REMARKS ON THE FOUNDATIONS OF GEOMETRY*

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1. *Relation between Matter and Space.* The foundations of geometry must be studied both as a branch of physics and as a branch of mathematics. From the point of view of physics we ask what information is given by experience and observation as to the nature of space and time. From the point of view of mathematics, we ask how this information can be formulated and what logical conclusions can be drawn from it.

It is from the side of physics that has come the most important contribution in the last two decades. The experimental physicists and the astronomers have uncovered facts which thus far can only be reconciled with one another by the method due to Einstein. This method consists essentially in regarding the events of space-time as a four-dimensional manifold in which there is a Riemann metric. The method is radically different in principle from that of the classical geometry and mechanics. Yet the quantitative results which flow from it are so closely in agreement with those of the older theory that they can be distinguished experimentally only in a few cases and with great difficulty.

To understand the exact way in which the euclidean geometry and the Newtonian mechanics can be regarded as a first order approximation to the Einstein theory of gravitation seems to me to be of great importance in understanding the physical significance of geometry altogether. For it requires us to be clear as to what we mean physically by straight lines, axes of inertia, absolute rotation,

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and so on. Therefore I propose, with your kind indulgence, to make my first remarks on this subject. It seems to me that it is a suitable question to discuss in this sort of an address, in which one is not expected to pile new bricks on the edifice of science but rather to make a few observations as to how the brick-layers are getting on with their work.

In the Newtonian theory of gravitation the motion of a particle is determined by the interplay of two forces, the attraction (according to the inverse square law) of all other particles, and the particle's own inertia, i. e., its tendency to move uniformly in a straight line. The inertia of a particle is an unexplained and unchanging property of the particle itself, although it clearly requires a euclidean geometry and a theory of time to state what is meant by the phrase "uniformly in a straight line".

The lack of a physical basis for inertia was seen by Mach, if not by others before him, as a weakness of the Newtonian mechanics, and Mach put forward the idea that the inertia of a particle as well as the gravitational attraction on it is determined by all the rest of the matter in space. This conception was very influential in developing Einstein's gravitation theory and is taken account of in this theory by merging the ideas of inertia and gravitational attraction into a new idea. The motion of a particle is completely described by saying that its world-line is a geodesic of the differential form

$$ds^2 = g_{ij}dx^i dx^j. \tag{1}$$

The effect of the rest of the matter in the universe is to determine the values of the functions g_{ij}, and the functions g_{ij} in turn determine the motion of the particle.

This motion can be described in terms of inertia, gravitation, etc., if we employ a properly chosen euclidean metric as an approximation to the Riemann metric of the differential form (1). The approximation may be described as follows.

Consider an event in our galaxy well separated in space and time from any large mass of matter. A set of coordinates with this event as origin may be chosen by a well known method (Riemann normal coordinates) so that (1) becomes

$$ds^2 = c^2dt^2 - dx^2 - dy^2 - dz^2 \tag{2}$$

at the origin, and the equations of the geodesics through the origin become

$$x = as, \quad y = bs, \quad z = cs. \tag{3}$$

Since there is no mass of matter near the origin it follows that the formula (2) which holds rigorously only at the origin is also valid to a high degree of approximation for a large region R. In this region the geodesics which do not pass through the origin, as well as those which do, will be represented to a high degree of accuracy by first degree equations.

In other words special relativity holds with high accuracy throughout R. Any two coordinate systems with regard to which the conditions (2) and (3) hold good are related by a Lorentz transformation and are said to be in uniform motion, the one with regard to the other. The three-dimensional locus of a moving particle is the same as the three-dimensional locus of a ray of light. This is the physical content of the statement that a particle moves along a straight line.

For regions in the immediate neighborhood of masses of matter such as the sun or the earth, it is not true that the equations of geodesics are linear. But this does not prevent us from using the coordinate system (x, y, z, t) all around, and all through, the earth and sun. In this region as well as in the region R a straight line is a curve whose equation is of the first degree with regard to the coordinates x, y, z.

