

that the case rests. This photograph, said to be the work of Prof. E. D. Congdon, of Harvard, is extraordinarily bad. It represents a Batrachian lying on its back, seen from in front. Were we not told that it is *Alytes*, the fact could not have been ascertained, for all but the hands is a blur. The hands are seen from their dorsal surfaces. On the radial side of the wrist of the right hand is a lump which Dr. Kammerer claims as a *Brunftschwiele*. The phalanges of the thumb, as Dr. Kammerer expressly declares, are unmodified in this specimen, and no *Schwienen* are visible on the left arm or hand at all. Though on analogy with other genera *Schwienen* might well occur on the wrist or forearm, the proposition which Fig. 2 is intended to support is not that set forth in the original paper which I criticised (cf. especially *Arch. Entw.* 1909, xxviii. Taf. xvi., where a modified thumb is vaguely represented). In the text of the present paper we are told that the *Schwienen* are very variable in position and extent. I do not, however, find any mention of modification in digit iv. This finger is, of course, external, and could scarcely function in the embrace; nevertheless, the outer side of digit iv. is most conspicuously thickened in the right hand of the animal shown in Fig. 2. So striking is this appearance that everyone to whom I have shown the figure at first sight supposes this thickening to be the *Schwiele* illustrated. I myself, on looking at the picture before reading the details, had no doubt that this was the *Daumen* with its excrescence, the hand being thus supposed to present a palmar view. Dr. Boulenger at once pointed out to me that this interpretation was impossible, for the reason, among others, that the comparative lengths of the digits proved the hand to be shown in dorsal view, and that the modified digit is iv. It must be remembered that the photograph is so indistinct that much is left to the imagination.

The peculiarity of the right digit iv. would be still more manifest if Fig. 1, which gives a normal *Alytes*, were a genuine photograph. It has, however, been so clumsily painted up that the extremities are not like those of any animal. Each finger and toe has a painted outline, not always in the right place, and only on comparison with actual specimens can the full extent of the modification in digit iv. of Fig. 2 be appreciated. As it stands, this digit is very like the *Daumen* of the original figure. I will not yet venture on a positive interpretation, but I may remark that what the new evidence suggests is that these modifications, whatever they may be, and to whatever cause they may be due, can also appear on the outside of digit iv.

I find it difficult to understand why, if these structures are as Dr. Kammerer declares, he did not make a proper series of photomicrographs of them *in situ*, showing their several positions and forms—no very hard task for such an institution as the Versuchsanstalt. Entomologists and students of fungi make such photographs constantly. Even one good ordinary photograph or drawing would have shown more than the ambiguous pictures now offered us. If anyone wishes to see how *Alytes* looks in a good photograph, he should turn to Boulenger (*Bull. Ac. Roy. Belg.*, 1912, p. 573). The latest of Dr. Kammerer's figures dates from July, 1913. A long series of *Arch. Entw.* has been published during the years of the war, often with magnificent plates. Dr. Kammerer does not state how many modified *Alytes* he has had, but by implication they have been numerous. If, on second thoughts, he was unwilling to send one to England, could he have resisted the temptation to send one to the Berlin Museum to be shown to Prof. Baur, and so confound him and other sceptics? Three years had elapsed since we openly expressed our disbelief, but

I know that up to January, 1914, no such specimen had been sent.

Prof. MacBride urges that sceptics should repeat experiments on the inheritance of acquired characters. We, however, are likely to leave that task to those who regard it as a promising line of inquiry. Why do workers in that field so rarely follow up the claims of their predecessors? Each starts a new hare. Scarcely has one of their observations been repeated and confirmed in such a way that we could be sure of witnessing the alleged transmission if we were to try for ourselves. Brown-Séquard's observation on guinea-pigs is an exception. That has been repeated by various observers, until at length, by the work of Graham Brown, the mystery may be regarded as explained. The observation was true, but the interpretation was faulty. As I have often remarked, acquaintance with the normal course of heredity is an indispensable preliminary, without which no one can interpret the supposed effects of disturbance. This knowledge of normal genetic physiology is being slowly acquired, and already we have enough to show that several variations formerly attributed to changed conditions should not be so interpreted. Even in this case of *Alytes*, were a male with incontrovertible *Brunftschwienen* before our eyes, though confidence in Dr. Kammerer's statements would be greatly strengthened, the question of interpretation would remain, pending the acquisition of a knowledge of Batrachian genetics.

W. BATESON.

June 22.

The Food of Rats.

IN NATURE of September 19, 1918 (vol. cii., p. 53) a summary is given of an article by Prof. P. Chavigny on the food of rats. Some of the statements in this article appear to me to be extraordinary, particularly the alleged necessity for rats to get cooked human food. The hordes of rats which swarm along our foreshores, and in granaries and like places, could not possibly get sufficient cooked human food to keep them alive, yet they are plump and well-fed. Any one who has kept fowls or ducks in a rat-infested place knows that rats will carry off and devour chicks and ducklings, even dragging them from under the brooding mother, eating them raw. Attacks on living and dead human beings and smaller animals are by no means rare. Along the water-front rats freely catch and eat crabs, and they will devour raw fish with avidity.

