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EINSTEIN'S LAW OF GRAVITATION[1]

THE by-laws of our society make it one of the duties of its president to deliver an address before its members. This fact renders it necessary for the president to select a subject; and this year the selection is to a certain degree forced by the public press. When a daily newspaper considers Einstein's work on gravitation a topic of sufficiently general interest to devote to it valuable space and cable funds, surely here is justification for my selection of this as the subject of my presidential address.

Einstein's original memoirs upon gravitation appeared in the years 1916 to 1918; and there are two excellent papers in English expounding and explaining his method, one by Professor deSitter, of Leyden, and one by Professor Eddington, of Cambridge. While Einstein's work may be known to many of you either in its original form or in one of the two papers mentioned, I fear that the attention of most of us was first directed seriously to the matter by the articles in the newspapers to which I have referred. I confess that I was one of those who had postponed any serious study of the subject, until its immense importance was borne in upon me by the results of the recent eclipse expedition. I have all the enthusiasm of the discoverer of a new land, and feel compelled to describe to you what I have learned.

Albert Einstein, although now a resident of Berlin and holder of a research professorship of the Kaiser Wilhelm Institute, is legally a Swiss. He is forty-five years old and was for some time a professor in the Zurich Technical School, and later in the University of Prague. He is a man of liberal tendencies, and apparently one whom any of

[1] Presidential address delivered at the St. Louis meeting of the Physical Society, December 30, 1919.

us would be glad to welcome for personal reasons in our international meetings of the future. He protested against the famous manifesto of the German professors in 1914 and was one of the eager supporters of the German Republic when it arose from the wreck of the Empire.

But, in presenting the subject of Einstein's study of the law of gravitation, I must begin many years ago. In the treatment of Maxwell's equations of the electromagnetic field, several investigators realized the importance of deducing the form of the equations when applied to a system moving with a uniform velocity. One object of such an investigation would be to determine such a set of transformation formulæ as would leave the mathematical form of the equations unaltered. The necessary relations between the new space-coordinates, those applying to the moving system, and the original set were of course obvious; and elementary methods led to the deduction of a new variable which should replace the time coordinate. This step was taken by Lorentz and also, I believe, by Larmor and by Voigt. The mathematical deductions and applications in the hands of these men were extremely beautiful, and are probably well known to you all.