The coordinate system was undetermined up to a Lorentz transformation. We now determine it uniquely by the condition that it shall be at rest with respect to the sun.

This particular coordinate system we call an inertial frame of reference F. For small velocities, the Lorentz transformation is very closely approximated by the transformation

$$(4)\qquad \begin{cases} \bar{x} = a_{11}x + a_{12}y + a_{13}z + c_1 t + d_1, \\ \bar{y} = a_{21}x + a_{22}y + a_{23}z + c_2 t + d_2, \\ \bar{z} = a_{31}x + a_{32}y + a_{33}z + c_3 t + d_3, \end{cases}$$

in which the coefficients are all constants and the numbers a_{ij} form an orthogonal matrix. Any frame of reference which arises from F by a transformation of this sort is also called an inertial frame.

With respect to an inertial frame the path of light is still very approximately linear even close to the earth or the sun, but the path of a material particle is more nearly a second degree curve. This divergence of the particle paths from the light paths is accounted for by introducing the concept of force and the Newtonian mechanics. This mechanics says that in the absence of forces, particles would move uniformly in straight lines—i. e., in light paths. But the matter composing the earth brings an attracting force to bear on any particle and consequently the actual path diverges from that of light.

Thus the Newtonian mechanics is to be regarded as a device for separating out the local influence of matter (using an astronomal scale for the term "local") from the influence of matter as a whole. All the matter in the universe determines what lines are straight, what pairs of lines are perpendicular or parallel, what triangles are similar to each other, and so on. It also determines whether any given type of motion has or does not have acceleration; for example, it determines the plane in which a Foucault pendulum swings and thus gives an absolute magnitude for the rotation of the earth. On the other hand the local influence of the earth, the sun, and the planets is taken care of by means of forces which show how the actual paths of particles are diverted from what they would be without the presence of these bodies.

Consider the state of affairs inside a spherical shell of matter. The Newtonian theory assures us that the shell exerts no force of attraction on a particle inside it, a statement which must have seemed paradoxical to all of us so long as we took it to mean that the shell exerts no influence on the particle. We now see that the shell has a share in determining the very geometry according to which the particle moves, and that if the shell is set into rotary motion, say with regard to another shell which contains it, there must be a tendency to rotate the axes of inertia and thus to modify the whole geometry of the interior.

A related question might conceivably arise in astronomy. Suppose we have a very large but very sparsely distributed mass of matter which is far away from other masses. It is conceivable that such a mass might have a certain degree of autonomy in the determination of its axes of inertia. To see this, let us suppose an observer to start somewhere in the interstellar space of our galactic system and to travel without ever coming close to a large mass of matter until he arrives somewhere in the interstellar or intermolecular space of the distant mass. Anywhere he stops and sets up a coordinate system of the type we have been considering, he will find that he has, to good approximation, an inertial system extending over a quite extensive region. But as he travels, the functions g_{ij}, expressed in the original coordinate system, are gradually changing and therefore the relation between the original coordinate system and the inertial system is changing. The relation between his first inertial system and his last one will not in general be capable of representation by a Lorentz transformation. Hence the one system will appear to be accelerated with respect to the other. Such an acceleration would be expected to show itself most obviously in one or all of three ways: in a linear acceleration of the mass from or toward us, in an acceleration toward or from its center, in a rotation of its axes of inertia relative to ours.

