THE

AMERICAN JOURNAL OF SCIENCE.

[THIRD SERIES.]

ART. XXIX.—BENJAMIN PEIRCE.*

BENJAMIN PEIRCE was born in Salem, Mass., on the 4th day of April, 1809, and he died at Cambridge, on the 6th day of October, 1880.

In his early years he had the good fortune to come under the influence of Doctor Nathaniel Bowditch. It is said that their first acquaintance was made while Dr. Bowditch's son Ingersoll and young Peirce were schoolmates. Ingersoll showed his comrade a solution which his father had prepared of a problem that the boys had been at work upon. Some error, real or conceived, was pointed out in the work, which was reported by Ingersoll to his father. "Bring me that boy who corrects my mathematics!" was the invitation to an acquaintance, the importance of which in Professor Peirce's own estimation is told in the dedication, more than thirty years later, of his "Analytic Mechanics" "to the cherished and revered memory of my Master in Science, Nathaniel Bowditch, the father of American Geometry."

Peirce entered Harvard College in 1825. As Doctor Bowditch was now in Boston, having removed from Salem in 1823, and was preparing the first volume of his translation of Laplace's "Mécanique Céleste" for the press, it followed almost as a matter of course that the college student was more influenced in his studies by him than by the college course. Doctor Bowditch's first volume was completed and the second entered for

* The Journal is indebted for this memoir to advance sheets from the Proceedings of the American Academy of Arts and Sciences, Boston.

copyright in 1829, the year of Peirce's graduation, and the proof-sheets were regularly read by him.

After graduation, two years were spent by Professor Peirce in teaching at Northampton. In 1831 he was appointed Tutor in Harvard College, and in 1833 was made Professor of Mathematics and Natural Philosophy.

The earlier years of his professorship were fruitful as to publication, principally in a series of text-books for use in college. The first that appeared were treatises on "Plane and Spherical Trigonometry" in 1835 and 1836, which were published in a more complete form, with a "Spherical Astronomy," in 1840. Next came a "Treatise on Sound," in 1836, which was based upon Herschel's work in the "Encyclopædia Metropolitana," but with very important changes. The bibliography of the subject in the Introduction is of permanent value. This was followed, in 1837, by his "Plane and Solid Geometry," and by a "Treatise on Algebra."

A work on "Curves, Functions and Forces" was begun in 1841 by the publication of a volume on "Analytical Geometry and Differential Calculus." A second volume, on the "Calculus of Imaginaries, Residual Calculus, and Integral Calculus," appeared in 1846. As the word "forces" in the title shows, he intended to complete this work by a third volume on the "Calculus of Variations, and on Analytical Mechanics, with its Applications," but in this form it was never done.

Instead of this, however, and so to be mentioned in this place, though not properly a text-book, there appeared in 1855 the "Analytic Mechanics" in a quarto form, a work that more adequately expresses Professor Peirce's peculiar power than any other of his productions, with perhaps one exception.

In all of these books he departed not a little from the beaten path. In geometry the idea of direction was made the basis of the theory of parallels. Infinites and infinitesimals are introduced, along with the axiom, "Infinitely small quantities may be neglected." The demonstrations are given only in outline, being in respect of fulness the entire opposite o Euclid. A like brevity is characteristic of the other books, and in fact of everything mathematical that Professor Peirce ever wrote. He used a notation to which he gave much thought, by which his formulas were more concise than they could easily be made with the usual symbols. The Integral Calculus was at the period of its appearance much in advance of similar works, especially in the treatment of differential equations. It is an excellent example of Professor Peirce's concise and logical style.

The "Analytic Mechanics" was rather a treatise than a text-book. In it Professor Peirce set forth the general principles and methods of the science as a branch of mathematical theory, and embodied in a systematic treatise the latest and best methods and forms of conceptions of the great geometers. He aimed to reduce them to their utmost simplicity by freeing them from every superfluous element. He made free use of the idea of the *potential*, developing nearly the whole subject from it. Determinants are used regularly as a standing instrument of analysis, and especially in the integration of the differential equations of motion. Both of these features, as well as Jacobi's method of integration by his principle of the last multiplier, were at the time new in English treatises.

