THURSDAY, MARCH 9, 1882

VIVISECTION

THE discussion on this subject which has been carried on during the last few months in the *Nineteenth Century* and *Fortnightly Review* has now, we think, proceeded sufficiently far to render it desirable that we should give our readers a short summary of its progress. But as there are altogether some twelve or fourteen articles to be dealt with, we shall only have space to supply a general analysis of the facts and arguments, without being able to give an abstract of each article separately.

As regards the general tone or manner of pleading, there can be no doubt that the advantage inclines largely to the side of the physiologists; for while—with perhaps a slight exception in the case of some of the passages in the essays by Prof. Owen and Dr. Wilks—the physiologists state their arguments in a calm and tolerant spirit, the essays on the other side—with the exception of one by Mr. Hutton—present, in a painfully marked degree, the features of bitterness and ill temper. But, disregarding this conspicuous difference in the style of writing, we shall endeavour to give an analysis of the arguments on both sides, the impartiality of which shall not be affected by the fixed opinion which this journal has always held upon the subject.

The *utility* of Vivisection is upheld by Sir James Paget, Prof. Owen, Dr. Wilks, Dr. Carpenter, Sir William Gull, Mr. Fleming, Dr. Brunton, and Dr. Yeo. This is done, not merely by stating the general truth, obviously *a priori*, that "it would be more reasonable to hope to make out the machinery of a watch by looking at it, than to hope to understand the mechanism of a living animal by mere contemplation"; but chiefly by enumerating instances in the past history of research where important advances of knowledge have been made by vivisection, and could not have been made otherwise. The cases mentioned are very numerous, so we must restrict ourselves to re-mentioning the more important.

Sir James Paget says—and on such a topic he is entitled to speak with at least an unsurpassed authority—"Before Hunter's time it is nearly certain that ninety-five out of a hundred persons who had aneurism of the principal artery of the lower limb died of it. . . . At the present time it is as certain that of a hundred persons with the same disease less than ten die," and if we contemplate cases of aneurism in all other arteries, as well as deaths from bleeding after large operations, the saving of life due to Hunter's experiments is seen to be "very large." But Sir James does not needlessly prolong his article by enumerating specific instances; he says: "others have already abundantly illustrated them; I will rather suggest some general considerations on the whole subject. Looking back over the improvements of practical medicine and surgery during my own observation of them in nearly fifty years, I see great numbers of means effectual for the saving of lives and for the detection, prevention, or quicker remedy of diseases and physical disabilities, all obtained by means of knowledge to the acquirement or safe use of which experiments on animals have contributed. There is scarcely an operation in surgery of which the mortality is now more than half as great as it was forty years ago; scarcely a serious injury of which the consequences are half as serious; several diseases are remediable which used to be nearly always fatal; potent medicines have been introduced and safely used; altogether such a quantity of life and of working power has been saved by lately-acquired knowledge as is truly past counting. And in these advantages our domestic animals have had due share of the improvement of veterinary medicine."

The next paper in the series is by Prof. Owen, and is concerned mainly with the history of the indispensable part which vivisection played in Harvey's discovery of the circulation of the blood and in Hunter's experiments on the ligature of arteries. Although the latter topic occupies common ground with a part of Sir James Paget's paper, the overlapping due to independent writing is not to be regretted, because Prof. Owen traces the history of the subject into more detail, in order to expose the fallacy of the anti-vivisectionists, who say that Hunter was anticipated in his results by other surgeons working by other methods. This, we think, he is completely successful in doing—so much so, indeed, that Mrs. Algernon Kingsford M.D., who supplies an essay in a succeeding number of the *Nineteenth Century* on "The Uselessness of Vivisection," while disputing Sir James Paget's mere statement of the fact that the surgical treatment of aneurism is due to Hunter's experiments on deer, nevertheless finds it convenient entirely to ignore the historical details which are given by Prof. Owen; and the same remarks apply to this lady's treatment—or rather evasion—of the facts concerning Harvey's discovery of the circulation of the blood.

Dr. Wilks makes utility the central portion of his argument, and gives so many instances of the service which vivisection has rendered that we cannot here quote them. The instances he refers to concern the heart, circulation, functions of the brain, spinal cord, and nervous system generally, including its influence over the heart, lungs, stomach, kidneys, bladder, skin, and muscles, &c. "What," he asks, quoting from Prof. Humphrey, "has been the influence of this upon medical treatment? . . . Take away the knowledge which we have received through vivisection, and conceive what a chaos would be our knowledge of the human body, and our ideas of the treatment of the diseases of the human body; you can scarcely conceive to what we should be reduced. Every man in the whole history of medicine, every man who has made real advances in the knowledge of the workings of the human body, has done it through vivisection."

The utility of vivisection is further shown by Sir W. Gull, Dr. Carpenter, Mr. Fleming, and Dr. Brunton. The former alludes to the discovery of the lymphatic system, and of the capillary circulation, to "the great advances made by Boyle, Mayow, and Lower, in the same century"; and more especially to the work of Claude Bernard on animal heat. This is specially alluded to in consequence of Miss Cobbe in her article "writing of the title of Claude Bernard to be honoured by physiologists, saying that such title is, at least partly, based on the invention of a stove which should enable him to watch the process of 'baking dogs alive'"—a statement "calculated, if not intended to convey a totally false impression both of the purpose and the details of these memorable experiments." The importance of the latter in relation to preparing the way for a full understanding of the deadly phenomena of fever is then clearly exhibited, together with the fact that "if a dog be put into a heated chamber and his blood be raised to the temperature of a bird's, he quickly dies"—so that the "baking alive" really means raising the temperature of the dog's blood through ten degrees. Every year in this country alone 40,000 persons die of scarlet and typhoid fevers—"baked alive" by them; and this constitutes but a small part of the annual deaths in which exalted temperature is a fatal factor. No wonder therefore that medical men pay tribute to the memory of Bernard for opening the way to an understanding of the subject, such that "this fiery furnace, with its uncounted millions of victims, science hopes to close; and it is quite reasonable to believe that the time will come when fever will be as much under our control as are the movements of a chronometer." When to this it is added that "Bernard, in these experiments on fever, sacrificed two pigeons, two guinea-pigs, less than twenty rabbits, and six dogs," we cannot think that the selection by Miss Cobbe of her favourite atrocity is a very fortunate one.

Sir William Gull proceeds to consider the great gains which have accrued to medical science through some of the experiments of Magendie, and those of Marshall Hall; and Dr. Brunton, in a singularly telling article, shows how in an ordinary diagnosis it is impossible to advance a step without using at every point knowledge gained by experiments on animals. He also appeals to the British Pharmacopœia to prove that to vivisection "we owe the introduction of the most valuable of our new remedies." Between the editions of 1864 and 1867 there are added seven new drugs, of which at least the two most useful—viz. carbolic acid and physostigma—are due to vivisection. Again, between the editions of 1867 and 1874 we find eleven new remedies, of which the three most useful—pepsine, chloral, and a nitrite of amyl—were discovered, or their uses perfected, by experiments on animals. So that, without considering "many other new remedies which are still on their trial, and which will, in all probability, be added to the next edition of the Pharmacopœia, it is a matter of already accomplished fact that "the introduction of nearly all the most valuable new remedies which have been added to the Pharmacopœia since the year 1864" have been discovered by vivisection.

Still confining ourselves to the question of utility, we have next to notice the essay by Mr. Fleming, who undertakes to show that even in the exclusive interests of animals themselves, it is most ill-advised to tie the hands of science in its investigation of disease. Anthrax alone, in a single district of France, kills about 178,000 sheep a year, and in 1857 100,000 horses perished from this disease in Russia alone. Many other equally startling statistics are given; and now, owing to the laboratory experiments of Pasteur and others, "there is no longer any doubt as to the value of protective inoculation;" and the same method has been found equally effectual in protecting poultry from "fowl-cholera." It is not improbable that hydrophobia, glanders, cattle-plague, pleuro-pneumonia, swine-plague, sheep small-pox distemper, and tuberculosis, will all admit, by modifications of the same method, of being similarly brought under control.

In connection with utility we have space only to refer to one other case, but this the most conspicuous. We allude to the work of Lister, which, as Dr. Carpenter says, "constitutes by far the greatest single improvement ever introduced into surgical practice," and which, as Dr. Wilks says, "has been the means of saving the lives of thousands every year, both in England and on the Continent." Yet Prof. Lister "found that he was obliged to discontinue his important investigations or conduct them abroad. He chose the latter course, and went to France; for, he said, 'even with reference to small animals, the working of the Act is so vexatious as to be practically prohibitory of experiments by a private worker like myself, unless he chooses to incur the risk of breaking the law.'"

Such, then, is a brief abstract of the evidence on the head of utility. This evidence is not disputed by the writers on the other side, with the exception of Miss Cobbe and Mrs. Kingsford; but the writings of these ladies upon the subject are so extravagant and ill-advised that even an ignorant reader must feel their judgment upon this head to be valueless.[1] With the unanimous opinion before them of the International Medical Congress, the British Medical Congress, and of all persons whose knowledge of physiological science entitles them to be heard on this point, Lord Coleridge and Mr. Hutton adopt a line of argument which, so far at least, is more judicious. Lord Coleridge says: "I will not dispute with them as to the fact. A lawyer ought at any rate to know the folly of encountering an expert without the knowledge necessary for success in the conflict. I deny the practical conclusion sought to be drawn from it on grounds of another sort, which appear to me of overwhelming force." And Mr. Hutton says: "I have never believed all these experiments to be scientifically, or even medically, worthless," and he allows that some of them (inoculations) have been "*very* fruitful."

We may, then, take the evidence of utility as being beyond all question by any reasonable and impartial mind. Next let us consider the arguments which are adduced against vivisection other than the suicidal one of inutility. These may broadly be classed under two headings—those which assert that vivisection is immoral, and those which assert that it is irreligious.

In considering these arguments we may best begin with the essay of Lord Coleridge, and in doing so we find it difficult to strike the balance between our respect for the

[1] The authority of Sir W. Fergusson and of Sir Charles Bell is indeed quoted in support of the uselessness of vivisection to surgery, but their opinion on this subject—or rather the opinion of the former, because the latter did not live to see all the results alluded to by Sir James Paget—is so immeasurably outweighed by professorial opinion in general that it is interesting chiefly because of its isolated character. The only other feature in these papers that deserves the name of argument is that inference from the results of experiments on animals to the physiological economy of man may be erroneous, or even misleading. But what does this argument show? Surely not that, *for this reason*, experiments on the nearest analogues of the human body should not be made. No instance can be pointed to of a fatal or even deleterious mistake having been made as a consequence of any such erroneous inference, nor is it at all likely that such an instance can ever arise.

man, and our astonishment at the feebleness of his production. Taking as fair and unbiased a view as we can, it appears to us that, as a mere matter of pleading, if this is all that can be said, even by a Lord Chief Justice, in favour of abolishing physiological experiment, the physiologists could not well find a better advocate. Indeed, as the paper throughout shows ill-concealed evidence of intolerable irritation, its manner, as well as its matter, suggests that the writer himself begins to feel that he has committed a mistake in too early and too warmly espousing an irrational cause. But, be this as it may, we shall endeavour to state, as fairly as we can, the course of his argument.

