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Title page

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Charles Sanders Peirce: Contributions to *The Nation*
Part Four: Index

Kenneth Laine Ketner

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Copyright page

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INTRODUCTION

This index was derived from a word list generated using a computer program written by Virginia Kelemen and Kenneth Ketner. David Pfeifer, as associate project director, also contributed to the system design. The software produced a word list keyed to volume and page numbers in the following three titles.

a. Ketner, Kenneth Laine, and Cook, James Edward, eds. *Charles Sanders Peirce*: *Contributions to* The Nation. *Part One*: *1869-1893*, 1975.

b. -----. *Part Two*: *1894-1900*, 1978.

c. ----- . *Part Three*: *1901-1908*, 1979.

Each word or phrase in the word list is accompanied by at least one four-character code, each of which gives a location for its corresponding entry in the three text volumes. Thus, the entry

means that Andrea is mentioned on page 110 of Part Three (c).

The word list was culled in two ways. First, as a part of the initial sort routine, a number of words, such as 'the', 'a', or 'and', were excluded. Second, because the resulting word list was too large for the publishing budget, a number of additional lines were later deleted. The items that were thereby omitted were considered to have no philosophical or scientific relevance.

At that point we had our present list. We had decided very early in the project not to provide a cross-referencing feature. However, as a further aid to users, we decided to reorganize some of the phrase entries. In those cases, we placed first the word of the phrase that was regarded as most likely to be sought in a search. That is of course a judgment call. In any case, users should know that some phrase entries are alphabetized by the first word in the phrase, and some others by what was considered the more important concept (in the context of Peirce's works) within that phrase.

The introductory notes, and the editor's notes from the first three parts including Bernstein's essay in Part One, were not incorporated into this list; neither were the recently discovered items by Peirce presented below.

I have added a few additional reviews for which recently discovered evidence indicates Peirce's authorship. For notes here, I have used the same conventions employed in the previous three volumes.

Persons who wish to obtain a copy of the full Peirce/*Nation* concordance derived from the original machine sort may write to Texas Tech University Press, Sales Office, Lubbock, TX 79409-1037 USA. The complete concordance is available in cloth, paper, or loose-leaf editions.

I gratefully acknowledge the advice and assistance of Berti Ketner, Max Fisch, Susan Wauer, C. J. W. Kloesel, Don Roberts, John Ryan, and Wendell Broom.Kenneth Laine Ketner

Volume 4

CHARLES SANDERS PEIRCE: ADDITIONAL CONTRIBUTIONS TO *THE NATION*

5 (19 September 1867) 238: MATHEMATICS IN COURT

The letter by V. X. was answered by Chauncey Wright (see Haskell, *Index to* The Nation). Benjamin Peirce testified in the Howland Will case, using, in part, results suggested by his son Charles.

TO THE EDITOR OF THE NATION:

The question of forgery is involved in the "Howland Will Case," and it is contended that two signatures must have been traced.

Mr. Crosman was engaged in examining different signatures of various individuals with reference to the question of signatures repeating themselves. He "stated at great length the result of the comparison of these signatures; that he found many of them to cover much better than the signatures of Miss Howland to the second pages and to the will; that in one case both lines, 'Your obedient servant' and 'Joseph B. Spear,' covered much better than the two signatures of Miss Howland."

The whole case was closed by the testimony of Prof. Benjamin Peirce upon the doctrine of chances. I quote from this testimony as follows:

"In the case of Sylvia Ann Howland, this phenomenon could occur only once in the number of times expressed by the thirtieth power of five or more--exactly, it is once in two thousand six hundred and sixty-six millions of millions of millions of times, or 2,666,000,000,000,000,000,000. This number far transcends human experience. So vast an improbability is practically an impossibility." How then to account for "Your obedient servant, Joseph B. Spear," and other duplicate signatures?

Now, I would ask if all this testimony of Prof. Peirce's is not irrelevant? The signatures that do correspond, and are not forgeries, are facts, and by the side of this doctrine of *chance* they seem to prove that figures cannot always be trusted. It appears to me that the case in question is not amenable to the laws of chance. It is always a person's *intention* to make the signature similar to others as nearly so as possible every time. The elements of will and desire unfit it for judgment by such laws. Figures can be prostituted to prove almost anything, and were it not for Prof. Peirce's high position, one might be led to think his evidence nothing more than a special plea. And the tone of his testimony is arrogant and positive, as if he were charging the judges.

I doubt very much whether "all the mathematicians of the world" will instantly recognize the correctness and applicability of this doctrine.

