nine families of Bombyces, ending with the *Psychidæ*, is written in the same careful and painstaking manner as its predecessor. The first volume has been well received abroad, but the foreign critics regret the absence of references, a deficiency more felt by them than by British lepidopterists. The foreign critics speak of the plates as a veritable storehouse of remarkable varieties; but we must again comment very severely on the action of the publishers in issuing two editions of the work, one with, and the other without illustrations, without any reference to the illustrated edition in the letterpress of the other, so far as we have noticed; and in the case of the second volume, without even as much as an advertisement to call attention to its existence.

There are several points of general scientific interest suggested by an examination of Mr. Barrett's book. A great number of species recorded as British by the older entomologists, but rejected by Doubleday and Stainton, have latterly been rediscovered and reinstated. This has happened so often, that it seems likely that when we eliminate accidentally introduced species (chiefly North American), and European species wrongly determined, it will be found that the information given by the older writers was far more accurate than the writers of the middle of the century were at all disposed to admit. Nor did the latter allow for the difficulty of communication with the continent at the beginning of the century, which added much to the improbability of specimens asserted to have been taken in England, having been simply brought over from the continent.

In estimating the probability of a reputed species being truly British, the chief factor to be taken into account is its continental range. It is evident that the British fauna is slowly changing, some specimens becoming rarer or even disappearing, and others becoming commoner, or establishing themselves in England for the first time. There is also some tendency in Mediterranean species to extend their range further north in Western Europe. As the late Mr. Stainton once remarked, the comparison of our present lists with those of the future, will be likely to yield highly unexpected and interesting results.

W. F. K.

Quellenkunde. Lehre von der Bildung und vom Vorkommen der Quellen und des Grundwassers. Von Hyppolyt J. Haas. 8vo. pp. 220. Illustrations in the text. (Leipzig: J. J. Weber, 1895.)

PROF. HAAS, of Kiel, when asked to edit and bring up to date the "Quellenkunde" of Abbé Paramelle, came to the conclusion that in order to state the present position of the science of springs and underground water in a satisfactory form, an entirely new work was necessary. Hence the book under notice. In such small compass, nothing approaching a complete treatise could possibly be attempted. The chief features of springs, their classification and relation to geological conditions, are discussed according to a clearly arranged plan under five principal heads. First comes a discussion of springs in general, including an historical introduction, in illustration of which several of Athanasius Kircher's quaint pictures are reproduced. The following sections deal with thermal and mineral springs, underground water, and the art of finding springs. In the last division we find some remarks on the divining-rod. The book should prove useful to students of physical geography and to those concerned with the practical utilisation of a water-supply derived from wells.

A number of diagrams are reproduced from the works of Daubrée and other authorities. Although several English authors are cited, we fear that Prof. Haas has not made himself familiar at first hand with the literature of the subject in English, which is by no means meagre in records of original observations on the movements of underground water, and deserves more recognition than it receives.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Uniformitarianism in Geology.

DR. ALFRED WALLACE, in his letter to NATURE of May 2, calls attention to the significant fact that catastrophes caused by volcanoes "may be of greater magnitude now than in geologic times," owing to the crust of the earth being thicker now than it was then. He, however, is mistaken in supposing that this consideration has been overlooked by geologists. If he will kindly refer to "Geology," vol. i. p. 449, he will find it there stated, speaking of the older fissure and explosive eruptions, that "there is nothing to show that this [the explosive] action was on the same scale of magnitude and permanence as those of late Tertiary and recent date. With the greater thickness of the earth's crust and the greater resistance presented by its rigidity, volcanic eruptions must with time, as suggested long ago by Elie de Beaumont, have altered with the alterations of those conditions, and may now be exhibited under a phase very different from those of the earlier periods."

Or again, he will find in "The Position of Geology" ("Collected Papers," p. i.) it stated that, though one form of volcanic action (the fissure) was more active in the past than at present, that "explosive eruptions are more violent now than in former times." And again, at p. 145 of the same work, I remark that "while with the thinner crust of former times, there would be a more frequent extrusion of the molten rock, there are probably with the thicker crust now formed and consequently its greater resistance, greater forces stored in the explosive eruptions of the present day."

The instance relied upon by Dr. Wallace is, however, another striking example, if others were needed—though in this case it is on the inverse side as against meteorological agencies—of the non-uniformity *in degree* between the action of the forces of past and present times. The increased thickness of the crust is not, however, the sole cause of the violence of recent eruptions, nor are they, I imagine, due to the presence of occluded water in the volcanic foci. The terrific eruptions of Krakatão and other volcanoes are, I conceive, due simply to the access of vast volumes of surface waters and their sudden flashing into steam.

