of piece-meal legislation bearing upon the public health has been passed; that much of this legislation, despite subsequent amendments, still remains obscure and unsatisfactory is clearly shown in the results of proceedings undertaken by those whose duty it is to put it in force. What cause to wonder, then, if the lay reader, by reason of obscurities in the particular Act itself, or from the fact that either amendments have been introduced by succeeding enactments or the particular Act is itself an amendment of earlier statutes, becomes bewildered, and a laudable desire to master an important subject is nipped in the bud? Those who are concerned in the administration of this branch of the law have frequent occasions to regret the lamentable ignorance existing among all sections of the community as to their powers and liabilities in matters which may seriously affect their vital interests. Any simplification and consolidation, therefore, more especially when it is undertaken, as in this instance, by gentlemen of recognised legal ability, should prove very welcome not only to health officers, but also to the general public.

The decisions of the authors of the work to collate the various provisions contained in different enactments dealing with the same subject, and to present these—so far as possible—freed of all legal phraseology, was a happy one; it makes the work unique in its serviceability to the lay reader, who will gain from its perusal a clearer and more definite knowledge of the public health laws of the different parts of the United Kingdom than he would succeed in doing—at a much greater sacrifice of time and patience—from any other publication dealing with the same subject.

Involution and Evolution according to the Philosophy of Cycles. By Kalpa. (London: Eyre and Spottiswoode, 1894.)

THIS is one of the books that most people would be glad to lay aside, and, indeed, it is very difficult to say with what object it has been written. The cycles described have nothing to do with approximate commensurability of planetary motions, and certainly not with evolution as understood in the modern acceptance of the term. The author is a disciple of the school of Mme. Blavatsky, and draws his inspiration from that source, tinged, it may be, with something of esoteric Buddhism, and a good deal "spider-wove from his own brain." If anyone wants to know what absurdities modern theosophy is capable of, by all means let him read it, but most people will be satisfied to take the contents at second-hand. A very objectionable feature in the book is the occasional quotation at the heads of chapters of extracts from recognised writers of authority, conveying the impression that the contents of the chapters following are based upon modern science, and would meet the approval of the authors from whom the quotations are made. One illustration will be sufficient to show the style of the author's reasoning and the character of the information conveyed. The particular object is to demonstrate the birth of comets and worlds (p. 148). "But the least subtilised type of those disembodied groups does not take the same direction as the others. It keeps going in orbits round the sun, shooting beams at him, which, expelled (seemingly, at least), spread out behind as a lengthy tail. Then, when the sun takes a short rest, his brilliancy nearly spent, that entity moves off, its beams showing the way, but greatly reduced, and of which nought remains ere the comet disappears for parts unknown. It will be known to us as comet I." We have, approximately, 200 pages of this sort of stuff, paragraph after paragraph, all of which are utterly incomprehensible, and to wind up the whole we have sheet after sheet of diagrams or illustrations which no man can understand, and on which we should imagine the author himself would pass a very doubtful examination.

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The Mountains of California. By John Muir. Pp. 381. (London: T. Fisher Unwin, 1894.)

FEW regions offer more remarkable subjects for the student of nature than the State of California. There are the two great mountain ranges—the Coast Range on the west, and the Sierra Nevada on the east. Great cañons furrow the latter to depths of from two thousand to five thousand feet, and in the middle of the deepest of them flourish the Sequoia, the noble sugar and yellow pines, Douglas spruce, Libocedrus, and the silver firs, each a giant of its kind. Floods of lava cover the north half of the High Sierra, and volcanic craters, recent and in all stages of decay, are dotted over it. Mount Shasta is one of these volcanic cones, rising to a height of more than fourteen thousand feet above sea-level. Deep grooves flute the sides of the mountains, and testify to glacial erosion. It appears that so far south as latitude thirty-six degrees, traces of glacial action abound. Mr. Muir has found sixty-five residual glaciers in the portion of the Sierra lying between latitudes thirty-six and thirty-nine degrees. The first one of these was discovered by him in 1871 between two of the peaks of the Merced group. He also determined the rate of motion of the middle of the Maclure glacier, near Mount Lyell, to be but little more than an inch a day. Mount Shasta has three glaciers; while Mount Whitney, though the highest mountain in the range, has none.

The special features of the volume are the descriptions of the glaciers, glacier lakes, and glacier meadows in the Californian mountains, and the interesting account of the grand forest-trees of the Sierra.

