theory will do. The latter theory seems to rest on a fundamental confusion between facts and the estimates which different observers will form of them.

III. By C. D. Broad.

I shall deal first with the difficulties found by Mr. Ross in arguments that have been used for the special theory of relativity. I think that these difficulties rest mainly on misunderstandings, and that they can easily be removed by a little explanation.

(i) Mr. Ross regards it as a weakness that the constancy of the velocity of light should be the keystone of the special theory and yet be discarded in the general theory. There is no real difficulty here, when we remember the different subjects with which the two theories are concerned. The special theory explicitly confined itself to systems in uniform translational motion with respect to a Newtonian frame of reference. It did not profess to tell us what would happen if a system rotated with respect to such a frame or moved with an accelerated rectilinear motion with respect to it. Now the general theory professes to deal with *all* motions, no matter to what they may be relative or what may be their kinematic characteristics. There is nothing startling in the fact that a proposition which is true and important for a restricted class of motions should not be true of all motions whatever. Mr. Ross would not, I trow, feel any difficulty if he were told that certain phonetic laws are the keystone of the sound-changes in Teutonic languages, but that they are not true without modification when we take into account all Indo-European languages.

(ii) Mr. Ross blames relativists for not having exhausted all the possibilities of the older theory. On their own admission all that we directly know is that the earth and the stars move with respect to each other. If there be an ether this fact is quite compatible with the earth being at rest with respect to it. Now the results of the Michelson-Morley experiment are paradoxical only because the earth is assumed to move through the ether, not because it moves with respect to the stars. And the latter, we have seen, does not imply the former. Mr. Ross's alternative would split into two forms according as he holds: (*a*) that there is, or (*b*) that there is not relative motion between different parts of the ether. On the former alternative both the earth and the stars might be at rest relatively to the parts of the ether in their immediate neighbourhoods. On the latter alternative the stars would have to be moving through the ether and to have the same velocity with respect to it as with respect to the earth. The former hypothesis has been tried, and is known to lead to conflicts with the facts about aberration. The latter, I think, is the one that Mr. Ross has in mind. It cannot be regarded as plausible to hold that the earth is the one body at rest in an ocean of stagnant ether, whilst the stars are all moving about in it. If the ether be a real physical substance pervading the whole universe, as those who take it seriously enough to entertain either of these alternatives must hold, this second alternative places our small planet in a strangely unique position. But apart from these *à priori* objections, the physical difficulties in any such view are colossal. To account for aberration we shall have to suppose that all the stars describe ellipses in the ether in the period of a year. These ellipses will have to be adjusted to each other in a very intimate way, for which the present theory supplies no explanation. Moreover, considering the extreme remoteness of many of the stars, the ellipses will be of gigantic size, and therefore the velocities with which the stars must move in order to describe them in a year will be stupendous—in some cases of the same order as that of light. Not only are the dynamical difficulties of supposing such large masses to be in such swift motion very great, but the shifting of the lines of the spectrum in light from such stars, due to the Döppler effect, would, I imagine, make stellar spectra utterly different from what they are found to be.

(iii) But Mr. Ross's main difficulty is that he thinks that relativists take absolute motion as a premise in their proofs of the relativity transformations, and that these results are then supposed by them to disprove absolute motion. Before considering in detail whether relativists actually do this we may point out what exactly would be the logical consequences of such procedure. If the observable facts and the assumption of absolute motion imply the relativity transformations, and these in turn imply the denial of absolute motion, it will follow that the facts and the assumption of absolute motion imply the denial of absolute motion. From this we *should* be justified in going on to deny absolute motion. But we should *not* be justified in taking the further step of asserting the theory of relativity. Thus, if the relativistic arguments were of the form which Mr. Ross believes, and if there were no internal fallacy in them, we should be justified in denying absolute motion but not in asserting the theory of relativity.

