a notice of the chemico-algebraic method of graphs devised by Sylvester and extended by Clifford, and of MacMahon's remarkable transformation of the question of seminvariants into a question of symmetric functions. Under the other heads of this division, and under the last division of the Report, that on "Specielle Substitutionsgruppen und Formen," the number of points that come up for treatment is so great that a continuation of even such cursory notice as we have been giving would be fatiguing. Suffice it to say, therefore, that whoever consults the Report will be impressed by the fact that the development of the Theory of Invariants in recent years, while overshadowed by the brilliant conquests made in the domain of the Theory of Functions, has been by no means at a standstill. Not to speak of the excursus into the field of differential invariants made by Sylvester and his followers, MacMahon, Hammond, and others, signal advances have been made in the central theory, especially by Capelli, Stroh, Study, and Deruyts. Dr. Meyer, in his preface, expresses regret that he found it impossible, except in a few instances, to include the geometrical applications of the theory in the scope of the Report. With this exception, the student of the theory of algebraic forms and invariants will find in the Report a remarkably full abstract of researches in this domain, accompanied by accurate bibliographical references, and will feel under great obligation for the assistance rendered by this result of Dr. Meyer's great learning and painstaking industry. It ought to be especially useful to any one undertaking to present, in a systematic work, the body of doctrine which is the outcome of the varied and often heterogeneous researches outlined in this compendious report.

F. FRANKLIN.

BALTIMORE, *April*, 1894.

CAJORI'S HISTORY OF MATHEMATICS.

A History of Mathematics. By F. CAJORI. New York, Macmillan & Co., 1894. 8vo, 14 and 422 pp.

IT is a long time since an American work has been awaited with so much anticipation by readers of mathematics as Professor Cajori's recent history. The book had been extensively advertised, there was and is a growing demand for such works, and the supply of material was well-nigh inexhaustible. But while few books have ever enjoyed such advantages, few books have ever so seriously failed to improve them. This is a harsh statement and should neither be lightly made nor lightly accepted. It is based upon the following facts, which are stated as concisely as an adverse criticism allows.

First. The work is, in very considerable measure, merely a paraphrase of portions of better works to be found in the libraries of most readers. Witness the following extracts. (Bracketed clauses are merely transposed.)

Cajori.	*Gow.*
"Plato was born at Athens in 429 B.C., the year of the great plague, and died in 348. He was a pupil and near friend of Socrates, but it was not from him that he acquired his taste for mathematics. After the death of Socrates, Plato travelled extensively. . . . He went to Egypt, then to Lower Italy and Sicily, where he came in contact with the Pythagoreans. Archytas of Tarentum and Timæus of Locri became his intimate friends." (p. 29.)	"Plato was born . . . at Athens in 429 B.C., the year of the great plague. [He died . . . in 348.] He was a pupil of Socrates, . . . but he did not derive from this teacher his enthusiasm for mathematics. . . . After the death of Socrates [Plato] went away from Athens. . . . He went certainly to Egypt . . . and lastly to Magna Græcia and Sicily, where he [consorted with Pythagoreans. . . . He] became a close friend of Archytas and Timæus of Locri." (p. 173.)
	Ball, 2d Edn.
"At the age of thirteen he is said to have been familiar with as many languages as he had lived years. About this time he came across a copy of Newton's *Universal Arithmetic.* After reading that he took up successively analytical geometry, the calculus, Newton's *Principia*, Laplace's *Mécanique Céleste*," etc. (p. 318.)	"When thirteen he was able to boast that he was familiar with as many languages as he had lived years. It was about this time that he came across a copy of Newton's Universal Arithmetic; . . . he soon mastered the elements of the analytical geometry and the calculus. He next read the *Principia*, and . . . Laplace's Mécanique Céleste," etc. (p. 474.)
	Fink.
"This problem was reduced to another, now generally known as Malfatti's problem: to inscribe three circles in a triangle that (*sic*) each circle will be tangent to two sides of a (*sic*) triangle," etc. "Steiner gave without proof a construction, remarked that there were thirty two solutions, generalised," etc. (p. 296.) (A reference is given to Fink in a preceding sentence, but not on the part transcribed.)	"Diese Aufgabe reduzierte er auf die jetzt allgemein als "*Malfatti'sches* Problem" bekannte Forderung, in ein gegebenes Dreieck drei Kreise so einzubeschreiben, dass jeder Kreis," etc. "Steiner gab (ohne Beweis) eine Konstruktion, führte an, dass es zweiunddreissig Lösungen gebe, und verallgemeinerte," etc. (p. 202.)

