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. Weissmann'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-œsophageal in place of the sub-œsophageal 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-œsophagien* being rendered *sub*-œsophageal 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 zootomical 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, Physical and Vital." As I am very anxious that my *amende* to Dr. Carpenter should be all that he could desire, I shall deem it a favour to be permitted to publish in NATURE the passages to which, by marginal pencil marks, he has directed my attention. The first of them is this :—

"We now come to the memoir 'On the Mutual Relations of the Vital and Physical Forces,' communicated to the Royal Society by Dr. Carpenter, which bears date June 20, 1850, and which is published in the 'Philosophical Transactions' for last year. This, we believe, is the first *systematic* attempt that has been made, in this country at least, to work out the subject, and, as it is mainly an expansion of the ideas which had been put forth in our own pages at the beginning of 1848, the author may claim priority as regards the enunciation and development of the idea, both of Dr. Fowler and Dr. Radcliffe, although to a certain degree anticipated by Mr. Newport. We shall presently find, however, that both these gentlemen were themselves anticipated in a quarter they little guessed, and the whole case is obviously one of a kind of which the history of physiology as well as of other sciences furnishes many examples, in which a connecting idea, developed in another department of inquiry, struck many individuals at once as applicable to the same class of facts, and was wrought out by them in different modes, and with various degrees of success, according to their previous habits of thought."

The impersonal way in which this and other passages of the article distribute merit among scientific authors caused me to ask Dr. Carpenter who wrote it. His reply to me was "I thought I had made it sufficiently plain to you that the article was written by myself."

Here follow the other marked passages quoted in full :—

"We must not omit, however, to give our readers some account of the remarkable production of Dr. Mayer, who seems to have arrived at conclusions in all essential respects similar to those of Prof. Grove and Dr. Carpenter previously to the publication of the first edition of the 'Correlation of the Physical Forces,' though subsequently to the delivery of the lectures in which Prof. Grove first announced his views and to the publication of the abstract of them. Of the existence of this treatise we have only recently been made aware, and we venture to affirm that Prof. Grove and Dr. Carpenter were alike ignorant of it. We bring it before the public now, both as an act of justice to its author, and also because it affords additional evidence in favour of the Correlation doctrine, that it should have been independently worked out by a clear and intelligent thinker.

"The first part of Dr. Mayer's treatise is concerned entirely with physical forces. He starts with the two axioms, 'Ex nihilo nil fit,' and 'Nil fit ad nihilum,' and founds upon abstract considerations his first argument for the unity of force, and for the convertibility of those which are commonly accounted distinct forces. Of this convertibility he then proceeds to adduce experimental proof, in very much the same mode with Prof. Grove, and he at last arrives at the following scheme expressive of their relations.

1. Force of Gravity. } Mechanical Force.
2. Motion. } Mechanical Effect.
A. Simple.
B. Undulating, vibratory.

Imponderables. { 3. Heat. 4. Magnetism, Electricity, Galvanic current.

5. Chemical decomposition of certain elements. Chemical combination of certain other elements. } Chemical Force.

"He then passes on to the study of vital phenomena, and he finds, like Dr. Carpenter, the source of all change in the living organism, as well animal as vegetable, in the forces acting upon it *ab externo;* whilst the changes in its own composition he considers to be the immediate source of the forces which are generated in it. He does not enter, like Dr. Carpenter, into an analysis of the phenomena of growth and development, but fixes his attention rather upon the production of heat, light, electricity, and (above all) motion by living bodies, and aims to show that all these forces are developed in the course of material changes in the organism, and hold a certain definite relation to them. On these points his exposition is very full and complete, and the perusal of his essay will amply repay any who desire to see how much may be done in imparting precision and clearness to physiological reasoning by minds trained in the school of exact science."

To these passages I would add one other brief quotation regarding the conversion of heat into electricity :—

"Of the production of electricity by heat, the phenomena first brought into view by Seebeck, and known under the name of 'thermo-electricity,' afford the most characteristic example. When dissimilar metals are made to touch, or are soldered together, and are heated at the point of contact, a current of electricity is set in motion, which has a definite direction according to the metal employed, and which continues as long as an increasing temperature is pervading them, ceasing when the temperature is stationary, and flowing in the contrary direction whilst it is decreasing" (pp. 213-14).

Having thus, it may be tardily, done justice to Dr. Carpenter, a very few words regarding his letter will complete the subject.

1. Dr. Carpenter has *not* correctly apprehended what I said at the dinner of the Royal Society in regard to Dr. Mayer. Neither at that dinner nor on any other occasion did I say that the ignorance of Mayer's labours in this country was "*entire.*"

2. I have not been altogether unmindful of Dr. Carpenter's desire to have his name mentioned in connection with this subject. In the printed report of the lecture referred to by Dr. Carpenter, delivered not in 1863 but in 1862, and published in the Proceedings of the Royal Institution for that year, these words appear—"Mayer's physiological writings have been referred to by physiologists—by Dr. Carpenter, for example—in terms of honouring recognition. We have hitherto, indeed, obtained fragmentary glimpses of the man, partly from physicists, partly from physiologists; but his total merit has never yet been recognised as it assuredly would have been had he chosen a happier mode of publication."

3. If this be not sufficient, my error was one of ignorance, not of will; for it is an entirely new idea to me that Dr. Carpenter regarded his relationship to Dr. Mayer in the light of a "spontaneous abdication," and it explains to me, what I could not previously understand, the importance attached by Dr. Carpenter to the passages above quoted.

4. I have looked at p. 227, and, indeed, throughout the entire article in the *Medico-Chirurgical Review* (and elsewhere), for evidence to prove that "at that date" (or at any other date), Dr. Carpenter had "correctly apprehended Mayer's fundamental idea," which is that of quantitative or numerical equivalence. Had I found such evidence, it would give me sincere pleasure to reproduce it here, but my search for it has not been successful.

5. This however entirely depends on my ability to appreciate such evidence. Holding the opinion that he does regarding the claims of his work to public recognition, Dr. Carpenter is perfectly consistent in demanding that even in an after-dinner speech those claims shall not be ignored.

JOHN TYNDALL