We have already seen that he expressly disregards the question of utility, and bases his argument "on grounds of another sort." These grounds are that, even if practically useful to the extent claimed by physiologists and medical men, knowledge gained by vivisection ought not to be sought or tolerated, inasmuch as it is "unlawful," or "pursued by means which are immoral." Here, at least, we have a definite marking out of the "grounds" on which Lord Coleridge justifies his determination, as he says, "earnestly to support the Bill which Mr. Reid is about to submit to the House of Commons," *i.e.* the Bill for total abolition. We think, therefore, that it becomes a matter of importance clearly to define what is here meant by "immoral" and "unlawful." The only indication given by Lord Coleridge of such a definition is as follows: "I deny altogether that it concludes the question to admit that vivisection enlarges knowledge; I do not doubt that it does, but I deny that the pursuit of knowledge is in itself lawful; still more do I deny that the gaining knowledge justifies all means of gaining it." So far as these general propositions are concerned the principles of morality are obvious, and would be disputed by no one; but now for the special case of vivisection. "To begin with, proportion is forgotten. Suppose it capable of proof that by putting to death with hideous torment 3000 horses you could find out the real nature of some feverish symptom, I should say without the least hesitation that it would be unlawful to torture the 3000 horses." Now in the first place, supposing—as we must suppose if the illustration is to stand as argument—that the knowledge gained concerning the "feverish symptom" is to be knowledge useful in the saving of human life, we think that a truer note of "morality" is struck by Sir James Paget when he writes of a man whom he saw die under chloroform faultlessly given, "he was so good and generous a man that I felt it would have been right to kill a hundred animals either to save his life or to find out why he died, and to be able in the future to avert so awful a catastrophe." And if it is sound morality thus to feel that one hundred animals may be sacrificed to avert one such catastrophe, can it be otherwise to feel that three thousand animals may be "lawfully" sacrificed with the certainty of gaining assured knowledge—for this is the argument—which is to save many human lives?

But, in the second place, this brings us to the question of proportion, which is rather vaguely presented in Lord Coleridge's illustration. And on this question the physiologists are perfectly ready to join issue; in fact it is one of their strongest positions, and cannot be more tersely stated than it is by Dr. Carpenter, whose very temperate and most judicious essay on the "Ethics of Vivisection," appears in the same number of the *Fortnightly Review*, and in this, as in several other points, anticipates Lord Coleridge's arguments in a manner singularly complete. He here says: "My argument then, is that if in all the foregoing cases (*i.e.* of animal labour, &c.) the moral consciousness of those who consider themselves most elevated in the scale of humanity justifies the infliction of animal suffering for what is obviously a real benefit to man, even though the continuance of such benefit involves *the constant renewal of the suffering*, much more is *a temporary and limited infliction* justifiable, for the discovery of such scientific truths as have a clear prospective bearing on human well-being, moral as well as physical, since every such discovery, once established, is *a boon for ever*, not only in its direct applications, but in serving as a stepping-stone to further discoveries, which may prove of still more priceless benefit."

Again, Sir W. Gull very properly asks: "Why does Lord Coleridge, for the purpose of his argument, select *horses*, and why so large a number as three thousand? He must know that the horse has been but little experimented upon . . . so that the supposition of *three thousand horses* and *hideous torment* is an exaggerated expression, out of proportion to the facts—misleading, and in no way conducive to a fair judgment of the question at issue." The truth is, it would be better for the cause which Lord Coleridge has embraced if "proportion" *could be* "forgotten"; for the strongest point in the counter-argument is that there is no comparison between the ratios—as Pain inflicted on animals for other purposes : Pain inflicted by Vivisection : : Prevention of Pain resulting from the former : Prevention of Pain, or other utility, resulting from the latter. And therefore, just because they do *not* forget the question of proportion, physiologists maintain that it is unreasonable in humanitarians to attack the only kind of "cruelty" that is really fruitful—and fruitful a hundredfold—in mitigating pain, not only in the case of man, but, as Mr. Fleming shows by his very astonishing tabular statements, also in the case of animals. Yet this essential argument has wholly escaped the observation of Lord Coleridge, and as a consequence he entirely misapprehends the subordinate argument of the physiologists who point to cases of admitted, wholesale, and useless kinds of cruelty as those towards which the energy of humanitarians should be directed. For he represents this argument as saying: Because there are many other kinds of cruelty of greater magnitude in the world, therefore "something which, *consistently with all this argument*, may be horribly cruel and utterly worthless, is to be let alone." Truly Lord Coleridge may be "positively mortified to have to notice" such an argument as this; only no one, so far as our knowledge extends, has ever advanced it. No physiologist could be simple enough to defend vivisection on the supposition that it "may be horribly cruel and utterly worthless." The real argument to which Lord Coleridge refers is this:—Because there are many practices permitted which are without question horribly cruel and utterly useless, therefore it is irrational folly to waste the energy of humanitarian feeling in a warfare against the only kind of pain-giving practice which is directed towards the mitigation of pain, and which has already been successful in this its object to a degree *out of all proportion* to the pain inflicted.

If it is true, as the physiologists agree with Lord Coleridge in maintaining, that the ethics of vivisection turn upon this question of proportion, it becomes morally as well as logically incumbent on all those who take an active part in the anti-vivisection movement to make themselves acquainted with the facts by which alone this question can be determined. We therefore recommend all who are interested in the subject to read the very powerful essay by Prof. Gerald Yeo in the current number of the *Fortnightly Review*. This essay, when contrasted with that of Lord Coleridge or Miss Cobbe, exhibits in a striking manner the difference between knowledge and imagination, and therefore we do not think it can be said that the measured censure bestowed by this writer is too strong where he thus alludes to the above illustration of 3000 horses put to death by "hideous torment"—"I should have thought it impossible that a man who declares, 'I am not conscious of any distorting influence on my judgment; I have no anti-scientific bias,' could have suggested a case so horribly improbable. The extravagant irrelevancy of such a sacrifice, and its utter incompatibility with anything that can be called physiological research, are so manifest as to need no comment. Surely the writer cannot really imagine that such sheer brutality is within the range of possibility; or does he merely make the hideous suggestion in order to frighten those who have no knowledge of the matter? I refer to this sentence simply as an illustration of how unfounded and inaccurate ideas sometimes originate. For there can be no doubt that the mere mention of this appalling problem by such an authority, however repugnant it may be to common sense, cannot fail to leave some very unpleasant traces in the minds of many who imagine that a Lord Chief Justice would not undertake to write articles in a leading review, unless he had some accurate knowledge of the practical bearings of the subject."

Prof. Yeo has written his article in order to place such knowledge within reach of the general public, and he has evidently done so with the honest purpose of being "accurate." He says: "The exact relation of painful experiment to physiology may be best seen in a short analysis of physiological methods. Practical physiology is made up of four departments, in which its histological, chemical, physical, and vital branches are respectively studied. . . . Thus in fully three-quarters of practical physiology living animals do not appear at all. All vivisections are found in the fourth branch of physiology, but even here they form a very small part, for a large number of experiments on living animals (including man) are carried on without either cutting or pain." Taking then the subdivision of possibly paingiving experiments, Prof. Yeo gives an analysis of the Parliamentary Reports during the last five years, in order to get at the precise number of paingiving experiments which, during that time, have been made in this country. From these figures it appears that about three-fourths of the possibly paingiving experiments were rendered painless by the administration of anæsthetics, and of the remaining fourth, four-fifths were "like vaccination or the hypodermic injection of morphia, the pain of which is of no great amount." The tabular percentage is, therefore, as follows:—

Absolutely painless	75
As painful as vaccination	20
,, healing of a wound	4
,, surgical operation	1
	100

Thus, since the statistics have begun to be taken under the new Act, it is a matter of numerical statement that in this country only 1 per cent. of experiments in vivisection are attended with pain greater than that caused by pricking with a needle or healing from a cut. Therefore we must here repeat our judgment that in this discussion it is shown to be the anti-vivisectionists, and not the physiologists, who have "forgotten" the question of "proportion"; for nothing can prove more conclusively than these figures that Lord Coleridge's statement of the case becomes true only if it is quoted with inverted meaning—"There is no proportion between the end and the means."

But Prof. Yeo is speaking of physiology as practised in England. Foreign usages he does not feel that it devolves on him to defend, and he appears to have an easy task where he shows that all the array of horrors which the anti-vivisectionists have been able to collect from the past history of physiological research have been derived from abroad. And it appears but fair argument to draw this distinction. This country cannot legislate for foreign physiologists, and no particle of evidence has ever been forthcoming to show that English physiologists are less scrupulous than the rest of their countrymen in their regard for animal suffering. On the contrary, long before the agitation began these physiologists themselves at the British Association formally laid down and formally accepted a carefully worded code of rules (quoted in the essay by Sir W. Gull) to guide their action with a view to minimizing of pain; and they have never, either collectively or singly, objected to legislation against possible abuses, while many of them have distinctly expressed their approval of such legislation. The long array of atrocities which constitutes the bulk of Miss Cobbe's paper is therefore quite irrelevant to any question in which this country is concerned. At most she can only argue, as Mr. Hutton argues—Because such things have happened on the Continent they *may* also possibly happen in England; and the answer is, By all means let there be legislation to guard against the possibility. And Prof. Yeo proves, we think conclusively, that the existing Act is abundantly sufficient for this purpose.[1]

Another argument on the score of morality that has been advanced is one which is well and temperately stated by Mr. Hutton. He says: "You cannot take a step so certain to stimulate the thoughtless cruelty which still survives among us, as to sanction the deliberate infliction of a great mass of thoughtful cruelty, justified only by the prospect of ultimate benefit to man at the cost of untold agonies to his miserable fellow-creatures." But here, if the mis-statement of the "proportion" question presented by the concluding words is disregarded, it is evident that the point of ethics raised must be determined solely by consideration whether the "thoughtful cruelty" is *cruelty*—*i.e.* pain inflicted without an adequate

[1] It seems to require pointing out to Lord Coleridge and Miss Cobbe that to quote a brutal expression from any foreign physiologist is no argument for the *abolishing* of physiological experiment. Even if such expressions were English, or if the Royal Commission had found cases of abuse to occur in this country, there would be no such argument.

moral justification. And to assume that it is so is to beg the whole question.

