Elementary Practical Chemistry and Qualitative Analysis.

By Frank Clowes, D.Sc. Lond., and J. Bernard Coleman, A.R.C.Sc. Pp. xvi + 224. (London: J. and A. Churchill, 1896.)

THIS book, which is founded on Prof. Clowes' larger "Practical Chemistry and Qualitative Analysis" is intended for the use of general students and of technical students in schools and colleges who are desirous of acquiring a general elementary knowledge of chemistry, and who propose to acquire this knowledge in the only true way, viz. by themselves performing experiments in a laboratory. For such students the book furnishes an admirable guide. The first eighty pages contain excellent instructions as to the preparation and use of apparatus, the methods of carrying out ordinary chemical operations, and the modes of demonstrating the properties of common gases and liquids. The remainder of the book is occupied with a course of qualitative analysis, which treats first, at considerable length, of the reactions for metals and for acid-radicles, and then of the actual analysis of simple and complex substances. There is, further, an appendix of useful tables and a good index.

The hand of the experienced and careful teacher is manifest throughout. The importance attached to cleanliness, neatness, and system in the rules given for working; the directions for the verification of the statements made, and for the keeping of the student's notebook; the precautions indicated as necessary for success in certain experiments, the careful attention to detail, and the emphasis given just where it is needed, show that the authors have knowledge not only of chemistry, but also of the "general and technical student," who, if he will observe the instructions, and work fairly through the book, cannot fail to acquire a real knowledge of his subject.

For boys and girls at school, we ourselves should recommend a course on somewhat different lines, starting, for instance, with air rather than with oxygen, following generally a historical sequence, taking the various chemical operations not *en bloc*, but as required in the course, and relegating qualitative analysis to a comparatively subordinate place. But taking things as they are, and accepting as a fact the existing requirements of various public examining bodies, this little work should prove widely useful as a carefully-arranged, clear, and accurate text-book.

Entomological Notes for the Young Collector. By William A. Morley. Pp. viii + 129. (London: Elliot Stock, 1896.)

THIS is a little book of the most popular kind, written with the intention of rendering the collecting of butterflies and moths easy to the youngest of beginners. It is illustrated by eight pages of figures representing apparatus, setting, &c., and the text is divided into twelve chapters, corresponding to the months of the year, each including a lesson on apparatus, collecting, rearing, &c., and a list of some of the principal *Lepidoptera* which appear in each month. The book may be useful to those for whom it is intended; and we congratulate the author on his good judgment in advising his readers to learn the Latin names, and to forget the English. Here and there a little revision would be useful; thus Entomology is defined as "that branch of natural history which bears special reference to four-winged insects known as butterflies and moths" (no other insects being even mentioned in the book); Cambridgeshire is the only locality given for *Papilio machaon* and *Vanessa antiopa*; *Lycena artaxerxes* is said to be "generally distributed in England"; of *L. corydon*, we read "On chalk cliffs, common"; and moths which come to sugar are said, as a rule, not to come to light.

W. F. K.

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LETTERS TO THE EDITOR.

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The Utility of Specific Characters.

IT is dangerous for a mere mortal to take part in the strife of the gods, or for "gyrating"¹ mathematicians to join battle with biologists. But as the enthusiasm of Prof. Weldon for his subject has so largely perturbed my normal gyrations, that I have devoted many months to the statistical theory of evolution, perhaps I may be permitted a word or two on the subject of the present controversy.

To demonstrate that natural selection, whether secular or periodic, is actually taking place in any species, and to measure its amount, is in the present state of our knowledge one of the grandest pieces of work that could be done. It would achieve for the Darwinian theory what Hertz achieved for the Maxwellian theory of light. At present no one has gone further in the direction of this demonstration than Prof. Weldon, and I am inclined to think with Mr. Thiselton-Dyer—and I take it in opposition to Prof. Lankester—that such a demonstration can only be achieved by the statistical method. If, however, we are to obtain a really solid result of that method, then the mathematical theory, and the logic used, must both be beyond suspicion. Now in any demonstration of the existence of natural selection two points must be borne in mind:

(a) A selective death-rate must be actually demonstrated. This is a problem for fine statistical theory.

(b) The correlation between organ and death-rate must be shown in itself to be not fortuitous. The character must have been selected because it is useful. This I take to be Prof. Lankester's point.

I propose to say a few words as to both (a) and (b).

