NON-EUCLIDEAN GEOMETRY.

The Elements of Non-Euclidean Geometry. By D. M. Y. SOMMERVILLE. London, G. Bell and Sons, 1914. 16mo. xvi+274 pp.

FEW recent writers upon non-euclidean geometry have approached their task with better chances of success than attended Dr. Sommerville in the preparation of the present volume. Anyone who has seen his scholarly and painstaking "Bibliography of Non-Euclidean Geometry"* will realize that in so far as a knowledge of what others have written upon a subject is a desirable qualification, the present author was most fortunately placed. Furthermore he is the happy possessor of an excellent literary style. A book written by such a writer should be interesting and stimulating; the present book has both of these characteristics. The choice of material is admirable, and the narrative continually illumined by historical notes.

When the fairies were invited to the christening of the Sleeping Beauty one of the sisterhood was unfortunately overlooked, and her absence caused all the trouble that came afterwards. So here, one thing is lacking, singleness of aim. Says the author (page vii):

"It is hoped that the book will prove useful to the scholarship candidate in our secondary schools who wishes to widen his geometrical horizon, to the honours student at our universities who chooses geometry as his special subject, and to the teacher of geometry in general who desires to see how far strict logical rigour is made compatible with a treatment of the subject matter capable of comprehension by schoolboys."

Does not this programme spell "failure" from the start? Complete rigor and a treatment comprehensible by schoolboys, even by Scotch ones, who indubitably work harder and know more than Americans of like age, are so far incompatible that it is quite useless to make the attempt. The needs of the schoolboy and of the candidate for honors are so different that a book intended for both will suit neither. In the present work if we confine ourselves to the first four chapters,

* London, Harrison, 1911. Reviewed in the BULLETIN, vol. 18, Feb., 1912.

which the author declares (page vi) constitute the rudiments of the subject, and omit all material in fine print, we still find such topics as the exponential function with a complex argument, hyperbolic functions, imaginary points and lines, the differential equation

$$\frac{d^2a}{dr^2} + \frac{a}{k^2} = 0,$$

the bizarre integral

$$\int_0^c k \sin \frac{c}{k}\, dAdc,$$

the plane at infinity, and the desmic configuration. We have yet to find any actual schoolboy who was able to make very much of such expressions. But if we consider the book from the point of view of the "honours student" we observe that he will find a romantic optimism in many statements and proofs altogether at variance with what he is learning in his analysis and his algebra. The author's disclaimer (page vi) of any attempt to make the book rigorously logical with a detailed examination of all assumptions is no sufficient excuse.

These are surely serious charges to bring against any author, especially against one so well equipped as is Dr. Sommerville. Let us try to sustain them in detail, without losing sight of the various attractive features which the book surely possesses.

The first chapter is purely historical and deals with the landmarks in the history of non-euclidean geometry, much as they are described in a score of books. Historical or not, the author is true to his British didactic instinct and closes this chapter, like all the others, with a number of examples for the student to work out. Surely the British text-book writers lead the world in this respect. Says Cremona in the preface to the English edition of his Geometria proiettiva:*

"Unless I am mistaken the preference given to my Elements over the many treatises on modern geometry published on the continent is to be attributed to the circumstance that in it I have striven, to the best of my ability, to imitate the English models. . . . I aimed therefore at simplicity and clearness of expression, and I was careful to supply an abundance of examples of a kind suitable to encourage the beginner."

If previous writers of text-books on non-euclidean geometry have omitted such examples, was it from conviction or laziness?

* Second edition, Oxford, 1893, p. xiii.

In Chapter II we begin the systematic treatment of the hyperbolic plane. The writer says little about axioms, except to give Hilbert's classification into axioms of connection, order, congruence, continuity, and parallelism. It is by no means easy to say how the author wants us to treat these axioms. The inference is that we are to accept all save the last, for we read (page 27): "We shall assume as deductions from them the theorems relating to the comparison and addition of segments and angles," and two pages later we have Pasch's axiom that a line which meets a side of a triangle and a second side produced meets the third side. But this axiom depends for its statement upon the axioms of order, i. e., the axioms of an open order. Yet if we accept an open order at the outset why trouble ourselves at all about the elliptic plane where a straight line has a closed order and where Pasch's axiom may not be true? The fact is that the whole axiom question is beset with difficulties. If a writer who has not had the needful special training undertake to make up his own set of axioms, he is likely to make a botch of it; if he accept uncritically a set which some one else has developed, he is in grave danger of running into contradictions.

