and such a discussion, though independent of optics, would be certain to have important applications in it, because its results would often still apply when translated into language of the electro-magnetic theory. The mathematical investigation of vibrations might be made more clear and definite when it is freed from the necessity of adapting itself to experimental verification.

Chapter xviii. is a useful one, dealing with "theories based on the mutual reaction between ether and matter," but we might have wished for a more satisfactory introduction to the electro-magnetic theory that is given in the last two chapters. The way in which the subject is approached may illustrate some of the remarks made in the beginning of this review. There is no doubt a very serious difficulty in explaining the fundamental notions underlying the theory, and Mr. Basset, instead of making an attempt to help the student over the difficulty, suddenly plunges into a series of equations, referring us to Maxwell's book for an explanation even of his symbols.

We have perhaps given an inadequate idea of the contents of Mr. Basset's book, which no doubt lends itself to criticism from the physicist's point of view, but which nevertheless fills a gap and possesses merits which will render it of great value to the student of optics.

ARTHUR SCHUSTER.

THE APODIDÆ.

The Apodidæ: a Morphological Study. By H. M. Bernard, M.A. Cantab. (London: Macmillan, 1892.)

THE title of this little book is misleading. It is not a treatise on the Apodidæ, but a statement of the author's speculations on the relations of the Phyllopodous Crustacea and Branchiate Arachnida to the Chætopod Worms. The new observations recorded are few, and the most important, that as to the presumed hermaphroditism of *Apus cancriformis*, quite insufficiently set forth, and, so far as can be judged from the author's meagre statement, erroneous.

Mr. Bernard appears to be completely misinformed as to current views on the relationships of Apus to other Crustacea, and of that group, through it, to the parapodiate worms. Apparently he addresses himself to a lay audience, and poses, to start with, as the discoverer of a new and unsuspected agreement between the lower Crustacea and the Chætopoda. This may serve to excite the interest of uninstructed readers, but the zoologist knows that such pretensions are due either to defective acquaintance with the subject or to a want of candour on Mr. Bernard's part. The arguments by which Mr. Bernard endeavours to support his thesis are, many of them, those which have been effectively used by his predecessors in the same cause; others are new and remarkable only for their arbitrary character and the evidence which they give of the author's boldness in writing a book on a morphological problem. Mr. Bernard draws attention to the absence of developed articulations in the limbs of Apus as giving them a resemblance to the parapodia of Chætopoda. He states that this absence "has already been pointed out by Lankester and others, but its true significance does not seem to have been noticed." This is an incorrect allusion to my essays on the appendages and nervous system of Apus (*Q. J. Micr. Sci.*, 1881), and on Limulus an Arachnid (*ibid.*), which is the more to be regretted since they appear to have furnished Mr. Bernard with such of his theories as well as his facts as will bear examination. At p. 368, *loc. cit.*, my statement runs—

"I have long been of the opinion which Prof. Claus appears to hold, that the appendages of the Arthropoda are homologous (or, to use a more distinctive term, 'homogenous') with the appendages of the Chætopoda, and on this account I consider it a proper step in classification to associate the Chætopoda with the Arthropoda and Rotifera in one large phylum—the Appendiculata."

Yet Mr. Bernard comes forward to tell us that he now for the first time draws attention to the true significance of the absence of articulations in the limbs of Apus, although (as he admits) this condition was especially noted and very carefully described eleven years ago by me in the same essay in which the above paragraph as to the relationship of Arthropoda and Chætopoda occurs. This is a sample of Mr. Bernard's method of claiming novelty for what he has to say when dealing with old materials. Frequently he asserts in strong language novel propositions which are purely speculative and of the truth of which no evidence is adduced. There is in no part of this little book any evidence that the author has made use of living or of well-preserved material, or has had any special opportunities of studying the genera and species of Apodidæ; nor does it appear that he has any experience as a zoologist which might give some weight to his fanciful conceptions. On the contrary, these crude speculations and dogmatic assertions are his first original contributions to zoological literature. I regret to be obliged to say that in my opinion (which I am called upon to express candidly in these pages) "The Apodidæ" is not a book which can be recommended either as a repository of fact or as a model of the method in which a morphological problem should be attacked.

E. RAY LANKESTER.

OUR BOOK SHELF.

Anatomy, Physiology, Morphology, and Development of the Blow-fly (Calliphora erythrocephala). Part III. By B. Thompson Lowne, F.R.C.S., F.L.S. (London: R. H. Porter, 1892.)

WE have before us another section of Mr. Lowne's work, which has grown upon the author's hands, and will form two volumes instead of the one originally intended. Part iii. is occupied with the internal anatomy of the imago, embryonic development, histology, and the development of the imago. On each of these heads a great amount of information is supplied, and the author's statements are illustrated by many figures. As to the puzzling question of the way in which the alimentary canal of the blow-fly is developed, Mr. Lowne holds an opinion which is probably shared with no second person. What Voeltzkow and Graber take to be the proctodæum, and what Korschelt and Heider believe to be the amniotic cavity, Mr. Lowne calls archenteron. He is content, as he tells us in his preface, to await the verdict of posterity on such conclusions as this. We are content to wait too. The subject is too difficult for full consideration in this place, and it would be unfair to express a strong opinion without ample discussion of the evidence. It is not unfair, we think, to characterize many of Mr. Lowne's

morphological speculations as simple mistakes. To compare an insect-embryo and its membranes with a Lamellibranch or an Ascidian in the extempore manner assumed so lightly by Mr. Lowne (p. 244) is not creditable. He tells us that he has no facts to guide him except the similarity of the form and disposition of the parts. Any reader who is not able to judge for himself should be very much on his guard when our author mentions Vertebrates or Ascidians, or indeed any other animals outside the class of Insects.

