CERTAIN ASPECTS OF MODERN GEOMETRY[1]

IN his letter inviting me to lecture at the Rice Institute, President Lovett suggested that I might take as a model the course of lectures which Felix Klein gave in Chicago in 1893. I felt sure that President Lovett was not trying to tease me by setting up a standard of exposition and insight which could only make me uncomfortable, but was trying to indicate a type of subject matter. So I looked up the volume of *Lectures on Mathematics*, the first in the Colloquium series of the American Mathematical Society, and found that what Klein did was to pass in review the various branches of mathematics that were being studied in Germany at that time with particular reference to the research under way at Göttingen. He did this in a series of fifteen or twenty lectures. Since I am to give only three lectures, I felt at once excused from so general a survey and decided to limit the field to certain aspects of modern Geometry.

In thinking over the general problem of such a survey I made an observation which is by no means new but which I think ought to be impressed on all mathematicians, namely, that one cannot fairly excuse himself for a failure to have a comprehensive view of mathematics no the ground of the increased complexity and extent of the subject matter. For although it is true that the mass of material is enormously larger than it was in 1893, our progress has not consisted only in the discovery of new facts and theorems. It has consisted still more in the discovery of more comprehensive

[1]A course of three lectures delivered at the Rice Institute on Jan. 8, 11, and 12, 1932, by Oswald Veblen, Ph.D., D.Sc., Professor of Mathematics, The Institute for Advanced Study.

points of view from which we can see large groups of mathematical phenomena in their relationship to each other. We can carry over what we have learned in one field into many other fields, and we can leave many details out of consideration which were formerly regarded as essential. Thus by the processes of generalization and selection which result from mathematical research it is possible, I believe, to have as good a grasp of the whole of mathematics, now, as one could have had in 1893.

Another reflection which is bound to occur to one in thinking back to 1893 is that no one could now come from Göttingen to lecture in America with the idea that he was carrying a light into the wilderness. Of course, Klein did not let any such feeling show in what he said, but there is no doubt that that is what he felt he was doing. Today (January, 1932) Göttingen is quite as great as it was in 1893, but the wilderness has changed. There is no one, indeed there are no two or three, mathematical centers that can be regarded as dominating the rest. I cannot think of any branch of mathematics that can be regarded as localized in less than two centers. A new discovery made in Texas is not so apt to be seized on for development by a student of the man who made it as by someone in Warsaw or Moscow, and the next step is just as likely to be made in Vienna as anywhere else. The nearest thing to a nationally localized mathematical movement was the Italian school of algebraic geometry and that has been a thing of the past since this subject was turned in a new direction by the work of an American mathematician who was born in Moscow.

So I propose to talk about mathematical subjects rather than about the work of particular individuals or groups of people and to try to describe general ideas and tendencies of thought rather than details.

I

THE MODERN APPROACH TO ELEMENTARY GEOMETRY

IN this first lecture let us try to see what effect, if any, modern research has had on our views about elementary geometry, and how the latter ought to be presented to students. According to old tradition the Euclidean geometry was the type example of a logically perfect science. Starting from the axioms it proceeded by inexorable logic from one theorem to another. On the other hand, what we now call analysis was admitted to be on rather a shaky foundation, but justified by the importance of its results. Then, during the nineteenth century, there came, on the one hand, the discovery of a number of non-Euclidean geometries which tended to shake confidence in the Euclidean geometry and to produce a more critical attitude toward it. On the other hand, there came the refounding of analysis by deducing the whole theory of real and complex numbers from that of the whole numbers. The negative numbers and rational fractions were defined as certain pairs of whole numbers. The irrational numbers were defined as certain infinite sets of rational numbers. The complex numbers in their turn were pairs of reals. The existence of the real number system was thus carried back by a series of constructions to the existence of the whole numbers. On this seemingly secure basis there was then constructed a complete system of analysis in which every operation (differentiation, integration, solution of differential equations, etc.) was justified by an appropriate existence theorem.

In the latter part of the century, analysis had come to be regarded as perfectly secure and geometry was often held to be a dubious affair resting on doubtful "geometric intuitions" which could only be justified by reformulating them so as to be susceptible of a purely analytic treatment.