Lorentz' paper on this subject appeared in the Proceedings of the Amsterdam Academy in 1904. In the following year there was published in the *Annalen der Physik* a paper by Einstein, written without any knowledge of the work of Lorentz, in which he arrived at the same transformation equations as did the latter, but with an entirely different and fundamentally new interpretation. Einstein called attention in his paper to the lack of definiteness in the concepts of time and space, as ordinarily stated and used. He analyzed clearly the definitions and postulates which were necessary before one could speak with exactness of a length or of an interval of time. He disposed forever of the propriety of speaking of the "true" length of a rod or of the "true" duration of time, showing, in fact, that the numerical values which we attach to lengths or intervals of time depend upon the definitions and postulates which we adopt. The words "absolute" space or time intervals are devoid of meaning. As an illustration of what is meant Einstein discussed two possible ways of measuring the length of a rod when it is moving in the direction of its own length with a uniform velocity, that is, after having adopted a scale of length, two ways of assigning a number to the length of the rod concerned. One method is to imagine the observer moving with the rod, applying along its length the measuring scale, and reading off the positions of the ends of the rod. Another method would be to have two observers at rest on the body with reference to which the rod has the uniform velocity, so stationed along the line of motion of the rod that as the rod moves past them they can note simultaneously on a stationary measuring scale the positions of the two ends of the rod. Einstein showed that, accepting two postulates which need no defense at this time, the two methods of measurements would lead to different numerical values, and, further, that the divergence of the two results would increase as the velocity of the rod was increased. In assigning a number, therefore, to the length of a moving rod, one must make a choice of the method to be used in measuring it. Obviously the preferable method is to agree that the observer shall move with the rod, carrying his measuring instrument with him. This disposes of the problem of measuring space relations. The observed fact that, if we measure the length of the rod on different days, or when the rod is lying in different positions, we always obtain the same value offers no information concerning the "real" length of the rod. It may have changed, or it may not. It must always be remembered that measurement of the length of a rod is simply a process of comparison between it and an arbitrary standard, *e. g.*, a meter-rod or yard-stick. In regard to the problem of assigning numbers to intervals of time, it must be borne in mind that, strictly speaking, we do not "measure" such intervals, *i. e.*, that we do not select a unit interval of time and find how many times it is contained in the interval in question. (Similarly, we do not "measure" the pitch of a sound or the temperature of a room.) Our practical instruments for assigning numbers to time-intervals depend in the main upon our agreeing to believe that a pendulum swings in a perfectly uniform manner, each vibration taking the same time as the next one. Of course we can not *prove* that this is true, it is, strictly speaking, a definition of what we mean by equal intervals of time; and it is not a particularly good definition at that. Its limitations are sufficiently obvious. The best way to proceed is to consider the concept of uniform velocity, and then, using the idea of some entity having such a uniform velocity, to define equal intervals of time as such intervals as are required for the entity to traverse equal lengths. These last we have already defined. What is required in addition is to adopt some moving entity as giving our definition of uniform velocity. Considering our known universe it is self-evident that we should choose in our definition of uniform velocity the velocity of light, since this selection could be made by an observer anywhere in our universe. Having agreed then to illustrate by the words "uniform velocity" that of light, our definition of equal intervals of time is complete. This implies, of course, that there is no uncertainty on our part as to the fact that the velocity of light always has the same value at any one point in the universe to any observer, quite regardless of the source of light. In other words, the postulate that this is true underlies our definition. Following this method Einstein developed a system of measuring both space and time intervals. As a matter of fact his system is identically that which we use in daily life with reference to events here on the earth. He further showed that if a man were to measure the length of a rod, for instance, on the earth and then were able to carry the rod and his measuring apparatus to Mars, the sun, or to Arcturus he would obtain the same numerical value for the length in all places and at all times. This doesn't mean that any statement is implied as to whether the length of the rod has remained unchanged or not; such words do not have any meaning—remember that we can not speak of true length. It is thus clear that an observer living on the earth would have a definite system of units in terms of which to express space and time intervals, *i. e.*, he would have a definite system of space coordinates (x, y, z) and a definite time coordinate (t); and similarly an observer living on Mars would have his system of coordinates (x', y', z', t'). Provided that one observer has a definite uniform velocity with reference to the other, it is a comparatively simple matter to deduce the mathematical relations between the two sets of coordinates. When Einstein did this, he arrived at the same transformation formulæ as those used by Lorentz in his development of Maxwell's equations. The latter had shown that, using these formulæ, the form of the laws for all electromagnetic phenomena maintained the same form; so Einstein's method proves that using his system of measurement an observer, anywhere in the universe, would as the result of his own investigation of electromagnetic phenomena arrive at the same mathematical statement of them as any other observer, provided only that the relative velocity of the two observers was uniform.

