SPACE TIME AND GRAVITATION

Sir Arthur Stanley Eddington

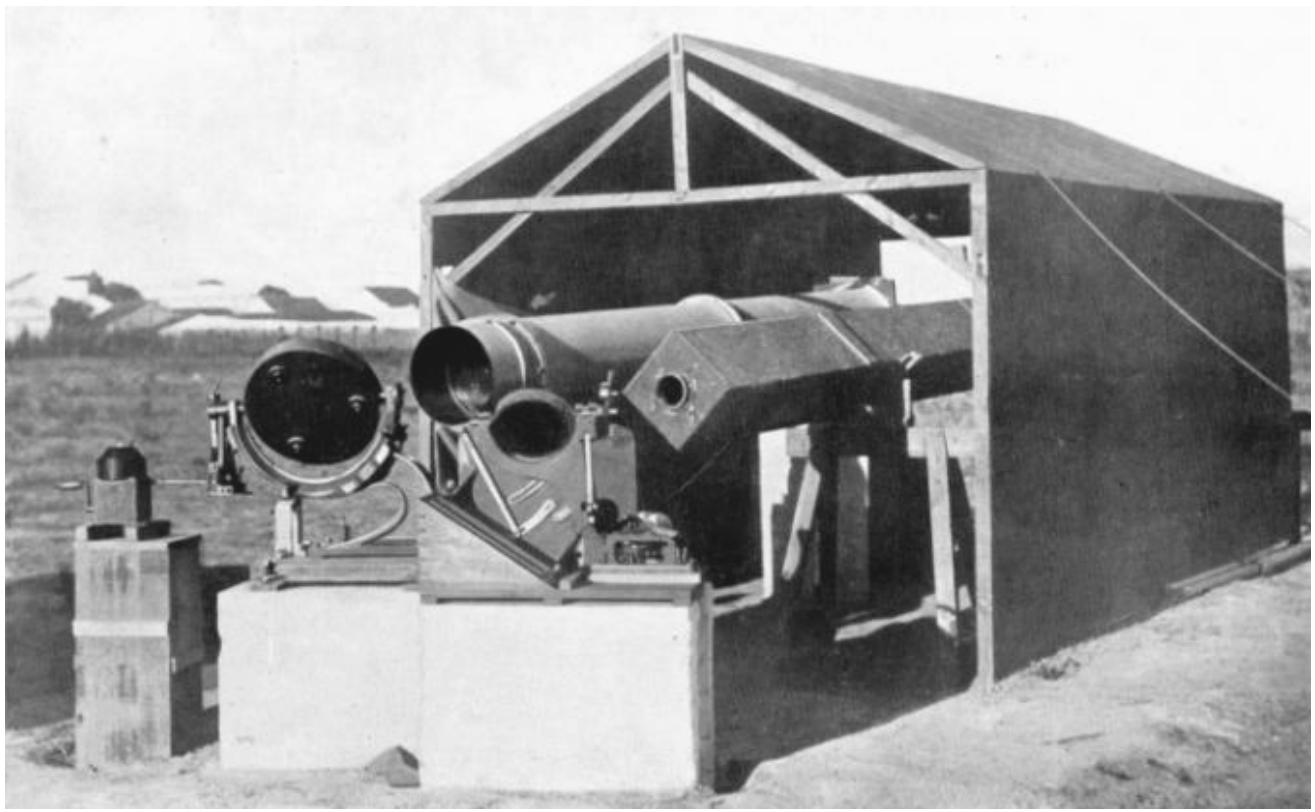
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C. Davidson *Frontispiece* *See page* 107

ECLIPSE INSTRUMENTS AT SOBRAL

SPACE TIME AND GRAVITATION

AN OUTLINE OF THE GENERAL RELATIVITY THEORY

BY

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Perhaps to move
His laughter at their quaint opinions wide
Hereafter, when they come to model heaven
And calculate the stars: how they will wield
The mighty frame: how build, unbuild, contrive
To save appearances.

Paradise Lost.

PREFACE

By his theory of relativity Albert Einstein has provoked a revolution of thought in physical science.