The whole volume is marked by a directness of thought and a brevity of expression which make it difficult reading for those who have been accustomed only to the usual forms of notation and reasoning, and who do not read the book in course from the beginning. Several of the chapters are made peculiarly interesting by the development of a large number of special problems as particular cases of general theorems. In his later years the author often said he wanted to rewrite the "Analytic Mechanics" and introduce quarternions into it.

In 1842 Professor Peirce published, in connection with Professor Lovering, four numbers of the "Cambridge Miscellany," a quarterly journal devoted to mathematics, physics and astronomy.

In the same year he assumed the care of the mathematical part of the "American Almanac," ten volumes of which were prepared by him. In one of these (1847) he published a list of the known orbits of comets, arranged in convenient form, to which he added to the usual cometic catalogue several approximate orbits computed by him for historic comets that had been imperfectly observed.

In 1849 Congress established a Bureau for the publication of the "American Ephemeris and Nautical Almanac," under the superintendence of Lieutenant (afterwards Admiral) Davis. Professor Peirce was at once appointed Consulting Astronomer. In this capacity he prepared and published, in 1853, his "Tables of the Moon," which have been used in making the "Ephemeris" up to the volume for the year 1883. In coöperation with Lieutenant Davis, he designed the form and general plan of the Ephemeris, and he decided upon all the coefficients to be used. He commenced a revision of the theory of the planets, especially the four outer ones; but this seems not to have been carried to serviceable results, if we except certain separate communications to this Academy. He retained the position of Consulting Astronomer until 1867. The high place which the "American Ephemeris" has ever held among like publications owes much to the character given to it by Professor Peirce in these its earliest years.

When, in 1846, Galle discovered the planet Neptune in the place pointed out to him by Leverrier, Professor Peirce took the liveliest interest in the admirable researches of Leverrier and Adams. He entered with zest into all the questions which were thus raised. What is the orbit of the new planet? What its mass? How much do they differ from the assigned orbits and masses? Does the new planet explain all the irregularities of Uranus? Did the data lead necessarily to the assigned place, and to it alone?

The results of his investigations were at various times given to this Academy, but more especially on the 4th of April, 1848. He then gave the perturbations of longitude and radius vector of Uranus by Neptune, and announced that Neptune and either of the two hypothetical planets of Leverrier and Adams would equally explain the observations of Uranus, within reasonable limits of error.

Leverrier had proposed to himself to solve the following problem:—From the observed irregularities of the planet Uranus to compute the elements of the orbit of an assumed exterior planet that has caused these irregularities. He ought perhaps to have limited himself to the other problem, to which he gave so correct an answer, Where among the stars astronomers must look in order to see the disturbing body. The elements of the orbit could be had from observations when once the planet was seen. He found for the unknown planet an orbit and a mass by processes that will always command the admiration of men; and the place in that orbit, as is well known, was less than one degree, as seen from the earth, from the actual place where Galle found Neptune.

Yet Professor Peirce declared that Leverrier's geometric planet and Neptune were not the same bodies. He praised without question the work of Leverrier and of Adams, asserting for them their right to all the praise and *éclat* which the world had given them. But Leverrier had distinctly stated that the planet which disturbed Uranus could not be at a less mean distance from the sun than 35; that is, that no planet that was within this distance could cause the observed irregularities of the motion of Uranus. Neptune, however, is at a distance of only 30, and does account for the perturbations of Uranus.

In this and in other communications Professor Peirce claimed that the perturbations changed their character at the points where the mean motions had the ratios 2 : 5 and 1 : 2, and that the reasonings of Leverrier were thereby vitiated. Not a little controversy has come from these papers of Professor Peirce; and we cannot say that the last word in regard to the question has even yet been spoken. As is not unusual in like discussions, there is probably a portion of truth and a portion of error with either party. Leverrier and Adams each, as Professor Peirce has himself shown, by his own laborious researches, did point out correctly a place where a planet should be looked for, and assigned paths which that planet could have been traveling for more than one hundred and twenty years previously, and have caused the observed irregularities. Yet the elements of that planet's orbit and its mass and those of Neptune differ widely enough to justify the assertion that for the latter *they* were not correctly given.

