Survey, stated that a fluviatile origin for the tertiary beds of the west was not considered, because their lacustrine nature was indicated by physiographic evidence.

Mr. Whitman Cross cited Blanford's description, published in 1879, of the Gondwâna beds in India, and pointed out that the conclusion, then announced, as to the probable origin of these and other beds in India had probably been overlooked by geologists quite generally. The same criteria applied to the tertiary and mesozoic beds of the Rocky Mountain region would lead to the conclusion that many of them were of fluviatile origin. Mr. Cross, however, questioned the value of the criteria employed by Blanford, Penck and Davis, and would give most weight at present to the extent and distribution of the formations in question, and their relation to continental areas.

Mr. Bailey Willis remarked that he had been in the habit of reasoning back from conglomerates in order to reconstruct former physiographic conditions. Thus the conglomerate of the Puget Sound Basin, covering perhaps 10,000 square miles, was formed by glacial streams in Pleistocene time. The Pliocene conglomerates of California are delta deposits and are associated with uplift. The Eocene conglomerate of Washington State was laid down at the foot of steep bluffs of granite. The Pottsville conglomerate, composed almost wholly of residual quartz and widely distributed, can have been derived only from a coastal plain where it had been concentrated by marine action, and thence distributed by marine or fluviatile currents.

Mr. G. F. Becker pointed out that a lake was often only an expanded river and suggested that a more useful distinction than that between lacustrine and fluviatile deposits, would be one between materials laid down in rapidly moving and in comparatively still water. Deposits laid down by streams have their particles imbricated in one dominant direction. Beach deposits are capriciously imbricated and their pebbles are asymmetric.

F. L. RANSOME,
DAVID WHITE,
Secretaries.

BIOLOGICAL SOCIETY OF WASHINGTON.

THE 319th meeting was held on Saturday evening, February 24th. W. A. Orton spoke of 'The Sap Flow of the Maple in Spring,' describing a series of experiments undertaken with a view of ascertaining the cause of the the phenomenon. The results showed that it was due to plant physics rather than plant physiology, and had a direct relation to temperature, the sap being expelled by the expansion, caused by warmth, of the gas contained in the wood cells. M. B. Waite described 'The Peach Orchards of Michigan,' stating that they were located on the eastern shore of Lake Michigan, this body of water having the effect of mitigating the temperature of the region. Most of the farms, the speaker stated, were comparatively small, running from fifty to eighty acres in size, but owing to the methods of cultivation they yielded a good profit. Various methods of cultivation were discussed and the speaker touched briefly upon the disease of the peach known as 'little peach.' Both papers were illustrated by lantern slides.

F. A. LUCAS.

DISCUSSION AND CORRESPONDENCE.

INFINITESIMALS.

TO THE EDITOR OF SCIENCE: Will you kindly accord me space for a few remarks about Infinity and Continuity which I seem called upon to make by several notes to Professor Royce's Supplementary Essay in his strong work 'The World and the Individual'? I must confess that I am hardly prepared to discuss the subject as I ought to be, since I have never had an opportunity sufficiently to examine the two small books by Dedekind, nor two memoirs by Cantor, that have appeared since those contained in the second volume of the *Acta Mathematica.* I cannot even refer to Schröder's Logic.

1. There has been some question whether Dedekind's definition of an infinite collection or that which results from negativing my definition of a finite collection is the best. It seems to me that two definitions of the same conception, not subject to any conditions, as a figure in space, for example, is subject to geometrical conditions, must be substantially the

same. I pointed out (*Am. Journ. Math.* IV. 86, but whether I first made the suggestion or not I do not know) that a finite collection differs from an infinite collection in nothing else than that the syllogism of transposed quality is applicable to it (and by the consequences of this logical property). For that reason, the character of being finite seemed to me a positive extra determination which an infinite collection does not possess. Dr. Dedekind defines an infinite collection as one of which every *echter Theil* is similar to the whole collection. It obviously would not do to say a *part*, simply, for every collection, even if it be infinite, is composed of individuals; and these individuals are parts of it, differing from the whole in being indivisible. Now I do not believe that it is possible to define an *echter Theil* without substantially coming to my definition. But, however that may be, Dedekind's definition is not of the kind of which I was in search. I sought to define a finite collection in logical terms. But a 'part,' in its mathematical, or collective, sense, is not a logical term, and itself requires definition.

2. Professor Royce remarks that my opinion that differentials may quite logically be considered as true infinitesimals, if we like, is shared by no mathematician '*outside of Italy.*' As a logician, I am more comforted by corroboration in the clear mental atmosphere of Italy than I could be by any seconding from a tobacco-clouded and bemused land (if any such there be) where no philosophical eccentricity misses its champion, but where sane logic has not found favor. Meantime, I beg leave briefly to submit certain reasons for my opinion.

