INTRODUCTION

Summary. It is explained that the book, of which this is the introductory volume, is intended to be a complete treatise on physics of which the main object is criticism. Criticism does not involve adverse judgement, but only analysis, which is more likely to strengthen than to weaken the evidence for the propositions criticised. It is suggested that such criticism may have a value, though an indirect value, for those pursuing original researches as well as for teachers and students. The general plan of the work is sketched and some of the main questions which are considered mentioned.

Criticism of this kind is not novel, but it has not been applied to experimental science as fully as to mathematics. Such criticism as has been applied to physics has almost always come from mathematicians. It is suggested that criticism by one interested in the experimental rather than the mathematical side of the subject may have some special interest.

One reason why criticism has been left so largely to mathematicians is that physicists are afraid of being led into any discussion which they regard as philosophical. Some remarks are made on the origin and basis of this attitude: the obvious fact is pointed out that, if it is true that fundamental scientific discussion necessarily lands us in philosophy, then philosophy must be a part of science and merits our attention. The opinion is, however, expressed that the fear is not justified and that science can be adequately discussed without any philosophy at all. On the other hand, there are connections between science and philosophy which it has seemed desirable to notice in a special chapter sharply distinguished from the rest of the book.

The object of the book. I want to explain rather carefully what is the object of this book; for if the reader does not understand that object thoroughly, he will not be able to utilise whatever value the book may possess.

The book aspires to be a treatise on physics, complete within its limits, written by a serious student of the science for other serious students. It is not in any sense a popular work addressed to those whose chief intellectual interests lie elsewhere; it assumes throughout entire familiarity with all the facts and theories of physics, ancient and modern. Its primary purpose does not exclude the possibility that some portions of it may be comprehensible and even mildly interesting to those who have not such familiarity; for even when reference is made to matters beyond their knowledge, the context will sometimes show what the example is intended to illustrate. But I must insist that any value of this kind which the book may have is purely incidental; the needs of professional physicists and their needs alone have been considered in writing it.

Nevertheless its object is not the same as that of most of the works which are addressed to professional physicists. The work is neither an original memoir, a description of original investigations in science, nor a standard treatise designed to replace, for example, the monumental *Handbuch* of Winkelmann. It does not pretend to add to the sum of distinctively scientific knowledge; indeed, if there is in it any scientific statement which is not perfectly familiar to those for whom it is written, that statement can hardly be anything but a blunder. On the other hand it is not a mere compilation of information already existing; large tracts of knowledge will be passed by without mention and those which are selected for the most detailed examination will not always be those which are generally considered of the greatest scientific importance or interest. And again it is not a text-book for students or their teachers. Its subjects have not been chosen because they fall within the syllabus of any Board of Examiners, actual or such as might exist in an ideal world; they are not those which are most suitable for immature minds.

Criticism. It is easier thus to explain what the book is not than to explain what it is. For it is only because the questions which it discusses are not usually asked and because the bare possibility of asking them is often not recognised that there is any need to enter on this explanation. Briefly it may be said that what is aimed at here is not investigation or exposition, but criticism. In addition to the attitudes towards physics of the original investigator and the teacher there is possible another, the attitude of the critic. The teacher does not want criticism in his text-books, because the faculty of appreciating it is one of the latest to appear in the process of education; a critical attitude towards a subject can only be adopted usefully when a complete mastery of its content has been attained. So long as the pupil is in the text-book stage he is prepared to accept, and is usually only too ready to accept, statements without any very searching inquiry into their foundation. The original investigator, on the other hand, does not want criticism in his treatises, since the exercise of the critical faculty is a necessary part of investigation; he feels perfectly prepared to provide it for himself and would rather resent having it offered to him, cut and dried, by others.

It is, I think, largely because scientific literature is usually designed to meet directly the needs of one or other of these great classes that criticism in science has been neglected—so neglected, indeed, that everyone does not realise what it involves. It seems often to be thought that to criticise a proposition is merely to judge whether it is true or false, with a strong bias in favour of the second alternative. But before such a judgement can be passed reasonably an important process must be carried out which is applicable just as much to the most certainly established as to the most dubitable statement, the process of analysis. The critic must determine exactly what the statement means, on what evidence it is advanced, what relation it bears to other statements either involved in it as assumptions or derived from it as consequences. It is only when these matters have been decided that judgement can be passed, and in many cases the preliminary analysis will be found a much more important part of the criticism than the final judgement.