On the other hand, astronomers will not probably agree with Professor Peirce in regarding the change of character of the perturbations when the mean motions of the new planet and of Uranus pass through the exact ratios 2 : 5 and 1 : 2 as of vital importance. In the usual form of development these fractions do indeed make certain terms infinite. That belongs, however, to the form of the development, not to the perturbations. In solving the question, "Where is the disturbing body?" the solution need not have involved these forms; and it has not been shown that they entered into the work of either Leverrier or Adams in such a way as to vitiate it.

That the problem was really indeterminate has been steadily held by Professor Peirce. In January, 1878, he read to this Academy a paper, which has not been published, and the conclusions of which, therefore, will not compel the assent of astronomers until some one else shall have gone over the same questions. He showed a chart of the plane of the ecliptic with the orbits of Uranus and Neptune, and having those parts of the plane shaded within any part of which a planet of arbitrary mass might have been situated in September, 1846, and yet have caused, in the preceding years, the observed irregularities in the motions of Uranus, within reasonable limits of error. With a circular orbit, a large fraction (more than one half) of the ecliptic, as seen from the earth, contained some of the shaded portions. If an eccentricity not greater than one-tenth be allowed, the region was greatly enlarged. While, therefore, the solutions of Leverrier and Adams gave a place and a path that explained the disturbances, the problem in its nature was not, he claimed, one having a single answer, or even a finite number of answers.

In 1852, Professor Bache, then Superintendent of the United States Coast Survey, induced Professor Peirce to take up the subject of the longitude determinations in the Survey. As a result, there appeared in the successive volumes of the "Coast Survey Reports," communications from him upon the several questions that arise in the treatment of that subject. The most noteworthy referred to the determination of our longitude from Greenwich, since local differences were determined by the telegraphic method. The whole subject of errors of observations, the law of facility of error which is assumed in the method of least squares, its limits and defects, and the habits of observers, were carefully examined. The method of occultations was decided to admit of greater accuracy than any other that was then available, and the occultations of the Pleiades to furnish the most convenient means of its application. Formulæ and tables were prepared, old observations collected, and new ones made to apply this method. The question of our longitude is now, thanks to the ocean telegraph, one of history; but the questions of errors in observing, which Professor Peirce so thoroughly treated, will always be of practical import.

It seems as though there was a connection between this engagement with the Coast Survey and the appearance, in July, 1852, in Gould's "Astronomical Journal," of an article by Professor Peirce, entitled, "Criterion for the Rejection of Doubtful Observations." His object was to solve this problem: There being given certain observations, of which the greater part is to be regarded as normal, and subject to the ordinary law of error adopted in the method of least squares, while a smaller unknown portion is abnormal, and subject to some obscure source of error, to ascertain the most probable hypothesis as to the partition of the observations into normal and abnormal. This method or rule given for deciding whether an observation had better be left out of account has received the name, "Peirce's Criterion," and must be regarded as one of his best contributions to science. Tables for use in applying it were soon afterward published by Dr. Gould.

The "Criterion" has been criticised by Professor (now Sir G. B.) Airy as defective in its foundation and illusory in its results; and he was even of opinion that no rule for the exclusion of an observation can be obtained by any process founded purely upon a consideration of the discordance of those observations. This position of the Astronomer Royal must be regarded as entirely untenable; for no observer hesitates to call a widely discordant observation a mistake, and to reject it (when he can find no other reason for so doing), simply because of that discordance. What the mind thus instinctively does, there must be basis at least for a rule for doing. Professor Airy's objections were answered by Professor Winlock at the time of their appearance. The "Criterion" has been used considerably in this country, though not, perhaps, in Europe. The uniform testimony of our computers is, we believe, that it has given excellent discrimination, and that it does not come into conflict with proper judgment based upon experience. This shows the good working of it in actual practice.

