly, none at all if we accept the substitution: 'Let x = the number of men required.' If we have proved that 8 is the answer, then with the same inevitability we have proved that - 3 is the answer; and if we have not proved that - 3 is the answer, then we have not proved that 8 is the answer. The two solutions stand or fall together as soon as we allow mathematical symbols to represent facts of experience. Yet the inexorable fact is that one answer is true and the other false.

Now in this example it is experience alone that distinguishes the true from the false solution. We cannot prove by pure reason that there cannot be creatures who, with regard to the qualities here considered, can be interpreted as negative men; we know from experience alone that they are as unreal as centaurs. If the problem had been one concerning charges of electricity, of which there are two kinds which we call positive and negative, it might have led to the same equation, and then both solutions would in all probability have been true. There is nothing intrinsically impossible in the existence of negative men, any more than in the existence of black swans: experience alone enables us to reject the solution - 3 as false.

But it is possible to obtain perfectly valid mathematical solutions of a problem which we can see without experience to be physically false because the physical interpretation requires what can be seen without experience to be impossible. Here is an example. Suppose we have a cubical vessel whose volume is 8 cubic feet, and we wish to find the length of one of its edges. Now physically what we are asking is the reading of a standard measuring rod when it is placed along the edge. But suppose there is no such rod handy. That does not matter, for we can solve the problem by mathematics. We let x be the required length, and all we have to do is to solve the equation, $x^3 = 8$. But this equation has three solutions, viz. 2, $\sqrt{(-3)} - 1$, $-\sqrt{(-3)} - 1$ — all having the same mathematical validity. But we know that the only one of these solutions that can possibly correspond to the reading of a measuring rod is 2, because of the necessary properties of measuring rods, which we should understand even if we had never made or seen one. We *might* one day discover negative men, but we cannot conceivably discover a standard measuring rod that can read $\sqrt{(-3)} - 1$ because, owing to the accepted standards of measurement, such an object would not be a measuring rod. So we just ignore two of the mathematical solutions, and quite overlook the significance of that fact — namely, that in the language of mathematics we can tell lies as well as truths, and within the scope of mathematics itself there is no possible way of telling one from the other. We can distinguish them only by experience or by reasoning outside the mathematics, applied to the possible relation between the mathematical solution and its supposed physical correlate.

Now it is this latter kind of reasoning that — according to the argument outlined in the Introduction, to which I can get no answer and which seems to me plainly unanswerable — invalidates the special theory of relativity. The problem here is to find the relation between the rates of two exactly similar standard clocks, A and B, of which one is moving uniformly with respect to the other, on the assumption that the motion is indeed truly relative, i.e. that there is no justification for ascribing it to one rather than to the other. Now this is a problem that can be solved mathematically, and we find that there are two solutions, known technically as the 'Galilean transformation' and the 'Lorentz transformation'. According to the first the clocks work at the same rate, and according to the second they work at different rates. The special theory of relativity regards the second as true and the first as false; the usual expression is that 'a moving clock runs slow'. But, as we have said, it is a condition of the problem that *either* clock can be regarded as the 'moving' one, so this second solution (subject, of course, to the truth of the postulate that the motion is truly relative) requires equally that A works faster than B and that B works faster than A, and just as we know enough about measuring rods to know that they cannot read $\sqrt{(-3)} - 1$, so we know enough about clocks to know that one cannot work steadily both faster and slower than another. Hence, without in the least rejecting the Lorentz transformation as a *mathematical* solution of the problem, we can say at once that it is not a possible *physical* solution. Nevertheless, in modern physics it is universally assumed to be so, on the sole ground of its mathematical validity.

How such an obvious error could have occurred and escaped immediate recognition is explained in Part Two, but it may be said at once that the apparently simplest way of exposing it — by setting two clocks in relative motion and observing their rates — is impracticable because the difference which the theory requires is too small to be detected except at velocities far too high to be yet attainable. Experiments have been made in which elementary electrically charged particles (conceptual bodies, such as electrons, protons, etc.) have been used instead of clocks, and observations of what have been regarded as their 'rates' have been made, and these have shown that such 'rates' differ for particles which, according to electromagnetic theory, have vastly different velocities. These observations have been held to constitute an experimental proof that the Lorentz transformation is a physically valid solution of our problem. But there are two reasons why this argument fails. In the first place, even if it be fully granted, it shows only that one 'clock' works more slowly than the other — which would be quite possible if the motion of each was absolute, as Lorentz showed before Einstein's special relativity theory appeared. If the motion is relative, however, and the Lorentz transformation is a valid solution, then also the second 'clock' must work more slowly than the first — and this, it need hardly be said, has been left unproved. The second reason for the failure of the argument is that the interpretation of the particles as 'clocks' and of the observed phenomena as their 'rates', and the assumption that they move with velocities, ascribed to them (it is, of course, quite impossible to observe them; their existence and properties have all to be inferred on theoretical grounds) depend on the truth of a theory that itself depends on the truth of the Lorentz transformation (this is explained in Part Two), so the argument is circular: the observation proves the physical truth of the Lorentz transformation only if we first accept a theory which itself requires that transformation to be physically true.

An *experimental* test of this requirement of the special relativity theory is therefore at present impracticable, and the claims often advanced that such a test has been made are spurious. But surely, one does not need an experiment to prove that one clock cannot at the same time work both faster and slower than another. And this brings me to the most serious aspect of this whole matter. How is it possible that such an obvious absurdity should not only have ever been believed but should have been maintained and made the basis of almost the whole of modern physics for more than half a century; and that, even when pointed out, its recognition should have been universally and strenuously resisted, in defiance of all reason and all the traditions and principles of science expressed by Sir Henry Dale in the statement quoted at the beginning of this chapter?

This question has two aspects, an intellectual and a moral one. Both are astonishing, but of their reality and profound importance there can be no question. The former is the less difficult to understand, though it needs a careful survey of the history of the subject to make it credible: this I attempt in Part Two — necessarily less completely than is desirable, but sufficiently, I hope, to show that what appears patently absurd in one context may present quite a different semblance in another, and to explain how the special relativity theory came to be accepted in spite of its contradictions (disguised as 'paradoxes') in the early decades of this century. After all, it was not so very long ago that men of the highest intelligence believed that Moses wrote the account of his own death recorded in the Pentateuch. But the more serious lapse is the moral one, not only because of the intrinsically greater seriousness of a moral as compared with an intellectual fault, but also because the nature of science itself does not ensure its eventual correction as it does when the mistake is intellectual. When Dale wrote of the unflinching fidelity of science to the answers which nature gives to its questions, he took it for granted that those answers would, in the long run, be unmistakable, and the contribution that science had to offer to civilisation lay in the moral sphere, in its acceptance and publication of those answers, at whatever cost to expectancy and without prejudice or preconception of any kind. It is in the failure of present-day science to live up to Dale's ideal in this respect that, notwithstanding the incalculable physical danger involved in the intellectual error, lies the ultimate offence. That is so, not only because fidelity to truth for its own sake is ultimately more compulsory than that for the sake of physical well-being (if that is disputed I shall not argue the question), but also because the loyalty of science to truth has a far wider relevance than that exhibited in the matter of special relativity alone, wide though that is. In an age in which science has begun to play a dominant role, quite beyond the control or even the comprehension of the non-scientific citizen, the whole future of civilisation is dependent on the absolute unqualified fulfilment by scientists of their moral obligations.

That, I repeat, is why this book has become necessary. It is evident to me, in the fact that the simple question that I have put has remained unanswered while experiments continue on the assumption that the single sentence required to answer it can be withheld with impunity, that science has failed to accept nature's answers humbly and with courage and to give-them to the world with unflinching fidelity. However, I cannot rest content with my own judgment in such a matter. I shall simply relate the course of events, asserting nothing for which I have not complete objective evidence. If on occasion it seems necessary to insert comments of my own, it will be perfectly clear that they represent my own judgment and not objective facts. I then leave the reader to judge for himself what conclusions are to be drawn from the facts. Whatever they may be, I think it is unquestionable that the public has a right to be informed of what is actually occurring in a matter that concerns it so vitally, and, as will be seen, this is my only means of informing it. I begin, then, with the moral aspect of the matter, presenting it in narrative form, and necessarily, the story being so long and involved, omitting many minor details which do not modify the general import. The intellectual problem is reserved for Part Two.

2

The Origin of the Controversy

It will, I think, help towards a general understanding if I begin with a brief picture in outline of the world of physical science as it is today, for the sake of those unacquainted with it. In accordance with what I have said at the end of the preceding chapter, I must add that this is my own description, but I do not think its essential accuracy will be questioned by anyone familiar with the situation. In any case its effect can only be to illuminate, and not to distort, the account of the facts which is to follow.

The world of physical science today cannot be defined with precision. It includes, of course, pure physicists and almost all astronomers, as well as many chemists and even a few biologists, though we may leave these out of consideration without affecting the general discussion. I shall refer to all these, for brevity, as 'physicists' or 'physical scientists'. When the editor of *Nature* wrote in a leading article (reproduced here in the Appendix) that 'the special theory of relativity has been enormously successful in the past half-century, and in spirit as well as in detail has come to pervade the whole of modern physics', the subjects which he included in the term 'modern physics' can with truth be said to include the work of all these scientists.

Within this field we may make a general division into two classes which I will describe as *experimenters* and *mathematicians.* There is a little overlapping, of course, and the designations arc broadly descriptive rather than meticulously exact, but the impression they give is a true one: practically everyone in whose work special relativity plays a significant part would be assigned without hesitation to one or other of these classes, and his inclusion would not be challenged. Mathematicians, in the strict sense of the word, may differ as to whether so-and-so should be called a pure or an applied mathematician, and the work of some of both kinds is mainly independent of special relativity: moreover, scientists who do experiments may indulge in mathematics at times. But in relation to the present problem the distinction is unambiguous.

Now, still keeping to my own generalisation from the whole of my experience — the reader may judge for himself how it applies to the examples which I shall give, though he cannot, of course, confirm my statement that they are truly representative — the reactions to my question from the 'experimenters' are, with almost [1] complete unanimity, either that they do not understand the theory, at all, although they assume it in their experiments, or else that they regard it as nonsensical; they take it to be true, nevertheless, in their experiments which depend on it with various degrees of directness, and justify this procedure on the ground that the theory has been tested by those who understand it (i.e. the 'mathematicians'), and therefore all questions about it should be passed to them. So long as the 'mathematicians' declare themselves satisfied that the theory is trustworthy, the 'experimenters' are satisfied to go on using it. The 'mathematicians', on the other hand, either refuse to say anything at all in answer to my criticism, or else reply in terms of politeness, mysticism, irony, (these terms arc roughly in descending order of age of the person in question) or other quality, with this alone in common, that they do not answer the question asked. In the meantime, complete trust is placed in the theory by almost everyone, and experiments proceed as though it had never been questioned.

This may seem to the uninitiated so incredible, in view of the popular image of the scientist which corresponds to Dale's description — I know from experience that it does so appear — that I am forced in my examples (which, however, will not include the least reputable ones) to give the names of those whom I quote. I do this with the greatest reluctance, but it is clearly necessary. General statements about 'modern scientists' would inevitably, and rightly, be dismissed as unconvincing. Only when those statements arc known to proceed from scientists of the highest reputation, and given in their own words, can they possibly carry the conviction that the circumstances demand. I hope it will be believed that it is this consideration alone, and nothing of personal feeling, that forces me at last to take the course which I have shunned for so long.

Another generalisation, which I think has an important significance not only for the question of the truth or falsity of special relativity but also for the matter of the education of scientists, is this: the readiness to respond to my criticism decreases steadily with increasing distinction of those who read it. The leaders in the subject reply, if at all, only when pressed, and as briefly as possible. Those of intermediate status cite experiments of greater or less irrelevance or present calculations of greater or less complication and with no relevance at all. Students and young Ph.D.s are vociferous. 'After your argument in *Nature* with Professor Max Born' (see p. 42), wrote the former editor of *Nature* to me in 1963, 'I had a large number of communications and quite a number of individual unannounced visitors at *Nature* office. As you implied in your letter they each one felt that he could prove you were wrong in your view and each one got about it in a different way... all the people who submitted communications or wished to discuss this problem with me, could scarcely be considered first class men of science as compared with Max Born.'

I think this shows how, even in science, what at its beginnings is recognised as a speculation, with greater or less plausibility, develops with time into a compulsory dogma, which whosoever disbelieves thereby brands himself as an ignorant fool. To exaggerate slightly, but not to distort, it is a microscopic reproduction of the macroscopic development of the place of hypotheses in science from Newton to Eddington which was noted earlier. But, without attempting to relate it in detail to the present generalisation, I will record one instance for the reflection of the reader in this connection. Some years ago Dr. W. Cochran, now Professor of Physics at Edinburgh, but then a lecturer on relativity among other things at Cambridge, offered a prize of 10 pounds to the member of his class who wrote the best refutation of my criticism of special relativity. The winner sent me a copy of what he had written. It is not worth recording, but Professor Cochran himself has refrained from publishing what *he* considers a refutation, though later, in his capacity as editor of *Science Progress,* he rejected a paper of mine on the subject, sent to that journal for publication, the only reason given to me being that its conclusions were at variance with those of some calculations which he made, which were unrelated to the contents of my paper.

Let me now, after this preamble, record in outline the history of this matter. It began with a revival of an old problem, known as the 'clock paradox' or 'twin paradox', which dates from the early days of special relativity. I shall deal with this more fully in Chapter 9, so a brief description will suffice here. One of the earliest deductions from the theory was that if a traveller sets out from the Earth at a high speed and later returns, he will have aged less than his twin brother who has remained at home, because 'a moving clock runs slow' and the physiological processes of a man are equivalent to a clock. But equally, according to the theory, it is the Earth that might be regarded as having moved while the 'traveller' has remained at the same place, in which case the Earthbound twin would be the younger at the end of the process. These results obviously cannot both be true. In 1955 I adverted to this problem as a result of reading Sir George Thomson's book, *The Foreseeable Future,* in which it was stated that, according to the most authoritative view, the former result was correct and the latter therefore incorrect. In an article in *Nature*1 I claimed that the twins must necessarily age at the same rate because it was an essential requirement of the special theory of relativity, which I then believed to be sound, that no observation was possible that would enable one to ascribe the motion preferentially to either twin.

I need not here describe the course of the ensuing discussion, for all that is necessary will be said later. I mention the controversy here because it was the origin of my realisation that the special relativity theory (which, as I have said, at the beginning of that discussion I believed sound) was impossible: it made me see that the theory required that the twins would age both at the same rate and at different rates, which is clearly contradictory. My first presentation of the contradiction appeared in the *Bulletin* of the Institute of Physics[2], which hardly constitutes publication since that is a journal issued only to members of the Institute — admittedly numerous and including many of the most distinguished physicists — and not generally available in libraries. I there expressed the contradiction, not in terms of clock readings, which Einstein had considered in his first presentation of the theory, but in terms of readings of space-measuring rods, and I showed that the theory required each of two such rods to be shorter than the other. I also ventured some speculations on electromagnetic aspects of the theory, which I should have been wiser to have left for further reflection after the kinematical question had been settled.

The immediate consequences were more evident in private correspondence than in printed discussions. They showed such failure to meet the essential point that (very shortsightedly, **as** I now realise) I tried to deal one by one with the purely incidental points that were raised and to show that they did not meet my criticism. I should, of course, have ignored them and pressed my correspondents to answer my argument instead of side-stepping it. During 1960 I published papers — in *Science Progress*[3]*,* *Philosophy of Science*[4] (an American journal), the *British Journal for the Philosophy of Science*[5]*, on* various aspects of the matter — all, I believe, in essence sound but, from a tactical point of view, untimely since they allowed the essential contradiction in special relativity to escape attention. Being a poor psychologist, I did not realise that scientists, like other people, are far more ready to search for flaws in other people's reasoning than to eliminate prejudices from their own, and I remained in a state of bewilderment at my inability to make clear to others what seemed so obvious to me. Had I, from the beginning, pressed for an answer to the question put in the Introduction to this book, and refused to be diverted into other aspects of the matter, the question might by now have been settled. But crying over spilt milk is a useless occupation.

However, in 1961, in a dialogue with the late Viscount Samuel[6] dealing with this among more general matters, I gave at some length an account of the difficulties I had had in getting any attention paid to my criticism of special relativity, notwithstanding the fundamental importance of the matter, and I gave in an Appendix a different form of the proof that the theory contained a contradiction. It would be superfluous to repeat here what is said there (pp. 70-75). It will suffice to say that it records attempts to get publication for my criticism through the Royal Society, the Physical Society and the *Philosophical Magazine,* which were all rejected for reasons which the reader of the dialogue can evaluate for himself; and that not a single reviewer of the volume mentioned that the question had even been raised. I pass to a letter published in *Nature* of 8 September 1962,[7] in which, after calling attention to the importance of either accepting or refuting my criticism, I quoted verbatim from Einstein's paper his proof that, according to his theory, a 'moving' clock, B, worked more slowly than a 'stationary' clock. A, and then gave, in exactly the same form, a proof that, in exactly the same circumstances, clock A worked more slowly than clock B. 'The conclusion of the first passage', I wrote, 'is that each reading of B is behind the corresponding reading of A, and that of the second passage is that each reading of A is behind the corresponding reading of B.' Applying the result to a particular case, I concluded: 'Hence, when B reads 6, A reads both 12 and 3. That is a contradiction. To avoid this outcome it must be explained not why the two cases are different — that is obvious — but *why, consistently with the theory, the former result must be accepted as true while the latter must be rejected as false.*

This brought a host of replies, from correspondents of all degrees of distinction in the subject except the highest, which had only one representative — the late Professor Max Born. The bulk of the letters were sent to me by the editor to deal with collectively, but Professor Born's letter was printed separately with my reply to it. The general article is given in *Nature* of 30 March 1963, which contains also Born's letter and my reply just mentioned[8]. I need not summarise the former, which may be looked up by anyone interested in the general sort of reaction to my very simple argument, but I must comment on my correspondence with Born, who was for many years until his death an honoured and much loved friend of mine, because of his exceptional sense of responsibility in replying to what others of comparable distinction in the subject had ignored, though it must be left to the reader to judge whether, in the end, his reaction met the full needs of the case. 'Prof. H. Dingle', he wrote, 'has sent me a reprint of his communication published under the above title' [Special Theory of Relativity] 'on p. 985 of *Nature,* September 8 1962, with the handwritten remark: "With kindest regards. Test case for the integrity of scientists". Though former experience has taught me that discussing relativity with Dingle leads to no agreement I have to answer a challenge which is directed against the "scientific integrity" of myself and of others.' He then proceeded to discuss the problem, and I will quote that part of his letter which bears directly on the main point. It may be verified, by reference to the original, that I have omitted nothing that modifies the significance of the quoted part, but since Part One of this book is designed to be entirely non-technical it is necessary to exclude everything that would tend to frustrate that intention, and I can assure the reader that I am giving the full essence of the matter, in so far as it concerns the central question whether my criticism has been met or not. Dingle, writes Born[8],

> quotes a passage from Einstein's paper, the first paragraph of which ends with the question: *'What is the rate of this clock, when viewed from the stationary system?'* ... Dingle now proceeds in this way: 'And here is the passage leading to the opposite conclusion'. The first paragraph of this new passage is completely identical with that of the original including the last sentence just quoted (in italics)... The mistake is in the first paragraph quoted above (in italics); it should read, in the two cases:
>
> *1st case,* clock at rest in *k*: *What is the rate of the clock in k, when viewed from the 'stationary' system K?*
>
> *2nd case,* clock at rest in *K: What is the rate of the clock in K, when viewed from the 'moving' system k?*

Born then proceeds to answer his own questions, shows that they do not lead to a contradiction, and concludes: 'Dingle's objections are just a matter of superficial formulation and confusion.'

I need not record my reply, which can be seen in *Nature* immediately after Born's letter, because I think it is obvious at once that it is no answer to a criticism to say that the critic should have asked questions which he did not ask, and charge him with 'superficial formulation' because of his omission. The question which Born calls my 'mistake' is not mine; it is Einstein's. What I showed was that it had two mutually contradictory answers, equally authentic, of which Einstein had given only one which had been accepted as uniquely valid.

I believe that if Born had allowed himself to entertain the possibility that the theory might be wrong, he would have had the greatness of mind to see that it was and to have admitted the fact. Unfortunately, however, it was no longer possible for him to conceive that possibility. Although I sent him an offprint of my reply (he was then living in retirement in Germany and no longer following the English press), he would not read it.

> I am completely fed up with the matter [he wrote], I don't know what you have answered to my note. As I think my argument irrefutable, I am convinced that you have made again some elementary mistake... I am sorry that I have to say

> such words to a man so kind and friendly as you are. But as I am over 80, the time left to me is too short to waste it on such futile discussions.

I will add only that I am equally sorry to have to say such words about a man so kind and friendly as Max Born was, but the matter is too deadly serious to leave them unsaid. The reader must form his own opinion on the matter.

On reflection several years later on the course which this controversy has taken, I realise that, in my ignorance in the earlier stages of the degree to which conviction of the final truth of special relativity had displaced, in the minds of physicists, the openness indicated in Dale's description of Science, I took the less effective of two possible courses. I could have put my criticism of the theory in the form of a statement and invited critics to find a flaw in it; or I could have pointed out that the theory left open a question and asked for an answer to that question. Put more specifically, I could have pointed out that the theory contained a contradiction — that it required each of two clocks to work faster than the other — or asked the question: how does one tell from the theory which clock works the faster?

The difference seems slight, but its effects have been wholly different. Unfortunately, throughout most of the controversy I took the first course, and that opened a way for all sorts of spurious 'faults' to be found in my statement — quibbles over words, and so on: for instance, those of Born just mentioned, and the innumerable others to which the then editor of *Nature,* Mr L. J. F. Brimble, referred (p. 39); a notable example is that given in the Appendix, in which it will be seen how Professor McCrea smothers the simple passage given in my article, to which I asked for exclusive attention to be given, by entirely superfluous comments including a space-time diagram, to dispose of a consideration that had not been raised except by himself.

As soon as I took the other course, however (the asking of a question), the effect was completely opposite; instead of bringing on myself a flood of discordant 'refutations' I was met by complete silence. This is illustrated by what followed my letter to *Science* (p. 81); by the successive failures of the present editor of *Nature* to fulfil his repeated promises to deal with the matter in an editorial (p. 90); and by a reply from Professor Bondi (a well-known mathematical authority on relativity, who, as Chief Scientific Adviser to the Ministry of Defence, may be held to have a special responsibility in such a matter) who simply wrote:

> It is kind of you to invite me to participate through adding a reply, but I do not feel able to accept your offer. *In my view my published work (particularly 'Relativity and Common Sense', also 'Assumption and Myth') amply refutes your views.* I do not think I can usefully add to what I said there; I am only sorry that you do not find it convincing.

The sentence I asked for would have occupied less space than this.

I think the difference is most instructive — a deluge of evasive replies in one case and total silence in the other. It will greatly assist the full appreciation of what follows if I give here the two forms of criticism of the theory in what seem to me to be the terms in which they can best be understood and, if possible, answered: I call them 'The Argument' and 'The Question' respectively.

THE ARGUMENT

According to the special theory of relativity, two similar docks, 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.

Since I wish in this book to concentrate on The Question, and let the reader judge the cogency of any answer that may be offered (none has been offered yet), I put this in more extended form, to anticipate as far as possible comments which are not answers; but I think it will be realised that The Question could have been put as briefly as The Argument (it is in fact summarised in the Introduction) and that a valid answer would be equally brief.

THE QUESTION

According to the special relativity theory, as expounded by Einstein in his original paper,[9] 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?

A single sentence would be sufficient for an answer, and such a limitation is highly desirable to prevent obscuration of the essential point by irrelevant considerations. To guarantee its relevance it should be applied to justify Einstein's choice.

Failing an answer the theory clearly becomes untenable, for, as Professor J. L. Synge has said after long consideration (p. 77), either the theory or the conception that a regularly running clock cannot work both faster and slower than another must be abandoned.

The remainder of this book is concerned with The Question and the attitude of the scientific world to it. That being understood, I resume the story and describe what followed my exchange of letters with Born.

3

Reactions to Criticism

This discussion in *Nature* naturally stimulated a large amount of correspondence (unpublished) from persons of various degrees of distinction and qualification in the subject. Some of the more important parts of this will be more fittingly dealt with later, but I record now the outstanding revelation which the whole matter had forced on me, namely, the almost complete failure of scientists to recognise the distinction between the question whether the theory was right or wrong and the moral obligation to approach that question without prejudice. I had been prepared for objections on scientific grounds to the arguments which I had advanced: I had not been prepared for the lapse from scientific integrity which the various evasions and distortions of the simple point at issue showed to be so general, and it became clear to me that this was an even more serious matter than the tenability of special relativity itself. For, in an age in which science had acquired such power over a public which was of necessity quite unable to exercise any control over its quite incomprehensible operations, complete integrity was a prime necessity, and unless that was preserved the consequences might be inestimably disastrous. Accordingly, in the autumn of 1963 I sent the following letter to *Nature,* with reasons for its publication; it bore the title, 'Scientific Integrity':

> The purpose of this letter is not to discuss a scientific question but to call attention to a decline in standards of scientific integrity which, unless it is checked, may have grave consequences. The letter is addressed to all to which this is a matter of concern, regardless of their qualification to pass judgement on the particular scientific questions involved.
>
> I have recently, on three successive occasions,[1] given a proof that the special relativity theory is false. This was universally ignored until, on the last occasion, attention was practically forced to it, whereupon only one answer from a recognised authority was forthcoming,[2] and that confessedly changed my statement into another which it was not difficult to expose as invalid. My proof has accordingly been ignored, and the theory remains at the basis of current experiments and predictions.
>
> In addition to the profound implications of this result for theoretical physics and philosophy, there is the undeniable fact that modern physical experiments are of such a character that an error in theoretical expectations might have the most dire consequences. Should a disaster occur at an atomic energy establishment, the cause might be undiscoverable, but there can be no doubt of the public reaction to the knowledge, which would undoubtedly then transpire, that a clear warning of the possibility of such an event had been repeatedly given, side-tracked if noticed at all, and altogether unheeded; and such reaction would be entirely justified.

This is not a unique case. Before I realised the untenability of the special relativity theory I was engaged in a vigorous controversy on the question whether relativity theory entailed the possibility of 'asymmetrical ageing', e.g. of postponing the date of ones' death, almost without limit, by high speed travel. It was, and still is, generally believed that relativity demands this possibility. I reduced to a single syllogism a proof that the *postulate* of relativity (which is part of the basis of the *theories,* special and general, of relativity) not only did not require asymmetrical ageing but absolutely forbade it. [3] I pointed out that the question could be settled immediately by the location of my error, if I had made one, in a particular one of the three elements of the syllogism. The next contribution to the discussion[4] began: 'May I suggest an alternative approach to this problem...’: no notice was taken of the syllogism. Why 'an alternative approach' when the problem had already been reduced to the simplest possible terms? Yet so it has gone on: new approaches galore, but complete absence of any answer to the syllogism. The very few comments that have been made on it, of which I have given samples elsewhere,[5] can at once be recognised as merely fatuous. I repeated the syllogism at least three times,[6] but without eliciting a single answer. Yet it continues to be asserted, without qualification, that asymmetrical ageing is an established consequence of relativity theory.[7]

The most fundamentally serious aspect of this state of affairs lies not in the answers to the scientific questions involved, but in the fact that reason has been jettisoned and prejudice substituted for it. If I am right, the asymmetrical ageing controversy shows that the relativity theory is generally misunderstood, and the later controversy shows that it is wrong, and the seriousness of this can hardly be exaggerated; but even more menacing is the attitude, unmistakably revealed, which allows untested theories to be accepted as established truths and criticism of them to be bypassed instead of directly faced. Yet that is the prevailing attitude in mathematical physics today.

I write this with great reluctance and after long hesitation: I would far rather have adopted a course, had such been possible, unattended by the risk of creating sensation and endangering friendships which I value highly. But I now sec no alternative. For eight years, in world-wide private correspondence and such published matter as I have succeeded in getting accepted, I have stressed the realities of the situation, and the consistent and universal response has been evasion, suppression, and even, on occasion, falsehood, and that from the highest quarters. For the greater part of that time I have been fully aware of the peril in which we stand, but I have kept hoping that continued insistence on the pure logic of the matter would suffice to awaken some mathematical physicist (no one else can command an effective hearing in these matters) to the possible consequences of working with a misconceived theory. It is now clear beyond doubt that that hope was illusory, and I have no right to nurse it any longer — if, indeed, I have not already done so unwarrantably long. The fact must be plainly stated that, in a situation in which the safety of the world lies in the hands of a comparatively minute body of men whose activities are necessarily so abstruse as to be

> altogether beyond the comprehension of the vast majority, the obligation that rests on them to honour unreservedly the traditional scientific principle of utter subservience to truth and rejection of prejudice is one of which they are quite unaware. This needs no scientific knowledge for its verification; the references I have given, though far from telling the whole story, will make it clear beyond question to anyone, whether physicist or not, who cares to examine them. He will see the reiteration of my syllogism, but he will find no answer to it. He will see that the only authoritative answer to my thrice published disproof of the special relativity theory openly changes it into something else and then answers that.
>
> I do not imagine that those who behave in this way are fully conscious of what they are doing: the fact is simply that the *sine qua non* of true scientific research — the ever-present consciousness that the demands of no theory, however successful, must be allowed to qualify those of fact and reason — has silently faded away. The automatic reaction to criticism is not to face it but to look elsewhere for some independent justification for ignoring it. The depth to which we have descended is exposed with Gallic frankness by one ardent believer in the relativity theory, H. Arzelies[8], who asserts that criticisms of that theory are symptoms of mental abnormality and that to treat them seriously is a waste of time. That this — though not usually so candidly acknowledged — truly describes the general attitude I have overwhelming evidence, and until it is brought clearly to light and ruthlessly transformed our peril is inestimable. In these circumstances I can do no other than to bring the whole matter before those whose influence is greater than mine — perhaps some biologist who is still capable of perceiving a distinction between mathematical consistency and physical necessity — in order that the deterioration may be arrested before it is too late.

The then editor, the late Mr L. J. F. Brimble, replied courteously, but would not consent to publish this letter. Like my correspondents in general, he seemed unable to distinguish the moral from the scientific question. 'I do not think,' he wrote, 'any useful purpose would be served by publishing this form of communication in *Nature.* I have had a very large number of Letters since your original publication dealing with special relativity, and have had to reject them since there are such demands on space in *Nature;* in any case, as you have already yourself stated, most of these authors are not very outstanding in their fields of research.' A further attempt to convince him that the point of my letter was not the status of special relativity but the title which I gave it — "Scientific Integrity" — likewise failed. 'As you are aware', he wrote, 'for a number of years now this question of special relativity has been raised time after time... As you can well imagine, it is impossible for *Nature* to publish this apparently incessant correspondence which invariably seems inconclusive. Moreover I do not feel disposed to challenge the integrity of scientists in the columns of *Nature* without much further evidence than I have at the moment.'

It then seemed to me that my most promising course would be to write to scientists of distinction who, for one reason or another, would be expected to appreciate the ethical side of the question more vividly than the average scientist, especially so if their work was not directly related to relativity. Names which occurred to me were those of Sir Robert Robinson, O.M., former President of the Royal Society (who, rightly or wrongly, it had been represented to me was dissatisfied on other grounds with the ethical conduct of modern scientific research), Sir Julian Huxley, Professor C. A. Coulson of Oxford, and the late Professor (later Dame) Kathleen Lonsdale. I therefore wrote to Sir Robert Robinson after the rejection of my letter by *Nature,* describing the situation, mentioning that I had found it impossible to persuade any physicist of repute (other than Born, who had misread it) to say a word about my criticism, and adding: 'I have wondered whether, as a last resort before raising an unholy row, you would be prepared to use your influence to persuade some mathematical physicist who himself has influence in these matters, really to rid his mind of prejudice and read my reasoning without the presupposition that there *must* be a flaw in it; and then, if he can't find one, to have the guts to say so openly and prevent any further risks being taken.' Sir Robert replied as follows:

> I started to read your letter with considerable apprehension because I thought you might be asking me to express some opinion on a matter which is quite beyond my comprehension. I have read your letters and the proposed Letter to 'Nature' very carefully and am quite clear that your demand for discussion and attempted refutation is absolutely just and must be met.
>
> This is only a note acknowledging yours and I will in the meantime see what I can do by either getting a statement that no one is sufficiently interested to discuss your ideas, or by finding somebody with sufficient authority in the field who will do his best to try and understand your point of view and make some pronouncement.
>
> You have given me an exceedingly difficult task but my blood will certainly boil if I am unable to do something about it.

This was encouraging, for it showed that one scientist at least of universally acknowledged distinction was still aware of the ethical obligations of scientists, and I had some hopes of a successful issue. Unfortunately they did not materialise. When, about six months later, I met Sir Robert at a social function, he told me that he had tried to induce several scientists, generally regarded as authorities on relativity, to answer my criticism, but had failed; not one of them would do so.

I shall mention later the results of my appeals to Professors Coulson and Lonsdale, although they came before that to Sir Julian Huxley which, since it can be dealt with more compactly, I will next describe. After setting before him the general position which I have already outlined, and being not unacquainted with the attitude of Thomas Henry Huxley to the ethical aspects of science, I wrote:

> I see no way of getting the situation rectified except by enlisting the help of someone with influence, who is concerned that the principles of strict scientific inquiry shall be observed and not merely preserved as a tradition, and who is also

sensible of the responsibility of scientists to the public. I therefore write to ask you what, in the circumstances, you think it best to do. Would you, for instance, think it fitting to write to *Nature* as a non-specialist in the subject but one who perceives its possibly very serious connotations, saying that the correspondence with Max Born has been left in an unsatisfactory state, and asking that some authority on relativity should either give a clear answer to the point I raised or else acknowledge that it is unanswerable and that therefore its conclusion, with all its necessary implications, must be accepted and acted upon? Such a letter from you could hardly be refused publication. But I do not wish even to appear to dictate any particular course, but would rather, having set out the bare essentials of the position as well as I can, leave it to your judgment to advise me as you think best.

Sir Julian replied as follows:

I feel I really cannot intervene in this matter. I am so un-mathematical that I cannot begin to understand your reprints, and I have never even tried to follow the theory of relativity, because I knew I couldn't! However, more important than this, a letter from me to *Nature* would be worse than useless — all the physicists would say 'here is another biologist butting in on something he knows nothing about'. I am sorry not to be more helpful, but I really feel that any intervention on my part would be worse than useless.

I confess that it surprised me to learn that a Huxley should be deterred from urging that the ethical obligations of scientists should be honoured, by the fear of provoking disrespectful irrelevant comment, but the reply showed once more the paralysing effect, on the intellects of even leading thinkers, of the word 'relativity'. I had not asked Sir Julian to comment on relativity, but only to help to ensure that criticism should be met, and not evaded or ignored, yet his immediate reaction was to explain why he had never tried to understand it. The magical influence of this word has transformed science in this field into a superstition as powerful as any to be found in primitive tribes.

I pass over much correspondence with interested persons, and proceed to a second attempt to get my criticism of the theory published by the Royal Society, which would increase the likelihood of its receiving the attention to which any serious criticism of a fundamental scientific theory is entitled. I had already made one (unsuccessful) attempt, as is recorded in my discussion with Viscount Samuel, *A Threefold Cord,* but by the time of which I am now writing I had not only reduced the criticism to a simpler (though not more logically sound) form, but also had obtained, through my correspondence, a clearer idea of both the genuine and the spurious difficulties of those who rejected it. I was thus able to accompany my simplified treatment of the main point by answers in advance to the likely objections.

It may be helpful here to interpolate a note on the function — or one of the functions — of the Royal Society, and the manner of its procedure in dealing with papers submitted to it. In general terms, its aim is the discovery and publication of the truth in scientific matters. At the time of its foundation its purpose was expressed in these words[9]:

> To examine all systems, theories, principles, hypotheses, elements, histories, and experiments of things naturall, mathematicall, and mechanicall, invented, recorded or practised, by any considerable author ancient or modern.... In the mean time this Society will not own any hypothesis, system, or doctrine of the principles of naturall philosophy, proposed or mentioned by any philosopher ancient or modern ... nor dogmatically define, nor fix axioms of scientificall things, but will question and canvass all opinions, adopting nor adhering to none, till by mature debate and clear arguments, chiefly such as are deduced from legitimate experiments, the truth of such experiments be demonstrated invincibly.

There has been no revision or modification of this aim but, of course, practical considerations demand that papers submitted to the Society shall be scrutinised by competent persons before a decision is reached concerning their publication. As may easily be imagined, there is no lack of communications from those whose unrealised ignorance of essential facts invalidates the ideas they present, so a rule has been laid down that papers may be submitted to the Society only by a Fellow, who is recommended, though not compelled, to assure himself before doing so that the paper is worthy of serious consideration. It is then, as a general rule, submitted to one or more referees who report, anonymously, on the character and suitability for publication of the paper, in the light of which information a decision is reached on whether the paper shall be published or not. However, the Society in earlier days was very conscious of the greater danger resulting from rejection of the truth than from publication of error (as I have already pointed out, the very nature of scientific investigation ensures that error must inevitably reveal itself sooner or later), and it made a rule, which is still held to be binding on referees of papers, which requires that 'a paper should not be recommended for rejection merely because the referee disagrees with the opinions or conclusions it contains, unless fallacious reasoning or experimental error is unmistakably evident'. In other words, a paper is not required to prove its innocence; it is held to be innocent unless proved guilty.

It is easy to see how necessary such a rule is, if the basic aim of the Society — the discovery of truth — is to be achieved, for without it the most dangerous of all errors — those universally held — are automatically preserved from discovery. However disinclined a referee may be to accept the implications of a paper, the rule lays on him the duty of pinpointing a specific error in it before recommending its rejection: if he cannot do so, then, however unexpected or unwelcome or revolutionary the consequences and probable effect of the paper may be, he is precluded from recommending its rejection — and therefore, by implication, the Society is committed to the duty of presenting it to the world. I do not know if the Royal Society is unique in this respect, but, as the leading scientific society in this country, if not in the world, and as a body supported by, and so responsible to, the public, its ordinances, if they are strictly obeyed, ensure that anyone who has a contribution to make to the advancement of science has at least one medium through which he can be sure that that contribution can in fact be made. There is only one means by which *this* obviously desirable and originally intended object of the Society may be circumvented: there is nothing to require that a Fellow of the Society shall submit a paper to it, whatever the import of that paper may be. If he does so, the referee is bound, if he fulfils his obligation, to pass it if an error in it is not 'unmistakably evident' — not merely suspected, but clear beyond doubt — but there is nothing to ensure that it shall ever reach a referee. I make no comment on this: I simply point out the fact because of its relevance to much that follows.

To resume the story, then — I wrote a rather detailed paper, setting out as clearly as I could the fundamental defect of the theory and discussing the problems which its abandonment would arouse. I then wrote to Professor C. A. Coulson, Professor of Mathematics at Oxford, whom I chose first because, although his work was not specially concerned with relativity, he was a mathematical physicist, and the work of no such scientist today can be independent of it. The theory had — quite wrongly, I think, but still undoubtedly — been transferred from its proper field of physics to the field of mathematics, and Professor Coulson, besides being highly distinguished in that field, was also well known for his strong adherence to an ethic that places a high value on truth. He replied in the most friendly terms, and not only expressed his readiness to help, but took much trouble to ensure, so far as he conceived it to be within his power, that the paper should receive proper consideration. What, however, he would not do was to read the paper himself or communicate it to the Royal Society for attention. Had he read it he would have seen that it contained nothing that was not fully within the comprehension of his own undergraduates, but, so convinced was he, like practically everyone else not specialising in the subject, that relativity was a profoundly recondite matter — and (I have no doubt) that if my criticism should be such that he could detect no flaw in it, that would in all probability indicate his incompetence in the subject — that he did not feel justified in going further than to try to persuade colleagues who were generally regarded as authorities on relativity to read and, if they then felt able to do so, to communicate my paper to the Royal Society. This he did, but seven months later he had to report that, after several attempts, he had failed to find anyone who would consent to look at the paper. I do not know who the unwilling specialists were, but, knowing the narrowness of the field and the extent of my own unsuccessful efforts to get my question answered, I cannot doubt that, at any rate, most of them were already aware of the problem that would face them if they agreed to communicate my paper — for, being specialists themselves, the duty of recommending it for publication or else making 'unmistakably evident' an error in it would inevitably be laid on them if they did so. Had Professor Coulson himself communicated the paper, he would not, of course, have been called upon as a referee of it since the subject did not lie sufficiently close to his field of work, but he felt that, though permissible, it would be improper for him to do so.

