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GEOMETRY AND PHYSICS[1]

Twenty years ago the abstract point of view in geometry was becoming a familiar one to mathematicians. The essential element in the movement of thought at that time seemed to be the freeing of geometry from all reference to physical reality. Geometry as studied by mathematicians must be a set of propositions arranged in a sequence of logical deduction, proceeding from a set of unproved propositions (the axioms, or postulates) which are stated in terms of undefined elements. If the undefined elements are points and lines, for instance, the mathematician does not inquire what is a point or line. All he cares to know about them is stated explicitly in the axioms.

This point of view made it possible for the first time in history to see geometry as a clear-cut whole. It was definitely separated from philosophy on the one side and from other branches of physics and mathematics on the other. The result was a great gain for clearness of thought in all these fields, a gain which has not been accompanied by any loss of mutual contact or support.

During the following years mathematicians have continued to develop the postulational or logistic method, so that by now it has demonstrated its value as a practical scheme of arrangement and exposition in the most diverse branches of mathematics. While doing this it has, of course, lost in freshness what it has gained in respectability. But during the same period a series of brilliant discoveries in physics has been making the abstract point of view a vital issue in that science also.

If we examine the classical branches of physics we shall find that the main elements of the abstract point of view have been im-

[1] Address as retiring vice-president and chairman of Section A—Mathematics, American Association for the Advancement of Science, Boston, December, 1922.

plicit in them for a long time. In fact, if we state with sufficient clearness in physical terms what we mean by undefined elements, unproved propositions, and so on, we are apt to find that a physicist classifies these ideas as truisms of little importance. I am inclined to think that he is justified in this attitude, so far as practical results are concerned, during the earlier and cruder stages of physical theory. But experience is showing that when the results of a more refined experimental technique force a reconsideration of fundamental assumptions, the technique of the study of these assumptions must undergo a corresponding refinement. Let us therefore take the risk of banality and glance at some of the branches of physics from the point of view of an axiomatist.

Lest any one should expect any closely reasoned body of doctrine to result from such a survey—and thereby be sadly disappointed—let me remind any non-mathematicians who may be present that when a mathematician lays down the elaborate tools by which he achieves precision in his own domain, he is unprepared and awkward in handling the ordinary tools of language. This is why mathematicians always disappoint the expectation that they will be precise and reasonable and clear-cut in their statements about everyday affairs, and why they are, in fact, more fallible than ordinary mortals. Therefore, please be satisfied in this case with some rather disconnected remarks.

We shall begin with elementary geometry, the oldest branch of physics. Having the mathematical, or abstract, science of geometry before us in its present highly developed form, we wish to apply it to the world of experiment. It consists of a sequence of statements arranged in a certain logical order but void of all physical meaning. In order to apply them to nature we identify the undefined terms (points, lines, etc.) as names of recognizable objects. The unproved propositions (axioms) are then given a meaning, and we can ask whether they are true statements. If they are true, then we expect that the theorems which are their logical consequences are also true and that the abstract geometry will take its place as a useful branch of physics.

This, I think, is a fair statement of the accepted point of view. But it is full of serious difficulties. The most obvious one is that it is impossible to identify anything in nature as a point or a line or a plane except by means of more or less gross approximations. The statement that a point has neither length, breadth nor thickness is a useful description in many of the applications of abstract geometry, but it is never strictly true of a physical point. The identification of any physical object as a point (or a hexagon or a sphere) takes place only with a certain margin of error. But if this error is imperceptible in verifying the postulates themselves, there may well be an accumulation of errors when the postulates have been used many times in the proof of a complicated theorem. Thus the postulates may appear true within the limits of error of a direct test, and yet some of the theorems may be perceptibly false. This makes it necessary to verify not merely the postulates, but also as many theorems as possible.

Here let me digress long enough to point out the bearing of this on the problems of teaching. The branch of physics which is called Elementary Geometry was long ago delivered into the hands of mathematicians for the purposes of instruction. But, while mathematicians are often quite competent in their knowledge of the abstract structure of the subject, they are rarely so in their grasp of its physical meaning. In recent years this defect has become glaringly apparent and the teachers of elementary geometry are beginning to cultivate the experimental technique of the subject. What I wish to say is that they should do this with a view not merely to making the concepts of geometry clear to their students, but also with a view to removing the legitimate doubts of its truth which students have a right to entertain.