V. X.

[Mr. Crosman's testimony does not in any way conflict with Professor Peirce's, for the conclusion of the professor's testimony is not that there may

not or do not occur exact coincidences in the signatures of certain persons, but that in the case of Sylvia Howland such a coincidence would be a most extraordinary event, unless designedly produced by tracing or other form of forgery. The evidence on which this conclusion is founded is a minute comparison of about fifty signatures of Miss Howland each with all the others. So irregular were the signatures found to be that a coincidence so close as that of the alleged forgery ought not to be expected more than once in 2,666,000,000,000,000,000,000 cases of comparison. This calculation is not affected by the fact that other people write more regularly than she; it is based simply on an investigation of *her* habits of writing. This investigation might be called in question as being in the nature of things somewhat arbitrary so far as determining what are and what are not cases of coincidence. No two individuals of any species whatever could be found exactly alike if examined with sufficient minuteness. It was accordingly necessary to resort to the practical judgment of common sense to determine the data for this calculation; and the value of the calculation, which is a very simple one, depends wholly on the judgment used in observation, and is not a mathematical question at all. The procedure was substantially this: The thirty down-strokes of Miss Howland's signature were chosen as test objects. In comparing any two signatures, as many of these lines were made to coincide with corresponding ones as in the judgment of the observer could be made to do so. The coincidences, or what in the judgment of the observer were deemed coincidences, were counted, and the whole number of coincidences in the 1,250 comparisons was found to be to the number of non-coincidences as one to four nearly, or to be about one-fifth of the whole. The relative frequency of the occurrence of different numbers of coincidences in single comparisons followed the law of chance very closely, showing that the several coincidences were independent,

accidental events, and hence, for the whole thirty to occur (the independent chance of each being one-fifth), there is only the chance measured by one-fifth to the thirtieth power. This conclusion is not invalidated by the fact that in the signatures of other persons exact coincidences do happen, unless it can also be shown that these persons generally write as irregularly as Miss Howland did. The effect of a person's *intention* to make all signatures alike, which "V. X." refers to, is fully taken account of in the investigations of the person's *habits*. There is nothing whatever in what "V. X." says about the "elements of will and desire." His comment on the tone of Professor Peirce's testimony is hardly just. Mr. Peirce has a not very wise way of putting his testimony in the most paradoxical and at the same time positive form he can devise, thereby making it very much less effective on the minds of common folks. If he appears arrogant, it is probably from a desire to make up in the earnestness of his statements the lack of convincing clearness--to supply his audience with a lively faith in default of a clear understanding. He is a little too much given to impute a certainty to the results of mathematical computation which only belongs to the processes

and not to the data of the computation. The value of the present testimony depends wholly on the judgment of his son in estimating coincidences, and does not depend on the judgment of either father or son as mathematical experts.--ED. NATION.]

13 (9 November 1871) 307: NOTES

Attributed to Peirce by Max Fisch on the basis of very strong internal evidence. For this case, see K. L. Ketner, "Peirce and Turing," forthcoming in *Semiotica*. Unassigned in Haskell, *Index to* The Nation.

--The death of Mr. Charles Babbage, the inventor of calculating machines,

is announced. He was born December 26, 1792. The analytical power of his mind was early manifested. In 1815, when he was only twenty-two years old, appeared his remarkable "Essay towards the Calculus of Functions," a very general and profound sort of algebra, of which he was the chief author. About 1822, he made his first model of a calculating machine. It was a "difference engine," that is, the first few numbers of a table being supplied to it, it would go on and calculate the others successively according to the same law. This, at least, is as correct as so short and easy a statement can be. In the following year, at the request of the Royal Society, the Government made a grant of £1,500 to enable Mr. Babbage to proceed with the construction of his machine. In 1829, the Government largely increased this sum, and in 1830 assumed the property of the machine, and declared their intention of defraying the cost of completing it. This Mr. Brunel estimated at £12,000 at a time (February, 1831) when from £8,000 to £9,000 must have been spent. It was in 1830 that Babbage published his "Enquiry into the Causes, of the Decay of Science in England," a savage attack on the management of the Royal Society; on Mr. Pond, the Astronomer Royal; on Captain Sabine, and other influential scientific men. But it was after the publication of this book that Government agreed to furnish the engine. In 1833, a portion of the engine, sufficient to illustrate the working of the whole, was put together. It was a wonderful piece of workmanship, of a precision then unknown, and since unrivalled. To make it, it had been necessary not only to contrive new tools, but to lay a scientific foundation of the principles of tools, and to educate the mechanics who were to use them. Not a penny of the money paid by the Government ever went into Mr. Babbage's pocket, but, on the contrary, he had always advanced the money to pay the workmen until the Treasury warrants were issued, so that he was usually in advance from £500 to £1,000. In 1833, Mr. Babbage declined to continue this system, and, in consequence, the engineer discontinued the construction of the engine, dismissed the workmen, and took away all the tools. During the suspension of the work caused by this circumstance, the great misfortunate of his life