"Revenons à nos moutons." The first dozen pages of Chapter II go to a discussion of parallel lines in the hyperbolic plane and give the important theorems in good form. All is clear and well defined. The first break occurs on page 39 where we read:

"*Two parallel lines can therefore be regarded as meeting at infinity, and further the angle of intersection must be considered as being equal to zero.*"

We find further on (page 46):

"We shall extend the class of points by including a class of fictitious points called points *at infinity*. These points function in exactly the same way as ordinary, or, as we shall say, actual points. . . . On every line there are two points at infinity."

Our comment on these statements is as follows: We only know two ways of extending the class of points to include new members. We may define the new points by means of already recognized figures, as for instance, we might define a "point at infinity" as the totality of lines parallel to a given line and to one another, or, secondly, we might define a "point at infinity" by a set of postulates as

There exists a class of P.I.'s.

Each line contains two P.I.'s.

Each point and each P.I. determine a line.

Every two P.I.'s determine a line.

Starting with either of these methods we may go on to define lines and planes at infinity, and then similarly ultra-infinite points, lines and planes. Either plan is permissible, neither is entirely simple. But when our present author tells us (page 48) that a bundle of lines perpendicular to a plane *have* an ideal vertex, and further that "*ideal points* thus introduced behave exactly like actual points" we are left wondering. They surely do if we confine ourselves to projective properties, but if, perchance, we seek the distance from an actual to an ideal point we are in very serious difficulty.

There are two other points to be noticed while we are upon these thorny pages of the book. We read (page 41):

"Thus the distance between the two given lines AB' and LX first diminishes and then tends to infinity. It must therefore have a minimum value." This is, of course, a pure assumption and should either be proved, or made explicitly. Then we read in the note to page 46:

"The definition of a *conic* which we shall use is 'a plane curve which is cut by any straight line in its plane in two points.' For the explanation of the case of 'imaginary intersections' see Chapter III, § 5."

Here, if we overlook the removable objection that a tangent meets a conic in but one point, we still wonder what is the author's definition of a curve. If he mean an analytic curve, the definition for a conic is entirely proper. For if we take the origin upon such a curve and the axes parallel to the asymptotes, the abscissa and ordinate of every other point will be analytic functions of the slope of the line from the point to the origin, and these functions are single valued, and have single valued inverses. Hence we may express the curve in the form

$$x = \frac{al + b}{cl + d}, \quad y = \frac{a'l + b'}{c'l + d'}, \quad l = \frac{y}{x},$$

and we have the usual conic of commerce. But in the present book there is, up to the present point, no machinery for an exact statement of this sort and as for the method of introducing imaginaries, well, we shall deal with that presently.

The last part of the chapter goes to the development of trigonometry following the classical methods of Lobachevsky which depend upon the parallel angle. Here everything is elementary, though at times a bit involved. Moreover, it is stated upon page 58 that the solution of

$$f(x + y) = f(x)f(y)$$

must be $f(x) = c^x$. This is true if, and only if, the restriction be imposed that $f(x)$ shall be a continuous function not $= 0$.* Our author can not have been ignorant of the necessity for this restriction, since on page 114 in discussing the corresponding equation for $\cos x$ he mentions the requirement of continuity. Query, how many schoolboys will understand what a functional equation means anyway? The chapter ends with an ingenious discussion of the relation between the defect of a triangle and its area.

Chapter III is given to elliptic geometry. This must be worked out "ab initio" since the methods of hyperbolic geometry, based on the parallel angle, are inapplicable. A curious omission occurs (page 89) where the author recognizes that if a straight line be a closed curve, two coplanar lines might intersect once or twice, but fails to show that they might not have more than two intersections. As a matter of fact the proof is a bit delicate. He rightly appreciates that the most attractive feature of elliptic geometry is the perfect duality between point and plane, angle and distance. To bring this out he needs projective geometry, and since he deems it unwise to assume a knowledge of the latter on the part of his readers he gives a short introductory account of the subject on pages 93–98. This is no easy task. The idea of a cross ratio is fundamental in projective geometry, but the elementary definition in terms of the ratios of certain segments is invalid in non-euclidean space, while (wrongly, as we believe) he considers that a definition based upon successive quadrangle and quadrilateral constructions is too difficult or abstract. He therefore adopts the following unhappy expedient. We read (page 94):

"If two ranges of points are made to correspond in such a way that to every point P on one range corresponds uniquely a point P' on the other and *vice versa*, the ranges are said to