It is painful to speak with any disrespect of an author so laborious and so independent as Mr. Lowne. But these good qualities do not suffice to make a really good book. Advice will probably be thrown away, but we will offer one hint in the most friendly way. If Mr. Lowne before going to press would get his sheets revised by any cautious and well-informed zoologist, he would be saved from making statements which seriously impair his work.

L. C. M.

A Mendip Valley: its Inhabitants and Surroundings. By Theodore Compton. With Original Illustrations by Edward Theodore Compton. (London: Edward Stanford, 1891.)

THIS is an enlarged and revised edition of the well-known "Winscombe Sketches," and will be cordially welcomed by readers who can appreciate the presentation of natural facts in a poetic spirit. The author has spent the greater part of "thirty-three years of rural life" in the valley about which he writes, and every aspect of it he knows and loves. He tells much that is interesting, not only about the valley itself, but about the people who inhabit it, and about its archæological remains, its wild beasts, past and present, its birds, fish, reptiles, butterflies, and flowers. The style is simple and clear, and a certain charm is added to the writer's descriptions by the quaint reflections with which many of them are associated. An excellent chapter on the geological history of the Mendips is contributed by Prof. Lloyd Morgan. The illustrations are daintily conceived and executed, and harmonize well with the general tone of the text.

Key to Elementary Dynamics. By S. L. Loney, M.A. (Cambridge University Press, 1892.)

THOSE who are using the author's Elementary Treatise, whether they be teachers or students, will find this key very useful. The solutions to the examples are here worked out in full, so that even one who is going through the subject by himself will learn much in the nature of attacking problems by direct methods. The author's treatise is now so widely used that this key will come as a great boon to many.

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.*]

The Lightning Spectrum.

DURING the brilliant display of lightning on the evening of June 28, I took the opportunity of making some observations of the spectrum. The way in which the spectrum varied was very remarkable, some of the flashes giving apparently perfectly continuous spectra, while others gave a spectrum of bright lines, as already recorded by Kundt and others. The continuous spectrum appeared to be associated with the flashes of longest duration, which were accompanied by very little thunder, and the bright line spectrum with the more instantaneous flashes. Using a Liveing direct-vision spectroscope with a very accurate scale, I succeeded in measuring the positions of six lines in the green, all of which no doubt have been *observed* before, but in two cases at least the *positions* have not been previously measured. The wave-lengths of the lines observed were as follows—those determined by Vogel, Schuster, and Colonel John Herschel, being added for comparison:—

	Schuster.	Vogel.	Herschel.	Remarks.
(1) 5002	—	5002	5009	Brightest line
(2) 5168	5160	—	—	Rather dim
—	5182	5184	—	—
(3) 5350	5334	5341	—	Fairly bright
(4) 5430	—	—	—	Rather dim
(5) 5515	—	—	—	Fairly bright
—	5592	—	—	—
(6) 5675	—	—	—	Fairly bright

Other lines were seen both in the red and blue, but time did not permit any accurate determinations of their positions.

The lines (1) and (6) are undoubtedly the two brightest double lines of the air spectrum which occur in this region, but in the case of the other lines the coincidences are not so definite. The proximity of the line 5168 to the brightest carbon fluting (λ 5165) would suggest that it has its origin in the carbonic acid gas, which is always present in the atmosphere. The remaining lines do not appear to coincide with air lines, and their origins for the present are undetermined.

A. FOWLER.

Royal College of Science, South Kensington.

On the Line Spectra of the Elements.

PROF. RUNGE has not improved the position he has taken up by the new instance of a motion which he brings forward in last week's NATURE. The instance he gave in his preceding letter is a motion which, as I pointed out, could not take place within molecules. The motion he now gives is one which cannot even exist anywhere in nature. It would require a supply of power (energy per unit of time) increasing *ad infinitum*. The first instance he gave belongs to inapplicable kinematics, his new one to impossible dynamics. Neither has anything to do with the subject of my memoir.

He quotes the enunciation of a theorem from chapter iv. of my paper, but does not quote the sentence introducing that theorem, which would have made it plain that the motions spoken of in it are *motions which can take place within molecules and which can produce an undulation in the ether*, not the motions of a mere mathematical exercise irrespective of whether they are real or imaginary. The introductory sentence (p. 588) is in the following words:—"The motions of the electrons, the electric charges in the molecules, which are what excite the ethereal undulation, may be motions that are not confined to one plane. Accordingly *to study them* we must investigate what theorem corresponds to Fourier's theorem when the motion takes place along a line of double curvature." And then follows the demonstration and the enunciation quoted by Prof. Runge. In the foregoing words, in the introductory paragraphs of chapter iv. of my memoir, and in other passages scattered up and down through that chapter, I made it abundantly clear, as I thought, that I was dealing throughout *with a real physical problem of nature*, not engaging in mere mathematical exercises that travel into the infinite and impossible. I now see that I ought to have made more explicit statements upon this point for readers who would judge of each sentence apart from its context.

In order that a motion, $x=f(t)$, may be susceptible of treatment by Fourier's theorem, the following are conditions that must be fulfilled:—

1°. The motion must be recurrent, or capable of being approximated to by recurrent motions.

2°. The quantity represented by x must not become infinite.

3°. The quantity represented by t must not retreat.

I have been familiar with these limitations since I was a student, more than forty years ago. *They are known to all students.* I therefore thought it superfluous, and still think it ought to have been superfluous, to state them in my memoir. I thought it also irrelevant, *since none of the limitations could occur in the motions I was investigating;* and I wished to shorten my memoir by excluding all irrelevant matter. Prof. Runge, however, objects that I have not treated of violations of the first