Criticism of this nature has secured a large part of the attention of pure mathematicians for the last 30 years and has become almost a new branch of their study. In this treatise on physics I hope to extend in some measure such criticism to a portion of experimental science. Our inquiries will speedily discover to us reasons why criticism can never be as complete or as fundamental in an experimental science as in one based on pure logic; the necessary limitations which we shall find provide another reason why this branch of the study has received so little attention. But it seems worth while to make the attempt. I want to inquire into the precise meaning of the many propositions which make up the science of physics and into the evidence on which they are based; I want to examine the significance and connotation of the ideas in terms of which its results are expressed and into the character of their mutual relations.

These inquiries will be clearly the more interesting the more fundamental are the propositions and the ideas concerning which they are made; the investigations will be chiefly directed to those basic principles of each department of the science which are so firmly established that criticism of them, in the narrower sense of a judgement of their truth or validity, is not generally thought necessary. In examining these fundamental matters it will not be our object to raise doubts concerning them; it will be rather to examine why no doubts are possible. The further developments of the science will not require so much notice; the nature of their connection with the fundamental principles is more generally understood and more adequately treated in text-books of the usual type. Least attention of all will be required for those branches of our knowledge where opinion is still not wholly agreed; to examine in detail propositions which may yet be rejected would be waste of time.

The value of criticism. But of what use is such criticism if it is to add nothing to the established doctrines of physics and subtract nothing from them? What is the use of criticism, in any sense of the word, applied to matters concerning which everyone is perfectly agreed? Such questions are sure to be asked. The most direct answer that I can give, and the only one on which I am prepared to insist, is that criticism of this kind is to me intrinsically interesting; I want to undertake it for its own sake and not for any ulterior object. I express my thoughts in the form of a book, chiefly because the best way to clear them from confusion is to explain them to others, but also because others may possibly have the same interests. But I venture also to think that greater attention to these matters on the part of the two main classes of professed physicists, who are at present satisfied with treatises of a more ordinary type, might sometimes help them in their own special work, even if it were not worth giving for its own sake.

Nothing that I have to say is likely to offer any direct help in the solution of the problems of original research. The physical propositions about which most discussion will range are so fundamental, so long and so solidly established, that it is in the highest degree unlikely that an attack upon any new or outstanding question would be aided by a reconsideration of their meaning or validity. And while they remain undoubted a detailed analysis of them has no immediate bearing on the progress of the science. But it is always possible that some unforeseen development may drag once more into the arena of discussion laws and theories which nobody dreams of doubting to-day. Poincaré has pointed out that it might become more convenient to deny that "space is Euclidean"; if once the validity of that proposition were called in question a discussion of its precise meaning, of the evidence on which it is based, and of its relation to other propositions which must stand or fall with it—such a discussion might become of very immediate practical interest. It is not certain that even those who stand in the forefront of original investigation would be able to answer all the questions that might be raised without a good deal of thought and trouble[1].

But any value that criticism may have for the "practising physicist" will probably be more indirect and arise from a better understanding of the methods that he practises. Measurement and calculation are two of the most important weapons in the armoury of physical research, and though it is not suggested that actual errors in their use are frequent or, if they were, would be avoided by such discussions as are to be offered, I cannot help thinking that a complete inquiry into their nature and relations may be useful. What is meant by an arbitrary scale of measurement? Why is Mohs' scale of hardness more "arbitrary" than the Centigrade scale of temperature or the metric system of length? What conditions must be fulfilled before a scale of hardness which is not arbitrary can be substituted? What is the basis of the "argument from dimensions"? Why are the dimensions of a magnitude always expressible by rational indices? If a magnitude can have a dimension $T^{-\frac{1}{2}}$, why should it not have a dimension log T? What are the assumptions added in direct calculation to those employed in the argument from dimensions, the assumptions which permit the determination of the "undetermined constant"? Why is the undetermined constant never very different from 1?[2] How far is the method applicable to electrical quantities which have two different dimensions? These questions are such as men of science may properly ask.