The above extracts are only specimens of many cases that might be cited, in which the author seems to have copied, without giving due credit, from Gow, Ball, Hankel, Cantor, and Suter, and apparently from Halsted's introduction to his monograph on Bolyai. Sometimes the extract is from one author and the credit is assigned to another, but usually no credit is given to any one. The first seventy-five pages are largely paraphrased from Gow; then Hankel is drawn upon; and, finally, Ball, Cantor, Suter, and a few other sources are utilized. Now and then a note refers to one of these writers, but it rarely happens that, at such times, the writer cited is followed more closely than on other occasions. One cannot but wonder why Professor Cajori did not, at the beginning, frankly say that he had copied *ad libitum* from three or four authors, instead of giving only occasional credit.

But it may be said, laying aside the ethics of the matter, that the work claims (which it does not) to be merely a compilation; that good authors have been selected, and their words carefully transcribed. A single selection may be given in reply to such a suggestion. This particular one is taken because, laying aside the seriousness of the discussion for a moment, it may cause a pardonable smile.

Cajori.	*Hankel.*
"Creditable work in theory of numbers and algebra was done by *Fahri des Al Karhi,* who lived at the beginning of the eleventh century. His treatise is the greatest algebraic work of the Arabs. In it he appears as a disciple of Diophantus. He was the first to operate with higher roots and to solve equations of the form $x^{2n} + ax^n = b$." (p. 111.)	"Das grösste algebraische Werk der Araber, das wir besitzen, der Fahrîdes Al Karhî aus dem Anfange des 11. Jahrhunderts, welches ein genaues Studium des Diophant zeigt, geht über die ältere Algebra nur insofern hinaus als es auch mit höheren Wurzeln als Quadratwurzeln operiren und Gleichungen von der Form $x^{2p} \pm bx^p = \pm a$ auflösen lehrt." (p. 265.)

Fahri des Al Karhi! Does not every reader of the history of mathematics know that Fahri (or Al-Fakhrî) was the name of the *book* that Alkarkhî wrote? It is all explained on p. 245 of Hankel: "Einem Vezir Fahr ul Mulk (†1017) eines anderen Buyiden Beha-ed-daula widmete Al Karhi ein algebraisches Werk, *dem er zu Ehren seines Gönners den Namen Al Fahrî gab.*" A glance at Cantor (Vol. I, p. 655), or at Müller and his references, or at Heath (p. 24) would have saved the author from this most awkward blunder.

Second. The work is weak in bibliography, where it should be exceptionally strong. One has a right to expect a rich set of references to the standard literature of the day. Such references are offered by other histories, however humble, and every student needs them. Yet in this work there is not a single reference by volume and page. The bibliography is unscientific and meagre, and the use made of it may be called fictitious. It consists of a few standard histories, a few text-books, a few periodical articles not paged, and a number of works of no special value. As an example of the authorities cited, let any one who is familiar with the twenty-two volumes of the *Jahrbuch über die Fortschritte der Mathematik* already published (the last volume having over 1200 pages) consider such a reference to that work, without volume or page, as the one (p. 250) on the statement that Euler used π for 3.14159 . . . in 1737! A reference to vol. 21, p. 30, or to the *Bibliotheca Mathematica* for 1889, p. 28, would have been of some value.

It is true that even the least of the works cited might, if referred to by volume and page, be useful to the student. But it is surprising that many works of so much more value than most of those mentioned are ignored. Some of these are easily accessible, like the valuable historical articles in the last edition of the *Britannica*, by Allman, Chrystal, Glaisher, Cayley, and Tait, or those in Smith's Dictionary of Biography, or those in Leslie Stephen's current work on National Biography. One misses all reference to the many biographical contributions by Battaglini, Cremona, Beltrami, Bertrand, Brioschi, de Comberousse, Darboux, Jacoli, and others, and to the numerous historical memoirs of Boncompagni, Cantor, Chasles, Curtze, Favaro, Friedlein, Tannery, Treutlein, Günther, de Haan, Henry, Mansion, Martin, Narducci, and Steinschneider, and to such common works, of much higher value than many named, as those of Unger, Sterner, La Cour, Heath, Rudio, Treutlein, Weissenborn, Wolf, Reiff, Woepcke, and to the recent contributions of Heiberg and Hultsch. These men are not unknown, nor are their works rarities. They have written extensively, and their contributions are valuable and are of our generation. Why should the only reference on Pascal, for example, be Madame Perier's Life, which was translated into English in 1744, while the valuable contributions by Cantor, Chrystal, Chasles, Todhunter, Wolf, Desboves, Pisko, Tannery, Henry, Bianco, to say nothing of Ball, Marie, Suter, Williamson, Hoefer, Montucla, Bossut, and others, are unmentioned? That the bibliography should omit the works of older writers like Kästner and Libri and Bossut, or a writer like Hoefer (of whom, however, Allman speaks with some favor), is not strange, but that such a sweeping omission is made is quite remarkable.