The achievement consists essentially in this:—Einstein has succeeded in separating far more completely than hitherto the share of the observer and the share of external nature in the things we see happen. The perception of an object by an observer depends on his own situation and circumstances; for example, distance will make it appear smaller and dimmer. We make allowance for this almost unconsciously in interpreting what we see. But it now appears that the allowance made for the *motion* of the observer has hitherto been too crude—a fact overlooked because in practice all observers share nearly the same motion, that of the earth. Physical space and time are found to be closely bound up with this motion of the observer; and only an amorphous combination of the two is left inherent in the external world. When space and time are relegated to their proper source—the observer—the world of nature which remains appears strangely unfamiliar; but it is in reality simplified, and the underlying unity of the principal phenomena is now clearly revealed. The deductions from this new outlook have, with one doubtful exception, been confirmed when tested by experiment.

It is my aim to give an account of this work without introducing anything very technical in the way of mathematics, physics, or philosophy. The new view of space and time, so opposed to our habits of thought, must in any case demand unusual mental exercise. The results appear strange; and the incongruity is not without a humorous side. For the first nine chapters the task is one of interpreting a clear-cut theory, accepted in all its essentials by a large and growing school of physicists—although perhaps not everyone would accept the author's views of its meaning. Chapters x and xi deal with very recent advances, with regard to which opinion is more fluid. As for the last chapter, containing the author's speculations on the meaning of nature, since it touches on the rudiments of a philosophical system, it is perhaps too sanguine to hope that it can

ever be other than controversial.

A non-mathematical presentation has necessary limitations; and the reader who wishes to learn how certain exact results follow from Einstein's, or even Newton's, law of gravitation is bound to seek the reasons in a mathematical treatise. But this limitation of range is perhaps less serious than the limitation of intrinsic truth. There is a relativity of truth, as there is a relativity of space.—

> "For IS and IS-NOT though *with* Rule and Line
> And UP-AND-DOWN *without*, I could define."

Alas! It is not so simple. We abstract from the phenomena that which is peculiar to the position and motion of the observer; but can we abstract that which is peculiar to the limited imagination of the human brain? We think we can, but only in the symbolism of mathematics. As the language of a poet rings with a truth that eludes the clumsy explanations of his commentators, so the geometry of relativity in its perfect harmony expresses a truth of form and type in nature, which my bowdlerised version misses.

But the mind is not content to leave scientific Truth in a dry husk of mathematical symbols, and demands that it shall be alloyed with familiar images. The mathematician, who handles x so lightly, may fairly be asked to state, not indeed the inscrutable meaning of x in nature, but the meaning which x conveys to *him*.

Although primarily designed for readers without technical knowledge of the subject, it is hoped that the book may also appeal to those who have gone into the subject more deeply. A few notes have been added in the Appendix mainly to bridge the gap between this and more mathematical treatises, and to indicate the points of contact between the argument in the text and the parallel analytical investigation.

It is impossible adequately to express my debt to contemporary literature and discussion. The writings of Einstein, Minkowski, Hilbert, Lorentz, Weyl, Robb, and others, have provided the groundwork; in the give and take of debate with friends and correspondents, the extensive ramifications have gradually appeared.

A. S. E.

1 *May*, 1920.

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PROLOGUE

WHAT IS GEOMETRY?

A conversation between—
An experimental Physicist.
A pure Mathematician.
A Relativist, who advocates the newer conceptions of time and space in physics.

Rel. There is a well-known proposition of Euclid which states that "Any two sides of a triangle are together greater than the third side." Can either of you tell me whether nowadays there is good reason to believe that this proposition is true?

Math. For my part, I am quite unable to say whether the proposition is true or not. I can deduce it by trustworthy reasoning from certain other propositions or axioms, which are supposed to be still more elementary. If these axioms are true, the proposition is true; if the axioms are not true, the proposition is not true universally. Whether the axioms are true or not I cannot say, and it is outside my province to consider.

Phys. But is it not claimed that the truth of these axioms is self-evident?

Math. They are by no means self-evident to me; and I think the claim has been generally abandoned.

Phys. Yet since on these axioms you have been able to found a logical and self-consistent system of geometry, is not this indirect evidence that they are true?

Math. No. Euclid's geometry is not the only self-consistent system of geometry. By choosing a different set of axioms I can, for example, arrive at Lobatchewsky's geometry, in which many of the propositions of Euclid are not in general true. From my point of view there is nothing to choose between these different geometries.

Rel. How is it then that Euclid's geometry is so much the most important system?

Math. I am scarcely prepared to admit that it is the most important. But for reasons which I do not profess to understand, my friend the Physicist is more interested in Euclidean geometry than in any other, and is continually setting us problems in it. Consequently we have tended to give an undue share of attention to the Euclidean system. There have, however, been great geometers like Riemann who have done something to restore a proper perspective.