Then came the intensive studies of the axiomatic foundations of geometry initiated by Pasch and Peano and continued by Hilbert, E. H. Moore, and others, which restored the prestige which had been lost by the Euclidean geometry. It was once more possible to regard geometry as a set of propositions proceeding by the laws of logic from a set of axioms. Also it was possible to prove that the axioms were logically independent, i.e., no one deducible from any of the others.

Furthermore the theory of groups had been developed to a very high degree of perfection and applied to the classification of geometries, so that it was possible to see the Euclidean geometry as a single citizen in an orderly community. All this is so well known that I will take the appropriate remarks as having been made about the Erlanger Program and pass on.

In arriving at this clear cut view of geometry it was necessary to regard it as an abstract science. This meant that it was necessary to distinguish between geometry as a branch of mathematics and geometry as a branch of physics.

As a branch of mathematics, geometry is an orderly sequence of theorems and definitions proceeding by logical steps from a set of unproved propositions, or axioms, stated in undefined terms such as point, betweenness, and congruence. The axioms are perfectly arbitrary, subject to the requirement that they should be consistent and mutually independent.

They have nothing to do with experience or observation.

As a branch of physics, on the other hand, geometry is the description of the results of a vast body of experiments and observation. *It tells what will happen if you do certain things.* For example, if you will mark three non-collinear points A, B, C on a blackboard with chalk and then mark three other points, A' midway between B and C, B' midway between C and A, and C' midway between A and B, and then draw the three straight lines AA', BB', CC', these three lines will have a point in common.

It is true of course that none of the operations described can be performed exactly. When you try to make a point you really make a spot, and when you try to draw a line you really make a strip. But the more accurately you succeed in indicating the points and drawing the lines, the more beautifully will the result of the experiment be in accord with the theorem stated. This state of affairs is characteristic of physics as a whole. No experiment can be carried beyond a certain degree of accuracy, and no physical object can be completely characterized.

But in order to avoid the complication that would ensue if we tried to describe the physical situation as it actually presents itself, we postulate the existence of certain idealized objects, which in the case before us we call points and lines, and assign to them certain exactly stated properties. The theory of these ideal objects is a branch of mathematics. The body of experiment and observation in which this mathematics is used is a branch of physics. It is the business of the mathematician to reason accurately and intelligibly about the ideal objects. It is the business of the physicist to determine what the ideal objects shall be and to verify or reject the axioms and theorems about them by successively finer and finer experiments.

This is what is meant when we say that mathematics is an exact science and that the mathematician need not know or care whether his axioms are true or false, and even need not know what they mean. It is the business of the physicist to know and care about all these questions.

All this was well understood by mathematicians thirty years ago. At the University of Chicago where I studied these matters under Professor E. H. Moore, we used to call it "the abstract point of view." But in those days the physicists regarded the abstract point of view as a plaything with which they, fortunately, had nothing to do. The weakness of the abstract point of view, as applied to geometry, was that although we know plenty of geometries, the Euclidean geometry was the only one which was actually used physically.

This state of affairs changed radically with the advent of Relativity. After many physicists had tried unsuccessfully to state the results of experiment in terms of the classical geometry, mechanics, and electromagnetism, Einstein found that it was possible to give a good account of these phenomena by using a four-dimensional Riemannian geometry for space-time. This is sometimes expressed by saying that Relativity "fitted the facts" better than the previous theories.

But it is not quite true that we have a set of "facts" on one side and a "theory" on the other, and a process of matching one against the other. For it would be quite impossible to describe the facts without stating them in terms of some sort of theory. Besides, if we had a clear account of the facts without the intervention of theory, the theory would be superfluous. It is rather that, as in the example I cited above of the medians of a triangle, the theory is a self-consistent body of statements which serve in the description of the facts. When it is no longer possible to describe all the facts with the aid of the theory, then it is time to look for a new theory.

This does not mean that the old theory is absolutely discarded. For the old theory served to describe a large body of observations. It is in general only a few new and difficult experiments that refuse to fit in. For example, the theory that the sun, moon, and stars go round the earth is still in daily use in spite of our conviction that a quite different astronomy is the correct one. For we still say that the sun rises in the east and sets in the west.