befell Mr. Babbage. He discovered the possibility of a new *analytical*

engine, to which the difference engine was nothing; for it would do all the *arithmetical* work that that would do, but infinitely more; it would perform the most complicated *algebraical* processes, elimination, extraction of roots, integration, and it would find out for itself what operations it was necessary to perform; and the principle of this machine was such as immensely to simplify the means of attaining the object of a difference machine. One would suppose that, finding himself so unlucky as to have thought of such a thing, Babbage would at least have had the sense to keep it strictly to himself. Instead of that, he wrote immediately and communicated it to the Government! Before that, all was going smoothly; after that, they never would advance another penny. But it must be admitted that Mr. Babbage himself does not seem to have been very ardent to go on with the old machine after the new one was invented. Of course, neither has been constructed. Another difference engine has since been made by a Swede, named Scheütz. This machine is now at the Albany Observatory, and a duplicate of it is used in the office of the Registrar-General in London. Recently, an important new plan for such an engine has been invented in this country; and careful estimates show that it could be constructed for at most $5,000. But the analytical engine is, beyond question, the most stupendous work of human invention. It is so complicated that no man's mind could trace the manner of its working through drawings and descriptions, and its author had to invent a new notation to keep account of it. This mechanical notation has been found very serviceable for simpler cases.

--Mr. Babbage wrote some works which come within the department of political economy. He has introduced several principles of rather subsidiary importance; but his books are more valuable for the striking facts which they contain. He was also the author of one of the Bridgewater Treatises. He was a single-minded and honorable man of science, who hated intrigues and charlatanry. He was witty and entertaining, and knew how to make himself agreeable to the public, but he did not do it by anything verging upon claptrap. He would invent a ballet or invent an automaton to play *tit-tat-too*, but he did not confound such things with his scientific claims. He was a real genius, but with a not infrequent fault of genius, an egregious and lamentable vanity. There is a trace of it, perhaps, in the following sentence, which may be taken as his epitaph: "If," he says, "it is the will of that Being who gave me the endowments which led to the discovery of the analytical engine that I should not live to complete my

work, I bow to that decision with intense gratitude for those gifts, conscious that through life I have never hesitated to make the severest sacrifices of fortune, and even of feelings, in order to accomplish my imagined mission."

13 (9 November 1871) 307-308: NOTES

Probably by Peirce. James Mills Peirce was his brother. Charles had considerable experience as a "computer," one who calculated (often using logarithmic tables) routine equations for scientific experimenters.

--A very handsomely printed and well-arranged collection of "Three and Four Place Tables of Logarithmic and Trigonometric Functions," by Professor James Mills Peirce (published by Ginn Brothers, Boston), which lies upon our table for notice, reminds us of another debt which the world owes to Mr. Babbage. The publication of his logarithms in 1826 makes an era in the art of computation. They were the first ones in which the proper pains were taken to avoid errors, especially by the thorough examination of the stereotype plates. They were also the first ones of which the arrangement, shape, and size of type, manner of ruling, and color of ink and paper, had been determined upon only after careful experimentation. Babbage tried fifty different colors of paper and ten of ink, and found that the blackest ink upon light buff paper was the least fatiguing to the eye. Much attention has since been paid to all such points which facilitate or expedite computation, and some principles of dividing the page by ruled lines have been discovered which were unknown to Babbage. In 1841, Mr. De Morgan called attention to the great advantages of four-place tables. They can be used with twice the speed of five-place tables, and with four times the speed of seven-place tables, and, as De Morgan pointed out, for navigation and most ordinary purposes have all the accuracy which is desirable. Three-place tables are a later notion. They were strongly advocated by Mr. T. Chappelier in 1863; and we know those who have

used them for the last four years with unspeakable comfort for all rough approximate work. For ordinary people who do not have enough calculations to make to keep them in practice in using even five-place tables, the three and four-place tables may, in many cases, be of real utility, if the use of them is once learned.

