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 beng 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 Olaffson) and N. (not R.) Mohr, to be found in Iceland, are not butterflies at all, but moths, as shown by the generic term *Phalæna* 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 *Limnophilidæ* 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 Palæarctic and Nearctic forms occurring in the Chilian 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 Palæarctic and Nearctic faunistic elements existing in no other part of the southern hemisphere.

Lewisham, January 19 ROBERT MCLACHLAN