Certainly rats will eat cooked food when they can get it, but they are omnivorous feeders, and I have personally known them not merely to gnaw, but to devour pumpkin, melon, apple, and other fruits. Of pumpkin-seeds they are very fond, and an apple-core makes a good bait for a trap. They do not seem to care much for raw beef; I have noticed them attack raw potatoes and pumpkin-seeds, neglecting raw steak which was lying alongside. Under a creeper in my garden near Sydney the common snail (*H. aspera*) was very abundant, and *M. decumanus* used to devour large quantities; the apex of the shell was always bitten off so that the mollusc could be readily extracted. On the Upper Waikato River, New Zealand, the same rat dives into the water and gathers the fresh-water *Unio*. On the river-banks the shells are gnawed open and the animal eaten. The shells are always bitten through at the same spot of one valve, but I forget now whether that was the right or left one.

In Australia at certain seasons a "cutworm" moth, known as the "bogong" or "bugong" (*Agrotis infusa*), swarms in myriads in many places, and is,

after the wings have been singed in a charcoal fire, used as an article of food by the aborigines. These moths sometimes invade the cities and crowd into houses and stores for the sake of darkness. At Melbourne, in a large sugar store, I have noticed *M. decumanus* collect the moths and eat the bodies, rejecting the wings.

There came under my notice lately at Pennant Hills, near Sydney, a case of a curious article of food for a rat. A rat gained access to the laundry attached to my house, and for some weeks it used to drag pieces of common soap behind any shelter and devour them. That the soap was really eaten was evident, because no particles were left lying about. Ultimately I succeeded in trapping the rat, which was a half-grown male, *M. decumanus*. An empty spring trap was placed open in a box having an opening just over the jaws. A piece of tissue-paper was arranged over the jaws and the whole covered with a thin layer of bran, a bait being laid at the far end of the box. On examination I found the intestines empty and the stomach gorged with fresh bran, which the rat had scooped up before entering the trap. Although I searched carefully I could never find any means of exit from the laundry or see the rat, but I presume it must have got other food somewhere, for absolutely nothing edible was ever placed in the laundry. The rats' excreta were always quite normal.

THOS. STEEL.

Sydney, April 28.

SOME RECENT ATOMIC WEIGHT DETERMINATIONS.

THE story, adequately told, of the evolution of ideas and the development of knowledge concerning the stoichiometrical constants we term atomic weights forms a most interesting chapter in the history of the philosophy of chemistry. In point of time it would extend over no very long span. There are men living who are personally cognisant of its most important phases, and some of them in early life were acquainted with others who may be said to have connected their own epoch with that of those who witnessed the beginning of experimental efforts to obtain quantitative estimations of their values.

The formulation of the laws of chemical combination involved the necessity for exact knowledge of the relative weights with which substances enter into such combination, and, as is well known, Dalton himself made tentative trials to obtain some definite conception of their measure. But Dalton was not a particularly skilful or accurate experimenter; his apparatus and methods of quantitative work were very crude and even below the standard of his time. This was fully recognised by his contemporaries, particularly by Berzelius, who may be said to have been the first to attempt precise determinations of atomic weights. The work of Berzelius and his coadjutors marks, in fact, an epoch in the history of the subject.

Of course, as is now well understood, the germ of Dalton's ideas, although he probably was unconscious of it, is to be found in the work of his predecessors, but it does not seem to be generally known that Cavendish, in effect, postulated and

put into practice the fundamental conceptions expressed in the laws of constant, multiple, and reciprocal proportions. He appears to have convinced himself years before the time of Proust and Berthollet that the same substance is invariably composed of the same elements united in the same proportion, and, as can be shown from his published writings, he made quantitative analyses on the implicit assumption of the other laws. This was first pointed out by George Wilson, and has been more fully developed in the course of a critical examination of Cavendish's memoirs in the Phil. Trans. for 1786 and 1788 on "Freezing Mixtures," contained in an annotated edition of his complete papers, published and unpublished, which it is to be hoped the Cambridge University Press may soon be in a position to issue.

It would occupy more space than is available to attempt to trace the several phases, which, like milestones, mark successive stages in the progress and development of knowledge concerning atomic weights, nor is it necessary to set out in detail the various reasons which have led chemists to recognise the imperative necessity of knowing these constants with the highest attainable precision. Philosophers like Berzelius always desired the utmost accuracy in the abstract interests of truth. But, to begin with, the only practical use of atomic weights, or combining proportions as they were called by Davy, was in quantitative efforts to elucidate the chemical composition of substances, and, considering the imperfections of quantitative methods, an approximation to exactitude sufficed. When substances began to be bought and sold on the results of analysis, atomic weights became of importance in commercial transactions, but even then, for the purpose of trade, no very high degree of accuracy was required. Even the numbers of Berzelius's time sufficed for the determination of exact formulæ, and enabled the nature and progress of a chemical change to be traced with precision.

But in recent time, and with the development of chemical theory, atomic weights have acquired a wider importance and a new significance, and a much higher degree of accuracy is demanded. It is, in fact, almost useless to discuss certain questions unless these constants have been rigorously determined. Very much now depends upon little differences—the little difference, indeed, frequently makes all the difference. But, unless this is established with reasonable certainty, it is a waste of time to base an argument upon it. We thus enter upon another and the latest phase in the development of the subject.

For this new departure, which may be said to start with Stas, the chemical world is greatly indebted to American chemists, such as J. P. Cooke and his colleagues, Oliver Huntington and Theodore Williams; and to J. W. Mallet, Morley, and Noyes. Prof. Theodore Williams has worthily maintained the traditions of the Harvard school, and it is largely to his work and example that the present high standard has been reached. We