Only one other argument of an ethical kind remains to be considered, and we are sorry to say that it has been advanced by Lord Coleridge—sorry because it is so childishly weak. It is the old argument that if the advancement of knowledge is taken to justify the vivisection of animals, as much or still more should it be taken to justify the vivisection of men; and in view of the horrible possibility thus supposed, Lord Coleridge exclaims—"I hope that morals may always be too much for logic; it is permissible to express a fear that some day logic may be too much for morals." Logic! Only on the assumption that an animal is a rational and a moral as well as a sentient creature, and that its reason and its morality are on a level with those of man, would the argument become logically valid; and it is just because the physiologists *do* "consent to limit the pursuit of knowledge by considerations not scientific but moral," that they are obliged to draw the same logical distinction between men and animals as that which is drawn by the Legislature.

Coming lastly to the side of Religion, Mrs. Kingsford concludes her article with a paragraph which we think worth quoting, as it may serve to indicate the value of her opinions generally: "If I should be asked what is the real position taken by the leading champions of 'free' vivisection, and concealed from the public under the plea that the practice conduces largely to the benefit of humanity, I would define it thus:—

"1. Repudiation of the religious and sympathetic sentiments, and of the doctrine of man's moral responsibility as superstitious and untenable.

"2. Deliberate determination to dissociate themselves from all but those who join in such repudiation; and to make the practice of experimental physiology on living animals a rallying-point for the expression of that determination."

Surely it must appear to Mrs. Kingsford that these "leading champions" are adopting somewhat roundabout means to secure their very remarkable ends.

Lord Coleridge asks: "What would our Lord have said, what looks would He have bent upon a chamber filled with 'the unoffending creatures which He loves,' dying under torture deliberately and intentionally inflicted?" And Prof. Yeo answers: "I cannot imagine any such chamber of horrors, any more than I can his other hideous suggestion;" and adds that concerning the real facts of vivisection as performed in this country, "my conscience unhesitatingly tells me that it would have met with the full authority and approval of our Lord. . . . And I like to bear in mind the texts which seem to have an accurate bearing upon the subject, 'Ye are of more value than many sparrows,' 'How much then is a man better than a sheep?'" Similarly Sir W. Gull and Dr. Carpenter support physiological research on grounds of Christianity and Theism, and it is evident that the religious side of the question really hinges on the ethical. If vivisection is cruel, it is also irreligious; but if it is the highest mercy, physiologists may claim, though from those to whom their work has been of priceless value they may not always receive, the beatitude of the merciful.

FISHER'S "EARTH'S CRUST"

Physics of the Earth's Crust. By the Rev. Osmond Fisher, M.A., F.G.S. (London: Macmillan and Co., 1881.)

MR. FISHER is well known to geologists as the writer of various important papers on Mountain Chains, Terrestrial Heat, and other physical phenomena of the earth. He has in this volume not merely collected these papers, but added so much new matter that they form only a small part of the book. It deals with those regions whither we cannot penetrate, and might be called a Treatise on Concealed Geology.

It has been made a reproach to geologists that their mathematics never get beyond the Rule of Three. Mr. Fisher may redeem them from the reproach. Indeed an unmathematical reader, when he sees pages covered with symbols, may be tempted to close the book in despair and imagine it a case of μηδεὶς ἀγεωμέτρητος εἰσίτω. However he would not act wisely. If he read steadily on, only omitting such calculations as he cannot understand, he will obtain many fruitful ideas, and follow several chains of sound and careful reasoning.

After a discussion of the rate of increase of temperature met with below the surface of the earth (which he concludes by adhering to the customary view of a uniform rate) Mr. Fisher reprints his former calculations of the enormous and overwhelming pressure to which the crust of the earth would be subjected, if the interior shrank away from it by contraction. The pressure would be such as the strongest rocks could not resist. The engineers of the St. Gothard Tunnel were almost baffled in attempting to sustain less than a mile's thickness of yielding rock. What arches or rings, what metal or granite would stand two thousand times that stress? There can be no doubt therefore that contraction is a cause adequate in intensity to contort any strata however thick, or uplift any continent however lofty. Adequate in intensity most certainly; but has it been sufficient in quantity? This question Mr. Fisher next considers. The answer will probably surprise many geologists.

When the earth first formed a solid crust with a glowing nucleus reaching to within a few feet of the surface, the nucleus would begin to cool and contract. As it shrank, the shell settling down on to it must crush itself into wrinkles. As successive internal portions solidified and were united to the solid crust, the remaining nucleus would continue to shrink, and the volume crushed out from the crust in process of accommodating itself would grow correspondingly greater. The wrinkles would be magnified. From Sir W. Thomson's formulæ for the internal temperatures of a cooling globe Mr. Fisher calculates the total volume of the wrinkles that could have been produced by now. He shows that this cannot possibly be so much as the fifteenth part of the volume of continental elevations above the sea bottom: more probably not even the sixtieth part. Though he considers the nucleus fluid, while Sir W. Thomson thinks the whole globe would have been solid or nearly so, this does not seem to affect the correctness of the conclusion. At the same time this cause, inadequate for continents, might yet be abundantly sufficient for the existing mountain chains, and for many predecessors of them.

The theory of the earth's constitution put forward in this volume is that the surface on which we stand belongs to a crust some thirty miles thick, floating on a substratum of slightly greater density. Below this substratum *may* be a solid nucleus, *must* be if Sir W. Thomson's proof of the earth's rigidity be accepted; but this book does not profess to go deeper. The floating crust cannot be supposed to possess much strength, so that the weight of mountain chains would break through it, unless they have beneath them corresponding protuberances on the under side of the crust, which shall support them by the additional buoyancy so produced. A plastic crust under compression would yield above and below, and thicken, as two pieces of hot sealing-wax spread out when pressed together, so as to give rise to such double bulges as are supposed. However, it is shown that even on this theory contraction cannot have produced the whole of existing terrestrial inequalities of surface, and could hardly even have lifted the continents above the sea-level. The general result of these suppositions would be that the crust beneath the ocean basins must be denser and thinner than that beneath the continents. To every ocean depression must correspond a similar larger concavity below, and continental elevations must have much greater protuberances answering to them on the under-side. Thus, could we strip off the crust like a hide, and turn it over, we should find the under-side reproducing the upper-side, only with every feature magnified.

This conception may be deemed at first sight strange and wild, yet it certainly affords an easy explanation of one or two rather singular phenomena. It was found during the Indian Survey that the mountain mass of the Himalayahs attracted a plummet much less than it ought to do, and that the cavities which contain the waters of the ocean, instead of causing a diminution of attraction, show an increase. Now it is shown that the protuberances of light material below the former and the concavities filled with the denser substratum below the latter would produce exactly such results. Also the hemisphere of water, which maintains its position in spite of continental attraction, is thus sufficiently accounted for. Again, since the floating crust must sink wherever weighted, and rise wherever material has been removed, we see how vast thicknesses of sediment might be accumulated without much perceptible change of depth, and mountains suffer continual degradation, and yet never be entirely effaced.

Another remarkable argument is derived from the observations in the St. Gothard Tunnel, which show that the rate of increase of internal temperature is slower there than beneath plain countries, and slowest where the mountain is highest. This should not be the case, perceptibly were the earth cooling as a uniform solid. Assuming these rates to be uniform, and allowing for the cold due to the elevation, it is easy to calculate the depths at which any particular temperature would be reached. If there be a molten nucleus its surface should be a surface of uniform temperature. But the depth at which a temperature of fusion can be reached will be found far greater under the mountains than under the plains. Hence, says Mr. Fisher, the solid crust must have protuberances below, answering to the mountains above.

This argument is weighty. It approaches near to demonstration. If this slower rate of temperature-increase below mountains were satisfactorily made out, and if we could be sure that the rate remained uniform at all depths, the existence of such protuberances would be almost proved. It is difficult to see what other supposition could be made. However, with the wide discrepancies at present experienced in the observations of such temperatures, and with the evidence that exists for a rate depending on the depth, a sceptic is not quite compelled to assent.

No theory of the earth's crust can be complete which does not provide the machinery for earthquakes and volcanoes. Mr. Fisher, for this purpose, supposes his subterranean fluid to contain, in intimate union with itself, vapour in considerable quantities. This vapour is to be retained in the fluid by the superincumbent pressure, as gas is in the liquid of a soda-water bottle, and will, if such pressure be removed, be disengaged from the molten matter as the gas disengages itself when the cork is drawn, though much more slowly, by reason of the viscosity of the fluid. This agrees with the view taken by Prof. Judd in his recent volume on Volcanoes. It will be a novel idea for many of us to imagine the earth like a globular bottle of effervescent liquid, and its crust like ice covering a lake of aërated-water. But such a constitution would account for many of the phenomena of eruptions. The earthquakes which usually herald them, the rise of molten material in a fissure, the existence of permanently liquid lava like that in Kilauea, the quiescence of neighbouring vents, the growth, death, and revival of a volcano, all follow as natural consequences. The difference in the lavas ejected from adjacent craters and the supposed order of succession in the products erupted are also accounted for, but not so satisfactorily. The theory is a very important one, and appears on the whole the most satisfactory that has yet been propounded.

It is natural to suppose that the emission of the vapour from this substratum would tend to produce a contraction of the nucleus. When we consider how far the volume of the ocean exceeds that of the continents it is surprising to be told in the chapter on the Extravasation of Water that this supposition cannot account for them. However it will be found on examination that much depends on the hypothesis. The supposition made is not local emissions of liquid producing cavities, but a general exudation and consequent crumpling of the crust. The analogy is not to the subsidences in Cheshire, where brine has been removed, but to the wrinkled skin of an apple as it dries.

The reader of this volume must bear in mind that most of the numerical results from time to time obtained and used are deductions from assumed data, and not independent truths. Such is a statement which often occurs in the calculations, that the contraction required to produce the existing inequalities of the earth's surface is 0·0105. He must also distinguish real confirmations of the theory such as the deviations in the plumb-line and the slower subterranean temperature-increase in the neighbourhood of mountains, from mere appearances of coincidence in numerical results. The latter are in several cases necessary consequences of identical assumptions. The agency of intruded dykes in producing elevation and compression does not seem altogether a natural one. We may conceive the crust passing down into fluid, but not so readily that the fluid should pass again into a solid centre. Another formidable difficulty is that a subterranean ocean must be subject to tides, as much as the sea would be though covered by ice. This is passed over somewhat lightly with the suggestion that viscosity may be sufficient to obscure all tidal phenomena. Doubtless, too, other difficulties will start up for which it may not be easy to find a solution. But every theory is sure to present difficulties. Time must show whether they multiply or die away.

One or two points do seem to emerge from this assemblage of calculations as fairly clear, and established on tolerably firm foundations. Such are, that contraction of the earth by cooling is inadequate to the production of its greater inequalities. The earth cannot be a mass quite so homogeneous as on the theory of having cooled from a perfect fluid it is often assumed to be. There must be subterranean irregularities of density. Besides these, the phenomena of volcanoes seems to be explained best, as yet, by the existence of vapours and gases in intimate mixture with the materials below its crust. And a substratum plastic, if not fluid, will account for many facts which are ordinarily very perplexing. But, to quote from a striking quotation made in the volume itself, "Of all known regions of the Universe the most unsafe to reason about is that which is beneath our feet." E. HILL

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.*]

Vignettes from Nature

ANXIOUS that popular scientific literature, especially that which deals with the Evolution-doctrine, should be strictly accurate in its *facts*, I would ask—in no unfriendly spirit—whether Mr. Grant Allen and Mr. Wallace have fully informed themselves upon each of the several positions taken in the paragraph cited with approval by Mr. Wallace (in the last number of NATURE, p. 381) from Mr. Grant Allen's "Vignettes," referring to the dimensions of the largest animals now existing, as compared with those of the faunæ of past epochs.