Einstein discussed many other most important questions at this time; but it is not necessary to refer to them in connection with the present subject. So far as this is concerned, the next important step to note is that taken in the famous address of Minkowski, in 1908, on the subject of "Space and Time." It would be difficult to overstate the importance of the concepts advanced by Minkowski. They marked the begining of a new period in the philosophy of physics. I shall not attempt to explain his ideas in detail, but shall confine myself to a few general statements. His point of view and his line of development of the theme are absolutely different from those of Lorentz or of Einstein; but in the end he makes use of the same transformation formulæ. His great contribution consists in giving us a new geometrical picture of their meaning. It is scarcely fair to call Minkowski's development a picture; for to us a picture can never have more than three dimensions, our senses limit us; while his picture calls for perception of four dimensions. It is this fact that renders any even semi-popular discussion of Minkowski's work so impossible. We can all see that for us to describe any event a knowledge of four coordinates is necessary, three for the space specification and one for the time. A complete picture could be given then by a point in four dimensions. All four coordinates are necessary: we never observe an event except at a certain time, and we never observe an instant of time except with reference to space. Discussing the laws of electromagnetic phenomena, Minkowski showed how in a space of four dimensions, by a suitable definition of axes, the mathematical transformation of Lorentz and Einstein could be described by a rotation of the set of axes. We are all accustomed to a rotation of our ordinary cartesian set of axes describing the position of a point. We ordinarily choose our axes at any location on the earth as follows: one vertical, one east and west, one north and south. So if we move from any one laboratory to another, we change our axes; they are always orthogonal, but in moving from place to place there is a rotation. Similarly, Minkowski showed that if we choose four orthogonal axes at any point on the earth, according to his method, to represent a space-time point using the method of measuring space and time intervals as outlined by Einstein; and, if an observer on Arcturus used a similar set of axes and the method of measurement which he naturally would, the set of axes of the latter could be obtained from those of the observer on the earth by a pure rotation (and naturally a transfer of the origin). This is a beautiful geometrical result. To complete my statement of the method, I must add that instead of using as his fourth axis one along which numerical values of time are laid off, Minkowski defined his fourth coordinate as the product of time and the imaginary constant, the square root of minus one. This introduction of imaginary quantities might be expected, possibly, to introduce difficulties; but, in reality, it is the very essence of the simplicity of the geometrical description just given of the rotation of the sets of axes. It thus appears that different observers situated at different points in the universe would each have their own set of axes, all different, yet all connected by the fact that any one can be rotated so as to coincide with any other. This means that there is no one direction in the four dimensional space that corresponds to time for all observers. Just as with reference to the earth there is no direction which can be called vertical for all observers living on the earth. In the sense of an *absolute* meaning the words "up and down," "before and after," "sooner or later," are entirely meaningless.

This concept of Minkowski's may be made clearer, perhaps, by the following process of thought. If we take a section through our three dimensional space, we have a plane, *i. e.*, a two-dimensional space. Similarly, if a section is made through a four-dimensional space, one of three dimensions is obtained. Thus, for an observer on the earth a definite section of Minkowski's four dimensional space will give us our ordinary three-dimensional one; so that this section will, as it were, break up Minkowski's space into our space and give us our ordinary time. Similarly, a different section would have to be used for the observer on Arcturus; but by a suitable selection he would get his own familiar three-dimensional space and his own time. Thus the space defined by Minkowski is completely isotropic in reference to measured lengths and times, there is absolutely no difference between any two directions in an absolute sense; for any particular observer, of course, a particular section will cause the space to fall apart so as to suit his habits of measurement; any section, however, taken at random will do the same thing for some observer somewhere. From another point of view, that of Lorentz and Einstein, it is obvious that, since this four dimensional space is isotropic, the expression of the laws of electromagnetic phenomena take identical mathematical forms when expressed by any observer.

The question of course must be raised as to what can be said in regard to phenomena which so far as we know do not have an electromagnetic origin. In particular what can be done with respect to gravitational phenomena? Before, however, showing how this problem was attacked by Einstein; and the fact that the subject of my address is Einstein's work on gravitation shows that ultimately I shall explain this, I must emphasize another feature of Minkowski's geometry. To describe the space-time characteristics of any event a point, defined by its four coordinates, is sufficient; so, if one observes the life-history of any entity, *e. g.*, a particle of matter, a light-wave, etc., he observes a sequence of points in the space-time continuum; that is, the life-history of any entity is described fully by a line in this space. Such a line was called by Minkowski a "world-line." Further, from a different point of view, all of our observations of nature are in reality observations of coincidences, *e. g.*, if one reads a thermometer, what he does is to note the coincidence of the end of the column of mercury with a certain scale division on the thermometer tube. In other words, thinking of the world-line of the end of the mercury column and the world-line of the scale division, what we have observed was the intersection or crossing of these lines. In a similar manner any observation may be analyzed; and remembering that light rays, a point on the retina of the eye, etc., all have their world lines, it will be recognized that it is a perfectly accurate statement to say that every observation is the perception of the intersection of world-lines. Further, since all we know of a world-line is the result of observations, it is evident that we do not know a world-line as a continuous series of points, but simply as a series of discontinuous points, each point being where the particular world-line in question is crossed by another world-line.