Actually, however, Mr. Ross is mistaken in thinking that relativists use the absolute theory as a premise to prove the theory of relativity. Let me take my own case, *e.g.*, as Mr. Ross accuses me of this procedure. For didactic purposes I started with the ordinary assumptions of absolute space, time, and motion, and an ether at rest in this space. I then drew a distinction between distances, time-lapses, etc., and our *measures* of these. And I showed that if we wanted to account for such facts as the Michelson-Morley on these assumptions we should have to assume certain physical changes in our rods and clocks when they moved through the ether. The results of these changes are summed up in the transformation equations, and at this stage these may be regarded as expressing the connexion between the distances and time-lapses which we should record if our system were at rest in the ether and those which we should record if we were moving through the ether with an uniform rectilinear velocity. At that stage I was not attempting to prove the *theory of relativity*, but only to prove that such and such relations must hold between our readings when we are in motion and the absolute magnitudes if the facts are to be squared with the *absolute theory*. The next stage is to reflect on these results. (*a*) We see that the physical processes needed to make the absolute theory square with the facts are unnatural in the last degree, and that they have neither the causes nor the consequences which such processes might be expected to have. (*b*) We notice that, since the result of the transformations is that the measured velocity of light will be the same for all systems in uniform rectilinear motion, we may just as well interpret the c of our formulæ as that relative velocity and drop all reference to the velocity of light with respect to the ether, which was its original meaning. (*c*) Next we notice that the form of the equations is such that the transformations from one system to another in uniform relative motion will be precisely the same as the transformations from a given system in motion to one at rest in the ether. We have merely to substitute everywhere in the formulæ the velocity of one system with respect to another for the velocity of a given system with respect to the ether. We can thus reinterpret the v of our formulæ provided we make a parallel reinterpretation of the x, y, z, and t. The v is now to stand for the velocity of one system as judged from a second, instead of the velocity of a single system with respect to the ether. The x, y, z, t are now to stand for the measures of length and time-lapses found by people on the second system, and the transformation equations give us the corresponding measures of length and time-lapse found by people on the first system. Thus absolute motion and the ether have dropped out altogether, and we are left with equations connecting the measurements of two observers who contemplate the same events. Had absolute motion been a *premiss* for proving these equations, of course we should have no right to reject the premiss and hold that we had proved the equations. But the real position is that the evidence for the equations is simply and solely that they account for the facts. If there be absolute motion it must have such physical effects as to lead to these relations between the measures found by two observers in uniform relative motion, for these relations are found to be necessary to explain the facts. But on the one hand, if there be no such thing the relations will still hold. And, on the other, the facts that absolute motion in any case cannot be observed, that it cannot be inferred from its effects because these are such as never to show themselves, and that the effects which we should have to ascribe to it accord very ill with the rest of our knowledge of nature, strongly encourage us to try to dispense with it altogether.

(iv) The last point in Mr. Ross's paper on which I want to comment is his remarks on simultaneity. His view is that we all know what simultaneity means, and that it always means the same thing. Einstein gives a *test* for it in certain difficult cases, this is never a *definition*, and as such it may be right or wrong, while a definition could only be convenient or inconvenient. I agree in part with Mr. Ross here; but I do not think that the point at issue is so important as he makes out. Certainly I do not primarily *mean* by simultaneity anything to do with light signals. And I do mean *something* by it. But (*a*) I may mean something by a word and not know all that I mean by it. I may think it stands for an absolute term whilst it really stands for a relative one. I talk, for instance, of *the* colour of a piece of gold and only learn afterwards that the colour is not a property of the gold by itself, but is relative to the physical situation in which the gold is placed. Similarly the fact that I mean something by simultaneity, and think that it is an absolute term, is quite compatible with its really being relative to a co-ordinate system. I think the colour of gold to be non-relational because I tacitly assume certain familiar conditions of illumination which are normally fulfilled. In the same way I may fail to notice that simultaneity has an essential reference to a co-ordinate system because I habitually assume a certain familiar system. It does not seem to me that we start life with a clear enough knowledge of what precisely we do mean by simultaneity to deny this off-hand. (*b*) Granted that we may mean something by a word without knowing with perfect definiteness what we do mean by it, and that this uncertainty allows the possibility of its standing for a relational term, I think Einstein is justified in assigning any meaning to it in doubtful cases which does not fall outside the range of variation of our meaning. He then naturally choses that particular meaning within this range which allows of a definite test and simplifies the statement of the laws of motion as much as possible. This is a general procedure in all sciences, and seems to me to be a perfectly legitimate one. We are not, as Mr. Ross thinks, claiming to give a perfectly arbitrary meaning to a previously meaningless noise; the noise has a restricted class of possible meanings, and we are choosing the most convenient and reasonable one within this range. (*c*) Lastly, if it be granted that relativity *to a co-ordinate system* falls within the range of possible meanings of simultaneity it follows that such relativity as is found need not be to *our minds* or our judgments, as Mr. Ross seems to think. And the fact that we are not dealing here with a relativity that merely refers to our minds and their judgments is proved by the fact that purely physical systems, such as spectroscopes or the moving liquid in Fresnel's experiment, themselves 'recognise' the relativity transformations.