1. It is asserted that "no known extinct animal was as large as some of our modern Whales." When, some thirty years ago, I visited the so-called "coprolite" pits in the Suffolk crag, I was astonished at the multitude of the ivory-like "ear-bones" of whales found in a certain group of them; which were described by Prof. Owen, and compared with those of existing *Balænidæ*, in his "Fossil Mammals of Great Britain." From the fragments of gigantic ribs and vertebræ which I then saw at Felixstowe, I should certainly suppose the extinct whales they represent (which Prof. Owen regards as of Eocene age) to have been fully as large as those of the present time.

I would ask, further, whether sufficient account has been taken, in the statement just cited, of the most gigantic types of *Reptilian* Mesozoic life? Any one who has placed himself by the side of the huge bones of the *Cetiosaurus* which form such a conspicuous feature in the Oxford Museum, must, I think, be disposed to regard the animal there represented as having probably at least equalled the whale in *bulk*, though very likely not in length. And even this colossal reptile must have been far exceeded in dimensions by the *Atlantosaurus montanus* described by Prof. Marsh from the Wealden of Colorado. I would respectfully ask the authors, therefore, whether they are prepared to show that such an estimate is fallacious.

2. Having been led to believe, by all I have seen, heard, and read, that the *ordinary* bulk of our existing Elephants (I do not speak of exceptional "Jumboes") was considerably exceeded by that of the Mammoth and Mastodon—the former surpassing them in *height* (see the comparative measurements given by Prof. Owen, *op. cit.*), and the latter in *length* of body, I cannot but feel surprised that Mr. Grant Allen should speak of elephants "as having been increasing in size from the earliest epoch of their appearance to the present day"; still more, that Mr. Wallace should endorse the statement. Of course I shall at once bow to the superior knowledge of the latter most distinguished zoologist, when he refers me to trustworthy measurements in support of his position.

3. I can speak with more confidence in regard to the relative size of extinct Sharks, none of which, in the judgment of Mr. Grant Allen and Mr. Wallace, surpassed the forty-feet sharks of the present time. For I have now before me a tooth of a fossil shark (found in one of the before-mentioned "coprolite pits") of pretty regular triangular form, measuring *four* inches in length, *three* inches across the base, and seven-eighths of an inch in thickness between its flat surface and the most protuberant part of its convex surface; and I have seen others much larger, the length of some being said to range to *six* inches. Now when I brought this tooth home, I took an early opportunity of comparing it with the largest teeth of existing sharks that I could find in the British and Hunterian Museums, and found these to be pigmies by comparison. Unless, therefore, I can be referred to some fresh source of information, I must continue to believe (*pace* Mr. Grant Allen and Mr. Wallace) that some of the older sharks were far larger than any of which we have any knowledge at present.

4. Is it clear that *Tridacna* is the largest known Mollusk? I should have thought it exceeded by the gigantic *Ammonitidæ*, the largest specimens of which are not always to be found in museums; for I have seen one at Redcar (whose diameter I am afraid to state from memory, for fear of exaggeration) so massive that no one had undertaken the task of removing it.

5. No mention is made of *Crustacea*, though I should have thought that important class worthy of notice. I would ask where any existing crustacean types are to be found, that surpass in size the gigantic *Eurypteridæ* or even the largest *Trilobites*.

6. Of the *Foraminifera*, one of the most important classes in the whole animal kingdom for the share it has taken in the formation of our limestone rocks, I venture to speak with some special knowledge. The largest examples of this group known to us at the present time are the *Orbitolites* and *Cycloclypeus*. The former is a very widely diffused type, but only under peculiar local circumstances exceeds an inch in diameter, or one-tenth of an inch in thickness; the latter is (so far as I am yet aware) restricted to one locality, and, though attaining the large diameter of 2¼ inches, is scarcely thicker than an ordinary card. If these be compared with the massive *Nummulites* and *Orbitoides*, of which the vast *Nummulitic* limestones are composed, the advantage will be found clearly on the side of the latter.

But, in conclusion, I think it will be conceded that in estimating the general dimensions of a Fauna, we must take into account not merely the *size* of its largest animals, but *the range of their distribution*; and I would ask Mr. Wallace (whose knowledge of this subject no one appreciates more fully than myself) whether this consideration has been duly weighed by him. Our existing *colossal* land mammals (elephants, giraffes, rhinoceroses, and hippopotamuses) are limited to the tropical and sub-tropical regions of the Old World; while the great American continent is entirely destitute of them. Let this state of things be compared with the former extension of the Mastodon[1] and Mammoth through North America (which had for its own also the gigantic *Brontotheridæ*), as well as over Europe and Northern Asia; and the nearly equal range of the Rhinoceros and Hippopotamus (some species of all which seem to have lived contemporaneously during the Quaternary Period); whilst at the same time the wide area of South America was tenanted by another Mastodon, as well as by the colossal Sloths. There can be no reason to suppose again that the great Balænidæ were less abundant during the later Tertiary and Quaternary epochs, than they were either previously or subsequently. And if the evidence of the *abundance* of some of the colossal land-Mammals—afforded by the vast accumulation

[1] That the Mastodon, though it appeared much earlier than the Mammoth in the Old World, continued to exist in the New during the Quaternary period, is now, I believe, generally admitted. I myself, at the request of Dr. Warren, examined the contents of the well-preserved specimen obtained by him, and found therein twigs quite fresh enough for the microscopic recognition of their Coniferous structure; and Prof. Asa Gray told me last summer that he could clearly identify them with a well-known existing type.

of Mammoth-tusks in the frozen mud of Siberia, and by the wonderful aggregation of Hippopotamus-bones revealed to us by Dr. Falconer's explorations in the Palermo caves—be also taken into account, we can scarcely, as it seems to me, avoid the conclusion, that the period in the later stages of which we get the first indubitable evidence of Man's existence (to say nothing of any anterior to it) was much more distinguished than the present for the aggregate bulk and wide distribution of the largest members of its fauna. WILLIAM B. CARPENTER

CAN Mr. Wallace throw any light on Mr. Allen's somewhat extraordinary sentence: "I feel a genuine respect for every donkey I meet, *when I remember that it was the mere accidental possession of an opposable thumb* that gave my ancestors a start over his in the race for the inheritance of the earth towards the very close of the tertiary period." I take Mr. Allen to be an evolutionist, but there is no place for accident in evolution, or in any other scientific theory. The "opposable thumb" must be the result of some conditioning factor, and this being so the word accident is quite out of place. ΦΠ

February 27

Moths Attracted by Falling Water

WHILST watching the great horse-shoe falls of the Skjálfandafljót near Ljósavatn in Iceland, I saw moth after moth fly deliberately into the falling water and disappear. Some which I noticed arriving from a distance, fluttered at first deviously, but as they neared the water flew straight in. The gleaming falls seemed at least as attractive as artificial light, and if the fact has not been observed in this country I should suppose it is because the moths likely to be attracted, fly by night, whilst in Northern Iceland there is no night during the summer. The preference trout show for pools near falls is more likely to arise from the extra food they find there, than the more aërated state of the water. The latter supposition, seeing the number of species of lake trout, always seemed to me a lame one, invented for want of a better, whilst the former explains why broken water is always inhabited by insectivorous fishes. The instinct of self-destruction in moths must be older than the introduction of artificial light, and cannot be of use exclusively to collectors, but though its benefits to salmon and trout are obvious enough, its advantages to the moths are not so apparent, unless this self-devotion checks an increase that otherwise would be disadvantageous. J. STARKIE GARDNER

Hypothetical High Tides

I HAVE no desire to constitute myself a champion of Mr. Ball's high tides, but I do not think that the testimony of the Coal-Measures, to which Mr. S. V. Wood calls attention, will decide much. These deposits are mainly of non-marine origin, the plants being terrestrial, and the prevailing mollusc, *Anthracosia*, closely resembling *Unio*. Marine strata do indeed occur, but in almost inappreciable proportion. If it be objected that, in these marine episodes, the hypothetical tidal wave must have wrought fearful havoc; I would suggest that there is no proof that in the Carboniferous epoch the speed of the wave was enormously greater than at present. When we reflect that by that time nearly, if not quite all the classes of the animal kingdom had come into existence; we can hardly avoid the conclusion that the Coal-Measures were formed in a period which, in comparison with the age of the globe, must be regarded as comparatively recent. Considering how slight is the denuding power of modern tides, I doubt if even a treble velocity would materially increase the effect.

Mr. Elsden's suggestion that the accelerated tidal wave may account for the absence of estuarine deposits before the Carboniferous epoch, takes for granted what remains to be proved. How do we know that there were no pre-Carboniferous deltas? We recognise estuarine strata by the intermixture of terrestrial or fresh-water fossils with marine organisms. The Old Red Sandstone of Britain, being a lacustrine deposit, does not bear upon the question; but I see no reason why the Devonian strata of Russia, in which, according to Murchison, fresh-water fishes are associated with marine shells, may not be in part of estuarine origin. Below the Devonian, the evidence of terrestrial life is very meagre; and to infer from its absence in a set of beds that they must be marine, would be hazardous reasoning.

I do not make these observations in the interests of any theory, but simply to evoke discussion on a very interesting question.

Wellington, Salop, March 3 C. CALLAWAY

Rime Cloud observed in a Balloon

I SEE in NATURE, vol. xxv. p. 385, an interesting letter from a German physicist, who comments on the recital of my last balloon ascent (January 25, 1882) as published in your columns. I am very grateful for the numerous instances of *frost-rime* that he quotes as having been observed on former occasions, but I cannot possibly admit his theory of the liquidity of minute water-drops suspended in the air at a low temperature. The reason why I object to this view was explained more than a century ago by the celebrated Bouguer, when describing in 1744, to the French Academy of Sciences the coronæ he observed in the Andes on the occasion of his ascending the Pichincha. I beg leave to quote this interesting account of a quite forgotten exploration:—

"On voit presque tous les jours sur le sommet de ces montagnes un phénomène extraordinaire qui doit être aussi ancien que le monde, et dont il y a bien de l'apparence que personne n'est été temoin avant nous. Chacun de nous vit son ombre projetée sur un nuage qui n'était point à trente pas. Le peu de distance permettant de distinguer toutes les parties de l'ombre—on voyait le bras, les jambes, la tête; mais ce que nous étonne c'est que cette dernière partie était ornée d'une gloire on d'une aureole formée de trois ou quatres petites couronnes concentriques d'une couleur tres vive, chacune avec le mieux nuance que l'arc-en-ciel primaire, c'est à dire le rouge en dehors.