I then approached Kathleen Lonsdale, who was not only a former colleague at University College London but also a close personal friend. Her work also was only indirectly related to relativity, and she shared the general belief in its essentially mysterious essence in even greater measure than most, for it had been presented to her in her student days cloaked in such metaphysical irrelevancies that, being naturally predisposed to ascribe the appearance of nonsense in the instruction of her tutors to her own incompetence rather than to actual fact, she had been rendered unable to hear the word 'relativity' and retain her power of simple reasoning. 'It interested me so little that I forgot it as quickly as possible,' she wrote. Nevertheless, she agreed to read my paper and try to follow it, but found herself powerless to do so. 'My difficulty is that I get so far and my mind goes blank. I never would have supposed that it was so difficult to read and understand something outside one's own field.' 'I spent about six months trying to make sense to myself of your paper, but each time I tried, my mind just went *blank.* Apart from trivialities I could neither criticise nor approve it. The best that I could say to myself, to justify my communicating it at all, was that — for what my judgment was worth — I could not see the fallacies in it, if there were any... If I were to spend six weeks reading it again it would still mean nothing to me. My mind is not built that way. The whole of Einstein's theory just seems esoteric nonsense, as far as I am concerned... My mind simply does not *care* whether clocks go at the same rate or whether they don't, and it refuses to work when I try to make sense of it. I'm sorry.'

Kathleen Lonsdale was one of the most intellectually honest people I have known, and that her mental endowments were too slight to enable her to follow the simple piece of reasoning given on p. 45 is too ludicrous to be entertained by anyone who knew her or even knew of her. The fact that she could write in these terms is an outstanding testimony to the harm which has been done by the illusions that are so widespread concerning relativity. We shall sec that it is general; the more distinguished and the more mentally honest and the more concerned in their work with the special theory of relativity the experimental physical scientists may be, the more convinced they are that the theory is unintelligible to them. What they cannot transcend is the conviction that the 'mathematicians' do understand it and cannot be wrong: they choose to believe themselves fools rather than that. We shall meet with other examples; Kathleen Lonsdale merely expresses it more starkly.

To continue, however, with the course of events. She did communicate my paper to the Royal Society, so that it could be submitted to a referee, but without any recommendation for or against its acceptance. In due course the reports of two referees were received, as a result of which the Society rejected the paper. It would be impracticable to quote them at length since they would be unintelligible without the paper itself, even if with it. It must suffice to say that the essence of the paper was the criticism of the theory already given, and that neither referee even attempted to refute it by answering the all-important question given on p. 45. There were comments on details, but on the essential point the conclusion of one referee was: 'In some cases of this type publication might still be justified because the alleged objections and the arguments which have to be used to deal with them may be instructive. However, in the present case the fallacy is so elementary that I must recommend the rejection of the paper.' The other referee merely wrote: 'Although he has much to say which is of interest to historians of science and which might with advantage be published elsewhere, my view is that the Society would make itself ridiculous by publishing this paper.' The remarks of both referees on minor points were such as to give an uninformed reader the impression that the author was unacquainted with the subject. Kathleen Lonsdale could make no more sense of the reports than of the paper, but agreed to act as postman between the unknown referees and me. I replied to their criticisms, but no further communication could be obtained from the Society.

4

Attitude of the Press

It was now clear to me beyond question that what for some time I had suspected was an established fact: the matter had taken on an entirely new character. Controversy in scientific matters is, of course, a commonplace; it is the means by which science progresses. Differing views are advanced and debated until agreement is reached or experiment closes the discussion. The twin paradox, for example, began its course more than half a century ago with a discussion between leading workers then in the field, and in the latest revival those most prominently concerned with the subject freely expressed their opposing views, and that not only in technical journals but through more popular media also. In all parts of the world various aspects of the problem were presented, and it was hard to find anyone who had written on relativity at any level of sophistication who had not made his contribution to the general exchange.

But when the validity of special relativity itself was called in question there was a sudden change. Almost all the leaders in the subject relapsed into silence. Among the younger physicists, however, the reaction increased tenfold, as Mr. Brimble, then editor of *Nature,* indicated (p. 39) and my private postbag confirmed only too embarrassingly. I did, indeed, receive a number of replies to personal letters from some of the recognised authorities, dissenting for various incompatible reasons from my conclusion, but with very few exceptions, which will transpire in due course, none of these correspondents was willing, as Max Born had been, to publish what he was ready to assert privately. As but one example, Professor G. Temple, of Oxford, who had shortly before written in a book: 'Much as I should like to disprove the special theory of relativity, and in spite of the many years I have given to this task, I have to admit I cannot find any flaw in the evidence'[1], after offering a plainly evasive answer, refused to publish it because, as he said, 'I do not wish to damage your reputation.' Nowhere in the published literature of the subject can one find any answer at all to the simple question posed on p. 45).

This, as I say, radically changed the situation, and that in two ways. In the first place, it was a direct violation of the fundamental ethical principle of science, expressed in Dale's statement (p. 23) that no anomaly shall be neglected and that nature's answers shall be given to the world with an unflinching fidelity. Nature's answer to the anomaly which I had pointed out could not by any means be brought to the knowledge of the world, although nature's recognised spokesmen in this matter claimed to know her answer (or rather answers, for there was little agreement among them) with confidence when writing privately or anonymously. And, in the second place, the matter was not one of academic interest only, but one that vitally concerned the safety of the whole population. Since I was the unintentional medium through which these things had declared themselves, and the sole possessor of the full evidence for their actuality, there was laid on me a double duty, which I would most willingly have escaped if I could have done so without dishonour, but from which no such escape was possible. As a scientist I was bound to do all I could to restore the obedience of scientists to their basic ethical obligations; as a citizen I was bound to do all I could to prevent a possible public disaster arising from the neglect of those obligations. I therefore, on 9 August 1966, submitted the following letter to *Nature* for publication. (Concerning the second point, I had already written to some of the more serious daily and weekly journals, calling attention to the need for scientists to give evidence that would carry conviction to the public that its interests were safeguarded, but, understandably, it was considered that the matter was more suitable for a scientific medium since it would almost inevitably develop into a discussion beyond the understanding of the general reader):

To the Editor of *Nature.*

Sir,

SPECIAL THEORY OF RELATIVITY

> More than four years ago[*] I gave in *Nature,* as the culmination of several similar efforts, a simple proof that the special relativity theory is untenable. This received only one reply from an acknowledged authority, namely, Professor Max Born[♣] who, unfortunately, as he himself said, assumed that I had expressed myself badly, and replied to what he thought I had meant to say. My assurance that what I had meant was what I had said[♣] has remained unnoticed.
>
> In the meantime continuous efforts have been made, by others, to get this disproof of the theory either refuted or accepted, and myself but without success. Many authorities have been approached personally, but, among the surprising variety of incompatible comments, there is none that its author will consent to publish.
>
> The argument is extremely simple and fully understandable without specialised knowledge. To compare the rates of two regularly running clocks, A and B, we must find the interval recorded by one for a given interval by the other. It is immaterial which is taken as the standard: if A records 2 hrs for an interval of 1 hr by B, then A works twice as fast as B, and B must record 1/2 hr for an interval of 1 hr by A. If the docks are in relative motion, however, they cannot be together throughout the interval, so if we remain with B to observe its readings, we can determine those of A only by means of a theory. The special relativity theory purports to serve this purpose, and Einstein so used it[♦], thereby calculating that (for a particular velocity of separation) A would record 2 hrs while B was observed to record 1 hr. He concluded that A worked twice as fast as B, and this result is universally accepted as the unique solution of the problem. He did not calculate the interval by B for an observed interval of 1 hr by A, but when we do so, by the same theory, we find it to be not 1/2 hr but 2 hrs, showing that B works twice as fast as A. The same theory thus requires each clock to work twice as fast

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[*] *Nature,* 195, 985 (1962).
[♣] *Nature,* 197, 1287 (1963).
[♦] *Ann. Phys.,* 17, 891 (1905).

as the other, which is contradictory. The necessary conclusion is that that theory must be wrong.

The importance of this result, if valid, is obvious and profound, and it is not in accordance with the ethical principles of science that its refutation or acceptance should be strenuously withheld over so long a time, while the theory continues to be acted upon as though it were unquestioned. The ethical requirement is in this case greatly reinforced by the consideration that the theory is so intimately involved in the whole of fundamental physics that, if it is indeed false and that fact is left to be revealed by the failure of some experiment in which its truth is assumed, the physical consequences may be incalculably disastrous. However, if those individual scientists who might have been expected to comment publicly on the matter do not recognise the moral obligation to do so, I know of no effective pressure that can be brought to bear on them.

The Royal Society, on the other hand, not only has honourable traditions to maintain but is a public body, supported by public funds, and therefore with undeniable responsibility to the public. Accordingly, an elaboration of the above argument, with a discussion of its implications, was submitted to the Royal Society for publication. It was rejected on the report of an anonymous referee who wrote: 'In some cases of this type publication might still be justified because the alleged objections and the arguments which have to be used to deal with them may be instructive. However, in the present case the fallacy is so elementary that I must recommend the rejection of the paper.' It appears, then, that the Royal Society is satisfied that the above argument is fallacious, but the nature of the allegedly obvious fallacy is tenaciously withheld from view.

I write this letter, as a member of the public and as spokesman for those who have expressed to me their grave misgiving at the state in which this matter now stands, to request that the Royal Society shall publish in *Nature* a statement of the fallacy in the argument expressed in my letter in this journal and summarised above; or, alternatively, acknowledge that there is in fact no fallacy and therefore that the special relativity theory can no longer safely be used as the basis for dangerous experiments.

It is within the competence of every reader of this letter, physicist or not, to see that there are only two ways in which this disproof of the theory can be refuted. It must be shown either (1) that it is permissible to determine the rate-ratio of two uniformly running clocks by calculating the interval by A corresponding to an observed interval by B, but not by calculating the interval by B corresponding to an observed interval by A; and in that case it must be shown how, in a particular instance, one determines, consistently with the theory, which clock is A and which is B; or (2) that there is an algebraical error in the second calculation (like the first, a direct deduction from the Lorentz transformation) that does not invalidate the first. If there is a fallacy in the argument, the referee's description of it as 'elementary' is an understatement, and any presentation of it

> that does not show one of these things, or transforms it into something expressible only in comparatively recondite terms, would be *ipso facto* a clear indication that the point was not being met.
>
> We are now at the beginning of an era in which the physical safety of the public is, and will increasingly be, in the hands of scientists whose activities are beyond general understanding or control. In such circumstances it is more than ever necessary that scientific integrity shall be both preserved and seen to be preserved.
>
> Herbert Dingle

After nearly four months I received from the editor, Mr. John Maddox, the following reply:

> I am sorry that there has been a delay in dealing with your manuscript, but you will appreciate that it is a particularly difficult one.
>
> I am writing to say that I would not be prepared to let you issue a challenge to the Royal Society and its referees in *Nature.* You will, I hope, realize that what other journals do is not usually our business.
>
> As to the argument that you restate about the special relativity theory, I should not be averse to publishing a letter from you saying more or less what you do on the first page of the manuscript which I am returning to you. I realize that you may not think this worth while, given that you have made the point before, but I shall look forward to hearing from you.

Some correspondence ensued between Mr. Maddox and me, in which I tried to persuade him of the need for the publication of my letter, but the net result was nothing at all: I was at liberty to say again what I had said many times, in public and in private, with no effect, but the actions of the Royal Society were, without any qualification, not open to questioning in *Nature.*

It appeared to me impossible to reconcile the principles on which the Royal Society was founded with its refusal to make public a piece of pure scientific knowledge — namely, the fallacy in my argument, which it had accepted, on the authority of its chosen referees, as established — and with its exemption from questioning in the leading scientific journal. It appeared equally impossible to reconcile the attitude of the editor of *Nature* with the principles on which the journal *Nature* was founded, of which I could claim to know something since I had written the Life of its founder and editor for its first fifty years. Sir Norman Lockyer, and examined the correspondence and other papers that recorded its original purpose, and had also worked in close association with Lockyer's successor for the next twenty years, Sir Richard Gregory, who had maintained its ideals and made the journal available for any inquiry, of no matter what person or organisation, that was directed towards the advancement and dissemination of scientific knowledge.

Again, therefore, I was faced by the same two intolerable abuses, as they seemed to me — one of the functions of the Royal Society and the other of that of the scientific press. To deal with the former the proper course seemed to be a direct appeal to the Fellows of the Royal Society to consider what their Fellowship required of them in these circumstances, while for the latter there existed the Press Council, one of whose terms of reference was 'To keep under review developments likely to restrict the supply of information of public interest and importance'. Though my approach to the Royal Society slightly preceded that to the Press Council, I record the latter first since the other had a lengthy sequel, which it will be more convenient to relate without interruption.

On 29 June 1968 I sent a letter to the Press Council of which, in order to indicate as clearly as possible the nature of my inquiry, I quote the most relevant parts. I began:

> I wish to bring to the notice of the Press Council a matter concerning the scientific journal. *Nature,* and to ask for its help in rectifying a potentially dangerous situation. Although of necessity technical questions are involved, the point here raised is quite independent of them and requires no decision to be made on such questions or any consideration given to them; it is entirely a matter of the ethics of journalism in circumstances of great public importance which I shall describe. I shall be as brief and clear as I can, but a statement of considerable length is unavoidable. The whole controversy, of which I select here only those aspects that are related to the functions of the Press Council, has been proceeding for nearly ten years and has involved scientists all over the world, so it will be understood that, in order not to mislead, I must place brevity second to clarity.
>
> I wish also to emphasize that, although there are details of the matter in which I think I should have personal ground for complaint against the present editor, Mr. John Maddox, I am here making no such complaint: my charge is entirely that his actions, and the principles underlying his conduct of the journal, constitute a grave public danger. The present relation of scientific research to public safety is such as to demand complete integrity on the part of scientists, and when there is reason to believe that this is not being preserved, the Press (naturally the scientific Press, of which *Nature* is the acknowledged leading representative, since general journals could not be expected to open their columns to discussions in which highly technical matters might intrude) is the only medium through which the situation can be examined and, if necessary, adjusted. The question at issue here was launched in *Nature* (following preliminary publication elsewhere) nearly six years, ago, and has there proceeded too far for another journal to be asked to take it over in the event of *Nature* failing to pursue it, so unavoidably the responsibility for reaching a definite conclusion lies with that journal. My indictment is that it has not discharged that responsibility, and gives no sign of doing so.
>
> This failure manifests itself in three ways, which I will first briefly indicate and then elaborate, (1) The editor refuses to allow, and will give no reason for not allowing, the Royal Society to be asked in *Nature* to publish

> knowledge of outstanding scientific importance, having momentous public implications (but not in the least involving matters of secrecy in any sense at all), which it claims to possess and acts upon. (2) He has, in a leading article, charged me with acting 'immodestly' in questioning the integrity of scientists who persistently refuse to answer informed criticism but proceed with their experiments as though the criticism had not been made, and — although his statement is too obscure to convey a precise meaning — gives a strong impression that one's duty in such circumstances, no matter how convinced one may be that a serious error is being made, is to remain silent and, in effect, admit the unauthenticated infallibility of the majority. This is a new and very dangerous principle in scientific ethics. Furthermore, my reply to this leading article has been refused publication, and still — nine months later — no comment on it from me has appeared or has any prospect of appearing. (3) He has persistently refrained from publishing letters — by others as well as me — which have a direct bearing on the point at issue, and has sought to justify repeated postponements by a series of excuses which, however regarded, can have no respectable explanation.

I then gave details, the more important of which have already been, or will be, recorded here, and my account included the statement: 'The point which I submit for consideration by the Press Council is thus perfectly clear and free from complications: Is it proper for the Royal Society to be exempt from questioning in the Press when there is reason to believe that it is pursuing a policy attended by risk of grave public danger?'

[With regard to item (2) in the third paragraph of this passage, concerning the editor's leading article, I should here interpose an explanation made necessary by the slight departure from chronological order in my account, which I have already mentioned and which I regret, but it seems the least of the evils which a condensed statement of a very complicated matter necessitates. The leading article which appeared in *Nature* of 14 October 1967 (the circumstances of its appearance are explained later — p. 74) is reproduced in the Appendix, and my reference is to its last paragraph. At the end of my article of 6 January 1968 dealing with McCrea's reply to me (also reproduced in the Appendix), as it was submitted to *Nature,* I wrote the following passage:

> The position must be clearly understood. Here is a challenge to a theory that not only, as *Nature* says, 'in spirit as well as in detail has come to pervade the whole of modern physics', but also has the deepest implications for general philosophic thought and public safety. The first painfully extracted response (Born) misread it as an undergraduate's error and missed the point. The second, still more painfully extracted, response (McCrea) has misread it as an idiot's error, and also misses the point. If it be regarded as possible that I wrote not wholly unintelligently and from knowledge, not ignorance, I do not think the statement in the preceding paragraph allows of any misreading that is not deliberately sought. I await a third response.

'There is an even graver aspect of the matter than that immediately evident. It has long been an accepted principle in science, abundantly exemplified by a former editor of *Nature,* the late Sir Richard Gregory, in his book, *Discovery: the Spirit and Service of Science,* that all theories, however well established, shall be kept in constant scrutiny, subjected to every possible criticism, and abandoned as possible representations of truth as soon as criticism shows them to be untenable. My ample correspondence reveals a growing suspicion (it is far from being mine alone) that the protracted reticence of all authorities on my criticism of special relativity shows that this principle no longer operates; that although formally acknowledged to have been useful when applied to the false theories of the past, it is now discarded since relativity theory is final truth. This suspicion is now confirmed by *Nature,* which holds it 'immodest' to press a criticism proved by long neglect to be negligible. If this represents the actual guiding principle of modern scientists, the fact cannot be too widely publicised, so that steps may be taken to curb the activities of those who, embracing this revised scientific morality, hold the lives of the population in their hands.

Nevertheless, I am still unwilling finally to conclude that, among those scientists of repute who are able to speak with understanding of special relativity — a far larger number than those few generally regarded as 'specialists' — there is not one who still rates loyalty to truth above that due to the opinions of the moment, to his own past statements and present intuitive expectations, and to all personal considerations of whatever kind, and who has the candour and the courage to face this criticism directly, without looking askance for paths of possible circumambulation. If there be such a one, he will recognise his duty in these circumstances, and either clearly show where this criticism is at fault or else unreservedly express his acceptance of it. If this is not done, the experimental revelation of the failure of the theory, which is unlikely now to be long delayed, will dart a fierce light on the state of current science, and the reaction to what it shows will not be pleasant.

However, three days before the appearance of this article, which had been with the editor for several weeks, he rang me up and said that he did not intend to print this passage, and asked me to dictate to a clerk, on the telephone within a few hours, a statement of equal length dealing with some relatively trivial points that other correspondents had raised. Under protest, and relying on his assurance that I should be given an opportunity later to criticise his article, I dictated the statement asked for (which may be seen in *Nature,* though I have omitted it here for reasons which will be obvious), and, since there was no time for proof corrections, it appeared in *Nature* with a number of errors, some of which made it nonsensical. The editor promised to correct these, which he did some weeks after his promise, but the correction is most unlikely to be seen by anyone trying to follow the controversy. I was never given the promised opportunity to comment on the editorial. I record this, not in order to exemplify the pinpricks received in the course of my dealings with Mr. Maddox, but because it is necessary to explain my reference to the more serious aspect of the matter which I reported to the Press Council.]

To my letter to the Press Council I received a' courteous reply, asking certain pertinent questions but giving the impression that the nature of my request had not been fully appreciated. I therefore replied as follows:

> I thank you for your letter of July 15 regarding my controversy with *Nature.* I appreciate the interest shown by your comments, and will do my best to answer the questions you raise.
>
> Before coming to the specific points, however, I want to make quite clear the nature of my application to the Press Council, because some of your remarks — forgive me if I have misinterpreted them — seem more appropriate to a personal controversy between a plaintiff and a defendant than to the description of a situation of great danger which I hope the Press Council will be able to relieve. The new scientific age has unavoidably made public safety dependent on the incomprehensible and uncontrollable activities of scientists, who have therefore now an additional obligation to preserve the utmost integrity in their work. If there is reason to suspect that they are not doing so, and thereby risking a disaster, the public has an unquestionable right to assurance that the suspicion is unfounded or, otherwise, that its cause shall be removed, and its only medium for obtaining such assurance is the scientific Press, of which *Nature* is the acknowledged head. What I am bringing to the notice of the Press Council is evidence that *Nature* does not measure up to the demands of this situation, and I ask for its assistance in an effort to see that it does so — first, by facilitating instead of consistently opposing the attainment of a solution of the relativity problem, which is immeasurably important in itself, and next, and more generally, by awakening to the responsibilities that lie on it so that such cases as this shall not recur. But I seek no reprisals for anything that has already happened (except, of course, such as may be involved in whatever action the Press Council may think necessary to produce the desired result), and nothing would please me more than to co-operate with the editor in bringing out the truth in this relativity matter, whatever it may be, as clearly and speedily as possible. I think you will see from the enclosed correspondence with him, to which I shall refer later, that I have done my best to secure such co-operation, but without success. The purpose of what I have written, and shall write here, is therefore solely to establish, as convincingly as I can, the fact of the editor's shortcomings, for the purpose of terminating and not seeking redress for them.

I then answered the specific questions put to me, but since the letter I had received, as well as its envelope, had been marked CONFIDENTIAL in underlined capital letters, I thought it necessary to conclude with the following paragraphs:

> I hope I have now met the points you raise, but if there is any further information that I can give, I shall be most pleased to give it. There is one thing, however, that I should say in conclusion. Your letter is marked 'Confidential', and I shall of course respect that request as far as it is possible to do so. But throughout this controversy I have not written or consented to receive any letters

> confidentially in an absolute sense, and if the Press Council should be unable to help in getting the existing situation rectified, which I sincerely trust will not be the case, I shall be compelled to take other measures which may involve the publication of the whole story in a book, in which case I must hold myself free to publish the whole or any part of this correspondence if necessary. I am convinced that the public has an inalienable right to know the danger in which it will stand if all normal agencies for its protection fail it, and therefore I cannot commit myself to any promise that may limit my power of enlightening it.
>
> I deeply regret any minatory aspect that this may appear to wear, which would be wholly foreign to my intention, but it would not be right not to make my position perfectly clear before there is any further risk of my being entrusted with confidences that I might not be able to honour. My aim throughout this business has been to produce the right result with the minimum of sensation, using normal channels whenever they are open and seeking abnormal ones only through necessity. I think my long letter to Mr. Maddox will confirm this. That I shall continue to do, but the end is so important that I cannot neglect whatever means may need to be employed.

This concluding statement, whose character the reader will be able to judge for himself, appeared to have driven from the mind of the Assistant Secretary of the Press Council, who shortly afterwards became its Secretary, all other aspects of the matter, for the *complete* reply which I promptly received was as follows:

> I acknowledge receipt of your letter of 19 Jul 68 in connection with your complaint against *Nature.*
>
> I note with regret the concluding paragraphs of your letter and must inform you that the Council adopts certain rules in the conduct of its inquiries in the interests of the parties concerned. It expects correspondents seeking its assistance to honour those rules and it will not, except in most exceptional circumstances, permit the publication of its letters, the copyright of which remains vested in the Council.
>
> In the circumstances I do not feel I can correspond with you further but will submit the matter as it stands to the consideration of the Council's Complaints Committee.

It would be tedious to give the whole of the ensuing exchanges; I proceed to my letter of 15 August, by which time I had found it necessary to put specific questions to avoid, as I hoped, further misinterpretations of my inquiry; the most pertinent of these questions was the following:

> In the view of the Press Council, is it proper that the actions of the Royal Society, in the present state of the relations between those actions and the public interest, shall be unconditionally exempt from questioning in the Press, as is

> implied by the following unqualified statement by the editor of *Nature:* 'I would not be prepared to let you issue a challenge to the Royal Society... in *Nature.'?*

The complete reply to this, dated 9 September 1968, was:

> Your letter of 15 August 68 has been carefully considered by my Council's General Purposes Committee and I am instructed to tell you that the finding in your complaint was that the Editor of *Nature* had not contravened his ethical duty. There was, therefore, no substantial case within the Council's purview, to send forward to the Council for adjudication.

No reference was made to my questions.

The net result of this effort, then, was to bring to light the following facts, on which I make no comment but which I leave to the consideration of the reader:

(1) The actions of the Royal Society, no matter what evidence there may be of their potential public danger, are not open to informed questioning in the scientific press.

(2) It is not possible to submit to the Press Council an inquiry clearly coming within its terms of reference.

I pass now to my approach to Fellows of the Royal Society. It seemed appropriate to circularise them, giving an account of the situation, and inviting them to consider it in the light of their responsibilities as Fellows of the Society. Accordingly, on 9 June 1967, I sent the letter given below to about half the number (roughly to 300 Fellows). This restriction did not seem to me to reduce seriously what effect the circular might have, in view of the number involved (these were chosen largely at random and included scientists of all types, since it was the moral, and not the technical, aspect of the question that was being presented), and it had the advantage that, should it become necessary later to reveal the story, it would not be possible for anyone to conclude that any particular Fellow had reacted in any particular way to the circular, since he might not have received it. The circular was as follows:

> To Fellows of the Royal Society.
>
> Dear Fellow,
>
> I enclose a copy of a letter, which I submitted to *Nature* on August 9 last. After four months it was refused publication on the sole ground that the editor would not allow the Royal Society to be 'challenged' in *Nature.* ('Challenge' is the editor's term for what will be seen to be a request for general enlightenment, which the Royal Society has privately declared itself able to provide, on a matter of great scientific and public importance). Repeated requests have failed to draw from him any justification of this decision; it is simply reiterated.

Two distinct aspects of this situation demand attention. In the first place, the facts that nothing will induce anyone to answer publicly a very simple disproof of the special relativity theory while in private or anonymously I am offered a variety of mutually contradictory answers all so palpably irrelevant that the reason for their authors' unwillingness to acknowledge them publicly is crystal clear — these facts admit of only one explanation. This disproof of special relativity is so plainly conclusive that no one guided by reason can doubt it; but also it is so unexpected that no one guided by prejudice can believe it. Those called upon to comment therefore cannot produce a refutation, but fear that, if they accept the disproof, someone will discover a flaw, which has escaped them and their incapacity will be exposed; hence they keep silent. No other explanation is consistent with the abundant evidence which I possess, but no such evidence is needed to confirm that there can be no respectable origin of more than eight years' unyielding refusal to meet publicly a simple and repeatedly published criticism of one of the most fundamental principles of modern physics. To the hazards attending this covert apostasy its practitioners seem indifferent.

The second aspect of the situation is this. From now onwards scientists will control the physical safety of the population to a degree never before attained by any man or body of men, since their free and potentially catastrophic activities are tar too abstruse for general understanding. Without complete prohibition of scientific research, the public is thus compelled to trust implicitly in the integrity of scientists, and it has therefore an undeniable right to assurance that integrity is being maintained. Its only medium for such assurance is the scientific press; the general press (as I know, since I have approached both daily and weekly journals) quite understandably considers itself unsuited to this purpose, since the raising of such matters there would almost certainly generate discussion beyond the understanding of most readers. In these circumstances the arbitrary withdrawal of *Nature* from an inquiry which it has accepted as legitimate until addressed to the one body obliged to answer it removes from the potential victims of scientific research their only remaining safeguard. The Royal Society, the supreme scientific body in this country, can freely violate its foundation principles (as in fact it is doing) and the public, whose servant it is, cannot question it, however clearly the abuse may be perceived and established. Such are the spirit and the conditions in which scientists, in this country at least, assume the responsibilities, which the new scientific age lays upon them.

This state of affairs seems to me so intolerable that no effort should be spared to rectify it. Every attempt to do so which I have made over the years — and such attempts are far too many and various to be recounted here — has met with frustration; those directly involved will do nothing, and others, to whom the facts have to be related, find them incredible and dismiss them as unreal. I can now see only one way open to a non-Fellow of the Royal Society. The President of the Society is fully aware of all these circumstances and is satisfied that they are in right ordering. I believe that this satisfaction would not be shared by the public if they knew the actual state of things, and the only means remaining to me of informing them is to make such open charges against the President as will compel him to sue me for libel, whereupon, withholding nothing, I shall be able to expose a situation that would profoundly shock those holding the popular belief in the disinterestedness of scientific research. I do not overlook the probable consequences, both immediate and by repercussion, of such action, and for various reasons, both personal and general, I would do anything possible honourably to avoid it, but since the only alternative that has now been left to me is passive acquiescence in a course of degradation which, if unchecked, must eventually lead to moral and physical disaster, I have no real choice.

Fellows of the Royal Society, however, have a right, not possessed by me, of requiring that they shall not be unconsciously implicated in behaviour incompatible with the ideals of the Society to which they belong and shall not unwittingly be the subjects of whatever indictment may become necessary. I write this letter, therefore, in the hope that there are Fellows who hold that the purpose of the Royal Society is still to seek and to make known the truth in scientific matters and that its manner of fulfilling that purpose should be such as not to need concealment behind a screen provided by the editor of *Nature.* If such a Fellow is prepared to see that the Royal Society causes to be published in *Nature,* in reply to my letters long remaining unanswered there, either (a) an authoritative statement, expressed in terms intelligible to anyone who can understand the letter herewith enclosed, of the elementary fallacy which it has accepted as invalidating my disproof of special relativity; or (b) an authoritative acknowledgement that the theory is now proved untenable and can no longer safely be used in theoretical investigations or dangerous experiments — I shall be grateful if he or she will so inform me within a month. In that case I shall most happily abandon the intention I have indicated, which otherwise will become a compelling duty.

Yours sincerely,

Herbert Dingle

To this circular I received twelve replies. Eleven of them ranged in type from (these, of course, are not literal quotations) 'Dear, dear; this is really too bad' to 'My dear fellow, you don't understand that it is not the function of the Royal Society to concern itself with such matters as this'. Only one Fellow (Dr. D. G. King-Hele) felt that his Fellowship of the Society obliged him to do something about it, and through his instrumentation at last one mathematician (Professor W. H. McCrea) was induced to agree to reply in *Nature to* an article of mine setting out afresh my criticism of the theory, and the editor of *Nature* was induced to publish the discussion. It appeared in *Nature* of 14 October 1967, together with a leading article entitled 'Don't Bring Back the Ether', in which the editor's view of the controversy was set out. By kind permission of Professor McCrea and the publishers of *Nature,* Messrs. Macmillan, the discussion and the editorial are given in the Appendix.

I shall describe presently the course of this controversy, but it would be unjust to leave the eleven unfruitful replies without mention of the most significant of them, which came from Sir Robert Robinson, a former President of the Society, whose efforts in the matter I have already mentioned. He wrote me on 25 July 1967 a letter from which I quote the following:

> As you know, I am very sympathetic to you in the trouble you experience in getting physicists to discuss your views, but I am not myself competent to assess the rights and wrongs of the scientific aspect of the case.
>
> I do not find in your letter any clear statement of the nature of the dangers, which you imagine, might follow the use of the special theory of relativity. You say the possibility of danger is vividly real to you and yet I cannot find in your letter, or in anything you have written, a clear statement of the nature of the danger you anticipate... unless you can clearly lay down the nature of the anticipated dangers, the possibility remains that you are 'starting at a shadow'.
>
> In regard to the Royal Society and its Fellows, I am quite clear that the Royal Society has, from the beginning, refused to make any pronouncement on any matters of scientific fact or theory, but I do not think that the relegation of such functions to individual Fellows can imply any control or direction of their efforts. If the Royal Society exercises pressure on the Fellows, it would be tantamount to an expression of opinion. I think your whole attitude to the R.S. and to its Fellows is somewhat misconceived. The general body of physicists should be involved and not merely those who happen to be members of a particular Academy.

On the matter of specifying the danger involved, I can only say that if this could be foreseen, steps could be taken to prevent it, but since we know only of what character this might be, it seems wiser to start at the shadow than passively to await the arrival of the substance casting it. However, the point at the moment is the view taken by so eminent an authority as, one of its former Presidents of the function of the Royal Society in such a matter as this. He agrees that it is the duty of 'the general body of physicists' to meet my criticism, but he gives me no guidance as to how to get them to do so: he says only that it cannot be done through 'a particular Academy' of which they are members, and it certainly cannot be done by bringing the laws of the country to bear on them individually, or, as we have seen, by the Press, scientific or general. The position, therefore, is that, on this view, the Royal Society has no responsibility for seeing that its members respect the principles on which it was founded and for conformity to which the public supports and trusts the Society. I need not express my view on this, but it is necessary that the public should know exactly where it stands, and that it is supporting a body which is free to sponsor operations, potentially of the greatest danger, without responsibility for seeing that its members act in accordance with its basic principles or with any regard at all to what is acknowledged to be their duty as scientists. How, in fact, the Society does regard the behaviour of its Fellows is shown indirectly by the response to my circular and directly by the reply of its President to the request for assurance that scientific integrity is still preserved (p. 100).

However, let us return to the one positive result of my circular — the discussion in *Nature.* Slight as this response was in comparison with the needs of the case as I saw them, the resumption of the matter in *Nature* did, to my great satisfaction, relieve me from the course which, at the time of writing the circular, seemed my only means of informing the public of facts which I considered it was imperative it should know. The grounds on which I should have used this means will, I think, be clear enough when I have described the attitude of the then President of the Royal Society to the situation. The *Nature* articles stimulated some correspondence in which, as ever, the leading 'mathematicians' (with one conspicuous exception to be mentioned immediately) refrained from engaging, though a few of the 'experimenters' took part. The exceptional mathematician was Professor J. L. Synge, of the Dublin Institute of Advanced Studies, who is by common consent one of the leading authorities on the mathematical side of the theory and advocates of it. I had previously been engaged in private correspondence with him, during which I succeeded in convincing him that I had made no mathematical error or misunderstood the mathematical requirements of the theory, though we could not agree on the vital question of its physical validity. For that reason I thought it best not to accept his first proposal — that we should write a joint letter to *Nature,* stating our agreement on the point at issue and adding that we differed on how it was to be resolved. This I considered would leave the matter in the air, and I suggested that instead he should write, stating where we agreed and giving his reasons for our divergence at that point. This would be a great step forward, for it would eliminate all the irrelevancies that our correspondence had cleared away but which other correspondents continued to pour in, and would enable me to give my line of advance from our common standpoint — not doubting that I should be permitted to do so. To this he kindly consented, and on 13 March 1968 he sent the following letter to *Nature,* which was published several months later:[2]

> As the result of a lengthy correspondence with Professor Dingle, I am of the opinion that the contradiction described by him in *Nature* 216 (1967), p. 119 is due to the incompatibility of
>
> (a) the concepts used in the special theory of relativity as ordinarily understood, and
>
> (b) the concept of clocks that run regularly, as understood by Professor DingIe.
>
> I believe that Professor Dingle agrees that this is a correct diagnosis of the cause of the contradiction. To resolve it, one must abandon either (a) or (b). Since (b), as elucidated in our correspondence, is equivalent to Newton's concept of absolute time, and since relativistic physics appears to me to represent nature more closely than Newtonian physics does, I cast my vote for the abandonment of (b) and the retention of (a).

Now I think it is clear that this opened the way to a final solution, for my conception of a regularly-running clock was that generally accepted in physics, my only relevant demand of it here being that one such clock should not be able to run steadily both faster and slower than another. I did not think it likely that this would be disputed, and if not, then, according to Synge's own statement, the special theory of relativity would have to be abandoned. I wrote at once to *Nature,* feeling that the long drawn out controversy was at last about to be ended. I was wrong: my reply was not published, and still, more than three years later, it remains unpublished, despite requests to the editor for it from various sources, some of which will be mentioned in due course. Meanwhile, the special theory of relativity continues to be used as though it had never been questioned.

In retrospect I think I made a tactical error in unduly lengthening my reply with an explanation of why, in my view, the Newtonian and relativistic concepts of time had nothing to do with the matter (these concepts are dealt with more fully in Part Two of this book), but that is a small point. If the editor of *Nature* had been concerned only to reach the truth of the matter, his failure to publish my reply is to me inexplicable. He did not close the correspondence. He continued to publish other contributions from lesser authorities than Synge in general estimation, re-introducing the side-issues that Synge and I had cleared away; he wrote to me that he had tried unsuccessfully to get Professor Bondi and some Americans to enter the lists; he asked my opinion as to whether he should seek X's view (X was a writer on relativity, not comparable in standing with Synge, who had shortly before sent me 21 foolscap pages of mathematics having nothing to do with the matter); he sent me several letters on relativity (though not on this controversy) that had been submitted for publication, asking me to report on their suitability and apparently acting on my reports; but the one essential thing that he would not do, or explain why he would not do, was to publish my reply to Synge. Inevitably, readers of *Nature,* unaware of all this, concluded that I had no reply to make, and, as we shall see later in connection with a correspondence in the *Listener,* I was chided for my neglect.

I should not leave this question without mentioning a sequel which has a significant bearing on a point which I have already mentioned and shall deal with more fully later, which I think lies at the heart of this strange matter — the misconception that is rife concerning the relation of mathematics to physics. When I sent my letter to *Nature* I sent a copy of it to Synge, and a few weeks later he replied as follows:

> It is on my conscience that I have not acknowledged your letter of 19 September, enclosing copy of your letter to *Nature.* I could not decide whether to pursue the argument with you or let the matter drop, leaving the last word to you.
>
> But just yesterday I had a thought. What if Dingle is pulling the leg of the world? It is to me the most reasonable hypothesis to explain what is otherwise inexplicable to me. Knowing you as well as I do (and I know you much better after our recent correspondence), I cannot bring myself to believe that you are as stupid as you make yourself out to be. If my hypothesis is correct, I salute your sense of humour. No harm has been done. Printers have had good employment.

> My humiliation in having been taken in is swallowed up in my admiration at the way you have put the thing across.
>
> You will of course deny the truth of my hypothesis. Or will you ? Within the range of semantic tolerance, the term 'leg-pulling' may have many interpretations.

I replied, of course, that I was in anything but a hoaxing mood, but the significant thing to my mind is that a man of Synge's undoubted mental power was so perplexed by the whole business that he could not answer his question himself. If I was indeed hoaxing, then I might be expected to deceive a beginner or an incompetent thinker or an ignoramus, but a man of Synge's calibre and long years of study of the theory should have been able to see through it at once: instead, he not only could not sec through it, but could not see whether it was indeed a hoax or not. This is to my mind an outstanding example of the stranglehold that the misconception of the function of mathematics in physics (see Chapter 6) has acquired on the minds of the leaders in physical science. It is not now very new. It was evident to me a third of a century ago, and in *Nature* of 12 June 1937, in an article which I shall mention later, I wrote:

> How many physicists to-day feel confident that they can read a statement concerning our ordinary everyday consciousness of time, for example, and say, not whether it is true or false, but even whether it is sensible or nonsensical, a serious idea or a clever hoax ? The criterion for distinguishing sense from nonsense has to a large extent been lost.

I did not then anticipate the form which this perversion of the intellect would take so far ahead, but it would not have surprised me if a crystal ball had then given me foreknowledge of Synge's letter.

To resume the story, however, it is necessary to interrupt the account of *Nature's* concern with it to recount other journalistic aspects of the controversy, through one of which *Nature* again became involved. While this matter was proceeding, but quite independently, I received a letter from *New Scientist,* inviting me to contribute 'freely' to a scries of articles about 'possible future developments in science and technology, particularly those likely to have substantial effects on society'. 'We give our contributors,' they wrote, no formal brief whatever other than that they should discuss and speculate on any trends or developments in science, the future effects of which they find worrying, exciting or intriguing in some way.'

I welcomed this as a means (as I thought) for me to inform the public, with a minimum of exposure of the more seamy side of the matter, of the facts of which I considered they had such an undeniable right to be informed, so I at once sent in an account of the general position, as I have described it here — with, of course, in accordance with the terms of the invitation as I understood them, special reference to the danger inherent in the existing attitude of scientists to criticism of their theories, the possible consequences if those theories were misconceived, and the closure of the Press (other, of course, than *New Scientist)* to all questioning of the activities of those possessing such power. It appeared, however, that I had misunderstood what I had been asked to do. After five weeks I received a letter from the journal, returning my article and, after some purely scientific comments, proceeding:

> I do, however, appreciate that if your ideas are correct they are a matter of no little significance. But they have now been subjected to some discussion in *Nature,* a journal of more learned standing than ours, and have not, apparently, succeeded in convincing experts who are far better equipped to assess them than ourselves. Because *New Scientist* has no system for refereeing contributions we are always reluctant to champion points of view which, for technical reasons, we cannot evaluate. I agree that your treatment at the hands of the Royal Society and *Nature* appears tardy and that both should certainly be accountable to the public. However, I would take issue over the third point of your summary: 'refutation' of a theory surely depends on the consensus of scientific opinion? Professor McCrea has pointed out what he considers to be the fallacy in your disproof. May not the absence of further reaction from the scientific world simply signify silent acquiescence with his explanation? After all, scientists are not known for loquacity.
>
> That a simpler answer, fit for the man in the street, is not forthcoming may be, I feel, because relativity is too difficult an idea in itself.
>
> I am sure I have written sufficient to make it plain that we are not in a position to carry this issue any further, and I am therefore returning your article to you. I have certainly found it interesting reading, but I do not really fear oligarchy in the scientific world -too many of the things that matter are done outside the precincts of the scientific establishment and require the approval neither of the Royal Society nor *Nature.*

I leave it to the reader to contemplate this reply in relation to the terms of the unprompted invitation extended to me, and the view that ' "refutation" of a theory surely depends on the consensus of scientific opinion' in relation to Sir Henry Dale's somewhat different view, remarking only that, whatever the result of his contemplation, it is certainly *New Scientist's* view of the criterion by which theories should be judged that operates today. Whether 'nature's answers', which will decide his fate, will be determined by 'the consensus of scientific opinion' is another matter, which also will bear contemplation.