* Conf. Wilson, "Note on the function satisfying $\phi(u)\phi(v) = \phi(u + v)$" *Annals of Mathematics*, series 2, vol. 1, 1899, and Darboux, "Sur le théorème fondamental de la géométrie projective," *Math. Annalen*, vol. 17 (1880).

be *homographic,*" and on page 95: " It can be proved that two homographic ranges can always be connected by a finite number of projections and intersections." This familiar statement has always reminded us of the sea-serpent. It is based upon an illusion, but is of venerable antiquity, is believed in by many, and is indestructible. Yet there is no difficulty in showing its falsity. Let us ask the writer in what domain we are supposed to be. If the answer be the real domain, since nothing yet has been said about imaginaries, we ask him how many projections and intersections are necessary to accomplish the transformation

$$x' = x^3.$$

If, however, we are in the complex domain, we have the classical example

$$x' = \bar{x},$$

i. e., each point of a line corresponds to its conjugate imaginary. Of course the statement is true if the domain be complex, and the transformation analytic, but these are just the restrictions which are usually overlooked.

The imaginary first appears explicitly on page 97 where we read, in discussing involutions: " If two real self-corresponding points do not exist we introduce by definition conjugate pairs of 'imaginary' points, much in the same way that ideal points were introduced in hyperbolic geometry." With this epigrammatic statement the author leaves the matter; he now feels just as free to use imaginary points as real ones.

It is fair to say that this happy solution of the imaginary difficulty is not new. The principle, so far as we understand it, is this. If by a continuous change we can make two real points coalesce and then disappear we are free to say that they have been replaced by two imaginary points, and repel with scorn all inquiries as to the nature of these latter. Let us make a careful drawing of the curve.

$$(x^2 + y^2 + 1)^{1492}(y - x^3) + \epsilon = 0,$$

where ϵ is microscopic. This curve will lie extremely near to

$$y = x^3.$$

Some lines will meet our curve in three real points, others in only one. Shall we, therefore, say that the latter meet the curve in one real and *two* imaginary points? If our pupils be too young to comprehend the truth in these matters, let us, at least, tell them nothing else.

A slight slip of a different sort occurs on page 99, where we read "Similarly, in three dimensions a polar system determines a surface of the second degree, or quadric surface." The author has clearly forgotten the null system. The remainder of the chapter is devoted to paratactic lines, Clifford surfaces, and trigonometric relations. The subject is interesting and the treatment good, although we find on page 111 the erroneous statement "*all the common tranversals of two right paratactic lines are left paratactic lines.*"

The fourth chapter is given to analytic geometry. A point in the plane is determined by its Weierstrass coordinates, namely

$$x = k \sin\frac{r}{k} \cos\theta, \quad y = k \sin\frac{r}{k} \sin\theta, \quad z = \cos\frac{r}{k},$$

where r is the distance from the origin, θ is the angle between the radius vector and the positive half of the X axis, and k the space constant. The subject matter is beset with fewer pitfalls than that which preceded, and the handling is satisfactory. The chapter closes with the account of two configurations, the triangle configuration of Desargues and the desmic tetrahedron configuration of Stéphanos.

A distinct change comes over the book with Chapter V. This excellent chapter is given to various representations of non-euclidean space in euclidean space. Three such representations are discussed. First we have the projective representation, and Cayley's projective metric; secondly the geodesic representation where the non-euclidean plane is developed on a euclidean surface of constant curvature, and lastly the conformal representation where non-euclidean lines appear as euclidean circles orthogonal to a fixed circle. Our only criticism is the mild wish that the chapter had been a little longer, and that the author had pointed out more clearly that in some cases we are able only to develop a part of our space.

Our author has next the curious idea of interpolating another philosophical chapter, to bring the history of non-euclidean geometry to date, and to discuss an interesting philosophical question. These are rather deep subjects to be disposed of in a twenty page chapter, and the treatment of the modern abstract view of geometry is scant. The particular philosophical question is this: is our space of experience euclidean or non-euclidean? The author asks (page 203):

(1) Can the question of the true geometry be settled a posteriori or experimentally?

(2) Can it be decided on philosophical grounds?

(3) Is it, after all, a proper question to ask, one to which an answer can be expected?

