

figures and describes several of the Chelonia and other reptiles which come from the same locality.

The above notice of the results arrived at by American men of Science show that they deserve the careful study of English palæontologists and geologists, as they have already thrown great light on the fauna of the Tertiary period, and give promise of adding much more to our knowledge of that epoch, so important to the student of the anatomy and classification of the higher vertebrata.

OUR BOOK SHELF

The Laboratory Guide, a Manual of Practical Chemistry for Colleges and Schools, specially arranged for Agricultural Students. By Arthur Herbert Church, M.A. (London: Van Voorst, 1874).

TEACHERS of chemistry will be glad to welcome the third edition of Prof. Church's "Guide," to which much new matter has been added. Being specially adapted for students of agricultural chemistry, the book is necessarily somewhat limited in its scope, but the amount of information conveyed within the small compass of 215 pages is very great, and is moreover lucid and accurate. The book is divided into three portions, the first treating of a chemical manipulation, the second of qualitative analysis, and the third of quantitative analysis. The author's preliminary remarks upon manipulation are excellent, and should be graven upon the mind of every chemical student. In the "Introduction" we are told that the student "must never forget that the experiment is the means, not the end. . . . Merely to make a coloured precipitate or a flash of bright flame is not the end of experimenting."

These remarks are much to the purpose, and we commend them to the notice of chemists of older growth, as well as to beginners. The sudden introduction of equations on p. 8 without any previous explanation of the meaning of symbolic formulæ appears somewhat unsystematic, but the student is recommended by Prof. Church to attend some course of lectures on inorganic chemistry, and to study the corresponding chapters in Roscoe's Chemistry, at the same time that he is working through the "Guide." As the "Guide" is at present arranged, the student will find this absolutely necessary. The classification of the metals adopted by the author calls for remark—iron and manganese are classed as dyads and aluminium as a triad. Further on it is explained that this last metal is only a pseudo-triad, being in reality a tetrad. Why not class it with the tetrads at once? Hexad metals and pentad metals are ignored altogether, although manganese forms a hexafluoride, arsenic, and antimony, penta-haloid compounds, &c. We must protest also against the use of the words "vinculant," "vinculance," "unvinculant," &c. No advantage is likely to accrue to the science from this new phraseology, and the terms "atomicity," "monatomic," "diatomic," &c., which are in general use, express the idea perfectly. The tables for qualitative analysis differ but little from those generally adopted. The quantitative processes for the analysis of natural products, soils, foods, &c., will be found very useful. In addition to the direct benefit arising from the issue of books like the present, there is an indirect benefit for which we ought to be also indebted to Prof. Church—we refer to the expulsion from the market of hastily compiled and inaccurate works by so-called "Science Teachers," such as it has been our duty to condemn on former occasions.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.]

Prof. Tait and Mr. Spencer

As is shown by the passage from his *Thermodynamics* which he re-quotes, Prof. Tait holds that "Natural philosophy is an

experimental, and not an intuitive science. No *à priori* reasoning can conduct us demonstratively to a single physical truth."

I hold, on the contrary, that as there are *à priori* mathematical truths, the consciousness of which results, not from our individual experiences, but from the organized and inherited effects of ancestral experiences, received throughout an immeasurable past; so are there *à priori* physical truths, our consciousness of which has a like origin.

I have endeavoured to show that Prof. Tait himself, by saying of physical axioms that the appropriately-cultivated intelligence sees "at once" their "necessary truth," tacitly classes them with mathematical axioms, of which this self-evidence is also the recognised character. Further, I have contended that the laws of motion are *à priori* truths of this kind; are enunciated by Newton as such; are adopted from him by Prof. Tait; and are not furnished by Prof. Tait with any such experimental proofs as he asserts are needful for the establishment of physical truths. And I have gone on to show that no experimental proofs of them are possible—that every supposed proof, whether derived from terrestrial phenomena or from celestial phenomena, involves a *petitio principii*.

In the course of the discussion I have examined the reason Prof. Tait gives for asserting that the laws of motion are not to be accepted as valid *à priori*. The reason is that "as the properties of matter might have been such as to render a totally different set of laws axiomatic, these laws must be considered as resting on convictions drawn from observation and experiment, and not on intuitive perception."

The worth of this reason I have tested by asking the origin of Prof. Tait's professed knowledge that "the properties of matter might have been" other than they are. Here is the passage:—

"It will suffice if I examine the nature of this proposition that 'the properties of matter might have been' other than they are. Does it express an experimentally-ascertained truth? If so, I invite Prof. Tait to describe the experiments. Is it an intuition? If so, then along with doubt of an intuitive belief concerning things as they are, there goes confidence in an intuitive belief concerning things as they are not. Is it an hypothesis? If so, the implication is that a cognition of which the negation is inconceivable (for an axiom is such) may be discredited by inference from that which is not a cognition at all, but simply a supposition. Does the reviewer [a critic whose attack I was answering] admit that no conclusion can have a validity greater than is possessed by its premises? or will he say that the trustworthiness of cognitions increases in proportion as they are the more inferential? Be his answer what it may, I shall take it as unquestionable that nothing concluded can have a warrant higher than that from which it is concluded, though it may have a lower. Now the elements of the proposition before us are these:—As 'the properties of matter might have been such as to render a totally different set of laws axiomatic' [therefore] 'these laws [now in force] must be considered as resting . . . not on intuitive perception:' that is, the intuitions in which these laws are recognised, must not be held authoritative. Here the cognition posited as premiss, is that the properties of matter might have been other than they are; and the conclusion is that our intuitions relative to existing properties are uncertain. Hence, if this conclusion is valid, it is valid because the cognition or intuition respecting what might have been, is more trustworthy than the cognition or intuition respecting what is! Scepticism respecting the deliverances of consciousness about things as they are is based upon faith in a deliverance of consciousness about things as they are not!"

From this passage Prof. Tait has quoted a small part which, standing by itself, appears somewhat strange; but which ceases to appear strange when read along with the rest. In seeking the authority which Prof. Tait has for asserting that "the properties of matter might have been" other than they are, I have tried all possible suppositions; and as he professes to have faith only in experimentally-ascertained truths, I have asked whether this is one; by way of showing, unmistakably, that in the absence of experimental warrant he must admit it to be, if not a mere hypothesis, then an intuition. Whence results the incongruity I have pointed out.

Prof. Tait says this argument of mine reminds him of a student whose conceptions of algebraic processes were shown by asking—"But what if x should turn out after all not to be the unknown quantity?" His imagination suggests to Prof. Tait an analogy too remote for me to perceive; and one which I think few will