Several correspondents, including Professor Coulson and Dame Kathleen Lonsdale, had suggested that I should publish my criticism of the theory in the United States, where there might be a greater likelihood of its being considered with an open mind. I had not much hope of this, for English scientific journals of course circulate freely among American scientists, and I had in fact already made certain approaches there which had met with a similar response to that accorded to them in this country. However, it did at this stage, when I was able to put the essential point into a single brief question, seem worth while to put that question to American physicists directly, in case there was one among them who might agree with me that it deserved the very brief answer that would have sufficed. Accordingly, on 14 June 1969 I submitted the following short letter for publication in *Science,* the leading American journal devoted to general science:

> For many years I have vainly sought from British scientists an answer to a very simple but profoundly important question: may I, through the courtesy of your columns, lay it before American physicists ?
>
> Two exactly similar clocks, A and B, are in uniform relative motion. Einstein's special relativity theory requires (1) that the motion is wholly relative, i.e. it belongs no more to one dock than to the other; (2) that the clocks work at different rates, i.e. one works faster than the other. My question is: what, consistently with the theory, determines which clock works the faster?
>
> There is no subtlety of terminology here. 'Rate' is Einstein's word (in translation, of course), and has never, in any other connection, called for explanation. No acceleration is involved, the whole process concerned occurring while the relative motion is uniform. I take an example to avoid ambiguity. Suppose the relative velocity is 161,000 miles a second. Then, according to the theory, the time according to one clock (A, say) between the readings 1.0 and 2.0 o'clock of B is 2 hrs., so that A works twice as fast as B. This is a particular case of a general result obtained by Einstein in 1905 and universally accepted. But, similarly, the theory requires that the time according to B between the readings 1.0 and 2.0 o'clock of A is 2 hrs., so that B works twice as fast as A. (Einstein did not consider this case). These results are clearly contradictory.
>
> My conclusion is that the theory must be false, since it demands that each of two clocks works faster than the other, which is impossible. Otherwise, something must determine which clock *really* works the faster. What is that something? I ask authorities on the subject either to identify it in terms intelligible to anyone who can understand the question, or else to acknowledge that the theory is false.
>
> This is as momentous a question as any now facing physicists and the public generally. By common consent this theory is fundamental in modern physics, and not only profoundly affects our whole conception of world-structure and the nature of space and time, but also has the most serious implications for public safety. Modern experiments are such that, if based on false ideas, they must sooner or later produce unexpected results that might have tragic consequences for everyone. I therefore conceive it to be the duty of those whose pronouncements on the subject carry weight with high energy experimenters to answer this question as quickly, clearly and candidly as possible: I hope they will do so.

The letter, however, was rejected with the following brief note:

Thank you for your letter of 14 June.

We have consulted two distinguished physicists in this country who feel that your letter adds little to the discussion in *Science* of 1957-8.

Now this was clearly quite beside the point. It was an answer, not a question, that could alone add anything to any discussion, and this question could in any case bear no relation to whatever discussion the editor had in mind, for the question had not been asked in 1957-8. It is true that at that time there was a discussion in *Science* concerning the clock paradox (see chapter 9), but all who took part in that tacitly accepted the special relativity theory as valid, while the question now asked related only to its validity. Furthermore, even if the question had been answered in 1957-8 in *Science,* no one in all the recent controversy concerning it seemed to be aware of the fact, and it would seem obviously desirable that a leading scientific journal in possession of the knowledge that could end that controversy should publish the few lines needed to do so, even if it meant a repetition of what had appeared so long ago. Correspondence with the editor, however, produced no effect, and the letter was not published, nor was I informed privately what the answer was.

It would be both tedious and profitless to record other attempts to improve the situation through the medium of the Press that led to nothing: I pass to a most significant discussion in the *Listener,* which began with the publication there on 3 July 1969 of a broadcast talk of mine entitled 'Definitions and Realities', and the ensuing correspondence took a course that enabled me for the first time to make known certain aspects of the modern scientific movement that had been veiled so long in impenetrable obscurity, notwithstanding their vital relation to public welfare. I should like to pay tribute here to the *Listener* for allowing a freedom of discussion that I had not experienced elsewhere. The talk in question was concerned with a more general matter than the one with which we are here dealing, but it permitted — indeed, almost compelled — as a natural example the substitution in modern physics of mathematical definitions for experiences, and the consequent mistaking of mathematical truths for physical ones which in my view invalidated the special relativity theory. Its publication in the *Listener* started a series of letters which continued until 30 October 1969 and was concentrated wholly on the relativity question. At once two correspondents took me to task (how many more did so, of course, I do not know, but the letters of two were published in the same issue) for writing as I had done when I had failed to follow up in *Nature* 'the clarification which Professor Synge has achieved, to clinch the matter', as one put it. 'It seems to me,' wrote the other, 'it is Professor Dingle's clear duty to give an unambiguous answer.' This, of course, gave me the opportunity of explaining that my unambiguous answer clinching the matter had been languishing in *Nature* office (as it still does) for eleven months, and in the ensuing correspondence I was able to reveal other facts which have caused profound astonishment. I shall presently relate the more relevant consequences of this correspondence, but the only contributor whose letter calls for mention here is Professor McCrea, whom I have already mentioned in connection with the *Nature* discussion, and it is now convenient to revert to that before describing his letter in the *Listener.*

Professor McCrea is among the most distinguished of mathematical workers in the field of relativity. Although we differ profoundly in our whole view of this matter (he and I had been at variance long before in the 'twin paradox' controversy, when I as well as he believed special relativity to be valid), I cannot with hold recognition of his almost unique courage, among those leaders in the subject who do not accept my criticism of the theory, in publishing his reasons for dissenting instead of hiding them behind a veil of anonymity or refusing to say openly what he writes in private: I wish I could extend to those reasons the respect I hold for his courage. However, as I have said, the one tangible result of my circular to the Royal Society Fellows was the discussion between him and me which is reproduced in the Appendix. I now add a few remarks for those whose technical equipment is insufficient for me to leave it to speak for itself.

The substance of my criticism, as I have already said, is that although the theory is mathematically sound, the relation which it postulates between the mathematical symbols and clock readings (and, by inference, the readings of length-measuring scales) requires that each of two relatively moving clocks works more slowly than the other, which is impossible. Einstein himself had shown that one of the clocks worked more slowly than the other, but had not shown how that clock could be identified. His demonstration was represented by equation (3) of my article, and equation (4), derived by an exactly similar argument, showed that the slower clock by (3) was the faster by (4). The relevant point of McCrea's answer — it contained much unnecessary mathematics — was that in my paraphrase of Einstein's (and correspondingly in my own) argument I had used the phrase, '[the clock] A must be held to read t_1 at [the event] E_1' — evidently, as the context shows, in the same sense as one might say 'the pavilion clock must read 6.30 at the drawing at stumps'. McCrea maintained that this rendered my argument 'meaningless' because 'A is not "at" E_1' — as though the cricket rule was meaningless because the pavilion clock was not at the place where the stumps were to be drawn. Indeed, if this were a sound argument, it would clearly invalidate Einstein's argument as well a*s* mine, and so discredit the theory in a different way; but I think no one could possibly have been misled by such a 'refutation' had it lot been embedded in a lengthy mathematical matrix, including a wholly unnecessary 'Minkowski space-time diagram' to prove the impossibility of A ever being at the event E_1' and so playing upon :he innate conviction of most readers that the whole subject was a mystery comprehensible only to the mathematically initiated.

McCrea did not reply to my exposure of this device, but it served to 'convince' even Lord Blackett, as we shall see, that my criticism of the theory had been disposed of (pp. 99-100).

Naturally, in the *Listener* discussion I was careful to avoid the phrase that could be so misread, and instead (21 August, 1969) presented Einstein's proof from his theory that the readings of a clock P, passing along a row of relatively stationary synchronised clocks Q, fell steadily more and more behind those of the Q clocks as it went along. I then showed, in exactly the same way, that if P also was one of a row of relatively stationary synchronised clocks, each Q clock also must fall steadily behind the P clocks as it went along. Hence, as the motion progressed, every P clock was losing steadily with respect to the Q clocks, and *vice versa.* Einstein had not considered the second case, and so had not encountered the contradiction: he merely concluded, from the first alone, that P worked steadily slower than any arbitrarily selected Q clock, for the Q clocks, being synchronised, all worked at the same rate. In other words, we have the same result as before: two relatively moving clocks, P and Q, work at different rates, but the same reasoning that requires P to work more slowly than Q also requires Q to work more slowly than P.

McCrea's reply to this in the *Listener* (4 September 1969) was astonishing. He claimed that Einstein had never compared the rates of two relatively moving clocks: he had considered, said McCrea, only the case of one clock passing along a row of relatively stationary clocks, and shown that it fell steadily behind them, but had not inferred from this anything about the relative rates of one P and one Q clock. What he understood it to mean to say that the Q clocks were 'synchronised' he did not explain. 'The assertion,' he wrote, 'that, according to the theory, a moving clock appears to go slow ... is admissible, provided we remember that the statement concerns the behaviour of one clock (here called the moving clock) as compared with a set of clocks... Dingle's 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... If we thus say that, according to relativity theory, a moving clock appears to go slow, then we are not making a symmetric comparison of one single clock with another single clock.'

It is hard to know what comment to make on this: even Mr. Maddox, the editor of *Nature* (who had based his editorial, given in the Appendix, on McCrea's quite different 'refutation' of my criticism) had to write to me, 'I agree with you that what McCrea said is mystifying'. That is hardly the word I should use, but ' 'tis enough, 'twill serve'. In my statement on p. 46 I have quoted Einstein's comparison of one equatorial with one polar clock, and if I were interested in relating instances of inconsistencies in McCrea's statements (which I am not; I am concerned only with the validity of the special relativity theory) I could quote similar comparisons of his own. But it would be unpardonable if I were not to quote a passage from Einstein and Infeld's book. *The Evolution of Physics,* which is so apt that it might have been written in anticipation of this misunderstanding by a reader unacquainted with the theory, for whom the book was written: they write, using 'C.S.' for 'coordinate systems':

> When discussing measurements in classical mechanics, we used one clock for all C.S. Here we have many clocks in each C.S. This difference is unimportant. One clock was sufficient, but nobody could object to the use of many, so long as they behave as decent synchronised clocks should.'

'One clock was sufficient': they half apologise for introducing the set which, according to McCrea, is all that the theory is talking about.

The general import of all this may be summed up thus. McCrea and Synge are, by common consent, two of the leading mathematical authorities on relativity. McCrea has given two totally different 'refutations' of my criticism of special relativity and Synge a different one again. In none of these 'refutations' is there any answer to the question I put, that can be applied to Einstein's own examples such as that concerning the equatorial and polar clocks, yet without such an answer the theory is clearly false. My closing letter to the *Listener* (30 October) was as follows:

> In closing this valuable discussion, may I, avoiding further controversy, state two indisputable and vitally important facts which it has elicited? 1. I have asked for an answer in one sentence to the question: What is it, on Einstein's theory, that distinguishes which of two similar relatively uniformly moving clocks lags behind the other (to use the translation of Einstein's own words) by an amount that increases regularly with time? Lorentz answered: its velocity through the ether, the faster moving clock working at the slower rate. Ritz answered: nothing, for there is no lag. Neither of these answers is possible to Einstein, and no one has told me of a substitute that is permitted by his theory, despite my repetition of the question and the fact that without an answer the theory is invalid.
>
> 2. In view of the widespread and growing disquiet at the continued use of Einstein's theory 'every day in the most dangerous operations yet devised by man', notwithstanding that the above question has been continually asked and remained unanswered for nearly n years, I asked the President of the Royal Society *(Listener,* 21 August) to reassure the public that this fact was consistent with the continued preservation of integrity among scientists. He has told me in a letter that he is not prepared to give such an assurance.

The last paragraph requires explanation. Before giving it, however, (see p. 100) I will record one more attempt to get the importance of this matter realized by scientists and a settlement of the question arrived at. It was made possible — or seemed to be — by the Presidential Address to Sections X and N of the British Association in 1970, on 'Some Pathologies of the Scientific Life', by Professor J. M. Ziman, F.R.S.[4] This seemed particularly timely because the British Association is probably the most widely recognised medium between scientists and the British public, and therefore the most suitable agency for resolving the matter with which we are here concerned. Professor Ziman is himself a physicist of distinction who has taught relativity, and the subject of his Address was precisely that which this discussion is all about — the moral aspect of scientific research. No more favourable opportunity could have been looked for than this, for reaching a satisfactory conclusion.

The omens were propitious. Professor Ziman in his Address had assured the public of the 'fierce and uncompromising honesty' which was 'one of the standard attributes of the so-called "scientific attitude"'; he had said that scientists 'act in the expectation that their contemporaries will behave according to certain conventions. Any serious breach of these conventions is a pathological symptom, deserving our attention', and I could not doubt that he would have regarded the 'conventions' described by Sir Henry Dale (p. 23) as prominent among those which his audience would associate with dentists; he had said also that a scientist, 'if he has studied in a good institution, will have internalised very high standards of honesty, scepticism and criticism, so that he will never find it easy o let his mind slide over difficulties and objections'. Notwithstanding this, however, he did enumerate some minor foibles of present-day scientists, though his summing-up was this: 'Let me say, then, most emphatically, that I do *not* believe that the internal state of the scientific community is desperately unhealthy. Some of the phenomena to be discussed are mildly scandalous, but they are mostly rare exceptions that "prove" the rules.'

It seemed to me evident from this that Ziman was not familiar with, at any rate, most of the facts that I have related in the previous pages (I may say that he happened not to be among the Fellows of the Royal Society to whom I had sent my circular),' so I at once wrote him giving an outline of those facts, which seemed to me to denote a 'pathology' far more serious than those he had mentioned, and soliciting his aid in effecting a cure within the scientific world itself, so that it would not be necessary for me to expose the whole matter in a book. I was encouraged in so doing by the fact that he had spoken in his Address of 'the absolute trust that we have in a reputable fellow scientist', and I had a real hope that, after all, the fateful task that would otherwise be inescapable, and by this time had seemed most likely to be so, might indeed become unnecessary.

Ziman replied in friendly terms, implying that I had indeed exemplified the picture of the scientist that he had presented, and commenting on the relativity problem itself; he added some remarks in the contrast between the duties of individual scientists and scientific societies in the matter, as he understood them. He did not, however, in defending the special relativity theory, make any attempt to answer the crucial question on which everything defended, and concluded: 'I am really rather sorry that I cannot be more helpful, for iconoclasm is my favourite sport, but honestly, I don't think I can go along with you in this one.'

I replied at some length, explaining and stressing the importance, from the point of view of scientific honesty as well as that of public safety, of extracting an answer from physicists, in a single sentence, to my simple question, and adding:

> I am sure that at bottom you are as desirous as I am that the pathological state of the scientific world shall be as healthy as possible, and I hope you will regard this letter as a proposal of collaboration towards that end whose unfortunate form is made necessary by antecedent circumstances which have regrettably to be recognised as actual and which determine the course to be taken.

I begged him not to make it necessary for me, in the public interest, to make public yet another lapse from the 'fierce and uncompromising honesty' which he had assured his audience was characteristic of scientists. He replied:

> I regret that I must disappoint you. I am certainly not prepared to enter into a correspondence in which you claim the right to quote me to others without reservation. I admire the energy, integrity and enthusiasm with which you put

> forward your point of view, but I take leave to remain unconvinced and the freedom to turn my mind to other matters where scientific argument may prove more fruitful. Perhaps, in the end, you will have been proved right and I, with all my colleagues, wrong, and a sorry lot of fools we will seem. However, life is full of such gambles and I am prepared to take my chance on it.

Again I must leave the reader to judge whether gambling on a matter clearly a subject for decision by the application of scientific principles can properly be described as 'fierce and uncompromising honesty'.

I return now to the consequences of the discussion in the *Listener* where, for the first time, it had been possible to make generally known some of the facts concerning this matter. As I have said, this aroused astonishment in a number of readers, expressed to me in private letters and otherwise, which it would be tedious to summarise. I restrict my account to a few salient points which will sufficiently indicate the essence of the situation.

The editor of *Nature,* to whom I sent a copy of the whole correspondence (which concluded; as I have said, in the issue of 30 October 1969), wrote me on 24 November 1969 as follows:

> What I now propose to do is to write a long leader summarising the position. We shall publish this before the end of the year and although I shall refer to your correspondence in the *Listener* and also summarise your view as expressed in your latest reply, I do not think that deserves publication in full. Naturally I shall let you see what I write before it appears.

(I presume that 'your latest reply' means my long suspended reply to Synge; if not, I do not know what it means). In the meantime I had had an interview with Lord Soper, who had kindly taken an interest in the matter, and in view of the letter from Mr. Maddox, which arrived immediately before that interview, it was agreed to wait for the appearance of the promised leader before considering the matter further.

The leader, however, did not appear before the end of the year. As I had informed a number of enquirers of Mr. Maddox's intention, the perplexity aroused in them by its non-appearance led me to write him a few weeks later, asking when it might be expected. He replied on 21 January 1970 that "the article you mentioned is now almost ready'. It still, however, did not appear, and towards the end of March Lord Soper wrote to Mr. Maddox with a further inquiry on the matter, and received the reply that it would be 'a week or two' before the article was ready for publication. More than a week or two elapsed, however, without any sign of it, but the forthcoming election held matters up for a while, and it was not until 6 July that Lord Soper made a further inquiry. To this he received no reply. The article has still not appeared, nor has any reference at all to the issues raised in the *Listener* appeared in *Nature.*

These are the bare facts of the matter so far as *Nature* is concerned, though I should add that, after hearing that Mr. Maddox intended to publish a leader, I wrote him pointing out that unless it contained either a clear answer to my crucial question or an acknowledgement that, since none was possible, the theory must be abandoned, as Synge had stated, it would achieve nothing. The reader must interpret these facts for himself, but it is only fair to point out that it is utterly impossible for any human being to write authoritatively on the whole field of science which *Nature* must cover, and an editor is compelled to seek advice from experts on at least most of the matters with which he must deal. It is therefore at least a possibility that when Mr. Maddox promised to write his leader, he wrote in confidence that his experts on this subject would be able to provide him with an answer to my question. The fact that the leader has not appeared invites the speculation that they have not been able to do so. If that is so the implications are obvious, though the reader must judge whether such an eventuality ought to lead to the non-appearance of the leader or to determine its character. If the speculation is beside the mark, the question why *Nature* has still not fulfilled its promise is completely open.

As I have said, there would be little point in recording all the reactions to the *Listener* correspondence that reached me, but it is instructive to give one because it emanates from a reader with undoubted sense of responsibility and ability to form a balanced judgment. In its general character it is typical of others, and it summarises some of the main points brought out in the correspondence which it is impossible to give in full. On 19 August 1970 the Rev. Dr. W. J. Platt, formerly General Secretary of the British and Foreign Bible Society, submitted the following letter, of which he has kindly sent me a copy, for publication to *The Times:*

> In the *Listener* last year there appeared a long correspondence following an article entitled "Definitions and Realities' by Prof. H. Dingle, which was published on July 3. In its course, certain alleged facts transpired which, if true, are manifestly of public concern. I have been waiting for some authoritative statement showing either that the assertions were unfounded or that steps were being taken to rectify a dangerous situation. As far as I am aware, none has appeared, and the implications of the matter seem so serious that public interest demands one without delay.
>
> Prof. Dingle, who, I believe, is recognised as a leading authority on Einstein's special relativity theory, on which physicists acknowledge that they rely, has advanced what he claims to be a fatal criticism of that theory. On such a matter the layman is, of course, not qualified to speak: he is, however, entitled to an assurance that the scientific world remains true to its principle of answering or accepting informed criticism. This appears to be not only, as it has always been, a moral duty of scientists, but in these days, when the experiments performed are of such enormous potential danger, a necessity. According to the uncontradicted assertion in the *Listener* of October 30 last, however, the President of the Royal Society failed to give an assurance that scientific integrity is still preserved. If earlier statements in the correspondence are true, he could hardly, of course, do so.

May I give a few of these statements ?

(1) Some of the most eminent workers in modern physics have admitted privately that they either do not understand the theory or regard it as nonsensical: nevertheless, they continue to teach it to students and to use it in high energy experiments.

(2) It is stated that the Royal Society has declared privately that Prof. Dingle's fallacy is 'too elementary even to be instructive', but the Society has not stated what the fallacy is, and the journal *Nature,* which had previously published the criticism without eliciting a refutation of it, has refused to publish a letter from Prof. Dingle, asking that the Royal Society shall state the fallacy.

(3) *New Scientist,* after asking Prof. Dingle to write an article on public dangers inherent in modern scientific research in which he would "not be restricted in any way', refused to publish the article offered, which stated these and similar facts, on the ground that 'refutation of a theory surely depends on the consensus of scientific opinion' — not now, it seems, on reasoned argument.

(4) After correspondence between Prof. Dingle and Prof. J. L. Synge, who, I understand, is an acknowledged mathematical authority on relativity, the latter in a letter published in *Nature,* agreed that the point at issue was not an abstruse mathematical one but concerned only the possible behaviour of clocks, and Synge 'cast his vote' for relativity. It is accepted that relativity, which concerns itself with matters of space and time, must be dependent on measurement of time, i.e. on clocks. Dingle replied that the matter was not to be decided by voting and that his demand of one clock was that it should not work both faster and slower at the same time than another. This reply was not allowed publication in *Nature,* a fact which led two correspondents in the *Listener* to assume that Dingle had not replied.

The situation thus disclosed, if the facts are as stated, is alarming. According to Dingle's closing letter (October 30) all that is required to settle the matter is an answer to the question: What is it, on Einstein's theory, that determines which of two clocks, relatively moving uniformly, lags behind the other, as Einstein says. Dingle's contention is that to be true the theory demands that the clocks must work faster and slower at the same time! It is therefore untenable. I repeat, Sir, that I make no attempt to judge the issue, but ask, in the public interest, since the foregoing assertions have been published and remain uncontradicted, that an authoritative and conclusive assurance shall be given that scientific integrity continues to exist.

Dr. Platt received a reply at once that the letter was under consideration. As, several weeks later, it had not appeared and he had heard nothing further, he wrote asking if a decision had been reached: he received no reply to this enquiry. The letter has not been published.

5

Attitude of the 'Elder Statesmen'

The personal examples I have so far given from physicists and mathematicians, though all from those of repute, are (except, of course, that from Professor Max Born) from those whose training in the subject took place at a time when special relativity had already become an accepted part of physics. As will be explained in Part Two, those who learnt physics from the 1920s onwards were presented with a metaphysical interpretation of the mathematical equations as though it were a necessary requirement of those equations and so possessed the logical necessity of pure reason. The *physical* impossibility of that interpretation was accordingly obscured. The theory was therefore, so to speak, embedded in their minds as a necessary truth, rendering them incapable of separating the speculative (and indeed impossible) physics and the superimposed metaphysics from the irreproachable mathematics of the subject, and leaving them unaware, for example, that two quite different physical interpretations (see Chapter 8) — those of Lorentz and Einstein — were equally compatible with the same mathematical structure and the same apparently confirmatory experimental facts. It is therefore readily understandable — which is not the same as justifiable — that they found, and still find, the difficulty of discarding the physical theory, without denying the inescapable necessity of mathematical truth, practically insuperable. Hence the present multiform graspings at any device for escaping the obvious impossibility of each of two clocks working faster than the other.

It is otherwise with the older physicists. These were sufficiently grounded in the fundamental principles of science to realise that the new conceptions — space turning into time, and so on — were meaningless, but they could not challenge them without facing the counter-challenge of giving a better interpretation of the mathematics. This was easy enough with special relativity alone — Lorentz, in fact, had done it, and, as we shall see, from 1904 until 1919 the 'relativity theory*1 was generally ascribed to Lorentz, not to Einstein. But with the apparent success in 1919 of Einstein's *general* theory with its then quite new and terrifying mathematical machinery of tensor calculus, came the fatal climax. Almost overnight 'the relativity theory of Lorentz' became 'Einstein's special relativity theory', and it was immediately hailed as such by the mathematical experts. The established physicists, therefore, had to face the alternatives of accepting, without understanding, the metaphysics of the newly christened 'Einstein's special theory', or mastering tensor calculus sufficiently to show that the so-called general relativity theory was not necessarily a generalisation of the earlier Einstein form of the 'relativity theory', and therefore carried with it no justification of 'Einstein's special relativity theory' (this is explained in detail in Part Two). They chose the former alternative. They gave up trying to understand the whole business, surrendered the use of their intelligence, and accepted passively whatever apparent absurdities the mathematicians put before them.

They had the seeming excuse that the mathematical equations worked. They could use their accustomed electromagnetic equations, which by themselves gave the wrong experimental results, and apply 'the relativity correction', whereupon they gave the right experimental results. They accordingly ignored the physically intelligible (though, of course, not necessarily true) interpretation given by Lorentz — that the electromagnetic equations were incomplete since they failed to include a postulated effect of the ether on bodies moving through it — and simply went on with their experiments, accepting and confessing their inability to make any sense of waves interfering with one another in a strictly specified way in a medium which nevertheless did not exist, and other such mysteries, and leaving the mathematicians free to propose any interpretation they wished of their mathematical symbols, regardless of physical absurdity.

This, as I say, is explained and corroborated in detail in Part Two. In this chapter my task is to show the reaction to my criticism of special relativity of those to whom I have just referred, who in their early days yielded their intelligence, which showed them plainly that the new conceptions were physically meaningless, and left the field open to the 'mathematicians'. A few of these men are, after 50 years, still alive and have attained positions of great eminence in the scientific world. I select two whose reactions I record in detail and with evidence drawn from their own statements, so that there can be no question of my misrepresenting them. My purpose, I repeat, is not a personal one (indeed, I have no right to blame them, for I myself was for long at fault for failing to recognise that the mathematics of the time was simply another form of mediaeval logic restored to its old position of authority over experience. I offer no excuse for this, nor do I regard my own intellectual pilgrimage as of sufficient general importance to relate here the course it took and how later I awoke from my dogmatic slumber. Shutting the eyes when the fact is pointed out is, of course, quite another matter), but solely that of showing, beyond all possibility of doubt, that the now prevailing state of mind in the world of physical science, which controls the future of our material civilisation, is directly opposed to the moral principles of science, and is fraught with the greatest danger to the future of mankind. The two I select are Lord Blackett, lately President of the Royal Society, and Sir Lawrence Bragg, formerly Head of the Cavendish Laboratory at Cambridge and Director of the Royal Institution.

I have related (pp. 55-0) my failure, as well as that of Professor Coulson, to find anyone willing to read the paper which I had prepared for submission to the Royal Society. Among those whom I had approached was Professor P. M. S. (later Lord) Blackett, who afterwards became President of the Royal Society. There was more than one reason why he was an appropriate choice. In his student days he had worked under — and, like every other physicist, had the highest respect for — Lord Rutherford, who could be more accurately described as scornful than as critical of the relativity theory; he was a specialist in high energy physics, a field in which a failure of the theory would have the most serious consequences; he was then Head of the Physics Department at the Imperial College, where I had worked for more than 30 years, though I had left before his arrival there; and I had heard independently that, when my discussion with Born appeared in *Nature,* he had pointed out to his mathematical colleagues that this matter demanded attention. I therefore asked him if he would read my paper and, if he thought it worthy of consideration, submit it to the Royal Society for publication. His reply was as follows:

> I am afraid I cannot help. I am naturally interested in relativity, particularly as I have taught the special theory for many years to our first-year students, and of course anybody dealing with cosmic rays or high energy particles uses it every day; one could not think accurately about any of these phenomena without using it. Moreover, if there had been no general argument in its favour, it would have been deduced from the experimental results on high energy particles. However, on the subtle point you are interested in I am afraid I have no contribution to make. I have often consulted my theoretical colleagues on this question, and can find none of them who has serious doubts about the ordinary formulation. I confess I cannot completely follow the details of yours and Max Bern's argument, or rather, I have not had the time or inclination to do so. But to my superficial knowledge of the subject, there is nothing obviously wrong with the ordinary formulation.

It was impossible to interpret this as other than a refusal to read my paper, and, notwithstanding the shocks I had already received to my one-time naive belief that scientists did at least try to conform to the image represented by Dale's description, I must confess that I was not prepared for so radical a departure from the ideal as this. A leader in the most highly dangerous field of scientific research then existing was not prepared even to look at a criticism of a theory fundamental to that research which he was using every day; and had been for many years 'teaching' to students, shortly to be entrusted with the use of instruments employed in that research, principles of which he had only 'superficial knowledge' and which he had neither time nor inclination to examine. He had merely accepted uncritically the opinions of others who had only theoretical (which in this case meant mathematical) acquaintance with the theory, while the essence of my criticism, as he would have seen if he had consented to look at it, applied not to the mathematics but to the physical interpretation of the mathematics.

However, shortly after this Professor Blackett became President of the Royal Society, and in that capacity he had, of course, a new responsibility, over and above that resting on a practical physicist as such — namely, that of ensuring that scientific research in this country was conducted in accordance with the principles on which the Royal Society was founded and to which formally it still adhered. These certainly included the critical examination of the fundamental theories of physics, so I wrote a very short paper, setting out the bare essence of my criticism of special relativity, in much the same terms as those given on p. 45, and asked Professor Blackett — not now as a physicist but as President of the Royal Society — to submit it to that Society for publication. He replied as follows:

> I am sorry I have not replied earlier to your letter of 14 February 1966 but I have been making various enquiries. I am very sorry to say that I do not feel able to communicate your paper for publication. I have looked back at a lot of the old correspondence about other discussions between you and Officers of the Society on related ideas. This confirms my decision.

> For one thing I have a firm rule not to communicate papers which I do not fully understand and agree with. Now I am no relativist, that is except in the sense that all we practical high energy physicists are, and have not the time nor the ability to discuss fundamentals. There are, of course, in this country quite a number of people who have expert knowledge of these fundamental, logical and experimental phases of relativity. Unless you can find at least one of them to sponsor your idea I do not see how the Royal Society can publish your paper.
>
> I am really sorry about this but I do feel that if you were in my position you would take the same view. With many regrets.

I of course at once disclaimed the final statement: I could in no circumstances have taken the view expressed by Blackett had I been in his position. It is not necessary to explain why; I am merely recording the course of events, so that the reader may form his own opinion of them. But there are two points which I should mention since they are not evident from the contents of this letter alone. First, when Blackett refers to the discussions between me and the Officers of the Society on related ideas, he can have seen only the referees' reports on my papers and possibly my replies to them; he cannot have seen the papers themselves because they were returned to me. Since comments without knowledge of the material on which they were made must necessarily be beyond possibility of appraisal, I cannot see how they could have confirmed his decision regarding this new paper unless that decision also was arrived at without reference to its contents. Secondly, when he says that 'all we practical high energy physicists have not the time nor the ability to discuss fundamentals' ('fundamentals', of course, meaning here what the 'mathematicians' proclaim as such) he is stating, as I can confirm from many years of experience, nothing but the simple truth. That is one of the basic facts that have made this book necessary. The 'experimenters', as I have called them, with scarcely one exception, if any at all, simply do not understand the principles on which their experiments are based: they blindly accept whatever the 'mathematicians' tell them, even though, as Dame Kathleen Lonsdale acknowledged with more frankness than most, it seems 'esoteric nonsense'. How could it be otherwise when they are taught by those with only 'superficial knowledge'? The difference between the more responsible, first-class minds — like those of Lord Blackett and Dame Kathleen Lonsdale and Sir Bernard Lovell who wrote me: 'I have never been one of those who pretended to understand either the theory of relativity or its implications' (notwithstanding that it is profoundly related to theories of cosmology, with which radio-astronomy is largely concerned) — and the general run of 'experimenters' who form the great majority, is that the former have the insight to perceive and the candour to acknowledge the fact (would that the revelation would come to them that the theory appears to them to be nonsense because it is nonsense and not because they are too stupid to understand it!) while the latter utter meaningless phrases like 'time dilation' and think they are saying something profound. It is a vast difference, but unfortunately one to which nature will pay no heed at all. Her response to the questions asked her in the experiments performed will be exactly the same, whoever performs them.

The next point at which Lord Blackett became involved in the matter occurred during the course of the *Listener* controversy. In view of certain facts which there transpired it became necessary for me to write (issue of 21 August 1969):

> In these circumstances it is my right, and dear duty, to ask the President of the Royal Society, the body unquestionably responsible to the public in such a matter, to inform your readers what it is doing to allay the natural and fast-growing suspicion of the integrity of scientists which my large correspondence reveals, and to prevent this pre-Aberfan mentality from ensuring a super-Aberfan outcome.

On this I received the following letter from the President:

> I am afraid I have nothing more to say about the relativity question. As far as I am concerned, the two papers we arranged to be published in *Nature* by you and by McCrea adequately convince me of the correctness of the conventional view. I do not intend to take any further action.

But I had not asked Lord Blackett for his conclusion concerning the conventional view on the relativity question: I had asked the President of the Royal Society for an assurance that the existing suspicion of the integrity of scientists was unfounded; and since this was the only reply I had received to that request, I could do no other than write as I did in my closing letter (p. 86) to the *Listener,* which naturally has aroused the misgiving among readers of that correspondence expressed in Dr. Platt's letter (p. 91). That letter, as I said, was unpublished, but I have ample evidence that the misgiving remains and is only too well founded.

I turn now to the second of my 'elder statesmen'. Sir Lawrence Bragg.[*] His work has not been directly related to special relativity, but, like all modern physics, it is no less dependent on it although the relation is much more indirect than that of Lord Blackett. Sir Lawrence's life work, which has been concerned with the interaction of X-rays (electromagnetic waves) with matter, is inseparably connected with the Maxwell-Lorentz electromagnetic theory, and the whole purpose of Einstein's special relativity was to save that theory in the face of apparently fatal experimental test. 'The special theory of relativity has rendered the Maxwell-Lorentz theory plausible,' wrote Einstein,[1] and he several times repeated the assertion. Clearly, then, according to Einstein, if special relativity is wrong the Maxwell-Lorentz theory, which is the basis of Sir Lawrence Bragg's work, fails, so its tenability is a matter of vital importance to his chief scientific interests. However, it is true that, in the purely practical field, where division of labour becomes more and

more unavoidable every day, the credentials of special relativity are not a matter which it is in any way incumbent on him to examine in detail, and herein lies the chief significance of the fact that I did not — as a last resort, as I told him — bring the matter personally to his notice (though I have no doubt that he was aware of the controversy)

[*] See Introduction, p. 20. [p. 9 of this file]

until I had failed with all the specialists in the subject whom I had approached. For, as I have said and cannot repeat too often, the real gravamen of this matter — notwithstanding the extreme seriousness of the special relativity question in itself — is not the truth or falsity of a particular theory, but *the moral attitude of scientists to the great responsibility that now rests on them,* and in approaching Sir Lawrence Bragg last, and laying a representative account of the way in which my criticism had been received by those whose direct duty it was to appraise it before one who had no such duty (I did, in fact, enclose a fully representative account of the behaviour of the individual authorities, journals and societies which is given in the earlier chapters of this book), I was adopting, as I thought, the most effective way of presenting the moral issue without the risk of its being mistaken for a technical one. Deservedly, no living person stands higher in general estimation as an embodiment of the scientific ideal throughout most of this century, so I sent him a letter from which I quote the following extracts, omitting only passages which would be merely repetitions, in other words, of what I have already given here:

> In these circumstances I realise that I have now reached a stage at which — failing this final appeal to you as almost, if not quite, the only remaining older physicist of outstanding distinction who has not lost the ideals on which those of our generation were reared and saw exemplified in their seniors, and who has an influence far beyond mine with those to whom it should be a matter of course to see that the one necessary sentence which I ask for is provided or its impossibility, with all the necessary consequences of that, frankly acknowledged — I can no longer postpone a duty which I have tried for years to avoid; namely, the publication in a book, with all the clarity and unqualified starkness that I can command, of the whole disgraceful story, from the beginning to its state at the time of writing, of which the enclosed is a small but typical part, and so inevitably bring shame upon those now most honoured in the scientific world. This I have been urged to do for a long time by distinguished interested but scientifically uninfluential followers of the controversy, and since the *Listener* correspondence referred to in the enclosed, money has been offered, unsolicited, by astonished and indignant readers to support the publication of such an account if financial assistance is needed, but I have shrunk from so distasteful a course, perhaps longer than I should have done, in the vain hope that something would happen to bring to responsible physicists a realisation of what they are doing. I am now too old and conscious of failing physical powers, and have had too much experience of the futility of every other effort I can devise, to feel justified in delaying longer. You will realise, therefore, that this letter is a final appeal to make a complete exposure of the most inglorious phase in the history of science (this is not rhetorical; I say it deliberately, with some knowledge of that history) unnecessary.
>
> …
>
> Therefore I venture, with all respect, to ask if you would use your influence to persuade Blackett to take steps to see that my obviously legitimate question is clearly faced in *Nature* — honestly, straightforwardly and promptly, with a complete and clearly evident avoidance of all evasion and subterfuge —

> and that a clear and convincing answer in a single sentence (it could be elaborated afterwards to any extent thought necessary) is given to it; or else an equally clear and convincing acknowledgement is made that, since no answer is possible, the special relativity theory, in Synge's words, 'must be abandoned'. Science is now at the crossroads, and the behaviour of Blackett and *Nature* in this crucial situation will determine whether its future will be as Dale described its past, or as McCrea, Bondi and others are acting, or failing to act, in the present. It is a humiliating exposure of the depths to which we have sunk that what I have to plead for, as the culmination of 13 years of world-wide vain effort, is not some unprecedented, abnormal act, controversially called for by exceptional circumstances, but simply what is always understood by everyone to be the normal, everyday routine of scientists — just open-minded and honest attention to legitimate criticism of a theory, followed by refutation or acceptance of it.
>
> …
>
> If, therefore, you can do something that will attain that end, by any legitimate means at all, in such a way as to enable me to retire into obscurity and leave the full disgrace of the past unrevealed, I shall be more thankful than I can say.

Sir Lawrence's immediate reaction was what I had feared, though I had hoped against hope that it might be otherwise. He at once returned the papers I had sent him, with the following letter:

> Thank you for your letter of 21st November. I cannot help because I have never had any claims to be an expert in the relativity field. I could not therefore venture to criticise or try to distinguish between the two sides. I do appreciate, however, your sending your notes to me.
>
> I am keeping the letter but sending back your typescript in case it is useful to send to people who can be more helpful than I can.

As in every such case, the word 'relativity' had produced the familiar conditioned reflex. It would be almost an impertinence to say that Sir Lawrence Bragg is far more than intelligent enough to realise immediately that a theory that requires one clock to work steadily both faster and slower than another, must be wrong, and that if special relativity is to be acceptable it must be defended against the charge that it requires such an impossibility. Yet, such is the state to which even the leading physicists have been reduced, that the mere mention of the word 'relativity' makes it impossible for him to perceive such an obvious fact. It at once conjures up a dread image, compelling an instant, unreasoning retreat, and automatically transforms a simple ethical question into the semblance of an esoteric intellectual one. However, I replied as follows:

> I am very sorry that, despite the care I took, I did not succeed in making it clear that the question on which I wrote was a moral, not a technical, one; yet, on

re-reading my letter I do not see how I could have made that clearer. The fact that, nevertheless, a request for assistance in restoring integrity in science can be read as a request to 'try to distinguish between the two sides' on a particular scientific point I can only regard as one more example of the evil spell cast by the word 'relativity' — a word that immediately reduces the mental power of even leading physicists to impotence and is the greatest stumbling-block to my efforts to bring home to them the extreme seriousness of the state to which we have been reduced. Apart from that word-magic, there is nothing in the whole course of events which I related which might not have happened if 'crystallography' had been substituted for 'relativity': it is just a historical accident that Einstein's theory caused, or showed up, the corruption.

I profoundly regret, therefore, that I shall now have to proceed with the book I mentioned, and make it as plainly as possible an indictment of the scientific community by presenting its moral degradation and indifference to its responsibility to public safety in such a way as to make it impossible for the situation to be obscured by technical considerations. As a necessary part of that I cannot avoid including this correspondence, on account both of your scientific eminence and of the unmistakably clear contrast which it affords (to anyone but a physicist) between the nature of my request and the character of your reply. I therefore return the statement which I sent you, for I must have complete justification for my assertion that your satisfaction with the present moral situation is that of one fully acquainted with the facts which the statement presents. The promptness with which you have returned it makes it at least possible that you have not yet read it with the attention necessary to appreciate its import, and I must leave no room for doubt on that point.

I need not repeat with what reluctance I do this, but I have no alternative now. I should be culpable in the extreme if, with the experience that has been forced on me, I let the public any longer remain in ignorance of the measure of trustworthiness to which scientists have shown themselves entitled to retain their present uncontrolled power over its safety. What the outcome will be I cannot, of course, foresee, and for that I am not responsible; but I am responsible, first, for trying to awaken scientists to a realisation of the state into which they have lapsed, and then, that having failed, for making generally known the whole truth on a matter of such transcendent importance, and then leaving the public to react with a full knowledge of the facts as they are. With the deepest regret.

To this Sir Lawrence replied a month later (after an immediate brief acknowledgement which I took to imply that he would make some enquiries):

It seems to me that you have had a very fair and patient hearing from a number of people who are competent experts. I trust their judgement and I think no useful service to science is done by reopening the correspondence. I think it best to be frank.

I need not quote my brief acknowledgement, for I can comment here at greater length — thus, I hope, revealing the implications of the exchange with correspondingly greater clarity. I would direct attention to two points.