The actual situation of the Euclidean geometry is quite analogous. There is a vast body of experience which is adequately described by language which presupposes the validity of the Euclidean Geometry. There is a growing but still relatively small body of experimental data which it has not been possible to describe in this manner. When we look at the problem of describing the complete situation, taking all the data of Astronomy into account as well as what is now called "classical" physics, we are forced to say that the Euclidean geometry is not true, that a better description of nature can be obtained by using a Riemannian geometry.

If we attempt to take into account also the small scale phenomena, those that are dealt with by quantum theory, it seems quite possible that not even a Riemannian geometry will suffice. At least, no one has yet succeeded in formulating a mathematical theory that will serve for both macroscopic and microscopic phenomena.

But the domain of experience to which the Euclidean geometry does apply is so great and so important in everyday life, and the geometries which could be substituted for it are so complicated, that we cannot conceive of its being displaced from its position among the subjects which every civilized person must study. Only we must be less dogmatic than we used to be about its validity and significance.

Another phenomenon of our time which must affect the way in which we look at elementary geometry is the rapid advance of analysis. Analytic methods are being extended not only into all domains of mathematics but also of science altogether, even into the biological and social sciences. In geometry this movement has meant the almost complete disappearance of synthetic methods, as we used to call them. The work on the foundations has made the synthetic methods ideally clear and easy to understand in such subjects as projective and Euclidean geometry, but when it comes to developing a new subject, say a new branch of geometry, no one nowadays would think of overlooking the tools of analysis.

It seems to me that elementary geometry should be presented in such a way as to prepare the students for the other sciences which he is to study later, and in which this very geometry is going to be used. This means that the methods of geometry should not be singular ones, peculiar to this subject itself, but should as far as possible be methods which can be used over and over again in the other branches of science.

If you ask a modern mathematician or physicist what is a Euclidean space, the chances are that he will answer: It is a set of objects called points which are capable of being named by ordered sets of three numbers (x, y, z) in such a way that any two points (x_1, y_1, z_1) and (x_2, y_2, z_2) determine a number called their "distance" given by the formula,

$$(1)\qquad \sqrt{(x_1-x_2)^2+(y_1-y_2)^2+(z_1-z_2)^2}.$$

With a little refinement of logic this answer is a perfectly good set of axioms. The undefined terms are "points" and certain undefined correspondences between points and ordered sets of numbers, (x, y, z), which we call "preferred coordinate systems." The axioms state that the transformations between preferred coordinate systems preserve ratios of distances. I will not stop to formulate them more closely here. This way of stating the axioms is discussed in some detail in a forthcoming Cambridge Tract[1] by J. H. C. Whitehead and myself.

I do not advocate the use of any particular set of axioms in the schools but I do advocate the introduction of analytic methods in elementary geometry. There are of course many practical pedagogical objections which can be made to this proposal. I do not feel confident enough of myself as a pedagogue to try to meet these difficulties in detail. But I think that the development of science is forcing us in this direction and that those whose business it is to do so will have to find out how to overcome the pedagogical difficulties.

The obvious thing to do is to recognize that we have a subject to present which has to do with physical reality. Let us approach it as any modern scientist would by first studying some of the observations and experiments with which we have to deal, in crude everyday terms. This would correspond, I suppose, to the "observational geometry" which is already in our schools. After getting a start in this way a scientist feels the need of a systematic language in terms of which to organize the phenomena. This language is mathematical analysis. In other words, as soon as the experimental basis is established, the study of geom-

[1] Cambridge Tracts in Mathematics and Mathematical Physics, No. 29, The Foundations of Differential Geometry, by Oswald Veblen and J. H. C. Whitehead, Cambridge University Press, 1932.

etry should proceed by the use of coordinates and the methods of analytic geometry.

The student, however, does not yet know enough about algebra and the use of numbers. Hence geometry and algebra should be learned together. The theory of the real number system and one-dimensional geometry overlap at every point and each helps in understanding the other. Let us limit our geometry at first to a single dimension.