There is good reason to believe that the domain of our axes of inertia reaches well out toward the limits of the visible universe. For the velocities of the stars are small and there is nothing known about their motion which conflicts with a Newtonian mechanics based on our inertial axes. It is not impossible, however, that the spiral nebulae might be masses of the sort to which we are trying to attribute partially autonomous inertia. They are well isolated, enormously large, and enormously distant, as recent work by Hubble establishes more clearly than ever before. Moreover their radial velocities relative to the sun are very large, they have an expansive motion relative to their own centers and they have large rotary motion. Indeed, measurements on the internal motions of certain spirals have been made by Van Maanen which Jeans found he could explain by Newtonian mechanics relative to the usual axes only if he assumed a law of attraction different from the inverse square law. Of this law Jeans says "The general effect of the modified gravitational force appears to be one of seizing the particle and compelling it to revolve about the nucleus of the nebula with an angular velocity which approximates to that of the nucleus itself rather than to that demanded by the conservation of angular momentum."

Referring to the work of Jeans, E. W. Brown asked me whether there might not be a possible relativity explanation of the apparent rotation of axes of reference of the nebula. My reply was that the relativity theory provides a physical reason for expecting this rotation, provided the nebula is sufficiently large to have a degree of autonomy with respect to inertia. In this case we should expect to describe the internal motions of the nebula in terms of a Newtonian mechanics stated in terms of inertial axes different from ours.

I am citing this highly speculative possibility in the hope that it will serve to make the problem of the foundations of geometry more vivid from a physical point of view. It may also serve to indicate some of the elements which must be thought of in a non-static solution of Einstein's gravitational equations. In order to take account of the distribution of matter throughout space in determining the values of the functions g_{ij} at any point, it would seem that we ought to have integral equations to be satisfied by the g's. But whether the partial differential equations given by Einstein are compatible with such integral equations, I do not know.

2. *The Geometry of Paths.* The studies of Riemann geometry which have been stimulated by the relativity theory have revived interest in the foundations of differential geometry. Here one starts with the observation that when arbitrary coordinates x undergo a general analytic transformation the differentials of these coordinates suffer only a linear transformation.

The differentials at a point may be regarded as representing directions emanating from this point. Thus we have at each point a projective, and also an infinity of affine and euclidean geometries to represent the relations among these directions.

But to begin with there is nothing to connect the differentials at any point x with the differentials at a point $x + dx$. Such a connection must be obtained by relations involving the higher differentials of the coordinates. It is accomplished in one manner by the introduction of a Riemann metric dependent on a quadratic differential form (1) which makes the process of taking higher differentials definite. A clear view of the Riemann geometry from this standpoint was first obtained by means of Levi-Civita's conception of infinitesimal parallelism. This amounted to finding a geometrical interpretation for the Christoffel symbols

$$\left\{ \begin{matrix} ij \\ k \end{matrix} \right\}$$

as the coefficients of a transformation which carries the components of a vector at a point x into the components of a uniquely determined vector at any point $x+dx$. It provides a definite way to visualize the limiting process by which we carry over the conceptions of euclidean geometry in a finite domain to the Riemann geometry in an infinitesimal domain.

Levi-Civita's construction was obtained by regarding the Riemann manifold as immersed in a euclidean space. The euclidean space is not essential and Weyl defined infinitesimal parallelism as an intrinsic property of the manifold itself. He also generalized it so that

$$V^i = \Gamma^i_{\alpha\beta} V^\alpha dx^\beta$$

represents an arbitrary affine transformation from x to $x+dx$. He thus arrived at the conception of a general affine connection determined by arbitrary functions $\Gamma^i_{\alpha\beta}$ which are symmetric in the lower indices.

A further step in generalization was taken by Cartan. Let us imagine that with each point x of the manifold which we are talking about there is associated a projective space S, one point of the projective space being identical with x itself. Then if we can specify a definite projective transformation from S to the projective space associated with each point $x+dx$, and do so by means of a set of functions of x, we have a projective connection in the sense of Cartan.

All these methods of connecting up the differentials at one point with the differentials at the points in its neighborhood can be seen very clearly in terms of the geometry of paths. By a geometry of paths I mean the theory of any system of analytic curves in an n-dimensional manifold, the curves being such that any point is joined to any other point in a sufficiently near neighborhood of itself by one and only one curve of the system. Such a system of curves is the natural generalization of the straight lines of euclidean geometry and of the geodesics of Riemann geometry.