First, Sir Lawrence's sole concern is with the fact that I have had 'fair and patient hearing' by other people — which I have never denied. Indeed, I have little doubt that they have tried long and patiently to think of a way of dealing with the problem I have set them. On the vitally important fact — that I have had no reply to my patiently heard question, which could have been given in a sentence if a reply had been possible — he makes no comment at all, and there is nothing to suggest that the need for a reply has occurred to him.

The second point is embarrassing, and I would willingly omit it but for the fact that it is compulsory for me to present the situation faithfully and completely, no matter what that might involve, and the tenor of Sir Lawrence's letter, which is that of one written to a misguided, though perhaps well-meaning, ignoramus whose delusions have received sufficient, if not over-generous, attention, forces me to state the following facts. I cannot, of course, compare my qualifications directly with those of the competent and trustworthy experts, for they are unnamed, nor can I defend my judgement against theirs, for I cannot by any means ascertain what the grounds for theirs may be, and I am not allowed to ask for them through the Royal Society or *Nature* or any other person or agency that I know of; I am told only that my fallacy is so elementary that it is not even instructive and that the Royal Society would make itself ridiculous if it published the grounds of my criticism. In' these circumstances I can only give some of the reasons why I think my criticism merits more serious notice than Sir Lawrence seems to consider adequate.

To the best of my knowledge there is no one now living who can give objective evidence that he is more competent in the subject than I am, and I can only conjecture Sir Lawrence's reason for regarding as untrustworthy my judgement on a matter on which he disclaims all title to form a judgement of his own. I have been studying relativity for more than 50 years. I learnt it in the first place from the late Professor A. N. Whitehead, who encouraged me in 1921 to write my first book on the subject *(Relativity for All* - Methuen) and read the typescript of that book before it was published. During the following half-century I have studied intensively the field of investigation to which it belongs, and discussed the theory with practically all those physicists whose names are best known in connection with it — Einstein, Eddington, Tolman, Whittaker, Schrodinger, Born, Bridgman, to name but a few: I knew some of them intimately. I worked for a year (1932-3) with Tolman while he was writing his now standard work, *Relativity, Thermodynamics and Cosmology* (Clarendon Press), and he went through the MS with me and included in the book what he called 'Dingle's Formulae' which I worked out for him. When, in 1940, I published my second book on the subject (*The Special Theory of Relativity* — Methuen), now in its fourth edition and still much used in universities in this country and in America, Max Born wrote me: 'I have enjoyed it very much, as your explanations of the difficult subject are very clear and well presented. I hope the book will find many readers.' When, some 20 years ago, Whittaker, who had direct, first-hand knowledge of the origin of the theory, published his history of the whole field of thought of which special relativity forms a part — now recognised as the standard work on the subject — I sent him some comments (on matters of substance, not mere typographical errors), to which he replied: 'Many thanks for the corrections and comments. You have detected several mistakes which had been missed by my two proof-correctors and myself; and some of the remarks and suggestions you make could have originated only from a vast background of knowledge, which fills me with admiration.' When the volume on Einstein in *The Library of Living Philosophers* (published in 1949) was prepared, there were only two Englishmen among the twenty-five contributors selected from the world; I was one: the other has long been dead, so he could not have been one of the 'competent experts' whose judgement Sir Lawrence Bragg trusts. When Einstein died I was summoned to broadcast a tribute to him on BBC television, which I did. Later, Granada television invited me to give a course on relativity, but by that time I was fairly well convinced that the special theory was untenable, so I refused. There are two articles on the subject in the *Encyclopaedia Britannica,* one by an American and the other by me. It was written before I had found reason to reject the special theory, and when recently I was asked to revise it for the forthcoming new edition I refused because I felt that my then unorthodox views made it undesirable for me to write, for a publication of that kind, the only article I could honestly write. The editors, however, would not accept my refusal, but agreed to my writing on the subject as a controversial one and increased the length originally assigned. On that understanding I agreed, and the article is now in print.

I could continue in this vein, but it is distasteful and, moreover, I consider that the question should be decided on its intrinsic merits and not by a comparison of personal records. However, since Sir Lawrence thinks otherwise I am bound to set out my qualifications, and I think I have now said enough to justify me in asking the question: why does Sir Lawrence Bragg regard my judgement as untrustworthy? If there are competent experts in the subject, as he asserts, he can scarcely, in view of the above facts, exclude me from their number or distrust my judgement on account of relatively insufficient knowledge or understanding of the subject. Nor can it be from his own perception of the truth of the matter, for he has declared his inability 'to criticise or try to distinguish between the two sides'. It may be for personal reasons, but if so I cannot conceive what they can be. I have never, so far as I know, given him cause to regard me with distrust. Indeed, his father, the late Sir William Bragg, early in 1935, when he was invited by the Bishop of Bath and Wells and the Bishop of Taunton to go to Wells and explain the scientific attitude to a number of clergy who were perturbed by the writings of Sir James Jeans on theological matters, did me the honour of asking the Bishops if they would allow him to take me with him, as he felt that there were some aspects of the matter on which I could be of assistance: they agreed, and I went. We remained on the friendliest terms until Sir William's death, and although my relations with Sir Lawrence have been very slight, they have never, so far as I know, given the slightest reason for either of us to regard the other with anything remotely savouring of distrust. Why, then, I ask again, does he now, suddenly, without any critical examination at all (which, with all due respect to his modesty, I have not the slightest doubt that he is perfectly capable of making, and would make if he had not already taken it for granted that anything to do with relativity — a subject which Lord Blackett considered suitable fare for first-year undergraduates — must be to him an impenetrable mystery) straightway stigmatise my judgement as untrustworthy and consider that, since it has already been ignored with all due politeness, no useful service to science would be done by anyone taking any further notice of it or allowing me to say anything more on the matter ?

I can conceive of only one reason — *that my judgement does not reach the orthodox conclusion;* and, that being so, it may be dismissed without further attention: Special relativity must be right because trustworthy experts say so: the experts are trustworthy because, they say that special relativity is right, and I am untrustworthy because I deny it. It is a perfect example of a circular argument.

If this is the true explanation — and I can conceive of no other — then Sir Lawrence Bragg has committed the cardinal sin of the scientist. He has closed his mind to the possibility that the theory of the moment, however plausible, might be wrong, and those experts, however competent, are fallible. He has forgotten that the final arbiters in science are experience and reason, and that the judgement of human authorities must be submitted for their approval, and that due retribution will unfailingly follow if this duty is not fulfilled. Like Lord Blackett, he has acquiesced in the neglect of this duty, and blindly given his allegiance to the *ipse dixits* of those whose pronouncements I have already related — to that of Max Born, who had nothing but praise for my *exposition* of special relativity, but refused to read my *criticism* of it after 25 years of further study because, since he knew the theory was right, it necessarily followed that I had made 'some elementary mistake'; to that of Synge, who 'casts a vote' that special relativity is right; to that of Ziman, who 'gambles' on its being right; and to those of other 'authorities' which are equally at variance with the genuine canons of scientific judgement. The fact that no one submits my simple question to the arbitration of reason he ignores, accepting the verdict of dogma, of the majority of 'experts', of chance, of anything but the only judge whose authority true science recognises. To the action of the editor of *Nature* in abruptly closing a correspondence at a point at which a single communication, submitted but withheld from publication, would have settled the question conclusively but unpalatably, he gives his approval, holding that 'no useful service to science' would be done by allowing the communication to appear. If such a thing had occurred in 1935, I at least — and, I have no doubt at all, Sir William Bragg also — would have been ashamed to present the actual scientific attitude at Wells — though, in justice to the memory of the then editor of *Nature,* Sir Richard Gregory, it must be added that the supposition is fantastic; he would have been incapable of such conduct.

Such is the state now reached by the specialists in relativity and by those individuals and journals whose eminence in the scientific world inescapably imposes on them a responsibility for seeing that scientific integrity is preserved. This is the contribution that science is now making to the moral framework of civilisation — not, as Dale saw it, something that the world cannot afford to lose, but something that, unless the world *does* lose it with all speed, must sooner or later lead to general disaster. Science no longer refuses to tolerate the neglect of any anomaly; it refuses to tolerate anything but neglect of a most outstanding anomaly. It no longer fears only prejudice and preconception; it fears to the point of terror a particular threat to its prejudices and preconceptions, and does everything in its power to suppress such a threat. Its criteria of truth — if that word can still be used in connection with it — are no longer reason and experience, but strict conformity to a theory that, despite its apparent successes, is still less plausible and less supported by observation than, in their day, were Ptolemaic astronomy and Newton's law of gravitation, both of which are now, rightly or wrongly, out of fashion. That is the state of mind in which the scientific world faces the responsibility that the development of experimental techniques has now laid upon it. The outcome, if it is allowed to continue, is only a matter of time; its character is certain. That, I repeat once more, is why this book has had to be written.

In spite of the difficulties I still believe that Sir Henry Dale's ideal scientist is worth the effort to make actual, but I do not underestimate the magnitude of the task. Its nature differs with the 'experts' and the 'elder statesmen'. It is understandably humiliating for the ‘experts' to acknowledge — especially after so long a resistance — that they have made so elementary an oversight; yet, since the fact is inescapable, they must either do so or remain silent. That is, I have no doubt, why the editor of *Nature* has had repeatedly to break his promise to Lord Soper and to me to write his 'long leader summarising the position' over a period which has extended from a month to nearly two years and still persists: he has not been able to obtain from his advisers the information which he confidently expected when the promise was made, and the quality of mind needed to retract what he had written in an earlier leading article is one which he has shown no evidence of possessing. In the blindly trusting 'elder statesmen', however, belief in the theory is irremovably implanted; it cannot be dislodged, for there is nothing to which to appeal to dislodge it. What is misunderstood can — granted the moral stamina — be corrected, for there are grounds for the misunderstanding which may be rectified; but what is accepted and not understood has no intellectual grounds at all, and so no basis on which one may stand to remove it. All that one can do is to hope that their sense of responsibility can be sufficiently awakened to lead them to demand of the 'experts' an open, candid answer to criticism, despite the cost, instead of an evasion of it. That is what I hoped to do through Lord Blackett and Sir Lawrence Bragg, only to find that the former had 'a firm rule' not to allow to be submitted for consideration to others what he himself did not understand and agree with, though he had no rule at all against 'teaching' what he did not understand to those who would later apply their ignorance to the operation of the most dangerous instruments yet devised; while to Sir Lawrence Bragg, Synge's voting and Ziman's gambling and *Nature's* trimming seemed 'fair and patient' methods of meeting criticism.

As I have said, however, I am writing this book not to indict but to inform and let the information bring whatever indictment is called for, so I cannot rest content with my interpretation of Sir Lawrence Bragg's response to my letters, inescapable as it seems to me: I am compelled to ask him to give his own interpretation. I am sure that at rock bottom (if only one could reach it!) he is as anxious as I am that the truth, whatever it may be, shall prevail as quickly and as harmlessly as possible, so I finally put to him the following questions:

> (1) Why do you consider it compatible with the ethical principles of science that an objective scientific question should be automatically closed to further inquiry when it has been dismissed unanswered by the *ex cathedra* judgement of human ‘authorities’?
>
> (2) Why, having decided that such closure is ethically justifiable, do you accept the judgement of those who refuse to give reasons for it, and reject that of one having at least equal qualifications in the subject, who gives reasons for his judgement in which no fault has been found?
>
> (3) Will you use your influence to persuade the 'competent experts', whose judgement you trust, to state clearly and publicly what factor of the situation described on p. 17, in which *everything with which the theory is concerned* is entirely symmetrical, enables *the theory* to distinguish which of the two clocks in question works the more slowly, as it requires one of them to do; and if they cannot specify that factor and validate the choice by applying it to Einstein's own examples, to acknowledge publicly, in the honourable scientific way, that the theory, since it requires what is physically impossible, is untenable, despite its mathematical impeccability? If you are not prepared to do this, what is your reason for refraining, in view of the unquestionable importance of the matter?

To this Sir Lawrence replied in the friendliest manner, but without answering my questions. Instead he reverted to the scientific question which I had put to the experts and had not expected him to deal with. 'I was very interested in your Introduction,' he wrote, 'because for the first time I thought I began to see where you had gone wrong. I say this with some diffidence, because I am not expert on relativity.' He then cited one of the electromagnetic observational arguments for special relativity, concerned with the decay of fundamental particles in cosmic ray phenomena, which I had already answered many times (see, for instance. Part Two and section II of my *Nature* article in the Appendix, p. 232), and added:

> If I may say so, I do think you are very wrong to attack physicists for refusing to acknowledge that relativity is wrong. I am sure that reasoning like the above has convinced them that it is right. It is of course quite fair for the two parties to hold different views, but not fair to accuse the other party of lack of scientific integrity. Frankly, in this case, I think you are wrong in your interpretation of relativity. I say this in a friendly way, hoping you will consider cases like the one I have quoted carefully, because it might save you from dropping a terrible brick.

I had not the slightest doubt of Sir Lawrence's genuine conviction (though, as he himself had admitted, it was diffidently based on reasoning outside his own field) or of his wholly friendly intention in thus writing, and I was glad to assure him of this. However, as I say, he had not answered my questions or made any reference to them. After repeating my answer to the cosmic ray argument I wrote on 3 May 1971:

I couldn't agree more both that physicists are 'convinced that s.r. is right' and that I should not 'accuse the other party of lack of scientific integrity'. On the first point, it is simply *because* they are convinced that it is right that their minds are closed to the possibility that it might be wrong. There are two kinds of integrity: (1) the practice of asserting only what you believe; (2) keeping your mind open to the possibility that what you believe may be wrong. They have the first kind all right; the second — what Dale referred to as 'not neglecting any anomaly' — is now, in this matter, a dead letter. That needs no further proof than the fact stated in my Introduction, that more than a decade's persistent attempts to elicit the one sentence needed to dispose of my anomaly have all failed. Your 'very simple direct physical proof that the clock is going at a different rate' is a matter of common agreement. But it stops there. When I ask which clock goes faster (for you can't have clocks going at different rates without one going faster than the other), and why, I get no answer. So what is their 'conviction' worth?

One the second point — that I should not accuse the other party of lack of scientific integrity — I have been most careful not to do so. I have simply stated the bare facts and left the judgement of them to the reader. That, for instance, is why I have asked my three questions of you, so that I can report *your* answers, not make charges of my own, or the absence of answers if that should be the case.

Consider a few facts. McCrea says that relativity doesn't compare the rates of two clocks and that Einstein never did so. But he himself, as well as Einstein, has done so many times through s.r. I simply quote them and leave judgement to the reader. Blackett is 'convinced' by McCrea and refuses to assure the public that integrity is preserved: I have his letter stating this; it is not my opinion, so I quote it. The R.S. referee says that my fallacy is too elementary to be instructive, but nothing will make him, whoever he is (and of course I don't ask his name), publish the fallacy, and *Nature* refuses to allow the R.S., whose principles require it to discover and make known the truth, to be asked by the public to do so. All this is in writing, and I simply state it; the reader can do the judging. Maddox closes down the discussion with Synge when it reaches a point where a decision is inevitable, and has suppressed my decisive reply for three years (it is still unpublished), during which two *Listener* correspondents (goodness knows how many more, but two letters were published) call me to task for not answering Synge. That is verifiable fact, not my accusation. Maddox assures Lord Soper and me time and again that he will deal with the matter In a long leader 'in a week or two': after 18 months it has still not appeared. The reader can judge that, but the fact is verifiable. And so on, and so on.

The problem that faces me is not whether I shall charge physicists with dishonesty or not, but whether, knowing that the attitude exemplified by these and other incidents is that with which physicists as a whole face the responsibility that lies before them in the present age; that nature will pay no attention to what McCrea, etc. say, but will take her own course; and that I alone possess all this conclusive evidence of the actual state of affairs — I shall hush it all up or make

> the public acquainted with *the actual facts* of a situation that concerns everyone so vitally. I know only too well what its judgement will be. I have had too much assurance from non-scientific people of intelligence and responsibility, arising merely from the facts I was able to state in the *Listener* (Dr. Platt's letter to *The Times* is typical) to have any doubt about that. I hate it with all my heart, but I have no doubt at all about what I must now do. It is too much to hope now for a miracle that would make it unnecessary. From my knowledge of them, there is not one of the 'authorities' who has the moral stamina to face the humiliation that, after his evasions for so many years, would inescapably attend his coming clean now, and I have not been able to persuade any of those who have the guts of the need to force the 'authorities' to answer my question with patent straightforward honesty. I expect it takes my years of experience to make their behaviour credible. I can therefore only thank you once more for the kind terms of your letter, and get on with the job.

Sir Lawrence finally replied on 18 May 1971:

> I very much appreciate your kind letter. I think I have shot my bolt, my knowledge of relativity has never been very thorough, and now that I am 81 and well into my retirement, it is not easy for me to make the effort to recall what knowledge I had of the subject, I am getting out of my depth. I do appreciate your writing as you do.

(As stated in the Introduction, this chapter was written before Sir Lawrence Bragg's death on 1 July 1971, and he had read the whole of the section relating to him, as it appears here, before writing his final letter just quoted.)

The picture, which I have tried to present in the foregoing pages, is of necessity incomplete. It includes very little of the efforts in other countries, and of those in this country attention has been concentrated almost wholly on the attitude of the leading workers, for these, though far less numerous, have much greater influence than the general body of physicists, both 'mathematicians' and 'experimenters', with whom I have had communication. Moreover, I have omitted all reference (apart from the slight incident mentioned on p. 39) to the prevailing frame of mind of students and young research workers in this subject of which I have had ample experience over the years through visits to universities to address student societies and discuss the problem with them. It will suffice to say that this is, in one respect, the most saddening aspect of the whole matter, for it is evident that the students have been trained, consciously or unconsciously, to believe that criticism of special relativity is a sure sign of ignorance, not to say stupidity, on the part of the critic, and I have been informed, with various degrees of tolerance, of fallacies that I had learnt to outgrow before the fathers of my instructors were born; only comparatively rarely have I been asked questions for information.

It would be a serious omission, however, if I failed to state that the general attitude of scientists, which I have presented, does not exist wholly without protest. There are many intelligent, interested, but scientifically uninfluential thinkers who are not willing to surrender their power of judgement and supinely to accept the implication that the world is essentially irrational and unintelligible, and there are 'experimenters', even among the academic physicists themselves, who have succeeded in preserving their intellects from submission to the general state of passivity. Among these I should mention particularly Dr. L. Essen, whom some years ago the late Sir Charles Darwin, one-time Director of the National Physical Laboratory where Dr. Essen's work is conducted, described to me as probably the world's greatest authority on the practical problem of time-keeping, and Dr. G. Burniston Brown, formerly Reader in Physics at University College London. These, in different ways, have for many years criticised the relativity theory by an analysis of its implications. Dr. Essen has given attention to the actual procedure used in the determination of the times (instants) of distant events, and Dr. Brown has examined the relation of the theory to the foundations of electromagnetism. I should explain briefly why I think it would be undesirable to attempt to relate what I have said here to their criticisms.

If you have a theory that requires clocks to do impossible things, then it is to be expected that if you examine in detail the procedure by which clocks determine the instants of distant events you will find that the results which the analysis shows they must yield will not agree with the results which the theory requires them to yield. Dr. Essen has made such an examination and has consistently maintained that Einstein's statements concerning the determination of distant instants are erroneous. But I have thought it best to avoid all such considerations because long experience has taught me that the moment one enters into details, the door is open to endless quibbles over words or phrases that invariably deflect attention from the real point at issue, and succeed only in persuading those who already suspect, or have become convinced, that the matter is too intricate for their understanding, that it really is so and that they may safely trust 'The Establishment' to have disposed of the criticism satisfactorily. That has happened over and over again. I therefore now refrain from entering into any discussion at all of Dr. Essen's papers, and concentrate on the single question, which I have asked. If that cannot be answered — and it certainly has not been — then the theory must be false; the details of its failure are then of secondary importance.

Dr. Brown, besides objecting to particular details in the presentation of special relativity by its founders and expositors, has given special attention to the Maxwell-Lorentz electromagnetic theory which Einstein's special theory was designed to protect against what at first seemed fatal criticism. Again it is true that, if the Maxwell-Lorentz theory is inherently faulty — as others, such as Ritz, long ago maintained and as is supported by the fact that it remains unreconciled with quantum phenomena — a theory that makes it appear 'plausible', as special relativity was designed to do thereby condemns itself. But this also diverts attention from the one simple and utterly fatal criticism of the theory, and opens another door into endless discussion of irrelevant points. I therefore, without prejudice, leave Dr. Brown's, like Dr. Essen's, criticisms of the theory out of consideration here. Nevertheless, I wish to pay tribute to the integrity and independence of mind, by virtue of which they have refused to be carried away by the tide of mystification by which the great body of experimenters have allowed themselves to be swept along.

A more profound reason for leaving their work out of consideration here lies in the fact that, as I have stressed more than once, the most serious aspect of this whole discussion is the moral aspect -not the question whether the theory is right or wrong, but the attitude of physicists to its rightness or wrongness. Indirect analyses of the details of time determination and of electromagnetic theory both allow scope for genuine differences of view, and opposite sides may be taken by physicists, both of whom preserve their scientific integrity intact and differ on purely intellectual grounds. In my discussion of the theory, however, the distinction between the moral and the intellectual aspects is absolute. I simply ask the question: what determines which of the relatively moving clocks works (and not merely appears to work, as Einstein's example of the equatorial, and polar clocks shows) the more slowly? The obligation to answer this question, or to admit, as Synge does, that if no answer is possible the theory must be abandoned, is a purely moral obligation. The physical justification of the answer, if one is offered in terms of the theory, is an intellectual matter. The evidence which I have given I think shows conclusively that the moral obligation has not been met, but the reader will judge for himself on that point. Certainly no answer has been offered. When an approach, through Synge's letter, to a final settlement has been made possible, it has been resolutely blocked; yet the theory remains at the foundation of modern physics as though it had never been questioned. That, I repeat, is the state of mind in which science faces the responsibility of the scientific age.

I have called this book *Science at the Crossroads* because science has indeed now two courses before it: it can, on the one hand, resume progress along the course which Dale, generalising from its past history, believed it still to be following, or it can, on the other hand, remain on the path which I hope I have enabled the reader to see that it is now pursuing. The consequences of the choice are important beyond measure.

One question, however — relatively academic, but still fundamentally important — remains and will inevitably arise, namely: how can such a situation as that which I have described have arisen in a movement whose sole aim is the discovery of truth, and which has not only nothing to gain by departing from that aim, but also the certainty that the departure will ultimately be discovered? Although the fact itself is so surprising that scarcely any conceivable explanation can be dismissed on grounds of improbability, I do not think it can be seriously entertained that the whole body of physical scientists has, within a generation or two, bargained its soul, Faust-like, for unworthy ends. Although there is an occasional instance is which I am not able, however hard I try, to persuade myself that there is not a conscious departure from rectitude, I have no doubt at all that the great majority of physical scientists are genuinely unaware that they are not acting according to the strict requirements of their calling. How, then, can they behave as they do?

The cause must be sought in the history of modern physics, and in the second part of this book I shall try to present in outline what I believe it to be. Although I have studied the literature pretty widely over a considerable period, and have had personal experience of the later part of it, I am aware that a full explanation needs more detailed treatment than I am able to give, and, were I still in my former position and a suitable student were available, I should set him the problem of studying, and presenting the results as a thesis for a doctorate, the development of physics during the last hundred years or so, with the special object of tracing the steps by which physics, the scope and character of which was once understood clearly enough, gradually became enslaved to mathematics and metaphysics until its present state of almost complete unconscious subordination to those subjects was arrived at. I hope that in the future that will be done. In the meantime, the explanation which I am now able to offer, though I am fully aware of its inadequacy, is, I believe, essentially true.

PART TWO

The Intellectual Issue

6

Four Outstanding Errors

The problem to be faced here, as I have said, is that of explaining how it has come about that physical scientists, almost to a man, have for so long allowed themselves to accept a theory that demands of a clock such an obvious impossibility as that it shall work steadily both faster and slower than an exactly similar one. The problem is anything but simple, in either sense of the word; i.e. it is both complex and difficult — though the difficulty, I believe, lies only in the discarding of false notions that have been automatically accepted as true, and not in grasping the actually true ones. It is therefore largely psychological, and, being no psychologist, I can only record what commonsense indicates concerning the various attitudes which physicists have adopted towards criticisms of the theory. Just as I cannot explain why physiologists of distinction rejected Harvey's demonstration, which to us seems so convincing, of the circulation of the blood because it conflicted with what Galen drought in the second century, so I cannot explain why physicists *think* that calculations which they perform on measurements connected with cosmic rays, which Einstein had never heard of, can answer my question why he felt entitled to conclude from his theory that an equatorial clock worked slower, and not faster, than a polar one. I can only record it as a fact, and do my best to analyse the course of events in physics that led to the fact.

Before I come to the historical events, however, I think it will be helpful to pinpoint four basic misunderstandings, an awareness of which will make it easier to understand why special relativity has been accepted for so long in spite of its clear untenability. They are concerned with — first, the relation between mathematics and physics; second the confusion of different meanings of the word 'time': third, the misinterpretation of 'co-ordinate systems' as 'observers'; and fourth, what I can best describe briefly as the literal interpretation of metaphors — the acceptance, as direct observations, of what are actually remote implications of possibly erroneous theories.

I begin, then, with the most dangerous of all, that pervades not only this subject but the whole of modern physics — the false conception of the relation between mathematics and physics (or, more generally, experience; but the experiences, or observations, dealt with in physics are those which specially concern us here). For, though our problem has two distinct aspects — first, why do the 'mathematicians' tell the 'experimenters' to believe such absurdities? second, why do the 'experimenters' believe them? — the first is the basic one.

There is a vitally important distinction between mathematics, which belongs wholly to the realm of pure thought, and physics, which belongs wholly to the realm of experience; and these two elements of our general awareness are (at least at the level with which science is concerned; I make no assertion at all concerning philosophies which may attempt to relate them fundamentally) wholly independent of one another. We can, however, use mathematics in the service of science because our aim in physics is to relate together experiences which at first seem unrelated, and by limiting our considerations to the experiences revealed by *measurements,* as we do in physics (though science in general, of course, goes beyond measurement), we may express them in terms of numbers, or symbols representing numbers, and then apply our mathematical knowledge to those symbols, regarded now as elements with which our mathematical theorems deal. Let me try to elaborate this a little.

Mathematics in itself, as I say, is independent of experience. It begins with the free choice of symbols, to which are freely assigned properties, and it then proceeds to deduce the necessary rational implications of those properties. Thus, the symbols may be straight lines, circles, etc., having properties such as those described by Euclid, or other similar ones, and their consequent relations form the mathematical discipline of geometry, euclidean or otherwise. If the symbols are numbers, their relations, according to assigned rules of addition, multiplication, etc., constitute arithmetic. If the symbols are letters, with rather more extended properties, we have the discipline of algebra, and so on. There is nothing of experience in this: the symbols and properties may be chosen arbitrarily, and all that is required of or implied by the resulting corpus of theorems is that it conforms faithfully to them.

Science is the attempt to find relations between *experiences.* Some such relations are obvious; e.g. that the fruits of an apple-tree are like each other and unlike those of a pear-tree. Others, such as the relation between the rate of fall of an apple and the motion of the Moon, are discovered only by careful research. It is here that mathematics is of such service to science. If we try in general to find a relation between the apple and the Moon, we shall fail; they seem wholly independent of one another. But by making *measurements* related to the two things, we arrive at numbers, and these are things with which mathematics deals. We can accordingly apply the relations between numbers discovered by mathematical research, to the measurements related to the apple and the Moon, and discover a parallelism which enables us to say that our *experiences* of the motions of the apple and the Moon are related to one another in a way not at all predictable or discoverable without the use of mathematics.

By extending this over as wide a range of measurements as possible, we reach the vast body of related experiences that constitutes modern physics. Those experiences whose properties are paralleled by the properties of me symbols in a particular existing mathematical discipline are naturally selected for special study, since we can at once see that they will be related in accordance with me theorems of that discipline. Thus, having ascertained mat the experience of counting certain objects is a process, which can be represented by numbers; we can apply the abstract truths of arithmetic to the corresponding operations with those objects. We can conclude, for example, that if, to a box of apples, we add three and then subtract two, we shall be left with the same number as if we had first subtracted two and then added three; we shall not need to perform the actual operations to assure ourselves of this. Similarly, if we add 3,521 apples to 765, we shall not need to count the total to know that it is 4,286; we can determine that in a few seconds with pencil and paper. The whole relation between mathematics and physics is of this character. Mathematical structures of thought are built up by pure reason applied to arbitrary axioms, and men, having found a realm of experience mat is paralleled by one such structure] we can use the parallelism to extend our knowledge of physical relations over the whole field of experience to which it applies.

But now certain questions arise. How do we know what branch of mathematics applies to a particular realm of experience? How do we know that a parallelism which we have discovered in a limited range of experiences of a certain kind will be valid over the whole range of the mathematical structure on the one hand and the whole range of experiences of me same kind on the other? How do we know that a particular branch of mathematics will have *any* corresponding possibilities in experience? The answer to all these questions is that we can know these things only by experience itself, i.e. by trial and error. Since, as I shall maintain and have adumbrated in Chapter 1, it is the oversight of this fact, and the illegitimate assumption that there is some *necessity* for whatever is true in mathematics to impose its inevitability on experience, that is primarily responsible for the error of special relativity, I should like to quote a passage from Einstein which shows that, as a general proposition at least, he was well aware of this contingent element in the relation between mathematics and experience. In a letter to the late Viscount Samuel, published with his consent in the latter's book, *Essay in Physics,* in the German original and in English translation, he wrote this:

> For example, Euclidean geometry, considered as a mathematical system, is a mere play with empty concepts (straight lines, planes, points, etc., are mere 'fancies'). If, however, one adds that the straight line be replaced by a rigid rod, geometry is transformed into a *physical theory.* A theorem, like that of Pythagoras, then gains a reference to reality. On the other hand, the simple correlation of Euclidean geometry is being lost, if one notices that the rods, which are empirically at our disposal, are not 'rigid'. But does this fact reveal Euclidean geometry to be a mere fancy? *No,* a rather complicated sort of co-ordination exists between geometrical theorems and rods (or, generally speaking, the external world) which takes into account elasticity, thermic expansion, etc. Thereby geometry regains physical significance. Geometry may be true or false, according to its ability to establish correct and verifiable relations between our experiences.[1]

What I believe to be the basic misconception of modern mathematical physicists — evident, as I say, not only in this problem but conspicuously so throughout the welter of wild speculations concerning cosmology and other departments of physical science – is the idea that everything that is mathematically true must have a physical counterpart; and not only so, but must have the particular physical counterpart that happens to accord with the theory that the mathematician wishes to advocate. I have already (Chapter 1) given some examples of this, associated with the greater generality of mathematics as compared with physics; here I wish to show some other aspects of the same fundamental misconception. It is seen easily enough in some very simple examples where it is so obvious that no one could possibly make the error in question; nevertheless, that error is made almost automatically when we get into realms unfamiliar in ordinary experience.

Take, for example, the simple mathematical equation, 1+1=2. Over a wide range this holds good in experience as well as in mathematics, where it is always true. If we have one penny, and someone adds another to our wealth, we have two pennies. If we have one apple and add to it another and count the total, we find that it is two apples. And so on. We may therefore be inclined to generalise, and say that if we add one anything to another of the same thing, we have two of those things; in other words, $x+x=2x$, whatever x may be. But this is far from the truth. If we add one water drop to one water drop we get not two water drops but one larger drop. If we add one rabbit to one rabbit we may get a continent of rabbits. Even believers in special relativity will assert that if we add a velocity of 1 foot a second to a velocity of 1 foot a second we get a velocity slightly less than 2 feet a second. If we add one idea to one idea we may get a philosophy. If we add one commandment to one commandment we get all the law and the prophets. Browning wrote that it was only in music that one note added to two notes made a star, but in fact experience abounds in that kind of addition. Similarly, although adding three apples and subtracting two gives the same result as subtracting two and adding three, it is certainly not true that adding cocaine to a gum and subtracting a tooth gives the same result as subtracting the tooth and adding the cocaine.

So it is with other operations of mathematics. In algebra, if a = b, then 2a = 2b. This was applied in the Middle Ages to prove the immortality of the soul. To be half dead was the same as to be half alive: double both, and it follows that to be dead is to be alive. This particular argument would carry little weight now, but equally naive applications to experience of mathematical truths do flourish. Not long ago the mathematical fact that log 1 = 0 was applied to prove that there was no difference between something and nothing. The late Professor E. A. Milne proposed a theory called *kinematical relativity*[2], according to which it was equally legitimate to represent the measurement of time by a certain symbol and by its logarithm. It was a short step from this to the conclusion that the question whether a distant nebula was moving rapidly away from us or remaining at the same distance was a 'no-question'; the two processes, were the same, since the only difference lay in our free choice of the way of measuring time, and we could equally well measure it directly or logarithmically. When it was pointed out that, if this were true, the principle could be applied equally well to a stone that is thrown at you, so that whether you would experience the impact or not would depend on what kind of watch you carried, Milne refused to consider the physical application of the mathematics. On one scale of time the stone hit you a few seconds after being thrown; on the other an infinite time would elapse; this was mathematically certain, and therefore the two cases were equivalent.

> I attach no weight whatever to the verbal descriptions I have occasionally attempted [he wrote]: the core of the matter lies in the mathematics.... It is no use objecting to the results themselves; the critics should find flaws in the trains of mathematical deduction.... Until then, I have nothing to add to my constructive papers.'

This theory receives little attention now, though its obituary notices speak of it with great respect[4], but its neglect in application appears to be due only to the fact that other theories, equally unrelated to experience, have superseded it — notably the so-called 'steady state' theory of the universe, according to which, because a certain tensor (a mathematical quantity) can give values for another mathematical quantity which changes from o to 1 when a third called *t* increases, matter is continually being created out of nothing. There is not the slightest physical evidence for this, or for anything like it; there is only the fact that, in another connection, other tensors can be associated in a reasonable way with other physical quantities which *can* be observed. In other words, the argument has the same validity as this — that because two water drops when brought together coalesce into one, so two apples similarly treated will coalesce into one — except that we can so treat the apples, and the processes postulated in the steady state theory are wholly imaginary.

Examples could be multiplied *ad lib,* but I think these are enough to show how general and how dangerous is the prevailing illusion that all that is necessary to entitle a physical theory, however absurd, to respect is to discover some mathematical process whose symbols can be arbitrarily correlated with the physical entities of the theory, without regard to evidence or probability or commonsense. We shall see in due course that the supposed justification of special relativity by the 'mathematicians', to whom the 'experimenters' entrust it, lies wholly in the impeccability of its mathematical structure: the impossibility of the application to experience of that structure, in the manner postulated by the theory, is left out of consideration altogether, just as the fact that the stone hits you within five seconds is left out of account in assessing the credentials of kinematical relativity: the mathematics shows that if you measure time differently the moment of impact is infinitely far ahead, so you have no cause for alarm. Similarly, in special relativity mathematics proves that one clock goes both faster and slower than another; therefore it must do so.

I cannot leave this subject without bringing to attention an aspect of it, which has very serious general implications. I think it is impossible for anyone who reflects on the few examples I have given, and realises that they are not exceptional in their general character but typical of most mathematical physics of the present day, to doubt that, as a general rule, the practice of mathematical physics goes hand in hand with lack of elementary reasoning power and of that normal form of human wisdom, somewhat misleadingly called commonsense, that provides its own corrective of premature judgement and never allows the requirements of reason and experience to be overcome by the seductions of attractive speculations. I repeat that I am no psychologist, and it is with diffidence that I admit an unwillingness to conclude that this is an inescapable psychological necessity; it is more comforting to hope that it denotes a failure of our educational system to recognise an ever-present danger and to take precautions against it. It is usually taken for granted that the processes of mathematics are identical with the processes of reasoning, whereas they are quite different. The mathematician is more akin to a spider than to a civil engineer, to a chess player than to one endowed with exceptional critical power. The faculty by which a chess expert intuitively sees the possibilities that lie in a particular configuration of pieces on the board is paralleled by that which shows the mathematician the much more general possibilities latent in an array of symbols. He proceeds automatically and faultlessly to bring them to light, but his subsequent correlation of his symbols with facts of experience, which has nothing to do with his special gift, is anything but faultless, and is only too often of the same nature as Lewis Carroll's correlation of his pieces with the Red Knight and the White Queen — with the difference that whereas Dodgson recognised the products of his imagination to be wholly fanciful, the modern mathematician imagines, and persuades others, that he is discovering the secrets of nature.

The processes of mathematics are to be contrasted rather than identified with the process of rational drought. Professor A. N. Whitehead, himself an accomplished mathematician, long ago recognised this truth before the unawareness of it became the ominous danger that it now is.

> By the aid of symbolism [he wrote] we can make transitions in reasoning almost mechanically by the eye, which otherwise would call into play the higher faculties of the brain. It is a profoundly erroneous truism, repeated by all copybooks and by eminent people when they are making speeches, that we should cultivate the habit of thinking of what we are doing. The precise opposite is the case. Civilisation advances by extending the number of important operations, which we can perform without thinking about them. Operations of thought are like cavalry charges in a battle — they are strictly limited in number, they require fresh horses, and must only be made at decisive moments.'

If this is a true diagnosis — and I think facts available since Whitehead wrote these words unmistakably confirm it — mathematical ability and ability to conduct operations of thought are distinct faculties, and although I know of no reason why they should not co-exist in the same person, it is only too clear that at the present time, except in a rare instance, they do not. The danger that the cultivation of the former should cripple the latter is thus so real that positive steps should be taken to counteract it. In fact, however, our methods of education augment it. In the application of mathematics to physics the results of this are shown in such examples as those which I have given, and in many others.

Of more general concern is the possibility that the movement to introduce scientists (in particular, mathematical physicists) into the government of the country may receive support based on ignorance of the realities of the matter. What Whitehead did not foresee (he wrote before the first world war) was that civilisation would soon be concerned less with advancing administratively than with ensuring its own survival, so that operations of thought would need to be the rule rather than the exception. Government now demands, above all things after moral rectitude, intelligent thought, and it must be recognised that mathematical physicists are, of all our citizens, the least fitted to provide it. 'To the scientist,' writes Professor (now Sir) Fred Hoyle, 'war starts because human behaviour is representable in terms of mathematical equations possessing discontinuous solutions.'[6] This must not be dismissed as a humorous wisecrack: Hoyle, and others of his type, really believe that this is so. They were not necessarily born with a deficiency of commonsense: they have exceptional mathematical ability, which has been mistaken for exceptional intelligence, and have been so trained that their normal intelligence has expired through desuetude; much mathematics hath made them — what they are. They are now no more able to perceive the advantages of intelligence than the blind men in H. G. Wells's story could perceive the advantages of sight.

Let us, however, return to our present concern. The circumstances in which special relativity came to birth and acquired its present almost impregnable position in physics will be recounted later, but it will be appropriate here to give, in the barest outline, an account of the way in which the ground was gradually prepared for such an event to occur. As we have seen, Galileo — who, more than any other single person, can be regarded as the originator of modern physical science — claimed that the book of nature was written in the mathematical language. He meant exactly what he said: mathematics was a language, a means of expressing something, not the thing that it was important to express. Both Galileo and Newton took *experiments* or *observations* as their starling-point, and used mathematics only as a tool to extract the maximum amount of knowledge from the experiments and as a means of expressing that knowledge. In the later developments of their work throughout the eighteenth and nineteenth centuries in the field of mechanics, this relation between experience and mathematics was maintained. Although Adams and Leverrier 'discovered' Neptune by mathematical calculations, it was Galle's *observation* of the planet that was rightly regarded as its discovery: had he looked and found nothing, the work of Adams and Leverrier would have been forgotten. In chemistry also it was observations that determined the ideas held. When, in order to explain certain observations, it was suggested that phlogiston had a negative weight, that was not because the symbol w in mathematics could be given a negative as well as a positive sign; it was because, in terms of the phlogiston theory, the observations required it. (This may be contrasted with a recent cosmological theory in which it has been proposed that the 'pressure' in the universe — a conception to which it is impossible to assign any physical meaning other than a positive one — is negative, solely because the mathematical symbol p can be given a positive or a negative sign). In electricity and magnetism, of the two great pioneers in the first half of the nineteenth century, Faraday was almost completely innocent of any mathematical knowledge, while Ampere, equally proficient in experiment and mathematics, invariably used mathematics to interpret the results of his experiments and never to dictate them.

