molecular motions, and that it does not include the potential energy of molecular configuration—the most important doctrine, if true, in molecular science—is introduced in a footnote under cover of the unpretending German abbreviation “d.h.”

J. CLERK MAXWELL

(To be continued.)

WOLF'S HISTORY OF ASTRONOMY

Geschichte der Astronomie. Von Rudolf Wolf. (München : R. Oldenbourg, 1877.)

THE “History of Astronomy,” by Prof. Rudolf Wolf, of Zurich, a volume of 800 pages issued at a very moderate figure, is a contribution to the literature of the science of no ordinary value to the student. The production of such a work, involving an outline of the progress of astronomy from the earliest times to the present period, must have been a labour of great extent, requiring much research, notwithstanding the assistance that might be afforded by historical treatises previously in the hands of astronomers, and it is only due to Prof. Wolf to acknowledge the very able and complete manner in which he has accomplished the heavy task he had imposed upon himself some years since.

Those of our readers who may have been desirous of acquainting themselves with the general history of practical astronomy, and of familiarising themselves with the names and the nature of the services of the principal workers who have successively contributed to advance our knowledge of the science, more especially during the last three centuries, will, we think, have experienced difficulties which the volume before us is well calculated to obviate. The English reader has, it is true, Prof. Grant's classical work, the “History of Physical Astronomy,” but there is much to be found in this volume, which it was hardly within the scope of Prof. Grant's work to incorporate. The writer of these lines very well remembers the fragmentary manner in which, some thirty-five years since, an English student of practical astronomy was under the necessity of obtaining information, more especially in private reading; and it is one of the most happy circumstances for the astronomical student of the present day that this want of suitable guides has been to a great extent removed, and his time therefore need not be wasted in a search for knowledge in second-rate or doubtful authorities, mistakes which he would be not infrequently led into thereby, being corrected only after vexatious delay and trouble.

Prof. Wolf divides his work into three books. The first deals with ancient astronomy and progress down to the fifteenth century, including theories, instruments, and writings. The second commences with “the reformation of astronomy” consequent on the publication of the great work of Copernicus, “De Revolutionibus Orbium Coelestium,” and treats of the advances made to the time of Newton; we find therefore in this division a summary of the labours of Galileo, Apian, Tycho Brahe, Kepler, Fabricius, Harriot, Hevelius, Huyghens, Gascoigne, and many others, including notices of the more important publications of the period, which are of interest and value. The third book treats of “the new astronomy,” commencing with the discovery of universal gravitation and

brings down the history of astronomical research and discovery to the present epoch. A very great amount of information is compressed into this last section of the work, and it is here that the care and research of the author are more particularly evidenced. There is much to be found in it, for which we should look in vain in a collective and compendious form elsewhere. It is well and accurately put together, the few errors we have remarked being of comparatively trifling nature; thus the Saturnian satellite *Tethys* appears as *Thetis*. The biographical notes, which are extended to contemporary astronomers, will be a welcome feature to many readers.

Students and others interested in the history of the most ancient of the sciences, who can command a sufficient knowledge of the German language, will find their advantage in the possession of Prof. Wolf's elaborate work, and we must not omit to say that that great desideratum in all works of the kind—a very sufficient index, at least as regards names mentioned in the history, will render it of easy reference.

J. R. HIND

(To be continued.)

OUR BOOK SHELF

Photographic Spectra. 136 *Photographs of Metallic, Gaseous, and other Spectra printed by the Permanent Autotype Process.* With Introduction, Description, &c., by J. R. Capron, F.R.A.S. (E. and F. N. Spon.)

WE gather from the author's introduction that he has chiefly aimed “to popularise a subject hitherto somewhat of a sealed book, confined to the laboratories of workers in special research.” In this he should certainly succeed, though we think that his readers would not have been driven away if they had found a little more reference to the explanations of the various phenomena and the conclusions which have been drawn from them. As it is, the book is a good companion to Lecoq de Boisbaudran's “Spectres Lumineux.” The spectra are sharp and clear, and the autotype process has lent itself well to this reproduction. The results are all the more commendable because Mr. Capron has not had the advantages of considerable dispersion.

The account of the method employed is full and clear, and will make the book a very useful one to beginners in spectrography.

