this year be made by July 1. After that date the various applications will be sifted and an attempt made to choose the most likely ninety applicants. It has also been decided to increase the requirements for admission in chemistry and, in addition to the 150 hours of laboratory work in inorganic chemistry now required, an additional 90 to 100 hours of laboratory work in organic chemistry will be required of all students desiring to enter the school after October, 1914.

Beginning next year the two-year courses in the college of agriculture at the Ohio State University will be lengthened to three years. The Tuesday before October 15 is the date set for opening and the Friday before March 15, that for closing. Farmers' sons may, with this change made, come to school after harvest and complete the year's work before the spring work begins on the farm. No attempt to extend the subject matter is intended, and the length of the course is practically the same, but boys from the country may engage in practical farming while taking the agricultural course under the new system.

Plans are being perfected for the centennial of the first conferring of degrees by the Yale medical school. Special exercises will be held in Woolsey Hall on Monday afternoon of commencement week from 4 to 6, and historical addresses, the conferring of honorary degrees, exhibits and other features will be arranged.

Mr. A. W. McCoy (A.B., A.M., Missouri) instructor in geology at the University of Missouri, has been elected instructor in geology at the University of Oklahoma.

The General Board of Studies of Cambridge University have appointed Dr. Assheton to be university lecturer in Animal Embryology.

DISCUSSION AND CORRESPONDENCE

REPLY TO A RECENT CRITIQUE OF AN OLD REVIEW IN SCIENCE

In the current number of the *Bulletin of the American Mathematical Society,* December, 1913, pages 147–151, Professor E. B. Skinner makes erroneous statements regarding my review in Science[1] of Professor L. W. Reid's "The Elements of the Theory of Algebraic Numbers," and also regarding the history of the subject.

1. In my review I had said:

> After stating formally theorem *A* and devoting fifteen lines to its proof, the author informs us that the "theorem therefore fails." Similarly, on pages 250–251, theorems are formally stated and later shown not to hold in general. This peculiar style of pedagogy is decidedly a novelty to the reviewer.

Quoting only the first sentence and that incorrectly, Professor Skinner insists that the quotation puts the author in a wholly erroneous light. But the entire passage certainly makes clear that I was merely questioning the wisdom of this peculiar style of pedagogy. There was no need whatsoever for any comment in Science on the bare fact that the author stated a formal theorem in italics, devoted a half page to a "proof," and then indicated that the proof failed and that the "theorem" itself was false, repeating the same process on pages 250–251. I think I was justified in presupposing upon the part of a reader of my review that small degree of acumen which would enable him to conclude unguided that if an author devoted considerable space to a false theorem, the failure of the theorem was regarded by him as of sufficient interest to warrant attention. It is unfortunate that the author and Professor Skinner speak also of general theorems which they nowhere state explicitly and which if stated would be false, except in the very simplest cases, as they well knew.

2. In my list of important topics omitted from the book, I included erroneously that of class number. It occurs first on page 434, just seventeen pages before the end of the book. One may be pardoned for not looking at the end of a long book for a topic which should play a fundamental rôle in the whole theory.

3. The last paragraph of my review has gone through a remarkable metamorphosis in the hands of Professor Skinner. What I actually said was:

[1] Science, N. S., Vol. XXXIII., pp. 188–89, February 3, 1911.

In the matter of references the author has been particularly unfortunate. In a book barely entering upon the threshold of the theory, a scarcity of references would have been entirely justifiable. But to give hundreds of references to a certain report on the subject (excellent although it be) and to completely ignore the literature and not even mention the names of the discoverers of the theorems is against all scientific traditions.

What Professor Skinner says I said is:

Again, the reviewer, deploring the omission of references, says: But to give [as above].

My second sentence above (not quoted by Professor Skinner) shows that I did not deplore the omission of references. This sentence together with the one actually quoted by him show that what I deplored was misplaced references. As should be well known, Kummer created a highly complex theory of ideal numbers for the case of fields built upon roots of unity; then Dedekind created a simpler theory of ideals in complete generality and developed the subject at great length; then Hurwitz made a simplification which yields a brief and attractive exposition of the theory; also Dirichlet, Kronecker, Hilbert, Minkowski, Hensel and others have contributed to the development of the subject in various directions. A very large proportion of the theorems stated on pages 218–451, the part dealing with quadratic fields other than Gauss's important case, should have been attributed to Dedekind, provided a reference was to be given. But in these 234 pages, I find only four references to Dedekind, once to an alternative proof, once to a symbol, once to a simple lemma, and finally to a wholly subsidiary theorem. There are two references to Minkowski and one to each Woronoj (on cubic number fields), Hurwitz, Sommer, H. J. S. Smith and to Chystal's algebra. The references to the main theorems are to that excellent report by Hilbert, recently translated into French. As against the four wholly minor references to the originator of the general theory, Dedekind, there are 45 references to Hilbert's report (Professor Skinner's count of 38 for the entire book is misleading as he neglected references given in the body of the text). With a single exception, these 5 references are to passages in Hilbert's report in which Hilbert expressly attributed the results to other writers; had the author reached the higher parts of the theory, he would have needed many references to Hilbert's own important contributions. On my own part the impression that there were hundreds of such references was wrong; but that exaggeration is really beside the mark. The references are largely misplaced and that is evidently all I was emphasizing in the passage quoted above. I do not begin preparation for writing a book review by counting references, and I do not care a straw whether or not Professor Skinner's count of 158 as the total number of footnote references is correct; in any event only about 44 of these relate to the part under discussion. In the above extract from my review I expressly limited myself to the subject of the report and hence to algebraic numbers; consequently it is not a fair comment on that extract to speak of the large number and nature of the references in the introductory part on rational numbers. In all probability these references would have been like those discussed above had the report treated also rational numbers.