One of the first experiments in geometry is to lay off equal intervals along a straight line with the aid of a graduated ruler. It is an important result of these experiments that a sequence of equally spaced points on a line can be marked with the whole numbers. The observation that this process can also be carried out in the reverse direction shows the necessity for the negative numbers, and for the number zero.

The logical thing to do at this stage is to give a set of postulates for the system of positive and negative numbers. The postulates are simply the rules for adding and multiplying the numbers and arranging them in order of magnitude.

A similar process of experimentation would acquaint the student with the rational fractions and with their use as labels for denoting points on a straight line. Here would be the occasion for a lot of arithmetic and algebra as well as of the geometry of the straight line.

This is the place for the postulates for the rational numbers. Also for the postulates of one-dimensional geometry. The chief postulate of one-dimensional geometry is that the points of a line can be named by numbers in such a way that the distance between the point named a and the point named b is the absolute value of the number $b-a$. By limiting ourselves to one dimension we have avoided the complicated formula (1).

It will be found that a very respectable body of arithmetic, algebra, and geometry can be made systematic at this stage. But there are a variety of experiments which make it clear that the number system is still capable of extension. Logically here is the place for the postulates of the real number system. These postulates state (1) that the numbers are subject to relations of greater and less, (2) that they can be added according to certain rules, and (3) that they can be multiplied according to certain rules.

It is a problem for the elementary teachers how explicitly these postulates should be presented to young students. I should say that they are easier to grasp than many of the abstract processes now studied in school courses in geometry.

The point that I should like to emphasize most, however, is that in whatever way we formulate the postulates they should not be presented as dogmatically as mathematicians used to present them. We have seen that an examination of the physical basis for our assumptions induces anything but a dogmatic frame of mind. But dogmatism has also suffered a severe blow from the side of pure mathematics.

I referred some time ago to the seemingly impregnable position which analysis had attained at the end of the last century, how everything was carried back by a series of constructive definitions and existence theorems to the solid ground of the whole numbers. But these constructions and existence theorems involve the free use of infinite sets of objects and the application to infinite classes of a logic which is generalized from the logic of finite classes. Doubts as to the validity of this logic have been growing more and more definite during the last three decades. The well-known paradoxes about infinite classes were the first stage of doubt. The difficulties which arose in the attempt by Russell and Whitehead to carry out the constructive definitial theory of mathematics in a systematic way in their *Principia Mathematica* increased the feeling of skepticism. Finally a most serious attack has come from L. E. J. Brouwer, who rejects entirely any existence theorem which does not exhibit explicitly the object whose existence is to be proved, in a very narrow sense of the words "exhibit explicitly." He goes back to a question which had lain dormant since the time of Aristotle and finds that the principle of the excluded middle is unjustifiable in many of the cases in which it has been used by mathematicians. It is not possible for me to go into Brouwer's critique here and now—it was expounded here three or four years ago by Professor Weyl[1]—but the outcome of it is that we cannot any longer feel confident about the constructive theory of the real number system or about the main existence theorems of analysis.

The same sort of a result seems to be coming out of the attempts by Hilbert and others to prove the consistency of mathematics. The consistency of geometry could be reduced to that of the real number system, that of the real number system could be carried back, by the constructive processes to which we have referred above, to the whole numbers. So the problem reduces to that of the consistency of the system of logic. But this logic is a logic which employs infinite processes and classes. Hilbert's idea was that these infinite classes and the other concepts used are *ideal elements* (just as a point is an ideal element) introduced because of their convenience. But we use only a finite number of signs to denote the ideal elements of logic, and we operate with these signs according to definite rules. Is it not possible to have a sort of behavioristic theory of these operations, to look on from the outside and determine what can be done with these signs according to these rules? If we can show

[1] *The Rice Institute Pamphlet*, Vol. XVI, No. 4, 1929.

that the person who operates with these signs according to these rules can never arrive at both "A" and "not A," we will have proved the consistency of mathematics. But no such proof has been forthcoming and the efforts to find one have made us more than ever conscious of the difficulties inherent in the constructive theory of the real number system.