The knowledge that the experimental verification of any theorem, however far removed from the axioms, is a real argument in favor of the validity of the whole science should strengthen the hands of those who want to make the teaching of geometry as concrete and physical as possible. Gauss and other experimenters who have taken pains to verify that the sum of the angles of a triangle is 180 degrees were not wasting their time; and neither is a teacher doing so who finds new tests for this and other theorems of geometry.

At the same time it will not be forgotten that the physical reality of geometry cannot be put in evidence with full clarity unless there is an abstract theory also. The faults in the traditional teaching of geometry, about which we hear so much to-day, are in a large measure due to the opinion that geometry is a system of *a priori* truth of such a nature that our belief in it cannot be influenced by experiment. A science resting on such a supernatural basis was fittingly taught by the method of dogmatic indoctrination.

The existence of a margin of error in the process of identifying concretely the abstract terms of geometry means that we never verify a very large number of cases of a very large number of theorems in a single group of experiments. In one experiment, and with one interpretation of the terms, we verify the theorem of Pythagoras, but it is in another experiment, and with another interpretation of the terms, that we verify the theorem of Desargues. Thus we can know the physical truth of geometry only, as it were, in patches. The unity of the science is in its abstract formulation.

This situation is not an unusual one in physical science, for theories are by no means unknown in which the postulates and many of the theorems are quite beyond the reach of experiment. Such theories have to be tested by verifying some of their consequences. I suppose that it is a very exceptional theory which can be fully tested by a single series of experiments.

There is, however, an experimental difficulty which is especially characteristic of elementary geometry. This is the bewildering multiplicity of concrete interpretations for the same abstract term. A point, for the purposes of instruction, is usually a spot on a blackboard. If you are trying to steer a course at sea you may fix your attention on two points which are a lighthouse and a red spar respectively. In this case you are making use of the proposition that a straight line is uniquely determined by two of its points. The same proposition can be verified by driving the nails *A*, *B*, and *C* into a wooden board and observing that if a stretched string which touches *A* and *B* also touches *C*, then a stretched string which touches *B* and *C* can be made to touch *A*. The same experiment can be repeated by sighting from one nail to the others or by the use of a straight edge or by firing a bullet from a gun.

I could continue enumerating these illustrations indefinitely, but the point I am making is sufficiently evident. There is no unique way of defining a point or a line for the purpose of experiment. Indeed, the great usefulness of elementary geometry is very largely due to the fact that there is such an extraordinary multiplicity of things which can profitably be regarded as points and lines.

In this multiplicity of interpretation of its fundamental terms, elementary geometry is in sharp contrast with the more recent and recondite branches of physics. Thus, for example, while the term electron may have more than one physical meaning, it is by no means such a protean object as a point or a triangle. The old way of accounting for this difference was to say that the electron is a substantial object, whereas the point is only an abstraction. This way of dismissing the question will not satisfy us to-day, for we believe that the electron and the point are both abstractions. Moreover, the difference which we are seeking to explain is one of degree rather than of kind.

What we are calling elementary geometry is, of course, not a single logical unit. It comprises first of all a group of theorems of analysis situs. These culminate in the theorem that the points are in one-to-one correspondence with the totality of ordered sets of three numbers, (x, y, z); in other words, that an analytic geometry is possible. In this part of geometry, the multiplicity of possible physical interpretations of the terms is at its highest pitch. Following this we have projective geometry, the general theory of straightness; affine geometry, the theory of parallels; and, finally, the metric geometry. Each one of these groups of theorems is logically distinguished from its predecessor by the appearance of new relations which are brought in either by means of new axioms and undefined terms or by means of definitions which limit attention to a restricted class among the totality of possible geometrical objects. At each stage the freedom of physical interpretation is restricted until, at the final stage, it is necessary to be able to specify the physical significance of a measuring stick and of a rectangular cartesian coordinate system.

Next after geometry, according to the classical way of looking at things, we can take up either kinematics or statics or the geometry of masses. Let us choose the first of these three alternatives.

For kinematics we must have a theory of time. This is very simple; the undefined terms are "instant" and "before" or "after," and we use as postulates one of the sets of postulates for the linear continuum. The main theorem is that there is a one-to-one continuous correspondence between the instants of time and the numbers of the real number system. This makes possible the subdivision of time into equal intervals, and the measurement of time. While the abstract theory is very simple at this stage, the physical applications involve all the technique of clocks and other time devices.