4. Professor Skinner states that my review was freely interspersed with exclamation points. As a matter of actual fact only two exclamation points appear in my two-column review. One is in

The author desires to bring out a closer relation between rational numbers and quadratic numbers. This he accomplishes by complicating the elements of rational numbers with the unnecessary machinery of quadratic numbers! We find on page 91 Wilson's theorem stated in the form

$$r_1 r_2 \cdots r_k + 1 \equiv 0 \pmod{p}, \quad k = \phi(p),$$

where $r_1, \cdots, r_k$ form a complete set of residues modulo p, a prime.

According to the Index, this is the first statement in the text of Wilson's theorem, which has been known since 1770 under the familiar form that $1 \cdot 2 \cdot 3 \cdots (p-1) + 1$ is divisible by the prime p. After the complicated theorem is stated, proved, generalized and illustrated by several examples, the usual form is finally given. The second exclamation point was used in discussing a three-

page proof which could have been given in a few lines.

5. If, in the three years intervening between my review and his attack on it, Professor Skinner had given less time to the counting of footnotes and more time to the comprehension of the passages quoted from my review and to the unquoted context of those passages, he would possibly have saved himself from "careless and inaccurate statements," instead of attributing that term so freely to my review.

6. Professor Skinner makes on page 150 the remarkable statement:

> Furthermore, the author has put in a very clear light the historical sequence of the ideas which led to the development of the theory.

On the contrary, the author made no such pedagogical blunder. He wisely did not attempt to give any idea of Kummer's ideal numbers, the operations on which are so delicate that one must use the utmost circumspection (as remarked by Dedekind in his important historical papers in Darboux's *Bulletin*). Nor did the author present the second stage (Dedekind's viewpoints) in the historical development of the theory. For most obvious reasons the author refrained from presenting "the historical sequence of the ideas," and confined himself to the simplified present-day exposition of the theory, as far as he went.

L. E. DICKSON

A REJOINDER TO DR. DAVENPORT

THE task of the critic is always a disagreeable one, and it is only the conviction that the fate of eugenics as a science depends on the repudiation of much of the recent work of the Eugenics Record Office which impels me to reply to Dr. Davenport's letter in SCIENCE of November 28. I shall confine myself to the three points he raises regarding the paper on heredity in epilepsy although these points are not in the least representative of my criticism of that paper. Indeed, I dealt with not one, but a whole series of publications in which Dr. Davenport is concerned.

(*a*) Dr. Davenport states that

> First, Dr. Heron seems to assume that whenever a symbol in a pedigree chart is not accompanied on the chart by some special description it stands for a person about whom nothing is known. He calls attention to numerous cases where, notwithstanding, the corresponding individual is described in the text. The assumption is a gross error. The chart shows mainly the interrelationship of individuals, and indicates only certain traits.

Bulletin No. 2 of the Eugenics Record Office[1] is entitled "The Study of Human Heredity" and the opening sentence reads:

> The following methods are in use at the Eugenics Record Office. . . .

The "plan of charting" adopted is described in section 2 and it is there stated that while the letters E, F, I, N, etc., placed in or around the square or circle which stand for male or female, indicate that the individual in question is epileptic, feebleminded, insane or normal, etc., "when no letter accompanies the individual symbol it means that no definite data had been secured at the time the chart was made" (page 4). Further, Plate V. on page 16 is entitled "Key to Heredity Chart" and there examples of the symbols used are given. The first two are the square and circle without any accompanying letters and the description given is "No data." Again, in his tables Dr. Davenport uses a symbol X which he defines as "Unknown" (I pointed out that more than half the individuals entered in the tables were described by Dr. Davenport himself as "unknown"). Now in the great majority of cases the square or circle without any accompanying letter corresponds to an individual marked "unknown" in the tables, but I pointed out several cases where mistakes had been made. To take the first example I gave in my paper, Fig. 10, case 469, the chart shows two sisters one of whom is marked epileptic while the symbol for the other is left blank to indicate that "no definite data had been secured at the time the chart was made" or that there were "no

[1] It was reprinted in Bulletin No. 7 of the Eugenics Record Office, September, 1912.