After having insisted on the description of the phenomenon (*Mémoires de l'Académie* pour 1744, p. 264 and 265), Bouguer says:—"Le phénomène ne se trace que sur les nuages formés de gouttes de vapeur et même sur ceux dont les portraits sont glacées, mais non sur les gouttes de pluie comme l'arc-en-ciel." Having seen the *corona* for more than an hour, almost without interruption, and nothing resembling a rainbow, I cannot possibly admit any liquid water in the cloud, and I am obliged to oppose the surfusion theory as advocated by M. Jamin, to explain the crushing by ice-crystals of the loftiest trees of the Forest de Fontainebleau. W. DE FONVIELLE

Paris, February 26

The Markings on Jupiter

MR. DENNING'S interesting communications in NATURE (vol. xxv. pp. 223, 265) led me to consult my notes of observations of Jupiter made in the summer of 1878. I used a telescope of only 3⅜ inches aperture, but of exquisite definition, made by John Byrne, of New York. Under date of July 7, 1878, I find this entry:—"10 p.m.—There is a remarkable light spot near the centre of the light equatorial zone of Jupiter."

On July 27 I wrote:—"I saw on the bright equatorial belt of Jupiter a spot of obviously greater brightness than any other part of the disk. Just above and to the west of it was a dark spot on the southern belt. The bright spot grew more distinct as it approached the centre, and caught the eye the instant it was placed at the eyepiece. The bright spot was equal in diameter to about two-thirds of the width of the south belt."

Again on July 31:—"Saw a white spot on the light equatorial belt, probably the same seen on the 27th."

I have also sketches of Jupiter made in the fall of 1879, from which I see that on September 4, at 10 p.m., there was a distinct white spot indenting the northern border of the great south belt, and opposite the forward end of the red spot. On September 6 this white spot had advanced, so that it was ahead of the red spot. Other fainter white spots are shown in my sketches. These rude observations may be of some use in assisting Mr. Denning to trace back the history of the remarkable markings that for three or four years have attracted so much attention to Jupiter. G. P. SERVISS

New York, February 9

The Level of the Mediterranean

AMONG the "Notes" in NATURE, vol. xxv. p. 395, I read Prof. Naudin's opinion on the apparent lowering of the level of the Mediterranean along the whole Riviera during the months of January and February; but I think there is a far more simple explanation of the phenomenon. In Genoa we had for many days as much as 43 centimetres below the standard level, but that was caused by the northerly winds that prevailed during the above-mentioned period, and which drove the water off the coast. Just now the lowering of the atmospheric pressure, that had been as high as 778 millimetres, gave a prevalence to southerly winds, and the sea reached again its former level.

L. LUIGGI,
Resident Engineer at the Pier Works, Genoa, Italy
February 28

A Strange Phenomenon

RELATIVE to the letter of Mr. James Moir, under the above title, which appeared recently in NATURE, I beg to observe that in the Highlands of Perthshire, some forty years ago, two men found themselves enveloped in flames, somewhat in the same style as Mr. Moir was on February 18 last. One Mr. John Stewart, who, for many years, drove the Mail gig between Dunkeld and Aberfeldy, told me that on a certain dark night, he and another man, climbing a rocky, heathery height in Rannock, were all at once set on flames by some mysterious fire, which appeared to proceed from the heather, which they were traversing, and the more they tried to rub the flames off the more tenaciously they seemed to adhere, and the more the fire increased in brightness and magnitude. Moreover, the long heather agitated by their feet, emitted streams of burning vapour, and for the space of a few minutes they were in the greatest consternation. They believed that they barely escaped a living cremation. Of course their liberal share of native superstition, along with the weird gloom of the night in the weird wilderness remote from human habitation, rendered their position the more alarming. Mr. Stewart did not mention whether the weather was stormy or not; but without doubt the object of their fear was St. Elmo's Fire. The ignis fatuus has been frequently seen in these Highland districts hovering over marshes, rivers, and churchyards, which was believed by the superstitious to be the ghosts of the dead. When the ignis fatuus was seen flickering over the graveyard, it was a sign—with them—that some one was to be buried there soon, and when seen floating over a river, it was a sign that some one was to be drowned there that night or soon after, the floating, wandering lights being their ghosts. Drainage, in this respect, has effected many changes. DONALD CAMERON

45, Calder Street, Govanhill, Glasgow, March 6

MR. JAMES MOIR, in last week's NATURE (p. 410), mentions a probable peculiar manifestation of St. Elmo's Fire, and asks if any one can give instances of a similar occurrence. About twenty years ago I was returning, during the evening, to my house from Great Yarmouth, a distance of three miles, and took the road of the Denes, intending to cross by the lower ferry. Before reaching it a dark cloud coming from the south-east, off the sea, suddenly surprised me, and drenched me with rain. I jumped into the boat, and when the boatman had pushed off, I remarked that every drop of rain hanging from my hair, beard, and clothes was luminous with white light, well seen, as it was very dark at the time. I found the same appearance had been observed by several pilots exposed to the same shower. I always attributed the occurrence to a species of St. Elmo's fire. It was mentioned at the time by a friend of mine at a scientific meeting in London, and thought curious. W. H. C. B.

Cheltenham, March 7

Parhelia

OF the parhelia of January 27 seen by M. Albert Riggerbach (NATURE, vol. xxv. p. 364) I was a spectator, and noted my observations at once. I was walking near Pavia when I observed the phenomenon about 3.45. A mock sun (one only) was in the same altitude with the sun on the horizon; M. Riggenbach's *faint cirrus* obviously corresponds to the *filamenti nebbiosi* in my note; they were as I well remember, with the mock sun in the eastern part of the sky, while in the opposite region some blackish *cumuli* approached slowly. FRANCIS PORRO

Pavia, Lombardy, February 27

Red Flints in the Chalk

ARE red flints common in the Chalk? A portion of our College farm lies on a gentle slope on the Upper Chalk, which rises westward from the banks of the Hampshire Avon. On the higher parts of this slope black flints are excessively abundant, so much so that after sheep have been folded on the land, the fields present the appearance of a newly macadamised road, and the flints are picked up and put into heaps until an opportunity offers to use them for road-metal; in the course of a year they "grow" again as thickly as before. But one field on a ridge near the foot of the slope is remarkable for the number of red flints it contains; on the dusty soil they look just like bits of broken earthenware, and might at first fail to attract attention. Their size is much less than the average size of the black flints; some are rounded and some angular, others almost flake like. As to the frequency of their occurrence, I found I was able to pick up at least one at every step I took. W. FREAM

College of Agriculture, near Downton, Salisbury,
February 28

THE SALMON DISEASE[1]

FOR some years an epidemic disease, followed by a very large number of deaths, has been observed to prevail among the salmon of certain Scottish and British rivers, from the Tay, on the north, as far as the Conway on the south.

The first obvious symptom of the malady is the appearance of one or more whitish patches upon the skin of parts of the body which are not covered with scales, such as the top and sides of the head, the adipose fin, and the soft skin at the bases of the other fins.

Such a patch, when it first attracts attention, may be as big as a sixpence. It is nearly circular, with a well-defined margin and a somewhat raised softer centre, from which faint ridges radiate towards the circumference. It is important to observe that a single small patch of this kind may be seen on the skin of a fish which, in all other respects, is perfectly healthy, and when there is no indication that the skin has ever been bruised or abraded in the place occupied by the patch. The patch, once formed, rapidly increases in size, and becomes confluent with any other patches which may have appeared in its neighbourhood. The marginal area, as it extends over the adjacent healthy skin, retains its character; but the central part undergoes an important change. It takes on the consistency of wet paper, and can be lifted up in soft flakes, as if it were a slough, from the surface of the derma or true skin, which it covers. In fact, it is obvious that this papyraceous substance has taken the place of the epidermis, so that the sensitive and vascular true skin is deprived of its natural protection. As the patch spreads, the true skin beneath the central papyraceous slough ulcerates and an open bleeding sore is formed, which may extend down to the bone, while it passes outwards into burrowing sinuses.

When the disease has reached this stage it obviously causes great irritation. The fish dash about and rub themselves against stones, and thus in all probability aggravate the evils under which they suffer. One vast open sore may cover the top of the head from the snout to the nape, and may extend over the gill covers. The edges of the fins become ragged; and, sometimes, the skin which invests them is so completely frayed away that the fin-rays stand out separately.

Although the affection of the skin appears, usually, if not invariably, to commence in the scaleless parts of the body, it does not stop there, but gradually spreads over the whole of the back and sides of the fish, though I have not yet seen a specimen in which it covered the whole ventral surface. The disease extends into the mouth, especially affecting the delicate valvular membrane attached to the inner sides of the upper and the lower jaws. It is said to attack the gills, but there has been no sign of it on these organs in any fish which I have had the opportunity of examining.

Fish which succumb to the disease become weak and

[1] A Contribution to the Pathology of the Epidemic known as the "Salmon Disease." Paper read at the Royal Society, March 2, by Prof. T. H. Huxley, LL.D., F.R.S.

sluggish, seeking the shallows near the banks of the river, where they finally die.

The flesh of a salmon affected by this disease presents no difference in texture or colour from that of a healthy fish; and those who have made the experiment declare that the flavour is just as good in the former case as in the latter. So far as my observations have gone the viscera may be perfectly healthy in the most extensively diseased fish; and there is no abnormal appearance in the blood.

It is known that a disease similar to that described is occasionally prevalent among salmon in North America and in Siberia; and I do not see any ground for the supposition that it is a novelty in British rivers. But public attention was first directed to it in consequence of its ravages in the Solway district a few years ago; and, in 1878, a Commission was appointed to inquire into the subject.

The evidence taken by the Commissioners leaves no room for doubt that the malady is to be assigned to the large and constantly increasing class of diseases which are caused by parasitic organisms. It is a contagious and infectious disease of the same order as ringworm in the human subject, mascardine among silkworms, or the potato disease among plants; and, like them, is the work of a minute fungus. In fact, the *Saprolegnia* which is the cause of the salmon disease is an organism in all respects very closely allied to the *Peronospora*, which is the cause of the potato disease.

It is a very curious circumstance, however, that while the *Peronosporæ* are always parasites—that is to say, depend altogether upon living plants for their support—the *Saprolegniæ* are essentially saprophytes; that is to say, they ordinarily derive their nourishment from dead animal and vegetable matters, and are only occasionally parasites upon living organisms. In this respect they resemble the *Bacteria*, if the results of recent researches, which tend to show that pathogenic bacteria are mere modifications of saprogenic forms, are to be accepted.

As I have said, I do not think that the evidence laid before the Commission of 1878 can leave any doubt as to the causation of the salmon disease on the minds of those who are acquainted with the history of the analogous diseases in other animals and in plants. Nevertheless, this evidence, valuable as it is, suggests more questions than it answers, and in November, 1881, hearing that the disease had broken out in the Conway, I addressed myself to the attempt to answer some of these.