The benefits which a teacher of science might derive from criticism is more direct and obvious. A complete logical and analytical view of science may not be of benefit to his pupils, but he is less likely to puzzle them if he has himself the clearest possible view of the matters he teaches. That he

[1] See Preface. When Einstein's Principle of Relativity first provoked discussion, several physicists of the first rank condemned some of the propositions, involving notions of "time" and "space," implied by it on the ground, not that they were actually false, but that they were self-inconsistent and contradictory. Even those who regard the Principle as nothing more than an elegant mathematical device admit now that such criticisms were misdirected. If Part III of this volume had been written at that time, doubtless it would not have prevented these errors directly; it is far more likely that I should have made the blunders or worse blunders myself. But perhaps the mere raising of the questions, which could hardly have been avoided, would have helped others to escape them.

[2] This question has been asked and not answered by a physicist no less distinguished than Einstein (*Ann. d. Phys.* 35. 687. 1911). Of course the constant does sometimes differ considerably from 1: in one actual application of the argument it is as great as $6\pi^2$, and other applications could be devised in which it would be still greater. But there is some truth in the statement for which the question seeks an answer.

often has not such a view is apparent to everyone who has done much reviewing of elementary text-books. Whenever any fundamental matter is approached there is a tendency to repeat, with hardly verbal alteration, statements of which the original source is now difficult to trace; they may have been lucid and true in their original context, but now they are merely confusing or deliberately misleading. That delightfully naïve definition of mass as "quantity of matter", so redolent of the 18th century, is fortunately vanishing from our text-books, but students are still told that "matter is that which occupies space", and are left to gasp when they learn later, probably from some other book, that the "aether which pervades all space" is not matter. The prevalent explanation of the paradox: The day is the unit of time, but the length of the day is increasing, can deceive few intelligent pupils, but they may well think that it is their imagination which is at fault, and not that of the author of the phrase (surely the least imaginative of mankind), when they are surprised to learn that "negative weight is inconceivable."

Of course these are extreme instances, and most teachers are able to supply the deficiencies of their text-books; but if such obvious absurdities can pass almost unchallenged, it is likely that minor errors or, what is worse, ambiguities escape detection. They would be avoided if writers would not write anything unless they are quite sure what it means themselves; if they are not sure, the cause is much more likely to be found in some real obscurity than in their own stupidity. Once it is clear what a scientific statement means, it is seldom difficult to determine whether it is true or false; the elucidation of meaning is one of the chief tasks which this book is designed to undertake.

The order of criticism. However, in this volume of the treatise detailed criticism of actual scientific propositions will hardly be attempted; its purpose is wholly introductory. There are certain matters which are common to all branches of the science and certain features common to all physical propositions. It is more convenient to discuss them once and for all before beginning detailed inquiry than to discuss them one by one as they arise. For instance, almost all scientific propositions involve to some extent temporal and spatial ideas, and their proof depends on measurements of times and distances. They would not be significant and would not be true unless certain other propositions, involving these ideas and these measurements, were also true. In Part III of this volume we examine the temporal and spatial propositions on which all other physical propositions are based; they are not usually regarded themselves as physical propositions; indeed it seems not always to be recognised that there are such propositions. In Part II we recede yet a further stage into the foundations of science. The propositions of Part III, like those that are based on them, involve the conceptions of measurement and calculation. It is the use of these conceptions which distinguishes physics from all other sciences[1]. Our inquiry therefore is conveniently prefaced

[1] This statement may be disputed, but I think it is true. Measurements in all other sciences are those of magnitudes for which a method of measurement has been developed

by a discussion of these conceptions and a consideration of what is common to all systems of measurement and of calculation; we ask what distinguishes measurable properties from those which are not measurable and exactly how or why measurement leads to the introduction of mathematical calculation.