It appears to me that both Prof. Lankester and Mr. Thiselton-Dyer allow that a selective death-rate has been established in the report on *Carcinas menas* of 1894-5. This view I take to be entirely erroneous, and I so expressed myself to Prof. Weldon and several members of the Committee before the *Report* was published. What Prof. Weldon demonstrates is this—that if crabs *chanced to grow in a particular manner*, then there would be a relation between death-rate and the size of a certain frontal ratio. What is quite certain is that at the time the *Report* was published, nobody knew how crabs grew; and I very much doubt whether Prof. Weldon, after his laborious two years' study of the growth of crabs, would now uphold the hypotheses he then adopted, e.g.:

(i.) That size could be taken as a safe measure of age.

(ii.) That young crabs of the same frontal ratio do not "scatter" as they grow older.

(iii.) That the amount of growth of crabs of any given frontal ratio is entirely independent of that ratio.

Yet if these—to me very improbable—hypotheses be not accepted, the supposed demonstration of a selective death-rate in *Carcinas menas* falls completely to the ground. The very hypothetical character of the conclusions of the *Report* of 1894-5, appears by his letter of August 26 to be now very fully recognised by Prof. Weldon himself. I am not, however, sure that it has been generally recognised. When the law of growth of crabs has been accurately ascertained, then I am convinced that it will require much more complex analysis than that of the *Report* to ascertain whether a selective death-rate does or does not exist. I should not have said so much on this first point did I not believe that next to blindly rejecting natural selection, the most dangerous course open to biologists is to accept a proof of its existence which is sure one day to be demonstrated as fallacious by one of the many opponents of Darwinism.

On the second point, surely Prof. Lankester is entirely in the right? It is not sufficient to show that there is a correlation between a certain frontal ratio and death-rate in order to assert that the frontal ratio is a cause of death-rate. Very probably it may be, but the demonstration is not logically complete, or at any rate a definition of cause has been adopted which does not appear

¹ The term is due to Prof. Lankester, who thus described us—I think it was to Mr. Thiselton-Dyer—in the early days of the Teaching University movement.

of much utility to science. If the height of the barometer be correlated with the frequency of explosions in mines, it would not appear utile to describe the barometer as a "cause" of the explosion. Or, to take another case, which is purely hypothetical, but which will, I think, illustrate Prof. Ray Lankester's point. There is, we will suppose, a purely random distribution of supernumerary teats in cows. But in my particular herd the two best milkers possess supernumerary teats (although there is no correlation between such teats and good milkers in general). I keep the calves of these two cows because they are good milkers, and by reason of this selection supernumerary teats become more and more common in my herd. At last I begin to preserve calves with supernumerary teats, really because this is a test of their descent from the good milkers, practically because I find them in themselves good milkers. Now, because I am a careful breeder, my cows may get a reputation at sales all over the country, and a correlation between supernumerary teats and good milkers may come to be generally recognised. This happens not because supernumerary teats are a cause of good milkers, but owing in the original instance to a *random* association of this variation with a utile variation. Thus, a primarily random association with a favourable variation may by the principle of heredity quite easily lead to a correlation which it would be of no profit to consider causal. If two characters be not correlated, and one be favourable to survival, then any selection of the favourable character, which hits a group of individuals having more than the average of the second character—and this may easily arise if we breed from comparatively few individuals—will by the principle of heredity lead to a *fortuitous* correlation. I do not assert that this is the case in the frontal ratio of crabs, but it seems to me that a link is really missing in the chain of demonstration. All causality is of course correlation, but the converse, which Prof. Weldon seems to hold and Prof. Lankester to controvert, is surely a dangerous doctrine?

KARL PEARSON.

September 10.

Specific Characters among the Mutillidæ.

THE discussion in your columns as to the utility of specific characters leads me to offer a few remarks on the Mutillidæ, an interesting family of Hymenoptera. In the arid region of the United States, this family is very numerously represented, and its members may be seen running about in warm weather, especially frequenting sandy places, roads and pathways. It is not at first apparent why the species should be so numerous, living under what seem to be identical or almost identical conditions; in 1893 (*Trans. Amer. Ent. Soc.*, xx, 343), I wrote: "It is difficult to account for the origin of so many species under conditions which can hardly at any time have been very diverse." But the region in question is inhabited by very many species of bees, the modifications of which have relation to a varied flora, as I have illustrated by particular instances elsewhere (*Proc. Ac. Nat. Sci. Phila.*, 1896, pp. 33-41). The various Mutillidæ are parasitic in the nests of these bees, and consequently do not live under identical conditions; we have a varied flora with its varied insect-visitors, and these with varied parasites, the whole series of phenomena intimately connected, though at first sight it would seem impossible to see any connection between the flowers and the mutillids, however indirect.

