

lbs. to the ton. Most large corporations do likewise. Personally we are entirely willing to pay for a hundredweight of coal and receive 112 pounds, that is, eight stones (a stone as we all *readily* remember being 14 pounds). But we only get an uncertain number of stones in each ton; and the ton remains 2,000 pounds. We are thankful that there is not a Troy ton, or we might get that (1,500 lbs.).

Mr. Russell argues that the old English weights and measures are uniform and consistent. He therefore proposes a new ounce to weigh 436.947 grains. We now have two ounces, one of 437.5 grains and another of 480. Who then could tell how many grains there were in a pound, a hundredweight or a ton? Why not let the grain depart in peace? No one now uses it, whereas the grain is in general use among scientific men and is entirely satisfactory.

ALEXANDER MCADIE

AN ILLUMINATING METHOD OF HANDLING DATA

THERE has come to my observation a notable method of treating data, one worthy the attention of the curious in such matters, and casting light upon certain other things of interest. A circular letter entitled "Biologists under cross-examination on the inheritance of acquired characters," to which is attached the name of Casper L. Redfield, has recently been distributed. Appended to it are what purport to be "parts of letters received" from certain biologists in answer to inquiries from various persons concerning the inheritance of the effects of the organism's responses to its environment (as distinguished from the direct action of the environment). With regard to these letters it is stated that "in the cases in which the question was directly answered, that answer was usually confined to one or two sentences in a letter filled with irrelevant matter," and as to the replies quoted it is said that "the replies given are simply those parts of the letters received which answer the questions asked—irrelevant matter being omitted." Among these replies the following (in answer to an inquiry from a Mr. Herdman) is given as my contribution:

Dear Mr. Herdman: I have little or nothing that will help you. Redfield's work has been criticised as unsound. Otherwise, nothing has been published.

H. S. JENNINGS.

The pertinent points regarding this are as follows:

(1) My letters to Mr. Herdman contain no such passage or passages. Not one of the sentences quoted is found in my letters to Mr. Herdman or to any one else.

(2) Except for the trend of the comment on Mr. Redfield's work the passage does not give even re-

motely the sense of what I wrote. In addition to Mr. Redfield's writings I referred the inquirer to Kammerer's extensive work, which is almost entirely on the heritability of the organism's responses to the environment; to Semon's book, "Das Problem der Vererbung Erworbener Eigenschaften," which contains accounts of many investigations along this line; to the recent work of Griffith and Detlefsen on the inheritance of the reactions produced in rats by whirling, and to other works. I did not say, "I have little or nothing that will help you," for I hoped that these references would help him. And I obviously did not say that other than Redfield's work "nothing has been published," since I gave references to other things that had been published. I am driven to conclude that these two sentences are metamorphoses of the following. After the somewhat extended letter, with references to the literature, above mentioned, a brief second letter to Mr. Herdman (in answer to an inquiry as to the nature of the criticisms on Redfield) said: "I felt that I had little or nothing to add that would help you, so that I have not hurried about replying." (Note the words "to add," the omission of which from the ostensible quotation completely changes the sense). After referring him to Pearl's review of Redfield, my summary of the situation concluded by saying that "a great many persons have worked along lines similar to this, but in most cases the results have been negative, so that nothing has been published on the work or the work has attracted no attention, since there were no definite results." Nothing else in my letters bears the least resemblance in either words or meaning to the first and third sentences in what purport to be "parts of the letters received" from me.

The student of scientific method will find it an enlightening exercise to analyze in detail the methods employed in the author's treatment of the raw data given above, in order to get out of them his finished product; to formulate the general principles under which these operations are carried out; and to meditate upon the wonderful potentialities opened up by the application of these methods and principles to the data of genetics. Upon the reader that will carry out this analysis a great light will dawn as to how it happens that the author claims that the matters discussed in his published works demonstrate the inheritance of acquired characters; and as to the weight to be given to those claims.

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SOME IMMIGRANT CLOVERS

IN April, 1923, my attention was called by Professor Paul Tabor, of the Georgia Agricultural College, Athens, Georgia, to a clover said to be growing in