For kinematics we must also have the concept of substance, something which can move and which can have a duration in time. The postulates for substance will state its existence, as we say, in space and in time. They may be phrased somewhat as follows:

1. Given any substance and any instant of time, there exists a unique set of points called the *position* of the substance at the given instant of time.

A substance whose position at any time is a single point is called a *particle*.

2. The position of any particle at any time is a single point. This position is a continuous function of the time.

3. For every point P of the position of a substance S at any time there is a particle whose position is P. The position of S at any other time t' is the set of points which are the positions of these particles at the time t'. These particles are called the particles of the substance S.

4. No two particles of the same substance can have the same position at the same time.

5. Let (x_o, y_o, z_o) be the coordinates in a cartesian coordinate system of the position of any particle of a substance S at a time t_o and let (x, y, z) be the coordinates of the same particle at a time t, then there exist three analytic functions f_1, f_2, f_3 such that

$$\begin{aligned} x &= f_1\ (x_o, y_o, z_o) \\ y &= f_2\ (x_o, y_o, z_o) \\ z &= f_3\ (x_o, y_o, z_o) \end{aligned}$$

for all the particles of S.

The first four of these postulates correspond to our most general intuitions about substance, and the fifth is intended as a basis for analytic operations. While I have studied out some of their consequences, I have not made anything like a full investigation and should not be surprised to find that they contain both omissions and redundancies. I give them here largely to emphasize the fact that very little work has yet been done in this direction.

Before we have the actual structure of the classical kinematics we must limit our attention not merely to the consequences of these axioms of time and substance, but also to a group of theorems determined by certain definitions. The most important of these is the definition of uniform motion of translation. A substance is in uniform motion of translation if with respect to a definite cartesian coordinate system and a definite time variable, we have for every particle of the substance

$$\frac{d^2x}{dt^2} = \frac{d^2y}{dt^2} = \frac{d^2z}{dt^2} = 0.$$

This simple way of stating the definition can be replaced by the apparently more complicated statement:

If a substance S is at rest there is a cartesian coordinate system and a system of time measurement such that the coordinates of each particle of S satisfy the conditions

$$\begin{aligned} x &= \text{constant} \\ y &= \text{constant} \\ z &= \text{constant} \\ t &= \text{arbitrary.} \end{aligned}$$

If a substance S' is in uniform motion of translation there is another coordinate system such that each particle of S' is denoted by

$$\begin{aligned} \bar{x} &= \text{constant} \\ \bar{y} &= \text{constant} \\ \bar{z} &= \text{constant} \\ \bar{t} &= \text{arbitrary.} \end{aligned}$$

and the relation between the two coordinate systems is given by equations

$$
\begin{aligned}
(1)\quad \bar{t} &= a\ \ t + b \\
\bar{x} &= a_{11}x + a_{12}y + a_{13}z + b_1 t + c_1 \\
\bar{y} &= a_{21}x + a_{22}y + a_{23}z + b_2 t + c_2 \\
\bar{z} &= a_{31}x + a_{32}y + a_{33}z + b_3 t + c_3
\end{aligned}
$$

in which the coefficients are constants such that

$$d\bar{x}^2 + d\bar{y}^2 + d\bar{z}^2 = dx^2 + dy^2 + dz^2.$$

Before the physical applications can be made in detail, corresponding definitions must be made of uniform motion of rotation and of other special types of motion. But we need not go into this question here.

The physical interpretation of the abstract theory is much more definite and restricted at the present stage than it was at that of geometry. The table before us, the floor on which we stand, we ourselves, the whole earth, are all substances moving together through space with a high velocity. Any description of the whole or any part of this aggregation of substances is a concrete application of kinematics and gives rise to an experimental test of it.

We now see that the multiplicity of concrete interpretations of geometry was due in part (though not wholly) to the fact that geometry is used to describe an instantaneous cross section of the substantial universe. At this stage also we have to meet the difficulties due to the fact that the motion of any substance can only be detected physically as the motion of the one substance relative to the other substances.

The abstract theory which we have described provides for absolute motion, *i. e.*, a substance is in motion if the set of points which we call its position is not the same at all instants of time. But it is also true that the theorems of the universe of substance will be unchanged in meaning if we replace the abstract time and space which underlie the theory of substance by a new time and space related to the old ones by formulas of the same form as (1). It is for this reason that an absolutely definite statement of what we mean physically by particular substances does not carry with it a unique determination of what we mean by particular instants of time or points of space.

