Manson's theory of the ice age has been favorably received by some eminent geologists. Thirteen years ago, shortly after Manson's memoir entitled 'Geological and Solar Climates' was first published, I wrote,[1] from an astronomer's point of view, as follows:

Under the above title Dr. Marsden Manson has published a thesis, issued by the University of California, of more than ordinary merit. Geologists tell us that large areas of now densely populated regions of the earth were at one time covered with ice to a depth of many feet. To most scientists the explanations hitherto given, to account for the cause of the so-called *Glacial Epoch*, seem wholly inadequate. Dr. Manson's treatment of the problem is unique, and to many it will appear quite convincing. We do not hesitate to recommend it for careful study to those interested in astro-geological physics.

I now copy, word for word, the last paragraph of a recent paper entitled 'The Causes of the Glacial Epoch,' written by a recognized leader in science. He concludes as follows:

It does seem to the writer that unless it can be shown that the temperature prevailing at the beginning of the glacial epoch could not have been high enough to maintain a cloud envelope, Manson's theory as outlined above must be considered as the most probable among those that have heretofore been suggested, as fulfilling both qualitatively and quantitatively the postulates of the great Ice Age; not excluding of course the probable influence of the agencies claimed by Arrhenius and Chamberlin as the chief ones, but which appear to the writer to be inadequate to account for the phenomena in actual evidence.

Such is the testimony of a geologist of world-wide fame.

J. M. Schaeberle.

Ann Arbor,
August 30, 1906.

[1] See No. 32, 'Publications of the Astronomical Society of the Pacific.'

NON-EUCLIDEAN GEOMETRY.

To the Editor of Science: My attention has been called to some quotations from a private letter of mine in an article by Professor George Bruce Halsted on 'The Value of the Non-Euclidean Geometry,' which appeared in the November number of the *Popular Science Monthly*, 1905. The letter referred to was written by me to the author in answer to a query of his of March 21, 1904, couched in the following words:

I am *curious* to know, if in the face of such a statement as Poincaré's in his review of Hilbert, 'The postulate of Euclid then can not be demonstrated; and this impossibility is as certain as any mathematical truth whatsoever,' you actually still think that you have proved it, or that you have proved that external space is necessarily Euclidean.

In view of the fact that the quotations do not adequately express my views, I beg you for the privilege of being granted some of your valuable space for the publication of my letter in full. The true copy of my letter dated March 25, 1904, follows. The quotations are enclosed in brackets:

My dear Professor Halsted—Your letter of the 21st inst. has just reached me. From its tone I conclude that you are in earnest about the matter, and I am glad to have found in you a man who intends to read the work. The dissertation was written for the purpose of bringing before the mathematical world certain contentions—no matter how seemingly heterodox—for which a scientific basis is claimed to have been laid down in the new treatment and in the new point of view; and, of course, if the claim is not well established, then either the treatment or the point of view is open to criticism—and *fair criticism*, whether favorable, or *unfavorable*, is cordially invited, even solicited. [As to Poincaré's assertion about the impossibility of proving[1] the Euclidian postulate, it is no more than a belief—though an enthusiastic one—never proved mathematically, and in its very nature incapable of mathematical proof,] unless we are certain that space is non-Euclidian. [Poincaré is undoubtedly a great mathematician, perhaps the greatest now living; but his assertion of his inmost conviction, no matter how strongly put, can not pass for mathematical truth, unless *mathematically* proved. His conclusion—shared also by many another noted mathematician, as well as by the founders of the non-Euclidian geometries—can only be based on the fact of the existence of these last geometries, self-consistent and perfectly log-

[1] I stand corrected with regard to the germanisms, 'impossibility to prove,' 'impossibility to establish,' which appeared in the original text of the letter.

ical. But this is a poor proof of the impossibility of establishing[1] the Euclidian postulate,] since the non-Euclidian systems have to deal with a different class of phenomena; such are the metrical relations upon the sphere and the pseudosphere in two-dimensional point-space, and those holding in three-dimensional curved manifolds contained in n-dimensional space, or in space whose element is changed from that of a point in the ordinary Euclidian sense to some other geometrical entity depending on n coordinates, like Plücker's four-dimensional line-space. I should refer you for the elucidation of this point of view to pp. 27–32 of the dissertation, especially to p. 29 and sequel, where a quotation from Bianchi is discussed and refuted.

The difference between my position and yours is, it seems, as follows: while you maintain that external space is either Euclidian or non-Euclidian, and there is no possibility of ever finding out which, for the Euclidian postulate can neither be proved nor disproved, I assert that external space is both Euclidian and non-Euclidian, according to the point of view. [If space is regarded as a point-manifold, it is Euclidian, and the postulate can be proved, as soon as we are allowed to look for its establishment in three-dimensional geometry,] of which two-dimensional geometry is only a part. If space, however, is regarded as a line-manifold, say, then *certain* two- and three-dimensional manifolds contained in it are non-Euclidian. So, for instance, all lines passing through a point represent [the two-dimensional elliptic geometry discussed by Klein, Lindemann and Killing], which, [according to my opinion, is an absurdity for a point-space in the ordinary sense of the term]. As to [Poincaré], he seems to stand on a very similar basis—namely, in that he does not oppose the non-Euclidian to the Euclidian geometry and [says that all depends upon convention] as to what we understand by distance, straight line, angle, etc. [But still he deduces from this the perfectly gratuitous conclusion that therefore the parallel-postulate can not be proved.] It is gratuitous, according to my opinion, because, as the simultaneous existence of both the Euclidian and the non-Euclidian groups of motion have been proved beyond a shadow of doubt, they must evidently refer to different classes of phenomena, and hence there must exist a Euclidian space and a non-Euclidian space. And as the actual space is only one, all must depend upon the point of view (the entity taken as the space element). Therefore, for point-space the postulate may be a necessity, without involving its necessity for other three-dimensional manifolds, like certain line-complexes, for instance,—just as plane geometry, even if it were admittedly Euclidian, would not have to hold for the geometry of the sphere or the pseudosphere.

You will observe that the groups of motion in Lie's treatment are deduced from the assumption of an *analytical* point, that is some entity depending upon a certain number of coordinates $x_1, x_2, \cdots x_n$, and, evidently, the entity in this case is indeterminate. You may call it point, but it may actually correspond to something quite different from what we understand by this name in *elementary geometry.*

I trust that, according to the maxim that *curiosity* is the mother of all knowledge, the perusal of my treatise, in pursuance of the gratification of this laudable feeling, may change your attitude upon this question, and will convince you that, instead of the different systems of geometry warring with each other, they are actually in peace,—the non-Euclidian systems, however, still needing interpretation in *many particulars*—an interpretation realizable in *our* space, in the space in which all of us live and think and work and strive for perfection.

I. E. RABINOVITCH.

SPECIAL ARTICLES.

INHERITANCE OF COLOR COAT IN SWINE.

MR. Q. I. SIMPSON, the well-known swine breeder of Palmer, Ill., is conducting several series of crosses between different breeds of swine, the breeds thus far used being Tamworth (red), Yorkshire (white), Poland China (black with white points), the wild boar of Europe and Duroc-Jersey (red).

He bred a wild boar to a Tamworth sow, securing a large litter all much resembling the wild boar, having his color, snout, eyes, ears, length and size of legs, tail, shape of body, size, wildness and characteristic movements. From two of these hybrid pigs and a Tamworth boar he has secured three litters, each containing four pigs. What the usual litter of wild pigs is I do not know, but the Tamworth litter is usually eight or more pigs. The body color of these three litters is as follows: