mination of the nature of that phenomenon, if true, serves to confirm the statements of others who have observed similar occurrences. The confirmation, however, only extends so far, as proving that, a yellowish substance has been seen on bodies after a fall of rain. I can mention instances where sudden appearances have taken place, and would be called *yellow* rain, without the aid of ordinary rain.

The earliest case I am acquainted with occurs in Pliny,* who says :—

"In like manner it rained iron in Lucania, the year before that in which M. Crassus was slain by the Parthians ; and together with him all the Lucani, his soldiers, of whom there were many in his army. That which came down in the rain resembled in some sort sponges ; and the aruspices gave warning to take heed of wounds from above."

This records two kinds of showers, viz., iron and sponges, but they were, I believe, one shower, although I have never seen any explanation of this curious passage. An account of a few "preternatural showers," I have lately come across, seems to contain exactly the information I have been seeking to clear up this difficulty. As it is novel, and was written in apparent ignorance of Pliny's record, it has at the present time some value. It says, in attempting to explain the cause of "red snow" :—

"This singular phenomenon, which has been observed in the Arctic regions, seems to be owing to the presence of oxide of iron in a state of minute division, and also of a resinous vegetable principle of an orange-red colour, belonging apparently to some lichen, of which too, perhaps, the iron may form one of the immediate principles."†

This explains Pliny to a certain extent ; in the common ignorance of both ascribing the redness of the snow to iron rust, or oxide of iron, and the matter that remained after the dissolution appearing of a sponge-like texture, and called a sponge or sponges by Pliny, and a resinous vegetable principle of an orange-red colour by Phillips.

This I think is the most probable meaning, and is, perhaps, the earliest "yellow" shower on record. The colour, as stated by the latter, is nearly correct, as shown by the investigation of MM. De Candolle and Prevost, who discovered, microscopically, that the red snow was due to the presence of small globules of a bright red colour, which were surrounded by a gelatinous membrane, transparent and slightly yellow, and were mixed with fragments of moss and dust. "An examination of the crimson snow found by Captain Ross in the Arctic regions by M. De Candolle proved it to be identical with the Alpine red snow ; the globular bodies are of a vegetable nature, and were once thought to belong to the *Uredo*, but M. Bauer disproved this, and named the plant *Protococcus nivalis*. There are cases, however, in which the presence of animalcules gives a reddish tinge to snow." ‡

Honeydew is mentioned by Pliny (Bk. XVIII. c. 28), who states that a great many of the ancients affirmed that dew burnt up by the scorching sun is the cause of honeydew on corn.

In the *Chronicum Scotorum* § is the earliest direct record of a "shower of honey" I know of. § It says : "A.D. 714 . . . . it rained a shower of honey upon Othan Bec. . . . ."

When it is known that any sudden appearance, giving a colour to the ground, or prominent places, or on trees, &c., is generally thought to have descended from above, this passage is quite intelligible. The "shower of honey" was nothing more than a "secretion of *aphides*," whose excrement has the privilege of emulating the sugar and honey in sweetness and purity."‖ Some contend that it is due solely to the exudation of the saccharine juices of trees ; but, feasible as this may seem, it is not sufficient to account for this phenomenon, which often extends over very large tracts of land. If the exudation is promoted by the aphides, and the dew increased by their own excrement, then this explanation is, I believe, the true one. The former view is not to be discarded without some consideration ; for one observer states that, in the course of thirty years he had attended to this subject, he had never met with any honeydew which did not seem to him to be clearly referable to aphides as its origin.¶ This view does not go counter to what I conceive to be the correct one ; for exudations do take place, and the quantity of "dew" can be increased by the aphides.

There is a very curious account given in a now little known work of what was considered the real cause of "honeydew," but I will not trespass further upon the valuable space of this journal in quoting it. I give the title at foot.*

More can be said upon this interesting subject ; and on another occasion I hope to resume the investigation, by attempting to explain the "yellow rains" of a different kind to those treated of in this letter.

JOHN JEREMIAH

43, Red Lion Street

Black Rain

THE following notice of a shower of black rain, which has been sent to me by my friend Mr. G. J. A. Walker, of Norton Villa, near Worcester, though not so exact in its description as I could have wished, may call attention to the subject, and elicit a more detailed account, if in this ungenial season rain of a similar nature has fallen elsewhere. Mr. Walker's residence is about three miles south-east of Worcester, and he says, that after three or four hours of common rain on Tuesday June 6, it became suddenly dark about seven o'clock, P.M., and shortly after a rain like ink poured down for a quarter of an hour, after which light returned upon the scene. The following morning the sheep at Woodhall (an adjacent farm) appeared as if their fleeces had been dyed black ; also the dog and a grey pony that Mr. Walker had out in a field close by appeared as it they had been rolling in soot or in a coal hole. The black matter brought down with the rain was of an adhesive nature, and at Littleworth, within a mile of Norton, where this rain fell into some tubs, it was observed to be as black as ink. This black rain was particularly remarked, as clear ordinary rain had been falling for some hours on the day mentioned, but had ceased an hour previously to the commencement of this black downfall. The actual rain of that evening did not extend to Worcester, but I have a note taken at my residence here at the time, that "the gloom was singular and overpowering all the evening." I regret that, going into Herefordshire the next day, I was not aware of this occurrence till some days after, and none of the black rain or the adhesive matter it brought down had been preserved for microscopical examination.

EDWIN LEES

A New View of Darwinism

I HAVE noticed that NATURE is very catholic in its sympathies, and allows all views which are not palpably absurd to be discussed in its pages, and I therefore venture to ask for some space in which to present a few of the difficulties which have been suggested by Mr. Darwin's theory of Natural Selection, and which have not, so far as I know, been as yet discussed. I have not the taste for the language nor the arguments which were used by a *Times* reviewer, and I have much too great a reverence for one of the most fearless, original, and accurate investigators of modern times, to speak of Mr. Darwin and his theory in the terms used by that very ignorant person. Approaching the subject in this spirit, and knowing how very small a section of biologists are now opposed to Mr. Darwin, I may be very rash, but hardly impertinent, in stating my difficulties.

I cannot dispute the validity and completeness of many of Mr. Darwin's proofs to account for individual cases of variation and isolated changes of form. Within the limits of these proofs it is impossible to deny his position. But when he leaves these individual and often highly artificial cases, and deduces a general law from them, it is quite competent for me to quote examples of a much wider and more general occurrence that tell the other way. In this communication I shall confine myself to Mr. Darwin's theory, and shall not trepass upon the doctrine of evolution, with which it is not to be confounded.

The theory of Natural Selection has been expressively epitomised as "the Persistence of the Stronger," "the Survival of the Stronger." Sexual selection, which Mr. Darwin adduces in his last work as the cause of many ornamental and other appendages whose use in the struggle for existence is not very obvious, is only a by-path of the main conclusion. Unless by the theory of the struggle for existence is meant the purely identical expression that those forms of life survive which are best adapted to survive, I take it that it means in five words the Persistence of the Stronger.

Among the questions which stand at the very threshold of the

* Bk. II. c. lvi.

† Lectures on Natural Philosophy, 1st series, by Montagu Lyon Phillips. London, 1839, pp 47-48.

‡ Vegetable Physiology, by Dr. W. B. Carpenter, 1858, p. 580.

§ This I take to be equivalent to "honeydew."

‖ Kirby and Spence's "Entomology," 1867, p. 119.

¶ Kirby and Spence, foot-note, p. 119.

* Rohault's "System of Nat. Phil.," by John Clarke, D.D., vol. ii. p. 217. London, 1735.

whole inquiry, and which I have overlooked in Mr. Darwin's books if it is to be found there, is a discussion of the causes which produce sterility and those which favour fertility in races. He no doubt discusses with ingenuity the problem of the sterility of mules and of crosses between different races, but I have nowhere met with the deeper and more important discussion of the general causes that induce or check the increase of races. The facts upon which I rely are very common-place, and are furnished by the smallest plot of garden or the narrowest experience in breeding domestic animals. The gardener who wants his plants to blossom and fruit takes care that they shall avoid a vigorous growth. He knows that this will inevitably make them sterile; that either his trees will only bear distorted flowers, that they will have no seed, or bear no blossoms at all. In order to induce flowers and fruit, the gardener checks the growth and vigour of the plant by pruning its roots or its branches, depriving it of food, &c., and if he have a stubborn pear or peach tree which has long refused to bear fruit, he adopts the hazardous, but often most successful, plan of ringing its bark. The large fleshy melons or oranges have few seeds in them. The shrivelled starvelings that grow on decaying branches are full of seed. And the rule is universally recognised among gardeners as applying to all kinds of cultivated plants, that to make them fruitful it is necessary to check their growth and to weaken them. The law is no less general among plants in a state of nature, where the individuals growing in rich soil, and which are well-conditioned and growing vigorously, have no flowers, while the starved and dying on the sandy sterile soil are scattering seed everywhere.

On turning to the animal kingdom, we find the law no less true. "Fat hens won't lay," is an old fragment of philosophy. The breeder of sheep and pigs and cattle knows very well that if his ewes and sows and cows are not kept lean they will not breed; and as a startling example I am told that to induce Alderney cows, which are bad breeders, to be fertile they are actually bled, and so reduced in condition. Mr. Doubleday, who wrote an admirable work in answer to Malthus, to which I am very much indebted, has adduced overwhelming evidence to show that what is commonly known to be true of plants and animals is especially true of man. He has shown how individuals are affected by generous diet and good living, and also how classes are so affected. For the first time, so far as I know, he showed why population is thin and the increase small in countries where flesh and strong food is the ordinary diet, and large and increasing rapidly where fish or vegetable or other weak food is in use; that everywhere the rich, luxurious, and well-fed classes are rather diminishing in numbers or stationary; while the poor, under-fed, and hard-worked are very fertile. The facts are excceedingly numerous in support of this view, and shall be quoted in your pages if the result is disputed. This was the cause of the decay of the luxurious power of Rome, and of the cities of Mesopotamia. These powers succumbed not to the exceptional vigour of the barbarians, but to the fact that their populations had diminished, and were rapidly being extinguished from internal causes, of which the chief was the growing sterility of their inhabitants.

The same cause operated to extinguish the Tasmanians and other savage tribes which have decayed and died out, when brought into contact with the luxuries of civilisation, notwithstanding every effort having been made to preserve them. In a few cases only have the weak tribes been supplanted by the strong, or weaker individuals by stronger; the decay has been internal, and of remoter origin. It has been luxury and not want; too much vigour and not too little, that has eviscerated and destroyed the race. If this law then be universal both in the vegetable and animal kingdoms, a law too, which does not operate on individuals and in isolated cases only, but universally, it is surely incumbent upon the supporters of the doctrine of Natural Selection, as propounded by Mr. Darwin, to meet and to explain it, for it seems to me to cut very deeply into the foundations of their system. If it be true that, far from the strong surviving the weak, the tendency among the strong, the well fed, and highly favoured, is to decay, become sterile, and die out, while the weak, the under-fed, and the sickly are increasing at a proportionate rate, and that the fight is going on everywhere among the individuals of every race, it seems to me that the theory of Natural Selection, that is, of the persistence of the stronger, is false, as a general law, and true only of very limited and exceptional cases. This paper deals with one difficulty only, others may follow if this is acceptable.

Derby House, Eccles HENRY H. HOWORTH

Ocean Currents

MR. PROCTOR concludes his letter on Ocean Currents, NATURE for June 15, with the remark that in theories respect oceanic circulation "the vast distance separating the Polar fr the Equatorial regions must not be overlooked." Will allow me to point out to him that in the experiment he sugge that vast distance is entirely overlooked; that, in fact, any s experiment, with whatever difference of detail it may be formed, whether in his cylinder or in Dr. Carpenter's trough, no way illustrates the natural condition of things, and in no tends to answer that objection to the "temperature" theor currents which is founded on the infinitesimal nature of thermometric gradients. The difference of temperature betw Arctic and Equatorial water is about 50° F., or 1° F. in miles; or, reducing it to smaller units suitable for an exp ment, is $\frac{1}{000000}$ of a degree in one foot; this, if the exp mental trough is five feet long, or if the cylinder is ten feet diameter, gives an extreme difference of $\frac{1}{1000000}$ of a degree Fahrenheit's scale. Can such a difference be represented in experiment? I think not; but no experiment which sh a much greater relative difference can be accepted as satisfacto for it is the infinitesimal nature of the thermometric grad existing in the ocean that constitutes the physical objection to temperature theory. There are other objections which I not allude to now; but it is manifestly no answer to this objection to show that under certain other circumstances—w bear no resemblance in degree to those of nature—hot water cold will establish a circulation. I, for one, have, for a g many years, been perfectly well aware that they will; but I do if it has ever been shown that a sensible motion will result fr a thermometric gradient of $\frac{1}{000000}$ of a degree in a foot.

J. K. LAUGHTO

Alpine Floras

THE fact mentioned in last week's NATURE of the absenc any Alpine flora on the Atlas Mountains, Morocco, though appointing, is interesting. It seems to show that, during glacial period, icebergs did not drift to the Atlas. This, ho ever, must have been from local causes only. Mr. Wallace fo a European flora on a mountain in the Eastern Archipelago think in Borneo—which, most probably, must have got th during the glacial period. JOSEPH JOHN MURPH

Old Forge, Dunmurry, Co. Antrim, June 19

A Suggestion

Is it possible that the following facts may account for the sence of *Elastrus dolosus* in the Azores? At all events, I c them as suggestive, and for the information of Messrs. Wall Godman, Murray, Crotch, &c.

Lawrence Almeida, son of the first Portuguese Viceroy India, was the first European known to visit the coast of Ma gascar in the year 1506. The Portuguese circumnavigated whole island within two years, and subsequently consta anchored at it in their voyages to the East Indies. They established a settlement on a steep rock on the bank of the r Franchere and near the village of Hatore, in the provinc Anosi (*i.e.*, at the south-eastern extremity of the island). valuable timber, as ebony, as well as the rich dye-woods, w be well worth taking to Europe, and thus doubtless afford conveyance for living larval or pupal Elaters, without any ra improbable concurrence of events, to the Portuguese island the Atlantic. Many of the extremely beautiful and attrac flowering shrubs and plants would also not improbably be warded to Europe by the same route, in which some Elate might find shelter. Is the lapse of 300 years sufficient to acc for change of development? S. P. OLIVE

Southsea

HYDROUS SILICATES INFECTING THE PORES OF FOSSILS

DR. T. STERRY HUNT directed attention some t ago* to a remarkable limestone of Silurian age fr Pole Hill, New Brunswick, in which I had found

* Proceedings of the Natural History Society of Montreal.

tained in the volume as to the various relationships of the natural orders described in it, the morphology of their genera, the distribution of the different types, and the economic products obtained from the species, is immense. It possesses, however, the defect so common in foreign scientific works, of the absence of any table of contents or index to the subjects treated of. Had the publishers of the English edition supplemented the index of genera and subgenera with one referring to the various topics discussed, they would have rendered the English edition a practically more useful contribution to botanical literature

CALYCANTHUS FLORIDUS: Floriferous shoot.

than the French original. The illustrations are profuse, and of that excellence which we look for in vain in works originally published in this country. We append one of the well-known "Allspice Tree," the *Calycanthus floridus*. The small order Calycanthaceæ, including only the American *Calycanthus* and the Japanese *Chimonanthus*, is one the true position of which has been much disputed by systematists. Baillon makes it a "series" of Monimiaceæ, with which he also unites the Australian Atherospermeæ, bringing this order forward from its usual position among the Incompletæ to close alliance with Magnoliaceæ and Anonaceæ. A. W. B.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

A New View of Darwinism

I HAVE only just seen the two letters in answer to one from me on Darwinism which you were good enough to insert in NATURE, and to which I ask the favour of being allowed to reply. I have to thank Mr. Darwin for his references and for the tone of his letter, which is in such marked contrast to the angry dogmatism of Mr. Wallace.

Mr. Wallace commences by ridiculing the phrase the Persistence of the Stronger. The phrase was not mine, it has been used by a better man than I, namely, by Prof. Jowett, and it has the advantage of not involving an identical expression, which the Survival of the Fittest does. "That those forms of life survive which are best adapted or best fitted to survive," is not a very profound discovery; it might have suggested itself even to a child, and if Mr. Wallace means nothing more than this when he speaks of the theory of Natural Selection, he cannot claim to have added much to the world's philosophical opinions.

He then complains that I have only touched one of the many facts relied upon by Darwinians; I refer him to my letter, in which I distinctly say that it contained only one of my objections, and that I have many more which will follow if the Editor have patience with the discussion. The reply to Mr. Wallace will confine me, however, in this letter to the ground covered by the former one. Having disposed of the formal and personal matters, I now approach the matters of fact about which we are at issue.

Here, I am sorry to say, I am met in a very different spirit by Mr. Wallace to that in which Mr. Darwin meets objections. Dogmatism, bold and unwavering, was the privilege of the philosophy of the Schools, but in the 19th century it is puerile. Mr. Wallace states boldly, without any authorities, merely as an imperial *ipse dixit*, that the most vigorous plants and animals are the most fertile. I had, at least, the decency to quote the book of Mr. Doubleday, containing a magazine of facts and examples in support of my view, and which tells exactly the other way.

This view has not been correctly stated by Mr. Wallace. The position I maintain is this, that, as a general law, those individuals which are underfed and lead precarious lives, are more fertile than those whose advantages make them vigorous and healthy. The ringing of the bark and the pruning of the roots of barren fruit trees and the starving of domestic animals to make them fruitful were examples to this end.

Mr. Wallace quotes only one example in his own support, and I will accept it as a crucial test of my position, which he will acknowledge to be fair; the case of the Red Indian and the Backwoodsman. The Red Indian lives entirely on flesh, the Backwoodsman almost entirely on vegetable food. Like meat livers in every part of the world, in Mexico, on the River Plate, in Siberia, in Turkestan, and in some parts of Russia, the Red Indian is not a fertile creature. The Backwoodsman, like vegetable feeders everywhere who are not luxurious, in India, China, Poland, and the Russian provinces bordering on it, Ireland, &c., is comparatively fertile, but only comparatively. It is a mistake to suppose that the Backwoodsman is specially fertile, and in a few years he becomes, as the inhabitants of Kentucky and Tennessee have been long known to be, diminishing in numbers, the population of the States being kept up by immigration.

Mr. Chadwick, in his "Sanitary State of the Labouring Classes," observes that where mortality is the greatest there is much the greatest fecundity; thus, in Manchester, where the deaths are one to twenty-eight, the births are one to twenty-six, while in Rutlandshire, where deaths are but one to fifty-two, births are one to thirty-three, showing that a state of debility of the population induces fertility. This only supports the common dicta of doctors that consumptive patients are generally very fertile. The pastoral tribes of Eastern Russia which have recently taken to agriculture, such as the Tchuvashes, &c., have begun to increase most rapidly. The Hottentots at the Cape, who were formerly a numerous race living very hard lives, are almost extinct now that they are carefully tended and well fed. The Yeniseians, the Yukahiri, and other Siberian tribes, have disappeared like smoke before the advance of Russian culture; they have suffered little if at all from the Russian arms.

Let me quote a curious example in answer to Mr. Wallace from the very race to which he has referred. Captain Musters, in describing his recent journey through Patagonia at the Anthropological Institute, told us that it was the custom for the Patagonian women to be bled at certain times referred to, as they believed *it made them fertile*. Among the Patagonians, therefore, we meet with empirical witnesses, unsophisticated by our philosophy, to the truth of the position I maintain. But those who live in large cities need not travel to Patagonia. The classes among us who teem with children are not the well-to-do and the comfortable, but the poor and half-fed Irish that crowd the lowest parts of our towns. I am not contrasting now the fat with the lean, but the comfortable classes with those who lead precarious lives—the vigorous in health with the sickly, the half-fed, and the weak. It will be asked, why rely so much upon man? The answer is that I quite agree with Mr. Darwin that man is subject to the same natural laws as the animals, and further I believe that since we have studied man more closely and under a greater variety of conditions, facts derived from our experience of man are of greater value than those deduced from our examination of the other animals.

But let us turn to these latter for a space; and here I tread with much greater diffidence, for I am aware of the vast ex-

perience and fund of illustration possessed by Mr. Darwin, and I have to say that I am unconvinced by the arguments he has adduced. With the transparent frankness of all his writings, Mr. Darwin, in one of the references to which he has commended me, has collected a very large number of examples that tell very strongly against him, and which I again commend to Mr. Wallace. I refer to the 18th chapter of Mr. Darwin's book on the "Variation of Plants and Animals under Domestication," and especially to that portion beginning on page 149. In speaking of animals, he says:—"The most remarkable cases, however, are afforded by animals kept in their native country, which, though perfectly tamed, quite healthy, and allowed some freedom, are absolutely incapable of breeding. Rengger, who in Paraguay particularly attended to this subject, specifies six quadrupeds in this condition, and he mentions two or three others which most rarely breed. Mr. Bates, in his admirable work on the Amazons, strongly insists on similar cases, and he remarks that the fact of thoroughly tamed wild animals and birds not breeding when kept by the Indians, cannot be wholly accounted for by their negligence or indifference, for the turkey is valued by them, and the fowl has been adopted by the remotest tribes. In almost every part of the world, for instance, in the interior of Africa, and in several of the Polynesian islands, the natives are extremely fond of taming the indigenous quadrupeds and birds, but they rarely or never succeed in getting them to breed," and so on, through sixty pages of closely-packed examples. And what is Mr. Darwin's commentary on these facts? I again quote page 158:—"We feel at first naturally inclined to attribute the result to loss of health, or at least to loss of vigour, but this view can hardly be admitted when we reflect how healthy, long-lived, and vigorous many animals are under captivity, such as parrots and hawks when used for hawking, chetahs when used for hunting, and elephants. The reproductive organs themselves are not diseased, and the diseases from which animals in menageries usually perish are not those which in any way affect their fertility. No domestic animal is more subject to disease than the sheep, yet it is remarkably fertile." Mr. Darwin, with equal clearness and conclusiveness, decides that this sterility cannot be due to a failure of sexual instincts, change of climate or of food, or want of food or exercise; and he concludes that certain changes of habits and of life affect in an *inexplicable manner* the powers of reproduction. But what is true of man it is reasonable to suppose is true of all these instances—namely, that it is a more luxurious habit, a more vigorous health, a less precarious existence, induced by the care and attention of domesticators, that have caused the sterility; that these animals are too well off, and not that they are ill off in any way; and this theory explains the whole most conclusively. On the other hand, and in opposition to this vast and uniform collection of examples, Mr. Darwin adduces a few instances which tell the other way, but they are very few in number, and seem to me explicable on other grounds. Ferrets, it is notorious, are always kept in a state of extreme depletion and as thin as possible. Domestic poultry are fed almost entirely on poor vegetable food, while their wild and semi-wild relatives feed much more on worms, insects, and on animal diet generally. In regard to sheep, it is notorious that very weak ewes generally bear twins, that Somersets and Dorsets are more fertile than Southdowns and Leicesters. We have, I may add, no facts to guide us in regard to wild dogs, and few in regard to wild cats; but we do know that in tame ones the half-fed lantern-ribbed curs are more prolific than their sleek relations. In regard to domestic fowls, and especially pigeons, we must remember that their condition is materially altered by the disuse or only very partial and irregular use of their powers of flight, this must reduce their circulation and vigour very considerably, and make them *pro tanto* so much weaker. But these instances, upon which Mr. Darwin relies to answer Doubleday and others, are very partial indeed. In his own pages, as I have already said, they form a very small element compared with the overwhelming cases he quotes on the other side. So much so, indeed, that these cases may be taken as exceptions which prove the rule that domestication and improved conditions of life induce sterility in animals.

It savours of scholastic philosophy to speak of Nature as exercising any influence on the regeneration of races, and yet there may be sound philosophy in the old notion that when an individual or a class is in danger of being extinguished from want, Nature puts forward a special effort to preserve it. The sickly mother, the half-starved plant, is more likely to breed than the healthy and the vigorous. If we remove the peasant's family to the drawing room, it will cease to be composed of ten and twelve children. If we remove our daisies and cowslips to the greenhouse, their flowers grow double, and they ripen no seeds. The vine that has felt the frost is the one to pay the rent. Wherever we turn, in fact, we meet with examples of the universal law; and this law seems to be at issue with an important portion of Mr. Darwin's theory, namely, that in the struggle for existence, the vigorous, the hearty, and the well-to-do, elbow the weak and decrepid until they elbow them out of existence, and supplant them. If I have said anything above which can be construed into an impertinence, I unconditionally withdraw it. The only excuse for soreness, is an impatience at what seems to the writer to be indefensible dogmatism. The days will not be ripe for scientific dogmatism until the Infallibility of Positive Philosophers has been generally accepted, and it does not do to forestal that millennium.

H. HOWORTH

MR. WALLACE has effectually set aside Mr. Howorth's new views on Darwinism, and it now only remains to point out that the latter gentleman, in his instances, puts the cart before the horse. Hens that are fat and don't lay are fat because they don't lay. When the sexual powers, either in plants or animals, are defective from accident or design, the overgrowth always takes place, and this among animals is chiefly by the increase of adipose tissue.

Birmingham LAWSON TAIT

Recent Neologisms

I HAVE been long accustomed to register the first appearance of new words and phrases. Of course the vast majority of these take no root, perishing where they fall. Here is a sample of the latest issue: *Survival*, introduced, I think, by Darwin; *indiscipline* and *impolicy*, which were brought in by the Franco-Prussian War, and also the vulgarism *to telegram*. The greatest atrocities in this line are committed by "physicists," if the shade of Faraday will pardon me the use of that word; and far away the worst coinage I ever encountered is due to Mr. Alfred R. Wallace. As it is "meet and right and our bounden duty" to stigmatise such intruders, and if possible prevent their adoption, I take the liberty of making my feeble protest against Mr. Wallace's "prolificness," which he introduces to our notice in his letter on Mr. Howorth (NATURE, July 6, 1871, p. 181). In this case the hideousness of the coinage is some guarantee against its reception.

Malvern Wells, July 8 C. M. INGLEBY

Affinities of the Sponges

I HAVE just read with much interest the paper in NATURE by Mr. W. Saville Kent, criticising my friend Carter's article in the "Annals of Natural History" for this month, in which I fully concur. How Mr. Carter can have fallen into such an error, for such I must call it, I cannot imagine, as comparing a group of animals in Botryllus to those sponge cells, even in so highly a developed form as Grantia. For, taking this as the highest known form of sponge animal, it is at most only a monociliated sac, as shown both by Prof. Clark and by Mr. Carter. Now, it is well known to all investigators, and Mr. Carter has shown it himself, that the animals of Botryllus have distinct oral and fæcal apertures, whereas the sponge cell, so far as has yet been seen, has only an oral aperture. Again, the Ascidian Botryllus is shown to be far higher in the scale when we come to compare its internal organisation, and not merely to confine ourselves to the sac-like tunic. The discharge of the fæcal matter into a common cloacal canal is to me not a sufficient reason for comparing these groups of animals to the sponge animals in *Grantia*.

But what I wish to draw attention to more particularly is this, that in the hurry and bustle of our investigators of the present day, all old associations are mostly, if not entirely, forgotten. I can scarcely think that they are ignored, but are forgotten. Thus, Prof. Grant was, I believe, the first to determine the character and the full importance of the seed-like body in *Halichondria* by placing watch-glasses in the vessel in which living specimens of the above sponge was placed; the bodies were thus discharged from the fæcal canal of the parent sponge, and attached themselves to the watch-glasses, and he then carefully watched their development. Mr. Carter, being a pupil of Dr. Grant, no doubt followed his teacher's plan of investigation, which has led to the brilliant results of this gentleman's in-

LETTERS TO THE EDITOR

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Mr. Howorth on Darwinism

WILL you allow me to reply to the various letters which appeared in your last number in answer to one from me? I gratefully welcome their general courteousness. Postponing the consideration of Mr. Wallace's letter, I come to Dr. Lionel Beale, the relevancy of whose arguments, and especially of the lugubrious moral attached to them, I fail to understand. It seems to me to be so incoherent and rhetorical that it is far beyond the reach of reply.

Mr. Tylor refers to the last census as disproving my position. He says the population has increased enormously, and yet our age is characterised by its luxury. These statements are correct. But the argument deduced from them has a missing link. The luxury of the upper strata of society has increased with its wealth, but the numbers of the pauper class have been increased in the same rate. In considering the published returns of the Poor Law Board, I am compelled to admit that the increased luxury has been limited to the surface of society, and that its lowest ranks have been correspondingly recruited, and to admit the force of Mr. Doubleday's argument, that the population of England under the Tudors was stationary because of the generally diffused wealth, while that of Ireland in the last century was increasing at an enormous rate, because it was steeped in poverty and want. I am not arguing about individual cases, but about general laws. Now, in Lancashire, where the increase has been so marked, I have it on the authority of owners of mills that the indigenous stock of the county, which is thrifty and well off, is not an increasing element, but is being replaced by the children of the Irish, or semi-Irish blood, from the poorer quarters of the large towns, among whom prudential restraint (which is surely a very visionary *causa causans* in any event) cannot be said to have much influence. At Rome, Venice, Basle, and in France, where the aristocratic class was not limited by primogeniture, it was always dying out, and was only recruited by fresh creations (see the details in Doubleday, chapter iv. *passim*). In all these cases we can appeal to figures, and not to a superficial survey of a Peerage, or the limited area of our own acquaintance.

The particular passage quoted by Mr. Tyler from Malthus has been conclusively answered by Doubleday (chapter vi.), and it is useless to repeat his arguments, which on this point I consider to be unanswerable.

Mr. Lownes repeats the odd charge of Mr. Tait against me, that I put the cart before the horse. The latter gentleman, whom

I have not yet answered, cited against me the elementary case of capons and other creatures of that ilk. They are entirely beside the question. It is as reasonable to quote them in this discussion as to conclude that all chaste people must be cowardly and effeminate because mutilated animals are so. He also said that I mistook the whole rationale of the question, and that it is infertile creatures that grow fat, and not fatness that causes sterility. The only test of the question is the one I have not shrunk from applying in this argument (which, by the way, has not to do so much with the fat as the hearty and strong). This test is that in a great number of cases we can make strong and vigorous but sterile plants and animals fertile by starving or bleeding them, which proves that it is not the organs that are defective, but that the creatures are too hearty.

The experience of Mr. Lownes on the fecundity of consumptive patients, and of the poorest classes as compared with the richest, is at issue with that of the doctors and midwives whom I have access to, and of all the authorities I know whose opinions are based upon statistics.

I am not sure that I understand the second and third paragraphs of his letter. Whichever way the problem is put, I am satisfied if it be admitted that in the more crowded and squalid portions of our towns, the population as a rule is more fertile than in the less crowded neighbourhoods. The case he cites of poor women losing their children early and ceasing to give milk, and, in consequence, soon becoming pregnant again, is counterbalanced by the fact that among the richest the proportion of those who suckle their children is small, and this not because of fastidiousness, but because they secrete little milk. Mr. Lownes once more drags out the Indian and the backwoodsman, but he has overlooked the answer I gave to Mr. Wallace in my former letter, which needs no alteration to meet the case as he has put it. It is the case of the meat-eaters against the vegetable-feeders, the strong and hearty and active against the comparatively stolid and low-conditioned, and as in such cases all the world over the former are not so fertile as the latter. Mr. Lownes objects to savages being cited, because of qualifying circumstances; he may as well say that it is not fair to test natural selection by wild animals, but only by domesticated ones. His treatment of the case of the Patagonian women is convenient but flippant. Mr. Lownes' experience in breeding both cattle and sheep and fowls and in rearing plants must be extremely limited, or he would hardly have made so rash an assertion as that contained in his last sentence. The starving of plants and animals to induce them to breed is one of the elementary axioms of both gardeners and stockkeepers.

I now come to Dr. Ross's letter, which, although somewhat patronising in parts, is altogether more to my taste than some others. He has properly referred me to Mr. Herbert Spencer, but I am afraid of venturing into his book, for fear that I should open upon myself the floodgates of Evolution. It is not the general problem of Evolution about which we are now at issue, but that limited form of it called Natural Selection. It is satisfactory, however, to find that, according to Dr. Ross, Mr. Herbert Spencer admits the main facts upon which my argument is founded. His doing so is quite a relief after the jaunty manner in which some of your correspondents have spoken about the matter. To speak of its being late in the day to be now defending Mr. Doubeday, to tell one that "what one says is ludicrous," "a monstrous error," &c., &c., is surely a sign that the crowing of the Gallic cock has been mistaken for more substantial arguments. I am very sorry that Mr. Spencer's book is not in my library, and that I cannot meet with it at the Manchester Free Library or Mudie's, so that until I am aware of Mr. Spencer's arguments I cannot say how far they affect the position I maintain. If the facts are admitted, as Dr. Ross says they are, I confess that I cannot see any other interpretation of them than the one given by Mr. Doubleday. Will Mr. Ross do me the favour of pointing out what other explanation they are capable of?

Mr. Wallace has misunderstood me if he thinks me capable of sneering at the good and sound work that has been done by himself for many years, the value of which I am as conscious of as I am of the worthlessness of mere Olympian dogmatism. Sneers are only justifiable in answer to contempt, and if he feels aggrieved with any of my words I withdraw them.

Mr. Wallace says my criticism of the phrase Survival of the Fittest is satisfactory. In regard to the phrase I used, and for which I was severely flouted by Mr. Wallace, he says it is unknown to Darwinians; that may be, but it can hardly be said to be unknown to Mr. Darwin himself. Speaking of the problem of the conversion of varieties into species, the latter says: "The inevitable result is an ever recurrent struggle for existence. It has been truly said that all nature is at war, the strongest ultimately prevail, the weakest fail, and we well know that myriads of forms have disappeared from the face of the earth" ("Variation of Animals and Plants under Domestication," i. 5). Let me especially commend this extract to Dr. Lionel Beale, for whom I entertain the profoundest respect, notwithstanding his vituperation of myself.

I find a difficulty in meeting Mr. Wallace's latest arguments, because they are entirely *a priori*, and Mr. Wallace asks me to admit as premisses the very thing I dispute, namely, the relative sterility of strong and hearty animals and plants. I cannot see the relevancy of his quotation of the effects of cross-breeding to the present argument, unless he means to infer that crosses are more vigorous and stronger than pure bred animals, on which position I should like to be furnished with a little evidence. Again, I cannot test the supposititious problem put by Mr. Wallace as to the strongest individual of an animal's progeny eventually being the stem-father of the race. He takes for granted that it is, and in doing so begs the question. I can only say the only experiments I know do not favour Mr. Wallace's *a priori* view, and that in the cases we can experiment upon, not the least satisfactory of which is the case of man himself, the condition most favourable to fertility, as I have quoted many examples to show, is that of comparative depletion.

Mr. Wallace, as before, is spare of instances. I can only extract two *bonâ fide* ones from his letter. He tells us the strongest bull leads the herd; this proves nothing, unless we are to infer from it that his progeny is the most numerous, and that the biggest and strongest therefore survive. I prefer to quote Mr. Darwin himself where I can. If Mr. Wallace's instance be worth anything, how does he account for the following: "The decrease in size of the Chillingham and Hamilton cattle must have been prodigious, for Prof. Rutimeyer has shown that they are almost certainly the descendants of the gigantic *Bos primigenius*. No doubt this decrease in size may be largely attributed to less favourable circumstances. Yet animals roaming over large parks and fed during severe winters can hardly be considered as placed under very unfavourable conditions" ("Variation of Animals and Plants under Domestication" ii. 119). What Mr. Darwin says of the wild cattle is equally true of the reindeer kept by the Laplanders compared with the wild ones on the Samoyede tundras, of the red deer of our larger forests compared with the skeletons of red deer from the turbaries, and is, perhaps, generally true of semi-wild races where man has not intervened with the special object of increasing the size by breeding from the largest individuals only.

In regard to the carnivora, I know of no reliable facts. I am not proposing the monstrous paradox that those animals which are so weak, diseased, or decrepid that they cannot sustain life at all, are the only ones that keep up the succession of the animal world. The toothless tigress, who cannot kill her food and is starving, will most certainly not be the mother of a long race. She can do nothing but die. But I say that, judging from analogy, it is probable that the lean and comparatively ill-fed tigress will breed more freely than the man-eater supplied with regular and abundant food.

The banks of the Chinese rivers and the rough country in the south and south-west of Ireland are both inhabited by teeming populations, remarkable for their poverty and fertility, and remarkable further for sending out immense colonies, which supplant wherever they go, in Mantchuria, in Songaria, in Glasgow, in Manchester, in New York, the strong hearty, indigenous races. This being so (and I only quote these two as examples of a whole class), when Mr. Wallace asks the question, "How can weak and sickly parents provide for and bring up to maturity their offspring, and how are the offspring themselves (undoubtedly less vigorous than the offspring of strong and healthy parents) to maintain themselves?" I can only reply that they actually do so: *Veni, vidi, et credi.*

I must correct a wrong impression that Mr. Wallace has got hold of. In this controversy I have no theory; my only theory is that Natural Selection is an ingenious but fallacious explanation of the varieties of life.

I cannot understand Mr. Wallace's last sentence if it be meant for an argument; while if it is only a *jeu d'esprit* and witticism, it requires a commentary to tell us where the point is.

Lastly, I will consider Mr. Wallace's reiterated complaint that I have only treated of what is in most cases the least important factor in determining the continuance of species. Let me turn

very briefly to another of these factors put prominently forward by both Mr. Wallace and Dr. Beale, namely, "Obscure Colour."

We are not arguing about exceptional and individual cases, we are dealing with a general law, applicable or supposed to be applicable to the great majority of cases. Can it be said gravely that obscure colour has tended to the preservation of particular forms of life to the exclusion of others, not in a few exceptions, but as a general biological law?

Daylight, it will be admitted, is more likely to disclose an object than darkness. If we compare diurnal forms of life with nocturnal ones, we ought to find, if I read the tendency of the Darwinian argument rightly, that in the daylight when a sombre, obscure, or indifferent colour, would be of great service to hide an object, that there are a much smaller proportion of conspicuous forms of life abroad than at night when there would be no such need for obscurity, and a bright colour might be worn with impunity. Is such the fact?

Again, if we compare the animals and plants that live in tropical climates, where the light is intense, with those found in temperate and severe ones where the light is not so great and objects are not so prominent, do we find that the former has a comparative monopoly of conspicuous objects, or do we find rather that the reverse is the case, and that all the brightest objects we know in nature—the parrots, macaws, humming birds, butterflies, orchids, &c.—are found in the greatest profusion in the tropics, while we proverbially console ourselves for the absence of colour in our birds by boasting of their singing, and hang the beetles of Brazil in necklaces round our sisters' and wives' necks, while we crush our sombre representatives of the same class under our heels? Is it not equally true of the sea? In the Mediterranean, for instance, do not the brightly decked out gurnards and mullets far outnumber the dingier fish, while on the banks of foggy Newfoundland the sober tinted cod and ling are the prevailing types? In the former we have the clear blue water that washes round Sorrento pierced through and through by the blazing sun, while in the latter we have everything gloomy except the fisherman.

If we separate the animal world into flesh eaters and vegetable eaters, we ought to find, if this theory be true, that the former (which as a rule are not themselves the prey of other animals) are more conspicuous than the latter, since they have less reason for adopting a secret costume. But is it so? Are the hawks and owls and carnivorous beetles as classes more conspicuous than their victims? Is it a not fact that the most beautifully coloured creatures are as a rule the most helpless, weak, and accessible; that those animals which are supplied by nature with weapons of defence or are strong and can defend themselves, are as classes more obscure in colouring than those not so protected, and that the same rule applies to plants which are poisonous, nauseous, or protected by thorns? If these facts be true in the great majority of cases, we have another factor in Mr. Darwin's theory which is not satisfactory, and the cases quoted to support it become mere exceptions, which, by being exceptions, disprove the particular law he is maintaining. This letter has already exceeded reasonable limits, and I must postpone a further consideration of this and other objections to another occasion.

Derby House, Eccles HENRY H. HOWORTH

MR. HOWORTH'S objections to the theory of Natural Selection have been fully answered. I therefore wish to direct attention to another objection which has been recently advanced, and which has not, so far as I know, been specially refuted. The objection is stated by its author in the following terms:—"And it has been affirmed that to 'the primitive properties of molecules' and 'Natural Selection' may be referred all the varying forms and structures known to us, as well as all the phenomena of the living world. But such terms explain nothing. By their use further inquiry is discouraged, and the mind bent upon investigating the secrets of Nature is misled at the very outset. Can any one of these very pretentious phrases be resolved into anything more than the statement of a fact or facts in the form and language of an explanation? Natural Selection is the formation of species, and species are produced by Natural Selection. Crystallisation is the formation of crystals, and crystals are produced by the operation of crystallisation."

This passage is extracted from p. 58 of "The Mystery of Life"—a little work by Dr. Beale, which was published a few months ago. Dr. Beale has a keen appreciation of the "ludicrous." He thinks Mr. Howorth's misrepresentation of the Darwinian theory "very curious and even ludicrous," and in the closing sentence of his letter in NATURE, he appears to have a bit of fun to himself which ordinary mortals cannot understand; and if he can prove that Natural Selection is a mere abstract statement of the fact that species are in some way or other formed, the Darwinian theory is the most "ludicrous" ever presented to mankind. Probably Mr. Wallace may take a different view of the subject, and he may even think that the objection is more ludicrous than the theory; at any rate, no harm can result from bringing Dr. Beale and the champions of Natural Selection face to face, so that stricter tests than the "ludicrous" may be applied to ascertain whether the truth lies in the theory or in the objection. JAMES ROSS

Newchurch, July 24

THE last paragraph of Mr. Howorth's letter in NATURE of July 13 reminds me of a fact which I have often noticed, and which is, I suppose, well-known to botanists, viz. that certain creeping plants which root at the joints, flower sparingly unless the sprays are so disposed that they cannot take root. I refer especially to the *Lysimachia nummularia* (larger moneywort or "Creeping Jenny"). This plant blossoms comparatively little when allowed to trail in the moist soil which is its natural *habitat*, and in which alone the leaves look healthy and thriving. A spray trained off the flower bed on to a flag-stone, or a plant grown in a pot so as to hang over the edge and not be able to take root, will look sickly, but will be covered with flowers. I think I have noticed the same thing in connection with the periwinkle.

Gardeners cut off the runners of strawberries and the suckers of fruit trees to increase the crop, because, as they say, runners exhaust the plant.

But is not the case, rather, that the possibility of continuing its own life by taking root at the runners makes the plant's constitution, as it were, lazy about propagating its kind?

It is, perhaps, worth noticing that the cutting off the runners or suckers does not in any way weaken the plant, or cause it to become sickly, but it does prevent the indefinite prolongation of the individual life.

THE OWNER OF A "WEED GARDEN"

Recent Neologisms

WRITING, as I did, from a little Midland village, where access to an English dictionary was impossible, I am not surprised to find that three words, which I treated as recent coinages, were only re-introductions. *Survival*, *impolicy*, and *indiscipline*, are all so naturally formed, that, whether old or new, they are "welcome to stay." My end was answered by putting a brand on Mr. Wallace's *prolificness*, by way of contrast. If he is bent on using that monster, he will help to naturalise it by spelling it with *ck* (instead of *c*) like *thickness*. But surely he is not driven between the Scylla and Charybdis of *prolifickness* and *prolificacity*, when *prolicity* is staring him in the face. For my part, I pray that the whole family will (to quote Sylvester again) "shake swift wing," and be no more seen. By-the-bye, I find the verb to *handwrite* in the *Quarterly Review*, April 1871, p. 332. That is a good, if not a new word, and well deserves re-introduction.

C. M. INGLEBY

The British Association and Local Scientific Societies

IT is to be regretted that the British Association does not exert its influence in stimulating local scientific societies towards greater efforts for the formation in their museums of collections representing the Geology and Natural History of their respective neighbourhoods, so that they might constitute local monographs. Such a system, combined with a central museum in London, representing an epitome of the collections throughout the country, would tend to the advancement of science with greater rapidity and accuracy than at present, when the provincial museums are little better than overstocked curiosity-shops, and with no recognised plan of arrangement which is greatly wanted. In general there is little space for additions of importance, from the fact that the museums already contain large miscellaneous collections, unconnected with the neighbourhood, and of little use to anybody. Many valuable private collections exist throughout the country, representing the geology, &c., of various localities, which are eventually too often dispersed and lost to