Rel. (to Physicist). Why are you specially interested in Euclidean geometry? Do you believe it to be the true geometry?

Phys. Yes. Our experimental work proves it true.

Rel. How, for example, do you prove that any two sides of a triangle are together greater than the third side?

Phys. I can, of course, only prove it by taking a very large number of typical cases, and I am limited by the inevitable inaccuracies of experiment. My proofs are not so general or so perfect as those of the pure mathematician. But it is a recognised principle in physical science that it is permissible to generalise from a reasonably wide range of experiment; and this kind of proof satisfies me.

Rel. It will satisfy me also. I need only trouble you with a special case. Here is a triangle ABC; how will you prove that $AB + BC$ is greater than AC?

Phys. I shall take a scale and measure the three sides.

Rel. But we seem to be talking about different things. I was speaking of a proposition of geometry—properties of space, not of matter. Your experimental proof only shows how a material scale behaves when you turn it into different positions.

Phys. I might arrange to make the measures with an optical device.

Rel. That is worse and worse. Now you are speaking of properties of light.

Phys. I really cannot tell you anything about it, if you will not let me make measurements of any kind. Measurement is my only means of finding out about nature. I am not a metaphysicist.

Rel. Let us then agree that by *length* and *distance* you always mean a quantity arrived at by measurements with material or optical appliances. You have studied experimentally the laws obeyed by these *measured lengths*, and have found the geometry to which they conform. We will call this geometry "Natural Geometry"; and it evidently has much greater importance for you than any other of the systems which the brain of the mathematician has invented. But we must remember that its subject matter involves the behaviour of material scales—the properties of

matter. Its laws are just as much laws of physics as, for example, the laws of electromagnetism.

Phys. Do you mean to compare space to a kind of magnetic field? I scarcely understand.

Rel. You say that you cannot explore the world without some kind of apparatus. If you explore with a scale, you find out the natural geometry; if you explore with a magnetic needle, you find out the magnetic field. What we may call the field of extension, or space-field, is just as much a physical quality as the magnetic field. You can think of them both existing together in the aether, if you like. The laws of both must be determined by experiment. Of course, certain approximate laws of the space-field (Euclidean geometry) have been familiar to us from childhood; but we must get rid of the idea that there is anything inevitable about these laws, and that it would be impossible to find in other parts of the universe space-fields where these laws do not apply. As to how far space really resembles a magnetic field, I do not wish to dogmatise; my point is that they present themselves to experimental investigation in very much the same way.

Let us proceed to examine the laws of natural geometry. I have a tape-measure, and here is the triangle. $AB = 39\frac{1}{2}$ in., $BC = \frac{1}{8}$ in., $CA = 39\frac{7}{8}$ in. Why, your proposition does not hold!

Phys. You know very well what is wrong. You gave the tape-measure a big stretch when you measured AB.

Rel. Why shouldn't I?

Phys. Of course, a length must be measured with a rigid scale.

Rel. That is an important addition to our definition of length. But what is a rigid scale?

Phys. A scale which always keeps the same length.

Rel. But we have just defined length as the quantity arrived at by measures with a rigid scale; so you will want another rigid scale to test whether the first one changes length; and a third to test the second; and so *ad infinitum*. You remind me of the incident of the clock and time-gun in Egypt. The man in charge of the time-gun fired it by the clock; and the man in charge of the clock set it right by the time-gun. No, you must not define length by means of a rigid scale, and define a rigid scale by means of length.

Phys. I admit I am hazy about strict definitions. There is not time for everything; and there are so many interesting things to find out in physics, which take up my attention. Are you so sure that you are prepared with a logical definition of all the terms you use?

Rel. Heaven forbid! I am not naturally inclined to be rigorous about these things. Although I appreciate the value of the work of those who are digging at the foundations of science, my own interests are mainly in the upper structure. But sometimes, if we wish to add another storey, it is necessary to deepen the foundations. I have a definite object in trying to arrive at the exact meaning of length. A strange theory is floating round, to which you may feel initial objections; and you probably would not wish to let your views go by default. And after all, when you claim to determine lengths to eight significant figures, you must have a pretty definite standard of right and wrong measurements.

Phys. It is difficult to define what we mean by rigid; but in practice we can tell if a scale is likely to change length appreciably in different circumstances.