Of course, there are numerous instances in which mathematics has *suggested* physical possibilities which have later been realised and might not have been thought of without the suggestion; its service in this respect can hardly be over-estimated. An outstanding instance is the re-naming of 'vitreous' and 'resinous' electricity as 'positive' and 'negative', and the subsequent deduction of their interactions on the supposition that they accord with those of the mathematical signs bearing those names — although it may be that we are in danger of forgetting that the correlation is ultimately empirical and may well break down in certain extreme cases. But, so far as I have been able to discover, the first serious example of the *mastery,* instead of the *servitude,* of mathematics in relation to physics came with Maxwell's theory of the electromagnetic field[7], and that, as would be expected, only in a very tentative way and not without resistance. In brief, Maxwell showed that Ampere's law in electromagnetism, expressed mathematically — which, of course, as I have said, was a mathematical expression of results found by experiment —

did not satisfy the equation of continuity but could be made to do so by a purely mathematical modification. Accordingly he assumed that this modified form was the actual physical law. But he was too conscious of the true relation between mathematics and physics not to be aware that this was quite unjustifiable unless an actual physical relation existed which was represented by his proposed equation; and since he knew of none, he made the assumption that what he called a 'displacement current' existed in a dielectric. The physical feature that distinguishes a dielectric from a conductor is that the latter, but not the former, can convey an electric current, so this was quite inadmissible on observational grounds, but Maxwell assumed that a 'displacement' of electricity could occur in a dielectric, which had the same physical effects, so far as these were required by the equations, as a current in a conductor. If that were so he could proceed to build up a general 'dynamical theory of the electromagnetic field' which gave an electromagnetic interpretation of light and was so beautiful and comprehensive that he could not refrain from postulating the actual physical reality of his 'displacement current' as a justification for his mathematics.

At that time, when the true relation of mathematics to physics was still a prevalent influence in science, this highly artificial conception naturally aroused strong opposition, though what is most significant from our point of view is the clear indication it gives of the need Maxwell felt to provide some physical basis for his mathematics. Among the foremost opponents of this way of doing it — i.e. of the invention of unobserved 'phenomena' to suit the mathematics instead of adapting the mathematics to what was observed — was Lord Kelvin, who said bluntly, 'I want to understand light as well as I can without introducing things that we understand less of.'[8] Other physicists had similar feelings, and even Hertz — who was instrumental, through his experimental discovery of electromagnetic waves, in establishing Maxwell's theory, notwithstanding its mystical character, as the fundamental truth about electromagnetism — wrote:[8] 'Many a man has thrown himself with zeal, into the study of Maxwell's work, and even when he has not stumbled upon unwonted mathematical difficulties, has nevertheless been compelled to abandon the hope of forming for himself an altogether consistent conception of Maxwell's ideas. I have fared no better myself.' He proceeded to describe three representations of Maxwell's theory, and went on: ' I shall thus have an opportunity of stating wherein lies, in my opinion, the especial difficulty of Maxwell's own representation. I cannot agree with the oft-stated opinion that this difficulty is of a mathematical nature.' He sums up: 'To the question, "what is Maxwell's theory?" I know of no shorter or more definite answer than the following: — Maxwell's theory is Maxwell's system of equations.'

Nothing could more clearly express the change that had come over physics. Experiments more and more confirmed the deductions that were made from the theory when the symbols in the equations were given certain physical meanings, while the justification for giving the symbols those meanings continued to elude everyone. Lorentz generalised Maxwell's theory to make it apply to moving as well as static systems — we shall come to this later — and, all unconsciously, a state of mind was generated in physicists by which, while still formally adhering to the principle that observation was basic and mathematics a useful tool, they were ready to accept mathematical requirements as an adequate substitute for a genuine theory, even though they could see nothing intelligible that corresponded to it physically. It was a short step from acceptance of the physically unintelligible to the physically absurd, but the description of this must be postponed until we come to the origin of the special relativity theory itself. In the meantime I hope it has been made clear how the atmosphere of the time had become propitious for the advent of a theory that in earlier days would have been dismissed without a second thought. Its survival, thus made possible, was rendered almost inevitable by the actual sequence of historical events, but of this anon.

(This will be a convenient place to interpolate a note on a general misunderstanding concerning the relation of Maxwell's to Faraday's ideas for which Maxwell himself is ultimately responsible but which would have been noticed and corrected long ago if writers on physics had cultivated the habit of reading original papers instead of relying solely on second-hand accounts of them. We shall, alas, meet with other misunderstandings of the same kind. Maxwell begins his 1865 paper[7], in which his theory of the electromagnetic field is set forth, by establishing the existence of the ether on which the whole of what follows is to be based.

> The theory I propose [he writes] assumes that in that space [space in the neighbourhood of the electric or magnetic bodies] there is matter in motion by which the observed electromagnetic phenomena are produced... We may therefore receive, as a datum from a branch of science independent of that with which we have to deal, the existence of a pervading medium, of small but real density, capable of being set in motion, and of transmitting motion from one part to another with great, but not infinite, velocity.

Later in the paper he writes:

> The conception of the propagation of transverse magnetic disturbances to the exclusion of normal ones is distinctly set forth by Professor Faraday *(Phil. Mag.,* May 1846) in his 'Thoughts on Ray Vibrations'. The electromagnetic theory of light as proposed by him is the same in substance as that which I have begun to develop in this paper, except that in 1846 there were no data to calculate the velocity of propagation.

A reference, however, to Faraday's 'Thoughts on Ray Vibrations' shows that his idea was quite different. He proposed, in fact, 'to dismiss the ether' which was the basis of Maxwell's theory, and to endow each elementary source from which light was emitted with a system of rays, extending indefinitely in all directions, the vibrations on which constitute light. The difference may be of fundamental importance, for Einstein's special relativity theory, designed to save Maxwell's *equations,* could do so only by sacrificing the ether which was the basis of Maxwell's *theory.* Had Einstein first sought to bring Maxwell's equations into conformity with Faraday's idea — which amounted to a separate 'ether' for every atom instead of a single universal ether which could serve as an absolute standard of rest — he might have produced a theory not subject to the fatal defects of the one he did produce. This is a task, which might well repay the effort it would demand).

I pass now to the second of the general considerations previously mentioned, that help us to understand the confusion and misconceptions that surround the word 'relativity': it is the multiplicity of meanings associated with the word 'time'. If it is the misconception of the relation between mathematics and physics that is chiefly responsible for the belief that special relativity is physically true, this second confusion is chiefly responsible for the widespread conviction that it is an esoteric and difficult subject and indeed anything but a rather simple physical theory of the well-understood traditional kind. In dealing with words I must, of course, restrict my comments to one of the numerous languages used in the literature of relativity. That must perforce be English, though reference to many German papers shows that what I have to say about 'time' applies also to the German word 'Zeit', though not every detail is necessarily applicable. The basic confusion, however, exists independently of the language in which it is expressed. Consider a journey. We may say of it the following three things:

I (a) The journey occurred in *time.*

(b) The *time* of starting was 1 o'clock.

(c) The *time* occupied by the journey was 2 hours.

The same word, *time,* is used here in three quite different senses, as may be seen by considering the corresponding statements about space:

II (a) The journey occurred in *space.*

(b) The *place* of starting was London.

(c) The *length* of (or *distance* covered by) the journey was 60 miles.

Here we use three different words — space, place, length (or distance), none of that could be substituted for either of the others without depriving the sentence of meaning. The same distinctions, thus brought to light, exist in the set I, but they are obscured by the use of the same word, 'time', for three quite different ideas.

To distinguish the three meanings of 'time' I will re-express the set I in the following not unnatural ways:

III (a) The journey occurred in *eternity.*

(b) The *instant* of starting was 1 o'clock.

(c) The *duration* of the journey was 2 hours.

(In using the word 'eternity' I wish to imply nothing concerning the philosophical problems associated with everlastingness, eternal recurrence, etc., which the word often suggests. I mean by it only what everyone understands by 'time' in the well-known lines

Time, like an ever-rolling stream,

Bears all its sons away.

Now what will probably surprise many readers is that Einstein's special relativity theory, as he expounded it in his 1905 paper, has nothing at all to do with time in the sense of 'eternity'; it is concerned only with *instants* and *durations* (as intervals between instants). This fact — for it is an unmistakably verifiable fact — has an importance that can hardly be exaggerated, because one of the chief factors — probably *the* chief factor — in creating the illusion that relativity is unintelligible, or even difficult, is the notion that it has something to say, and something quite unimaginable to say, about the *nature* of 'time', of the continuum that St. Augustine and Kant and other philosophers have puzzled themselves about. In fact, time, the ever-rolling stream, has no more to do with the existence of clocks than with that of sausages, while time, in Einstein's theory as in physics generally, means *only* clock-readings. It is because of this confusion that the 'experimenters' have left relativity to the 'mathematicians'. Their concern has been only with what can be the subject of observations, and 'eternity' cannot be observed; it can only be thought about, and the 'experimenters' leave that kind of thought to mathematicians and philosophers. These draw deductions about 'eternity', and pass them on to the 'experimenters' as relating to 'instants' and 'durations'. They are accepted as such, without understanding but with blind trust. The reader may foresee what will ensue, if this process is allowed to continue.

The achievements of physics — the establishment of relations between measurements of various kinds — are summed up in a number of equations, in which the symbol t occurs with great frequency, but *never* with the meaning of 'eternity'; it *always* means an 'instant' (i.e. directly or indirectly a clock-reading) and $t_2 - t_1$ means a 'duration'. Whenever a general physical formula has to be applied to any particular case, t means the instant of occurrence of some event, and, so far as physics is concerned, the idea of time as a featureless continuum, infinitely or finitely extended, has no more significance than the idea of a featureless continuum of mass or electric charge or any other physical quantity represented by a symbol in an equation. This was recognised perfectly clearly by Einstein (or, rather, was so obvious to him that he was not consciously aware that he was taking it for granted) when he formulated his special theory in 1905.

> The theory to be developed [he wrote] is based — like all electrodynamics — on the kinematics of the rigid body, since the assertions of any such theory have to do with the relationships between rigid bodies (systems of co-ordinates), clocks, and electromagnetic processes,

and nowhere in the development of the theory does any interpretation of the word 'time', other than that of instant or duration, appear at all. It was Minkowski who later took the fatal step of introducing 'eternity' into the theory, as we shall see in due course.

When once the distinction between eternity, instant and duration is recognised, the general literature of the subject of relativity is seen to be in utter confusion. The writer, quite unaware that the word 'time' has different meanings, unconsciously oscillates between them, and the reader, equally unconsciously, becomes the victim of one *non sequitur* after another, in which he can see no failure of reasoning but yet no possibility of making sense of the conclusion: thus is generated the illusion that relativity is incomprehensible to the ordinary mind.

Take, for example, Eddington's standard work, *The Mathematical Theory of Relativity.* [10] In the first chapter, after some general remarks about 'eternity' (called 'time', of course), he remarks that 'in the mass of experimental knowledge which has accumulated, the words *time* and *space* refer to one of the "fictitious" times and spaces' — i.e. he is asserting that experimental knowledge refers to 'eternity'. But almost immediately he goes on to say that our experimental knowledge is concerned only with 'durations', not with 'eternity' or even with 'instants'. Expressing himself now in terms of space, though it is clear that he intends his remarks to apply to time also, he says:

> in our common outlook the idea of position or *location* seems to be fundamental. From it we derive distance or *extension* as a subsidiary notion... The view which we are going to adopt reverses this. Extension (distance, interval) is now fundamental... Any idea contained in the concept location which is not expressible by reference to distances from other objects, must be dismissed from our minds... Accordingly our fundamental hypothesis is that — *Everything connected with location which enters into observational knowledge — everything we can know about the configuration of events — is contained in a relation of extension between pairs of events.*

So, within a page, the content of our 'experimental knowledge' or 'observational knowledge' has switched from 'eternity' and 'space' to 'durations' and 'distances' without a word of apology. But it does not last. Time, in the sense of 'eternity', returns and disappears apparently without rhyme or reason, until on p. 166 we read this:

> Events before $t = -\infty$ may produce consequences in the neighbourhood of the observer and he might even *see* them happening through a powerful telescope.

So presumably something that 'enters into observational knowledge' may be assigned to an instant before 'eternity' begins, and so is neither in time nor in a relation of extension between pairs of events. Such is the confusion that abounds in relativity literature, and arises without limit from the use of the word 'time' to denote quite different things.

An example very relevant to the main purpose of this book is afforded by Synge's reply to my criticism (pp. 76-7). He agrees that special relativity is incompatible with my concept of a regularly running clock and that one of them must be abandoned. He chooses to abandon my concept of a regularly-running clock because it is 'equivalent to Newton's concept of absolute time'. But, like Einstein's special relativity theory, it is altogether independent of any concept of absolute time, whether Newton's or another's. What Synge here calls 'Newton's concept of absolute time' can refer only to a concept of what I have called 'eternity'; my concept (or anybody else's for that matter) of a regularly running clock is a concept of an instrument that marks 'instants' and measures 'durations', and these imply no particular concept at all of 'eternity'.

To sum up, then, the whole of Einstein's special theory, as set out in his paper of 1905 which is still generally acknowledged to be its canonical expression, is concerned with concrete, observable things — clocks, instants, durations, distances, events; it is totally independent of all conceptions of the nature of space and 'eternity'. It treats of the relations between observable things in different 'coordinate systems'; i.e., apart from trivial differences, it deals with the values, which those things take when the observable physical system under consideration is regarded as having different states of uniform motion. That is a problem which had been considered for centuries and regarded as solved until an ambiguity arose when it was found that the relations accepted with the events treated in mechanics were incompatible with those which seemed to be demanded with the events treated in electromagnetism. Einstein's theory was designed to provide a relation that held for both kinds of events. It was wholly physical, and concerned wholly with a problem of the traditional kind, involving only traditional concepts. We shall see later how, through the delayed action of Minkowski's metaphysical interpretation of his own mathematics, it came to be enveloped in a metaphysical cloak that had nothing whatever to do with its essence.

The third of the four most prominent sources of confusion that have led to the general illusion concerning special relativity is the substitution of 'observers' for 'co-ordinate systems'. In the literature of relativity there is almost invariably a great deal about 'the observer', and statements about what different observers, in different states of motion, will observe; and the impression is given that this is an essential, if not *the* essential, feature of the theory. Indeed, the late Professor Milne based his whole conception of relativity on a comparison of the experiences of such observers; he declared that relativity was 'a complete denial of the solipsist position'[2] — a position with which it has no more to do than with bimetallism. That, however, is an extreme case, but writers generally have been prone to elevate 'the observer' from a convenient accessory in the task of explaining the real essence of the theory, into part of the essence itself. This distortion has misled not only the general reader, but many specialists in the subject as well.

The fact is that the observer is concerned in relativity no more and no less than in any other department of science — or perhaps it would be truer to say that he is concerned less in relativity than in most other departments. For example, if we are calculating the circumstances of an eclipse of the Sun, what the observer will see will depend very much on where he happens to be, and it is generally not difficult to choose one's station so as to observe the particular aspect of the eclipse in which one is interested. But in special relativity theory, the observers whom it is generally considered worth while to compare are those whose relative motion is very great indeed — far greater than anyone has yet managed to make possible. Apart, therefore, from the needs of science fiction, we can leave the observer out of our account of the theory altogether.

Indeed, a moment's thought will show that this must be so. All science is based on observation, and whatever we say about the world studied in science must justify itself ultimately in terms of what we actually observe, not of what we infer that hypothetical observers would experience in circumstances impossible yet to attain. Now effectively, in all matters with which special relativity is concerned, there is only one observer — a terrestrial one — for the relative motions possible to terrestrial observers are so small as to be negligible in this connection. Hence the theory must be wholly expressible in terms of the experiences of that one observer alone.

Why, then, does the observer figure so prominently in expositions of the theory? It is simply because he has been falsely identified with a *co-ordinate system.* Now a co-ordinate system apart from characteristics, which are trivial here and may be ignored — is simply a state of motion. A person in a train travelling at 60 miles an hour through a station is said to be at rest in a co-ordinate system which is moving at 60 miles an hour with respect to a coordinate system in which a person standing on the station platform is said to be at rest; and, conversely, the latter person is said to be at rest in a co-ordinate system moving at 60 miles an hour (but in the opposite direction) with respect to the co-ordinate system in which the former is at rest. Their observations of the surrounding landscape will, of course, be very different, in ways with which we are quite familiar, but this has nothing to do with the special relativity theory: so far as that theory is concerned *there are no differences at all in their observations,* for it is an essential feature of the theory that either of the two observers has the same right as the other to say that he is at rest and the other moving. In other words, no observations are possible that would entitle either person to claim a state of rest (or, indeed, any particular state of uniform motion at all) for himself, that are not available for the other person to make the same claim for *himself.*

It follows that *all* phenomena generally associated with relativity — relative contraction of rods, relative slowing down of clocks, etc. — are not matters of observation but are wholly concerned with co-ordinate systems, and the essential difference between an observer and a co-ordinate system is that the same observer (and we have seen that there is effectively only one observer In the universe on whose observations all the science we have yet achieved can be based) can choose any of an infinite number of co-ordinate systems that he pleases (provided that in special, though the limitation does not exist in general, relativity their relative motion is uniform). The observer on the station is not bound to suppose himself to be at rest; he can suppose the train to be at rest and himself (with, of course, the surrounding landscape) to be moving at 60 miles an hour. The observer in the train has an equal right to suppose whatever motion he pleases for himself. Obviously, nothing whatever that either will *observe* will be changed if he changes his mind about his state of motion — i.e. if he changes his co-ordinate system. It is, of course, usually convenient for both observers in this case to make the same choice — that of the co-ordinate system in which the station is at rest and the train moving (though I remember a cartoon, in the days when relativity was a popular sensation, in which a passenger calls out 'Hi, guard, does Manchester stop at this train?'), but that is incidental. In other circumstances we freely change our co-ordinate system as we change the problem under consideration. In laboratory experiments we usually choose a system in which the Earth is at rest. In dynamical astronomy we choose one in which the Sun is at rest and the Earth moving at 18 ½ miles a second. In stellar astronomy we choose one in which the Earth is moving round the Galaxy at a few hundred miles a second. And so on. Clearly, nothing whatever that we observe is changed by our change of mind.

In Einstein's basic paper on the theory 'the observer' is not mentioned after the first two short sections right up to the end of the description of the theory. In those two sections Einstein is clearly preparing the ground for the serious business of the theory, for he was well aware that, at the time of writing, he was introducing ideas at variance with what had up to then been taken for granted, and something in the nature of a picturesque account was necessary. But what he regards as the theory proper starts at section 3, which is entitled 'Theory of the Transformation of Co-ordinates and Times from a Stationary System to another System in Uniform Motion of Translation Relatively to the Former'. In everything that follows, down to the conclusion: 'We have now deduced the requisite laws of the theory of kinematics corresponding to our two principles, and we proceed to show their application to electrodynamics', there is no reference at all to the observer; it is all concerned with the change of values which a single physicist must make in the coordinates he assigns to objective events when he decides to change his co-ordinate system. What he observes will change no more than what we observe will change when we stop thinking of ourselves as resting in bed and reflect that we are moving round the Sun. The changes in the co-ordinates, however, will be different from those believed to be necessary before the theory was devised, and it is on these differences alone that the theory must be judged.

The last of the errors mentioned as permeating the literature and the general appraisement of relativity has been, I think, the most effective in persuading the 'experimenters' that the theory must be right, notwithstanding their inability to make sense of it. I have described it as the literal interpretation of metaphors, and I can best illustrate it by a particular example. I take the earliest of all the supposed experimental verifications of special relativity -that which is described as the increase of mass of a body with velocity — not only because it is perhaps the simplest example of what is at best an extremely complex matter, but also because it serves the additional purpose of exemplifying, quite indubitably, the general oversight of the fact that *all* the supposed experimental verifications of special relativity can with exactly the same justification be advanced as verifications of Lorentz's earlier and quite different theory which is described in Chapter 8. This is so because both theories have the same mathematical structure and give indistinguishable physical interpretations to the symbols involved so far as the experiments so far performed are concerned, though there are quite irreconcilable differences of interpretation between which it is not yet possible to decide — or at least between which no existing experimental knowledge can decide, though I think the necessary knowledge would be obtained readily enough if there were a sufficient appreciation of the fact that special relativity is still possibly open to doubt. The compatibility of the mass/velocity relation with Lorentz's theory is indubitable because Lorentz himself cited it, and shown to agree with observations already made, before Einstein's theory was published.

What do we mean when we speak of the *mass* of a body — a lump of lead, for example? We mean that if we place it in one pan of a balance, and the pointer rests in the central position when a weight of 10 lb. is placed in the other pan, the mass of the lump of lead is l0 lb. But what do we mean when we speak of the mass of an electron? We certainly do not put an electron in a balance pan and compare it with weights in the other pan. We could not do so because not only can we not capture an electron but also we do not know what it is. A hundred years ago the word denoted a rather vaguely conceived unit of electricity of unknown character. By the end of the nineteenth century it seemed to have been definitely revealed as a particle of negative electricity with measurable properties of the kind familiar in ordinary matter, but thirty years later it was found to possess undeniably wave-like characteristics. The idea then arose that it was a sort of mist of electricity, and Eddington probably gave it the most candid description as 'something unknown doing we don't know what'. We are no wiser today; nevertheless, we speak of the mass of an electron as though it was equivalent to the mass of a lump of lead.

Not only so, but we give it a fairly precise value about which there is no disagreement: how on earth do we reach that value? To explain that completely (i.e. to state first, in their bare essence, the actual observations made, and then the full course of the reasoning by which it is inferred from them that the mass of the electron is such and such) I should have to write a large text-book of physics. This, of course, is out of the question here, but two things can be said about it with absolute certainty, and no one would dream of disputing them: first, that nowhere, in the whole description, would there appear any comparison of the electron with standard weights in a balance; second, that whatever choice might be made of the basic operations and the various ways of reaching the conclusion from them, it would be impossible to avoid steps depending indispensably on the Maxwell-Lorentz electromagnetic theory.

What, then, can we mean when we say that special relativity receives confirmation from the verification of its prediction that the mass of a body increases with its velocity? I need hardly say that the "velocity" of the electron in the supposed verification resembles Roger Bannister's velocity of a mile in four minutes no more closely than the 'mass' of the electron resembles that of the lump of lead, in order to make it clear that what we confirm by the experiments (i.e. by the observations and our inferences from them) is that the whole complex of conceptions that yields the highly metaphorical 'mass' and 'velocity' hangs together if we include special relativity (or Lorentz's theory) as a part of it. This would indeed argue in favour of one of those theories if that theory were *independent* of the previously existing complex of conceptions, for our object in physics is to relate apparently independent phenomena in a single system, but when the theory (Lorentz's or Einstein's) is conceived for the purpose of justifying an essential part of that complex — to wit, the Maxwell-Lorentz theory — it proves nothing at all. It is like claiming, as a proof that a man always speaks the truth, the fact that he says he does.

We shall see that this is precisely the case with this (and indeed every other) supposed confirmation of special relativity involving hypothetical particles. Einstein, as he said (see pp. 159-60), designed his theory to conform to the Maxwell-Lorentz electromagnetic theory which he accepted as equivalent to 'certain'. All that the supposed confirmations support is therefore the fact that special relativity was well designed for its purpose. They tell us nothing whatever about the truth of either electromagnetic theory or the special relativity (or Lorentz's) theory itself. An example of the illusion that they do that we have already met is that advanced by Sir Lawrence Bragg concerning cosmic rays (p. in) and expressed in the usual jargon in the editorial in *Nature* (sec Appendix) in the words, 'short-lived mesons in the cosmic rays appear to observers on the surface of the Earth to last long enough to reach the ground'. It needs not saying that the duration and distance of their fall are not measured by a stop-watch and measuring-tape but are first inferred from a course of reasoning that includes the original Maxwell-Lorentz theory, and is then 'corrected' by the special relativity theory designed for the purpose of correcting it. Is it surprising that the answer comes out right?

It is impossible to believe that men with the intelligence to achieve the near miracles of modern technology could be so stupid as to fall into his elementary error had they not, through long familiarity with the words, unconsciously come to believe that mass, time, distance, and such terms mean the same for hypothetical particles as for the world of the senses. Physicists have forgotten that their world is metaphorical, and interpret the language literally. I do not think Einstein would for one moment have regarded these cosmic ray observations as *evidence* for his theory, but only as an *application* of it. His theory in itself was wholly kinematical: it corrects electromagnetic theory because it created a new kinematics for mat end; it can therefore be *tested* only by straightforward kinematics with sensible bodies, and by reasoning in which the words used have their literal, and not their metaphorical, meanings.

These four matters — the relation between mathematics and physics, the confusion of meanings of the word *time,* the mistaken identification of co-ordinate systems with observers, and me literal interpretation of metaphors — are, I believe, the chief sources of the misunderstanding of the theory and, above all, of the illusion that it is in any way more esoteric or mystical or generally unintelligible than any other department of physics. It is, on the contrary, a rather simple theory — far simpler than Maxwell's theory of electromagnetism, or thermodynamics, for example. It is no more difficult than the first principles of Newton's kinematics; indeed, the two systems are on a par with regard to practically every feature — they are alternative systems of kinematics, i.e. of the fundamental relations between motion and the readings of measuring rods and clocks. Both had initial prejudices to face — those of Newton (though Galileo had done much to smooth the path for him) were a mixture of mediaeval ideas based on the principles of Aristotle and the metaphysical ideas of Descartes, while those of Einstein were rooted in the conviction that Newton's kinematics was unquestionable. Both were framed with an ultimate purpose in view — that of Newton was to provide a basis for a theory of gravitation, that of Einstein to provide a basis for justifying the electromagnetic equations of Maxwell and Lorentz. They are both attempts to provide an impregnable basis for all physical science — fundamental principles on which all future theories can be built with safety and must be built if they are to survive. They can therefore appeal to nothing more fundamental, but each must justify itself on grounds of pure reason allied with experiences so simple as to be unquestionable.

It would seem that only one system of kinematics could possibly satisfy this condition, and when once stated must be self-evidently true. How, then, is it possible that two different systems have succeeded in convincing scientists, over periods of many years, that they are the necessary foundations of science? We know from Einstein's critique what he regarded as the defect in Newton's system: it was that Newton had assumed, on inadequate grounds, that the time by a clock of an event at a distance from that clock had a unique value, and had omitted to state how that" value could be determined. This is certainly true: nevertheless, since Newton's theory of gravitation could not be applied to distant bodies without assigning to events on them times according to the same clock as I that used for terrestrial times, there must have been implicit in Newton's work an assumption concerning what those times were, and what that assumption amounted to was that a clock was not I affected by uniform motion. Indeed, this almost followed from Newton's first law of motion because, since all clocks in uniform motion relative to a standard clock at rest were, like that resting clock, unacted upon by a force, it was only reasonable to suppose that there was nothing to change their rate. In effect, therefore, Newton's kinematics assumed that the time of a distant event was that shown by a clock at the place of that event, that had been synchronized with a terrestrial clock when adjacent to it and then moved at a uniform speed to that place.

Lorentz, as we shall see, was the first to challenge that assumption, by postulating that motion of the clock through the ether changed its rate; but Einstein, discarding the ether, fell back on the fact that it was an *assumption* that a distant event had any unique instant of occurrence at all (or, to put it in another way, if one spoke of the time (instant) of a distant event, it was necessary to give the word a meaning, and one was free to choose the meaning). In the absence of any self-evident, necessary way of determining such an instant, Einstein claimed the right to define it in such a way as to save the electromagnetic theory without violating the principle of relativity of motion. Furthermore, he succeeded in discovering such a definition. It was a veritable stroke of genius, but it is most important to notice this. Einstein had not *disproved* Newton's implied requirement that the rate of a clock was not affected by uniform motion; he had only shown that it was not a *necessary* requirement, and that, in the absence of evidence to the contrary, any other self-consistent assumption about the effect of motion on the rate of a clock was permissible. It is because the assumption which he made has been believed to be self-consistent — and, still more effectually, because, if it is, it *does* save the electromagnetic equations and make them accord with numerous electromagnetic observations that have been made since (including, for example, the cosmic ray phenomena cited by Sir Lawrence Bragg), that Einstein's theory has succeeded in displacing Newton's. The criticism of Einstein's theory made here is that his assumption is, after all, not self-consistent because it requires each of two clocks to work steadily faster than the other, which is clearly impossible.

The way is now clear for a description of Einstein's theory and an account of the circumstances that have led to the remarkable oversight of what is, in fact, a very simple defect. This I now propose to undertake. The one thing necessary — and it is absolutely essential — is the abandonment of the now almost instinctive conviction that there is anything mysterious in the whole thing, and the recovery of trust in elementary reasoning and commonsense. If that can be achieved the rest is simple.

7

Einstein's Theory in its Original Context

To understand Einstein's theory it is necessary to know the circumstances in which it arose and to consider it in what is essentially its original form — that presented in his 1905 paper[1]. Although it has been presented in various forms since, these have always been considered as equivalent to the original one and supposed (usually wrongly, I think) to make it easier to understand. But no advocate of the theory, to my knowledge, has questioned the soundness of the original presentation (apart from a few critics of the subsidiary mathematics, who have nevertheless agreed with its conclusions), and in 1955, when special meetings were held to celebrate the jubilee of the theory, no one thought of questioning the appropriateness of that date for such an occasion, or even hinted at the possibility that the theory had changed in any way since its 1905 presentation. I shall therefore take the 1905 paper as the canonical text for our present purpose, in the accepted English translation, and consider first the general situation in physics in which it arose at that time.

One general remark, however, it is necessary to make and to bear in mind constantly throughout the discussion. When one is dealing with a particular problem in physics (or in anything else, for that matter), it must inevitably appear with a prominence, in relation to the whole subject, that it did not at all possess at the time of its origin. When I spoke of the state of physics at the beginning of the twentieth century, I meant the state of physics in so far as it was concerned with the problem which we are now discussing; but, in fact, only a small proportion of physicists were then interested in that problem. The great majority occupied themselves either with detailed applications of the familiar nineteenth-century physics to particular situations, or pursued the exciting new experimental discoveries associated with X-rays, electrons, radio-activity and such things, and left to a few specialists the discussion of the difficulties concerning fundamentals, confident that, puzzling and unexpected as these were, their solution would be forthcoming in due time in terms of traditional basic conceptions. The field of thought with which Einstein's theory is associated, though now it seems so outstanding, was then relatively obscure and insignificant. That being understood, I shall for simplicity take the liberty of referring to 'the state of physics', 'prevailing conceptions', and such things as though no other physical problem existed, except where it becomes necessary to emphasise the isolation of these considerations in order to explain the later difficulties that arose in grasping them when they emerged into prominence.

I begin, then, not with Einstein, but with the general state of the subject at the end of the nineteenth century. The basic difficulty that then faced physicists in their fundamental work was that the elementary principles of the two most comprehensive fields of thought — mechanics and electromagnetism — were incompatible with one another. Despite a few superficial difficulties, Newtonian principles appeared to be the inevitable foundation for all mechanical problems, while by this time Maxwell's field theory, as I have already remarked, appeared the equally inevitable foundation for all electromagnetic problems; and these two sets of fundamental principles were mutually contradictory. We need not consider all the discrepancies that appeared, but the most essential one, as we can now see, was that mechanics, but not electromagnetism, obeyed the relativity principle — what we now call the *special* or *restricted* relativity principle, relating only to uniform motions — that all states of uniform motion (including a state of rest) were equivalent to one another, so that, of any single body it was equally true to say that it was resting or moving with any uniform velocity that one chose: this indifference is expressed in Newton's first law of motion, that implies, in effect, that these states are all indistinguishable one from another. It was quite otherwise in electromagnetism. An electric charge at rest was surrounded only by an electric field, but an electric charge in motion was equivalent to an electric current and was surrounded by a magnetic field also. There was thus an observable physical difference between the two cases, so that motion in electromagnetism was not merely relative — the motion of one body with respect to another — but absolute — something that had detectable consequences quite irrespective of any visible standard of rest to which the motion could be referred. But since the very idea of motion implied such a standard, an invisible universal medium — the ether — was regarded as acting in this capacity, and, this, as we have seen, was made by Maxwell the basis of his theory and the indispensable physical medium for conveying light and electric waves.

For many years this appeared to contain no necessarily fundamental contradictions, because it was quite conceivable that motion of an electrified body through the ether might produce observable effects, while that of a non-electrified body — ordinary mechanical motion — might not. The ether was just another physical body, with properties that were certainly mysterious, but certainly something that could serve as a standard of rest to which the motion of ordinary material bodies could be referred. Indeed, the velocity of a material body through the ether could be determined by measuring its velocity with respect to light; for light, according to Maxwell's theory, was an electromagnetic phenomenon having a known constant velocity through the ether. Nevertheless, delicate experiments based on Maxwell's theory — of which the famous Michelson-Morley experiment was the chief — failed to detect any difference at all between the velocities, with respect to light, of bodies that were known to be moving with respect to one another. This led inescapably to the conclusion that either the ether theory or the apparently self-evident requirement of Newtonian mechanics — that two bodies moving with respect to one another must have different velocities with respect to a third body — must be wrong. Notwithstanding the respect, almost amounting to veneration, which Maxwell's theory had by that time come to command, it seemed inevitable that it was the ether theory that had to be discarded. Michelson concluded his account of his experiment with the words: 'the hypothesis of a stationary ether is thus shown to be incorrect and the necessary conclusion follows that the hypothesis is erroneous'[2].

Various attempts were made to avoid this conclusion by suitable amendments of the ether theory. I shall consider these later, but I have said enough of the general situation to indicate the circumstances in which Einstein's theory was born, and I shall now proceed to give an account of that theory. This is, in a sense, an interruption, because his approach to the problem bore little relation to that of anyone else, and it did not for many years make any impact on general thought. However, it is our chief subject now, and I have said enough of the main stream of thought to indicate the point at which it made its almost unnoticed intrusion. Einstein has left it on record that the Michelson-Morley experiment — and presumably the other experiments directed towards the same end — was not an important influence in the deliberations that led to the special relativity theory.[3] We must, of course, believe him, and it is not difficult to do so, but there is no doubt that it was the chief preoccupation of other physicists working in this field and that Einstein's theory, if it was valid, did solve the problem that confronted them. I shall now attempt to describe what Einstein did in his 1905 paper, and show, I hope, that, although it was revolutionary and profoundly original, it was in no degree at all esoteric, mystical, metaphysical, or in more than a very elementary way mathematical, but was and is wholly intelligible to any normal person acquainted with the rudiments of traditional physics. Its reputation as the supreme model of the incomprehensible is wholly spurious.

Einstein's ultimate aim, of course, was to reconcile kinematics with electromagnetism, and his method of approach differed from that chosen almost automatically by others in that it proposed a modification of kinematics rather than of electromagnetism for this end. This was its most distinctive feature, and so little is it still understood that, as we have seen elsewhere (e.g. p. 143), it is still thought by most physicists that the theory can be vindicated by electromagnetic experiments. Since it was essentially and quite openly expressed as a reform of kinematics made for the very purpose of explaining such experiments, it can be tested only in kinematical terms. All that its success in electromagnetism, however extensive and various, can show is that, *if* the proposed kinematics is tenable, then it has achieved its object; it can do nothing at all to show whether the theory is right or wrong. Einstein divided his paper into two parts, which he called 'I. Kinematical Part'; 'II. Electrodynamical Part'. The whole essence of the theory is contained in the former, on which, for the reason stated, I should concentrate attention. If that is right, the rest follows without question; if it is not, its application to all electromagnetic phenomena, of whatever kind, is worthless, despite the profound impression it has made on the 'experimenters'.

The genius of Einstein is shown most clearly in his perception of an omission from Newton's system of kinematics that had not previously been noticed and that might, as he saw, provide an opening for a reform that would reconcile the two conflicting branches of physics. In such insight he was pre-eminent in his generation: his weakness, as we shall see, lay in his relative inability to follow up the implications of his insight and in a too great readiness to accept a promising starting-point as an achieved goal. He was rather like one of a body of men imprisoned in a dungeon, who alone perceives an opening offering a means of escape, but omits to verify that it does not lead merely to another part of the dungeon. However, it is Einstein's achievement, not his psychology, that is our concern, and what he perceived was that no one had thought of the necessity of providing some means of determining the time (instant) at which a distant event occurred. Physicists were agreed on the means of measuring the time (instant) of an event close at hand — in other words, they had adopted a standard clock — but if one said that an event at a distant, inaccessible place occurred at 4 o'clock, and another said that it occurred at 5 o'clock, no unquestionable means existed of deciding which, if either, was right. Moreover, according to all the knowledge available at the time. It was impossible to choose a means that did not depend on some assumption that it was impossible then to test. Accordingly, since it was often necessary in physics and astronomy to assign an instant of occurrence to a distant event, it was necessary to bring to light the assumptions that had unconsciously been made, for, as Einstein saw, it might be that the discrepancy between kinematics and electromagnetism lay in the falseness of those assumptions. If so, their abandonment and the adoption of others might bring about a reconciliation; and indeed, if it did so, that fact would itself be a strong argument for the correctness of the new assumptions, and one might expect them in due course to be confirmed when it became possible actually to transport clocks to places then beyond reach. Einstein not only saw this possibility but also, as he believed, achieved it. Let us now see how this was done.

The problem is to define a process for determining the instant of occurrence of a distant event — or, what amounts to the same thing, for setting a hypothetical clock, situated at the scene of the event, **so** that it is synchronised with our standard terrestrial clock. To understand the problem clearly, let us begin with a simple, purely terrestrial case. Suppose that our standard clock P, whose readings are accepted as giving the instants of occurrence of events happening at the place where it is, is fixed to the ground at a point A, and we wish to set a similar clock Q, at another fixed point B on the ground, in synchronisation with P. We might, of course, bring P and Q together at A, set them in agreement, and then carefully transfer Q to B, but it would be impossible to apply this process to inaccessible places, so we must devise an alternative method. Such a method would be to take Q to B, send something that we know travels at a constant speed from A to B, and immediately back to A again, and set Q so that its reading when the travelling agent (which for brevity we will call the *signal)* reaches it is half-way between the readings of P at emission and return of the signal. It does not matter what the signal is, or at what speed it travels so long as that speed is constant with respect to the relatively stationary clocks throughout the double journey; the result will hold good irrespective of these things.

Since this is, in principle, the method adopted by Einstein for his theory, I pause here before describing the details of his application of it, to consider a few of its essential characteristics, for there are certain misconceptions of it that are extremely common and are responsible for many of the failures to recognize the defects of the theory which it is one of the purposes of this book to demonstrate. To say that two clocks are *synchronised* means, in relativity theory, exactly what we should expect, namely, that they give the same instant of occurrence of any and every event. We will consider only clocks that are relatively stationary, for these are the only clocks for which a process of synchronisation is prescribed by the theory. It is assumed, of course, that the clocks are exactly similar, so that if they are once synchronised, they will continue so, and give the same time (instant) for every event, whenever it occurs. Suppose, then, that we have a number of relatively stationary clocks, at various places, and we want to synchronise them with one another. Then our process must be such that if each of them is synchronised with the standard terrestrial clock, it ensures that they are synchronised with one another, and any one of the whole set (call it set X for simplicity) can be used to time any event occurring anywhere, and it will give the same value as any other.

It is clear that the process we have described satisfies this condition. If there happens to be a clock at the event, then the time (instant) of the event is the reading of that clock when the event occurs. If we use a distant clock, then the time (instant) of the event by that clock is its reading when the signal, emitted from the event at the instant of its occurrence, reaches it, minus the time (duration) of travel of the signal, which we know from its speed and the distance of our clock from the event. It is obvious, from the method by which the clocks are synchronised, that the result will agree with the reading of the clock at the event when it occurred.

This is most important, because it means that if any independent, uniformly-working clock, (which may or may not be similar to the clocks of our set X) travels from any one to any other clock of the set X, and the difference of its readings at the beginning and end of the journey is less than the difference between the readings of the clocks of the set X with which it coincides at the beginning and end of the journey, then the travelling clock is working at a slower rate than the clocks of set X. For example, if the travelling clock leaves a clock of set X when both read 1 o'clock, and reads 2 o'clock when it reaches another clock of set X which then reads 3 o'clock, the travelling clock is running at half the rate of the X-clocks, for it gives a duration of 1 hour for the journey while the X-clocks (which all run at the same rate) give a duration of 2 hours. It does not matter in the least that different X-clocks are used to give the instants of beginning and end of the journey, for all the clocks of the set give the same instant for every event. This at once disposes of McCrea's objection (p. 85) that, according to the theory, one cannot compare the rates of two single clocks with one another. The process of synchronisation was devised for the very purpose of timing events by clocks, which were at a distance from them, and indeed, merely to say that two single clocks are synchronised is to compare them with one another.

A second very important point is that the process of synchronisation prescribed by the theory is an *experimental* one, and therefore wholly objective. It does not matter who makes the experiment: if f he does it correctly he will get one unique result. The clocks are synchronised if the reading of the distant clock when it receives the signal is halfway between the readings of the standard clock at emission and return of the signal. It is, however, extremely common to read that, according to special relativity, clocks which are synchronised for one observer are not synchronised for a relatively moving observer. It is sufficient to cite as an example a letter from W. Barrett[4] in the *Nature* correspondence following my discussion with McCrea (see Appendix), in which he claimed to refute my argument by the consideration that clocks which are synchronised for A are not synchronised for a relatively moving observer B. But it should surely be obvious that the readings of the clocks when they encounter the signal cannot depend in the least on who happens to observe them; their photographs could be examined afterwards by anyone at all and it is the relation between those readings alone that determines whether the clocks are synchronised or not. This is just one of the many evil effects of introducing 'the observer' into the theory, where he has no place at all, and in this case not even co-ordinate systems are relevant: if clocks are synchronised they are synchronised absolutely, for all observers and all co-ordinate systems.