The fact, somewhat obscurely understood, that it is possible to make these transformations of the space and time underlying kinematics without altering the kinematics itself has often been taken as an argument against the classical theory of space and time. I do not think it can be accepted as a valid objection, however. What it really does prove is that when the classical theories of space and time are combined in the theory of substance, the result is more complicated than a simple-minded person would expect.

This complication of the abstract theory shows itself on the physical side when we ask how we shall know what are the simultaneous positions of two distant substances. The discussions of the theory of relativity have shown that the most natural physical method of answering this question corresponds with sufficient accuracy to the classical kinematics when attention is limited to terrestrial objects of not too fine-grained a character. On an astronomical scale, however, the determination of simultaneity fits in much better with the type of kinematics known as the special theory of relativity.

This theory we can regard as proceeding from exactly the same axioms of space, time and substance as those we have proposed for the classical kinematics. But it makes use of a different definition of uniform motion: A substance S' is in uniform motion of translation if, and only if, there is a coordinate system such that each particle of S' is denoted by $\bar{x}$ = constant, $\bar{y}$ = constant, $\bar{z}$ = constant, $\bar{t}$ = arbitrary, and the relation between this coordinate system and the (x, y, z, t) system is given by any set of linear equations with constant coefficient such that

$$d\bar{x}^2 + d\bar{y}^2 + d\bar{z}^2 - c^2 d\bar{t}^2 = dx^2 + dy^2 + dz^2 - c^2 dt^2$$

where c is a constant.

The relation of the relativity kinematics to its underlying space and time is quite analogous to that of the classical kinematics to its underlying space and time. Analogous transformations of the underlying space and time (the Lorentz group) are possible without changing the kinematics. But in the relativity theory these transformations are put in evidence in connection with the simplest problems, whereas the classical kinematics can be treated in such a way as to mask them.

This is not the place to go further with an exposition either of the classical or of the relativity kinematics. I wish only to remark that either of them can be based on an underlying theory of space and time as well as upon the concept of a four dimensional space-time. There is no essential difference of logical simplicity between the two types of kinematics. The important difference is that the relativity definition of simultaneity is more nearly in accordance with the physical means in actual use by astronomers for determining when two events are simultaneous than is the classical definition. Therefore in those cases where the scale of operations is large enough, or the scheme of measurement fine enough, the relativity kinematics will be used. In other cases the classical kinematics will be retained as a sufficiently accurate approximation to the other.

In comparing the theorems of kinematics with experience there is much less overlapping than in geometry. For a physical object has to be identified as a substance in the kinematic sense not merely once but for all values of the time. Nevertheless, a great multiplicity of interpretation still persists, which will be further reduced when we come into the domain of mechanics.

Before we arrive at a full-fledged mechanics we must introduce the concept of mass. This may be done in two ways according as we are developing a mechanics of discrete particles or a mechanics of continuous substances. In the first case we have merely to postulate a number associated with each particle and in the second case to lay down a somewhat more extensive set of postulates from which the concept of density may be derived. These postulates will serve for example as a basis for the differential equations of continuity.

Finally the postulates must be introduced which determine how we shall use the terms force and cause. I shall not now try to set forth my ideas as to how these postulates should be formulated, for I have already gone more into detail than is desirable in a talk of this kind in expounding the idea of substance. It is enough to remark that there is an open field here for a valuable postulational investigation.

Suppose now that we have before us the complete logical structure which is built on these postulates and consider a particular mechanical problem, as, for example, the problem of two bodies. For the sake of this problem we add to the general postulates of mechanics the postulate that the substance to be considered consists of two particles of given masses moving under a particular law of force. The differential equations now become perfectly definite and numerical results can be worked out.

In order to have a physical application we may let the two particles be the two components of a double star. In this case the astronomical data have a low percentage precision and the theory gets along within a wide margin of probable error. Again, we may let the two particles be the sun and Jupiter. In this case the precision of the astronomical data is high. The theory serves as a first approximation. But it is soon seen that the sun and Jupiter must be regarded as parts of a more complicated mechanical system—and so, in order to pursue the astronomical problem further, we pass on to another abstract theory. In general a whole series of more and more complicated abstract theories will be applied to the same astronomical problem. But the further we go in this direction the more precise becomes the physical significance of each term and the further we are from that multiple interpretation of terms which we noted in elementary geometry.

The same sort of remarks can be made about any other mechanical problem—for example, the problem to find the position of equilibrium of a door hanging freely in a wind of given velocity on hinges whose axes make a given angle with the vertical. It is necessary to supplement the general assumptions of mechanics by additional ones which specify the particular problem. And when we have done so, the application to nature is very definite.