Rel. No. Do not bring in the idea of change of length in describing the apparatus for defining length. Obviously the adopted standard of length cannot change length, whatever it is made of. If a metre is defined as the length of a certain bar, that bar can never be anything but a metre long; and if we assert that this bar changes length, it is clear that we must have changed our minds as to the definition of length. You recognised that my tape-measure was a defective standard—that it was not rigid. That was not because it changed length, because, if it was the standard of length, it could not change length. It was lacking in some other quality.

You know an approximately rigid scale when you see one. What you are comparing it with is not some non-measurable ideal of length, but some attainable, or at least approachable, ideal of material constitution. Ordinary scales have defects—flexure, expansion with temperature, etc.—which can be reduced by suitable precautions; and the limit, to which you approach as you reduce them, is your rigid scale. You can define these defects without appealing to any extraneous definition of length; for example, if you have two rods of the same material whose extremities are just in contact with one another, and when one of them is heated the extremities no longer can be adjusted to coincide, then the material has a temperature-coefficient of expansion. Thus you can compare experimentally the temperature-coefficients of different metals and arrange them in diminishing sequence. In this sort of way you can specify the nature of your ideal rigid rod, before you introduce the term length.

Phys. No doubt that is the way it should be defined.

Rel. We must recognise then that all our knowledge of space rests on the behaviour of material measuring-scales free from certain definable defects of constitution.

Phys. I am not sure that I agree. Surely there is a sense in which the statement $AB = 2CD$ is true or false, even if we had no conception of a material measuring-rod. For instance, there is, so to speak, twice as much paper between A and B, as between C and D.

Rel. Provided the paper is uniform. But then, what does uniformity of the paper mean? That the amount in given length is constant. We come back at once to the need of defining length.

If you say instead that the amount of "space" between A and B is twice that between C and D, the same thing applies. You imagine the intervals filled with uniform space; but the uniformity simply means that the same amount of space corresponds to each inch of your rigid measuring-rod. You have arbitrarily used your rod to divide space into so-called equal lumps. It all comes back to the rigid rod.

I think you were right at first when you said that you could not find out anything without measurement; and measurement involves some specified material appliance.

Now you admit that your measures cannot go beyond a certain close approximation, and that you have not tried all possible conditions. Supposing that one corner of your triangle was in a very intense gravitational field—far stronger than any we have had experience of—I have good ground for believing that under those conditions you might find the sum of two sides of a triangle, as measured with a rigid rod, appreciably less than the third side. In that case would you be prepared to give up Euclidean geometry?

Phys. I think it would be risky to assume that the strong force of gravitation made no difference to the experiment.

Rel. On my supposition it makes an important difference.

Phys. I mean that we might have to make corrections to the measures, because the action of the strong force might possibly distort the measuring-rod.

Rel. In a rigid rod we have eliminated any special response to strain.

Phys. But this is rather different. The extension of the rod is determined by the positions taken up by the molecules under the forces to which they are subjected; and there might be a response to the gravitational force which all kinds of matter would share. This could scarcely be regarded as a defect; and our so-called rigid rod would not be free from it any more than any other kind of matter.

Rel. True; but what do you expect to obtain by correcting the measures? You correct measures, when they are untrue to standard. Thus you correct the readings of a hydrogen-thermometer to obtain the readings of a perfect

gas-thermometer, because the hydrogen molecules have finite size, and exert special attractions on one another, and you prefer to take as standard an ideal gas with infinitely small molecules. But in the present case, what is the standard you are aiming at when you propose to correct measures made with the rigid rod?

Phys. I see the difficulty. I have no knowledge of space apart from my measures, and I have no better standard than the rigid rod. So it is difficult to see what the corrected measures would mean. And yet it would seem to me more natural to suppose that the failure of the proposition was due to the measures going wrong rather than to an alteration in the character of space.

Rel. Is not that because you are still a bit of a metaphysicist? You keep some notion of a space which is superior to measurement, and are ready to throw over the measures rather than let this space be distorted. Even if there were reason for believing in such a space, what possible reason could there be for assuming it to be Euclidean? Your sole reason for believing space to be Euclidean is that hitherto your measures have made it appear so; if now measures of certain parts of space prefer non-Euclidean geometry, all reason for assuming Euclidean space disappears. Mathematically and conceptually Euclidean and non-Euclidean space are on the same footing; our preference for Euclidean space was based on measures, and must stand or fall by measures.