Third. But it may be said, and indeed it has been said, that this work is especially strong in relation to modern mathematics. While this will not excuse the errors in the treatment of the earlier development of the subject, to be mentioned hereafter, nor the weakness of the bibliography relating to that development, the claim should be considered. But however charitable the reader may be, he will close the final chapters with even greater disappointment than he experienced in reading the earlier ones. What, for example, does the work tell of the growth of the theory of substitutions and of groups? Say fourteen lines, all told. Might not one expect some mention of the contributions of Frobenius, Stickelberger, Kneser, Marggraf, Rudio, and our Professor Bolza, and, in general, a good résumé of the development of the subject? And on the theory of invariants, while the reader will find several helpful notes, might he not reasonably expect that a history of mathematics published in 1894 would give a fair condensation of Meyer's account in the first volume of the *Jahresbericht der deutschen Mathematiker-Vereinigung*, pp. 81–288? Even some reference to this elaborate memoir would have been of great value. And in the matter of theory of functions, if he were reading Forsyth, or Harkness and Morley, would he, after one or two attempts, ever go to this work again for any help in tracing the development of the subject? Or, to take a somewhat different illustration, if he were interested in the development of the considerable subject of mathematical tables from Herwart to our time, how little would he find in this work! A mere note, referring to Glaisher's monographs, would have been helpful. In the matter of modern biography, let the reader consider the two or three lines devoted to Sophus Lie, merely to say that he has applied finite continuous groups to the treatment of differential equations, and has helped to edit Abel's works. Much more is said of other living writers who are not worthy to unloose the latchet of Lie's shoe. Of "one of the most elegant contributions to differential geometry made in recent times," mentioned by Klein at Evanston, and indeed of the general scope of Lie's work, there is only what is contained in a dozen words.

It strikes an American pleasantly to see mentioned the names and labors of over thirty of his countrymen. While the number is disproportionate, and while thirty American mathematicians could not be found who would wish to be mentioned in a work which ignores the names of so many world-known promoters of the science, the effect in our own country may possibly be of value. The names or labors of from fifty to seventy-five men (not including contemporaries) who are much more entitled to mention than many who are given place, are wanting, while the selection of living mathematicians can scarcely be called a happy one.

Fourth. A final reason why the work is disappointing is apparent from the first page: the work is carelessly written. One who consults a history of any subject may reasonably expect to find the common facts of that subject, together with the names, dates, nationalities, and principal works of its leading contributors set forth in compact form. But when he reads of Metius without his value of π, of Nonius without mention of the nonius, of Napier's "analogies" and "rods" two pages after the discussion of Napier; when he finds the Christian name incorrectly given or frequently omitted; when he finds no dates assigned to a large number of writers, including men of the prominence of Galileo, Malfatti, Viviani, Bürgi, Cramer, and others equally well known,—may he not reasonably affirm that the greatest care has not been taken?

In the matter of the spelling of common oriental names, also, may not one rightly complain of the utterly unscientific policy pursued? It will be remembered that Hankel set forth (p. 225) a system of transliteration for such names, which he followed, but which has been accepted by no other leading writer on the history of mathematics. Among other things Hankel says that the symbol $\breve{h}$ shall be sounded like the German hard *ch*, and he so uses it. Other writers generally use *ch* or *kh* for the same sound. But Professor Cajori, ignoring all recent authorities, ignoring the common dictionaries (v. *algorithm*), and ignoring Hankel's essential subscript, merely uses *h* as the equivalent of the German hard *ch*. The result is often curious. Consider, for example, p. 106. The author is evidently writing with Hankel (p. 260) and Cantor (p. 611) open before him. For some reason he intends to take Hankel's spelling of Muhammed ibn Mûsâ Alchwarizmî, viz.—Mohammed ben Mūsā al Hovārezmi. But in doing so he forgets the very important $\breve{a}l$, and creates a spelling that is *sui generis*. Even this is not so strange as the fact that, attempting to follow most writers, he derives *algorithm* from *Hovarezmi!* That it should come from Alchwarizmî, or from Hankel's *al Hovārezmi*, is clear enough, but the above etymology is certainly noteworthy.