I hold then that, even when we were confined to the special theory, we had good grounds for viewing it with great favour, and that we committed none of the fallacies of which Mr. Ross accuses us in our arguments for it. But I think the general theory is in an even stronger position than the special theory. Let me explain just what I mean by this. Mr. Ross says he will confine himself to the special theory, because, until one has convinced oneself of it, it is useless to worry about the more general one. This seems a reasonable attitude to take, and yet I believe that it unconsciously does an injustice to the theory of relativity. The general theory has in its favour all the arguments that favour the special one, and in addition, certain arguments which do not apply directly to the latter. These arguments consist in the extraordinary unification which it introduces into physics, and the way in which it removes that deplorable scandal which had always hung over the Newtonian laws of motion. The unification of course is that it binds together in a single whole Newton's two great achievements, the laws of motion and the law of gravitation, and connects the two previously independent notions of gravitational and inertial mass. The scandal was the necessity of a particular frame of reference for Newton's laws. If you took this to be absolute space you had laws which were presumably discovered by observation, and intended for application to the empirical world; and yet they were stated in terms of entities which could neither be observed nor inferred. If you took the frame to be the fixed stars you felt that they were placed in an utterly unintelligible position of importance in nature. It seemed obvious that there must be some way of stating the laws of nature on the one hand entirely in terms of relative motions and positions, and on the other independently of some one special group of material objects such as the fixed stars. To have done this is the great service of the general theory and the overwhelming argument in its favour, to my mind.

To sum up as regards the evidence for the theory:—It seems to me that the general theory starts by shocking us through its unfamiliarity, but that the more we reflect on it and on the mass of perfectly gratuitous and essentially unverifiable assumptions involved in all the alternatives the more certain do we become that it, or something extremely like it, must be true. If men like Prof. Eddington or Prof. Lindemann, who have been constantly and successfully using the methods and results of the theory, were the only people to make the above statement, we might be inclined to discount it somewhat as expressing 'the bias of happy exercise'. But the fact that I am a mere philosopher, quite incapable of their mathematical and physical achievements, may at least serve to allay such suspicions when the statement comes from me.

I will conclude with some remarks on Prof. Eddington's most interesting theory as to the function of the mind in physics. I will not call them criticisms, but rather appeals to Prof. Eddington to clear up some places where his meaning seems to be doubtful. (i) He often speaks as if lengths, time-lapses, etc., were relations between Nature and the observer. He thus seems to make Nature simply the almost unknown referent of these and other relations. Would it not be nearer the truth to draw a much sharper distinction between the 'observer' in the sense of his body and his scientific instruments and the 'observer' in the sense of the observing mind? In the former sense the observer is part of nature, in the latter he is not. And we ought then to say that lengths, time-lapses, etc., are relations between one part of nature and another part of nature, and it is these relations —or the natural complexes related by them—which the mind of the physicist contemplates, measures, and describes. (ii)