In the first place, I proved in January, 1897, in an article in the *Monist* (VII. 215), that the multitude of possible collections of members of any given collection whatever is greater than the multitude of the latter collection itself. That demonstration is so simple, that, with your permission, I will here repeat it. If there be any collection as great as the multitude of possible collections of its members, let the members of one such collection be called the A's. Then, by Cantor's definition of the relation of multitude, there must be some possible relation, r, such that every possible collection of A's is r to some A, while no two possible collections of A's are r to the same A. But now I will define a certain possible collection of A's, which I will call the collection of B's, as follows: Whatever A there may be that is not included in any collection of A's that is r to it, shall be included in the collection of B's, and whatever A there may be that is included in a collection of A's that is r to it, shall not be included in the collection of B's. If there is any A to which no collection of A's stands in the relation r, I do not care whether it is included among the B's or not. Now I say the collection of B's is not in the relation r to any A. For every A is either an A to which no collection of A's stands in the relation r, or it is included in a collection of A's that is r to it, or it is excluded from every collection of A's that is r to it. Now the collection of B's, being a collection of A's, is not r to any A to which no collection of A's is r; and it is not r to any A that is included in a collection of A's that is r to it, since only one collection of A's is r to the same A, so that were that the case the A in question would be a B, contrary to the definition which makes the collection of B's exclude every A included in a collection that is r to it; and finally, the collection of B's is not r to any A not included in any collection of A's that is r to it, since by definition every such A is a B, so that, if the collection of B's were r to that A, that A would be included in a collection of A's that was r to it. It is thus absurd to say that the collection of B's is r to any A; and thus there is always a possible collection of A's not r to any A; in other words, the multitude of possible collections of A's is greater than the multitude of the A's themselves. That is, every multitude is less than a multitude; or, there is no maximum multitude.

In the second place I postulate that it is an admissible hypothesis that there may be a something, which we will call a *line*, having the following properties: 1st, points may be determined in a certain relation to it, which relation we will designate as that of 'lying on' that line; 2d, four different points being so determined, each of them is separated from one of the others by the remaining two; 3d, any three points, A, B, C, being taken on the line, any multitude whatever of points can be determined upon it so that every one of them is separated from A by B and C.

In the third place, the possible points so determinable on that line cannot be distinguished from one another by being put into one-to-one correspondence with any system of 'assignable quantities.' For such assignable quantities form a collection whose multitude is exceeded by that of another collection, namely, the collection of all possible collections of those 'assignable quantities.' But points are, by our postulate, determinable on the line in excess of that or of any other multitude. Now, those who say that two different points on a line must be at a finite distance from one another, virtually assert that the points are distinguishable by corresponding (in a one-to-one correspondence) to different individuals of a system of 'assignable quantities.' This system is a collection of individual quantities of very moderate multitude, being no more than the multitude of all possible collections of integral numbers. For by those 'assignable quantities' are meant those toward which the values of fractions can indefinitely approximate. According to my postulate, which involves no contradiction, a line may be so conceived that its points are not so distinguishable and consequently can be at infinitesimal distances.

Since, according to this conception, any multitude of points whatever are determinable on the line (not, of course, by us, but of their own nature), and since there is no maximum multitude, it follows that the points cannot be regarded as constituent parts of the line, existing on it by virtue of the line's existence. For if they were so, they would form a collection; and there would be a multitude greater than that of the points determinable on a line. We must, therefore, conceive that there are only so many points on the line as have been marked, or otherwise determined, upon it. Those do form a collection; but ever a greater collection remains determinable upon the line. *All* the determinable points cannot form a collection, since, by the postulate, if they did, the multitude of that collection would not be less than another multitude. The explanation of their not forming a collection is that all the determinable points are not individuals, distinct, each from all the rest. For individuals can only be distinct from one another in three ways: First, by acts of reaction, immediate or mediate, upon one another; second, by having *per se* different qualities; and third, by being in one-to-one correspondence to individuals that are distinct from one another in one of the first two ways. Now the points on a line not yet actually determined are mere potentialities, and, as such, cannot react upon one another actually; and, *per se*, they are all exactly alike; and they cannot be in one-to-one correspondence to any collection, since the multitude of that collection would require to be a maximum multitude. Consequently, all the possible points are not distinct from one another; although any possible multitude of points, once determined, become so distinct by the act of determination. It may be asked, "If the totality of the points determinable on a line does not constitute a collection, what shall we call it?" The answer is plain: the possibility of determining more than any given multitude of points, or, in other words, the fact that there is room for any multitude at every part of the line, makes it *continuous*. Every point actually marked upon it breaks its continuity, in one sense.