Phys. Let me put it this way. I believe that I am trying to measure something called length, which has an absolute meaning in nature, and is of importance in connection with the laws of nature. This length obeys Euclidean geometry. I believe my measures with a rigid rod determine it accurately when no disturbance like gravitation is present; but in a gravitational field it is not unreasonable to expect that the uncorrected measures may not give it exactly.

Rel. You have three hypotheses there:—(1) there is an absolute thing in nature corresponding to length, (2) the geometry of these absolute lengths is Euclidean, and (3) practical measures determine this length accurately when there is no gravitational force. I see no necessity for these hypotheses, and propose to do without them. *Hypotheses non fingo.* The second hypothesis seems to me particularly objectionable. You assume that this absolute thing in nature obeys the laws of Euclidean geometry. Surely it is contrary to scientific principles to lay down arbitrary laws for nature to obey; we must find out her laws by experiment. In this case the only experimental evidence is that measured lengths (which by your own admission are not necessarily the same as this absolute thing) sometimes

obey Euclidean geometry and sometimes do not. Again it would seem reasonable to doubt your third hypothesis beyond, say, the sixth decimal place; and that would play havoc with your more delicate measures. But where I fundamentally differ from you is the first hypothesis. Is there some absolute quantity in nature that we try to determine when we measure length? When we try to determine the number of molecules in a given piece of matter, we have to use indirect methods, and different methods may give systematically different results; but no one doubts that there is a definite number of molecules, so that there is some meaning in saying that certain methods are theoretically good and others inaccurate. Counting appears to be an absolute operation. But it seems to me that other physical measures are on a different footing. Any physical quantity, such as length, mass, force, etc., which is not a pure number, can only be defined as the result arrived at by conducting a physical experiment according to specified rules.

So I cannot conceive of any "length" in nature independent of a definition of the way of measuring length. And, if there is, we may disregard it in physics, because it is beyond the range of experiment. Of course, it is always possible that we may come across some quantity, not given directly by experiment, which plays a fundamental part in theory. If so, it will turn up in due course in our theoretical formulae. But it is no good assuming such a quantity, and laying down *a priori* laws for it to obey, on the off-chance of its proving useful.

Phys. Then you will not let me blame the measuring-rod when the proposition fails?

Rel. By all means put the responsibility on the measuring-rod. Natural geometry is the theory of the behaviour of material scales. Any proposition in natural geometry is an assertion as to the behaviour of rigid scales, which must accordingly take the blame or credit. But do not say that the rigid scale is wrong, because that implies a standard of right which does not exist.

Phys. The space which you are speaking of must be a sort of abstraction of the extensional relations of matter.

Rel. Exactly so. And when I ask you to believe that space can be non-Euclidean, or, in popular phrase, warped, I am not asking you for any violent effort of the imagination; I only mean that the extensional relations of matter obey somewhat modified laws. Whenever we investigate the properties of space experimentally, it is these extensional relations that we are finding. Therefore it seems logical to conclude that space as known to us must be the abstraction of these material relations, and not

something more transcendental. The reformed methods of teaching geometry in schools would be utterly condemned, and it would be misleading to set schoolboys to verify propositions of geometry by measurement, if the space they are supposed to be studying had not this meaning.

I suspect that you are doubtful whether this abstraction of extensional relations quite fulfils your general idea of space; and, as a necessity of thought, you require something beyond. I do not think I need disturb that impression, provided you realise that it is not the properties of this more transcendental thing we are speaking of when we describe geometry as Euclidean or non-Euclidean.

Math. The view has been widely held that space is neither physical nor metaphysical, but conventional. Here is a passage from Poincaré's *Science and Hypothesis*, which describes this alternative idea of space:

"If Lobatchewsky's geometry is true, the parallax of a very distant star will be finite. If Riemann's is true, it will be negative. These are the results which seem within the reach of experiment, and it is hoped that astronomical observations may enable us to decide between the two geometries. But what we call a straight line in astronomy is simply the path of a ray of light. If, therefore, we were to discover negative parallaxes, or to prove that all parallaxes are higher than a certain limit, we should have a choice between two conclusions: we could give up Euclidean geometry, or modify the laws of optics, and suppose that light is not rigorously propagated in a straight line. It is needless to add that everyone would look upon this solution as the more advantageous. Euclidean geometry, therefore, has nothing to fear from fresh experiments."

Rel. Poincaré's brilliant exposition is a great help in understanding the problem now confronting us. He brings out the interdependence between geometrical laws and physical laws, which we have to bear in mind continually. We can add on to one set of laws that which we subtract from the other set. I admit that space is conventional—for that matter, the meaning of every word in the language is conventional. Moreover, we have actually arrived at the parting of the ways imagined by Poincaré, though the crucial experiment is not precisely the one he mentions. But I deliberately adopt the alternative, which, he takes for granted, everyone would consider less advantageous. I call the space thus chosen *physical space*, and its geometry *natural geometry*, thus admitting that other conventional meanings of space and geometry are possible. If it were only a question of the meaning of space—a rather vague term—these other possibilities might have some advantages. But the meaning assigned to length and distance has to go along with the meaning assigned to space.

Now these are quantities which the physicist has been accustomed to measure with great accuracy; and they enter fundamentally into the whole of our experimental knowledge of the world. We have a knowledge of the so-called extent of the stellar universe, which, whatever it may amount to in terms of ultimate reality, is not a mere description of location in a conventional and arbitrary mathematical space. Are we to be robbed of the terms in which we are accustomed to describe that knowledge?

The law of Boyle states that the pressure of a gas is proportional to its density. It is found by experiment that this law is only approximately true. A certain mathematical simplicity would be gained by conventionally redefining *pressure* in such a way that Boyle's law would be rigorously obeyed. But it would be high-handed to appropriate the word pressure in this way, unless it had been ascertained that the physicist had no further use for it in its original meaning.

Phys. I have one other objection. Apart from measures, we have a general perception of space, and the space we perceive is at least approximately Euclidean.

Rel. Our perceptions are crude measures. It is true that our perception of space is very largely a matter of optical measures with the eyes. If in a strong gravitational field optical and mechanical measures diverged, we should have to make up our minds which was the preferable standard, and afterwards abide by it. So far as we can ascertain, however, they agree in all circumstances, and no such difficulty arises. So, if physical measures give us a non-Euclidean space, the space of perception will be non-Euclidean. If you were transplanted into an extremely intense gravitational field, you would directly perceive the non-Euclidean properties of space.

Phys. Non-Euclidean space seems contrary to reason.

Math. It is not contrary to reason, but contrary to common experience, which is a very different thing, since experience is very limited.

Phys. I cannot imagine myself perceiving non-Euclidean space!

Math. Look at the reflection of the room in a polished doorknob, and imagine yourself one of the actors in what you see going on there.

Rel. I have another point to raise. The distance between two points is to be the length measured with a rigid scale. Let us mark the two points by particles of matter, because we must somehow identify them by reference to material objects. For simplicity we shall suppose that the two particles have no relative motion, so that the distance—whatever it is—remains constant. Now you will probably agree that there is no such thing as absolute motion; consequently there is no standard condition of

the scale which we can call "at rest." We may measure with the scale moving in any way we choose, and if results for different motions disagree, there is no criterion for selecting the true one. Further, if the particles are sliding past the scale, it makes all the difference what instants we choose for making the two readings.

Phys. You can avoid that by defining distance as the measurement made with a scale which has the same velocity as the two points. Then they will always be in contact with two particular divisions of the scale.

Rel. A very sound definition; but unfortunately it does not agree with the meaning of distance in general use. When the relativist wishes to refer to this length, he calls it the *proper-length*; in non-relativity physics it does not seem to have been used at all. You see it is not convenient to send your apparatus hurling through the laboratory—after a pair of α particles, for example. And you could scarcely measure the length of a wave of light by this convention*. So the physicist refers his lengths to apparatus at rest on the earth; and the mathematician starts with the words "Choose unaccelerated rectangular axes Ox, Oy, Oz, ..." and assumes that the measuring-scales are at rest relatively to these axes. So when the term length is used some arbitrary standard motion of the measuring apparatus must always be implied.

Phys. Then if you have fixed your standard motion of the measuring-rod, there will be no ambiguity if you take the readings of both particles at the same moment.

Rel. What is the same moment at different places? The conception of simultaneity in different places is a difficult one. Is there a particular instant in the progress of time on another world, Arcturus, which is the same as the present instant on the Earth?

Phys. I think so, if there is any connecting link. We can observe an event, say a change of brightness, on Arcturus, and, allowing for the time taken by light to travel the distance, determine the corresponding instant on the earth.

Rel. But then you must know the speed of the earth through the aether. It may have shortened the light-time by going some way to meet the light coming from Arcturus.

