a mistake in stating the relation of Momentum to Energy or *vis viva*, and will endeavour to rectify it.

In quoting a dictum of the late Dr. Boole, Prof. Tait styles him the greatest logician the world has produced, or is likely to produce, for many a long day. In dissenting from this superlative, I do not refer to the men most widely known in recent years as logicians—Whately, Hamilton, Mill, Mansel; I consider that the comparison of them with Boole fails through the dissimilarity of the matters compared; his Logic was but to a very small extent the Logic of any one of these writers. The only person who cultivated Logic in the manner of Boole was another noted mathematician, the veteran De Morgan. Now, without undertaking to say which of these two had the greater genius, I do not scruple to affirm that the labours of De Morgan, in their common department, if only through longer continuance in time, resulted in a much larger number of contributions to the science than can be credited to Boole. The two men were friendly co-operators, not rivals; and they will, I have no doubt, be mentioned together as often as reference is made to the Algebraic extensions of Formal Logic.

Aberdeen, Dec. 9

A. BAIN

The Spectrum of the Aurora

THE brilliant displays of the Aurora Borealis observed in England on the 24th of September and the 14th of October, 1870,* were also generally observed in this country. The fact may be worthy of record in your journal, as indicating the unusual extent of the phenomena. As the newspapers in different parts of the United States contained full descriptions of these displays, a detailed account need not here be given. I may remark, however, that they were generally regarded as the most brilliant displays we have had since 1859. Fine auroras were again witnessed on Monday morning, October 24th, from 5 to 6.30 A.M., and on the evening of the same day, from 6 to 11 P.M. At 9 P.M. (on the evening of the 24th) an auroral arch passed very nearly through the zenith from the eastern to the western horizon, or rather from a point a few degrees south of east, to another somewhat north of west. An extraordinary number of more feeble auroras have been noticed during the last two months.

DANIEL KIRKWOOD

Bloomington, Indiana, Nov. 9

Can Aurora be Seen in Daylight?

I VENTURE to believe *not*, in spite of circumstantial accounts to the contrary, and I ground my belief on the following considerations:—I. No description of a daylight aurora that I have ever seen will bear a critical examination. Take that published in the last number of NATURE. Here two arcs of faint white lines are said to have been seen in a direction "almost due east," and certainly the illustration given is not very unlike the appearance that auroral arcs sometimes present. But auroral arcs, so far as I know, never appear in the east, and the conclusion, therefore, is unavoidable that the object observed was nothing more than a remarkably symmetrical form of cirrus cloud. In another instance, lately published, although the thing described is called a daylight aurora, I fail to see in the description anything more than an account of the appearances presented when a high canopy of cloud clears off bodily from the sky with a sharp, straight edge, which by perspective becomes an arch. In the case referred to, the clouds clearing off from the direction of magnetic north, the arch corresponded in position with that of an aurora, and hence was set down as auroral. In a third account of a daylight aurora, it is expressly mentioned that the sky was hazy, and a solar halo visible, a condition of things which, while it would make the occurrence of aurora-like cirrus extremely probable, would be specially unfavourable to the visibility of a true aurora; for certainly if so delicate and phosphorescent a light as that of an aurora is to be seen at all in the daytime, it can only be under circumstances the most favourable as regards clearness of the lower atmosphere.

2. A comparison of the auroral light with the light of other objects whose visibility can be more easily measured, tends strongly to confirm the view I have advanced. No one who remembers Donati's comet at its brightest will hesitate to allow that for intrinsic brilliancy that object surpassed the most vivid aurora. Yet Donati's comet at its brightest could not be detected

with the naked eye until about half an hour after sunset, and then only the head could be seen.

3. The modifications of cirrus cloud are so infinitely diversified, and sometimes so very remarkable, as to offer a great temptation to the observer to invest them with the mysterious attributes of the aurora. Moreover, they do occasionally present a very striking resemblance to pencils of auroral light, differing, however, essentially in the character of fixity which they possess, as well as in the absence of any determinate relation to the magnetic pole or zenith.

On the grounds now stated I venture to refer daylight auroras in general to the large class of "errors of observation."

Clifton, Dec. 13

GEORGE F. BURDER, M.D.

The North London Naturalists' Club

THE Secretary of the North London Naturalists' Club desires the Editor of NATURE to correct an incorrect statement which appeared in the last number of that journal. The North London Naturalists' Club is not broken up, it is not six years old, nor has it ever met on a Monday. Its last meeting was on Thursday, Nov. 24, at Myddelton Hall. Three subjects were then exhibited and explained, viz. "The Structure and Growth of the Yeast Plant," "The Structure of the Gastric Teeth in the Lobster," and "The Anatomy of Amphioxus." The meeting was thinly attended, but that is no alarming phenomenon for societies of this kind. The previous meeting on Oct. 27 was a very full one, owing to a paper read by a deservedly well-known member of the club on the highly interesting subject of "Spontaneous Generation." It must be confessed that the club is not so vigorous as when first started, but these facts show that it is by no means defunct.