Not only is this view admissible without any violation of logic, but I find—though I cannot ask the space to explain this here—that it forms a basis for the differential calculus preferable, perhaps, at any rate, quite as clear, as the doctrine of limits. But this is not all. The subject of topical geometry has remained in a backward state because, as I apprehend, nobody has found a way of reasoning about it with demonstrative rigor. But the above conception of a line leads to a definition of continuity very similar to that of Kant. Although Kant confuses continuity with infinite divisibility, yet it is noticeable that he always defines a continuum as that of which every part (not every *echter Theil*) has itself parts. This is a very different thing from infinite divisibility, since it implies that the continuum is not composed of points, as, for example, the system of rational fractions, though infinitely divisible, is composed of the individual fractions. If we define a continuum as that every part of which can be divided into any multitude of parts whatsoever —or if we replace this by an equivalent definition in purely logical terms—we find it lends itself at once to mathematical demonstrations, and enables us to work with ease in topical geometry.

3. Professor Royce wants to know how I could, in a passage which he cites, attribute to Cantor the above opinion about infinitesimals. My intention in that passage was simply to acknowledge myself, in a general way, to be no more than a follower of Cantor in regard to infinity, not to make him responsible for any particular opinion of my own. However, Cantor proposed, if I remember rightly, so far to modify the kinetical theory of gases as to make the multitude of ordinary atoms equal to that of the integral numbers, and that of the atoms of ether equal to the multitude of possible collections of such numbers. Now, since it is essential to that theory that encounters shall take place, and that promiscuously, it would seem to follow that each atom has, in the random distribution, certain next neighbors, so that if there are an infinite multitude in a finite space, the infinitesimals must be actual real distances, and not the mere mathematical conceptions, like $\sqrt{-1}$, which is all that I contend for.

C. S. PEIRCE.

MILFORD, PA., Feb. 18, 1900.

CURRENT NOTES ON PHYSIOGRAPHY.

DEFLECTION OF RIVERS BY SAND-REEFS.

AN article on 'The effect of sea barriers upon ultimate drainage' by J. F. Newsom (*Journ. Geol.*, vii, 445–451), describes several examples of rivers whose discharge is deflected to the right or left by the formation of an offshore sand-reef in front of their mouths, and suggests that such deflection may explain the course of rivers that now flow parallel to pre-existent coast lines; for example, the Delaware below Bordentown, N. J.

This suggestion is evidently valid as a possibility, but it is not accompanied by tests that sufficiently distinguish deflections thus caused from deflections that arise from the spontaneous adjustment of streams to the weak strata that underlie the cuesta-makers of coastal plains having longitudinal relief. The lower Delaware cannot be a normal example of the latter class, because as the master river of its region it is the very one that should not be deflected by adjustment; on the other hand, it may truly fall under the former class because its deflection is in the sense of the dominant sand-drift along our Atlantic Coast. Examples of sand-reef deflections ought to follow the strike of strong or weak rocks, indifferently; while normal deflections by adjustment can only follow belts of weak rocks.

DEVELOPMENT OF THE SEVERN.

THE systematic development of rivers seldom finds better illustration than in the interaction of the 'waxing Severn and the waning Thames,' concerning which a number of new details and suggestions are given by S. S. Buckman (*Nat. Science*, xiv, 1899, 273–289). The growth of the Severn by headward erosion along the weaker strata that underlie the firmer oölites of the Cotteswold hills is advocated on good evidence, and a restoration of the original consequent headwaters that have now been diverted from the Thames system is attempted. The growth of obsequent branches of the subsequent Severn on the line of the beheaded consequent branches of the Thames is well presented as the reason for the peculiar unsymmetrical arrangement of the Severn tributaries in the neighborhood of Gloucester. The Frome, a branch of the Severn, is shown to have captured several of the westernmost headwaters of the Thames in the Cotteswold hills between Chalford and Edgeworth. The progressive diminution of the Coln, a branch of the Thames, by the successive diversion to the Severn of the two large branches that once came from Wales is offered in explanation of the very curious features of the present Coln valley in the upland east of Cheltenham: a valley of large-curve meanders is taken as the work of the original river; a narrower valley of small meanders, cut in the floor of the larger valley, is the work of the river after one of its upper branches was captured by the Severn; the wriggling course of the present stream on the floor of these smaller meanders is due to the further loss of volume after the second upper branch was captured.