In Part I we again proceed in the same direction. Measurement and calculation are possible because certain laws and theories are true. Before we can understand measurement we must understand how laws and theories are established and precisely what they assert.

I do not mean that in this introduction no questions are raised which will not turn out to be of importance later in connection with definitely scientific propositions. There are many digressions which do not lead to the main goal of the inquiry; and it must be admitted that there is a danger that these side tracks may sometimes obscure the straight road. But I have endeavoured consistently to refrain from the discussion of any matters which do not arise directly from some strictly scientific question. Some of the discussions may have an interest apart from their bearing on the problems of modern physics, but that interest, I would repeat, is incidental. This volume is introductory and nothing more; it will have little value apart from its successors. Why it is issued without them is explained in the preface.

In describing the purpose of this introductory volume the three parts have been mentioned in reverse order. It may well be urged that this reverse order is logical and that it should have been adopted in writing. If our object is only to examine actual physical propositions, it would appear better to start with those propositions and to answer any questions about their derivation or foundation as they appear. Further by that process a grave danger might be avoided. Our introduction, if it comes first, must consist of the statement of general principles; only a few examples of their application can be given, and the choice of them will be determined by their simplicity or their aptness in illustration rather than for their intrinsic importance. There can be no guarantee that these principles are really those involved in much more important but much more complicated examples. There is clearly a grave risk that we shall be led into the error of laying down general rules about science without a sufficiently wide basis. This is an objection to our procedure which is not to be lightly brushed aside. Many awful examples could be quoted as a warning of the danger of laying down *a priori* doctrines about the nature of science and then considering how far science can be twisted into agreement with those doctrines. But forewarned is forearmed, and there is no alternative but to face this danger. For no criticism can proceed except on some predetermined principles; the critic must have some foreknowledge of the qualities he expects to find in the thing criticised. Even if these principles are not stated beforehand, they must necessarily be present in the critic's mind; and if they are erroneous, they are likely to be less harmful if attention is drawn to them. Before he discusses any particular

by researches which are distinctly physical. It is noteworthy that the portion of chemistry which involves accurate measurement and mathematical calculation, so closely associated with measurement, is called "physical chemistry."

example, the critic should consider in his own mind whether the principles he is about to apply are applicable also to other examples over as wide a range as possible; but, even if it were possible, it is not certain that he ought to bring all these examples to the reader's notice before he considers any one of them. I venture to claim that I have not neglected my duty in this matter. Every question which will be raised in this volume has been suggested by what appears to be a direct train of thought, starting from some definitely scientific problem which has occurred in thinking or writing about a considerable range of scientific topics. During the period between the inception of the work and its publication I have been led by ordinary scientific work to consider with some care many branches of the science far apart. Wherever a new branch has been considered, it has always been viewed in the light of the principles on which this treatise is based. It has usually happened that the examination has led to some revision of views previously expressed, and many portions of the book (especially in the second part) have had to be rewritten several times; but the main ideas have remained unaltered.

Relation to previous work. This then is the intention of the book. It may very well seem that there has been much ado about nothing and that the intention is far from being novel. Criticism is not new in physics. Mach's classical work on Mechanics is just such criticism as has been described; much of Thomson and Tait's *Natural Philosophy* and of the physical writings of Helmholtz is critical in this sense. Volumes and essays on the principles of science and on their application to particular problems are, or ought to be, on the bookshelf of every physicist; he cannot afford to neglect Poincaré any more than Landolt and Börnstein. Where such reapers have passed, what room is there for gleaners?

Of course I should be proud if my work were classed with theirs; but I think there is a difference of degree which almost amounts to a difference in kind. All the writings which have just been mentioned are more or less fragmentary; they consider either one particular scientific problem and do not consider how far the principles applied in its solution are applicable elsewhere; or they consider one particular principle and do not trace its application to more than a few isolated examples. Mach's *Mechanics* taken in conjunction with his lectures on Heat provides a more complete criticism of physics than the works of any other writer; but there is a large range, especially in the later developments of the subject, to which he makes no reference. If therefore this book were nothing but a compilation of the views of others with a slight extension of them to problems which have arisen since their day, it would probably have a value different from, but not necessarily greater than, that of the writings from which it would be compiled; it would represent a treatise as compared with original monographs. I have not attempted to give the book this value; the views of others are expressed indirectly through their influence on my own rather than directly; but I think the book has a special character through aiming at the completeness of a treatise rather than the detail of a monograph.

However that is not to my mind the chief difference. It is remarkable, though readily explicable, that all the writings that have been mentioned and most of those on similar lines are the work of those who were primarily mathematicians and not experimenters. They were either, like Kelvin or Helmholtz, members of the old school of physicists who always approached that science through the avenue of mathematics, or, like Mach and Poincaré, mathematicians by profession. Now of course mathematics is essential to physics, but nevertheless it is not physics; it is for the physicist a tool, a means to an end and not an end in itself. Nobody can hope to advance physical knowledge greatly unless he has at his disposal, either in himself or in another, some portion of the skill of the mathematician; but neither can he hope to advance it to-day unless he has also at his disposal the skill of the glass-blower[1]. It is hardly possible for a physicist to be wholly ignorant of mathematics—it is only if he is a Faraday that he can achieve that feat; but it is quite possible for a mathematician to be wholly ignorant of physics, especially in its more modern developments. The importance of experimental relatively to mathematical physics has increased very greatly, and on this account alone it is desirable that a criticism of the science should be undertaken by somebody whose knowledge and interests lie on that side; he is sure to see problems which the mathematician has overlooked.

But that is not all. It will be urged very strongly in what follows that, though a great part of science is independent of personality and derives its value from the fact that all men can agree about it, there is another part, not less important, which is valuable because it is personal. The very significance of this part depends upon personal intellectual tastes, upon things which are valuable just because men differ in them. Now the intellectual tastes of a mathematician necessarily differ from those of the experimental physicist; to the latter the mere handling of apparatus and the exercise of ingenuity in the overcoming of mechanical difficulties is a source of intense pleasure; to the former they are to be avoided. It is doubtless ridiculous to maintain that the practice of the experimental art confers in some mysterious fashion a power of appreciating science which is not to be attained by any other means; but it is not ridiculous to maintain that the enjoyment of that art indicates mental qualities different from those which make it seem simply laborious. And as the physicist possesses intellectual interests which the mathematician lacks, so the reverse is also true. The difference between them, quite unimportant in the normal course of scientific investigation, is vital when we come to inquire into fundamentals; for in such an inquiry a stage must ultimately be reached at which we have to accept without argument propositions as ultimate. What propositions we shall be prepared so to accept must depend in some measure on our intellectual tastes. In

[1] I hope I need hardly explain that I do not mean to place mathematics and glass-blowing on the same level. The difference is, of course, that the former, but not the latter, has an intrinsic value as an end as well as a value as a means. Nor do I mean by "either in himself or in another" that it is possible to hand over mathematical work to an assistant as completely as the making of apparatus. But it will generally be admitted that a man will be a better experimentalist if he can do his own instrument-making, even if he does not actually do it.

order to indicate how a difference may arise between physicists and mathematicians, it may be suggested that it will come from a different estimate of the relative importance and ultimate value of two processes of thought, Deduction and Induction. "Induction", says the mathematician[1], "appears to me either disguised deduction or a mere method of making plausible guesses." A physicist would be more likely to interchange the two terms in that statement.

Science and metaphysics. But the very fact is significant that those interested in experimental science have left to mathematicians the more fundamental inquiries into their study. Does it mean that criticism of science is impossible or even that it is undesirable? The possibility can only be decided by an attempt; it will be discussed better at the end of the treatise than at the beginning. But the desirability may be called in question here. There is no good in refusing to recognise that many physicists are not merely uninterested in fundamental criticism, but are positively hostile to it. If an attempt is made to introduce into any physical discussion considerations more general or more fundamental than would be appropriate to an ordinary text-book, it is apt to be met with some sneer about "philosophical" or "metaphysical" arguments, and with a suggestion that such matters are unworthy of the attention of a serious man of science.