This state of affairs is in part due to the fact that the postulates for mechanics do not form a categorical set, and can not form a categorical set until the substance and the forces are specified in a particular way. In the narrow sense of the words, mechanics is not a mathematical science, but is the group of theorems common to a collection of sciences. Each particular problem involves certain axioms in addition to those of mechanics in general.

It should perhaps be emphasized once more that nothing of this sort is true of elementary geometry. Its axioms form a categorical set. The relations among the points of space are completely determinate, and are unaffected by any of the additional assumptions required for a problem of mechanics. A typical problem of geometry is to determine the truth or falsity of a given theorem. Problems of this sort arise in mechanics, but a typical problem of mechanics requires the construction of a new theory. This is why mechanics is so interesting and so difficult.

The abstract treatments of a great many branches of physics fall within the province of mechanics as it is here understood, but there are a number of others, such as the theory of heat and of electromagnetic phenomena, which are not thus included and which I can not touch on now. But in their classical forms they have all had an underlying "space in which"—described by the Euclidean geometry—and an underlying time continuum. Situated in this space and time there are particles of substance (matter, electricity) all moving about under the influence of forces. The more abstract concepts, such as energy and entropy, have been defined in terms of these more easily comprehended undefined terms. But it is inevitable that the tendency to regard these new concepts as more and more fundamental should lead to the replacement of the old undefined terms by new ones which seem more adequate even if they are more perplexing.

An early illustration of the tendency to formulate problems in terms far removed from the obvious ones is to be found in the general equations of dynamics. Here we find as the coordinates of a dynamical system any set of parameters $q^1, q^2, \ldots q^n$ adequate to define it. In a very general class of cases these parameters are regarded as coordinates of a point in a Riemann space whose linear element is determined by the expression for kinetic energy. The geodesics of this space give the paths of the representative point of a mechanical system in the absence of impressed forces and the Lagrange equations express the divergences from these paths brought about by the impressed forces.

In this general mechanics the Riemann space figures as a mathematical device, in which it is highly suggestive to regard the geodesics as analogues of the straight lines in Euclidean space. For one of the most fruitful ways of looking at a straight line physically is to regard it as the path of a particle which has been given an initial velocity and is far enough away from other objects to be but little influenced by them. The motion of a particle in a general case is the result of a balancing of the tendency to follow a straight line with uniform velocity against the forces causing divergences from this norm. With the use of generalized coordinates this situation is reproduced in a generalized form in a Riemann space.

It is an important step beyond this to use the Riemann geometry from the very beginning as Einstein does in his theory of gravitation. To describe the motion of a particle which is left to itself, this theory does not presuppose an underlying space and time whose properties are completely determined in advance. Instead, it assumes that any event in the history of the particle can be identified in terms of four coordinates, x^1, x^2, x^3, x^4, and the totality of these sets of coordinates is assumed to constitute a Riemann space in which there is a linear element,

$$(2) \qquad ds^2 = \sum_{i,j} g_{ij}\, dx^i\, dx^j$$

The relations of these geodesics among themselves are different in the neighborhood of material bodies from what they are in regions where there are no material bodies. In regions of the latter type these relations are approximately the same as those among the geodesics of the space-time manifold of the special relativity.

The relations among the geodesics are determined by the functions g_{ij}. Therefore, if we wish to describe two different distributions of matter we require two different Riemann spaces. We can not, as in the old theories, simply put a new filler in the old container space. The allowable choices of the functions g_{ij} are restricted by requiring them to satisfy a set of partial differential equations which is chosen, in the main, on the basis of its simplicity. In one special case, the one-body problem, it is possible to solve these differential equations and obtain a set of g's and a Riemann space which correspond to a single mass of matter.

Let the numerical constant be chosen so that this mass represents the sun. The planets are particles in comparison with the sun. Therefore, the orbits should be determined by the geodesics of this Riemann space. This determines for each planet a one-parameter continuous family of values of the quantities x^1, x^2, x^3, x^4, which can be compared with values obtained by observation.

Contrast this with the treatment of the same problem by the Newtonian method. Here we have a central attracting mass in a Euclidean space and a system of ellipses as the particle paths. If we go to the space-time manifold we get a system of spirals which have the ellipses as projections. Thus, in this case as in the other, we have a four-dimensional manifold and in it a system of curves. Although the mathematical theories from which the two systems of curves were derived are radically different, yet with proper restrictions the one system of curves can be regarded as a good approximation to the other, and we can pass from the one to the other by means of a differential correction.