In a very general case the paths can be represented as solutions

$$x^i = f^i(s) \tag{5}$$

of a set of differential equations,

$$\frac{d^2x^i}{ds^2} = \Gamma^i\left(x, \frac{dx}{ds}\right) \tag{6}$$

in which the right hand members are functions of the x's and their first derivatives. The invariant theory of these differential equations under transformations of the coordinates x is the affine geometry of paths. This geometry can be developed in some detail in case the functions Γ^i are homogeneous of degree 2 in dx/ds. Its most powerful method consists in transforming (6) to normal coordinates, that is, to coordinates such that

$$y^i = \xi^i s, \tag{7}$$

with ξ^i constant, represent the paths through the origin. Whenever the coordinates x undergo an arbitrary analytic transformation the normal coordinates receive merely a linear transformation with constant coefficients.

If we define a system of straight lines by means of linear equations with respect to normal coordinates, we obtain an associated euclidean space in which the straight lines through the origin coincide with the paths (7) through it. Since an arbitrary transformation of the coordinates x brings about a linear transformation of the normal coordinates it brings about an affine transformation of the associated euclidean space.

The choice of the differential equations (6) to represent the paths has determined the type of the parameter representation (5) to a certain extent and has also introduced a relation between the parameters representing different paths through the same point. If we subject the parameter in (5) to an arbitrary transformation the equations of the paths will, in general, no longer satisfy differential equations of the form (6). But the differential equations of the paths can be written in a form

$$\frac{\frac{d^2x^i}{ds^2}-\Gamma^i\left(x,\frac{dx}{ds}\right)}{\frac{dx^i}{ds}}=\frac{\frac{d^2x^j}{ds^2}-\Gamma^j\left(x,\frac{dx}{ds}\right)}{\frac{dx^j}{ds}} \tag{8}$$

which is unaltered by transformations of the parameter s.

A transformation of the parameter brings about, in the euclidean space associated with each point by means of the normal coordinates, a transformation which is not in general affine. It thus requires the introduction of points at infinity in each of these euclidean spaces. In certain cases the adjunction of the points at infinity will convert the euclidean spaces into projective spaces.

If we specialize the functions Γ^i so that (6) takes the form

$$\frac{d^2x^i}{ds^2}+\Gamma^i_{\alpha\beta}\frac{dx^\alpha}{ds}\frac{dx^\beta}{ds}=0 \tag{9}$$

where $\Gamma^i_{\alpha\beta}$ are functions of x, we have the sort of affine geometry of paths which has been studied by Eisenhart, Thomas, and myself and which is equivalent to the affine geometry of Weyl. For infinitesimal parallelism is simply invariance of the components of a vector in the neighborhood of the origin of normal coordinates and covariant differentiation is merely differentiation with respect to normal coordinates at the origin. Indeed, differentiation of any order with respect to these coordinates at the origin is an invariant operation which enables us to find a complete set of invariants for the affine geometry.

When the parameter s in the equations of a path satisfying (9) undergoes a transformation which is not linear, the functions $f^i(s)$ in the equations of the path (5) are transformed into functions which are solutions of equations

$$\frac{d^2x^i}{ds^2}+\Lambda^i_{\alpha\beta}\frac{dx^\alpha}{ds}\frac{dx^\beta}{ds}=0,$$

in which

$$\Lambda^i_{\alpha\beta}=\Gamma^i_{\alpha\beta}+\delta^i_\alpha\varphi_\beta+\delta^i_\beta\varphi_\alpha,$$

the functions φ_α being components of a vector, and δ^i_j the Kronecker delta. In a very general case it is found that the normal coordinates undergo a linear fractional transformation when the functions Γ are changed in this manner into the functions Λ. We thus find that a set of homogeneous coordinates whose ratios are equal to normal coordinates for the functions containing the indeterminate vector φ are suitable for the determination of projective invariants. These projective invariants can be said to give genuine properties of the paths since they are independent both of the choice of the coordinates x and of the particular parameter representation of the paths.

