

has kept silkworms or bred butterflies. The assertion that there is absolutely only a difference in the time at which the successive skins are formed in this and in ordinary ecdysis, is but assertion on the part of Mr. Lowne. Indeed, controversy becomes profitless if authority is to be substituted for fact, and an attempt made to silence opponents and stop inquiry by such positive assertions as the above and the following:—"The imaginal skin is likewise derived from cells laid down in contact with the imaginal discs." If Mr. Lowne will be so good as to explain what no books tell me, and I fail to make out myself, I will study what he says with great attention, and thank him heartily. He knows me well enough to feel assured that I would do so; but it is useless, and he must permit me to say that it is not in good taste, for him to comment about the "return of darkness," and to use expressions more positive and arbitrary than are called for.

Let us, if we can, get at the facts concerning some of these marvellous changes. For this there is nothing like discussion, carried on with care and consideration, even for an opponent; and though the fittest may be certain that he will survive, don't let any one be in too great haste to proclaim himself either survivor or fittest, or call himself strong and others weak, as has been done once already by one distinguished evolutionist. Evolution is a much quieter and far more complex process than some enthusiasts would have us believe.

Mr. Lowne appeals to the fly. By all means let the fly be the subject of our inquiries. Of this creature he says, the nervous system undergoes *modification* but not *degeneration*. Now I ask, what part of the nervous system that is present in the maggot can Mr. Lowne find in the fly? I have studied both fly and maggot carefully, have worked at the matter long, and have utterly failed to find a trace of the nerve tissue of the maggot in the fly. Not only so, but I find the nerves of the fly as different as are the muscles from those of the maggot. The latter are altogether distinct in structure and in action. They contract at a very different rate, and are very different in many particulars.

Again, I must ask Mr. Lowne if he has seen any vestige of the mouth organs in the larva, for he says:—"It is the mouth organs of the larva which are new formations, not those of the imago." I have failed in my attempts to find any traces. There are other assertions about the alimentary canal and the sexual organs which are not proved. Does Mr. Lowne mean to say, for instance, that he or anyone else can adduce any reliable observations to prove that "the sexual organs are gradually developed, even from the time when the embryo is enclosed in the egg"? On p. 112 of his book on this very matter he says that he has not been able to verify Dr. Weissman's assertion as to their presence, even in the larva; and now he suggests they exist in the egg!

But I must ask Mr. Lowne to explain what he means by saying in his letter, that it is an "utter mistake to suppose that any insect is re-developed during the pupa state," and that the nervous system "never undergoes degeneration;" because on p. 116 of his own book, published only last year, I find the following passage: "All the tissues of the larva undergo *degeneration*, and the imaginal tissues are *re-developed* . . . under conditions similar to those appertaining to the formation of the embryonic tissues from the yolk!"

LIONEL S. BEALE

The Auditory Nerves of Gasteropoda

In your issue for October 26, I notice an account of Leydig's recent paper on the auditory organ of the Gasteropoda, which, though excellent in other respects, has an error of omission which I should like to see rectified. When so important a discovery for morphology is discussed as that of the innervation of the otolithic sac from the supra-oesophageal in place of the sub-oesophageal ganglion which is its apparent connection in all Gasteropoda (excepting the Heteropodous forms), the credit of it should be given to the right man. That man is the most eminent and accurate of French comparative anatomists—M. Lacaze-Duthiers. Prof. Leydig states in the beginning of his own paper that Lacaze-Duthiers' statements on this subject (published in the *Comptes Rendus* about three years ago, if my memory serves me, and curiously mistranslated, *sus-oesophagien* being rendered sub-oesophageal in one of the first numbers of the Monthly Microscopical Journal), caused him to direct his attention again to this subject, and he has, as a result, confirmed the observations of the French *savant*, which were in opposition to the previously-received views of all observers, himself and Leydig included. Germany has a host of indefatigable anatomists, and the services of Franz Leydig, of Tubingen, are brilliant enough to eclipse most zooto-

mical reputations; but let us not, at this moment above all others, forget to do justice, when the opportunity occurs, to a naturalist whose comprehensive, accurate, and beautiful zootomical monographs, rich in discoveries, have done more than those of any other Frenchman to sustain the great name of Cuvier's school.

Naples, Dec. 8

E. R. LANKESTER

DR. CARPENTER AND DR. MAYER

AT the Anniversary Dinner of the Royal Society on November 30, I was honoured by a request from the President to say a few words in acknowledgment of the toast to the Copley Medalist. I did so, stating briefly the origin of my acquaintance with Dr. Mayer's writings. Though Dr. Carpenter at the time was within sight of me, it did not occur to me to introduce his name into my remarks. A few days afterwards I was favoured by a letter from Dr. Carpenter, in which he reminds me somewhat sharply of this and other lapses as regards himself, and requests me to rectify the omission by a brief communication to the *Athenæum* or to NATURE. It will be fairer to Dr. Carpenter, and more agreeable to me, if he would state his own case *in extenso*. Here is his letter:—

"University of London, Burlington Gardens, W.,
December 5th, 1871.

"MY DEAR TYNDALL,—If I correctly apprehended what you said at the Dinner of the Royal Society in regard to Dr. Mayer, you repeated what you had previously stated in your Lecture at the Royal Institution in 1863, as to the entire ignorance of Mayer's work which prevailed in this country until you brought it into notice on that occasion.

"Now, I very distinctly remember that a few days previously to that Lecture, I mentioned to you that as far back as 1851 I had become acquainted, through the late Dr. Baly, with one of Dr. Mayer's earlier publications; and that, in bringing before the readers of the *British and Foreign Medical Review* (of which I was then the Editor) the 'Correlation' doctrine, as developed in Physics by Grove, and in Physiology by myself, I had stated that we had both been to a great extent anticipated by Mayer—as I should have shown much more fully if the pamphlet had earlier come into my hands.

"I also most distinctly remember that, as you stated in that Lecture, no one in this country—'not even Sir Henry Holland, who knows everything'—had ever heard of Mayer, I spoke to you again on the subject a few days afterwards; and that you then expressed your regret at having entirely forgotten what had previously passed between us on the subject.

"As it would seem that this second mention of the matter has also passed from your mind, I shall be obliged by your looking at the passages I have marked in pp. 227 and 237 of the accompanying volume, from which I think that you will be satisfied that I had at that date correctly apprehended Mayer's fundamental idea, and that I have done the best to put it before the public that I could under the circumstances—the article having been in type and ready for press before his pamphlet came into my hands.

"Since, in thus bringing forward Mayer, I spontaneously abdicated the position to which I had previously believed myself entitled, of having been the first to put forward the idea that all the manifestations of Force exhibited by a living organism have their source *ab extra*, and not—as taught by physiologists up to that time—*ab intra*, I venture to hope that you will do me the justice of stating the real facts of the case in a short communication either to the *Athenæum* or to NATURE.—I remain, my dear Tyndall, yours faithfully,

"WILLIAM B. CARPENTER

"Prof. Tyndall."

This letter was accompanied by a volume of the *Medico-Chirurgical Review*, containing an article headed, "Grove, Carpenter, &c., on the Correlation of Forces,