

publication is a frequent and a grossly careless error, for in every case the information is forthcoming. Examples are:—

P. 225.—Siddall, NATURE, vol. xv.—instead of *Annual Report Chester Soc. Nat. Sci.*

P. 230.—Williamson, NATURE, vol. xvii.—instead of *Proc. Manchester Lit. Phil. Soc.*

P. 250.—Suess, *Quart. Journ. Geol. Soc.*, xxvi.—instead of *Verh. k. k. geol. Reichs.*

Moreover, the hopeless nature of his published errata may indicate that the author was somewhat ashamed of his work, and it is difficult to understand why the book was not stopped and reprinted, before it was allowed to pass into circulation.

Enough has now been said of the original work—that is, the first attempted list; we will now pass on to the supplement I. In this, at least, we might have hoped that the compiler would have profited by experience, and used more care. There is certainly a difference in the proportion of typographical errors, but such details as volumes are still rather wild (*Bull. Soc. Géol. France*, for 1886, is quoted here and there as vol. x., xiv., &c.). We gather from the perusal of his supplement many things we could not understand in the original work. We recognize that the compiler is neither a born bibliographer, nor acquainted with scientific literature. We observe with satisfaction that the words “[not seen]” occur more frequently than in the earlier work, but can it be possible that the author has seen a copy of Silvestri’s paper noted on p. 62? It is exceedingly rare, it does not exist in English libraries, and the writer of this has only seen two copies, both of which were sent to him from Italy. It would have been interesting to learn the pagination of so scarce a paper: the title as it at present stands is strongly suggestive of a bookseller’s catalogue. And surely it was worth the compiler’s while to quote Ehrenberg properly (p. 65) while the book was presumably lying open before him? The book also is known as “*Monatsbericht*” not “*Verhandlungen*,” that is a secondary title. A very careless error is seen on p. 57, where *Orbitolina conoidea*, Alb., and *O. discoidea*, Alb., are quoted. The original gives Albin Gras as the authority, whose paper on the subject, moreover, is well known. There should not have been confusion here. On pp. 64, 65, 71, 72, and 74, the same careless duplication of entries occurs as seen in the first attempted bibliography. But worse than all, perhaps, is the rendering of different versions of the title of one publication. A good instance of this is seen on pp. 66, 67, where six variants of *Verh. k. k. geol. Reichs.* are used, some (*Ver. K. K. Geol.*) being quite unintelligible to the uninitiated. On p. 72 we see two versions of *Ann. Soc. Belge Microsc.*, and only those familiar with the book would recognize readily “*Vierteljahrsschrift d. Zür. Natur. Gesellsch.*” (p. 74), with its chief word abbreviated. The compiler should remember that there is no necessity to quote, but, if he quotes, he should quote correctly.

It is needless to waste space on such clumsinesses as Prof. Wm. King, S.C.D. (? D.Sc.) (p. 1), or *Jahrbuch. Geol. Reichs.* It is also advisable to have some method even in printing. The compiler of this list uses roman and italics indiscriminately for titles of works (p. 71, Steinmann—where more prominence is thus given to the review than to the original work), while on p. 63, in the entry Alth, the word *Rozprawy* begins the title of the book, and has nothing whatever to do with the title of Dr. Alth’s paper.

Many of these errors and defects might have been avoided had the compiler been accustomed to public libraries, or even endeavoured to find out the common books of reference, always at hand in these places. No bibliographer should ever think of working in scientific literature without his Carus and Englemann, his Scudder, and his Bolton, and for an American to omit to do so is sinful. No greater mistake was ever made by a writer than that made by the compiler, when he wrote in his preface that he had enjoyed facilities not enjoyed by many scientific students, those facilities afforded by the great public libraries of New York. We know what the resources of those libraries are, and the production which calls for this letter does not shake our faith in them. “Instructive” this bibliography certainly is, but not in the sense intended by its compiler.

CHAS. DAVIES SHERBORN.

Density and Specific Gravity.

MAY I ventilate a point in mechanical definition which has perplexed students within my experience—the use of the words density and specific gravity?

We are usually told that the quantity of matter in a body—as it is now called, the mass of the body—is proportional to the volume and density conjointly. This is Newton’s definition of density (see also Thomson and Tait’s “*Natural Philosophy*,” § 208). Thus, if M be the mass, V the volume, and ρ the density of a body, we have—

$$M = \rho V \quad . \quad . \quad . \quad . \quad . \quad (1)$$

if the unit of mass be taken as the unit of volume of a substance of standard density.

Again, we are told that specific gravity is the ratio of the weight of the given body to the weight of an equal volume of some standard substance (Besant’s “*Hydrostatics and Hydrodynamics*,” § 13). Since weights are simply proportional to masses, it follows that the numerical values of specific gravities and densities are exactly the same. It would seem better, under these circumstances, to use one word only to express the one physical property. Accordingly, we find that specific gravity is disappearing from many of our best books (I think from Thomson and Tait’s “*Natural Philosophy*,” for example), though it still holds its place to puzzle students in examinations, and therefore teachers are compelled to make the best of it they can.

But this is not the whole evil. The definition of specific gravity is usually followed by the equation—

$$W = sV \quad . \quad . \quad . \quad . \quad . \quad (2)$$

where W is the weight, s the specific gravity, and V the volume of the body. This equation is, no doubt, usually accompanied by the caution that the unit of weight chosen is not the unit of force proper to other dynamical equations, and for this reason the equation

$$W = \rho Vg \quad . \quad . \quad . \quad . \quad . \quad (3)$$

is far to be preferred.

If equation (2) is of practical value, would it not be as well to define specific gravity in accordance with it, and say that specific gravity is the weight of unit volume of the substance? Thus, the specific gravity of water would be expressed by 62.5 lbs. avoirdupois in British units, or by 1 gramme in C.G.S. units. I believe this would have the advantage of conveying a perfectly definite idea to minds which dislike such abstractions as mass and density.

L. CUMMING.

Rugby, March 31.

“Coral Formations.”

MR. MELLARD READE last week (April 5, p. 535) pointed out an error in my calculations which I had myself discovered when too late, and had intended to correct in sending you a further note on some experiments which are now in progress.

Mr. Reade seems to make use of my arithmetical blunder, and apparently attempts to discredit my experiments, and the new views as to coral-reef formations; but I leave the matter to those who have a practical knowledge of the subject.

The corals experimented upon were of the class known as hard corals, and consequently the amount dissolved must be much smaller, I imagine, than that dissolved from the softer varieties, such as Porites. The first experiment (p. 462) gives the highest result, but I have no reason to doubt that the rate of solution deduced therefrom is far below that actually taking place in the tropical areas of the Pacific and Indian Oceans.

I do not consider that Mr. Reade has given an answer to Mr. Irvine’s pertinent question, though he would have it appear that an answer is patent to everyone, and he must not take up your space with such a trivial matter.

Mr. Murray, speaking of his tow-net experiments in his Royal Institution lectures, says:—“I give this calculation more to indicate a method than to give even the roughest approximation to a rate of accumulation of deposits. The experiments were too few to warrant any definite deductions”; and he is evidently satisfied that we have no knowledge, other than relative, as to the rate of accumulation of calcareous deposits.

It is at once evident to all who have used the tow-net, that Mr. Murray’s experiments afford a very slender basis for calculations. Probably not more than one-fourth of the water in the track of the tow-nets actually passed through the nets, and not more than one-half of the organisms that entered them were retained; the Coccospheres, Rhabdospheres, and small Foraminifera, for instance, passing through and escaping with the