It is clear, moreover, that for the description of a world-line we are not limited to the particular set of four orthogonal axes adopted by Minkowski. We can choose any set of four-dimensional axes we wish. It is further evident that the mathematical expression for the coincidence of two points is absolutely independent of our selection of reference axes. If we change our axes, we will change the coordinates of both points simultaneously, so that the question of axes ceases to be of interest. But our so-called laws of nature are nothing but descriptions in mathematical language of our observations; we observe only coincidences; a sequence of coincidences when put in mathematical terms takes a form which is independent of the selection of reference axes; therefore the mathematical expression of our laws of nature, of every character, must be such that their form does not change if we make a transformation of axes. This is a simple but far-reaching deduction.

There is a geometrical method of picturing the effect of a change of axes of reference, *i. e.*, of a mathematical transformation. To a man in a railway coach the path of a drop of water does not appear vertical, *i. e.*, it is not parallel to the edge of the window; still less so does it appear vertical to a man performing manœvres in an airplane. This means that whereas with reference to axes fixed to the earth the path of the drop is vertical; with reference to other axes, the path is not. Or, stating the conclusion in general language, changing the axes of reference (or effecting a mathematical transformation) in general changes the shape of any line. If one imagines the line forming a part of the space, it is evident that if the space is deformed by compression or expansion the shape of the line is changed, and if sufficient care is taken it is clearly possible, by deforming the space, to make the line take any shape desired, or better stated, any shape specified by the previous change of axes. It is thus possible to picture a mathematical transformation as a deformation of space. Thus I can draw a line on a sheet of paper or of rubber and by bending and stretching the sheet, I can make the line assume a great variety of shapes; each of these new shapes is a picture of a suitable transformation.

Now, consider world-lines in our four dimensional space. The complete record of all our knowledge is a series of sequences of intersections of such lines. By analogy I can draw in ordinary space a great number of intersecting lines on a sheet of rubber; I can then bend and deform the sheet to please myself; by so doing I do not introduce any new intersections nor do I alter in the least the sequence of intersections. So in the space of our world-lines, the space may be deformed in any imaginable manner without introducing any new intersections or changing the sequence of the existing intersections. It is this sequence which gives us the mathematical expression of our so-called experimental laws; a deformation of our space is equivalent mathematically to a transformation of axes, consequently we see why it is that the form of our laws must be the same when referred to any and all sets of axes, that is, must remain unaltered by any mathematical transformation.

Now, at last we come to gravitation. We can not imagine any world-line simpler than that of a particle of matter left to itself; we shall therefore call it a "straight" line. Our experience is that two particles of matter attract one another. Expressed in terms of world-lines, this means that, if the world-lines of two isolated particles come near each other, the lines, instead of being straight, will be deflected or bent in towards each other. The world-line of any one particle is therefore deformed; and we have just seen that a deformation is the equivalent of a mathematical transformation. In other words, for any one particle it is possible to replace the effect of a gravitational field at any instant by a mathematical transformation of axes. The statement that this is always possible for any particle at any instant is Einstein's famous "Principle of Equivalence."