Volcanic action, therefore, does not seem to me to be in any way in contradiction to the conception of uniformity of *kind* or law, and to non-uniformity on the question of *degree*.

Sevenoaks, May 4. JOSEPH PRESTWICH.

Green Oysters.

I HAVE just received a "Note," extracted from the *Monitore Zoologico Italiano*, of Florence, by Dr. Carazzi, in which a number of unsupported statements are made as to "phagocytosis in Mollusca."

Amongst other statements, I find "Non solo sono osservazioni erronee quelle del Lankester, malamente ripetute dello Chatin, ma lo sono egualmente quelle del Pelseneer e del Bruyne." I am surprised that my zoological friends in Florence should publish a bare statement of this nature without a shred of evidence to support it. I desire to draw attention to the simple assertion made by Dr. Carazzi, and to let those who are responsible know that I and others expect him to show in detail what is the error in the observations published by me on the green oysters of Marennes.

It is certainly not a usual thing for a Society to allow an author to print vague accusations of inaccuracy in reference to other writers, without the smallest attempt to justify such accusations.

Dr. Carazzi's assertion is all the more remarkable, since it appears that he has not examined the true *huîtres de Marennes* at all, and is singularly ill-informed as to the histology and physiology of Mollusca.

I shall be very much surprised if Dr. Carazzi can show that the observations published by me on green oysters in 1886 (*Quart. Journ. Micr. Sci.* vol. xxvi.) are erroneous, and shall at once re-examine the matter if he succeeds in throwing doubt on the facts as stated by me.

Inferences from observed facts stand in a different position from the observations themselves.

I was the first to describe the cells laden with green granules which occur in the epithelium of the gills and labial tentacles of the Marennes oyster.

I also showed that such cells are present in the common oysters, but that the granules they contain are not green. I further showed that these cells occur abundantly on the *surface* of the gills, crawling about and exhibiting amœboid movement. I also showed that the Marennes oysters are specially fed upon *Navicula ostrearia* which contains a highly refractory blue pigment "Marennin," and I *inferred* that the granular cells of the gills derive their colour from the blue pigment of the naviculæ—since it was shown long ago by Gaillon (in 1824) that the *huitres de Marennes* are purposely placed by the oyster-culturist into tanks containing the *Navicula ostrearia*; that when placed there they have gills of the usual yellow-brown colour, but rapidly acquire the green colour; that they actually feed on the *Navicula ostrearia*, and that when removed from this article of diet, they lose the green colour of gills.

The inference that the "granular cells" are to be regarded as wandering phagocytes, was not first published by me; and, though I have no doubt of its justification, I may point out that it is an interpretation, and not an observation of fact.

Lastly, let me say that I showed by chemical analysis that the green colour of the oyster's gill is *not* due to any metallic base—either copper, iron, or chromium. The statement made by Carazzi that there is "abbondanza di sesqui-ossido di ferro" in the mud of the tanks where the oysters are fed, is therefore doubly futile. Every one knows that such mud contains abundance of iron; but as there is no iron in the green pigment of the oyster, it is useless to draw attention to the iron in the mud.

E. RAY LANKESTER.

Oxford, May 4.

The Origin of the Cultivated Cineraria.

I MADE two objections to Mr. Dyer's account of the history of the Cineraria; the careful reader will observe that his letter meets neither. Mr. Dyer informed us that the cultivated Cinerarias were produced "*by the gradual accumulation of small variations*," *i.e.* without the selection of definite sports. My object in adducing historical evidence of Cineraria sports was to prevent Mr. Dyer's pronouncement from being repeated without further evidence. That purpose I think has been attained; for I notice that in now restating his account Mr. Dyer does not refer to the point, though it was the object of his original exhibition of the Cineraria to the Royal Society. That the Cineraria was an excellent "illustration of the amount of variation which could be brought about under artificial conditions in a limited time" I should be the last to dispute. As I showed in my first letter, there is evidence that the time was very short indeed.

Compared with this point, the second question—that of the hybrid origin of cultivated Cinerarias—is of subordinate interest. For the view that they were originally hybrids, resulting from crosses between *C. cruenta*, *C. lanata*, and other species, I have given the evidence, quoting the explicit statement of contemporaries and the almost universal opinion of practical gardeners, with references to the sources of information. Mr. Dyer, however (with him Mr. Rolfe) declares that they are descended from *C. cruenta* alone. Is this statement a mere inference from the want of likeness between particular cultivated Cinerarias and the wild species, or have Mr. Dyer and Mr. Rolfe evidence of a more substantial character? Of course these authorities may be right, and the rest who have written on the matter may be wrong; but I ask for proof of this, and the request can hardly be thought unreasonable.