Of the other errors in the book it is necessary to speak at no great length. A few may be mentioned to show that, like all first editions, the work is not free from them. In the bibliography, not considering matters of taste in the capitalization of German adjectives, errors will be found in numbers 38, 39, 47, 50, 55, 60, 65, 67, 76, 84, 88, 92, 95, 96, 97. The spellings of Deinostratus (p. 25) and Dinostratus (p. 32),—of Jakob Bernoulli (p. 182), Jacob and James (p. 236), and James (p. 237),—of Gand when Ghent is well Anglicized,—these are errors of taste rather than of fact. The Egyptian fractional symbol *ro* is spoken of as a dot; and while a dot was used by Ahmes, and is, for convenience, sometimes used in printing Egyptian fractions, it was never used as Professor Cajori states; a glance at Eisenlohr's table, or at the examples in the 2. Abschnitt, would show this. A further error in symbolism occurs in Diophantus' sign of subtraction, a strange error in view of the full discussion in Heath (p. 72), not to mention Gow (p. 109) and Cantor (p. 401). But with Heath the author seems unacquainted, else he would not err in his symbol for the first power of the unknown (so fully treated in Heath, pp. 57–69). A similar error appears in his Greek numerals, apparently from following Hankel too blindly. Such errors of titles, as of one of Plücker's works, or of spelling, as Midorge (p. 174), Tchirnhausen (index), Professor Moor (p. 330), etc., need scarcely be mentioned, since they are manifestly misprints. Similarly for such errors of the index as "Moivre, de, 245," and "De Moivre, 240," and the reference to p. 292 under "Substitutions," while the more important reference to p. 329 is wanting. Neither is it worth while to dwell upon the occasional specimens of poor English, illustrated by the use of "insolvability."

There are a number of errors in chronology, however, that may annoy the student. Such are the date of Tschirnhausen's birth and the date of Ivory's death, which latter comes from depending solely on Ball. In many cases about which there is much doubt the dates are given with apparent certainty, while in cases about which there is little or no doubt the dates are frequently omitted entirely or given with a question-mark. Thus, to Hipparchus and to others heretofore mentioned are assigned no dates, while to Archytas are assigned the dates 428–347, both of which are probably wrong, and to Pythagoras are assigned the dates "580 ?–500 ?", both of which are probably within a year of exactness. It is stated of Thales that he died in 546, which is quite doubtful, and in the case of Heron the Elder Gow's "c. 120," and the even more probable date c. 110, are ignored, and "c. 155" is assigned; in this latter the author apparently depends on Marie I, 177, forgetting that Marie is notoriously untrustworthy. The date 814 for the beginning of Al Mamun's reign is incorrect, as are others which it is impossible here to enumerate.

From this effort to call attention, fairly and without exaggeration, to the chief deficiencies in Professor Cajori's work, it should not be inferred that the book is without merit. Far from it. It tells in a popular way the general story of the growth of mathematics. It is well printed and is altogether an attractive piece of book-making. It is a pioneer in America and should be welcomed as furthering interest in a very important subject. Moreover, it makes an effort in the way of tracing the recent development of mathematics. For all this the work deserves credit, and Professor Cajori deserves thanks. But in view of what has been said, it seems only a plain statement of the truth to add that as a scientific treatise the work cannot be regarded as an authority.

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GRAVITATION AND ABSOLUTE UNITS OF FORCE.

ABSTRACT OF A PAPER READ BEFORE THE NEW YORK MATHEMATICAL SOCIETY AT THE MEETING OF APRIL 7, 1894.

BY PROF. W. WOOLSEY JOHNSON.

THE writer held that the conflict between gravitation and absolute units was irrepressible because of the impossibility of reconciling the practical necessities of the engineer with the scientific needs of the physicist. Accordingly most modern text-books admit both kinds of units. The history of the matter was briefly summarized. Weighing being the inevitable manner of comparing masses, the same terms have always been used to describe masses and the pressures produced by their gravitation. With the rise of mechanical science the conceptions of force and of mass must be differentiated. The older writers were content to write $P \propto mf$; force, mass, and acceleration might be expressed each in its own unit; but the modern method is to write such a relation in the form $P = kmf$, and, first establishing fixed units to be employed, to proceed to determine k. Since no occasion had hitherto arisen for a unit of mass as distinguished from a unit of weight, no difficulty was at first felt in adopting for m such a unit that $k = 1$, and hence $P = mf$, while the pound, the foot, and the second were the units of force, length, and time. In other words, in using $W = mg$ no inconvenience was felt from the fact that in assigning a numerical value to m its unit was not a mass weighing one pound, but a mass weighing g pounds. There would rarely be occasion to employ the numerical value of m, W/g being substituted for it in final results.

But g is found to be variable, and since our standards furnish us with an invariable mass, it is seen that we have been using a variable unit of force. The engineer and practical man, however, while admitting that mass and not force is the third primary unit, still finds it more convenient for his purpose to use this variable, or rather let us say 'local,' unit of force, in spite of the fact that in using the formula $W = mg$ this implies also a variable or 'local' unit of mass.

This variable unit of mass seems intolerable to a certain class of writers who object *in toto* to gravitation units. With these writers "the British unit of mass is the Imperial