It is worth while slightly to anticipate what is to follow by pointing out here that this, in fact, is another of the anomalies of the theory that might have been chosen to show its untenability. For, as we shall see, the theory requires that although *clock-readings,* which are events that can be observed, are absolute, the *times* (instants) at which the clocks have those readings vary with the co-ordinate system chosen. Thus, if two separated clocks are synchronised, the times (instants) at which they read 2 o'clock, say, will both be 2 o'clock in a co-ordinate system in which they are regarded as being at rest, while in a co-ordinate system in which they are regarded as moving (with the same velocity, of course), the times (instants) will not only be different from 2 o'clock but different from one another. Hence the theory requires that clocks which are synchronised by the process which it prescribes ('They are "good" clocks and are *synchronised,* which means that they show the same time simultaneously' — Einstein and Infeld, *The Evolution of Physics,* p. 190), nevertheless give different times (instants) for the same event. It is clear enough, I think, that these requirements are contradictory, and might have been used, as I say, to show the untenability of the theory, but I have thought it best to choose as the paradigm contradiction the one given on p. 45, since that puts the matter in the form of a question, and the absence of a reply to a question speaks more eloquently than the absence of comment on a statement.

Let us, however, return to our description of the theory. We have shown how two relatively stationary terrestrial clocks, fixed at points A and B on the ground, can be synchronised. But now suppose that the points A and B, instead of being fixed to the ground, are carried on two aeroplanes, the same distance apart, and travelling at the same speed in the same direction, which is that of the line joining them, so that they are relatively at rest but both moving uniformly with respect to the ground and to the air, in which we suppose no wind blows. Suppose, to complete the picture, that A is in the rear. In this case, unlike the former one, it *does* matter what kind of signal we use, and at what speeds it and the aeroplanes travel, for we shall get different results for different choices of these things. If the signal is a bullet fired from a gun at A, it will travel at the same speed both ways (we neglect any resistance offered by the air) *with respect to A and B,* but *not with respect to the air or the ground.* If, on the other hand, the signal is a sound wave emitted from a whistle on A, it will travel at the same speed both ways *with respect to the air and ground,* but *not -with respect to A and B,* for the speed of sound depends only on the properties of the air through which it travels, and not at all, like the bullet, on the velocity of the source from which it is emitted. The result will be that the reading which is halfway between the readings of A at emission and return of the signal will be different in the two cases — and, moreover, the difference will vary with the speeds of the signal and aeroplanes. Consequently, before we can set Q so that it synchronises with P, we must decide what signal we shall use and what its speed shall be.

In this case, of course, in which we have other means of synchronising P and Q so as to get the result most suitable for conducting terrestrial affairs, we have no difficulty in deciding that it is the bullet that gives the 'right' result, and its speed is immaterial however fast the aeroplanes are moving. But it is quite otherwise when we are dealing with great distances, for here there is nothing to guide us in making our choice other than the need to make our observations fit together in a rational way. This was what Einstein saw, and accordingly he proposed the choice that he realised would reconcile mechanics with electromagnetism. He chose light (or, in general, electromagnetic waves) as the signal, and assumed, on the basis of electromagnetic theory, that it travelled between any two points with a constant velocity *c* which, like the velocity of sound through air, was independent of the velocity of the source from which the light was emitted. But that meant, just as with the clocks on the aeroplanes synchronised by sound waves, that the synchronisation of the clocks (and therefore the instant, according to the standard clock, at which a distant event occurred) depended on the speed of the standard clock — i.e. on the speed of the Earth, for the standard clock of physics must be stationary on the Earth. Hence we must know the speed of the Earth through the medium in which light waves travel (which is the ether, according to the Maxwell-Lorentz theory) before we can assign a time (instant) to the occurrence of a distant event. But this we do not know; the Michelson-Morley experiment, like all others, had failed to determine it. Here Einstein made his second assumption, (it is usually called the first, and the assumption that the velocity of light is independent of the motion of its source is called the second, but that is immaterial), which was that there was no ether with respect to which velocity had any meaning, so that all states of uniform motion of bodies were equivalent.

A reference to Maxwell's fundamental paper — or even to the extract from it given on p. 132, will show that this was a direct contradiction of Maxwell's basic axiom, that there existed an ether with respect to which the velocity of a body had a definite, in principle measurable, value. Light consisted of vibrations in that ether, that had physical properties, which also were, in principle, determinable. What Einstein was proposing, therefore, was to retain the finite velocity of light without the existence of any standard with respect to which that velocity had a meaning. Light consisted of waves, with a definite length, frequency and velocity, in nothing; it was the grin without the Cheshire cat. As I have said, this theory made no general impression at all at first, so the apparent absurdity of this called forth no appreciable protest (though I remember hearing Sir Oliver Lodge satirising it before Einstein's general theory brought his special theory into prominence), but the fact that it could have been proposed at all is inexplicable until we remember the nature of the acceptance which Maxwell's theory was accorded at that time, which was so well expressed by Hertz — 'Maxwell's theory is Maxwell's system of equations'. The *physical* part of the theory was expendable; only the equations needed to be saved. Einstein saw a way of saving the equations, and did not consider it worth while to 'explain' light. Kelvin was not willing to explain it in terms of 'things that we understand less of: times had so changed that Einstein was satisfied to 'explain' it in terms of things that we understood nothing of — in other words, not to explain it at all. If his assumptions were granted he did save the equations, and when his theory ultimately made its general impact on the world, mathematics had so dominated physics that the non-existence of the Cheshire cat was regarded as a triviality; the grin remained, and all was well.

However, there was an apparent absurdity that did *not* escape such notice as was taken of the theory, and that was that its two postulates — that the velocity of the signal was independent of the motion of its source, and that there was no ether (i.e. nothing 'corresponding to the idea of absolute rest', as Einstein put it, thus ruling out all possible kinematical connotations of the word 'ether') — seemed to contradict, not some independent fact or idea, but each other. If the velocity of light was finite, and there was no ether with respect to which it had that finite velocity, the only apparent alternative was that each beam of light had that velocity only with respect to its own source, and this the theory denied. However, apparent contradictions were at a discount, but what the two postulates, taken together, did imply was that the time (instant) of a distant event had now to be granted an infinite number of different values, all equally 'right'. For suppose we wish to date an event on a distant star. We send a beam of light from the Earth to reach the star at the moment of occurrence of the event and note the readings of our clock at the instants of sending it out and receiving it back. The time (instant) of the event is then halfway between these readings. But now suppose that, at the instant of sending out the light, there is another clock, momentarily coincident with the Earth clock, which is moving rapidly away from the Earth towards the star, and that its reading at the moment of coincidence agrees with that of the Earth clock. It also sends out a beam of light at that instant, and by Einstein's postulate that beam will travel together with the beam from the Earth as though they were a single beam. But clearly the second clock will receive the returning beam before it reaches the Earth, and therefore show an earlier reading for return. Its halfway value will therefore be earlier, and its time (instant) for the distant event will also be earlier, than the Earth clock's.

Which is right? We cannot say, because there is no ether to enable us to ascribe the relative motion of the two clocks to one rather than to the other. The motion of the star on which the event occurs has nothing to do with the matter, for all the events considered in the theory are supposed to be instantaneous, so there is no meaning in speaking of their motion. If a number of bodies, coming from all directions, happen to collide at a point at a single instant, that is one event, and it cannot be credited with the motion of any one of the bodies. There is no alternative but to allow that the time (instant) of a distant event has an infinite number of values, all equally 'right'.

However, this implication of the theory, which at first seemed so unacceptable, contains no contradiction. When it is once realised that the time (instant) of a distant event stands in need of a definition, no reason can be given why that definition should not be such as to give it many values rather than one. Neither of Einstein's assumptions, or 'postulates' as he called them, is in itself illegitimate. The postulate that the velocity of light is independent of that of its source accorded with the requirements of the already existing electromagnetic theory, and the postulate that any state of uniform motion may be ascribed to a single body accorded with the requirements of the already existing Newtonian mechanics. Although, as I maintain, there are contradictions in adopting both postulates and still regarding clocks as instruments for measuring time (instants and durations), the multiplicity of values is not one of them. The ancient Hebrews took it for granted that each star had a unique name, which only Jahweh was entitled to give it. We regard the naming of stars as a matter of free choice and free definition, and having decided to name them we do not hesitate to call one bright particular star ? Canis Majoris, Sirius or the Dog Star without feeling that we are open to criticism.

But what we are not entitled to do is to suppose, at the same time, that any of our freely chosen names is valid and also that only Jahweh's unknown unique name is valid. Einstein was justified in freely choosing a definition of distant times (instants), provided that he then meant by 'time' *only* what the definition required. He was not justified in supposing *also* that time (instant) was what the previously accepted instruments called *clocks* would record in prescribed circumstances, unless experiment showed that that was so, and the necessary experiment was, and still is, impossible for practical reasons. His theory, in fact, consisted in the postulate that clocks *would,* in fact, give readings that accorded with his definition.

But that is just what they cannot do, as may be seen without the need of experiment. To take his own example, if there are two clocks, relatively stationary and synchronised, at points A and B, and then 'the clock at A is moved with the velocity *v* along the line AB to B, then on its arrival at B the two clocks no longer synchronise, but the clock moved from A to B lags behind the other which has remained at B'.[1] But it is one of the postulates of the theory that *either* clock can be 'the clock moved from A to B', for you can assign the letters so that B is the point at which they finally come together. The means by which the movement is brought about plays no part in the matter, for the theory can tell you nothing about what effect, if any, it produces; it tells you only about effects of *motion,* however produced. If A and B are points on the station platform, then 'the clock moved from A to B' at 60 miles an hour may be the one to which a force is applied moving it from east to west. If they are points on a train moving eastwards through the station at 60 miles an hour, the same force applied to the foremost clock reduces it to rest with respect to the platform, while the other moves from west to east at 60 miles an hour. Hence, according to Einstein's statement, the clock whose rate is slowed down is in the first case that to which the force is applied, and in the second case that to which the force is not applied. It is therefore clear that, however sound Einstein's reasoning may be, he cannot maintain his definition of time (instant) and still use clocks to measure it. He did not, of course, propose to discard clocks in favour of an arbitrary definition, so he proposed, as a theory, that clocks *would* conform to the requirements of the definition. We may sum up the whole theory in the following way:

(1) The time (instant) of an event is given by the reading of a clock of an agreed type, which is not here in question.

(2) If the event is at a distance from the clock, a process, which may be freely chosen, must be prescribed for defining the reading of the clock that gives the time (instant) of the event.

(3) The theory prescribes a simple process for this purpose, based on assumptions familiar elsewhere in physics, which requires that clocks in uniform relative motion work at different rates, the 'moving' clock working more slowly than the 'stationary' one.

(4) The theory is therefore open to experimental test, at present impracticable, by a comparison of the rates of relatively moving clocks.

As I have just shown, the experimental test is unnecessary because the theory itself makes the 'stationary' and 'moving' clocks interchangeable by pure thought, and so requires the impossibility that each clock works more slowly than the other. It appears astonishing that Einstein could have overlooked so simple a fact, until one realizes the mastery which mathematics had acquired over the intelligence of even its most illustrious practitioners, and the rich reward which the theory offered if it could be right. But apart from this, there are two things, already noted but bearing repetition, that may be specially stressed.

The first is the extreme simplicity and ordinariness of the theory, and its freedom from any reference at all to time (eternity) and from all cabbalism (except, of course, the difficulty of conceiving of light waves without an ether, but that is rather the absence of what one feels ought to be said than a difficulty in understanding what is). The second is that the theory is wholly kinematical, electromagnetism having nothing to do with it. It does introduce light, but only as something having a velocity; the *nature* of light does not enter the theory at all. The connection with electromagnetism is simply that it was the desire to justify the Maxwell-Lorentz theory (i.e. its equations), that led Einstein to choose the particular definition of distant times (instants) that he did choose. That is why his theory was able (supposing it to be tenable) to reconcile kinematics with electromagnetism and make the Maxwell-Lorentz theory, in Einstein's words, a 'plausible' theory.[6] But the theory itself is wholly kinematical, and stands or falls by kinematical considerations alone. As I have repeatedly said, none of the supposed electromagnetic experiments and observations (including those connected with cosmic rays/can possibly do more than show that *if* the theory could be right, it would achieve its purpose, it would provide an effective correcting factor to the electromagnetic equations. But such experiments and observations, individually or collectively, are, as evidence for the truth of the theory, completely valueless.

I conclude this chapter with Einstein's own most succinct statement of the theory, to confirm that the account I have given is a true one:

> In order to give physical significance to the concept of time, processes of some kind are required which enable relations to be established between different places. It is immaterial what kind of processes one chooses for such a definition of time. It is advantageous, however, for the theory, to choose only those processes concerning which we know something certain. This holds for the propagation of light *in vacuo* in a higher degree than for any other process, which could be considered, thanks to the investigations of Maxwell and H. A. Lorentz.[6]

8

Non-Einsteinian Relativity

As I have said, Einstein's theory stood outside the main stream of physical thought during the years between its inception and the end of the first world war. I return now to the more general historical account.

We left it at the point at which the conflict between Newtonian mechanics and Maxwell-Lorentz electromagnetism was at its sharpest, and the outstanding demonstration of this conflict was the Michelson-Morley experiment. This has been described a countless number of times, but never, so far as I know,* without the tacit introduction of an interpretation of the experiment that vitiates the deductions drawn from its results. The experiment, described in outline as far as possible without such an introduction, was simply this. A beam of light was split into two parts, which were sent, by means of mirrors, to and fro along two equal mutually perpendicular, material arms. On returning to their starting-point they interfered with one another, producing a pattern of dark and bright fringes of the kind familiar to students of optics. Consider the case in which one of the arms lay along the direction of the Earth's orbital motion around the Sun according to the universally accepted Copernican astronomical system assumed in Newtonian mechanics. At two times (instants), six months apart, that motion would be in opposite directions, so that, according to the Maxwell-Lorentz electromagnetic theory, in which the velocity of light was independent of the motion of its source, it is easily calculated that the fringes seen should be in different positions at the two instants. In fact, however, the fringes remained in the same position throughout the year.

Clearly something was wrong, and there were three possibilities concerning that something: (1) the Maxwell-Lorentz theory was wrong; (2) Newtonian mechanics was wrong; (3) there was some unknown effect of motion that had been neglected. But what was universally overlooked (apart from a later suggestion by Ritz, of which more presently) was that the first possibility was virtually excluded from consideration by the manner in which the experiment was described. It is practically always presented as a comparison of the *times* (durations) taken by the two beams of light to travel along their respective paths, and it is stated that these durations, which should have varied during the course of the year, kept constant throughout the year. But in fact no 'times' at all, of any kind, were measured. No clocks were used, so that no modification of 'time', however the word be construed so long as it is regarded as something related to clocks, could have anything to do with the result of the experiment. Before the experiment can be expressed in terms of 'times', the fringes observed must be interpreted in terms of the Maxwell-Lorentz electromagnetic theory, so that that theory is embedded in the very description of what has to be explained. Consequently, possibility (1) is quite illegitimately ruled out before we begin the explanation.

* I must, of course, except a more detailed account of the considerations presented here which is given in a paper of mine in *Vistas in Astronomy,* 9, 97 (1967).

As we have seen, Einstein adopted possibility (2), which, though not proposed primarily for the purpose of explaining this experiment, was in fact applied to that purpose. But before that, possibility (3) — that there was some neglected effect of motion — had been suggested, first by FitzGerald and later independently by Lorentz; and here we meet another of those extraordinary oversights, of the same type as that by which Maxwell's theory is universally held to be at bottom identical with Faraday's although it is fundamentally different, which arise from the neglect of the study of the history of science, and the general practice by which erroneous statements, when once published, tend to be repeated perpetually by later writers.

The neglected effect of motion proposed by FitzGerald was a change in the dimensions of material bodies caused by their motion through the ether. Consider the case in which one arm of the apparatus (the *longitudinal* arm) lies in the direction of the Earth's orbital motion, while the other (the *transverse* arm) is at right angles to that direction. It is clear that if the length of one of those arms were changed by the motion while the other remained unchanged, a shift of the interference fringes would be expected to be caused thereby, for the distances travelled by the two beams of light would no longer be equal. If, however, this effect were equal and opposite to that expected to occur on account of the motion of the arms relative to the light, the absence of a fringe-shift would be accounted for. This is what FitzGerald proposed, on the basis of the electrical theory of matter which was very prominent at that time (the early 1890-s), and the suggestion is always known as the FitzGerald *contraction.* FitzGerald appears to have left no account of it, and we know it only through the report of it by Sir Oliver Lodge. The usual reference to this report is a brief indefinite mention in *Nature,* but a much fuller account — the most complete, I believe, that we have — is given in Lodge's book. *The Ether of Space*[1]. Here he records that the suggestion was made verbally to him while they were discussing the problem in Lodge's study in Liverpool, and he gives the following account:

> Electric charges in motion constitute an electric current. Similar charges repel each other, but currents in the same direction attract. Consequently two similar charges moving in parallel lines will repel each other less than if stationary, — less also than if moving one after the other in the same line. Likewise two opposite charges, a fixed distance apart, attract each other less when moving side by side, than when chasing each other. The modification of the static force, thus caused, depends on the squared ratio of their joint speed to the velocity of light.
>
> Atoms of matter are charged; and cohesion is a residual electric attraction. So when a block of matter is moving through the ether of space its cohesive forces across the line of motion are diminished, and consequently in that direction it expands, by an amount proportioned to the square of aberration magnitude.
>
> A light journey, to and fro, across the path of a relatively moving medium is slightly quicker than the same journey, to and fro, along. But if the journeys are planned or set out on a block of matter, they do not remain quite the same when it

is conveyed through space: the journey across the direction of motion becomes longer than the other journey, as we have just seen. And the extra distance compensates or neutralises the extra speed; so that light takes the same time for both.

Lodge's account, it is true, does not make it perfectly clear whether this is his explanation of the effect or FitzGerald's, but since he leaves no doubt that the fundamental idea was FitzGerald's, it is unlikely that he would change it without saying so, and in that case there is no such thing as the 'FitzGerald contraction'; it is FitzGerald expansion, for, according to this explanation, it is not the longitudinal arm that is contracted but the transverse arm that is lengthened — the effect on the fringes, of course, being the same. To put the matter in a nutshell, an unelectrified rod at rest, according to the ideas of the time, consisted of equal quantities of positive and negative electricity in some form or other. When the rod was set in motion, these charges became two parallel currents of electricity in opposite directions, and such currents were known to repel one another. Accordingly, there was a force increasing the *breadth* of the longitudinal arm (which did not affect the path of the light) and the *length* of the transverse arm. Hence the FitzGerald effect was not a contraction of the former but an expansion of the latter.

However, independently of this and in ignorance of it, Lorentz in 1904[2] proposed a much more comprehensive theory which, if valid, not only explained the null result of the Michelson-Morley experiment but provided a supplement to the Maxwell theory which implied that any experiment with material systems, carried out on bodies moving uniformly with respect to one another, would give exactly the same result, so that it would be inherently impossible to tell, from an experiment confined to a body, whether that body was at rest or moving uniformly through the ether. His proposal was that motion of a material body through the ether produced a contraction in the direction of motion, and a slowing down of all rhythmical processes, both by the factor $(1 - v^2/c^2)^{1/2}$, where v was the velocity of the body and c the velocity of light. Lorentz showed that if these physical effects were a reality, the relation between the co-ordinates, (x, t) of an event referred to one system, and the coordinates, (x', t'), of the same event referred to a system moving uniformly in the direction with respect to the first (for simplicity we consider one direction only — that of the relative motion — and suppose certain initial conditions to be satisfied) was given by the equations

$$x' = \frac{x - vt}{\sqrt{1 - v^2/c^2}}$$

$$t' = \frac{t - vx/c^2}{\sqrt{1 - v^2/c^2}}$$

These equations are known as *the Lorentz transformation.* Mathematically their significance lies in the fact that, in mathematical language, the equations of the Maxwell-Lorentz electromagnetic theory are invariant to them; that is to say, if, for x and t in those equations, we substitute the values given by the Lorentz transformation, we obtain identical equations with *x', t'* taking the places of *x, t,* and *v* changing to - *v.* This guaranteed that all measurements made on cither of two bodies, in uniform relative motion with velocity *v* (or - *v),* when interpreted in terms of the Maxwell-Lorentz theory, would be related in the same way, so that no physical observations confined to either of the bodies could distinguish the motion of that body from the motion of the other. It would still be possible, of course, by comparing observations on the two bodies, to detect effects of their *relative* motion, but experiments such as that of Michelson and Morley, for example, which were confined to the Earth, could not reveal the motion of the Earth.[*]

This proposal became known as *the relativity theory of Lorentz,* and certain features of it call for attention here. In the first place, Lorentz recognised that it was a purely *ad hoc* hypothesis: it did not, like the more limited FitzGerald suggestion, give any explanation of the proposed physical effects. These were proposed simply because they led to a transformation to which the equations of the electromagnetic theory were invariant. 'It need hardly be said,' wrote Lorentz, 'that the present theory is put forward with all due reserve.' Nevertheless, it was a *physical* theory, not a *mathematical* one; that is to say, the proposal was that motion through the ether produced physical effects on bodies, and the mathematics expressed the physical results produced. Like Maxwell, who realised the necessity, if he was to satisfy his mathematical desires, of postulating a 'displacement current' to justify them, so Lorentz, in order to justify his transformation equations, saw the necessity of postulating a physical effect of interaction between moving matter and ether, to give the mathematics meaning. Physics still had *de jure* authority over mathematics: it was Einstein, who had no qualms about abolishing the ether and still retaining light waves whose properties were expressed by formulae that were meaningless without it, who was the first to discard physics altogether and propose a wholly mathematical theory.

Moreover, Lorentz entitled his paper, 'Electromagnetic Phenomena in a System moving with any Velocity less than that of Light' — thereby implying that, unlike the later theory of Einstein, his proposal did not prohibit velocities of systems greater than that of light. Again, there was no suggestion of any modification of Newtonian mechanics, of which it is certain Lorentz had no intention at all. But, in view of later events, perhaps the most serious aspect of the comparison of Lorentz's theory with Einstein's was the fact that both were called 'relativity' theories, for this, as we shall sec, has led to a confusion that I think has been the most effective agent in allowing Einstein's theory to persist so long in spite of its manifest impossibility. Strictly speaking, the name 'relativity theory' should be applied only to a theory that regards motion as a purely relative phenomenon — i.e. a theory that, like Einstein's, allows no ether. Lorentz's theory demanded an ether. He, and the great majority of his contemporaries, never doubted the physical reality of the ether, as something that both had physical properties

[*] It is important to notice that, on Lorentz's hypothesis, it would still be possible to detect motion through the ether if velocities so high that terms of higher order than the second became significant were attained. Though this was, and is, practically impossible, it affords a theoretical distinction between the requirements of the theories of Lorentz and Einstein, which both include the Lorentz transformation.

and could serve as a standard of rest with respect to which 'absolute' velocities had a definite meaning.

The general effect, however, of Lorentz's theory was one of acceptance. It received the powerful support of Poincare, whose influence in this field at that time was very great; and with those who were seriously concerned with this problem, who were mostly more mathematically than physically minded, the physical arbitrariness of the theory was less impressive than the mathematical completeness with which it made the Maxwell-Lorentz electromagnetic equations tenable in spite of the menace of the experiments. As I have explained, Maxwell's theory had already been largely reduced to Maxwell's equations in the minds of physicists, so that to save the equations was, in effect, to save the theory. But, it must be repeated, the workers in this field were few, and even Einstein has stated that when he conceived his theory he did not know of Lorentz's work. This is perhaps not so strange as it might appear, for although Lorentz's paper was published in the year before Einstein's, it must have taken Einstein much more than a year to bring his very novel ideas to the state of maturity revealed by his 1905 paper. But, very different though the two theories were, they did result in the same mathematical equations — those of the Lorentz transformation — and this fact, together with their common name, 'relativity', goes a long way towards accounting for their subsequent confusion with one another.

But we shall not understand the position at all unless we realise that, from 1904 until the eclipse observations of 1919 that brought Einstein's *general* relativity theory to everyone's notice, 'the theory of relativity' meant, to almost all concerned, Lorentz's theory: Einstein's, if it was known at all, was regarded merely as a more obscure form of a theory that belonged to Lorentz. The name, 'Lorentz transformation', which is still used to denote the mathematical part of Einstein's theory, is a relic of that identification. Both Lorentz and Einstein, of course, knew the difference, but very few others did, or, for that matter, do now. The difference between that time and ours is that the earlier workers ascribed the supposedly single 'relativity' theory uniquely to Lorentz, and we to Einstein. Whittaker, in his *History** partly restored to Lorentz the credit due to him, but even he made the mistake of identifying two quite different theories. As a pure mathematician he was naturally predisposed to label a theory by its mathematical rather than its physical content, and since that was the same for both theories, he credited it to the author of the earlier paper. He had, moreover, first-hand recollection of the circumstances of origin of the relevant papers. But Lorentz himself said, so late as 1928, 'the theory of relativity is really solely Einstein's work',[4] and he was, of course, speaking at a time when the phrase 'the theory of relativity' had really come to mean to everyone a theory devised by Einstein — and, as Whittaker rightly points out, Lorentz did not accept it.

The present ignorance of the state of physical thought before Einstein's *general* theory appeared is so universal and of such cardinal importance to an understanding of the existing confusion, that a few of the many facts that anyone who cares to examine the literature of the time can verify for himself, should be cited in illustration of it.

Ritz, who was alone in adopting what I have called (p. 161) possibility (1) of explaining the Michelson-Morley experiment, in a paper of more than a hundred pages published in 19085 criticising the existing electromagnetic theory, scarcely mentions Einstein; he is wholly concerned with Lorentz's justification of the theory, and no one reading it would imagine that Einstein had done anything at all in connection with the matter. Lodge, in the book already mentioned[1] (published in 1909), also does not mention Einstein; he regards Lorentz alone as the author of a complete system into which FitzGerald's idea fitted. Poincare, in an address in 1912 published posthumously, says, during a discussion of a paper of Einstein's on the action of light on molecules: "nous n'avons qu'a appliquer le principe de relativite de Lorentz".[6] It is inconceivable that he would have used such a phrase in such a connection had he regarded Einstein as having any claim at all to authorship of 'le principe de relativite'.

Max Born first heard of the theory through attending the lectures of Minkowski, in which 'we studied papers by Hertz, FitzGerald, Larmor, Lorentz, Poincare and others, but also got an inkling of Minkowski's own ideas'. Later, 'I went in 1907 to Cambridge', where he heard nothing of Einstein, and afterwards (how long afterwards he does not say) he returned to Breslau, 'and there at last I heard the name of Einstein and read his papers ... Although I was quite familiar with the relativistic idea and the Lorentz transformations, Einstein's reasoning was a revelation to me.'[7]

These are all scientists concerned especially with the problem in question. As an example of the views of a more general physicist (but nevertheless one who would be specially watchful for publications relating to Newtonian mechanics) it is sufficient to cite an article by Professor Louis T. More of the University of Cincinnati, Ohio, the author of what the late Professor Andrade, so late as 1954, described as 'the standard life' of Newton. In an article in *The Hibbert Journal* for July 1910 on 'The Metaphysical Tendencies of Modern Physics' (which he deplored) he wrote:

> From the large number of physicists now writing on the theory of physics, three names stand out prominently as originators of the modern conceptions of electricity and matter. Professor H. A. Lorentz, Sir Joseph Larmor, and Sir Joseph Thomson are certainly the men who will be most prominently associated with this movement; others have aided, but mainly in the extension or modification of their ideas.[8]

There is no mention in the article of either Einstein or Minkowski (whose work will be considered almost immediately). Could he possibly have omitted these in an article on such a subject unless he had been unaware of them (which is unlikely) or regarded them as having merely 'extended or modified' the ideas of Lorentz?

I think this will be enough to show how little Einstein was thought of in connection with the theory of relativity until his name became associated with it through his general theory. Born in Germany, Ritz in Switzerland, Poincare in France, the Cambridge physicists in England, More in America — all physicists concerned with this or closely related fields of work — failed to connect Einstein's name with it for years after his definitive paper had appeared. But one man — Minkowski, a mathematician, not a physicist, whose contribution, in view of its later outstanding influence compels notice here — had certainly heard of Einstein as well as Lorentz, though he, a mathematician, did not see how essentially distinct the two theories were, and Born, who attended his lectures on the subject, reports that he did not hear the name of Einstein mentioned in them. His distinctive feature, among the few to whose notice the work of both men had come, was that he regarded Einstein's presentation of the theory as the one to be preferred, and that precisely because he was a mathematician and not a physicist. He gave yet another form to the general complex of ideas which later became known as the special theory of relativity, and this must be given special notice here, not only because it provided Einstein with the type of mathematical machinery which he was to use in his later general theory, but also, and chiefly, because it contributed perhaps more than any other single factor to the transformation of mathematics from the servant into the master of physics, and introduced more false ideas into the subject — pre-eminently the totally irrelevant idea of time (eternity) — than anything else. It is to Minkowski that we owe the idea of a space-time' as an objective reality — which is perhaps the chief agent in the transformation of the whole subject from the ground of intelligible physics into the heaven (or hell) of metaphysics, where it has become, instead of an object for intelligent inquiry, an idol to be blindly worshipped.

Minkowski's thoughts on the matter were published first in a highly technical paper in 1907, but in the following year he gave a relatively popular account which has been translated into English and is the medium through which it is now best known: I shall take it as the basis for comment here.[9]

Reduced to its essence, Minkowski's paper is a piece of pure mathematics — as such, extremely elegant and admirable, but, insofar as it purports to contribute to physics, as it does, calamitous. He takes (quite arbitrarily, if we regard his paper as *sui generis* as it claims to be) a particular mathematical expression,

$$c^2t^2\text{-}x^2\text{-}y^2\text{-}z^2=1$$

(which I will reduce to $c^2t^2—x^2=1$ for simplicity, since y and z play no part in the development of the work but merely give plausibility to the claim that the mathematics has a necessary physical significance) and shows that it is invariant to the transformation of co-ordinates already known as the Lorentz transformation. He gives also a very felicitous geometrical representation of the algebra, which has greatly simplified the task of giving the work a presentable form.

Now if we regard Lorentz's, or even Einstein's, theory as legitimate physics, no objection whatever can be made to Minkowski's method of representing its mathematical structure; on the contrary, it evokes respectful admiration. But Minkowski went much further than this. 'I should like to show', he says, 'how it might be possible, setting out from the accepted mechanics of the present day, along a purely mathematical line of thought, to arrive at changed ideas of space and time.' Indeed, he reproaches mathematicians for not anticipating physicists in arriving at the Lorentz transformation as a physical transformation.

> It looks [he says] as though the thought might have struck some mathematician, fancy-free, that after all, as a matter of fact, natural phenomena do not possess an invariance with the group G_∞ [the Galilean transformation] but rather with a group G_c [the Lorentz transformation], c being finite and determinate, but in ordinary units of measure, *extremely great.* Such a premonition would have been an extraordinary triumph for pure mathematics. Well, mathematics, though it can now display only staircase-wit, has the satisfaction of being wise after the event.

That is to say, the process of allowing mathematics to direct physics, which began with Maxwell — albeit apologetically and with a recognition of the necessity for a physical justification for following the direction — had now reached a point at which it is taken as the proper function of mathematics to order physics along the path which mathematics points out, and mathematics is chided for neglecting this duty and allowing physics to choose its own way. The return to mediaeval scholasticism, against which the protest of Bacon and the other pioneers of modern science was thought to have been finally successful, was now complete. With Minkowski's work physics had escaped from experiment and been captured by mathematicians.

How could this happen? Mainly, I think, because, as we have seen, the ground had already been partly prepared for it, but also because Minkowski's work was then hardly known and quite unnoticed by physicists; its influence came much later when, as we shall see, it was made overwhelming by the reinforcement which it received from the mathematical form of Einstein's *general* theory. But just consider what Minkowski's work actually was. Remember that, according to his own assertion, he is writing as a mathematician, doing what mathematicians should have done before the physical considerations of Lorentz and Einstein had been conceived. He takes — quite arbitrarily, of course, in these circumstances — one of an infinite number of mathematical expressions (he might as well, of course, have taken, say, $x — ty^2/z$ or any other), and finds the transformation of symbols to which it is invariant. Mathematically the symbols are just symbols and nothing else. He then says, again entirely arbitrarily, 'Let *x, y, z* be rectangular co-ordinates for space, and let *t* denote time... The multiplicity of all thinkable *x, y, z, t,* systems of values we will christen the *world.'* Why? Why should not a mathematician ('fancy-free'!) equally well say, 'Let *x* denote pressure, *y* volume, *z* specific heat, and *t* temperature', then choose some combination of them, and announce laws of thermodynamics which would save the physicists the trouble of making observations? Of course, as anyone can see, Minkowski was entirely dependent on what was known or believed on physical grounds for his very choice of starting-point, yet he claims that what he did should have been done *a priori* with no physics at all. It is no wonder that Einstein's reasoning was a revelation to Max Born, who had become familiar, as he says, 'with the relativistic idea and the Lorentz transformations' through attending the lectures of Minkowski. Compared with Minkowski's approach, Einstein's, though much less physical than Lorentz's, was empirical in the extreme.

But there is another, even more deleterious consequence of Minkowski's contribution that we must notice. In Chapter 6 I described four misconceptions characteristic of the modern appreciation of relativity theory, of which the first two were the subordination of physics to mathematics and the confusion regarding the word 'time'. Minkowski brought the first to completion, but he was almost, if not quite, wholly responsible for the second. When he wrote 'Let *x, y, z* be rectangular co-ordinates for space, and let *t* denote time', he introduced something new and something wholly metaphysical into the subject. Never before in physics — not even in the theories of Lorentz and Einstein — had *x, y, z* denoted space or *t* time (eternity); they had meant respectively *place* and *time (instant).* In no application of any formula of physics, and in no co-ordinate diagram, does any co-ordinate have the significance of an indefinitely extended continuum, for the simple reason that all the formulae and diagrams represent relations between what can be observed, and space and time (eternity) cannot be observed. When we plot volume against pressure in a thermodynamic graph, no one dreams of the F-axis as representing space; it is simply a direction along which we mark off the measurable volume occupied by the material system we are considering; and the same restriction applies to pressure. Hence, when Minkowski let *x, y, z* represent space, and *t,* time (eternity), he was doing something quite unrelated to physics, and his famous conclusion, 'Henceforth space by itself, and time by itself, are doomed to fade away into mere shadows, and only a kind of union of the two will preserve an independent reality', is utterly unwarranted: it is a conclusion about things not dealt with in physics, drawn from a purely gratuitous interpretation of an arbitrarily adopted mathematical formula. It is just as true or false or meaningless as an assertion that pressure and volume are shadows and only a kind of union of the two is real. The fact — if it be a fact — that the Lorentz transformation is physically significant simply means that, in the expression of the relation between our measurements when we refer them to different co-ordinate systems, the values of *x, y, z, t* are associated with one another and are not such that *t* always remains separate from the others. That no more means that space and time (in any sense) are arbitrary parts of a single absolute whole than the fact that pressure and volume do not change independently when the temperature is altered means that pressure and volume vanish to shadows, and only a union of the two has objective existence. It is impossible, I think, to form an adequate conception of the harm that this misrepresentation has caused, which quite overshadows whatever value Minkowski's mathematical representation of the mathematics of relativity theory has had in stimulating Einstein to the production of his general theory.

The immediate effect, such as it was, of Minkowski's paper was mainly one of mystification; Einstein himself is reported to have said that after reading it he felt he did not understand his own theory — which is not surprising, since Minkowski's 'time' was only 'eternity' and Einstein's was only 'instant' or 'duration'. Indeed, Einstein is often at pains to insist that 'time' means 'time of an event' and is denoted by the reading of a clock: what clock can read 'eternity'? Philipp Frank, in his Life of Einstein,[10] tells us that Einstein, in his early days, attended Minkowski's lectures and was repelled by them, and Born, as we have seen, found Einstein's papers a 'revelation' after studying under Minkowski, though he never escaped from the influence of his early training, as I dunk his reply to my criticism (pp. 42-3) shows: he automatically interpreted 'time' as 'eternity', and assumed that that was what I, quoting Einstein, should have meant by it. 'The simple fact,' he wrote, 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.'[11] This is but one example of the endless confusion that has been introduced into the special relativity theory by the identification of the quite different meanings of the word 'time' (or 'Zeit') in the work of Einstein and Minkowski.

Of course, as is well known, Einstein later adopted the Minkowski form of his special theory as a basis for his general theory,[12] and superficially this may be regarded as an acceptance by Einstein of Minkowski's idea of 'space-time' as a physical reality. But the appearance is only superficial. In Einstein's general theory also, in so far as it can be applied to observation and receive support from it, the symbols *x, y, z, t* always refer to the places and instants of events, and the speculations — some of them very wild indeed — that have been advanced concerning the nature of 'space-time', 'the universe', and so on, are meaningless unless they can be translated into terms in which 'space' is potentially 'place' and 'time' is potentially 'instant'. If that can be done, such a term as 'space-time' may afford a convenient means of expression, just as one may speak of 'a considerable length of time' without implying that a duration can be measured with a yardstick, but in much modern cosmology, those who speak of 'space-time' naively imagine that it refers literally to something existing objectively, and so deceive themselves and others into thinking that the world which is the world of all of us has an esoteric quality that only the specially gifted can understand.

To resume the historical account, however, the important fact to grasp is that, from 1904 to the time of the first world war, the relativity theory so-called — which was a matter of interest only to a comparatively few, highly theoretical, physicists — was ascribed by all of them to Lorentz. The work of Einstein and Minkowski was little known, and those who were aware of it regarded it as simply more obscure forms of the generally intelligible and acceptable, though admittedly in need of experimental demonstration, theory of Lorentz. Tolman, indeed, many years later told me that when he first read Minkowski's paper his immediate reaction was, 'This is all hooey', and he tossed it aside without further attention. With his excess of modesty he related this as an example of his lack of insight, but I am inclined to give it the opposite interpretation so far as its relation to physics, as distinct from mathematics, is concerned. But Einstein, whose preoccupation after the completion of his special theory was with a generalisation of the relativity postulate to cover accelerated motion and so provide a theory of gravitation, possessed insight of a different kind. Quite understandably, he did not recognise the physical aspect of his own special theory in Minkowski's work, but he did recognise the possibilities of Minkowski's mathematics, when combined with the new tensor calculus of Ricci and Levi-Civita, for his desired generalisation, and step by step he approached the complete general theory which he published in 1916, in which a theory of gravitation was advanced that satisfied the relativity postulate (i.e. it regarded all motion, of whatever kind, as purely relative, so that if two bodies were in any kind of relative motion, the motion could be divided between them equally legitimately in whatever way one chose).

Before proceeding with our main theme — the explanation of the acceptance of Einstein's special theory notwithstanding its obvious impossibility — a word should be said concerning the relation of that to his general theory. (I apologise for these digressions, but the subject is complex and does not admit of a simple straightforward narration without loss of clarity). The special theory is based on two postulates — the postulate of the relativity of uniform motion and the postulate of the independence of the velocity of light on motion of its source. Its purpose was to reconcile the theories of kinematics and electromagnetism — which, in their existing form, were respectively relativistic and non-relativistic — and it purported to do so by so modifying the former so as to make it capable of imposing its relativistic character, which the modification did not remove, on the latter. But such an achievement clearly called for generalisation. Kinematics was only the part of general mechanics that covered uniform motions: the next step would naturally be to reconcile the two departments of physics for *all* motions. The direct course to this end would be, first, to derive transformation equations for relatively accelerated systems of co-ordinates — corresponding to the Lorentz transformation equations for systems in uniform relative motion — and then to express the equations of electromagnetism in a form invariant to these new transformation equations. This Einstein never attempted — or at least he published nothing along these lines, nor has anyone else — but what he did in his so-called general relativity theory was to generalise the relativity postulate alone, and then construct a law of motion of bodies that included their motions under their mutual gravitational influence, and held good for all systems of co-ordinates, in accelerated as well as uniform motions.

It was a great achievement, but it had the serious disadvantage of destroying the possibility of preserving the reconciliation with electromagnetism, which the special theory had claimed for uniform motions. The generalisation of the relativity postulate had the consequence that the other postulate — that of the constancy of the velocity of light, as it is generally called — could no longer be maintained. 'It will also be obvious,' wrote Einstein at the beginning of his 1916 paper, 'that the principle of the constancy of the velocity of light *in vacuo* must be modified.' Einstein's chief aim during the remainder of his life was to construct 'a unified field theory', i.e. a theory in which his *general* relativity theory of mechanics was reconciled with electromagnetism, but he did not succeed. What is usually called 'the general theory of relativity' is thus not, in the full sense, a generalisation of 'the special theory of relativity'. If the criticism of the special theory here advanced — which seems to me unanswerable, and certainly has not been answered — is sound, a possible explanation of Einstein's failure is that he was attempting the impossible, but that does not mean that the 'general' theory is necessarily wrong. The failure of the special theory may lie in the falsity of the postulate of the constancy of the velocity of light and not the postulate of relativity, in which case a theory based on a generalisation of the latter alone may still be valid. The matter, however, is not important for our present purpose, except insofar as it does give some support — very far indeed from proof — for the view that the error in the special theory does lie in the postulate of constant light velocity, for Einstein's theory of gravitation does have some support — again very far from proof — from observation, and nothing is so far known fatally at variance with it.