That the "Criterion" has not come into use in Europe may in some degree have been due to the excessive brevity of the argument by which Professor Peirce established the equations to be used. Perhaps no one has read that argument for the first time without finding difficulty in understanding some parts of the reasoning. A want of confidence may thus have easily resulted. Professor Chauvenet has given us a simpler rule for use in rejecting a single divergent observation; but it is only an approximate solution, since one important element is left out of account. Computers need some such rule to guide them, and it would seem almost certain that "Peirce's Criterion," or possibly some modified form of it, will in time secure general acceptance. In any case, it will ever stand as the first, and as a satisfactory, solution of this delicate and practically important problem of probability. At present it is the only solution we believe that claims to be complete.

After the death of Professor Bache, Professor Peirce was, in 1867, made Superintendent of the United States Coast Survey, and he discharged the duties of that office for the next seven years. Soon after his appointment he made a tour of inspection among the parties at work in the field. Notwithstanding his previous intimate relations with the survey as adviser to Professor Bache, he was very much surprised and delighted with the practical skill which many of the officers had acquired. "I recognize at once," he said, "the masters of the profession." Unfortunately, he recognized also the awkward and inefficient, and the presence of these, which even the admirable executive abilities of his predecessor had not been able to eliminate, gave him great concern. Yet he determined to hold to the broadest line of policy, and introduce no rigid discipline that might damp the ardor and spontaneity of the faithful. "The lame and the lazy are always provided for," says the adage; and in the public service they are found, practically, to have the most friends from without, because needing them most. In a scientific service like the Coast Survey, which, unlike many of the departments of the civil service, furnishes absolute criteria from which to judge the merits of an officer, the task of discrimination, if undertaken by a superintendent well versed in the mathematics and physics underlying the manœuvres of the surveyor, would seem to be as easy as it is just. But it was a saying of Professor Bache, that "it would be easy enough to crush directly the men who betrayed the good repute of the service if it was not for uncles, aunts, and cousins, who proposed, in their turn, to crush him."

It was after his return from one of his earliest tours of inspection that Professor Peirce, in conversation with one of the older assistants, said he proposed to give, at least at the outset, greater freedom of action to the officers of the corps, that each might indicate the full scope of his powers and receive promotion, or give place to another according as the results of his work might determine. "The office," he said, "can add nothing to my reputation unless I can give it greater dignity by raising the standard of the service. I mean to bring the best men to the front and secure publicity to their merits, that they may feel directly responsible to the community and do their utmost for its approbation. To become the leader of a corps of distinguished men is the best thing I can do for the country, for the men themselves, and for my own reputation." This was the policy which he initiated in the Coast Survey, and its wisdom was demonstrated at once. A very large proportion of the officers appreciated his motives, caught the enthusiasm of his genius, and found a new delight in serving a master who coveted nothing, but with rare simplicity lent his own strength to secure to them the full rewards of their labors.

The most important work started by Professor Peirce, and much advanced under his direction, was the actual extension of geodetic work into the interior of the country by continuing the great diagonal arc from the vicinity of Washington to the southward and westward along the Blue Ridge, eventually to reach the Gulf of Mexico near Mobile. He also planned the important work, now in active progress, for measuring the arc of the parallel of thirty-nine degrees, to join the Atlantic and Pacific systems of triangulation, and for determining geographical positions in States having geological or topographical surveys in progress.

He conferred a very important benefit on public interests by so enlarging the scope of the Survey as practically to extend geodetic work into the interior States.

As soon after the war as vessels and officers could be had, he renewed operations for deep-sea soundings and dredgings, and he gave earnest support and aid to all scientific work in any way related to the Survey.

While Superintendent he also took personal charge of the American expedition to Sicily, to observe the eclipse of the sun in December, 1870.