Let us rest for a moment, while I call attention to a most interesting coincidence, not to be thought of as an intersection of world-lines. It is said that Newton's thoughts were directed to the observation of gravitational phenomena by an apple falling on his head; from this striking event he passed by natural steps to a consideration of the universality of gravitation. Einstein in describing his mental process in the evolution of his law of gravitation says that his attention was called to a new point of view by discussing his experiences with a man whose fall from a high building he had just witnessed. The man fortunately suffered no serious injuries and assured Einstein that in the course of his fall he had not been conscious in the least of any pull downward on his body. In mathematical language, with reference to axes moving with the man the force of gravity had disappeared. This is a case where by the transfer of the axes from the earth itself to the man, the force of the gravitational field is annulled. The converse change of axes from the falling man to a point on the earth could be considered as introducing the force of gravity into the equations of motion. Another illustration of the introduction into our equations of a force by means of a change of axes is furnished by the ordinary treatment of a body in uniform rotation about an axis. For instance, in the case of a so-called conical pendulum, that is, the motion of a bob suspended from a fixed point by a string, which is so set in motion that the bob describes a horizontal circle and the string therefore describes a circular cone, if we transfer our axes from the earth and have them rotate around the vertical line through the fixed point with the same angular velocity as the bob, it is necessary to introduce into our equations of motion a fictitious "force" called the centrifugal force. No one ever thinks of this force other than as a mathematical quantity introduced into the equations for the sake of simplicity of treatment; no physical meaning is attached to it. Why should there be to any other so-called "force," which, like centrifugal force, is independent of the nature of the matter? Again, here on the earth our sensation of weight is interpreted mathematically by combining expressions for centrifugal force and gravity; we have no distinct sensation for either separately. Why then is there any difference in the essence of the two? Why not consider them both as brought into our equations by the agency of mathematical transformations? This is Einstein's point of view.

Granting, then, the principle of equivalence, we can so choose axes at any point at any instant that the gravitational field will disappear; these axes are therefore of what Eddington calls the "Galilean" type, the simplest possible. Consider, that is, an observer in a box, or compartment, which is falling with the acceleration of the gravitational field at that point. He would not be conscious of the field. If there were a projectile fired off in this compartment, the observer would describe its path as being straight. In this space the infinitesimal interval between two space-time points would then be given by the formula

$$ds^2 = dx^2_1 + dx^2_2 + dx^2_3 + dx^2_4,$$

where ds is the interval and x_1, x_2, x_3, x_4, are coordinates. If we make a mathematical transformation, *i. e.*, use another set of axes, this interval would obviously take the form

$$ds^2 = g_{11}dx^2_1 + g_{22}dx^2_2 + g_{33}dx^2_3 + g_{44}dx^2_4 + 2g_{12}dx_1dx_2 + \text{etc.},$$

where x_1, x_2, x_3 and x_4 are now coordinates referring to the new axes. This relation involves ten coefficients, the coefficients defining the transformation.

But of course a certain dynamical value is also attached to the g's, because by the transfer of our axes from the Galilean type we have made a change which is equivalent to the introduction of a gravitational field; and the g's must specify the field. That is, these g's are the expressions of our experiences, and hence their values can not depend upon the use of any special axes; the values must be the same for all selections. In other words, whatever function of the coordinates any one g is for one set of axes, if other axes are chosen, this g must still be the same function of the new coordinates. There are ten g's defined by differential equations; so we have ten covariant equations. Einstein showed how these g's could be regarded as generalized potentials of the field. Our own experiments and observations upon gravitation have given us a certain knowledge concerning its potential; that is, we know a value for it which must be so near the truth that we can properly call it at least a first approximation. Or, stated differently, if Einstein succeeds in deducing the rigid value for the gravitational potential in any field, it must degenerate to the Newtonian value for the great majority of cases with which we have actual experience. Einstein's method, then, was to investigate the functions (or equations) which would satisfy the mathematical conditions just described. A transformation from the axes used by the observer in the following box may be made so as to introduce into the equations the gravitational field recognized by an observer on the earth near the box; but this, obviously, would not be the general gravitational field, because the field changes as one moves over the surface of the earth. A solution found, therefore, as just indicated, would not be the one sought for the general field; and another must be found which is less stringent than the former but reduces to it as a special case. He found himself at liberty to make a selection from among several possibilities, and for several reasons chose the simplest solution. He then tested this decision by seeing if his formulæ would degenerate to Newton's law for the limiting case of velocities small when compared with that of light, because this condition is satisfied in those cases to which Newton's law applies. His formulæ satisfied this test, and he therefore was able to announce a "law of gravitation," of which Newton's was a special form for a simple case.