It was already known that when the papyraceous slough-like substance which coats the skin of a diseased salmon is subjected to microscopic examination, it is found to be a *mycelium*, or fungus-turf, composed of a felt-work of fine tubular filaments or *hyphæ*, many of which are terminated by elongated oval enlargements, or *zoosporangia*. Within these the protoplasm breaks up into numerous spheroidal particles, each less than 1-2000th of an inch in diameter. These, the *zoospores*, are set free through an opening formed at the apex of the zoosporangium, and become actively or passively dispersed through the surrounding water. Herein lies the source of the contagiousness or infectiousness of the disease. For any one of these zoospores, reaching a part of the healthy skin of the same or of another salmon, germinates and soon gives rise to a mycelium similar to that from which it started.

But I could find no satisfactory information as to the manner in which the fungus enters the skin, how far it penetrates, the exact nature of the mischief which it does, or what ultimately becomes of it; nor was the identity of the pathogenic *Saprolegnia* of the salmon with that of any known form of saprogenic *Saprolegnia* demonstrated. It appeared to me, however, to be useless to attempt to deal with the disease until some of these important elements of the question were determined.

To this end, in the first place, I made a careful examination of the minute structure of both the healthy and diseased skin, properly hardened and cut into thin sections; and, in the second place, I tried some experiments on the transplantation of the *Saprolegnia* of the living salmon to dead animal bodies. Perhaps it will conduce to intelligibility if I narrate the results of the latter observations first.

The body of a recently killed common house-fly was gently rubbed two or three times over the surface of a patch of the diseased skin of a salmon, and was then placed in a vessel of water, on the surface of which it floated, in consequence of the large quantity of air which a fly's body contains. In the course of forty-eight hours, or thereabouts, innumerable white cottony filaments made their appearance, set close side by side, and radiated from the body of the fly in all directions. As these filaments had approximately the same length, the fly's body thus became inclosed in a thick white spheroidal shroud, having a diameter of as much as half an inch. As the filaments are specifically heavier than water, they gradually overcome the buoyancy of the air contained in the tracheæ of the fly, and the whole mass sinks to the bottom of the vessel. The filaments are very short when they are first discernible, and usually make their appearance where the integument of the fly is softest, as between the head and thorax, upon the proboscis, and between the rings of the abdomen. These filaments, in their size, their structure, and the manner in which they give rise to zoosporangia and zoospores are precisely similar to the hyphæ of the salmon fungus; and the characters of the one, as of the other, prove that the fungus is a *Saprolegnia* and not an *Achlya*. Moreover, it is easy to obtain evidence that the body of the fly has become infected by spores swept off by its surface when it was rubbed over the diseased salmon skin. These spores have in fact germinated, and their hyphæ have perforated the cuticle of the fly, notwithstanding its comparative density, and have then ramified outwards and inwards, growing at the expense of the nourishment supplied by the tissues of the fly.

This experiment, which has been repeated with all needful checks, proves that the pathogenic *Saprolegnia* of the living salmon may become an ordinary saprogenic *Saprolegnia;* and, *per contra*, that the latter may give rise to the former; and they lead to the important practical conclusion that the cause of salmon disease may exist in all waters in which dead insects, infested with *Saprolegniæ*, are met with; that is to say, probably in all the fresh waters of these islands, at one time or another.

On the other hand, *Saprolegnia* has never been observed on decaying bodies in salt water, and there is every reason to believe that, as a saprophyte, it is confined to fresh waters.[1]

Thus it becomes, to say the least, a highly probable conclusion that we must look for the origin of the disease to the *Saprolegniæ* which infest dead organic bodies in our fresh waters. Neither pollution, drought, nor overstocking will produce the disease if the *Saprolegnia* is absent. The most these conditions can do is to favour the development or the diffusion of the *materies morbi* where the *Saprolegnia* already exists.

Having infected dead flies with the salmon *Saprolegnia*, once from Conway and once from Tweed fish, I was enabled to propagate it from these flies to other flies, and, in this manner, to set up a sort of garden of *Saprolegniæ*. And having got thus far, I fancied it would be an easy task to determine the exact species of the *Saprolegnia* with which I was dealing, from the abundant data furnished by the works of Pringsheim, De Barry, and others,

[1] So far as I know there is only one case on record of the appearance of a fungus on a fish in salt water, and in this case it is not certain that the fungus was a *Saprolegnia*.

who have so fully studied these plants when cultivated on the same materials. For this purpose, it was necessary to obtain the oosporangia; and in ordinary course, these should have made their appearance on my *Saprolegniæ* in five or six days. Unfortunately, in the course of cultivation continued over two months, nothing of the kind has taken place. Zoosporangia have abounded in the ordinary form and also in that known as "dictyosporangia," but, in no instance, have any oosporangia appeared. After a few days of vigorous growth, the zoosporangia become scanty, and the fungus takes on a torulose form, segments of the hyphæ becoming swollen and then detached as independent "gemmæ," which may germinate. Sometimes the gemmæ are spheroidal and terminal, and closely simulate oosporangia.

Although, therefore, I have very little doubt that the *Saprolegnia* of the salmon is one of the forms of the "*S. ferax* group" of Pringsheim and De Bary, I have, at present, no proof of the fact.

Another very curious and unexpected peculiarity of the salmon *Saprolegnia*, both on the fish and when transmitted to flies, so far as my observations have hitherto gone, is that locomotive ciliated zoospores do not occur. I once saw one which exhibited a very slight motion for a few minutes after it left the zoosporangium; but although thousands must have passed under my notice, with the exception to which I have referred, they have always been perfectly quiescent and not unfrequently in different stages of germination. Whether the season of the year, or the conditions under which my saprolegnised flies were placed, have anything to do with the non-appearance of oosporangia and of locomotive zoospores in them I cannot say. But it is certain that the *Saprolegnia ferax* which commonly appears upon dead flies and other insects normally develops both oosporangia and locomotive zoospores in abundance.

From such notices by other observers as I can gather, oosporangia appear to be of very rare occurrence in the *Saprolegnia* of the salmon itself. Mr. Stirling mentions that he has met with them only four times. With respect to locomotive zoospores, I can find no positive evidence that they have been regularly, or even frequently, observed in the salmon *Saprolegnia*. But these points require careful investigation on freshly taken diseased fish.

Whether the zoospores are actively locomotive or not, they are quite free when they emerge from the zoosporangia; and, from their extreme minuteness, they must be readily carried away and diffused through the surrounding water. Hence, a salmon entering a stream inhabited by the *Saprolegnia* will be exposed to the chance of coming into contact with *Saprolegnia* spores; and the probability of infection, other things being alike, will be in proportion to the quantity of the growing *Saprolegnia*, and the vigour with which the process of spore-formation is carried on. At a very moderate estimate, a single fly may bear 1,000 fruiting hyphæ; and if each sporangium contains twenty zoospores, and runs through the whole course of its development in twelve hours, the result will be the production of 40,000 zoospores in a day, which is more than enough to furnish one zoospore to the cubic inch of twenty cubic feet of water. Even if we halve this rate of production, it is easy to see that the *Saprolegnia* on a single fly might furnish spores enough to render such a small shallow stream as salmon often ascend for spawning purposes, dangerous for several days. But a large fully diseased salmon may have as much as two square feet of its skin thickly covered with *Saprolegnia*. If we allow only 1,000 fruiting hyphæ for every square inch, we shall have 288,000 for the whole surface, which, at the same rate as before, gives over 10,000,000 spores for a day's production, or enough to provide a spore to every cubic foot of a mass of water 100 feet wide and five feet deep and four miles long. Forty such diseased salmon might furnish one spore to the gallon for all the water of the Thames (380,000,000 gallons per diem) which flows over Tedington Weir. But two thousand diseased salmon have been taken out of a single comparatively insignificant river in the course of a season.

It will be understood that the above numerical estimate of the productivity of *Saprolegnia*, has been adopted merely for the sake of illustration; that I do not intend to suggest that the zoospores are evenly distributed through the water into which they are discharged by the zoosporangia; and that allowance must be made for the very short life of those zoospores which do not speedily reach an appropriate nidus. Nevertheless, the conclusion remains arithmetically certain that every diseased salmon adds immensely to the chances of infection of those which are not diseased; and thus, the policy of extirpating every diseased fish as soon as possible, has ample justification. But, in practice, the attempt to stamp out the disease in this fashion would be so costly that it may be a question whether it is not better to put up with the loss caused by the malady.

There are many practical difficulties in the way of directly observing the manner in which the zoospores effect their entrance into the skin of the fish; but, on comparing the structure of the healthy integument with that of the diseased patches, the manner of the operation can readily be divined. The skin of the head of a salmon, for example, presents a thin superficial cellular epidermis covering the deep fibrous and vascular derma. The epidermic cells are distinguishable, as in fishes in general, into a deep, a middle, and a superficial layer. In the first, the cells are vertically elongated, in the second more rounded and polygonal, in the third flattened. Many of the cells of the middle layer are of the nature of "mucous cells." They enlarge and become filled with a mucous secretion; and, rising to the surface, burst and discharge their contents, which give rise to the mucous fluid with which the fish's body is covered. The openings of these "mucous cells" remain patent for some time and are to be seen in thin vertical sections. The hyphæ of the spores which attach themselves to the fish may enter by these openings, but even if they do not, the flattened superficial cells certainly offer no greater resistance than does the tough cuticle of a fly. However this may be, sections of young patches of diseased skin show that the hyphæ of the fungus not only traverse the epidermis, but bore through the superficial layer of the derma for a distance, in some cases, of as much as one-tenth of an inch. Each hypha thus comes to have a stem-part, which lies in the epidermis, and a root-part, which lies in the derma. Each of these elongates and branches out. The free ends of the stem-hyphæ rise above the surface of the epidermis and become converted into zoosporangia, more or fewer of the spores of which attach themselves to the surrounding epidermis and repeat the process of penetration. Thus the epidermis and the derma become traversed by numerous hyphæ set close side by side. But, at the same time, these hyphæ send off lateral branches which spread radially, forcing asunder the middle and deeper layers of the epidermic cells, and giving rise to the radiating ridges which are visible to the naked eye in the peripheral part of the patch. The force of the growth of the hyphæ which traverse the epidermis, is made obvious by the curious manner in which, when the central tract of a patch is teased out, the distorted epidermis cells are seen adhering to it as if they were spitted upon it.

In the derma, the root-hyphæ branch out, pierce the bundles of connective tissue, and usually end in curiously distorted extremities.

The effect of the growth of the stem-hyphæ is to destroy the epidermis altogether. Its place is taken by a thick, felted, mycelium, which entangles the minute particles of sand which are suspended in the water, and thus no doubt constitutes a very irritating application to the sensitive surface of the true skin.