By virtue of his office he was a member of the Transit of Venus Commission, and by his suggestions and active effort he greatly aided that undertaking. Two parties from the Coast Survey were sent out by him,—one to Nagasaki, and the other to Chatham Island, to take part in the work.

The "Quaternion Analysis" of Hamilton seemed to Professor Peirce to promise a very fruitful future. "I wish I was young again," he said, "that I might get such power in using it as only a young man can get." He took great pains to interest his students in it, and in his later years formed a class for its earnest practical study, with good results. His own thought was turned especially to the logic that underlies all similar systems, and to the limits and the extensions of fundamental processes in mathematics.

At the first session of the National Academy of Sciences, in 1864, he read a paper on the elements of the mathematical theory of quality. Between 1866 and 1870 various papers were read to that Academy, or to this Academy, on "Linear Algebra," "Algebras," "Limitations and Conditions of Associated Linear Algebras," "Quadruple Linear Associative Algebra," etc. These papers were not printed in form as read, but instead in 1870–71 appeared his "Linear Associative Algebra."

His own feeling about this contribution to science is expressed in the salutatory to his friends: "This work has been the pleasantest mathematical effort of my life. In no other have I seemed to myself to have received so full a reward for my mental labor in the novelty and breadth of the results."

An analysis of this treatise was given by Doctor Spottiswoode to the London Mathematical Society, which is characterized by Professor Peirce as "fine, generous and complete." Such an analysis can only come from one who has made a special study of the laws of mathematical thought. To some mathematicians, and other men of science, it may yet be a question, if the time has come for them to say with entire certainty whether this work is to share the fate of Plato's barren speculations about numbers, or to become the solid basis of a wide extension of the laws of our thinking. Those who have thought most on the course which contemporary mathematical science is taking will probably agree that the new ground thus broken can hardly fail to bring forth precious fruit in the future by adding to the powers of mathematics as an instrument.

In any case, the Associative Algebra can never lose its value as an important and most beautiful addition to Ideal Mathematics, and must ever remain a monument to the comprehensive grasp of thought and analytical genius of its author.

Professor Peirce defines mathematics as the science which draws necessary conclusions. Algebra is formal mathematics. Addition is taken to express a mixture, or mere union of elements, independently of any mutual action which might arise if they were to be mixed in reality. From this definition, the commutative character of addition necessarily follows. Multiplication is no further defined than as an operation distributive with reference to addition; but the only algebras treated are those whose multiplication is associative. The subject is further limited to linear algebras, that is, to such as contain only a finite number of lineally independent expressions; so that every quantity considered may be put under the form,

$$ai + bj + ck + \text{etc.}$$

where i, j, k, are peculiar units, limited in number; while a, b, c, are scalars,—a term borrowed from the language of quaternions, but here used in a modified sense to include, not merely the reals, but also the imaginaries, of ordinary algebra. A variety of highly general theorems are given, extending to all linear associative algebras. The author next introduces the conception of a pure algebra, as contradistinguished from one which is virtually equivalent to a combination of several. Methods are developed for finding all such pure algebras of any order. Finally, he obtains the complete series of multiplication tables of these algebras up to the fifth order, together with the most important class of the sixth order. They are in number as follows:

Single Algebras	2
Double "	3
Triple "	5
Quadruple "	18
Quintuple "	70
Sextuple "	65

Professor Peirce never made any extended study of the possible applications of his algebras; he was far from thinking, however, that their utility was dependent upon finding interpretations for them; on the contrary, he showed that certain of them could be advantageously employed, without any interpretation, in the treatment of partial differential equations like that of Laplace.

He read to this Academy in May, 1875, a memoir "On the Uses and Transformation of Linear Algebra," which is, we believe, his only published addition to the principal treatise. He had also made some progress in the investigation of the laws of *non-associative* algebras.

Professor Peirce could not fail to be interested in all questions that concern the equilibrium, the history, and the development of the solar system. At first he was loth to accept the nebular hypothesis in any form. But the results of his studies led him, at last, to defend its main propositions as the true laws of creation.