When it comes to the comparison of these two systems of curves with nature, we have to recognize that the astronomers work out what they assign as the coordinates of the heavenly bodies by elaborate and indirect observation and calculation. The parameters which they thus determine have to be compared in a somewhat arbitrary way with those which enter into the abstract theories. It was found that in the case of the planet Mercury this fit could not be made with complete exactness for the classical theory, but that when the differential correction was applied which corresponds to the transition to the new theory, the fit became as good as could be expected.

The differential equations of the geodesics can be written in the form

$$(3) \qquad \frac{d^2x^i}{ds^2} + \sum_{\alpha, \beta} \Gamma^i_{\alpha\beta} \frac{dx^\alpha}{ds} \frac{dx^\beta}{ds} = 0.$$

Among the curves which satisfy these equations are not only the geodesics which we have been talking about, but also a family of curves for which $ds = 0$. These curves differ but little from those which represent the motion of light in the older theories. Let us therefore assume that the new curves represent the motion of light. If we do, the differential change from the old to the new curves gives the magnitude of the effect on light of a gravitational field. This effect was predicted by Einstein and the prediction was brilliantly fulfilled.

Both of these successes of the new theory have to do with properties of the differential equations (3) and only indirectly with the functions g_{ij} of (2). The functions $\Gamma^i_{\alpha\beta}$ which appear in (3) are expressed in terms of the g's in (2) but do not depend on the physical meaning of the g's. It is otherwise with the third important prediction of Einstein, that having to do with a shift of the spectral lines. This depends on an explicit physical interpretation of the g's. Its experimental verification is very complicated and whether the outcome will be favorable or unfavorable is doubtful. In the meantime it is interesting to notice that this prediction is logically separable from the other two.

The general situation which arises here is worthy of a great deal of study. A set of differential equations (3) in which the functions Γ are arbitrary, determines a system of curves having the property that any two points in a sufficiently small region are joined by one and only one of the curves. We are thus in the presence of a broad generalization of the Riemann geometry which it has been proposed, in a paper by Professor Eisenhart and myself, to call the geometry of paths. The intuitive idea suggested by this name is that we are dealing not with the empty void of analysis situs, but with a manifold in which we find our way around by means of the paths. It may also serve to remind us that we have a generalization of an inertial field, for the characteristic of a field of inertia is that through every point and in every direction, there is a path which may be taken by a free particle.

A system of paths is also that which takes the place of the ether which has played so varied a rôle in the physics of the last hundred years. It is the paths in the space-time manifold which are the bearers of the energies and stresses which lead to the properties of light and electromagnetism.

From the work of Weyl, who has made the most important contributions to the geometry of paths, it follows that there is not only an affine but also a projective geometry of paths.

The affine geometry consists of those theorems which deal with the concept of infinitesimal parallelism defined by means of the functions Γ. The projective geometry deals with those properties of the paths that are so general as to be independent of any particular definition of parallelism. That there is such a projective geometry follows from the fact that more than one set of functions Γ can be found to define the same set of paths.

Incidentally, it is an interesting comment on the progress of mathematics that the classification of theorems into projective, affine and metric is not here based on the group concept. For the group of a space of paths is in general the identity.

These remarks are, of course, very general and can have little meaning to one who has not given some consideration to the analytic details. But it is essential that they should be made in the sort of a survey I am attempting, because the geometry of paths can be regarded as a generalization both of the earliest part of elementary geometry and of some of the most refined of physical theories. The study of the projective, the affine and the metric geometry of paths ought to result in a comprehensive idea of what types of physical theory it is possible to construct along the lines which have been successful in the past.

The development of physical theories in the recent past has been characterized by a progressively greater and greater use of different types of non-Euclidean geometry. In all cases, however, the underlying analysis situs properties are the same. Whether we are dealing with three, four or n dimensions, we are dealing with a cell, *i. e.,* a portion of a manifold which can be mapped continuously on the interior of a sphere in a Euclidean space of the right number of dimensions.