Certain choices of the vector φ bring about particular transformations of the parameters s in the equations (5) which amount to non-affine projective transformations of the euclidean space mentioned above. By adjoining points at infinity to this space we obtain a projective space which is essentially the same as that considered by Cartan. The projective transformations determined by φ bring about a class of transformations of the parameters of all paths through the origin of normal coordinates. It is an interesting fact that any transformations of the parameter on any path may be obtained in this way.

The successive specializations which we have been introducing in the specification of the system of paths can all be regarded as closer and closer restrictions on the manner in which the paths are simultaneously given a parameter representation. This process can be continued by assuming that the equations (9) possess a first integral, say a homogeneous quadratic first integral

$$g_{ij}\frac{dx^i}{ds}\frac{dx^j}{ds} = \text{constant}.$$

This enables us to define the parameter along any path by means of the definite integral

$$\int\sqrt{g_{ij}\frac{dx^i}{dt}\frac{dx^j}{dt}}\,dt$$

and gives the parameter many of the characteristic properties of distance. Finally by requiring that the path actually minimize this integral we specialize down to a Riemann geometry.

In thus shadowing forth something like a set of axioms for the various types of path geometry, I have not given the calculus of variations its due. The problem of determining those systems of paths which can be regarded as extremals of a definite integral is one whose solution will doubtless modify our views on the foundations of differential geometry.

3. *Choice of Undefined Terms.* In studying the Riemann geometry and its generalizations we make use of the euclidean geometry at every turn. Hence our interest in these subjects by no means kills our interest in the foundations of euclidean geometry. But these studies and the revival of the physical point of view necessarily have an influence on our preference among the many ways of formulating the axioms.

The Riemann geometry and its generalizations all start with the assumption that the set of points with which they deal is a portion of an n-dimensional manifold, or an n-cell in the sense of analysis situs. This assumption may be formulated by asserting that there exists a way of labelling the points by means of ordered sets of n numbers

$$(x^1, x^2, \cdots, x^n),$$

one set of numbers for each point and one point for each set of numbers. This is equivalent to assuming that an n-cell is a set of points capable of supporting a euclidean geometry. For if there is a coordinate system there is a system of curves which are linear with respect to it, and which function as the straight lines of a euclidean space; and if the points are in correspondence with a euclidean space, a coordinate system can be set up. Thus any set of postulates for a euclidean space gives rise to a set of postulates for an n-cell.

Of course, a set of postulates for any particular one of the generalized geometries which we are considering would characterize an n-cell just as well as the postulates for euclidean geometry. Any two of these geometries are in a relation which may be stated as follows: If A be an arbitrary point of the space described by one n-dimensional geometry, A is surrounded by an n-dimensional region R_A in which the points are uniquely denoted by coordinates x and in which the relations of the geometry (such as the formula for distance or the equations of the paths) are expressed by analytic functions in terms of these coordinates. If B be an arbitrary point of the space described by any other n-dimensional geometry there is a region R_B having analogous properties for this geometry, and such that between the coordinates y of R_B and the coordinates x of R_A there is an analytic relation

$$y^i = f^i(x)$$

with a unique inverse.

Thus in a certain sense the two geometries are analytically related. It is not that there is an analytic transformation of the one geometry into the other. But the fundamental loci of each geometry determine a class of coordinate systems in terms of which these loci are analytically representable, and there exists an analytic transformation of any coordinate system of the one geometry into any coordinate system of the other.

One of these geometries is that of Euclid. Hence a perfectly sound way of stating the postulates of any of the geometries we are considering is first to give a set of postulates for euclidean geometry and then to postulate that the geometry which we are describing is analytically related to that of Euclid.