In the true skin, the tracks of the root-hyphæ are not accompanied by any obvious signs of inflammation, but the hyphæ are so close set, that they cannot fail to interfere with the nutrition of the part, and thus bring about necrosis and sloughing. Such sloughing in fact gradually takes place, small vessels give way and bleed, and the burrowing sore, which is characteristic of the advanced stages of the disease is produced.

The skin of the head may thus be eaten away down to the bone and gristle of the skull, but I have not observed the fungus to enter these. On the scaly part of the skin, the fungus burrows in the superficial and in the deep layer of the pouches of the scales, but I have not observed the scales themselves to be perforated.

When I found that the fungus penetrated the true skin, and thus gained access to the lymphatic spaces and blood-vessels, it became a matter of great interest to ascertain whether the hyphæ might not break up into turuloid segments (as in the case of the *Empusa muscæ*), and thus give rise to general septic poisoning, or fungoid metastastis. However, I have never been able to find any indication of the occurrence of such a process.

But a very important practical question arises out of the discovery that the fungus penetrates into the derma. There is much reason to believe, that if a diseased salmon returns to salt water, all the fungus which is reached by the saline fluid is killed, and the destroyed epidermis is repaired. But the sea water has no access to the hyphæ which have burrowed into the true skin; and hence it must be admitted to be possible, that, in a salmon which has become to all appearance healed in the sea, and which looks perfectly healthy when it ascends a river, the remains of the fungus in the derma may break out from within, and the fish become diseased without any fresh infection. It has not infrequently been observed, that salmon in their upward course became diseased at a surprisingly short distance from the sea, and it is possible that the explanation of the fact is to be sought in the revival of dormant *Saprolegnia*, rather than in new infection. It is to be hoped, that experiments, now being carried on at Berwick, will throw some light on this point, as well as upon the asserted efficacy of sea water in destroying the fungus which it reaches.

These are the chief results of this season's observations on the salmon disease. Incomplete as they are, they appear to me to justify the following conclusions:—

1. That the *Saprolegnia* attacks the healthy living salmon exactly in the same way as it attacks the dead insect, and that it is the sole cause of the disease, whatever circumstances may, in a secondary manner, assist its operations.

2. That death may result without any other organ than the skin being attacked, and that, under these circumstances, it is the consequence partly of the exhaustion of nervous energy by the incessant irritation of the felted mycelium with its charge of fine sand, and partly of the drain of nutriment appropriated by the fungus.

3. That the penetration of the hyphæ of the *Saprolegnia* into the derma renders it at least possible that the disease may break out in a fresh-run salmon without re-infection.

4. That the cause of the disease, the *Saprolegnia*, may flourish in any fresh water, in the absence of salmon, as a saprophyte upon dead insects and other animals.

5. That the chances of infection for a healthy fish entering a river, are prodigiously increased by the existence of diseased fish in that river, inasmuch as the bulk of *Saprolegnia* on a few diseased fish vastly exceeds that which would exist without them.

6. That as in the case of the potato disease, the careful extirpation of every diseased individual is the treatment theoretically indicated; though, in practice, it may not be worth while to adopt the treatment.

ON THE CONSERVATION OF SOLAR ENERGY[1]

THE question of the maintenance of Solar Energy is one that has been looked upon with deep interest by astronomers and physicists from the time of La Place downwards.

The amount of heat radiated from the sun has been approximately computed by the aid of the pyrheliometer of Pouillet and by the actinometers of Herschel and others at 18,000,000 of heat units from every square foot of its surface per hour, or, put popularly, as equal to the heat that would be produced by the perfect combustion every thirty-six hours of a mass of coal of specific gravity = 1·5 as great as that of our earth.

If the sun were surrounded by a solid sphere of a radius equal to the mean distance of the sun from the earth (95,000,000 of miles), the whole of this prodigious amount of heat would be intercepted; but considering that the earth's apparent diameter as seen from the sun is only seventeen seconds, the earth can intercept only the 2,250-millionth part. Assuming that the other planetary bodies swell the amount of intercepted heat by ten times this amount, there remains the important fact that $\frac{224999999}{225000000}$ of the solar energy is radiated into space, and apparently lost to the solar system, and only $\frac{1}{225000000}$ utilised.

Notwithstanding this enormous loss of heat, solar temperature has not diminished sensibly for centuries, if we neglect the periodic changes, apparently connected with the appearance of sun-spots that have been observed by Lockyer and others, and the question forces itself upon us how this great loss can be sustained without producing an observable diminution of solar temperature even within a human lifetime.

Amongst the ingenious hypotheses intended to account for a continuance of solar heat is that of shrinkage, or gradual reduction of the sun's volume suggested by Helmholtz. It may, however, be urged against this theory that the heat so produced would be liberated throughout its mass, and would have to be brought to the surface by conduction, aided perhaps by convection; but we know of no material of sufficient conductivity to transmit anything approaching the amount of heat lost by radiation.

Chemical action between the constituent parts of the sun has also been suggested; but here again we are met by the difficulty that the products of such combination would ere this have accumulated on the surface, and would have formed a barrier against further action.

These difficulties have led Sir Wm. Thomson, following up Mayer's speculation, to the suggestion that the cause of the maintenance of solar temperature might be found in the circumstance of meteorolites falling upon the sun from great distances in space, or with an acquired velocity due to such fall, and he shows that each pound of matter so imported would represent a large number of heat units depending upon the original distance. Yet the aggregate of material that would thus have to be incorporated with the sun would tend to disturb the planetary equilibrium, and must ere this have shortened our year to an extent exceeding that resulting from astronomical records and observation. In fact, Sir William Thomson soon abandoned the meteoric hypothesis for that of simple transfer of heat from the interior of a liquid sun to the surface by means of convection currents, which latter hypothesis appears at the present time to be supported by Prof. Stokes and other leading physicists of the day.

But if either of these hypotheses could be proved we should only have the satisfaction of knowing that the solar waste of energy by dissipation into space was not dependent entirely upon loss of its sensible heat, but that

[1] Paper read at the Royal Society, March 2, by C. William Siemens, D.C.L., LL.D., F.R.S., Mem. Inst. C.E.

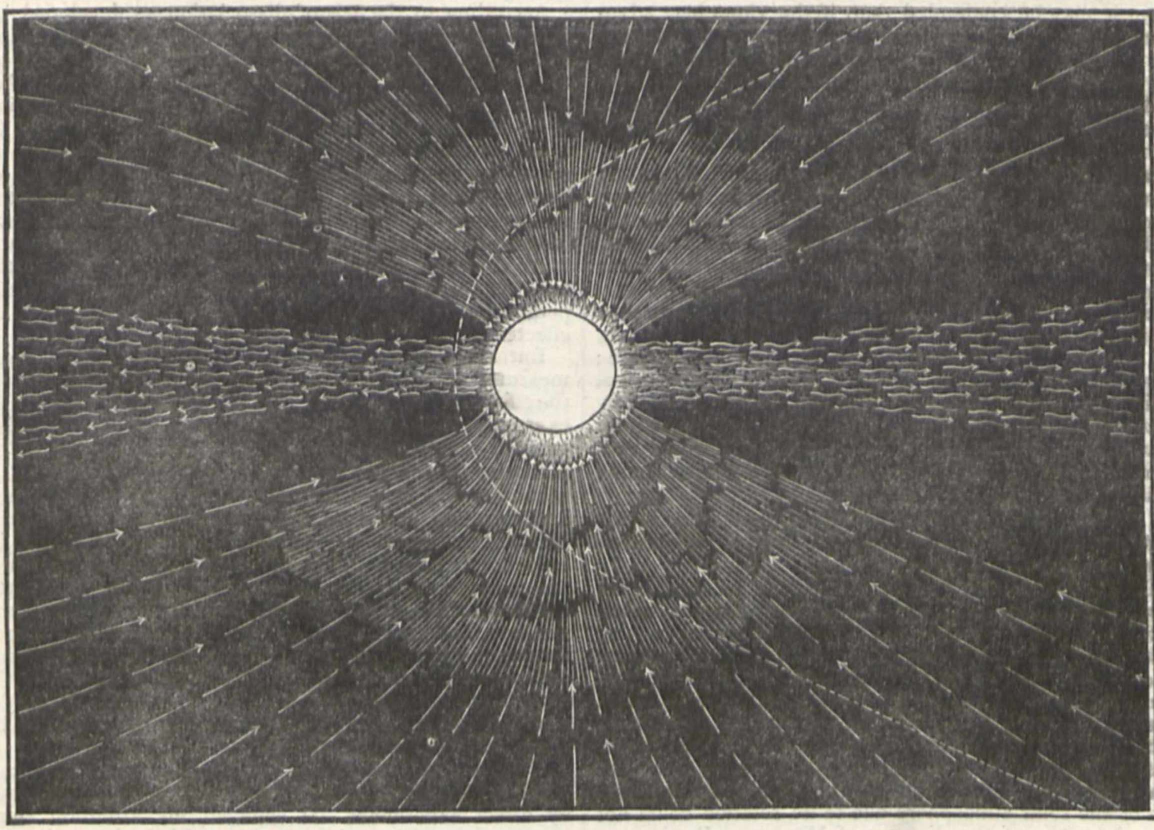
its existence as a luminary would be prolonged by calling into requisition a limited, though may be large, store of energy in the form of separated matter. The true solution of the problem will be furnished by a theory, according to which the radiant energy which is now supposed to be dissipated into space and irrecoverably lost to our solar system, could be arrested and brought back in another form to the sun itself, there to continue the work of solar radiation.

Some years ago it occurred to me that such a solution of the solar problem might not lie beyond the bounds of possibility, and although I cannot claim intimate acquaintance with the intricacies of solar physics, I have watched its progress, and have engaged also in some physical experiments bearing upon the question, all of which have served to strengthen my confidence and ripened in me the determination to submit my views, not without some misgiving, to the touchstone of scientific criticism.

For the purposes of my theory, stellar space is supposed to be filled with highly rarefied gaseous bodies, including hydrogen, oxygen, nitrogen, carbon, and their compounds, besides solid materials in the form of dust. This being the case, each planetary body would attract to itself an atmosphere depending for its density upon its relative attractive importance, and it would not seem unreasonable to suppose that the heavier and less diffusible gases would form the staple of these atmospheres; that, in fact, they would consist mostly of nitrogen, oxygen, and carbonic anhydride, whilst hydrogen and its compounds would predominate in space.

But the planetary system, as a whole, would exercise an attractive influence upon the gaseous matter diffused through space, and would therefore be surrounded by an interplanetary atmosphere, holding an intermediate position between the planetary atmospheres and the extremely rarefied stellar space.