It seems to me that these doubts and difficulties must profoundly affect anything that we say about the real number system. The clearest method, in my opinion, of saying what we mean by the real number system is by means of a set of postulates such as I have already indicated—that is to say, the real numbers are a set of objects satisfying certain conditions of order and subject to certain rules of addition and multiplication. This is what I would suggest not only for beginning students, but, even more confidently, for a graduate course in functions of real variables. In a course of the latter sort it is quite usual to base everything on the Dedekind-Cantor-Weierstrass constructive theory of irrational numbers. I would present this theory for what it is worth as a proof of the consistency of the postulates for the real number system. But any such presentation should be accompanied by a discussion of the logical difficulties inherent in the whole process.

These difficulties are also present in anything that we do with the postulates. They inhere in each existence theorem. But when we use a set of postulates we make no such sweeping claim to have done away with assumptions drawn from intuition and experiment as has been made in connection with the constructive theory. We simply say what some of our assumptions are and then go ahead with the usually accepted processes of Analysis. The critique of these processes which is under way at the present time is a separate enterprise which ought not to confuse the course in analysis, though it ought to influence it in the direction of greater caution and less dogmatic confidence in its foundations.

But let us return to elementary geometry. After arriving at a number system which includes surds as well as rational numbers, the postulates of one-dimensional geometry remain verbally the same as before but have as much more content as the number system is more extensive. At this stage the notion of measurement and of scale should be well established. Slide rules for addition and multiplication might well be in use.

It is now time to bring on a new series of experiments and observations the result of which is to establish that there is a way of naming the points of a plane by means of numbers in such a manner that each point has two names, a first name x and a second name y—(x, y) is the name in full—and such that the names of the points of any straight line satisfy an equation of the first degree.

In order to complete the plane geometry one more axiom is needed, namely, an axiom specifying a class of coordinate systems in which the distance of any two points is given by the formula (1). The axioms about the straight line are not independent of this one, but I judge that in an elementary course it is advisable to do what is possible with linear equations first, and then to go on to quadratic problems.

Obviously, the development of the propositions of geometry from these foundations should be closely related to the study of elementary algebra, linear and quadratic equations, and the like. Moreover, I ought to guard against one possible misunderstanding. The working out of this program does not mean the elimination of synthetic proofs from geometry. There are plenty of cases in which a synthetic or a mixed proof is easier than a purely analytic one. In such cases I would use the simplest and most direct process which I knew. The result would be, I am confident, that the student would have as good a grasp of synthetic methods as at present, and a much better idea of what it is all about.

May I take just a few minutes more to recapitulate? I have tried to point out a number of major movements of scientific thought which I think should influence our attitude toward elementary geometry and the way it is presented. These are:

1. The discovery of non-Euclidean geometries.
2. The group-theoretic classification of geometries.
3. The arithmetization of mathematics.
4. The axiomatization of geometry.
5. The demand of physics for more sophisticated geometries.
6. The advance of analysis.
7. The revision of mathematical logic.

The thesis I have tried to advance is that although we can now see Euclidean geometry more clearly than ever before as a distinct subject capable of being treated without reference to analysis by its own peculiar and highly elegant methods, nevertheless the spirit of our time requires that we should present it as an organic part of science as a whole. This requires the use of analytic methods from the first, emphasis on the approximate and provisional nature of its results when interpreted physically, and a non-dogmatic attitude throughout both as to physical and as to logical questions.

When we mathematicians state our results they necessarily sound very matter-of-fact, and yet we are often not matter-of-fact people at all. When we are talking in general terms, as on the present occasion, we are apt to get out of bounds.

Let me take advantage of this freedom and close with a completely speculative remark.

In indicating the sort of axioms which might be used I have considered a coordinate system as a one-to-one correspondence between points and ordered sets of numbers. This is the way a coordinate system figures in all branches of geometry at the present time. But the development of the quantum theory has several times suggested that mathematicians may be called on to devise a geometry in which there are no points. It may (or it may not) turn out that the process of infinite subdivision of chunks of space which is supposed to yield the concept of a point is physically impossible. If we come to such a geometry without points and without infinite resubdivision, it is likely that we will continue to use analytic methods and coordinate systems. But our coordinate systems will no longer be one-to-one correspondences between points and sets of numbers. They will be relations of some other type between sets of numbers and the entities which they describe.