The rings of Saturn are of prime import in any explanation of planetary development. The discovery by Professor Bond, in 1850, of the dusky ring, and his announcement of reasons for believing that the rings were fluid, multiple, and variable in number, led Professor Peirce to take up the mathematical theory of the rings. He announced, as the result of his analysis, that the rings could not be solid, that a fluid ring could not have its centre of gravity controlled by its primary, and that it must be supported by the satellites. The principles of the solution were indicated in an article, published in "Gould's Astronomical Journal" in 1851. At different times in the following years some portions of his theoretical treatment of the problem were published. The mathematical possibility of a large number of narrow solid rings was admitted. In the "Memoirs of the National Academy of Sciences" he published, in 1866, the formulas for the potentials and attractions of a ring. This problem has peculiar interest, from the mode of development of the formulas.

The place of comets in the solar system was a subject of his thought even earlier than the rings of Saturn. The discussions and the computation of orbits of various comets in the years 1846–1849, were followed in the latter year by an argument that the comets must have always been parts of the solar system.

In 1859 he applied the theory of solar repulsion of the matter of the comets' tails to the observed form of the tail of Donati's comet, and deduced the strength of the repulsive forces that drove off the nebulous matter. The next year he gave, in a letter to the Academy of Sciences, of Paris, twelve remarkable and suggestive theses on the physical constitution of comets.

In 1861 he made a communication to this Academy, suggesting the meteors as a cause of the acceleration of the moon's mean motion. The paper was not printed, and it does not appear whether he referred to the direct impact of the meteors upon the moon, or to the resistance due to the action of the moon in turning the meteors out of their paths. Probably he included both causes, since each has the effect, to a limited degree, of a resisting medium.

In the last two years of his life he presented to this Academy several communications upon the internal structure of the earth, and the meteoric constitution of the universe. Especially in October, 1879, he gave a series of eight propositions in Cosmical Physics. At an informal scientific meeting at Harvard University he stated five others, which have been since printed in the Appendix to his "Lectures on Ideality in Science." They were given rather as a basis for criticism and discussion than as fully proved. They are founded upon the theory of Mayer, which is advocated by Sir William Thomson, that solar heat, and in part planetary heat, are supplied by the collision of meteors with the sun and planets. Small portions of matter in space cool and become invisible solid meteors. These, by their impact with the sun, produce the violent commotions of the sun's surface. A portion of the earth's heat comes from the sun, another portion directly from the impact of meteors with the earth's atmosphere. The two portions, he afterwards shows, are equal.

These views are developed more fully in his "Lectures," recently published. The meteors, as Professor Peirce believed, come from the outer portions of the condensing solar nebula. In the course of development an outer shell was left, which furnished the matter to be collected in small masses. The smallest become meteors, the larger comets. Their numbers are enormously great. Arranged according to perihelion distances, the number of comets or meteors coming within a given distance of the sun varies directly as the distance. The heat of Jupiter and Saturn comes from the collisions with those planets. The interior of the earth may be liquid throughout, and the limits set to the lengths of the geologic ages may reasonably be greatly extended.

Any attempt to outline the history of the solar system is sure to lead, in the present state of knowledge, into serious difficulties. Necessarily the problems that arise do not, in many cases, admit of quantitative analysis. The number of unknown elements that appear with every new hypothesis is large; and the more we learn, the larger the number of questions which we cannot answer. It will be but natural if some of the theses of Professor Peirce shall be questioned, and even be proved unsound; but scholars who shall be led into this fascinating field of study will always find in them profound and most suggestive views of creation. Some of these theses will undoubtedly be found to be the true and previously unknown laws of nature.

Professor Peirce was always warmly interested in everything that promoted science in this country. He was generous in his estimate of merit, especially of merit in young men. He was one of the founders of the National Academy of Sciences, was an early President of the American Association for the Advancement of Science, was one of the most active members of this Academy, and was a frequent recipient of academic honors. American science mourns in his death the loss it cannot express, but has a higher life for his having lived.

H. A. N.