The obvious question suggests itself, whether it will not be necessary in the future to reconsider this assumption also. The question whether space should be assumed to be continuous has indeed already been raised in a shadowy form by Riemann and others. But even if space be assumed continuous it does not follow that every point in it can be enclosed by a region like the interior of a sphere. Many examples to the contrary are known to students of analysis situs. One such example is a singular manifold defined by "polar coordinates" r, θ, φ, in which $(0, \theta, \varphi)$ represent a single point, the origin, no matter what θ and φ are and in which (r, θ, φ) represents the same point as $(r, \theta + 2n\pi, \varphi + 2n\pi)$ in case $0 < r < 1$. The locus $r =$ constant is an anchor ring and encloses the origin. You can readily convince yourself that there is no sphere enclosing the origin and hence that the origin is a point which has no neighborhood of the sort that exists in the Euclidean or Riemann geometries.

A corresponding line singularity can easily be defined in a four dimensional manifold and may well turn out to be a promising candidate for consideration as the world line of an ultimate particle of some sort. It has an extraordinary degree of indestructibility, and the paths that can be drawn in its neighborhood fall into discrete classes in a most suggestive way. It is not at all impossible that this or some other type of analysis situs singularity will enable us to maintain the continuity of space and yet take account of the discontinuous phenomena that are being observed.

Suppose, however, that it should turn out to be necessary to assume some sort of a discontinuous space. This would have at least one effect that would be pleasing to some of us. It would upset a great many of the cosmological speculations of the present era. In particular it would do away with that depressing view which is so often presented to us of the future state of the universe, a dead, cold world at a uniform level of energy and entropy. It would also tend to prevent our following a light ray too far off toward infinity—or all the way round a closed path—whichever the case may be supposed to be.

In order to have a definite idea of a discontinuous space-time, let us consider a modular case, *i. e.,* a space-time defined by four coordinates (x, y, z, t) which are whole numbers reduced modulo P, and let us suppose that P is an enormously large prime. The number system, modulo P, is one in which every rational operation can be carried out. Let us set a portion of these numbers into correspondence with the ordinary number system of analysis in the following manner: Let k be a fixed number which is small in comparison with P and yet large in comparison with the numbers used in physics and astronomy; and let every rational fraction p/q in the modular number system for which p and q are less than k, correspond to the number p/q of the ordinary number system.

This correspondence between the number systems determines an analogous partial correspondence between the modular space-time and the ordinary space-time, by the expedient of letting the event (x, y, z, t) of the modular space-time correspond to the event (x, y, z, t) of the ordinary space-time, provided that the coordinates are all numbers which correspond.

If we wish to distinguish physically between the two space-times it is obvious that we must succeed in identifying an event in the one which does not have a corresponding event with the same coordinates in the other. This possibility can be obviated for all experiments that have hitherto been made by simply choosing k and P very large.

Nevertheless, if P is finite, the ordinary geometry, mechanics and electromagnetism are only a sort of approximation to the underlying modular geometry, mechanics and electromagnetism. They do not state actual theorems but only approximate results of a validity limited by the number k. For example, when we say that an event (x, y, z, t) is between two events (a, b, c, t) and $(\alpha, \beta, \gamma, t)$ we mean that $x = a + \lambda(\alpha - a)$, $y = b + \lambda(\beta - b)$, $z = c + \lambda(\gamma - c)$, where λ is less than 1 and expressible in the form p/q with q less than k. If we managed to subdivide the interval between the two events more than k times we should presently find that we were no longer between the two original events, but somewhere outside. For in the modular space-time there is no absolute system of order relations. We only have a partial system of order relations based on the correspondence limited by the number k.

It is for this reason that there are no absolute inequalities of the sort that appear in ordinary mechanics, and that no statements can be made which apply to unlimited amounts of space or of time or which require more than a limited subdivision of space and time. Hence the speculations to which I referred above about the ultimate fate of the universe, or about its distant parts, become impossible. Indeed, it becomes impossible to formulate the questions which these speculations purport to answer.

But if the current geometry and mechanics can be fitted so exactly to a space-time of so utterly different a character as a modular one, there are doubtless many other types of discontinuous space-time to which they can be equally well fitted. The moral with regard to all speculation which goes beyond the reach of experiment is obvious. But if it should be found that one of these discontinuous space-times fitted experimental results better than the continuous one, the situation might be radically altered.