It seems clear to me that when they are to be used for the purpose of characterizing an n-cell we can definitely prefer certain sets of postulates of elementary geometry to others. The purpose is to characterize a set of points; therefore there should be as few other elements as possible.

For example, a set of postulates in terms of points, lines, planes and congruence is less desirable than one in terms of points and lines only. Moreover a set of postulates about points and lines in which the lines are regarded as sets of points is to be preferred to one in which the lines are objects of some other sort to which the points are joined by means of an undefined relation.

Between a set of postulates in terms of points and lines and one in terms of points and the betweenness relation, there would doubtless be little to choose. In either case, space is characterized as a class of points which are arranged in sub-classes related among themselves in a particular way.

The fact that physical discussions seem always to treat the line and plane as sets of points would seem to confirm this preference. Also the fact that in its direct application to nature the euclidean geometry is used to give an approximate description of a portion of the universe would seem to establish a presumption in favor of axioms which are stated for only a finite region of space.

I do not mean to suggest that we should try to reduce the number of undefined terms to a minimum. For in a mathematical science which has physical applications there is much to be said for making an undefined term out of each object or relation which has an independent physical definition.

A physical definition is made in terms of human operations. Thus, for example, to define what a point is physically we have to describe a series of acts which another person must perform in order to locate a point. Or in order to say what we mean by the distance between two points we must describe the operation of making the measurements necessary to determine the number we call the distance.

This description is in general very complicated. For example, in the case of distance it would involve nearly all the technique of practical astronomy. Moreover the definition obtained is in general only approximate. For example, we never have an absolutely complete determination of what we mean by a point.

The physical definition of a point is notoriously without effect on the mathematical theory which follows. So far as the mathematical operations are concerned the point is purely and simply an undefined term. The abstract mathematical theory has an independent, if lonely, existence of its own. But when a sufficient number of its terms are given physical definitions, it becomes a part of a vital organism concerning itself at every instant with matters full of human significance. Every theorem can then be given the form "if you do so and so, then such and such will happen".

The places at which this life-blood of human meaning flows into the mathematical theory should, it seems to me, be the undefined terms. The process of making the physical application of the mathematical discipline will then consist simply in specifying the human operations by which we give meaning to the mathematically undefined terms. Each of the undefined terms will separate off a group of theorems in which the term appears, and these theorems will refer particularly to the set of physical operations involved in defining this term.

Thus in particular the question whether the plane as well as the line shall be an undefined term depends on how the plane is thought of physically. When solid objects and flat surfaces were fundamental physical objects, it was natural to think of the plane as a fundamental entity, somehow distinct from the set of points on it. But with particles and their paths playing the role which they do in modern physics it would seem more natural to regard lines and planes as sets of points.

I am not pretending to exhaust this question about the choice of undefined terms—only to indicate one respect in which the physical application seems to indicate a preference. In passing, however, I should like to refer to a remark by the philosopher Geiger* who has made up postulates in terms of points, lines, planes and 3-spaces, of which last he postulates that there exists only one. Geiger's remark is that these are not the only "Elementargebilde", but that circles, parabolas, spheres, ellipsoids, and so on, also have a right to be considered. So "vom konsequenten Standpunkt aus" these should also appear as undefined terms. But for obvious reasons this program has not yet been carried out.

4. *Postulates for Analysis Situs.* The characterizations of an n-cell in terms of coordinates or of its correspondence with euclidean space have the disadvantage that they make use of ideas which are not invariant under the group of continuous transformations which we study in analysis situs. The undefined terms which we should expect to use are point and region, as R. L. Moore has already shown is possible for the two-dimensional case, or point and limit point.

To carry out the latter program successfully one should be able to start with the most general type of abstract set in which there is a definition of limit point, and then by successively excluding various types of sets, narrow down to the n-cell. This ought not to be very far out of the range of possibilities at the present time.