Let us, however, return to the position in 1919, when the eclipse observations seemed to provide a confirmation of the general theory. This caused an unprecedented sensation, not only in the world of physics but in the world generally, for it seemed that what had for more than 200 years been regarded as the unshakable foundation of all physical science — Newton's mechanics — had been disproved. Now — for the first time so far as most physicists were concerned -'the theory of relativity' stood in the forefront of physics, and, since it had been brought there through work of Einstein's which was regarded by him as, a generalisation of his theory of 1905, the name of the theory changed, as though by magic, from 'the relativity theory of Lorentz' (known to a mere handful of specialists) to 'Einstein's special relativity theory' (known by name, though little else, to everyone). But the circumstances in which it was introduced ' were such as almost inevitably to invest it with an air of impenetrable mystery quite foreign to its real character. Einstein's general theory, which was the medium of its introduction, was undoubtedly extremely difficult to comprehend to those who met it for the first time. It had a double incomprehensibility: it employed a branch of mathematics — the tensor calculus which, though now taught to mathematics students at a fairly early stage of their training, was at that time practically unknown and extremely difficult of mastery by established physicists whose capability of entertaining quite novel ideas was of necessity less than it had been in their youth; and secondly, the language, both verbal and symbolic, in which it was expressed, was that introduced by Minkowski, and physicists therefore found themselves suddenly faced with the metaphysics of time (eternity) and space fused into a unity more densely obscure than even that of the traditional philosophers. Yet this was the medium through which the essentially simple special theory was first brought to the attention of the general body of physicists.

As if this were not enough, there was the additional complication resulting from the name of the theory. The 'relativity' theory was ascribed to Lorentz, Einstein and Minkowski as though these all contributed to the same set of ideas. The facts that Lorentz's theory was impossible without an ether and Einstein's impossible with one, that Lorentz and Einstein never thought of time as relating to anything but instants and durations while Minkowski never thought of it as relating to anything but 'eternity', that the basic idea of Minkowski was 'space-eternity', which meant nothing whatever in the original papers of either Lorentz or Einstein — all these differences were completely obscured by their unholy alliance under the one word 'relativity'. Phrases like 'time-dilation', which meant nothing to anybody, were freely used to describe 'Einstein's special relativity theory', and 'time' and 'space' were declared to be interchangeable ('one man's space is another man's time', as Jeans put it), so that even those who took the trouble to look up Einstein's 1905 paper and found there nothing to which they could possibly attach such notions, became convinced that what normally they would have understood without any difficulty at all must contain some mysterious essence which they were incapable of apprehending, The net result was that they gave up the attempt to understand the matter and submitted to the uncritical acceptance of 'Einstein's special relativity theory' with resignation. That state has persisted ever since, with increasing irresponsibility of the 'mathematicians', freed now from the task of having to justify their pronouncements, however extravagant, and increasing mental inertia on the part of the 'experimenters', until the present state is reached in which so simple a question as that which I have put has remained unanswered for 13 years and no one is in the least disturbed about it.

That the state of mind which I have described is that which actually existed among physicists in 1919 and the years immediately following I know from my own experience, but it can be amply verified from the records. I was, of course, a mere onlooker, but a specially privileged one, for at the Imperial College, where I was successively a demonstrator and lecturer in physics during this period, I was in close association with the two men — one a mathematician and philosopher and the other a leading experimental physicist and astronomer — through whom, perhaps better than any others, it was possible to observe and sense the general intellectual climate of the time. They were Professor A. N. Whitehead and Professor A. Fowler. The former, as is well known, had his own original ideas on relativity, while Fowler, in whose department I worked and to whom I acted as a sort of unofficial private secretary, was at that time President of the Royal Astronomical Society and General Secretary of the newly-formed International Astronomical Union, besides being the acknowledged leader in spectroscopy which was the foremost experimental activity in physics at that time. Thus I had exceptional opportunities of meeting, in both public and private gatherings, the outstanding visiting astronomers and physicists of the time and discussing with them relativity and other problems. Further, I was frequently in contact with Sir Richard Gregory, the editor of *Nature,* who had been a fellow-student of Fowler's, and he used me for reviewing and other purposes connected with the journal, so that I was enabled to see various communications of interest and importance from those best qualified (and, incidentally, many from those not qualified at all) to write on relativity. There could hardly have been a more favourable situation for observing the general effect of the relativity theory on scientists.

It was as I have described it.

> It is not going too far to say, [wrote Whitehead] that the announcement that physicists would have in future to study the theory of tensors created a veritable panic among them when the verification of Einstein's predictions was first announced.[13]

As for the changed estimate of the authorship of the theory, it will be sufficient to mention two books by E. Cunningham, a Cambridge mathematician who had been interested in relativity from the early days. In the first, *The Principle of Relativity* (1914), the work of Lorentz, Einstein and Minkowski is described, but the lion's share goes to Lorentz, who has the largest number of entries (13) in the index, and references to him appear throughout the book. In the index of the second, *Relativity and the Electron Theory* (2nd edition, 1921), in which 'Relativity' still means only the special theory, since the general theory had no relation to the electron theory, the name of Lorentz does not appear at all. Admittedly the indexes do not truly reflect the amount of attention given in the books to the work of the various writers, but they do give a correct idea of the transfer of credit that had occurred. In the second book we read that 'Lorentz's argument anticipates the principle of relativity', and the reference to 'what came to be known in

1905 as the principle of relativity' gives the reader the impression that that name was then applied to Einstein's theory towards which Lorentz was feeling his way, whereas, as we have seen, 'the relativity theory' was for many years after 1905 regarded generally as due to Lorentz alone.

But what is more important than the confusion regarding the authorship or name of the theory is that which surrounded its meaning. Whitehead certainly understood it, and admired without accepting it. Fowler, though he was prominent in criticising the experimental evidence for the spectrum shift predicted by the general theory, acknowledged that he had not the least idea of what the theory was all about, and those less gifted and less candid showed by their comments that they were quite unaware that the theories of Lorentz and Einstein were essentially different. Yet this could hardly have escaped attention if physicists had not been too bewildered to see what was plainly before their eyes. For instance, Eddington, in his Physical Society *Report on the Relativity Theory of Gravitation"* published in 1918 in anticipation of the eclipse observations of the following year which were to test Einstein's general theory, in describing the contraction of a moving rod required by Einstein's special theory, wrote: 'When a rod is started from rest into uniform motion, nothing whatever happens to the rod.' Lorentz, on the other hand, wrote in a special Relativity Number of *Nature* of February 1921" (he is referring to the same phenomenon): 'I may remark here that there can be no question about the reality of this change of length ... let there be two rods, I and II, exactly equal to each other ... II will be shorter than I., just as it would be if it were kept at a lower temperature.' This, of course, followed from his own theory of 1904 which preceded Einstein's and ascribed the shortening to an effect of the ether. Einstein's account of his theory in the same issue of *Nature* does not mention the ether, though it gives full credit to Lorentz for the transformation equations, which Lorentz could not have derived without it. Nevertheless, in an article also in the same issue of *Nature,* Jeans wrote: 'Early in the present century Einstein and Lorentz suggested a tentative generalisation of this type, which is now known as the hypothesis of relativity.'

No one remarked on all these contradictions. The general confusion was complete and, as I have said, it has proved the chief means of preserving Einstein's theory, in spite of its obvious untenability, because of the freedom which it allows of switching from Einstein to Lorentz and back as occasion makes convenient. An almost equally effective means of escaping difficulties is the introduction of 'the observer'. When the theory appears to lead to incompatible objective results, they are written off as merely different *appearances,* but claimed as *realities* when some actual phenomenon has to be explained. Again in this same issue of *Nature,* Einstein's account of his theory does not mention the observer; it is wholly objective. Eddington's article, on the other hand, is almost wholly concerned with the difference between our observations and those of an observer on Arcturus. Is it any wonder that the theory acquired a reputation for unintelligibility when the acknowledged authorities gave such contradictory accounts of what it was all about?

Examples of the persistence of this confusion during the succeeding years could be given galore, but I restrict myself to one. Even as late as 1942, no less an authority than Professor P. S. Epstein of California, who was active in the field at the time of origin of the theory, could quote,[16] in support of his contention that on Einstein's theory the 'relativity contraction' was 'real', Lorentz's statement made in 1927: 'I should like to emphasize the fact that the variations of length caused by a translation are real phenomena, no less than, for instance, the variations that are produced by changes of temperature'. What he made of Eddington's statement that 'nothing whatever happens to the rod', I do not know.

Epstein was writing as a mathematical physicist, and even he failed to see the essential distinction between the theories of Lorentz and Einstein. The plight of the experimental physicists may be imagined. I have quoted Whitehead to the effect that they were panic-stricken by the necessity of studying the theory of tensors. In fact they did not study it, but left it to the mathematicians. However, this was relatively unimportant because tensor theory, though essential for the general relativity theory, could be avoided when one was dealing only with the special theory, and it was only the latter that physicists in general were compelled to deal with. They could leave gravitation to the mathematicians, but they could not leave electromagnetism; that was essential, everyday physics, and they had to teach it and conduct their researches in terms of its theoretical requirements. Apart from a few like Rutherford, who ignored the whole thing and went on with his experiments, they took the only course available in the circumstances. Naturally they could not make sense of the confusions which I have described, but they could use the equations of the Lorentz transformation and apply them as a 'relativity correction' (blessed phrase) to the requirements of the Maxwell-Lorentz theory. To justify this to their students they learned the appropriate phrases from the 'experts' and escaped awkward physical questions by jumping freely between Einstein and Lorentz (they were both 'relativity', of course) according to the needs of the moment. The equations worked, so the 'experimenters' became convinced that the theory, whatever it was, must be right. The superior minds acknowledged that they did not understand it, but the majority could not rise to that height. Nothing is more powerful in producing the illusion that one understands something that one does not, than constant repetition of the words used to express it, and the lesser minds deceived themselves by supposing that terms like 'dilation of time' had a self-evident meaning, and regarded with contempt those stupid enough to imagine that they required explanation. Anyone who cares to examine the literature from 1920 to the present day, even if he has not had personal experience of the development, can see the gradual growth of dogmatic acceptance of the theory and contempt for its critics, right up to the extreme form exhibited today by those who learnt it from those who learnt it from those who failed to understand it at the beginning. They are not worth quoting; the candid admissions I have cited in Part One from the mature leaders in the subject are sufficient evidence of the present state.

I hope this brief recapitulation of the circumstances in which the theory suddenly forced itself on the necessary attention of physicists will make it more credible that it should have persisted so long with such an elementary inconsistency at its heart. It could not be understood; it could not be escaped. It could not be understood because incompatible ideas, having been given the same name, were regarded as identical; because the essentially physical ideas of the theory were exchanged for metaphysical ideas by a transformation of 'instants' into 'eternity'; and because subjectivity and objectivity were hopelessly mixed up by the conversion of co-ordinate systems into 'observers'. It could not be escaped, because the indications of the Maxwell-Lorentz theory which was universally accepted (except where quantum theory made it necessary to deny them) needed the correction of the Lorentz transformation to make them accord with experimental results, and the Maxwell-Lorentz equations, having been accepted, in accordance with Hertz's perception, as a substitute for a theory, could properly be corrected by another system of equations without too much attention being paid to the absence of any intelligible idea behind them.

It is easier to be wise after the event than before it. The impossibility of special relativity — so obvious now, when one looks at my simple question and realises its clear unanswerability — was by no means obvious then: incredible as it may seem, this nevertheless was so. I certainly had no suspicion of the obvious truth, but believed the theory to be a landmark in the history of physical thought — and, I may add, I still think that Einstein's perception of a possible escape from the dilemma of the time a mark of the highest genius, though his failure to see that it could not be actual when it needed such slight additional thought to make that unmistakably evident, reveals all too clearly the limitations of that genius. But the transformation that the theory had wrought in the attitude of physicists to their researches — a transformation so ominous for the future, especially at a time when the political situation on the Continent was revealing the displacement of reason by dogmas concerning race rather than mathematical fancies — seemed, nearly 40 years ago, so plainly apparent that I could not understand why physicists in general were so blind to it. With the encouragement of Sir Richard Gregory I wrote an article entitled 'Physics and the Public Mind' which he published in *Nature* of 2 June 1934 and in which I attempted to check the bartering by physicists of intelligent thought for blind acceptance of absurd interpretations of unintelligible mathematics. Thinking it at that time to be a calamitous effect of what in itself was a great advance in human thought, I began with a quotation from Browning:

> For I say, this is death and the sole death,
>
> When a man's loss comes to him from his gain,
>
> Darkness from light, from knowledge ignorance

and concluded:

> It is a question for the specialist now, but in a few decades it will be a matter of universal importance; for the abstract thought of one generation, operating unperceived by the majority, directs the practical activities of the next. It is not merely scientifically indefensible, it is socially tragic when a tremendous forward leap in human thought, about which the public is curious to a degree never before witnessed, is represented as a negation, by an unintelligible formula, of all that has been proved trustworthy in the past... Those who are wise enough to see how the social life of a people is related to its mental state will scarcely contemplate the future with equanimity.

This evoked much approval from thinkers of all types except those who, in this connection, alone mattered — the physicists themselves, who gave it no attention whatever. Now that the 'few decades' have passed I hope its universal importance may be recognised and acted upon by physicists before those whose interests are now so dependent on their activities are moved, by the course of events, forcibly to restrict those activities. However, at that time the ineffectiveness of that effort led to a second article entitled 'Modern Aristotelianism' — again with the approval of Gregory, who published it in *Nature* of 8 May 1937 and followed it by a special Supplement on 12 June 1937 containing comments from various writers (I have already quoted a passage from this discussion in connection with Synge's illuminating illustration of it concerning my supposed 'hoax'). I concluded this Supplement with the following paragraph:

> If this state of mind exists among the *élite* of science, what will be the state of mind of a public taught to measure the value of an idea in terms of its incomprehensibility and to scorn the old science because it could be understood? The times are not so auspicious that we can rest comfortably in a mental atmosphere in which the ideas fittest to survive are not those, which stand in the most rational relation to experience, but those which can don the most impressive garb of pseudo-profundity. There is evidence enough on the Continent of the effects of doctrines derived 'rationally without recourse to experience'. To purify the air seems to me an urgent necessity. I wish it were in other and better hands.

Unfortunately, no hands at all were extended to it.

So much for the past; now let us return to the present. The next step must be the determination of which of Einstein's two postulates is wrong, for if they are both granted the theory follows by logical necessity. That is a question needing an experimental answer and speculations in advance of experiment are of little profit. I should prefer not to make them, but the impossibility so far of persuading anyone of the need for experiment (why test a theory that you know cannot be wrong?) justifies an attempt to discuss the various possibilities, and this I shall attempt briefly to undertake. Before doing so, however, the occasion calls for some comments on the most famous problem in the criticism of special relativity, which has constantly arisen throughout its history — the so-called 'clock paradox' or 'twin paradox'. It has been a common and very effective device in this field to avoid the necessity of admitting a contradiction by calling it a 'paradox': it is thereupon automatically regarded by those who do not consider themselves experts in the subject as something to which there is of course an answer, like the paradox of Achilles and the tortoise, which it is not their concern to provide and to which therefore they need pay no further attention). Although the importance of this problem vanishes if the inadmissibility of the special relativity theory is admitted, it deserves attention for historical reasons, and I shall therefore give it that attention in the following chapter.

9

The 'Clock (or Twin) Paradox'

A paradox arises when, from the same premises P, two (or more, of course) apparently contradictory conclusions, X and Y, seem inescapably to follow. It can be resolved only if one of the following four things can be shown: (1) the conclusions are not, in fact, contradictory; (2) the conclusion X does not follow; (3) the conclusion Y does not follow; (4) the premises P contain an internal contradiction so that X and Y follow from incompatible parts of them. In the famous (or infamous, as Professor Bondi calls it,[1] and I would not quarrel with the description) clock paradox, the premises P are the special (or sometimes general) theory of relativity; since the many who have discussed it have differed on the question whether the general theory needs to be brought into the matter, this ambiguity must be admitted: the conclusion X is that, if two similar docks separate and re-unite and their readings agree at the moment of separation, they will agree at the moment of re-union since the theory allows the motion to be ascribed with equal right to either, and no influence on their readings other than their relative motion can be dealt with by the theory: and the conclusion Y is that one will read an earlier time than the other on re-union, because the special theory of relativity, by virtue of the Lorentz transformation, requires that their rates of working differ. X and Y are obviously contradictory, so solution (1) is impossible, and we have to choose between the others.

It was almost inevitable that this paradox should arise from Einstein's 1905 paper describing the special theory, from which I quote the following passage:

> If at the points A and B of [the coordinate system] K. there are stationary clocks which, viewed in the stationary system, are synchronous; and if the clock at A is moved with the velocity v along the line AB to B, then on its arrival at B the two clocks no longer synchronise, but the clock moved from A to B lags behind the other which has remained at B by $\frac{1}{2}tv^2/c^2$ (up to magnitudes of fourth and higher order), t being the time occupied in the journey from A to B.
>
> It is at once apparent that this result still holds good if the clock moves from A to B in any polygonal line, and also when the points A and B coincide.[2]

From this it follows that Einstein chose Y as the correct solution, and therefore must have rejected X. But he did not disprove X, which seems to follow from the postulate of relativity which is an integral part of the theory P; hence he did not resolve the paradox. I think it is true to say that no one has ever disproved X — at least, I have never seen a disproof of it, and I have read innumerable treatments of the problem; nevertheless, nearly everyone accepts Y, on grounds that are almost unbelievably diverse. They involve Doppler effects, observations by external observers in countless varieties of circumstances, the influence of the rest of the universe, electromagnetic considerations, and more ingenious situations than one would have thought possible. They have one thing only in common, apart from their conclusion — they are all unaccompanied by a disproof of X. Had that been given, one would have sufficed; without it their total contribution to the resolution of the paradox is precisely nothing.

The commonest attempt to justify the ignoring of X invokes the fact that, in order that 'the clock moved from A to B' shall return to its starting point, it must undergo an acceleration, which removes the problem from the scope of the special theory. But there are three comments that may be made on this. First, Y, like X, is drawn from the special theory, so that if X is nullified by this consideration, so is Y. Second, Einstein at this time evidently considered that the acceleration did not affect the matter, for he wrote:[2] 'It is at once apparent that this result [concerning uniform motion] still holds good if the clock moves from A to B in any polygonal line' (but see the comment made later — p. 200 — on his general failure to look beyond the immediate object of attention), and it cannot do this without being accelerated. And third, the conclusion X is drawn from the postulate of relativity alone, without the postulate of constant light velocity, and in his general theory Einstein generalised the former postulate to cover both accelerated and uniform motion, so that, if we accept the generalisation, the acceleration cannot invalidate X.

It follows, therefore, that X cannot be thus disposed of, and I know of no other proposal to this end, so either the paradox remains unresolved, or else a proof must be found that Y does not follow or that the premises P (the special relativity theory) contain a contradiction. Nevertheless, for a reason that I cannot understand, and prefer not to conjecture, practically everyone, as I say, rejects X and accepts Y and P.

Although the 'paradox' is obvious to anyone who reads Einstein's paper, it did not at first attract much attention, for the reason I have already given, namely, that Einstein's theory was regarded as merely a recondite form of Lorentz's, and on Lorentz's theory it does not arise. For, despite the name 'relativity theory' given to it, Lorentz's theory was not, strictly speaking, a relativity theory at all; that is to say, it did not regard the relative motion of two bodies as, with equal validity, divisible between them in any of the various conceivable ways; each body had its own absolute motion — i.e. its motion with respect to the ether — and, although we had not discovered how to find what that was, it was nevertheless real. Consequently, on Lorentz's theory, 'asymmetrical ageing', as Y has been called, would actually occur, and the clock showing the earlier time on re-union would be the one whose velocity through the ether had been the greater. Hence on Lorentz's theory there was no difficulty in disposing of X, because it followed from the postulate of relativity which that theory rejected.

This is, in fact, one of many examples of the way in which the two theories are confused. When the clocks A and B are only in uniform motion, and so receding steadily from one another, it is usual to emphasise that there is no 'real' difference between their rates, but each merely *appears* to go slow from the point of view of the other. ('Here is a paradox,' wrote Eddington,[8] 'beyond even the imagination of Dean Swift. Gulliver regarded the Lilliputians as a race of dwarfs; and the Lilliputians regarded Gulliver as a giant. That is natural. If the Lilliputians had appeared dwarfs to Gulliver, and Gulliver had appeared a dwarf to the Lilliputians — but no! that is too absurd for fiction, and is an idea only to be found in the sober pages of science.') But when the motion ceases to be uniform, the reciprocity of Einstein's theory is abandoned, and the asymmetry of Lorentz is invoked. A traveller to Arcturus at uniform speed, pictured by Eddington, merely appears to age slowly to an observer on the Earth, and the observer on the Earth likewise appears to age slowly to the traveller, but 'if in some way his [the traveller's] motion were reversed so that he returned to the Earth again, he would find that centuries had elapsed here, while he himself did not feel a day older.' Why a retardation of ageing before reversal is only apparent, so that 'really' the traveller ages at the normal rate, and then, having decided to reverse, he regains his lost youth, is explained, according to Eddington, by the claim that the motion 'must be reversed by supernatural means or by an intense gravitational force'. What happens if the traveller reverses by the natural means of suitably operating the engine of his space vehicle is not explained. Needless to say, this was written after Einstein's general theory had re-named 'the relativity theory of Lorentz' as 'the special relativity theory', so that conclusions could be drawn indiscriminately from either, according to what happened to be necessary to preserve it from refutation.

I do not propose here to survey the enormous mass of literature on this subject. As I have said, the 'paradox' did not become a matter for serious consideration until after the general relativity theory had made its appearance, although, as also I have said, opinions were divided on the question whether the general theory needed to be brought into the matter at all. In practically all the treatments of the problem, Einstein's original 'polygonal line' has been simplified by reduction to a single to-and-fro journey in a straight line: the traveller is supposed to set out from the Earth at uniform speed, and after a while to reverse his motion and return along the same path at the same uniform speed. He would, of course, have to accelerate in order to start his motion and to reverse it (and to stop it on return if he did so, but of course this would be unnecessary for the problem), but the duration of these accelerations is always regarded as negligibly brief compared with that of the motion at uniform velocity. One of the chief objectors to the view that asymmetrical ageing is compatible with the relativity postulate was the philosopher Bergson, who wrote a book on the subject, *Durée et Simultanéité,* in 1922; this has recently been translated into English by Professor L. Jacobson, and published, under the title, *Duration and Simultaneity,*[4] with a long Introduction by me on the modern phases of the controversy, most of which it will be unnecessary to repeat here. Also, in my discussion with the late Viscount Samuel, *A Threefold Cord,*[5] I described the earlier stages of the modern revival, and that also I can leave out of account. I shall therefore here restrict my remarks to a brief statement of my own relation to the problem, to a discussion of Einstein's treatment of it, which appears to be unknown to the great majority of those who affect to solve it, and to a mere mention of a recent treatment by Professor H. Bondi,[6] with whom, many years ago, I debated the 'paradox' on the BBC radio. He has recently given a new approach to the problem (leading, however, to the same conclusion — that Y is right — but still with no attempt to dispose of X), and I have since realised what I did not see at that time — that solution (4) gives the key to the problem. At the time of our debate I believed the special theory of relativity to be valid, but held that it made Y impossible, so the debate, if held now, would take a very different form. It would, however, be of little help to present our divergence, which still exists, in its new form.

It will be helpful, however, if I distinguish clearly between the clock paradox problem and the main subject of Part Two of this book, which is the validity of the special relativity theory — in other words, between solutions of the paradox (2) and (3) on the one hand, and (4) on the other — because at bottom they are quite different and of vastly differing importance. If special relativity is right, it is a relatively academic problem whether it entails asymmetrical ageing or not, because it will be a long rime before we shall attain speeds sufficiently great to make it of any practical effect. A failure of special relativity, however, revolutionises the whole of physics here and now, and its immediate consequences are quite incalculable. ('At present [1955] special relativity is taken for granted, the whole of atomic physics is merged with it', wrote Professor Max Born,[7] and I think there would be general agreement with this). Put briefly, then, the situation at the rime when Bondi and I debated the subject was that we both accepted the special relativity theory as valid, but he held that it necessarily entailed asymmetrical ageing while I held that it made that impossible.

My argument was very simple. I later put it into the form of a syllogism, to reduce the task of refuting it to the limit of simplicity: I have repeated this syllogism more times and in more places than I can now recall, without eliciting more than one answer (if it can be called such), which came from Professor McCrea. Here is the syllogism:[8]

> 1. According to the postulate of relativity, if two bodies (for example, two identical clocks) separate and re-unite, there is no observable phenomenon that will show in an absolute sense that one rather than the other has moved.
>
> 2. If on re-union one clock were retarded *by a quantity depending on their relative motion,* and the other not, that phenomenon I would show that the first had moved and not the second.
>
> 3. Hence, if the postulate of relativity is true, the clocks must be retarded equally or not at all: in either case, their readings will agree on re-union if they agreed at separation.

McCrea's comment was: 'In Professor Dingle's letter, his statement (1) is demonstrably false ... Of course, it is not necessary to say that 'one rather than the other has moved'.[9] The reader must make what he can of this.

Bondi's argument depended on the fact that the mathematics of the theory (the Lorentz transformation), which required the clocks to work at different rates, necessarily compelled a difference of reading on re-union. In other words, I argued for solution X and Bondi for solution Y. But, as I have pointed out, it is not sufficient to 'prove' X or Y; one must also disprove Y or X in order that such 'proof shall resolve the paradox. I cannot remember the details of our debate, but my disproof of Y was essentially this. The Lorentz transformation certainly required that the traveller's clock, when it reached the end of its outward journey, should be behind the 'time' (instant) prescribed by the theory for its arrival, according to the Earth clock. But that 'time' was freely defined, and the fact that the actual reading of the traveller's clock differed from it told you nothing about the rates of the clocks: a clock at the distant point had been artificially set to agree with the freely adopted definition, and the fact that the traveller's clock disagreed with it did not mean that it would disagree with the Earth clock, which had not been artificially set to agree with any definition. What the 'slowing down' required by the Lorentz transformation meant, therefore, was that the common reading of the two clocks on re-union was behind that calculated on pre-relativity principles, so that the journey had had a shorter duration than would have been expected from the distance and velocity of travel according to Newtonian kinematics.

Neither Bondi nor anyone else offered a disproof of X, so, although I was not altogether satisfied with my disproof of Y, it did seem to me free from fatal objection, and I worked out the mathematics in detail in a paper published in the Proceedings of the Physical Society,[10] showing that the mathematics of the theory was not inconsistent with the agreement of the clocks on re-union. But I soon realised that there *was* a fatal objection to this disproof of Y. If the traveller moved at a speed greater than $c/\sqrt{2}$, but less than c, where c is the velocity of light, the calculation showed that the common reading of the clocks on re-union would be earlier than the reading of the Earth clock when a beam of light, starting at the same instant as the traveller and covering the same distance, would return: in other words, the traveller, moving always more slowly than the light, would nevertheless get back first. This was clearly impossible; hence my disproof of Y had failed. I could see no alternative disproof, so I was faced with the situation that neither X nor Y could be disproved. All that was left was solution (4) — that the special relativity theory was self-contradictory.

It was then comparatively easy to prove this in other ways, of which the one I have chosen for this book seems to me the simplest and most direct. It seems to me quite unanswerable, but what is absolutely certain is that it has not been answered. It leaves the question of the possibility of asymmetrical ageing at present quite open, although one may incline with a variety of degrees of probability to one side or the other. The original question, to which either X or Y was an answer, was: is asymmetrical ageing compatible with relativity theory? and that was a purely abstract question, the answer to which was quite independent of the truth of relativity theory or the reality of asymmetrical ageing. But if, as I now hold, special relativity is false, then the reality or otherwise of asymmetrical ageing depends on which of its postulates is wrong. If the postulate of relativity is wrong, then there is a Lorentzian ether and asymmetrical ageing is possible. If, on the other hand, the relativity postulate is right and the postulate of constant light velocity wrong, then asymmetrical ageing is impossible. These possibilities will be discussed at greater length in the next chapter.

I turn now to Einstein's paper on the clock paradox, which, though in one sense it is not an attempt to solve the problem at all but aims merely at showing that the relativity postulate can survive either solution, does dispose of a large number of arguments for solution Y — in particular, all those which attempt to dismiss X by claiming that the effect of the acceleration on reversal invalidates it. In a paper in *Naturwissenschaften* in 1918,[11](shortly after he had published his general theory, Einstein discussed this problem — as he was forced to do because, having committed himself to the postulate of relativity with respect to accelerated as well as uniform motion, he had to show that the extended postulate was not violated by the asymmetrical ageing which he had originally deduced from 'the special theory. He puts his argument into the form of a dialogue between a relativist (who, of course, is Einstein himself) and a critic who argues that the general postulate cannot be true because asymmetrical ageing violates it. This paper, as I say, is surprisingly little known, and as it has not, to my knowledge, been published in English translation, I shall quote extensively from a rendering made for me by a competent translator.

The critic poses the problem thus:

> Let K be a Galilean system of co-ordinates within the meaning of the special theory of relativity — that is, a reference frame relatively to which isolated mass-points move uniformly in a straight line. Further, let U_1 and U_2 be two exactly similar clocks, free from external influences. They work at the same rate when at rest relatively to K, either immediately next to one another or at an arbitrary distance apart. If, however, one of the clocks — let us say U_2 — is in a state of uniform translatory motion relatively to K, then, according to the special theory of relativity — judging from the system of co-ordinates called K — it is supposed to work more slowly than the clock U_1, which is at rest relatively to K. This result seems odd in itself. It gives rise to serious doubts when one imagines the following familiar thought-experiment.
>
> Let A and B be two points of the system K at a distance from one another. To depict the situation more precisely, let us assume that A is the origin of K, and B a point on the positive x-axis. Let the two clocks at first be at rest at A, so that they work at the same rate, and let their readings be the same. We now impart to U_2 a constant velocity in the direction of the positive x-axis, so that it moves towards B. At B we imagine the velocity reversed, so that U_2 returns towards A. When it arrives at A its motion is stopped, so that it is now again at rest relatively to U_1 Since the change in the reading of U_2, judged from K, which might occur during the acceleration of U_2 certainly cannot surpass a definite amount, and since U_2 works more slowly than U_1 during its uniform motion along the line AB (judged from K), then, if AB is sufficiently long, U_2 must be behind U_1 on its return...
>
> Now comes the rub. According to the principle of relativity the whole process must surely take place in exactly the same way if it is considered in a reference frame K' which shares the movement of U_2. Relatively to K' it is U_1 that executes the to-and-fro movement while U_2 remains at rest throughout. From this it follows that, at the end of the process, U_1 must be behind U_2, which contradicts the former result. Even the most loyal adherent of the theory surely cannot maintain that, of two clocks at rest beside, one another, each is behind in time compared with the other.

The relativist, after accepting the last statement, objects that the special theory is inapplicable to this case, since it deals only with unaccelerated reference frames, while K and K' are at times accelerated. The critic points out that the general theory *does* deal with accelerated reference frames, and the relativist is forced to agree.

> It is certainly correct [he says] that, from the point of view of the general theory of relativity, we can use the co-ordinate system K' just as well as the system K. But it is easy to see mat, in their relation to the process under consideration, the systems K and K' are by no means equivalent; for while the process is to be conceived as above from K, it presents a completely different aspect when looked at from K', as the following comparison shows:

K Reference System	*K' Reference System*
1.The clock U_2 is accelerated by an external force in the direction of the positive x-axis until it reaches the velocity v. U_1 remains at rest.	1. A gravitational field, orientated in the direction of the negative x-axis, is set up, in which the clock U_1 falls with an accelerated motion until it reaches the velocity v. An external force applied to U_2 in the direction of the positive x-axis prevents U_2 from being moved by the gravitational field. When U_1 has reached the velocity v the gravitational field vanishes.
2. U_2 moves with constant velocity v to the point B on the positive x-axis. U_1 remains at rest.	2. U_1 moves with constant velocity v to a point B' on the negative x-axis. U_2 remains at rest.
3. U_2 is accelerated by an external force in the direction of the negative x-axis until it reaches the velocity v in the negative direction. U_1 remains at rest.	3. A homogeneous gravitational field in the direction of the positive x-axis is set up, under the influence of which U_1 is accelerated in the direction of the positive x-axis until it reaches the velocity v in this direction, whereupon the gravitational field vanishes.

	An external force applied to U_2 in the direction of the negative x-axis prevents U_2 from being moved by this gravitational field.
4. U_2 moves with constant velocity v in the direction of the negative x-axis back to the neighbourhood of U_1. U_1 remains at rest.	4. U_1 moves with constant velocity v in the direction of the positive x-axis into the neighbourhood of U_2. U_2 remains at rest.
5. U_2 is brought to rest by an external force	5. A gravitational field in the direction of the negative x-axis is set up, which brings U_1 to rest. The gravitational field then vanishes. U_2 is kept at rest during this process by an external force.

You must bear in mind that exactly the same process is described in the right and in the left hand columns, but the description on the left refers to the co-ordinate system K while that on the right refers to K'. According to both descriptions, at the end of the process the clock U2 is retarded by a definite amount compared with U_1. With reference to K' this is explained as follows: It is true that during the stages 2 and 4, the clock U_1, moving with velocity *v,* works more slowly than U_2, which is at rest. But this retardation is over-compensated by the quicker working of U_1 during stage 3. For, according to the general theory of relativity, a clock works the faster the higher the gravitational potential at the place where it is situated, and during stage 3 U_1 is indeed situated in a region of higher gravitational potential than U_2. Calculation shows that the consequent advancement amounts to exactly twice as much as the retardation during stages 2 and 4. This completely clears up the paradox, which you have propounded.

Now it is clear from this, first of all, that Eddington's remark which I quoted earlier, that the travelling clock is reversed by 'an intense gravitational force' (which presumably is taken from this account of Einstein's, since it is Einstein's theory that he is propounding) is based on a misreading. The travelling clock is reversed in a normal way by the traveller who, if we suppose him to remain at rest all the time, must have the motion which his action would otherwise give him neutralised by a postulated gravitational field which has no other source than our imagination. For, as Einstein says, 'exactly the same process is described' in the two cases. If we choose the co-ordinate system K, in which the traveller moves, there is no gravitational field, for the traveller's engine causes his motion, and the Earth remains at rest because there is nothing to move it; but if we choose the system K', then something must keep the 'traveller' at rest despite the working of this engine, and something must make the Earth move. The gravitational field that serves this double purpose must therefore be purely *ad hoc.*

At first this seems a wholly arbitrary procedure: if one is at liberty freely to invent agencies to perform whatever functions are necessary to save a theory, then science becomes a farce; we can prove anything at all. But this, in fact, is not so, because Einstein is looking at the problem from the opposite side, so to speak, from most of those who have discussed it. Whereas the usual procedure is to try to show that asymmetrical ageing is possible, notwithstanding the relativity postulate, Einstein's aim is to show that the (general) relativity postulate is tenable, notwithstanding asymmetrical ageing, which he takes for granted, as though it were an established fact. 'I have noticed with regret,' says the relativist near the beginning, 'that some authors try to escape from this unavoidable result.' In these circumstances he is no more open to criticism for introducing *ad hoc* fields than Newton is open to criticism for introducing his gravitational force to explain the acceleration of a falling body. Newton takes his first law of motion — that a free body moves uniformly — for granted, and when it is observed that a naturally falling apple does not move uniformly, he invents gravitational force to accelerate it. That force is no more observable than Einstein's fields, and has no other justification than that it is necessary to preserve an already accepted axiom (the first law of motion and asymmetrical ageing in the two cases) from violation.

Furthermore, Einstein's treatment has the unique merit that it does, in anticipation, give a direct answer to the question posed by my syllogism (p. 190). Whereas all other treatments either evade that question or give it a palpably spurious answer, Einstein's answer is straightforward — it is item (2) that is wrong; asymmetrical ageing does *not* enable one to say which body has moved, for it is compatible with both suppositions. No one else (except, of course, the few who reproduce Einstein's argument, with more or less amplification — Tolman, Møller, and Born and Biem are the only ones I can think of, and none of these relates it to my syllogism) has ventured even tacitly to imply that item (2) is wrong.

Nevertheless, this argument of Einstein's is clearly quite invalid, and affords one of the best examples we have of both his outstanding ingenuity and his failure to consider the connotations of his proposed solution of a problem. The former needs no emphasis, but the latter — of which his failure to notice the requirement of special relativity that it makes each of two clocks work faster than the other is the main theme of this book — is here exemplified most strikingly.

First of all, consider the arbitrariness of the postulated gravitational fields. As I have said, their introduction in itself is no more invalid than Newton's similar procedure, but there are attendant circumstances that make it altogether different and quite inadmissible. In the first place, Newton's gravitational force was not at all arbitrary; it was defined in terms of quantities — mass and distance — defined and measured quite independently of gravitation, so that the fact that a particular combination of these things did indeed give an acceleration agreeing with that observed was a discovery of the highest importance. We are not concerned here with general relativity, but it would be a culpable waste of an opportunity not to point out in this connection that, in the usual presentation of Newton's theory by those (including Einstein himself) who purports to show its inferiority to Einstein's, this discovery of Newton's is totally misrepresented as a defect. It is said that Newton's theory includes two kinds of mass — *inertial* and *gravitational,* which are mysteriously identical — a fact, which the theory is at fault in leaving unexplained. But Newton's theory does *not* contain two kinds of mass. He defines only one, the so-called inertial mass — 'it is this quantity that I mean hereafter everywhere under the name of body or mass': he says, and his magnificent discovery that this quantity plays a major part in determining the actual accelerations of bodies observed in nature is totally misrepresented by the assertion that gravitation requires a second mass which happens, in a magical way, to be identical with the mass that measures inertia.

However, the point at the moment is that, not only is Newton's gravitational force not arbitrary but something calculable in terms of independently measurable quantities while Einstein's 'gravitational fields' are wholly *ad hoc* — but also it is, in the nature of the case, impossible that Einstein's fields ever can be any other. For the one essential characteristic of such a field is that it keeps the clock U_2 permanently at rest in spite of the 'external force', which we may regard as, applied by a traveller carrying the clock. But that traveller can apply the reversing force as he likes — steadily, jerkily, rapidly, slowly, ... — and the gravitational field must therefore also be one of infinite variety. Obviously it is impossible for such a field to be expressible in terms of any independently known quantities at all, as Newton's force was expressed in terms of inertial mass and distance.

Even less is it permissible to say what, if any, effect the field necessary in any particular case will have on the rates of the clocks. Einstein says: 'Calculation shows that the consequent advancement amounts to exactly twice as much as the retardation' given by the Lorentz transformation during the uniform motion; but he does not make the calculation, here or anywhere else, and it is obvious that it cannot possibly be done. For it is misleading to call these *ad hoc* fields 'gravitational fields', since they are essentially different from the fields represented by Einstein's law of gravitation, which are applied to calculate the motions of the planets and such things. Those fields are not *ad hoc,* and they are related to the distribution of matter in the universe, otherwise they could not be applied to observation, whereas the fields postulated in the 'clock paradox' case are necessarily 'homogeneous', i.e. of me same strength throughout the universe, so that they apply equally to U_1 and U_2 however far apart they may be. In his 'Autobiographical Notes' Einstein claims that his approach to mechanics is justified 'if one regards as possible, gravitational fields of arbitrary extension which are not initially restricted by spatial limitations'.[12] It is certainly possible to imagine such things, but not to suppose that they 'exist' in the sense in which the field in which the apple falls 'exists', or to call them by the same name.

Although, according to Einstein's law of gravitation properly so called, the rate of a clock is dependent in a calculable way on the potential of the natural gravitational field at the place where it is situated, it by no means follows that the same, or any, effect on the rate will occur in these infinitely variable artificial fields. Møller, nevertheless, gives a calculation, on the assumption that this must be so." He chooses, moreover, a very special case in which simplifying assumptions are made, and, like all such simplifications, it breaks down when these are removed. It will be sufficient to give but one. Møller supposes that the external forces by which U_2 is accelerated at the beginning, middle and end of the journey (and therefore the gravitational fields by which they are neutralised when U_1 is supposed to move) are all constant and equal to one another. In this 'way certain terms are made to cancel out, and on re-union U_2 is indeed found to be behind U_1 by the same amount whichever is supposed to move. But all that the traveller with U_2 has to do to upset this is to use different forces on starting and reversing. The agreement is then destroyed, and the clock-readings will then reveal which clock has 'really' moved.

And, worst of all, even if we allow that the hypothetical fields affect the clock-rates in any way at all, that effect would at once enable the motion be ascribed uniquely to one of the clocks. For, if U_2 moves there is no field at the reversal of motion, while if U_1 moves there is one. Now suppose a third clock U_3 at B, stationary with respect to U_1 and synchronised with it. Then, if U_2 moves, U_1 and U_3 remain synchronised throughout, but if U_1 (and U_3 of course) moves, the field that comes into play on reversal puts U_1 and U_3 out of synchronisation. An observer of U_3 from U_1 will therefore in due course see it go wrong, so he will know that it is U_1 that has moved and that the relativity principle is false.

This is specially emphasised by a particular case raised by Lenard as an objection to the general relativity principle, which Einstein claims to have answered in this paper. Lenard imagined a moving railway train brought to a sudden halt by collision with a station buffer. The relativity principle would require that the sudden change of motion could be ascribed either to the train or to the rest of the world, but it is the train that is damaged, not the Church steeple outside the station. Einstein replies, as with the clocks, that we may suppose that the rest of the world has its motion suddenly stopped by a gravitational field, so the relativity principle is preserved. But he does not proceed to the necessary accompaniment of this stoppage (according to his explanation), that then all the previously synchronised clocks of the world go out of kilter, and that fact, if it is observed to occur, fixes the motion uniquely on the rest of the world.