To the ordinary scholar the difficulties surmounted by Einstein in his investigations appear stupendous. It is not improbable that the statement which he is alleged to have made to his editor, that only ten men in the world could understand his treatment of the subject, is true. I am fully prepared to believe it, and wish to add that I certainly am not one of the ten. But I can also say that, after a careful and serious study of his papers, I feel confident that there is nothing in them which I can not understand, given the time to become familiar with the special mathematical processes used. The more I work over Einstein's papers, the more impressed I am, not simply by his genius in viewing the problem, but also by his great technical skill.

Following the path outlined, Einstein, as just said, arrived at certain mathematical laws for a gravitational field, laws which reduced to Newton's form in most cases where observations are possible, but which led to different conclusions in a few cases, knowledge concerning which we might obtain by careful observations. I shall mention a few deductions from Einstein's formulæ.

1. If a heavy particle is put at the center of a circle, and, if the length of the circumference and the length of the diameter are measured, it will be found that their ratio is not π (3.14159). In other words the geometrical properties of space in such a gravitational field are not those discussed by Euclid; the space is, then, non-Euclidean. There is no way by which this deduction can be verified, the difference between the predicted ratio and π is too minute for us to hope to make our measurements with sufficient exactness to determine the difference.

2. All the lines in the solar spectrum should with reference to lines obtained by terrestrial sources be displaced slightly towards longer wave-lengths. The amount of displacement predicted for lines in the blue end of the spectrum is about one hundredth of an Angstrom unit, a quantity well within experimental limits. Unfortunately, as far as the testing of this prediction is concerned, there are several physical causes which are also operating to cause displacement of the spectrum-lines; and so at present a decision can not be rendered as to the verification. St. John and other workers at the Mount Wilson Observatory have the question under investigation.

3. According to Newton's law an isolated planet in its motion around a central sun would describe, period after period, the same elliptical orbit; whereas Einstein's laws lead to the prediction that the successive orbits traversed would not be identically the same. Each revolution would start the planet off on an orbit very approximately elliptical, but with the major axis of the ellipse rotated slightly in the plane of the orbit. When calculations were made for the various planets in our solar system, it was found that the only one which was of interest from the standpoint of verification of Einstein's formulæ was Mercury. It has been known for a long time that there was actually such a change as just described in the orbit of Mercury, amounting to 574″ of arc per century; and it has been shown that of this a rotation of 532″ was due to the direct action of other planets, thus leaving an unexplained rotation of 42″ per century. Einstein's formulæ predicted a rotation of 43″, a striking agreement.

4. In accordance with Einstein's formulæ a ray of light passing close to a heavy piece of matter, the sun, for instance, should experience a sensible deflection in towards the sun. This might be expected from "general" considerations. A light ray is, of course, an illustration of energy in motion; energy and mass are generally considered to be identical in the sense that an amount of energy E has the mass E/c^2 where c is the velocity of light; and consequently a ray of light might fall within the province of gravitation and the amount of deflection to be expected could be calculated by the ordinary formula for gravitation. Another point of view is to consider again the observer inside the compartment falling with the acceleration of the gravitational field. To him the path of a projectile and a ray of light would both appear straight; so that, if the projectile had a velocity equal to that of light, it and the light wave would travel side by side. To an observer outside the compartment, *e. g.*, to one on the earth, both would then appear to have the same deflection owing to the sun. But how much would the path of the projectile be bent? What would be the shape of its parabola? One might apply Newton's law; but, according to Einstein's formulæ, Newton's law should be used only for small velocities. In the case of a ray passing close to the sun it was decided that according to Einstein's formula there should be a deflection of 1″.75 whereas Newton's law of gravitation predicted half this amount. Careful plans were made by various astronomers. to investigate this question at the solar eclipse last May, and the result announced by Dyson, Eddington and Crommelin, the leaders of astronomy in England, was that there was a de-