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. No notice is taken of anonymous communications.]

The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Sun-spots and Terrestrial Magnetism

PROF. PIAZZI SMYTH will no doubt welcome from any quarter a satisfactory answer to his question about the discrepancy between Dr. Wolf's sun-spot period, 11.1 years, and the supposed 10.5 years' period for the magnetic needle. If Mr. Smyth will refer to Prof. Loomis's chart of magnetic oscillations given in Prof. Balfour Stewart's paper on the subject in *NATURE* (vol. xvi. p. 10), he will see that there are exactly seven minimum-periods from 1787 to 1871, the mean of which is twelve years; the mean of the seven corresponding maximum-periods is 11.8 years. The true magnetic declination-period is then the mean of these, viz., 11.9 years. In exactly the same manner I have found that the mean period of sun-spots is 11.9 years. The auroral displays also have the same period.

But what is this period of 11.9 years? It is Jupiter's anomal-

istic year, or the time which elapses between two perihelion passages.

Prof. Wolf and Messrs. De la Rue, Stewart, and Loewy have all distinctly stated their belief that Jupiter is the chief cause in the production of sun-spots. This 11·9 years' period will then, I believe, remove what little doubt remains in some minds on the subject. Mr. John Allan Broun, F.R.S., has already shown in NATURE (vol. xvi. p. 62) that Dr. Wolf, to be consistent with his own relative numbers, ought to take a period of 11·94 years rather than one of 11·1, and while he himself favours a 10·5 years' period, he admits that there is no combination of planetary positions which would produce such.

I may perhaps be allowed to state here that in a paper I have just forwarded to the Royal Astronomical Society I have given what I believe are satisfactory reasons for the variations of these curves, and such as will enable us for the future to calculate with considerable accuracy the lengths of the periods, and guided by these reasons I have ventured to state my belief that we are now passing through a long minimum-period—one very similar to that which occurred at the close of the last century, and that the next maximum of sun-spots will fall in the year 1887.

I make this statement from an examination of the causes which produce the sun-spots; and it is so far remarkably confirmed by the behaviour of the magnetic needle. Mr. Broun, in NATURE, vol. xvii. p. 183, speaking of the very gradual manner in which the curve has been going to a minimum during the last three and a half years, remarks that "no such constant state of the sun's magnetic action will have been observed since the last years of the eighteenth century." To this I would add that immediately prior to the commencement of that long sun-spot minimum period, the mean of the magnetic interval, which occurred then (reckoning the interval from minimum to maximum), fell in the year 1785, and corresponded with the time of Jupiter's perihelion passage. Suppose now we represent this synchronism by 0, it will be found that the mean point in the next period lagged behind the perihelion 1·6 year; next, 5·3 years; next, 5·3 years. Having reached its maximum of lagging, in the next period it lagged 3·9 years; next, 1·2 year; next, 0·6 year; and in the last period the mean point fell in the year 1868, coinciding for the first time since 1785 with Jupiter's perihelion, and will be represented by 0. So that the magnetic oscillation in 1868 was just where it was in 1785. Is it not a natural inference, then, that we have commenced another cycle of magnetic declination?

What produces this lagging? This is a very important question, and one which I have reason to believe can be satisfactorily answered.

B. G. JENKINS

January 19

On a Means for Converting the Heat Motion Possessed by Matter at Normal Temperature into Work

My attention has just been directed to Mr. S. Tolver Preston's two papers in NATURE, vol. xvii. p. 31 and p. 202, in which he points out what appears to be an exception to the second law of thermodynamics. Some years ago I illustrated the same subject in a somewhat different manner by an experiment which is in some respects better suited for lecture purposes, and while the subject is being considered may be useful to your readers.

Into the cork of a large bottle were fitted two glass tubes. One tube went to the bottom of the bottle, its upper end being terminated in a fine jet. The other tube only passed a short distance into the bottle, and its upper end terminated about an inch above the cork. To its lower end was fixed some pieces of blotting-paper, to its upper end was attached a small test-tube, the two being connected by means of a piece of india-rubber tube. Some water was put in the bottle and the cork fitted close in its place. The test-tube was then filled with ether or some volatile fluid, and fitted to the end of the india-rubber tube.