53 (17 September 1891) 215: NOTES

Probably by Peirce; unassigned in Haskell, *Index to* The Nation. He was studying this topic at that time; the discussion of "various other names" is a distinctive feature of his approach.

'Geometry of Position' is the title of a small octavo volume of 192 pages by Robert H. Graham (Macmillan). Strictly construed, the title denotes a branch of geometry in which the relations of forms in space are considered only in so far as they depend upon position, exclusive of all ideas of quantity or magnitude and all metrical relations. But the absolute separation of the geometry of position and the geometry of magnitude is impracticable if not impossible, and hence various other names have been

proposed. Prof. Cremona of the University of Rome, for reasons which seem to us sufficient, preferred the title 'Projective Geometry,' and applied it to his work on the subject already noticed in these columns. The name 'Perspective Geometry' would also not be inappropriate, for projection and

perspective are the two fundamental notions of the subject. It appears from Mr. Graham's preface that he was obliged to pursue the study originally under circumstances which made it necessary to work out large portions of it himself. This gives to his book a coloring of originality which we have no doubt will be interesting to those who are already acquainted with the subject; but for a beginner, and especially one who attempts to master the subject without a teacher, we think the book far less suitable than Cremona's treatise.

53 (17 September 1891) 215: NOTES

Probably by Peirce; unassigned in Haskell, *Index to* The Nation. H. B. Fine was a close associate.

The Professor of Mathematics at Princeton, Mr. H. B. Fine, is the author of 'The Number System of Algebra' (Leach, Shewell & Sanborn), a small octavo embracing a range and variety of topics the adequate discussion of which would require a library rather than a volume which in size is little more than a pamphlet. The references in the preface, text, and notes show that the book is the result of very extensive reading; of how much thought, the writer alone can tell. It is divided into two parts, the "Theoretical" and "Historical." The first is an attempt to explain and reduce into a consistent system those fundamental concepts and processes which form the basis of the whole superstructure of mathematical science. The nature and properties of numbers, integral and fractional, positive and negative, rational and irrational, real and imaginary, or (by a broader generalization intended by the author to embrace all these classes) natural and artificial, are discussed, and the conclusions at which the author has arrived are stated and explained. Intermingled, as must necessarily be the case, with this discussion of the nature and properties of the various kinds of numbers is a discussion of the fundamental operations to which they are subjected--addition and subtraction, multiplication and division, involution and evolution, series, and many other things for which we must refer to the

book itself. The work is written from both a mathematical and a philosophical point of view, but the mathematical decidedly predominates. The second part of Prof. Fine's book is an attempt to condense into 53 pages a sketch of the history of the development of mathematical ideas and processes from the first attempts at some systematic method of counting and notation up to the 'Quaternions' of Hamilton and the 'Ausdehnungslehre' of Grassmann. Perhaps all that can rightfully be demanded of such an

attempt is that it should contain no erroneous statements, should be interesting, and should stimulate to further inquiry. We think Prof. Fine's sketch answers these requirements. The book is perhaps the most striking of the recent indications that American mathematicians are beginning to look at their science from a more philosophical point of view, and to feel some curiosity as to the history of the development of those processes which they have hitherto regarded merely as useful methods of arriving at certain desired results. We recommend its perusal to all mathematicians who consider their science as something more than a mere tool, and especially to all who are or intend to become teachers of mathematics.

54 (19 May 1892) 385-386: **Record of Scientific Progress for the Year 1891.**

By Robert Grimshaw. Cassell Co.

CSP, indentification [[sic]] : MS 1574.

This volume is mainly taken up with engineering matter of no special interest from a scientific point of view; and even here the record of progress is a miserably careless one. The only sciences, properly so called, which receive any notice at all, are physics and astronomy. Now it so happens that the year 1891 was a great year for astronomers, and more general attention was directed towards this science than towards any other. In the first place, there was the discovery by Mr. S. C. Chandler of the 427-day period in the variation of the latitude--a fact which attracted the attention of all scientific men, and which will lead to the more accurate ascertainment of a good many other facts. Second, there were observations relating to the question whether the planet Venus has a period of rotation exceeding 200 days, or of about one day, as previously held. Third, there were the extraordinary discoveries in the history of Babylonian astronomy by Fathers Epping and Strassmaier. There were, besides, important discoveries in stellar photography. Will it be believed that this pretended 'Record of Scientific Progress' does not mention one of these things? Dr. Grimshaw says: "There seems to have been during 1891 but little astronomical matter worthy of special chronicle." On the contrary, there is probably no scientific man who has not remarked the exceptionally great general interest attaching to the astronomical work of 1891.