Taking these up in turn he first shows that if the space constant be not infinite, it is so large that we shall never make any serious error in assuming that our space is euclidean. The second question is easily disposed of: no one believes nowadays that we can settle the question of parallels a priori. The third idea, that the question is improper, and that we can no more say that the parallel axiom is true or false, than that the metric system is true or false has the great authority of Poincaré, and we believe that Dr. Sommerville inclines to a like opinion. He puts the case as follows (page 209):

"A further point—and this is the 'vicious circle' of which we spoke above—arises in connection with the astronomical attempts to determine the nature of space. These experiments are based upon the received laws of astronomy and optics, which are themselves based upon the euclidean assumption. It might well happen then, that a discrepancy observed in the sum of the angles of a triangle could admit of an explanation by some modification of these laws, or that even the absence of any such discrepancy might still be compatible with the assumptions of non-euclidean geometry."

This is rather a fascinating idea. It is evident, for instance, that if light were not propagated along straight lines, it would be hard to determine the parallel question astronomically. However, leaving aside the minor doubt as to just how far our science of physical optics *is* dependent upon the parallel postulate, the root of the difficulty seems to us that the argument is a little too strong. Could we not render all scientific deduction impossible by similar considerations? If we make any experiment whatever, the results can be interpreted in a transfinite number of ways and the best that we can hope for is to find what conclusions are compatible with various accepted hypotheses. Let us take a simple question. Are there any mountains on the other side of the moon? At present it seems hard to find a crucial experiment to settle the matter. Suppose, however, that in the course of centuries we set up some sort of wireless telegraphy with those eminent engineers who superintend the construction of canals on Mars, and put the question to them. We may well imagine their replying " We do indeed observe there markings which are in every way similar to those markings on the earth to which you give the name of Himalayas." What conclusion should we then draw? It might be that the laws of optics for the Martians were different from our own, it might be that whereas rays of light reflected from the earth travelled in straight lines those reflected from the other side of the moon pursued spirals. Endless other conjectures are possible. The obvious conclusion is, however, that there are such mountains, and that no vicious circle was involved in asking the question.

The seventh chapter is devoted to circles, and in fact, the amount of attention paid to circles and circle transformations is a distinguishing characteristic of the book. The treatment is greatly clarified by the use of a transformation first given analytically by Hausdorf* and later synthetically by Liebmann.† Confining our attention to the real and actual domain of the hyperbolic plane, we take a ground plane F. Then if E be any other actual plane, its intersection with the absolute quadric will be orthogonally projected upon F into a circle, if we include under this head not only circles with actual centers, but horocycles and equidistant curves. Conversely, every such curve can be reached in this way. This being shown, our author says (page 230):

" Hence there is a (1, 1) correspondence between the circles in a plane and the planes in space "

" Ahimé! " before saying this why did he not try to run his machine backwards? Had he done so, instead of taking Liebmann's dictum, he would have perceived that two planes which lie symmetrically with regard to F give the same circle therein, so that the correspondence is not (1, 1) but (1, 2). Hausdorf, proceeding analytically, points this out with care‡ and subsequently changes it to a (1, 1) correspondence by covering the plane over with two different layers of points.

* "Analytische Beiträge zur nicht-euklidische Geometrie," *Leipziger Berichte*, vol. 51 (1899).

† "Synthetische Ableitung der Kreisverwandtschaften, etc.," ibid., vol. 54 (1902).

‡ Loc. cit., p. 177.

It is curious that this mistake was not brought to our author's notice by the following inconsistency. He points out (page 218) that two circles may intersect in four (real) points, the resulting angles being equal in pairs, but warns us at the top of the next page that the four angles are not, in general, equal. We then reach this interesting theorem (page 231):

The angle between two planes is equal to the angle of intersection of their marginal images.

The marginal image of a plane is the circle which corresponds in the present scheme. Of course the correct theorem is this:

Given two circles in F, the one represented by the planes E_1E_2, the other by G_1G_2. Two angles of these circles will be equal to the angle of the planes E_1G_1 (or E_2G_2), the other two will be equal to the angle of the planes E_1G_2 (or E_2G_1).

Chapter VIII discusses circular transformations both geometrically and analytically. Part of the analytic work seems a bit messy; the geometrical treatment is ingenious, especially the proofs of the theorems that every circular transformation of the non-euclidean plane is conformal, that it can be factored into the product of a congruent transformation and an inversion or radiation, and that every circular transformation of the plane corresponds to a congruent transformation of space. These are valuable theorems which we are glad to see in such a work. Unfortunately we must note one more oversight, analogous to those which marred the chapter preceding, and common to most writers upon this subject.*

Let us take what our author calls (page 241) a *hyperbolic inversion*. Here corresponding points P and P' are collinear with the center of inversion O, the product of the hyperbolic tangents of the halves of their distances from O is constant, and they *are on the same side of O*. The analogy to euclidean inversion seems complete, for here we may define inverse points as being collinear with O, the product of their distances therefrom is constant, and *they are on the same side of O*. There is, however, an important distinction. The requirement that the two points should be *on the same side of O* is not