These terms are not likely to be used unless controversy has become heated. In our calmer moments we do not follow the practice of politicians who dismiss, by the attachment of a label, views which they find it inconvenient either to accept or refute. It is only rude and senselessly rude to call a man by a name which he does not accept. Nevertheless the feelings which inspire such outbursts are not to be neglected lightly, and it is worth while to consider for a moment whether there is any basis for the prejudice.

It has, of course, an historical origin. Science, unlike mathematics, had for a time to struggle against philosophers for recognition as an independent and important branch of learning. The battle is won, and it is now rather the philosopher who walks warily lest he bring on his head the contempt of firmly established science. But the memory of it remains in a reluctance to discuss quite freely the subjects (largely those of the third part of this volume) which once formed part of the exclusive province of philosophy. Those who attacked science—and their example is followed by those who to-day adopt the yet more offensive method of patronising it—seldom took the trouble to understand what they attacked; a discussion of such subjects came to be associated with an attack on firmly established propositions by persons with a complete and impenetrable ignorance of everything scientific.

But "fas est et ab hoste doceri" does not mean that it is always wise to adopt the errors of a defeated foe. Because philosophers have talked nonsense (as it seems to us) about "space" and "time," there is no reason why we should follow their example; because they would not take the trouble to find out what we mean, there is no reason why we should not find out what they mean, or even what we mean ourselves. It cannot be denied

[1] Mr Bertrand Russell, *The principles of mathematics*, Ch. II.

that science does state and use important propositions about space, time and motion; we say that space is three-dimensional, time one-dimensional and that motion can be compounded according to the parallelogram law[1]. And if we make such statements and believe them, it is allowable to ask exactly what we mean when we state them and on what grounds we believe them. They are not so simple as to be incapable of analysis, and if the evidence on which we base them has anything to do with experiment or observation we ought to be able to give some idea of what that evidence is. I have already explained that in applying criticism to them I do not mean to dispute, but rather to confirm, their value; it is possible that we shall discover limits to their truth, but that will only make their truth within those limits more certain. And if anybody thinks that, if they are not to be disputed, nobody who is not either stupid or ignorant could imagine that there is anything interesting or relevant to say about them—well, he might at least read what I have to say before coming to that conclusion.

And what applies to the discussions of time, space, and motion applies equally to other parts of the book. If anyone chooses to call them metaphysical, there is nothing to prevent him; but he must not fall into the egregious error of supposing that thereby he renders them unworthy of consideration; the question remains whether metaphysics, in his sense, is relevant to science. If it is found that propositions and conceptions which are distinctively scientific are based on other propositions and conceptions and derive from them their truth and significance, then, even though some people choose to term them metaphysical and not scientific, it remains the fact that these other propositions are essential to science. I am sorry to have to insist on considerations so simple and elementary; but even those who do not share the prejudice against which I am contending will admit that it exists. Now whatever faults this book may possess, they do not include that of being metaphysical in any reasonable sense. For one of the chief characteristics which distinguishes science from metaphysics, and the feature which makes men of science so averse from the latter, is that in science, but not in metaphysics, it is possible to obtain universal assent for conclusions, and to present results which do not lose their value because, when they are presented, they are so obvious as to be indubitable. I maintain that the results presented in this work are of that nature. I am quite prepared to find that the vast majority of men of science find everything I have to say dull and trite and so familiar that it is not worth saying; but if that should be their

[1] It is interesting to note as an illustration of the perversity of the prejudice that though everyone would state cheerfully that space is three-dimensional, many would feel that the corresponding statement about time is already beginning to have a dangerous taint of metaphysics. Accordingly I will substitute for "time is one-dimensional" another proposition closely allied to it: If an event *C* happens between two events *A* and *B*, and *D* happens also between *A* and *B*, then *D* must happen either simultaneously with *C*, or between *A* and *C* or between *C* and *B*. It will not be denied that this proposition is important and true; and nobody would, I think, call it metaphysical. The corresponding proposition concerning space is not true. Both London and New York are between the north and south poles: but London is neither coincident with New York, nor between New York and the north pole, nor between New York and the south pole.