J. SLADE,
Hon. Sec. N.L.N.C.

Browning's Spectroscope

IN the last number of NATURE there is a description by Prof. Young of a spectroscope, in which the prisms are made to alter their positions relatively to each other by bending backwards and forwards the metal work to which they are attached. I should of course not wish to offer any opinion on the efficiency or otherwise of this arrangement.

My reason for writing is that in the course of the article Prof. Young goes out of his way to remark that, in attaching bars at right angles to the bases of the prisms in my Automatic Spectroscope, I have adopted a plan of Mr. Rutherford's. Will you kindly permit me to state that I began my Automatic Spectroscope in the year 1862, and that, so far as publication consists in exhibiting anything to a large number of persons, I had published it in the year 1863. I have reason to believe that M. Duboscq also attached bars in a similar manner to the bases of prisms with the intention of obtaining a minimum deviation* adjustment, about the same time as myself, or soon afterwards. I do not know at what time Mr. Rutherford may have contrived his plan, but, as I have never read any description of his instrument, I must disavow having adopted any plan of his. At the same time I must remark that it is a small step towards obtaining the complicated movement required to produce an *automatic* minimum deviation adjustment, and it seems to me that it is a step every person would be likely to take who wished to obtain the adjustment by a mechanical motion.

111, Minories, Dec. 12

JOHN BROWNING

Evolution of Light

YOUR correspondent, who describes in the number of NATURE of the 17th ult. a faint light observed by him on tearing strips from a woven fabric in the dark, may be interested to know that a similar phenomenon is noticed by Mr. Grove in his "Correlation of Physical Forces," as occurring with indiarubber waterproof cloth (4th ed. p. 48).

Mr. Grove ranks it under phenomena of heat and light, rather than of electricity.

C. J. T.

Fungi

It is very unfair that the mushroom family should lie under a ban, because Locusta, at the instigation of Agrippina, employed

* NATURE, Nos. 49, 50, and 51.

some kind as a medium for conveying poison into the stomach of Claudius. With equal justice the mild Calenian wine would have been in ill repute because poison was not unfrequently mixed with it—

Occurrit matrona potens, quæ molle Calenum
Porrectura viro miscet sitiēte rubetam.

I cannot therefore think that the bad name clinging to the whole family of agarics was thus incurred; for Locusta did not employ a poisonous fungus for her deadly purpose: she mixed poison with some kind of mushroom of which Claudius was particularly fond, and of which he had no doubt often partaken. The words of Tacitus are explicit; he says that "the writers of those times have related that poison was poured into a dish of *boleti*, of which the Emperor was fond;" "Temporum illorum scriptores prodiderint infusum delectabili cibo boletorum venenum." (An. xii. 66.) Suetonius is equally clear: "Boletus in quo cibi genere venenum acceperat." (Nero 33.) Pliny, too, seems to regard the boleti, which he calls an excellent food, as the vehicle conveying the poison: "Veneno Tiberio Claudio principi per hanc occasionem a conjuge Agrippina dato." (Nat. Hist. xxii. 22.) Cases of accidental poisoning by fungi no doubt occasionally happened amongst the ancients as amongst ourselves, but I doubt whether any of the family of fungi were ever designedly employed as a poison. According to Pliny, Annæus Serenus, the prefect of Nero's guard, with his tribunes and centurions, accidentally met their death by eating some poisonous fungus; I am not aware that any other writer records the circumstance; it is rather curious that Seneca, a very dear and intimate friend of Serenus, makes no allusion to the cause of his friend's death, in his touching lament over it, when we remember the philosopher's intense aversion to the fungus tribe. Here is a specimen of his vigorous diatribe: "Good gods! how many men does one belly engage! What! Do you think that those boleti—a pleasant poison—albeit they hurt not now, conceal within them no hidden mischief?" (Ep. xcv.) In another place (Ep. cviii.) he speaks of boleti and oysters together as things he had for ever renounced: "For they are not food, they serve only to tickle the appetite, constraining those that are full to eat more; a very gratifying amusement to such persons as stuff themselves with such things as readily go down, and as readily return." The *boletus* instrumental in causing Claudius's death has been supposed to be the *Amanita caesarea*, the specific name being given to this fungus on that account, but the point cannot be decided. That the genus *Amanita* was known to Pliny appears pretty evident from his description: it is first covered by a volva, egg-like, and then it breaks through this and rises on its stem. I can find no distinctive mention of the tubes or pores, characteristic of the order Polyporei, in any classical author. The *boletus* of the ancients might have included the modern genus *Boletus* and some of the Agaricini. Some of the Polyporei are no doubt denoted by the *μύκητες ἀπὸ τῶν βίζων καὶ παρὰ τὰς βίβας φύμμενοι* of Theophrastus (iii. 7, § 6); and Pliny probably means the same when he speaks of fungi growing on trees. Whatever the boleti were, they were highly esteemed; we find them not unfrequently contrasted with *fungi* and *suilli*:—

Vilbus ancipites fungi ponentur amicis,
Boletus domino. (Juv. Sat. v. 146.)