I think this again inescapably snows Einstein's explanation to be untenable, but it does not necessarily disprove the relativity principle. If asymmetrical ageing is a fact, as Einstein assumes at the beginning, then indeed there is no way of saving the principle other than his, and since that fails there is no way of saving it at all. But there is still the alternative that asymmetrical ageing is *not* a fact (it has, of course, never been observed), in which case the relativity principle can survive. The choice is at bottom the same as that which faces us in connection with the special relativity theory: if that is false, which of its two basic postulates fails? This we shall consider in the next chapter, but now let us proceed to the final point in the analysis of Einstein's treatment of the 'clock paradox'.

This relates to the fact that Einstein considers only the *durations* of his five stages, and not the *instants* (clock-readings) at which they begin and end. Had he given those, as they are required by his explanation, he would have seen that although, when the clocks U_1 and U_2 alone are considered, the only such instants that are actually observed are those of the beginning and end of the whole process, and he had succeeded in getting these to agree, the matter is quite different if U_3 is included. For in that case there are two coincidences of U_2 and U_3 (at the end of stage 2 and the beginning of stage 4), and not only is it impossible to make these (which are observable) agree in the two co-ordinate systems, but also, in the K' system, quite impossible changes of reading would be needed — corresponding, if the clocks are actually human twins, to a change of one of them from old age back to babyhood. I think this needs no further comment.

All these considerations show, as I say, both the impossibility of this description of the process and the extraordinary manner in which the clay mingled with the gold in Einstein's remarkable intellectual make-up. It is impossible to imagine Newton overlooking such points as these. In perceiving possibilities of solution of the problem immediately before him, Einstein was without a rival in his generation, but he seems not to have thought of looking beyond the immediate solution to its necessary implications, or even of maintaining I consistency between his various achievements. Having, as he considered, solved a problem, he no longer gave it further thought. That is why, for instance, he does not here consider asymmetrical ageing as open to question: it has been established once for all by special relativity, and what he has to do now is to defend the generalised relativity postulate against disproof by this established fact. Born tells us that when he first met Einstein in 1909, Einstein 'had already proceeded beyond special relativity which he left to minor prophets'.[7]

A very striking example of this is shown by his complete, and apparently unconscious, change of attitude to the whole meaning of relativity between 1905, when special relativity was first (and for him finally) formulated, and 1918, when he gave the above justification of the general relativity postulate. At the opening of his 1905 paper he states quite plainly that the blemish in the existing statement of the electromagnetic theory, which prompted his proposed reform of it, was the fact that its *description* of phenomena dependent only on relative motion differed when the standard of rest was changed, whereas the *phenomena* themselves remained the same. He cites as an example 'the reciprocal electrodynamic action of a magnet and a conductor. The observable phenomenon here depends only on the relative motion of the conductor and the magnet, whereas the customary view draws a sharp distinction between the two cases in which either the one or the other of these bodies is in motion.' He then states that 'examples of this sort' prompt the formulation of the (special) relativity postulate.

This point is emphasised by Born in his reminiscences of Einstein; he writes: 'The second peculiar feature of the first relativity paper by Einstein is his point of departure, the *empirical facts* on which he built his theory. It is of surprising simplicity. He says that the usual formulation of the law of induction contains an asymmetry, which is artificial and does not correspond to facts. According to observation the current induced depends only on the relative motion ... while the ... theory explains the effect in quite different terms according to whether the wire is at rest and the magnet moving or vice versa.'[7]

But in 1918 — a point which Born seems not to have noticed at all — this difference of description in the two co-ordinate systems is no longer a defect; it is the very freedom to give different descriptions of the same phenomenon that is called upon to justify the relativity postulate. 'You must bear in mind', he writes, 'that exactly the same process is described in the right and in the left hand columns, but the description on the left refers to the co-ordinate system K while that on the right refers to K'... This completely clears up the paradox, which you have propounded.' How a defect in electromagnetic theory can become a merit in general relativity theory is not explained. Einstein had evidently forgotten what he had bequeathed to the minor prophets and uttered a new prophecy quite at variance with it. (A mathematical analogue — I will not say equivalent — of this change of front in passing from the special to the general theory of relativity is the fact that in the former the only co-ordinate transformations permitted are those which leave the expression for ds^2 unchanged in *form,* while in the general theory all transformations that leave it unchanged in *value* are allowed, no matter what change of form they may require. This, however, is a technical detail, which the general reader may ignore).

I am far from objecting to the right of theorists to change their *description* of a process when they change their co-ordinate system, provided that the two descriptions do not entail a difference in any observable phenomenon: I would therefore rather maintain that Einstein's original objection to Maxwell's electrodynamics was inadmissible than that he was at fault in violating that objection here. However, that is another matter, which is outside our present concern.

10

The Present Position

In this chapter I am going to assume that the disproof of Einstein's special relativity theory has been established. I make this assumption not only because I see it with a clearness that would have satisfied Descartes's criterion of truth — I know how deceptive such convictions, standing alone, can be — but also because of the universal failure to extract an answer to the simplest of questions. Only one sentence is needed to save the theory, and all I am offered, if anything at all, from those regarded as authorities on the theory are replies so vague and irrelevant as to be quite useless. Bondi 'does not feel able' to give the sentence asked for, but refers me to two published works of his where it is to be found (he does not say in what parts, and I cannot find it there). Temple tells me that I shall find a reply in Synge's book on the theory, but does not say where, and again I cannot find it. Synge himself does not mention his own book, but implies that the answer is in 'relativistic physics', which my expectation of life does not allow me time to explore. McCrea, alone among English speaking people, finds the question 'meaningless'. *Science* tells me that the answer appeared in that journal in 1957-8, but gives no reference and does not repeat the answer. So it goes on: the answer is everywhere but where it can be found.

> 'Each faculty tasked
>
> Has gained an abyss where a dewdrop was asked.'

I claim no genius for discerning that the name of the dewdrop is Mrs. Harris.

The question then arises: in what state is physics left when deprived of this fundamental theory? Although some aspects of this question are fairly obvious, it is in the main a matter of more or less probable conjecture — unlike the question of the tenability of Einstein's theory, which is open to settlement here and now by pure reason. The theory is based on two postulates and a definition: if these are granted the rest follows logically, so there must be an incompatibility in these foundations. The postulates arc; (1) the postulate of relativity — that nature contains no absolute standard of rest, such as the Maxwell-Lorentz ether, for example, would provide, that would enable a unique state of uniform motion to be ascribed to a single observable body; (2) the postulate of constant light velocity — that the velocity of light, with respect to any chosen standard, has a constant value c which is independent of the state of (uniform) motion of the source from which it is emitted; i.e. that if, from two sources in uniform relative motion, light pulses are emitted in the direction of motion at an instant at which the sources are adjacent to one another, those pulses will thereafter remain adjacent and reach a distant point at the same instant. The definition' is that the time (instant) of an instantaneous event, occurring at a distance r from a clock which is accepted as a standard (r being measured by a standard scale at rest with respect to the clock) is given by subtracting r/c from the clock-reading when a light-pulse, emitted at the time (instant) and place of occurrence of the event, reaches the clock.

It should be observed, however, that if, according to pre-relativity kinematics, which in this respect was never questioned by Einstein, we define the velocity of a uniformly moving body as the distance it covers in unit time (duration), the definition follows from the second postulate, so the incompatibility of the theory lies between the two postulates alone. Our problem, therefore, is to determine which of these is wrong: possibly, of course, both are wrong, but at least one *must* be so.

The first postulate is not susceptible to definitive proof, for it is impossible to prove the non-existence of something that might conceivably reveal itself. In this respect the postulate of relativity is like the second law of thermodynamics: we know of no violation of it in the whole field of physical investigation, but the discovery of only one such violation would be fatal to it. Whittaker has called attention to the prevalence in physics of what he calls 'postulates of impotence' — i.e. postulates of the impossibility of something — and he points out that a large part at least of modern physics is based on such postulates.

> A postulate of impotence, [he writes] is not the direct result of an experiment, or of any finite number of experiments; it does not mention any measurement, or any numerical relation or analytical equation; it is the assertion of a conviction, that all attempts to do a certain thing, however made, are bound to fail. We must therefore distinguish a postulate of impotence, on the one hand, from an experimental fact: and we must also distinguish it, on the other hand, from the statements of Pure Mathematics, which do not depend in any way on experience, but are necessitated by the structure of the human mind; such a statement as, for instance, 'It is impossible to find any power of two which is divisible by three'. We cannot conceive any universe, in which this statement would be untrue, whereas we can quite readily imagine a universe in which any physical postulate of impotence would be untrue.
>
> It seems possible that while physics must continue to progress by building on experiments, any branch of it which is in a highly developed state may be exhibited as a set of logical deductions from postulates of impotence, as has already happened to thermodynamics. We may therefore conjecturally look forward to a time in the future when a treatise in any branch of physics could, if so desired, be written in the same style as Euclid's *Elements of Geometry,* beginning with some *a priori* principles, namely, postulates of impotence, and then deriving everything else from them by syllogistic reasoning.[1]

The paradox that the whole of positive physical knowledge might be inferred from the unprovable assumption of impossibility deserves, I think, more attention than it has yet been given by philosophers of science, but this is no place to discuss it. We note only that Einstein's first postulate (not the whole of his theory), is a direct example of Whittaker's postulates, which has been not very happily expressed as *'It is impossible by* *any experiment to detect uniform motion relative to the aether.'* [2] — not very happily, because it implies the existence of an ether with respect to which uniform motion is undetectable only for practical reasons, whereas Einstein's postulate, which he expressed as 'the phenomena of electro-dynamics as well as of mechanics possess no properties corresponding to the idea of absolute rest', implies that the very idea of such an ether is excluded from physics. I think it is still true to say that no phenomenon has revealed itself that would disprove this postulate, so we may continue to use it in our theories so long as we do not forget that it is unprovable and might be false. All we can do is to keep our minds open to the possibility, when we meet with difficulties in interpreting experimental results, that we might have come across a fact that destroys it.

The second postulate, on the other hand, is directly testable by experiment or observation, and so is open to conclusive proof or disproof. Numerous so-called tests have been made, and have all given results, which have been held to prove the truth of the postulate. The failure to perceive that they are all invalid is, I think, one of the most remarkable examples of the paralysis of the intellect by which physics has been afflicted through the abandonment by the 'experimenters' of the use of their intelligence and their submission to the dictation of 'mathematicians', for the invalidity of these 'tests' is so easy to see when one looks at them with an unprejudiced mind that it could not possibly have been overlooked by anyone of even moderate intelligence had he used that modest gift.

The best known, and for long the chief, if not the only, 'proof of this postulate (it is the only one cited, for example, by Einstein and Infeld in their book. *The Evolution of Physics,* in 1938) was given by de Sitter in 1913.[3] There are certain double stars whose two components revolve around one another (to use colloquial language) in a plane, which passes through the Earth. As seen from the Earth, therefore, there will be instants at which one component is approaching and the other receding, while half a revolution later — it may be a matter of hours or days — these motions are reversed. We may suppose for simplicity, without affecting the essence of the argument (though strictly speaking, of course, it is usually not quite true), that the system as a whole — the centre of gravity of the double star — is at rest with respect to the Earth, and that the maximum velocities of approach and recession of the components are both equal to v. The point to be decided, then, is said to be whether the two beams of light emitted towards the Earth by the components at an instant when one is approaching and the other receding from the Earth with velocity f, travel to the Earth with the single velocity c, or with velocities $c + v$ and $c - v$, respectively. Now these stars are very distant, so that the light takes a long time (duration) to reach the Earth — let us say 100 years, to fix our ideas — and let us suppose that the period of revolution of the components is 2 days and their distance apart 1 light-minute — quite normal values. If, then, light from both components travels through space with the same velocity c (as the postulate states), the greatest discrepancy that can occur in assuming that light from the two components, received on the Earth at the same instant, actually left the components at the same instant, will be 1 minute — the time taken by the light from the farther component to cover its distance from the other. This, in an interval of 2 days, is negligible, so the orbits drawn from the times (instants) of reception of the light will be practically identical with the actual orbits of the components. On the other hand, if the beams, issuing at velocities $c + v$ and $c - v$, maintain those velocities throughout the journey, then, since *v* would be about 300 kilometres a second in the case under consideration, it is easily calculated that the beam from the approaching component would reach the Earth about 70 days before that from the receding component. One day later, however, the motions of the components would be reversed, so that the component issuing the winning beam on one day would issue the losing beam on the next, and during the interval there would be a continuously varying difference of time of travel of the two beams. An Earthbound observer would therefore see a hopeless confusion of light from the two components, bearing no resemblance at all to the orderly revolution that would actually be taking place. In fact, however, he does see such an orderly revolution. The conclusion drawn from this is that light must actually travel at the same speed *c* from both components at all times, as Einstein's postulate requires.

This is, I think, the most remarkable example in the history of science of the wish fathering the thought — with the possible exception of the 'proofs', following the Copernican heresy, that it was the Sun, and not the Earth, that moved, to which, in fact, this argument bears some resemblance. A finite velocity, of course — and it is not disputed that light *in vacuo* has a finite velocity — must be measured with respect to some standard, and if we do not accept the postulate, here regarded as on test, that the standard is empty space (Einstein's postulate says that 'light is always propagated in empty space with a definite velocity *c* which is independent of the state of motion of the emitting body'), the only alternative with any claim to consideration is that the velocity *c* is maintained with respect to the emitting body. But all that de Sitter's argument disproves is that the velocity is maintained constant with respect to the Earth, for it is with respect to the Earth that the velocities $c + v$ and $c - v$ are reckoned, and surely no one in his senses would now maintain that the Earth provided a standard of rest for all the light in the universe. If we consider the same observations on the supposition that each beam of light moves throughout with velocity c with respect to its own source it is at once evident that after any time (duration) *t*, however great, each beam will be at a distance *ct* from its own source, and therefore the beams can never be farther apart than their sources, i.e. 1 light-minute. The maximum discrepancy, therefore, between emission-intervals and arrival intervals is 1 minute — exactly the same as on Einstein's postulate, so these observations tell us precisely nothing to enable us to choose between Einstein's postulate (which is, of course, that of the Maxwell-Lorentz electromagnetic theory) and the postulate that light keeps a constant velocity with respect to its own source (which was proposed in 1908 by Ritz as an alternative to the Maxwell-Lorentz view, but he died before de Sitter's argument was conceived).

How could such a simple fact have escaped notice for half a century? It was pointed out several years ago,4 and universally ignored — which is to me inexplicable on any other grounds than the universal inability of present-day physical scientists to believe that any criticism of special relativity that they cannot answer can proceed from anything but misunderstanding, which entitles them to ignore it. I do not think this would have been possible had not the unconscious purpose of the argument been to *prove* that the postulate *was* true and not to *test if* it was true. From die reactions of astronomers to whom I have put this criticism personally I have received the impression that their immediate reaction was one of incredulity that light could approach us with a continuously varying velocity with respect to the Earth — fast, slow, fast, slow, . . . from one component, and slow, fast, slow, fast, . . . from the other — though when pressed they cannot offer any reason why it should not. It is a modern form of the difficulty that intelligent sixteen-century thinkers experienced in believing that the Earth could be moving, and a most instructive one, for it helps us to appreciate how, in one climate of thought, what seems simple to anyone but a fool, may in another be almost impossibly difficult to men of high intelligence. 'I cannot find any bounds for my admiration,' wrote Galileo, 'how that reason was able in *Aristarchus* and *Copernicus,* to commit such a rape upon their Sences, as in despight thereof, to make her self mistress of their credulity.'

It is most interesting, however, to note that Ritz's hypothesis is entirely in keeping with the conception of Faraday — a man of imagination, if ever there was one, who commands our admiration in both the seventeenth- and twentieth-century meanings of the word — which Maxwell (pp. 132-3) wrongly identified with his own. If each (atomic) source of light is accompanied by a system of rays, proceeding in all directions, which move instantaneously with it, and light consists of vibrations transmitted along these rays at constant velocity with respect to them, then it naturally follows that the velocity of light is constant with respect to its source, however the source may move. More serious attention to this idea is long overdue.

Although de Sitter's argument may be regarded as, in a sense, the canonical observational 'proof of Einstein's second postulate, there have been a large number of others in more recent times, which it is unnecessary to consider individually because they all suffer from the same fatal defect — they involve a circular argument. In brief, though they take various forms, they all involve the assumption, at some stage, of the present electromagnetic theory of light. Now, as I have already pointed out, Einstein stressed many times that his special theory was devised to justify that electromagnetic I theory (or, to be strictly correct, to justify the *equations* of that theory, for the theory itself is meaningless without an ether of the [kind that Einstein discarded), including the requirement that Einstein's second postulate expresses. All that these 'proofs' can possibly do, I repeat, is to show that *if* we use the present electromagnetic theory of light, we must supplement its equations by those of the Lorentz transformation in order to get agreement with experiment. They cannot throw any light at all on me truth or falsity of either the electromagnetic theory of light or the special relativity theory.

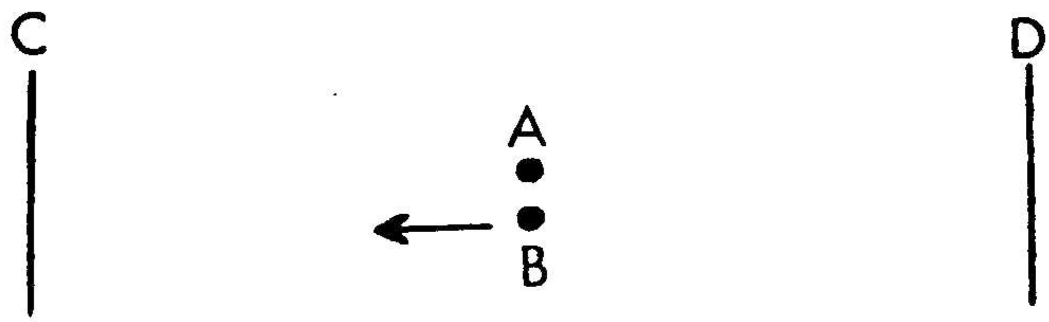
To this class, I again repeat, belongs the argument concerning cosmic rays advanced by Sir Lawrence Bragg (p. 111). It would be too complex a task to trace out in detail how the original calculations of the times taken by the particles in question to reach the Earth (which do not accord with observation) would be impossible without using the electromagnetic equations of which Einstein's second postulate is a necessary feature, but it is a matter about which there is no question or possibility of controversy. Sir Lawrence — repeating, of course, an argument that had previously been advanced and accepted by many — was contending that because the equations of special relativity succeeded in correcting the false requirements of the classical electromagnetic theory, special relativity must be true. This ignores both that the special relativity equations belong also to the very different theory of Lorentz, and that a true electromagnetic theory would need no correction. My article in the Appendix treats in a little more detail a much simpler example of arguments of this class — the experiment of Alvager, Nilsson and Kjellman[8] — but the mere fact that *any* argument for the truth of Einstein's second postulate in which the 'sources' arc hypothetical particles and not observable bodies, necessarily requires, in some form or other, the assumption of an electromagnetic theory that itself implies the truth of the postulate — that fact shows that the argument is circular and therefore invalid: further analysis of it would be redundant.

It should be remarked, however, that the double star argument of de Sitter, which I believe is the only one free from this circularity, has fairly recently been questioned by J. G. Fox[8] on grounds quite independent of the simple consideration I have already given. Fox's criticism depends on highly theoretical considerations involving what is known as 'the extinction theorem', which, in effect, would nullify any test of the postulate in which the light whose velocity is measured passed through any transparent medium at all after emission from the source. What the extinction theorem seems to amount to is the proposition that light passing through a transparent medium is absorbed by the particles of that medium and re-emitted by them, so that the velocity of the source of the emergent light is really that of the particles of the medium, and not that of the original source (it is suspected that the components of the double star in de Sitter's argument are surrounded by a gaseous envelope which does not share their orbital motion). Fox concludes that 'the material considered as evidence [for Einstein's second postulate] in the past has been shown to be possibly either irrelevant or inconclusive. This is a surprising situation in which to find us half a century after the inception of special relativity'.

It is indeed, and if this reasoning is sound an experimental test of Einstein's second postulate would appear to be impossible, though Fox considers that, under certain conditions, a sufficiently high vacuum could be produced for the postulate to be tested with radiation of high frequency. But an obvious objection to 'the extinction theorem', which no one appears to have taken into consideration at all, is that if the source of the light emerging from a transparent medium is the atoms or molecules of that medium, it should show their spectrum, but it does not. When light from a mercury lamp, for instance, is observed through glass, the spectrum of mercury, not that of the glass molecules, is seen: if, then, the velocity of the 'source' of the observed light is that of the glass particles, why is the wave-length of the light that of some quite different 'source'? In view of this enigma, and the extremely speculative nature of 'the extinction theorem' anyway, it does not appear that great weight need be assigned to this consideration.

But far more important than this, I think, is the attitude which Fox takes to the position in which the special relativity theory stands as the result of his conclusions, for it is a most revealing example of the radical departure of the physics of our time from the fundamental principle of science, which, as I repeat once more, is the chief reason why this book has had to be written. I leave comment on that, however, for the Conclusion.

I think a far simpler experiment than that suggested by Fox would suffice to settle the matter conclusively: clearly, so fundamental a point should be tested by as elementary an experiment as possible, wholly independent of such highly sophisticated notions as 'the extinction theorem'. Such an experiment is indicated in the figure:



A and B are two sources of light (visible, material sources, not hypothetical particles) of which B is moving rapidly to the left while A is at rest, the paper being the standard of rest. At the instant at which they are adjacent to one another they emit pulses of light towards C and D, which are photographic films whose distances from A are constant and which are moving rapidly downwards through the paper. The relative motion of A and B continues unchanged throughout the passage of the light. If Einstein's second postulate is true the traces on both films will be symmetrically side by side, while if Ritz's hypothesis is true, that of the light from A will be above that of the light from B on one film and below it on the other.

Such an experiment would involve no theory at all: the sources would be identifiable unambiguously, the fact of their relative motion would be indubitable, and no measurement of time of passage or assumption about synchronisation of clocks would be involved. It could be done in a vacuum if thought necessary. I suggested such an experiment many years ago,[7] with no response at all, though experiments such as that of Alvager, Nilsson and Kjellman, and theoretical discussions such as that of Fox, continued. The experimental difficulties, of course, might be great, but I have no doubt that they would be overcome readily enough with modern equipment if physicists could rid their minds of the conviction that an experiment to test the postulate is, in Fox's phrase, 'hardly worth doing'. What is certain, however, is that, unless some effort is made to determine which of Einstein's postulates is wrong, so that the direction in which to look for the truth of the matter becomes clearer, nature will indirectly and unexpectedly give an experimental demonstration of the fact that one of them fails, and the consequences may show that the supposedly superfluous experiment was worth doing after all.

For the present, however, since we do not know, all we can do is to consider the alternative possibilities. If the first postulate — the postulate of relativity — is wrong, men Lorentz's theory would seem me only way of reconciling mechanics and electromagnetism. This would have many advantages, for it would save the electromagnetic theory which has so many successes to its credit, but would present the problem of working Lorentz's *ad hoc* hypotheses into the theory — and, of course, that of reconciling it with quantum phenomena which appear so difficult to make compatible with a wave theory of light. It would restore the ether, with all the unsolved problems, which it presented to the nineteenth-century physicists. This would be decidedly unwelcome to physicists, but mat is merely a matter of fashion; those physicists — and they are many — who now regard belief in the possibility of an ether as a superstition have simply not learnt the lessons of history, which teach us that discarded ideas have a way of returning to favour. The title of the editorial article in *Nature* reproduced in the Appendix, 'Don't bring back the ether' (can one imagine Lockyer or Gregory heading an editorial on Rutherford's early hypotheses, 'Don't bring back alchemy'?) shows, however, how sadly we have unlearnt the lessons of history through the influence of our uncritical acceptance of the uncomprehended relativity theory. Nevertheless, the great difficulties, which a return of the Maxwell-Lorentz ether would bring, in view of quantum phenomena, must be set against the advantages, and these are most formidable.

A failure of Einstein's second postulate would, of course, have the opposite effect of converting the successes of the present electromagnetic theory of light into serious problems. Their seriousness, however, might be less than appears at first sight. It is often overlooked that the Maxwell-Lorentz theory rests on a very limited experimental basis, which we tend to imagine much larger than it is because we misinterpret theoretical requirements as facts of observation. This is particularly so with regard to velocities: we think we have reached velocities approaching that of light when we have, in fact, only inferred them theoretically as possessed by theoretically inferred particles. Our actual experience of directly measured velocities — the experiments, for instance, of which a celebrated one by Rowland is the chief, on the basis of which we connect a current of electricity with the movement of an electrostatically charged body by a particular formula — is confined to a range of velocities very small indeed compared with the velocity of light. It is conceivable that a modification of the formula by the introduction of a factor $\sqrt{(1 - v^2/c^2)}$ — a modification far too small to be invalidated by any existing experimental evidence — might convert the equations of Maxwell's theory into a form invariant to the Galilean instead of the Lorentz transformation, and permit the velocity of light to depend on that of its source in the way imagined by Ritz. This, of course, is a mere speculation, but it is certainly worth exploring by those experts in the theory of electromagnetism.

On the other hand, a failure of Einstein's second postulate might mean the abandonment of Maxwell's theory altogether, and a return to the general views of his predecessors. This was the belief of Ritz, who put forward a theory along such lines — which, however, he later discarded, though at the time of his death he expressed the belief that he was on the track of a much sounder theory. Since that time no one has attempted to develop pre-Maxwellian ideas — though it should be mentioned that Bondi, in his Tamer lectures, remarked, to my surprise and pleasure, that 'minds have been closed for perhaps rather longer than was necessarily desirable to the possibility of considering other kinds of theories' than field theories. Would that he would bear this in mind before resuming his mathematical speculations concerning what are cryptically called 'gravitational waves'! — but that is by the way.

It is evident, however, that whichever of Einstein's postulates has to be abandoned, serious problems are posed for the physicist — as, of course, is immediately obvious from the fact that the theory based on them both has been, in Born's words, 'taken for granted' (a most unscientific state of mind, but still the actual one) during most of this century. This stresses all the more the urgency of the definitive experiment I have described to test the second postulate in an indubitable way.

There is, however, possibly a scarcely less definitive means of testing the postulate now available by radar observations of the planets. Consider the solar system objectively — i.e. as seen by an outside observer stationary with respect to the Sun and far enough away to be considered equidistant from all parts of the system. It would include a number of elliptical orbits, of which he could draw a map showing accurately the relative positions of all the planets at each particular instant. This, of course, is what we try to do, but we are forced to observe the planets from one of them, which is moving, by light proceeding from the Sun to a differently moving planet and then reflected to the Earth. This inevitably involves some assumptions about the velocity of light relative to a moving body and about the effect of reflection (or scattering) on that velocity, and these assumptions must inevitably be made when we determine the orbits of the planets and their positions in the orbits at any given instant. I am no expert in this field of astronomy, but this is obvious, and I have confirmed from experts that it is so; and I have no doubt that the assumption made is that the velocity of all the light concerned is c with respect to any standard, in accordance with the special theory of relativity.

Now we cannot compare our map with that of the distant observer, of course, but we have a different means now of observing the nearer planets, namely, by the reflection of radar beams emitted from the Earth, so that we have two independent ways of determining the position at any instant (and therefore of the whole orbit) of, say, Venus — by visual light emitted by the Sun and reflected by Venus, and by radar beams (which, so far as velocity is concerned, we are entitled to assume equivalent to light) emitted from the moving Earth and reflected back to it from Venus. In both cases we have to assume velocities for the radiation concerned, and these will certainly be different on the Einstein and Ritz theories, though exactly what the difference should be is uncertain because of uncertainty in what velocity a valid theory containing the Ritz hypothesis would require of reflected light.

This experiment has, in fact, been performed a number of times and there has always been a discrepancy between the positions of Venus given by the visual and radar observations. Unfortunately, the difference (as would be expected on any conceivable explanation of it) is too small for its cause to be determined with certainty in view of the inevitable errors of the various observations. It would nevertheless be highly desirable to compare the orbits calculated from the two sets of observations, on the assumptions of Einstein's and Ritz's hypotheses concerning the relation of the velocity of light to that of its source (making the most probable assumption of the effect of reflection in the Ritz case), for if the Ritz hypothesis removed the discrepancy, or reduced it to an amount coming well within the inevitable errors of observation, that would be strong evidence of its truth.

I have confirmed from experts in this field that there is no error in this reasoning, but I have not succeeded in getting any response from those who make these observations or in getting the suggestion published. The former say and do nothing, and publication has been refused, on the advice of a referee who objected that the comparison would only possibly, and not certainly, show a discrepancy, which seems a strange reason for suppressing a line of research. However, it is to be expected that when sufficiently trustworthy radar observations of the more distant planets become possible, we shall be able to determine with certainty whether any discrepancy that might be revealed can be ascribed to observational errors; if not, it can hardly any longer be regarded as excusable if the requirements of Ritz's hypothesis continue to be ignored.

It is perhaps not out of place in this connection to point out that Einstein's general theory — which does not include the second postulate of the special theory but depends only on a generalisation of the first postulate — has survived all the tests so far made of it, but the differences between its requirements and those of Newton's theory are so small that a decision between the theories cannot yet be made with confidence. However, the strongest point in favour of Einstein's theory is its explanation of the orbit of Mercury. If, however, it should turn out that Einstein's second postulate has to be abandoned, the revised orbit of Mercury, the fastest moving of the planets, when the actual observations are corrected for the new time of passage of the light, may lead to a revision of the conclusion to be drawn concerning the relative merits of the two theories. This, of course, is entirely speculative, and I am too inexperienced in gravitational astronomy even to hazard a guess concerning its probability, but an alteration in the assumed value of the velocity of light would certainly make some change, which should not be overlooked.

From the point of view of astronomy and cosmology, however, an acceptance of Einstein's first postulate, the postulate of relativity, has the most profound effect on current ideas, the failure to recognise which can again be attributed only to the complete state of confusion which exists between the requirements of Einstein's and Lorentz's theories. This is so, no matter whether the second postulate fails or not, and it is most evident in the phenomenon of the Doppler effect. This, considered purely as a fact of observation, quite apart from its interpretation, is simply a relation between (1) the relative motion, along the line joining them, of a source of light and an observer, and (2) the spectrum of that light as seen by that observer (for 'observer' here we may, of course, substitute 'spectrometer', for the effect can be recorded wholly by instrumental means, and the record may or may not be observed; if it is, all who observe it, however moving, will see the same thing; the effect is wholly objective). If source and observer are approaching one another, the spectrum is shifted in one direction (which, without prejudice, we may describe as towards the shorter wave-lengths) and if they are receding from one another, it is shifted in the opposite direction — with respect, in each case, to its position when source and observer are relatively at rest.

Now on Lorentz's theory, in which the light consists of waves travelling through the ether, this effect has different causes according to whether it is the source or the observer, that is moving. Suppose for simplicity that at first both are at rest in the ether, at a distance r apart, and then one starts to move towards the other. If the source moves its light-waves are compressed, but the compressed waves do not reach the observer until a time *r/c* after the movement begins, so he does not see the spectrum shift until after the lapse of that time (duration). If, on the other hand, the observer moves, he at once receives the unaltered waves, but with a greater frequency, so he sees the spectrum shift immediately. The shift is the same in amount in both cases because, since the velocity of the waves through the ether is unchanged by the movement, being a property of the ether alone, and is equal to the product of the wave-length and frequency, a decrease of wave-length and an equivalent increase of frequency produce the same effect on the spectrum. The only observable difference, then, between a movement of the source towards the observer and an equal movement of the observer towards the source, is that in the first case observation of the effect is delayed and in the second case it is immediate. Synchronised clocks at the positions of source and observer would therefore reveal unquestionably which of the two has moved.

But now, suppose that, as on Einstein's theory, the relativity postulate is true. Then there can be *no* observable distinction between the movement of the source towards the observer and that of the observer towards the source. Hence, either observation of the movement is immediate in both cases, or it is delayed in both cases. Now we know from experience which of these alternatives to choose. We know that, with respect to a distant star, the orbital motion of the Earth round the Sun causes an alternation of approach and recession (on which, of course, a continuous movement between the star and the whole solar system may be superposed, but that would not affect the oscillation of the observed stellar spectrum caused by the Earth's orbital motion). The Doppler effect corresponding to this is observed to synchronise with the orbital motion in every case, so we know that, when the observer moves, the effect is seen immediately, just as on Lorentz's theory. But that means, according to the relativity postulate, that the effect must also be seen immediately when the star moves, otherwise there would be an observable distinction between the two cases. (Indeed, the very phrases, 'when the observer moves' and 'when the star moves' are a concession to ordinary modes of thought; on the relativity principle, the only proper description in each case would be 'when the relative motion occurs'). And this is true, no matter whether Einstein's *second* postulate is true or false, for it follows wholly and inevitably from the first. Therefore those who accept the first postulate, no matter whether they accept the whole of the special relativity theory or not, must accept that every Doppler effect observed is a result of a motion occurring at the time (instant) of observation, no matter how far away the source of light may be, and if they measure a velocity from it, that velocity must be that which exists when the observation is made.[8]

It is obvious that questions of the highest importance to cosmology arise from this consideration. It implies, for instance, that the red-shifts now observed in the spectra of the distant nebulae (if they are indeed Doppler effects, which, though it is the universal conviction, is a most hazardous speculation) denote velocities existing now, and not millions of years ago. But that is only one of the numerous examples of the general failure to appreciate the impossibility of accepting both the relativity postulate and assumptions concerning motion and gravitation that arc meaningless without a Lorentzian ether; it is the old story of identifying the quite incompatible Einstein and Lorentz theories and using whichever happens to be convenient at the moment.

There is much talk at present, for example, of 'gravitational waves', which associates the mutual gravitation of two bodies with waves travelling with velocity *c* between the bodies. But if gravitation is relative — if, for example, the gravitation between the Sun and the Earth is not something uniquely exerted by one (say the Sun) to which the other responds, but a relation between the two which cannot exist without both — then which way do the waves travel? If they travel both ways, forming standing waves, what does the velocity *c* mean? Nevertheless, not only have such waves been held to be possible, but also experimental observations have been interpreted as evidence of their existence by physicists who, at the same time, claim the validity of a relativity theory of gravitation.

The fact is that we know far less about these things than we imagine. The more one reflects intelligently on the nature of light, matter and gravitation, the more he realises that there arc problems connected with them that are quite insoluble in terms of our current notions. But we no longer reflect intelligently on these things. We have not only left behind Dale's conception of a science that accepts nature's answers humbly; we have cast off even the degree of humility needed to question her, and manfully overcome the fear of prejudice and preconception that so restricted science in the days of its bondage to truth. Professor Hoyle has plainly stated his advocacy of the process of telling nature what to do instead of looking to sec what she does.[9] After uncharitable observations had compelled an abandonment of one of his confident speculations, he thus described how the speculations arose: 'The struggle has been to invent a form of mathematics operating in the manner customary in physics, namely, starting from an action principle.' But his failure in the struggle left him undaunted, and was quite powerless to drive him back to what used to be the manner customary in physics: on the contrary, he announced his continued devotion to 'the motive underlying the investigation, the avoidance of a universal singularity, rather than an experiment in the laboratory'.

Margaret Fuller was thought presumptuous in declaring her acceptance of the universe: Hoyle, like the rest of the 'mathematicians', expects the universe, and us, to accept him. Our only sane comment is: 'By Gad! We'd better not!' The universe will not stoop to comment, it will act.

Conclusion

Little need be said by way of summing-up. The primary and inescapable purpose of this book, which Part One attempts to fulfil, is to make known, to those with an indefeasible right to the knowledge, the present state of the scientific world as revealed by its practice, and to bring it into comparison with what is generally believed, and implicitly trusted, to be its state as typically expressed by the late Sir Henry Dale. I leave the reader to judge the significance of the comparison for him, and to estimate what the consequences are likely to be if the present degree of conformity continues. I have no doubt that my view of these things has emerged during the process — I should be ashamed if it had not — but I hope that it has not in any way interfered with the objectivity of the presentation, and will not influence the reader in forming his own independent judgement from the intrinsic nature of the facts themselves.

It is not I who state that high energy physicists in general have not the time or ability, and himself (who is one) not the inclination, to understand the principles underlying his work; who 'teach' what he does not understand to those who will undertake that work on the basis of what they are 'taught'; who will not submit for the consideration of others what he does not himself understand and agree with; and who will not commit himself to an assurance that integrity is still preserved among physicists; it is the (then) President of the Royal Society. It is not I who do not feel able to provide one sentence in answer to a question on a subject in which he is a specialist and on which depends the safety of the population: it is the Chief Scientific Adviser to the Ministry of Defence. It is not I who state in private that he decides by gambling whether to take any notice of that question, and in public assure an audience of the fierce and uncompromising honesty of the scientific attitude: it is a Sectional President of the British Association for the Advancement of Science. It is not I who close the columns of the leading scientific journal to informed questioning of the Royal Society on a matter of outstanding public importance; who interpose an impassable obstruction to the conclusive settlement of that matter; and who leave promise after promise unfulfilled to prevent such a settlement from being reached: it is the editor of *Nature.* It is not I who decide that the action of that editor in refusing to allow the Royal Society to be questioned by the public on a matter vital to the public interest is not a fit subject to be submitted to the Press Council: it is the officers of the Press Council itself. And so on. These are facts, not opinions. I alone have indisputable evidence of them, and I alone am therefore able, and have a compulsory obligation, to bring them to the notice of those whom they so deeply affect.

Nevertheless, since they all arise from matters in which I have been one of the parties concerned, I should like to add one piece of evidence — again for the uninfluenced judgement of the reader -which is altogether independent of my activities. On p. 200 quote a statement by J. G. Fox, an American physicist (from considerations whose validity is irrelevant to the present point) that 'the material considered as evidence in the past has been shown to be possibly either irrelevant or inconclusive. This is a surprising situation in which to find us half a century after the inception of special relativity.' Supposing Dale's description of science to be a true one, what should one conclude from this? Something, I suggest, on the following lines: 'This is an anomaly which science cannot neglect. Nature's answer to the point in question (which is one of the two foundation stones of the special relativity theory) must be sought immediately and, whatever it is, accepted, made known, and acted upon. In the meantime, dangerous experiments based only on evidence that is possibly irrelevant or inconclusive should be suspended.' In fact, Fox's conclusion was this:

> The small gap [*sic*] in the experimental foundations of special relativity which has been pointed out in the foregoing is of far less interest now than it would have been a few decades ago ... The odds now that a decisive experiment will yield the expected result have become so overwhelming that the experiment may seem hardly worth doing ... the general principle of Lorentz invariance has long since so proved its worth in physics that it is all but incredible that some future experiment of the sort proposed above could come to any but the expected conclusion.
>
> Nevertheless if one balances the overwhelming odds against such an experiment yielding anything new against the overwhelming importance of the point to be tested, he may conclude that the experiment should be performed.

(The reference to 'the odds' as a factor in deciding whether to seek nature's answer to a question or to trust the infallibility of our own expectations is interestingly reminiscent of Ziman's gambling (p. 89), but let that pass).

I suggest that the reader who wishes to form a true idea of the present state of science in this field should formulate a description of science for himself, which is such that Fox's comment is as natural an inference from it as the one I proposed above follows from Dale's description. He will then see the two roads, which science is now facing and between which it must choose: shall it continue along Fox's road or return to Dale's? The question is not academic: on the answer will depend the moral contribution which science, in the position of authority which it has now acquired, shall in future make to civilisation, as well as the continuance of life on this planet.

In fairness to Fox I should add two things. First, he is the only supporter of the theory, so far as I know, who has had the temerity to suggest that Einstein's postulate is now even remotely open to question (as distinct from additional confirmation) at all, and he wrote a later paper going further into the matter, though (presumably in view of 'the odds') neither he nor anyone else has thought the experiment he proposed worth attempting during the last nine years. Secondly, he wished to get his paper published. His chance of that, if he ventured to make the comment that I suggest would naturally follow from Dale's view of science, may be assessed by the response of the American journal *Science to* my question (pp. 81-3).

Part Two is of secondary — though I believe still considerable — importance, but I should not have troubled to write it had not the necessity of Part One afforded an opportunity, if not issued a command, for it. I should have left it unsaid because I know its futility, since those to whom it should be of concern have lost the ability to read it. The eyes of a few of them might have passed along the lines, but the meaning could not have entered their minds because those minds are closed by an impenetrable barrier to any suggestion that special relativity is not irrefragable truth. It would at best have received the comment of me Royal Society referee (p. 57) that, although it contained matter of historical interest, anyone who took it seriously would make himself ridiculous. I have met none willing to face that indignity merely because he cannot find a fault in what he knows by supernatural revelation (though he would not call it such, yet would be at a loss to find an alternative name for its source) must nevertheless be faulty. Unless, therefore, the facts related in Part One should lead to the awakening of physicists of influence — either directly or through the compulsion of outside pressure — to an awareness of the state into which they have unconsciously lapsed, it will remain unheeded until the time comes when they will bitterly but vainly regret the lost opportunity of merely making themselves ridiculous.

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