Let us, however, apply our moral to ourselves and let these idle speculations go no further. Instead, let me urge once more the importance of further studies of the foundations of the classical branches of mechanics. It is true that the new conceptions of space and time are so varied as to have in common not much more than the idea that it is possible to characterize natural phenomena in an orderly way by means of sets of numbers. This is about as much as all experimental physicists and mathematicians would accept dogmatically. It is also true that elementary geometry and mechanics lay down a very particular system of rules according to which the sets of numbers are to be applied to phenomena. But these rules are such as our everyday experience has made second nature to us, for the experiments confirming them, although crude, are being continually repeated. Moreover, the rules are so good that they can be used in almost any theory as the preliminary way of assigning the coordinates from which the final coordinates can be calculated by variational methods. New theories are very apt to function merely as differential corrections to old ones.

Hence there need be no fear that further work devoted to perfecting the classical theories will be wasted. They will continue to be the instruments by which the vast majority of the facts of science are classified and to be regarded as the necessary preliminaries to the more esoteric theories. Those who have a taste for logistic work may therefore be urged wholeheartedly to devote their attention to formulating the postulates of the classical theories. There also seems to me to be a possibility of simplifying and modernizing the methods used in these theories and of extending the results by bringing in more of the modern methods of mathematics, but that is not the present subject of discussion. What I wish to emphasize now is the need of logistic studies which will make it possible to say more definitely than is yet possible in this field what is assumed, what is proved, and how the group of theorems and definitions hang together. Incidentally, I would propose that the number of undefined terms be made large, rather than as small as possible, for whenever we introduce a new undefined term we separate off a group of theorems in which this term appears. Thus the undefined terms should be so chosen as to subdivide the science into divisions which are convenient both for mathematical and for physical purposes.

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PASTEUR, THE MAN[1]

PASTEUR has told us that scientific understanding comes to the "prepared mind." In reading the splendid "Life of Pasteur" written by his son-in-law, Vallery-Radot, as well as the more recent tributes by Duclaux and by Descour, one is impressed by the lack of detail as to how Pasteur's own mind became "prepared." The story has lately been told in greater detail by Marc Tiffeneau.[2] Pasteur, as everybody knows, was the son of a tanner who had been a soldier under Napoleon. Pasteur entered the École Normale at Paris when he was twenty-one years old, and graduated three years later in 1846. He became a demonstrator (agrégé préparateur) in chemistry, a position which had just been established in order to allow young men to continue their laboratory researches instead of exiling them to provincial professorships. About the same period, in the year 1845, Auguste Laurent left the chair of chemistry at the University of Bordeaux, which he had occupied since 1838, because he did not find there sufficient opportunity for research. Arriving in Paris, he accepted the offer of a new laboratory in the École Normale, and it was not long before Laurent recognized the exceptional character of his young assistant, Pasteur, an ardent worker and a brilliant thinker. Laurent was at this time thirty-eight years old. He was the son of simple French peasants; he was born, lived and died poor. When twenty-two years of age he graduated as engineer of mines and later became the assistant of Dumas, who taught him the principles of organic analysis. His doctor's thesis considered the doctrine of chemical substitutions, which is one of the pillars of the atomic theory, and the thesis described specifically the action of chlorine on organic compounds. He and his inseparable friend, Gerhardt, were the real founders of the atomic theory. Laurent now suggested to Pasteur the subject of his thesis, which also involved the then great controversy of chemical substitutions. On August 23, 1847, Pasteur presented for his doctorate a paper entitled "Research into the saturation capacity of arsenious acid. A study of the arsenites of potash, soda and ammonia." Pasteur herein writes regarding Laurent that he had been "enlightened by the kindly advice of a man so distinguished both by his talent and by his character."

And on another occasion he says, "Laurent's lectures are as bold as his writings, and his lessons are making a great sensation among chemists." For Laurent, in 1846, gave the first course on "chemical anatomy" under the Faculty of Medicine of Paris to crowded classrooms, and here for the first time enunciated the atomic doctrine before medical students.

Laurent, having been trained as a mining engineer, had a remarkable knowledge of crystallography, and in 1845, according to Tiffeneau, had shocked his colleagues by declaring that substances which were isomeric could crystallize in different systems.

When Laurent left the laboratory in 1847 Pasteur was already the master of his technic. Shortly thereafter Pasteur became interested in the relation between chemical structure and the power to rotate polarized light, from which arose his celebrated studies upon the molecular dissymmetry of tartaric acid published in 1848.

To Biot, an old man of seventy-four who

[1] An address delivered on the centenary of the birth of Pasteur on December 27, 1922, before a meeting of the Federation of Societies for Experimental Biology held at Toronto, Canada.

[2] Tiffeneau, M.: *Bull. Soc. française de la histoire de médicine,* 1921, 15, 46.