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.]

Origin of Classes among the "Parasol" Ants.

MR. J. H. HART is Superintendent of the Royal Botanic Gardens in Trinidad. He has sent me a copy of his report presented to the Legislative Council in March 1893, and has drawn my attention to certain facts contained in it concerning the "Parasol" ants—the leaf-cutting ants which feed on the fungi developed in masses of the cut leaves carried to their nests. Both Mr. Bates and Mr. Belt described these ants; but described, it seems, different, though nearly allied, species, the habits of which are partially unlike. As they are garden-pests, Mr. Hart was led to examine into the development and social arrangements of these ants; establishing, to that end, artificial nests, after the manner adopted by Sir John Lubbock. Several of the facts set down have an important bearing on a question now under discussion. The following extracts, in which they are named, I abridge by omitting passages not relevant to the issue:—

"The history of my nests is as follows: Numbers one and two were both taken (August 9) on the same day, while destroying nests in the Gardens, and were portions of separate nests but of the same species. No. 3 was procured on September 5, and is evidently a different although an allied species to Nos. 1 and 2.

"Finding neither of my nests had a queen, I procured one from another nest about to be destroyed, and placed it with No. 1 nest. It was received by the workers, and at once attended by a numerous retinue in royal style. On August 30 I removed the queen from No. 1 and placed it with No. 2, when it was again received in a most loyal manner. . . .

"Ants taken from Nos. 1 and 2 and placed with No. 3 were immediately destroyed by the latter, and even the soldiers of No. 3, as well as workers or nurses, were destroyed when placed with Nos. 1 and 2.

"In nest No. 2, from which I removed the queen on August 30, there are now in the pupa stage several queens and several

males. The forms of ant in nests Nos. 1 and 2 are as follows: (a) queen (b) male (both winged, but the queen loses its wings after marital flight), (c) large workers, (d) small workers, and (e) nurses. In nest No. 3 I have not yet seen the queen or male, but it possesses—(a) soldier, (b) larger workers, (c) smaller workers, and (d) nurses; but these are different in form to those of nests No. 1 and No. 2. Probably we might add a third form of worker, as there are several sizes in the nest. . . .

"It is curious that in No. 1 nest, from which the queen was removed on August 30, new queens and males are now being developed, while in No. 2 nest, where the queen is at present, nothing but workers have been brought out, and if a queen larva or pupa is placed there it is at once destroyed, while worker larvæ or pupæ are amicably received. In No. 3 all the eggs, larvæ, and pupæ collected with the nest have been hatched, and no eggs have since made their appearance to date. There is no queen with this nest. . . . On November 14 I attempted to prove by experiment how small a number of 'parasol' ants it required to form a new colony. I placed two dozen of ants (one dozen workers and one dozen nurses) in two separate nests, No. 4 and No. 5. With No. 4 I placed a few larvæ with a few rose petals for them to manipulate. With No. 5 I gave a small piece of nest covered with mycelium. On the 16th these nests were destroyed by small foraging ants, known as the 'sugar' or 'meat' ant, and I had to remove them and replace with a new colony. My notes on these are not sufficiently lengthy to be of much importance. But I noted four eggs laid on the 16th, or two days after being placed in their new quarters; no queen being present. The experiment is being continued. I may mention that in No. 4 nest, in which no fungus was present, the larvæ of all sizes appeared to change into the pupæ stage at once for want of food [a fact corresponding with the fact I have named as observed by myself sixty years ago in the case of wasp larvæ]. The circumstance tends to show that the development of the insect is influenced entirely by the feeding it gets in the larvæ stage.

"In nest No. 2 before the introduction of a queen there were no eggs or larvæ. The first worker was hatched on October 27, or fifty-seven days afterwards, and a continual succession has since been maintained, but as yet (November 19) no males or queens have made their appearance."

In a letter accompanying the report, Mr. Hart says:—

"Since these were published, my notes go to prove that ants can practically manufacture at will; male, female, soldier, worker, or nurse. Some of the workers are capable of laying eggs, and from these can be produced all the various forms as well as from a queen's egg.

"There does not, however, appear to be any difference in the character of the food; as I cannot find that the larger larvæ are fed with anything different to that given to the smaller."

These results were obtained before the recent discussion of the question commenced, and as they agree with the results reached by Grassi in the case of the *Termites*, it can now scarcely be doubted that the various forms or classes among the social insects are wholly determined by the treatment of the larvæ.