flection of 1″.9. Of course the detection of such a minute deflection was an extraordinarily difficult matter, so many corrections had to be applied to the original observations; but the names of the men who record the conclusions are such as to inspire confidence. Certainly any effect of refraction seems to be excluded.

It is thus seen that the formulæ deduced by Einstein have been confirmed in a variety of ways and in a most brilliant manner. In connection with these formulæ one question must arise in the minds of everyone: by what process, where in the course of the mathematical development, does the idea of mass reveal itself? It was not in the equations at the beginning and yet here it is at the end. How does it appear? As a matter of fact it is first seen as a constant of integration in the discussion of the problem of the gravitational field due to a single particle; and the identity of this constant with mass is proved when one compares Einstein's formulæ with Newton's law which is simply its degenerated form. This mass, though, is the mass of which we become aware through our experiences with weight; and Einstein proceeded to prove that this quantity which entered as a constant of integration in his ideally simple problem also obeyed the laws of conservation of mass and conservation of momentum when he investigated the problems of two and more particles. Therefore Einstein deduced from his study of gravitational fields the well-known properties of matter which form the basis of theoretical mechanics. A further logical consequence of Einstein's development is to show that energy has mass, a concept with which every one nowadays is familiar.

The description of Einstein's method which I have given so far is simply the story of one success after another; and it is certainly fair to ask if we have at last reached finality in our investigation of nature, if we have attained to truth. Are there no outstanding difficulties? Is there no possibility of error? Certainly, not until all the predictions made from Einstein's formulæ have been investigated can much be said; and further, it must be seen whether any other lines of argument will lead to the same conclusions. But without waiting for all this there is at least one difficulty which is apparent at this time. We have discussed the laws of nature as independent in their form of reference axes, a concept which appeals strongly to our philosophy; yet it is not at all clear, at first sight, that we can be justified in our belief. We can not imagine any way by which we can become conscious of the translation of the earth in space; but by means of gyroscopes we can learn a great deal about its rotation on its axis. We could locate the positions of its two poles, and by watching a Foucault pendulum or a gyroscope we can obtain a number which we interpret as the angular velocity of rotation of axes fixed in the earth; angular velocity with reference to what? Where is the fundamental set of axes? This is a real difficulty. It can be surmounted in several ways. Einstein himself has outlined a method which in the end amounts to assuming the existence on the confines of space of vast quantities of matter, a proposition which is not attractive. deSitter has suggested a peculiar quality of the space to which we refer our space-time coordinates. The consequences of this are most interesting, but no decision can as yet be made as to the justification of the hypothesis. In any case we can say that the difficulty raised is not one that destroys the real value of Einstein's work.

In conclusion I wish to emphasize the fact, which should be obvious, that Einstein has not attempted any explanation of gravitation; he has been occupied with the deduction of its laws. These laws, together with those of electromagnetic phenomena, comprise our store of knowledge. There is not the slightest indication of a mechanism, meaning by that a picture in terms of our senses. In fact what we have learned has been to realize that our desire to use such mechanisms is futile.

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LEARNED SOCIETIES, OLD AND NEW[1]

IT would tax the younger men of science beyond the compass of their imagination, if

[1] President's address at the fourth meeting of the Annual Conference of Biological Chemists, held