It must be a long time before the actual host-relations of all the mutillids are known, but I have a species now under observation, which may serve as an example. The bee *Diadasia diminuta* lives in colonies, burrowing perpendicular tunnels in the beaten pathway, which are produced somewhat above the level of the ground by means of fragile cylinders of sandy particles, designed to keep the tunnels from being filled with sand. The little *Sphærophthalma heterochroa* is the parasite of this bee, and the females may be seen in numbers running about between the burrows, now and then looking into them or entering. At once we see the utility of one of the specific characters of *S. heterochroa*—its small size. The larger species could not enter the small burrows of the bee.

The females of *S. heterochroa* are splendid little insects, ornamented with scarlet, black, and whitish. Like the females of all Mutillidæ, they are wingless. The much more active winged males, which are not so elegantly ornamented as the females, may be seen bustling about, looking for the latter. In

the Mutillidæ, the females are very varied in colour, markings and structure; while the males are much more uniform. Thus, Cameron says ("Biol. Cent. Amer. Hymenoptera," p. 259): "This general resemblance of the males not only makes their specific determination a work of difficulty, but it adds greatly to the task of assigning them to their respective partners of the opposite sex." If the bright and varied colours and markings were due to activity or a "katabolic tendency," it is in the winged males that they ought to be found; not, as is actually the case, in the wingless females. But on the principle of utility there may be an explanation. The males have to look for their respective females, and I believe the ornamentation of the latter assists their recognition.

There is a whole series of Mutillidæ which are very plainly coloured, from tawny through various shades to black, never with any scarlet, or conspicuous markings. These (*Photopsis* and *Brachycistis*) are all nocturnal, without any exception, and come to lights in the evening. But the systematists who have described many of these insects, were totally unaware of this circumstance until I pointed it out recently!

The moral of all this is, that to understand the real meaning of specific characters we must study the species in nature. We are hardly more likely to understand natural phenomena from the examination of dead animals alone, than a Hottentot would be to understand the apparatus of telegraphy. And eventually, I believe even the pure systematist will have to base his work on biological observation. It has been fondly hoped all along that absolute criteria of specific distinction would be found in the insects themselves, without reference to their habits; and the searchers for such "hall-marks," driven from point to point, have at length taken refuge in the male genitalia. But only a few days ago I received the following in a letter from M. Ernest André, the distinguished French student of Hymenoptera, and particularly Mutillidæ.

"Comme je l'ai dit, je crois qu'on attache aujourd'hui une trop grande importance aux caractères tirés de l'appareil génital mâle. Ces organes sont très variables, difficiles à apprécier, et ne concordent pas toujours avec les autres caractères morphologiques."

T. D. A. COCKERELL.

N.M. Biological Station, Mesilla, New Mexico,
U.S.A., August 25.

The Khmer of Kamboja.

IN NATURE, June 11, p. 135, I see a short notice of the work being done in Australia by Prof. R. Semon, of Jena, and that he classes the "Khmer and Chams of Kamboja" as "primitive Dravido-Australians." I hope that some of your anthropological experts will, as soon as possible, correct this serious mistake.

Mr. A. H. Keane, in NATURE, January 6, 1881, p. 222, calls these people "Caucasian"; but I presume they are now (1896) known to be what Captain C. J. F. S. Forbes classed them, *i.e.* Mon-Anam, in his "Languages of Further India."

J. R. Logan, in his "Ethnology of the Indo-Pacific Islands," published at Singapur and Pinang, in the *Journal of the Indian Archipelago* (1847-63), rightly classes them as Mon-Anam, giving their linguistic peculiarities and alliances, pronouns, &c., and numerals up to ten, those from one to five being identical with our Kol, Sontal, Munda, Ho (the most western relatives of the Mon-Anam alliance), and quite different to the Dravidian, numerals.

The now civilised Kambojans admit that the "Khmer dom" are the older and purer stock, whence they are descended, and that they were *hill savages*, which carries out what we know so far of these early pre-Burmo-Tibetan races from the Asam side.

Both in physique and languages, the Dravido-Austral aboriginal of India (south of Himalaya) and the Mon-Anam group are very distinct; the former are seen purest in the Andamani and Negrito, in whom there is an entire absence of Tibetan elements.

But the Mon-Anam (which includes the Khmer) were formed, as a race, by the mixture, or fusion, of (Sifan) Tibetans with the Dravidians, lying south of Himalaya from Nipal to East Asam. At one time this "Mon-Anam" race appears to have covered all Northern and Eastern Bengal, the whole of Asam,