St. Leonards, December 2.

HERBERT SPENCER.

"Acquired Characters."

I DO not think we are in any way bound by the terms of the law enunciated by Lamarck. Those laws may be shown to be erroneous in all but the suggestion of a principle which may possibly be developed into an important and far-reaching doctrine, and if so the importance of the doctrine will be in no wise diminished by the crudity of the early suggestion. There is scarcely any scientific generalisation which does not require an amended enunciation in each generation if it is to be in accordance with the contemporary state of knowledge. Nevertheless it seems to me that the second law of Lamarck does not state that a character acquired by individuals *for the first time* is inherited, or "alters the potential character of the species." The law states that nature preserves by generation what has been acquired by individuals by the influence of the circumstances to which *their race* has been long exposed: *not* by the influence of the circumstances to which they alone have been exposed in their own individual existences.

Leaving Lamarck's laws and doctrines entirely out of the question, if we define an acquired character as one which is determined by the "operation on the individual of given and

related quantities of external agencies," I am not aware that anyone has ever asserted that such a character is inherited, in the sense of being completely reproduced in the offspring without the operation of those external agencies. But I think there is reason to believe that if the same quantity of external agency acts on successive generations, it will produce more effect on the second than on the first, or, to use more correct language, that the effect in the second generation will be increased by a potentiality derived from the first. It is argued that the very possibility of the acquisition of new characters by the individual under new conditions is a proof that the old character had not become fixed and congenital after the action of the old condition on thousands of successive generations. But this is an illustration of the difficulty of completely expressing the problem in abstract language without reference to particular cases. If we consider the case of the pigmentation of the skin of the flounder, we find experimentally that exposure to light of the lower side for some years produces some pigmentation, but not so much as that on the upper side exposed in the individual for the same time. The action on the two sides in the individual being thus equal, or even greater on the lower side, how are we to account for the difference in favour of the upper? Evidently the congenital potentiality of the two sides is different. The old character has then become fixed and congenital to a certain very important degree. If no effect were produced by the action of light on the lower side of the individual, there would be no evidence that the congenital difference in the two sides had been produced by the difference in the relation to light repeated in countless successive generations. On the other hand, if the equal exposure of both sides produced equal pigmentation in the same time, this would be evidence that the difference in the pigmentation under normal conditions was not a congenital character at all. But as the facts stand, the only conclusion which is in accordance with them is that the congenital difference between the two sides is due to the gradual accumulation of slight effects on the congenital potentiality of the germ consequent upon the action of light in the individual. I could mention many other similar instances, which I think do constitute a reason for "associating the somewhat superficial and late responses of the parts of a growing individual to normal or abnormal forces of its environment with that more subtle and profound disturbance which is permanent and affects the potential character of the germ."

I am far, however, from supposing that all specific, generic, or morphological characters are due to the direct action of the environment in the soma, and equally far from admitting that every one of these characters has a part to play in the struggle for existence.

J. T. CUNNINGHAM.

Plymouth, November 30.

THE distinction between the "acquired characters" of Lamarck and the other "responsive characters" which follow the "influence of the normal environment" is, I venture to think, not very important. The two kinds of characters are indeed admitted by Prof. Lankester to be "of the same order," and their essential unity is clearly shown when we attempt to trace the history of evolution as Lamarck conceived it.

The first increase in length of the neck of the giraffe or swan was no doubt, according to Lamarck, "an acquisition under new conditions of new character." But when the process had started, its subsequent stages could hardly be spoken of in this way. The effort of stretching, which was supposed to supply the condition for further increase, was then neither "new" nor "special and abnormal."

In the numerous discussions of the last seven years the term "acquired" has been employed to cover both classes of characters, and, indeed, the argument has chiefly turned on the effect of normal rather than abnormal and special conditions, because the evidence supplied by the former for or against hereditary transmission was so much more convincing than that supplied by the latter.

Although the term "acquired" is an unfortunate one, and has added many difficulties and obscurities which would have been avoided by the substitution of Prof. Lankester's term, "responsive," I think it would only increase the difficulties if it were now authoritatively maintained that, although the majority of instances discussed and the really crucial cases adduced are "of the same order" as acquired characters, they must no longer be called by this name.

I entirely agree with Prof. Lankester as to the mutual anta-