I am not sure that Prof. Eddington does not state his selection theory in needlessly subjective terms. To take a crude illustration: Suppose that a number of dots were scattered about at random on a plane. Any three of them would form a triangle and any four of them would constitute a tetragon. The triangles and the tetragons are equally real, and equally parts of nature, and you could completely analyse nature into either. But, on the other hand, only a small number of the points, if any, might be at the corners of squares. Now let us suppose that both triangles and tetragons have properties corresponding to 'conservation'. Then the whole of nature could be analysed exhaustively into entities obeying laws of conservation. If, on the other hand, only squares had the property corresponding to conservation, then, however much the mind might be interested in conservation, it could not give an exhaustive account of nature in terms of conservative entities, and it might be the case that *nothing* in nature obeyed such laws. Now the question I want to ask Prof. Eddington is this. Can *any* four-dimensional manifold be exhaustively analysed into complexes having the property of conservation, as *any* set of points in a plane can be exhaustively analysed into triangles or tetragons? If so, of course, the fact that nature everywhere obeys laws of conservation is in no way due to the mind but to the properties of four-dimensional manifolds as such. The result would be that such laws are necessary in all possible four-dimensional worlds. If not, then the important question would be: Does the actual four-dimensional world in which we live admit of exhaustive analysis into subordinate complexes of this special kind? The fact that the mind happens to like such complexes would of course throw no light on this question. The fact, if it be a fact, that it neglects all other complexes and yet seems able to describe and deal with nature satisfactorily would suggest that probably this condition is pretty nearly fulfilled. For, if there be other complexes and we be so constituted that we neglect them, it does not follow that they will neglect us. And we should therefore expect to get into serious practical and theoretical difficulties if the bent of our mind caused us to ignore types of complex which are real parts of nature and cannot be analysed into the complexes of the types that we do notice.

Scientists generally and rightly neglect the existence of minds while going about their lawful business. When at a later stage minds are forced on their attention they tend to be embarrassed. If they be stupid they deny minds altogether, which seems to be the last asylum of the dogmatic biologist. If, like Prof. Eddington, they have too much sense to do this, they are liable to go to the other extreme and, taking *omne ignotum pro magnifico,* to ascribe to minds powers and functions which they probably do not possess. I do not assert that Prof. Eddington has made this mistake but I have my suspicions.

IV. By F. A. Lindemann.

The difficulties of Mr. Ross seem to have been dealt with very completely by Mr. Broad so that I will confine myself to an attempt to restate the general case for Relativity in its simplest form in the hopes of providing a basis for discussion.

For this purpose I propose to examine the question why we study physics and attempt to establish the relation between physics and metaphysics. Then to state the impasse which led to the special theory of relativity, and finally to explain the essential difference between the general theory of relativity and the Newtonian point of view.

Mankind has evolved in the course of ages amidst hostile surroundings from the position of one of the minor fauna to that of unquestioned master. Whatever may be the reason for this we cannot therefore be surprised if man has many attributes of considerable survival value. There can be little doubt that one of the most valuable characteristics from the survival standpoint would be the faculty of forseeing future events, and it is not to be wondered at therefore that those races and men who have survived have an innate tendency, possibly strengthened by tradition, to seek to correlate events and establish relations between phenomena, which will enable them to predict subsequent happenings from observed data. The more easily such relations or laws are assimilated and applied, the simpler they will appear, hence the human mind, being what it is, always tends to accept the simplest laws consistent with observed facts.

Physical laws, and probably all laws, are based on observed phenomena. In order to establish a law a physicist observes a phenomenon under various conditions, formulates a hypothesis to account for the results, extrapolates new consequences of his hypothesis, tests these empirically, if necessary modifies his hypothesis, and so on. In this way, by a series of successive approximations he arrives at a rule or law or formula which is valid for all his experiments, which should be valid for all experiments carried out under conditions intermediate between those actually tried, and which is often valid when extrapolated for a considerable distance beyond