Phys. Is not that a small matter?

Rel. At a very modest reckoning the motion of the earth in the interval might alter the light-time by several days. Actually, however, any speed of the earth through the aether up to the velocity of light is admissible, with-

*The proper-length of a light-wave is actually infinite.

out affecting anything observable. At least, nothing has been discovered which contradicts this. So the error may be months or years.

Phys. What you have shown is that we have not sufficient knowledge to determine in practice which are simultaneous events on the Earth and Arcturus. It does not follow that there is no definite simultaneity.

Rel. That is true, but it is at least possible that the reason why we are unable to determine simultaneity in practice (or, what comes to pretty much the same thing, our motion through the aether) in spite of many brilliant attempts, is that there is no such thing as absolute simultaneity of distant events. It is better therefore not to base our physics on this notion of absolute simultaneity, which may turn out not to exist, and is in any case out of reach at present.

But what all this comes to is that time as well as space is implied in all our measures. The fundamental measurement is not the interval between two points of space, but between two points of space associated with instants of time.

Our natural geometry is incomplete at present. We must supplement it by bringing in time as well as space. We shall need a perfect clock as well as a rigid scale for our measures. It may be difficult to choose an ideal standard clock; but whatever definition we decide on must be a physical definition. We must not dodge it by saying that a perfect clock is one which keeps perfect time. Perhaps the best theoretical clock would be a pulse of light travelling in vacuum to and fro between mirrors at the ends of a rigid scale. The instants of arrival at one end would define equal intervals of time.

Phys. I think your unit of time would change according to the motion of your "clock" through the aether.

Rel. Then you are comparing it with some notion of absolute time. I have no notion of time except as the result of measurement with some kind of clock. (Our immediate perception of the flight of time is presumably associated with molecular processes in the brain which play the part of a material clock.) If you know a better clock, let us adopt it; but, having once fixed on our ideal clock there can be no appeal from its judgments. You must remember too that if you wish to measure a second *at one place*, you must keep your clock fixed at what you consider to be one place; so its motion is defined. The necessity of defining the motion of the clock emphasises that one cannot consider time apart from space; there is one geometry comprising both.

Phys. Is it right to call this study *geometry*? Geometry deals with space alone.

Math. I have no objection. It is only necessary to consider time as a fourth dimension. Your complete natural geometry will be a geometry of four dimensions.

Phys. Have we then found the long-sought fourth dimension?

Math. It depends what kind of a fourth dimension you were seeking. Probably not in the sense you intend. For me it only means adding a fourth variable, t, to my three space-variables x, y, z. It is no concern of mine what these variables really represent. You give me a few fundamental laws that they satisfy, and I proceed to deduce other consequences that may be of interest to you. The four variables may for all I know be the pressure, density, temperature and entropy of a gas; that is of no importance to me. But you would not say that a gas had four dimensions because four mathematical variables were used to describe it. Your use of the term "dimensions" is probably more restricted than mine.

Phys. I know that it is often a help to represent pressure and volume as height and width on paper; and so geometry may have applications to the theory of gases. But is it not going rather far to say that geometry can deal directly with these things and is not necessarily concerned with lengths in space?

Math. No. Geometry is nowadays largely analytical, so that in form as well as in effect, it deals with variables of an unknown nature. It is true that I can often see results more easily by taking my x and y as lengths on a sheet of paper. Perhaps it would be helpful in seeing other results if I took them as pressure and density in a steam-engine; but a steam-engine is not so handy as a pencil. It is literally true that I do not want to know the significance of the variables x, y, z, t that I am discussing. That is lucky for the Relativist, because although he has defined carefully how they are to be measured, he has certainly not conveyed to me any notion of how I am to picture them, if my picture of absolute space is an illusion.

Phys. Yours is a strange subject. You told us at the beginning that you are not concerned as to whether your propositions are true, and now you tell us you do not even care to know what you are talking about.

Math. That is an excellent description of Pure Mathematics, which has already been given by an eminent mathematician*.

* "Pure mathematics consists entirely of such asseverations as that, if such and such a proposition is true of *anything*, then such and such a proposition is true of that thing. It is essential not to discuss whether the first proposition is really true, and not to mention what the anything is of which it is supposed to be true.... Thus mathematics may be defined as the subject in which we never know what we are talking about, nor whether what we are saying is true."

BERTRAND RUSSELL.