* We make an exception in favor of a really careful article by Beck, "Ein Seitenstück zur Möbiusschen Theorie der Kreisverwandschaften," *Transactions Amer. Math. Soc.*, vol. 11 (1910). Apparently Dr. Sommerville was not familiar with this article, at any rate there is no mention of it in his bibliography. Beck's circles are oriented, and treated as the envelops of oriented straight lines. His paper might equally well have been called a Seitenstück to Laguerre's "Géométrie de direction."

an analytic requirement. In the euclidean plane it is unessential, for we may also define inverse points as being collinear with O and conjugate with respect to the circle of inversion. In the non-euclidean plane we have not this refuge, and, analytically speaking, a point in general position has *two inverses*. We bring out this important fact as follows.

We may map the upper half of the euclidean plane ($y > O$) upon the actual domain of the hyperbolic plane in such a way that the correspondence is one to one, conformal, and carries circles to circles; i. e., a circle in the hyperbolic plane will correspond either to a circle in the euclidean plane, or to a part of such circle and the reflection of the rest in the x axis. Let P be a point of the upper euclidean half-plane and c_1 any circle entirely in that half-plane. Let P' and c_1' be the corresponding point and circle of the hyperbolic plane. Then the inverses of P' in c_1' will be the points which correspond to the inverse of P in c_1 and that which corresponds to the reflection in the x axis of the inverse of P in the reflection of c_1. We may go a step further in this direction, and announce a theorem which is, perhaps, new.

The only analytic circular transformations of the non-euclidean plane which are single valued are congruent transformations.

It is ridiculously easy to prove this analytically, but perhaps it will be more sportsmanlike to follow the methods of the book before us. Since our transformation carries a point to a point, a circle to a circle, and the points of a circle to points of a circle for our plane F, it is (page 237) a conformal transformation. Hence a circle orthogonal to itself will go into another such, i. e., a null circle into a null circle. In space we have a transformation of plane to plane, which carries a plane tangent to the absolute into another such plane, in fact a congruent transformation of space (page 238). Since the transformation of F is to be one-to-one, two planes tangent to the absolute which correspond to the same null circle in F, i. e., two planes tangent to the absolute whose common line is in F, will correspond to two other such planes. Here a line in F is carried into a line in F, i. e., our congruent transformation carries F into itself.

The concluding chapter goes to a classification of non-degenerate conics under the non-euclidean congruent group. The subject matter is not particularly novel, and the treatment calls for no special comment.

If we were to pass a final judgment upon the book before us it would be this: It is in some respects an improvement upon its predecessors; it is by no means the best book that the present author could have written.

J. L. COOLIDGE.

CAMBRIDGE, MASS.,
December, 1914.

MINKOWSKI'S WORKS.

Gesammelte Abhandlungen von Hermann Minkowski. Herausgegeben von D. HILBERT. Leipzig, B. G. Teubner, 1911. Band 1, xxxi + 371 pp. Band 2, iv + 465 pp. Two portraits.

MINKOWSKI's work divides itself naturally, and his collected works are divided, into four parts: Theory of quadratic forms, 242 pages, Geometry of numbers, 230 pages, Geometry, 180 pages, Physics, 163 pages. In addition to this the volumes before us contain the author's address on Dirichlet, 15 pages, and Hilbert's commemorative address on Minkowski, 27 pages. This heartful and touching tribute of a life-long friend and fellow-worker is in reality also a critical review of Minkowski's great achievements in mathematical science, and it may be that the best thing for us to do in reviewing these volumes would be to follow the example of the reviewer in another Bulletin* and translate the chief portions of that address. The availability of the address in the original, where it should be read as a whole, and, in abstracts, in French makes repetition here seem really unnecessary.

We are accustomed to precocious exhibitions of genius in mathematicians, and we often cite the case of Galois, who died in his twenty-first year after accomplishing work of which the fundamental importance was not and perhaps could not be appreciated until a much later date. Minkowski in his eighteenth year submitted to the Paris Academy a memoir on quadratic forms with integral coefficients which fills 142 pages of his collected works and which received the Grand Prix des Sciences mathêmatiques. Measured in pages, one-sixth of Minkowski's work was written before he was 18. His work of the next ten years deals almost exclusively with quadratic forms.

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* *Bull. Sci. Math.*, France, vol. 36 (1912), p. 73.