

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 sitientæ 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 proderint infusum delectabili cibo boletorum venenum." (An. xii. 66.) Suetonius is equally clear: "Boletos 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 cesarea*, 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 *μύκρον ἀπὸ τῶν βύζαν καὶ παρὰ τὰς βίβλας φούμενοι* 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*:—

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

Compare also Martial (iii. 60):

Sunt tibi boleti; fungos ego sumq̄ 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.

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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

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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.

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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