five times per week, throughout the year, and follow in the fresh tracks of Hermite, Halphen, and Weierstrass. But even Freshman students here are familiarized with $x + iy$ and imaginary exponentals, while for the whole body of geometric discipline the researches of Bolyai, Lobatchevsky, and Riemann are regulative.—Respectfully, WILLIAM BENJAMIN SMITH.

COLUMBIA, MO., February 19, 1892.

[We are pleased to learn that mathematics is so deeply studied in Missouri. The lectures of Hermite form a good introduction to the theory, and the work of Halphen to the practical side, of the doctrine of functions. As we are unacquainted with any treatise of Weierstrass capable of being used for a text-book, the study of this author (concurrently, perhaps, with that of the orations of Calgacus) cannot fail to impress us very much. Nothing is said of projective geometry, which ought to be a compulsory study where there are any compulsory studies; but we cannot expect a relatively small university to cover every branch of mathematics, nor would such an ambitious attempt be wise. We are glad the ideas of Riemann (including doubtless those of Cayley and Klein) are regulative, and that Lobatchevsky is at hand for students who wish to approach the non-Euclidian geometry by the elementary method. Bolyai is a make-weight.—ED. NATION.]

54 (10 March 1892) 190-191

IS INDUCTION AN INFERENCE?

CSP, identification: internal evidence here is very good. Furthermore, this reply continues a series of letters and replies arising out of Peirce's review of Halsted's translation of Lobachevski at 54 (11 February 1892) 116. See also: Fisch, *First Supplement*. The editorial comments are unassigned in Haskell's *Index to* The Nation, vol. 1.

TO THE EDITOR OF THE NATION:

SIR: In the communication of Maxime Bôcher in the *Nation* of February 18, referring to the unsatisfactory condition of geometrical studies in our schools, he says:

"If we could lead the student first to see the truth of a proposition, and then, perhaps much later, to prove it, we might hope in time to have mathematicians in America. For every mathematical discovery is made in this way; let the mathematician conceal his footprints as he will; it must come as an intuition, and the man to whom it has thus come is its discoverer, even though he never succeed in finding a proof."

Is not this statement of general application? Is ever the boundary of knowledge advanced in any other way, whether in mathematical or physical science? Reasoning serves merely to verify and confirm the intuitions of genius by applying general principles to concrete cases; but *inference*, in any proper sense of the word, cannot *advance* knowledge—it cannot grasp more than is contained in the premise.

I know the common doctrine represents this as the province of induction. But induction is not reasoning; it is intuition, or happy guessing, if you like. Take any so-called inductive syllogism, and substitute for the major premise what is tacitly assumed, and it is converted at once into a strictly deductive argument. By no possible reasoning, or *inference* proper, could Newton have attained to the law of gravity. It was a happy guess, an inspiration of genius. It was based on wide knowledge, it is true; but it was not a necessary consequence of that knowledge. Assuming the law to be true, reasoning applied it, and the conclusions were found to agree with experience and observation. But the conception of the law was an intuition; it was not a *conclusion* involved in any known premises.

Take Whately's old school-book illustration of an inductive syllogism: "The ox, sheep, goat, deer, bison, etc., are a sample of the class 'horned animal,' or represent the class; the ox, sheep, etc., are ruminants, therefore all horned animals are ruminants." But what do you mean by "the ox, sheep, etc., are a sample, or represent the class 'horned animal' "? Evidently you mean that whatever is true of them is true of all horned animals. Unless this is true, your conclusion is worthless. But if you substitute this, which is tacitly assumed, the argument is deductive, not inductive. The same is true in every case of so-called inductive argument. The real induction—the advance in our bounds—is contained in the assumption that what is true in the cases we know will be found true in all cases having a certain other similarity to these. But this is not an inference, it is not reasoning, it is intuition. J. McL. S.

DAYTON, O.

[It is plain that in no case of genuine induction is *everything* that is true of the sample true of the whole class; so, according to our correspondent, all inductions must be worthless. But he supports this position by nothing, nor does he notice a single one of the objections to it which have been urged from the days of Philodemus to our own, and are found in common American books, such as the 'Studies in Logic, by Members of Johns Hopkins University.' The main distinction between induction and statistical deduction (the only kind of deduction which bears much resemblance to induction) is that the prediction made by the deductive form of inference is applicable in many cases, and, while it may be false in any one, it will probably and approximately be true in the long run; but the inductive conclusion, on the other hand, may be false—only, if so, the further pursuit of the same method will in the long run probably and approximately *correct* it. The distinction is between getting confirmed in the long run and being corrected in the long run.

Our correspondent's proposition that induction is not inference will meet with less favor than it might do, owing to his use of the unfortunate word *inference*, which most people particularly appropriate to the designation of uncertain presumption, amounting to little more than conjecture. But in its philosophical sense inference is defined in the 'Century Dictionary' as "the formation of a belief or opinion, not as directly observed, but as constrained by observations made of other matters or by beliefs already adopted." For instance, we wish, let us suppose, to know whether among negroes male births are more numerous than female births or not. This general proposition cannot be directly observed. We turn, then, to the compendium of the tenth census, and find a considerable excess of female over male births there recorded among negroes in this country for one year. This brings us to the belief that the same phenomenon would generally occur among large populations of negroes, and our proceeding is *inference* according to the received definition, as certainly as it is induction.

But the 'Century Dictionary' adds this remark: "The act of inference consists psychologically in constructing in the imagination a sort of diagram or skeleton image of the essentials of the state of things represented in the premises, in which, by mental manipulation and contemplation, relations which had not been noticed in constructing it are discovered." This recognizes an intuitive or perceptive element as an important part of reasoning itself—a doctrine which results from the study of the logic of relatives, where the perceptive element comes into great prominence. Proof believed to be conclusive has been offered of the truth of this view, which has been accepted by many philosophers. "J. McL. S." seems to offer no rational objection to it. He says, indeed, that Newton's discovery was "a happy guess, an inspiration of genius"—that is, it came directly from on high, or from the action of chance, and was not based upon any knowledge already in Newton's mind, or dependent from any luminous conception which he had carefully worked out. But the truth is, our correspondent seems to have taken his notion of reasoning from the 'Elements of Euclid,' which was written before logic was much understood, and from texts-books of logic inspired by theological doctors. If, as he says, "reasoning serves merely to verify [something] by applying general principles to particular cases," there is next to no reasoning in mathematics; for the *nodus* of a mathematical demonstration does not consist in the application of a general principle.

The original passage quoted has some truth in it. The mathematician usually sees a thing dimly before he sees it clearly. But between the processes of coming to see a mathematical truth as probable, and coming to see it as evident, there is no radical difference. It is all reasoning, and, as such, it is an act of perception—or, rather, of experiment, followed by observation.—ED. NATION.]

54 (17 March 1892) 211-212

EXPERIMENTAL PSYCHOLOGY

TO THE EDITOR OF THE NATION:

SIR: Your review of Prof. Lombroso's book, 'The Man of Genius,' in the *Nation* of February 25, shows, I think, that his method of inquiry is of very little value. His first induction (in reference to the connection between genius and stature) more especially seems open to various objections, and though much labor must have been bestowed upon the subject, how many great names are omitted, and how many obscure names are brought in seemingly because some account of stature was accessible! Your statement of the case at once brought to mind Lotze, who was a remarkably small man, and who, in his lectures on psychology,