Compare also Martial (iii. 60):

Sunt tibi boleti; fungos ego sumo suillos.

Boleti were so good that you could not trust a slave to convey them to a friend; he would be sure to eat them on the way:—

Argentum atque aurum facile est, lænamque togamque
Mittere; boletos mittere difficile est. (xiii. 48.)

What the kind known as *suilli*, "hog-fungi," were, cannot be determined.

W. HOUGHTON

Hereditary Deformities

THE facts about hereditary epilepsy in guinea-pigs, mentioned in NATURE of 3rd ult., on page 14, appear to show that mutilations may be inherited when accompanied by functional derangements; though there appears to be very little, if any, evidence of mutilation being inherited when not so accompanied.

Dr. Carpenter says somewhere (I cannot find the reference) that small scars are sometimes more persistent than large ones. We might consequently expect that they would be liable to become hereditary. But this does not appear to be the fact. To mention an obvious instance: in many countries, the ears of all the girls, and of many of the boys, are pierced for earrings. We could not expect to find the perforation hereditary, but it would

not be wonderful if the external scar were to be so; the smallness of the operation, not amounting to mutilation, and not producing any functional disturbance, might be thought to be in favour of this result. But I am not aware that it is ever found.

JOSEPH JOHN MURPHY

Old Forge, Dunmurry, Co. Antrim

The Colour of Feathers and of Butterflies' Wings

THE change of colour observed by E. V. F. (NATURE, No. 55) in the red parts of the wing of a butterfly by the application of muriatic acid, is in all probability due to the red colouring containing a trace of copper in its composition. I have demonstrated the almost universal presence of that metal in the sea, in the earth, in fish, in flesh, in vegetables.

Not long ago Professor Church showed it to exist in the red feathers of birds.

SEPTIMUS PIESSE

Chiswick

Man's Bare Back

WILL you be good enough to favour me with a small space in your excellent journal for these few lines, in answer to Mr. Wallace's difficulty with regard to the nudity of the back of man. According to Darwinian principles, there is what is called correlation of growth, by which I believe is meant, that an organ, or some part of the organism, is selected, not because itself is useful, but because its growth is somehow correlated to some other organ which is useful. Now, as a growth is admitted which exists by virtue of its correlation to some useful organ, why should not an atrophy of some part of the organism also be admitted as correlated to some organ which has been naturally selected on account of its usefulness? Although the nudity of the back of man is not in itself useful, nevertheless the atrophy of the hair on his back may be correlated to the development of some organ peculiar to man, and which is useful to him; or, in other words, the growth of the hair on the human back, although in itself useful, is incompatible with the growth of some other organ which may be infinitely more useful to him. Such atrophy, for all we know to the contrary, may be in some way correlated to cerebral development, to the erect posture, to the development of the hand, to the organs of speech, &c. At all events, if we cannot positively state that our dorsal nudity is so correlated, we certainly cannot say that it is not. I do not think that Mr. Wallace is justified in excluding the nudity of the back of man from the theory of natural selection, because he cannot show that it is useful. It may not in itself be useful, but it may be subordinate to some organ which is most useful. I consider that if the principles of a correlated atrophy be admitted, the stumblingblock of our bare backs will cease to trouble Darwinian thinkers. The argument struck me when I first read Mr. Wallace's remarkable "Contributions to Natural Selection," but as I now see, by his article in p. 9, No. 53, Vol. III. of NATURE, that his difficulty has not been answered, I venture to address the foregoing to you, with the hope that my argument may be of some utility.

E. BONAIVIA, M.D.,
M.E.S. of London

Nov. 13

Loss of Temperature in Climbing

FREQUENT reference has been lately made to the thermometrical results obtained by Dr. Lortet while walking up Mont Blanc, in which, as stated by Dr. Corfield in NATURE of Dec. 1, his temperature fell about 4° C. in ascending nearly 4,000 metres. Mr. E. R. Lankester informs me that when undertaking the same journey, he also found his temperature much lower when he was up high than when he started.

At first sight this result is unexpected, but it was predicted long ago by Joule, who, in the appendix to a paper read at the British Association in 1843, states thus:—"If an animal were engaged in turning a piece of machinery, or in ascending a mountain, I apprehend that, in proportion to the muscular effort put forth for the purpose, a diminution of the heat evolved in the system by a given chemical action would be experienced."

This is evidently the key to the whole subject, and I hope shortly to publish other results, now in an incomplete form, which bear on the point.

It is evident that the potential energy which results from ascending a hill is gained by the expenditure of work, and a loss of heat from the body must naturally follow; while in walking on