Mr. Dyer has referred to a remark I made at the meeting respecting the Camellia. At the risk of diverting attention from the real issues, I feel bound to speak of this, for I was then in the wrong. In justice the circumstances must be stated. Speaking of the Cineraria, Mr. Dyer declared that though the flowers have changed so much, the foliage, which had not been an object of Selection, still resembled that of his wild plant. I replied that though this might be true of the Cineraria, it led to no universal induction, for it is well known that the foliage of many plants selected solely for their flowers or for their fruits had varied greatly. As an illustration taken on the spur of the moment, I said that though the matter had not come within my own observation, there was, I believed, a passage in one of Darwin's books to the effect that the foliage of the several kinds of Camellia differed so much that they could be recognised by it alone. Upon Mr. Dyer interjecting that this was not true, I immediately gave up the illustration as not coming within my own knowledge, and substituted that of the Apple, of which I myself know several kinds to have distinct and characteristic foliage. Such examples may be multiplied indefinitely. Now the passage in Darwin is as follows:—"Verlot mentions a gardener who could distinguish 150 kinds of Camellia when not in flower" ("Animals and Plants," ed. 1885, II. chap. xxii. p. 238); but Darwin takes the case as an illustration of the fact that structures "though appearing to an unpractised eye absolutely undistinguishable, yet really differ." My use of this case was therefore a wrong one, and as Mr. Dyer has thought fit again to refer to the matter, I take the opportunity of withdrawing it once more.

W. BATESON.

St. John's College, Cambridge, May 5.

The Assumptions in Boltzmann's Minimum Theorem.

MR. CULVERWELL'S letter in your issue of April 18 leaves many important points in connection with the reversibility of Boltzmann's Minimum Theorem untouched. On the question as to what different people mean (or think they mean) when they assert that the theorem is true, enough has already been said. What we want to know is what assumptions are involved in the mathematical proofs of the theorem, why they have to be made, and for what systems they are likely to hold. This question has been ably treated by Mr. Burbury, but in view of Prof. Boltzmann's assertion that the theorem is one of probability, it is desirable to examine more fully where probability considerations enter into proofs such as Dr. Watson's, which contain no explicit reference to them.

Dr. Watson starts by assuming two sets of molecules so distributed that the *numbers* having coordinates and momenta within the limits of the corresponding differentials are

$$F(P_1 \ldots Q_m)dP_1 \ldots dQ_m \text{ and } f(p_1 \ldots q_n)dp_1 \ldots dq_n.$$

If, however, the differential elements are taken very small (as when we consider a volume-element comparable with molecular dimensions), these expressions no longer represent numbers of molecules, and it is *assumed* that in this case they represent the *probabilities* of a molecule having coordinates and momenta within the given limits.

It is then necessary to *assume* that *the probabilities for the two kinds of molecules are independent of each other*. This assumption was pointed out to me by Mr. Burbury, and is what I intended to imply in my previous letter when I said that Dr. Watson's assumption was more *natural* than any other. Under these circumstances alone can we assert that the probability of a given combination of coordinates and momenta of *two* molecules is proportional to

$$F dP_1 \ldots dQ_n \times f dp_1 \ldots dq_n$$

To make the proof independent of the choice of coordinates, let $y_1 \ldots y_{m+n}$ be *any other* system of coordinates specifying the *pair* of molecules, so chosen that $y_1 = O$ at the beginning of an encounter. Then if $\dot{x}_1 \ldots x_{m+n}$ denote the corresponding momenta, we may employ the theorem proved in my last British Association Report, § 14, to write the above expression in the form

$$Ff/dy_1 dy_2 \ldots dy_n dx_1 \ldots dx_{m+n}.$$

and if we write $(dy_1/dt)dt$ for dy_1, the probability of a configuration in which an encounter will take place in the time-element dt becomes

$$Ff/dy_2 \ldots dx_{m+n}(dy_1/dt)dt$$

corresponding to Watson's expression with (dy_1/dt) in place of (dq_n/dt). This step involves the assumption (made above) that dy_1 is small in comparison with the dimensions of a molecule.

From this point on Dr. Watson's proof is easy. But it will be seen that the probabilities for two molecules are not independent of each other *after* a collision between them. The method would fail if the same pair of molecules were likely to collide repeatedly. Thus the Minimum Theorem depends on the free motions of the molecules quite as much as on the collisions themselves, and it only applies to gases whose molecules mix freely among each other between collisions, not to media where they are densely crowded. In such cases, however, we have Mr. Burbury's investigation (*Phil. Mag.* January 1894).

If we were to reverse the motion exactly, we should have one in which the probabilities for two molecules *before* an