After the apparatus had attained a uniform temperature, the test-tube was inverted, so as to cause the ether to flow down the tube, and enter the bottle, where it spread itself over the blotting paper and, rapidly evaporating, produced a pressure inside the bottle. The addition of the ether vapour to the air already at atmospheric pressure, produced a pressure sufficient to force the water up the tube and out of the jet, causing it to rise to a considerable height into the air. At the beginning of the experiment all the apparatus was at a uniform temperature, and, according to the generally received opinion, ought to have been incapable of developing energy, yet on account of the ether vapour not

being diffused through the system, it was able to do work at the expense of part of the heat in the system.

JOHN AITKEN

Darroch, Falkirk, January 18

No Butterflies in Iceland

ALLOW me to point out that the lepidopterous insects said by Olafsen (not Olafsson) and N. (not R.) Mohr, to be found in Iceland, are not butterflies at all, but moths, as shown by the generic term *Phalena* applied by each of those authors to every one of them—a term whose meaning your correspondent and his informant have failed to see. Those venerable authors, though dead and buried long before I ever heard of them, are old friends of mine, and I feel it incumbent on me to ask your readers not to impute to them this and other mistakes in Dr. Rae's letter. Whether there have been or still be butterflies in Iceland I am not competent to declare. I did not see any when I was there, but they may have got out of my way. I have, however, yet to learn that they exist in that country, and therefore I am inclined to believe Mr. McLachlan is right when he said that there are none. We have the testimony of the late Sir William Hooker ("Tour," &c., ed. 2, vol. i. p. 333) that no butterfly had ever been met with in Iceland up to 1809, the year in which he visited that island. Gliemann ("Geogr. Beschreib. Isl.," p. 165) in 1824 was unable to add to Mohr's list of twelve species of moths, and included no butterflies. If any of the latter have since been found it would be well for Dr. Rae to give his authority for the fact, otherwise his ingenious supposition that Icelandic butterflies and their larvæ have been destroyed since 1786, is unnecessary, and his "only possible way" of reconciling "perfectly opposite authorities" falls to the ground through the absence of any opposition on the part of the authorities he has cited.

ALFRED NEWTON

Magdalene College, Cambridge, January 25

[Dr. Rae writes "to explain and correct a mistake which, by a little care and attention on my part could and should have been so easily avoided."]

On some Peculiar Points in the Insect-Fauna of Chili

My friend Mr. Birchall misconstrued the meaning of my notes (NATURE, vol. xvii. p. 162) in a manner incomprehensible to me, when penning his own (p. 221). I, and many others, will share his "surprise" when he can produce any species of the genera *Carabus*, *Argynnis*, and *Colias*, or any of the *Limnophilidae* from Australia or New Zealand. If he will do me the favour to again read my notes he will find that I refer solely to Palearctic and Nearctic forms occurring in the Chilean sub-region and (unless by exception) nowhere else in the southern hemisphere.

Mr. Wallace's rebuke (p. 182) is to some extent merited. I did not give sufficient attention to the chapter in his work, to which he refers, in consequence of its general character. Mr. Wallace greatly extends the number of genera published by me as a sample. Some of these were perfectly familiar to me; others, I think, will fail to stand the test of minute application, partly because their distribution is more extended, partly because generic definitions are vague. I could add several interesting and marked genera. *Colias* may possibly be represented by more than one species on the Northern Andes; but it is the opinion of naturalists who, from practical acquaintance with the fauna of South America, and who, on a special point like this, are more competent than I to judge, that most of the very marked forms upon which I especially rely do not occur on the Northern Andes, which of late have been most assiduously worked by entomologists hunting insects for sale and perfectly alive to the value of such forms.

Mr. Darwin's theory alluded to by Mr. Birchall had not been overlooked. I was dealing with insects, and with a few marked genera, &c., of them, only. In plants there appears to be a tendency towards the appearance of analogous or identical forms all over the world when a sufficient altitude (varying according to the latitude) is reached. The laws that govern the distribution of the one ought equally to affect the other. Still the facts alluded to in my former letter remain unexplained. The southern portion of South America forms, as it were, an island, with a large admixture of Palearctic and Nearctic faunistic elements existing in no other part of the southern hemisphere.

Lewisham, January 19

ROBERT McLACHLAN