Another question about the foundations of analysis situs which deserves to be studied, and which can, I think, be studied with some hope of success, is the problem of a combinatorial set of axioms. This question has been before us with increasing clearness for some time. The researches of Poincaré made it evident that a large class of the fundamental problems of analysis situs are combinatorial in character. A combinatorial set of postulates was sketched in the Dehn-Heegaard article in the Encyclopädie. My Cambridge Colloquium lectures contain a treat-

* M. Geiger, *Systematische Axiomatik der Euklidischen Geometrie*, Augsburg, 1924.

ment in which combinatorial elements are separated out, but in which it is necessary to use the definition of an n-cell by means of correspondence with an ordinary geometric figure in order to obtain the proofs of invariance of the combinatorial constants. More recently Weyl has given a treatment from which he has excluded everything which he cannot obtain by combinatorial processes and into which the notions of "circuit" and of "partition" are brought by means of axioms instead of definitions.

There are several ways in which we can formulate the problem of a combinatorial analysis situs attack on the foundations of geometry. One of them would be this: Our undefined terms are 0-cells, 1-cells, 2-cells, and so on up to n-cells. We have relations of incidence between 0-cells and 1-cells, between 1-cells and 2-cells, and so on. We can in the three-dimensional case—but not yet in the four-dimensional one—state in abstract terms what conditions these incidence relations must satisfy in order that the complex shall be a manifold. Progress is being made —notably by Alexander—toward finding out what further conditions must be satisfied in order that this manifold shall be of a particular type, say a sphere or a 3-cell with its boundary. When we have this result we should be able to combine with the postulates so obtained further assumptions about the subdivision of the cells into complexes of cells and the combination of sets of cells to form single cells. From these we should be able to get the theorems of invariance and thus be able to prove all the theorems of analysis situs which can be formulated in terms of cells.

Such a treatment would put in evidence the properties of space which have to do with continuous deformations and indefinite subdivision, without carrying the subdivision to any limit. We should thus remain within the range of finite sequences of operations and avoid the difficulties which beset the theory of classes of large cardinal number. My impression is that Weyl has referred to this advantage of the combinatorial attack on the foundations of geometry in a letter to me. The letter is unfortunately not at hand. In any case, it cannot be doubted that he has considered the question in connection with the doubts which he and Brouwer entertain as to the validity of the ordinary theory of the continuum.

The matter is also of interest from the physical point of view in that a theory of this sort could doubtless be worked out in terms of n-cells as the sole undefined objects. It would give a kind of n-dimensional continuum without necessarily implying the existence of any points. Some twenty years ago I had a similar project in mind, that of stating the foundations of geometry in terms of overlapping 3-cells or "chunks" of space, and discussed it a great deal with R. L. Moore without getting very far. Since then Huntington and Whitehead have given sets of postulates using solid portions of space as undefined elements. But they both have let the point in as a limiting case, whereas the true solution of the problem I have in mind would be a set of postulates which would lead to the main physical properties of space without providing a limiting process to define points. It would correspond to the true state of our physical knowledge of space, which is vague both for the infinitely large and also for the infinitely small.

5. *The Arithmetical Point of View.* While such an enterprise as that of which we have just been speaking is inspired by physical considerations, it would probably not be of any immediate interest to physicists. For the everyday uses of physics all the methods of approaching the Riemann geometry or the foundations of analysis situs must give way to the straight-forward statement that the points we mean to talk about are objects capable of representation by coordinates. Indeed, the definition of mathematics which will be most readily accepted by physicists is that which has been so cogently set forth

by Study* in various recent publications, that "mathematics includes computation with natural numbers and everything that can be founded upon it, but nothing else". This is a more inclusive account of mathematics than that given by Kronecker because Study allows the Dedekind cut process in building upward from the integers.

Whether we accept the arithmetical definition of mathematics or not, we have to admit that it gives us one way at least of treating the material in each of the branches of mathematics which we now regard as important. Moreover it puts us in touch at once with an indispensable process in each of these branches. Therefore from the point of view of teaching mathematics I think that the arithmetical method is bound to prevail. Most of the people who set out to learn mathematics do so with a view to some use which they are going to make of it. The arithmetical method will give them a maximum of mathematical information and power in a minimum of time. Everything that is irrelevant to this will have to be discarded.

I mean this statement to be as sweeping as possible. The treatment of elementary geometry modelled on that of Euclid is already being discarded. It will be replaced, not by a modern equivalent which stands apart as a logical structure whose chief methods are peculiar to itself, but by a properly developed analytic geometry. Likewise the synthetic treatment of projective geometry, however seductive it may be to some of us individually, will have to be left to one side as a study for specialists and as a theory which may be expounded to students at the stage when it is desirable for them to see what a well-rounded logical structure is like.

But the arithmetical method will be used for the primary exposition of all those theorems which we encounter in

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* *Die Realistische Weltansicht und die Lehre vom Raum*, Braunschweig, 1914, and *Mathematik und Physik*, Braunschweig, 1923. Our quotation is from the latter book.

the main stream of mathematics. This will happen not because mathematics has been thus defined by any person however brilliant, but simply because life is too short for people who are not primarily mathematicians to learn mathematics otherwise.

I am tempted to add a remark that will not be so easily accepted by some of my friends who are devoted to analysis. Namely that much of the present theory of functions of one complex variable (which is really a conformal geometry incapable of generalization) will suffer the same fate as synthetic geometry. Its highly elegant but highly introversive methods will be left to one side for the specialist. The main current of mathematics will flow by, carrying with it only the more important facts of this theory; and these facts will be formulated as we are now learning to formulate them with the aid of analysis situs, in terms of the theory of functions of n real variables.

But while the arithmetical definition of mathematics specifies the actual content of present day mathematics and also corresponds to the most practicable way of expounding the subject to those who use it, it hardly seems adequate when we examine its foundations. If it is true that the basic ideas of geometry, mechanics, thermodynamics, etc., can all be defined in terms of real numbers, and real numbers in terms of natural numbers, it is also true that the natural numbers are mixed up with the basic ideas of formal logic in the most sticky fashion. Therefore the whole arithmetical structure is a portion of the edifice of formal logic which we are not as yet able to separate off. So, for example, the *Principia Mathematica* of Whitehead and Russell begins with the logic of propositions and goes far with the logic of classes before the natural numbers fully emerge. After that their treatment amounts essentially to the process of arithmetization.

But even after we are fully embarked on the arithmetical stage of the theory we are still beset by difficulties as to what we mean by a class of objects and what are the allowable reasoning processes which can be applied to classes. We can neither obtain our definition of irrational number nor prove all the theorems desired, without reasoning about classes which are very different from those accessible to the simple experiments upon which the elementary logic is based. I mean experiments in counting and in identifying the same object at different times. The theory of infinite classes is obviously incomplete and beset with difficulties and paradoxes which have baffled the many excellent mathematicians who have attempted to set it in order. The latest and, I think, the most promising attempt to make it comprehensible is that of Hilbert who seems to propose that we apply our elementary logic to the symbols which are used in writing out the statements made in the logic required for a general theory of classes. It thus becomes a feasible problem to state what sorts of combinations of symbols give self-contradictory statements and then to study the possible combinations of symbols with a view to finding out by means of our elementary logic whether the postulates of the higher logic give rise to self-contradictory statements.

Whether or not this is a correct statement of Hilbert's program, the conclusion seems inescapable that formal logic has to be taken over by mathematicians. The fact is that there does not exist an adequate logic at the present time, and unless the mathematicians create one, no one else is likely to do so. In the process of constructing it we are likely to adopt the Russell view that mathematics is coextensive with formal logic. This, I suppose, is apt to happen whether we adopt the formalist or the intuitionist point of view.

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