In support of this view it may be urged, that in following out the molecular theory of gases as laid down by



Clerk Maxwell, Clausius, and Thomson, it would be difficult to assign a limit to a gaseous atmosphere in space and, further, that some writers, among whom I will here mention only Grove, Humboldt, Zoellner and Mattieu Williams, have boldly asserted the existence of a space filled with matter, and that Newton himself, as Dr. Sterry Hunt tells us in an interesting paper which has only just reached me, has expressed views in favour of such an assumption. Further than this, we have the facts that meteorolites whose flight through stellar, or at all events through interplanetary space, is suddenly arrested by being brought into collision with our earth, are known to contain as much as six times their own volume of gases taken at atmospheric pressure; and Dr. Flight has only very recently communicated to the Royal Society the analysis of the occluded gases of one of these metorolites taken immediately after the descent to be as follows:—

CO_2	0·12
CO	31·88
H	45·79
CH_4	4·55
N	17·66
	100·00

It appears surprising that there was no aqueous vapour, considering there was much hydrogen and oxygen in combination with carbon, but perhaps the vapour escaped observation, or was expelled to a greater extent than the other gases by external heat when the meteorolite passed through our atmosphere. Opinions concur that the gases found occluded in meteorolites cannot be supposed to have entered into their composition during the very short period of traversing our atmosphere, but if any doubt should exist on this head, it ought to be set at rest by the fact that the gas principally occluded is hydrogen, which is not contained in our atmosphere in any appreciable quantity.

Further proof of the fact that stellar space is filled with gaseous matter is furnished by spectrum analysis, and it appears from recent investigation, by Dr. Huggins and others, that the nucleus of a comet contains very much the same gases found occluded in meteorolites, including "carbon, hydrogen, nitrogen, and probably oxygen," whilst according to the views set forth by Dewar and Liveing it also contains nitrogenous compounds such as cyanogen.

Adversely to the assumption that interplanetary space is filled with gases, it is urged that the presence of ordinary matter would cause sensible retardation of planetary motion, such as must have made itself felt before this; but assuming that the matter filling space is an almost perfect fluid not limited by border surfaces, it can be shown on purely mechanical grounds, that the retardation by friction through such an attenuated medium would be very slight indeed, even at planetary velocities.

But it may be contended that, if the views here advocated regarding the distribution of gases were true, the sun should draw to itself the bulk of the least diffusible, and therefore the heaviest gases, such as carbonic anhydride, carbonic oxide, oxygen and nitrogen, whereas spectrum analysis has proved on the contrary a prevalence of hydrogen.

In explanation of this seeming anomaly, it can be shown in the first place, that the temperature of the sun is so high, that such compound gases as carbonic anhydride and carbonic oxide, could not exist within it, their point of dissociation being very much below the solar temperature; it has been contended, indeed, by Mr. Lockyer, that none of the metalloids have any existence at these temperatures, although, as regards oxygen, Dr. Draper asserts its existence in the solar photosphere; there must be regions, however, outside that thermal limit, where their existence would not be jeopardised by heat, and here great accumulation of these comparatively heavy gases that constitute our atmosphere would probably take place, were it not for a certain counterbalancing action.

I here approach a point of principal importance in my argument, upon the proof of which my further conclusions must depend.

The sun completes one revolution on its axis in 25 days, and its diameter being taken at 882,000 miles, it follows that the tangential velocity amounts to 1·25 miles per second, or to 4·41 times the tangential velocity of our earth. This high rotative velocity of the sun must cause an equatorial rise of the solar atmosphere to which Mairan, in 1731, attributed the appearance of zodiacal light. La Place rejected this explanation on the ground that the zodiacal light extended to a distance from the sun exceeding our own distance, whereas the equatorial rise of the solar atmosphere due to its rotation could not exceed 9-20ths of the distance of Mercury. But it must be remembered that La Place based his calculation upon the hypothesis of an empty stellar space (filled only with an imaginary ether), and that the result of solar rotation would be widely different, if it was supposed to take place within a medium of unbounded extension. In this case pressures would be balanced all round, and the sun would act mechanically upon the floating matter surrounding it in the manner of a fan, drawing it towards itself upon the solar surfaces, and projecting it outwards in a continuous disk-like steam.

By this fan action, hydrogen, hydrocarbons, and oxygen, are supposed to be drawn in enormous quantities toward the polar surfaces of the sun; during their gradual approach, they will pass from their condition of extreme attenuation and extreme cold, to that of compression, accompanied with rise of temperature, until on approaching the photosphere, they burst into flame, giving rise to a great development of heat, and a temperature commensurate with their point of dissociation at the solar density. The result of their combustion will be aqueous vapour and carbonic anhydride or oxide, according to the sufficiency or insufficiency of oxygen present to complete the combustion, and these products of combustion in yielding to the influence of centrifugal force will flow towards the solar equator, and be thence projected into space.

The next question for consideration is: What would become of these products of combustion when thus rendered back into space? Apparently they would gradually change the condition of stellar material, rendering it more and more neutral, but I venture to suggest the possibility, nay, the probability, that solar radiation would, under these circumstances, step in to bring back the combined materials to a condition of separation by a process of dissociation carried into effect at the expense of that solar energy which is now supposed to be lost to our planetary system.

According to the law of dissociation as developed by Bunsen and Sainte-Claire Deville, the point of dissociation of different compounds depends upon the temperature on the one hand, and upon the pressure on the other. According to Sainte-Claire Deville, the dissociation tension of aqueous vapour of atmospheric pressure and at 2800° C. is 0.5, or only half of the vapour can exist as such, its remaining half being found as a mechanical mixture of hydrogen and oxygen, but that with the pressure, the temperature of dissociation rises and falls, as the temrerature of saturated steam rises and falls with its pressure. It is therefore conceivable that the temperature of the solar photosphere may be raised by combustion to a temperature exceeding 2800° C., whereas dissociation may be effected in space at a lower temperature.

But these investigations had reference only to heats measured by means of pyrometers, but do not extend to the effects of radiant heat. Dr. Tyndall has shown by his exhaustive researches that vapour of water and other gaseous compounds intercept radiant heat in a most remarkable degree, and there is other evidence to show that radiant energy from a scource of high intensity possesses a dissociating power far surpassing the measurable temperature to which the compound substance under its influence is raised. Thus carbonic anhydride and water are dissociated in the leaf cells of plants, under the influence of the direct solar ray at ordinary summer temperature, and experiments in which I have been engaged for nearly three years[1] go to prove that this dissociating action is obtained also under the radiant influence of the electric arc, although it is scarcely perceptible if the source of radiant energy is such as can be produced by the combustion of oil or gas.

The point of dissociation of aqueous vapour and carbonic anhydride admits, however, of being determined by direct experiment. It engaged my attention some years ago, but I have hesitated to publish the qualitative results I then obtained, in the hope of attaining to quantitative proofs.

These experiments consisted in the employment of glass tubes, furnished with platinum electrodes, and filled with aqueous vapour or with carbonic anhydride in the usual manner, the latter being furnished with caustic soda to regulate the vapour pressure by heating. Upon immersing one end of the tube charged with aqueous vapour in a refrigerating mixture of ice and chloride of calcium, its temperature at that end was reduced to 32° C., corresponding to a vapour pressure, according to Regnault, of 1-1800 of an atmosphere. When so cooled no slow electric discharge took place on connecting the two electrodes with a small induction coil. I then exposed the

[1] See Proceedings, Roy. Soc. Vol. xxx. 1 Mar. 1880 and a paper read before Section A of the British Association 1 Sep. 1881 and ordered to be printed in the Report.

end of the tube projecting out of the freezing mixture, backed by white paper, to solar radiation (on a clear summer's day) for several hours, when upon again connecting up to the inductorium, a discharge, apparently that of a hydrogen vacuum, was obtained. This experiment being repeated furnished unmistakable evidence, I thought, that aqueous vapour had been dissociated by exposure to solar radiation. The CO_2 tubes gave, however, less reliable results. Not satisfied with these qualitative results, I made arrangements to collect the permanent gases so produced by means of a Sprengel pump, but was prevented by lack of time from pursuing the inquiry, which I purpose, however, to resume shortly, being of opinion that, independently of my present speculation, the experiments may prove useful in extending our knowledge regarding the laws of dissociation.

Assuming, for my present purpose, that dissociation of aqueous vapour was really effected in the experiment just described, and assuming, further, that stellar space is filled with aqueous and other vapour of a density not exceeding the 1-2000th part of our atmosphere, it seems reasonable to suppose that its dissociation would be effected by solar radiation, and that solar energy would thus be utilised. The presence of carbonic anhydride and carbonic oxide would only serve to facilitate the decomposition of the aqueous vapour by furnishing substances to combine with nascent oxygen and hydrogen. By means of the fan-like action resulting from the rotation of the sun, the vapour dissociated in space to-day would be drawn towards the polar surfaces of the sun to-morrow, be heated by increase in density, and would burst into flame at a point where both their density and temperature had reached the necessary elevation to induce combustion, each complete cycle taking, however, years to be accomplished. The resulting aqueous vapour, carbonic anhydride and carbonic oxide, would be drawn towards the equatorial regions, and be then agaln projected into space by centrifugal force.

Space would, according to these views, be filled with gaseous compounds in process of decomposition by solar radiant energy, and the existence of these gases would furnish an explanation of the solar absorption spectrum, in which the lines of some of the substances may be entirely neutralised and lost to observation. As regards the heavy metallic vapours revealed in the sun by the spectroscope, it is assumed that these form a lower and denser solar atmosphere, not participating in the fan-like action which is supposed to effect the light outer atmosphere only, in which hydrogen is the principal factor.

Such a dense metallic atmosphere could not participate in the fan action affecting the lighter photosphere, because this is only feasible on the supposition that the density of the in-flowing current is, at equal distances from the gravitating centre, equal or nearly equal to the outflowing current. It is true that the products of combustion of hydrogen and carbonic oxide are denser than their constituents, but this difference may be balanced by their superior temperature on leaving the sun, whereas the metallic vapours would be unbalanced, and would therefore obey the laws of gravitation, recalling them to the sun. On the surface of contact between the two solar atmospheres, intermixture induced by friction, must take place, however, giving rise perhaps to those vortices and explosive effects which are revealed to us by the telescope, and have been commented on by Sir John Herschel and other astronomers. Some of the denser vapours would probably get intermixed and carried away mechanically by the lighter gases, and give rise to that cosmic dust which is observed to fall upon our earth in not inappreciable quantities. Excessive intermixture would be prevented by the intermediary neutral atmosphere, the penumbra.

As the whole solar system moves through space at a pace estimated at 150,000,000 of miles annually (being about one-fourth of the velocity of the earth in its orbit), it appears possible that the condition of the gaseous fuel supplying the sun may vary according to its state of previous decomposition, in which other heavenly bodies may have taken part. May it not be owing to such differences in the quality of the fuel supplied that the observed variations of the solar heat may depend? and may it not be in consequence of such changes in the thermal condition of the photosphere that sun-spots are formed?

The views here advocated could not be thought acceptable unless they furnished at any rate a consistent explanation of the still somewhat mysterious phenomena of the zodiacal light and of comets. Regarding the former, we should be able to return to Mairan's views, the objection by La Place being met by a continuous outward flow from the solar equator. Luminosity would be attributable to particles of dust emitting light reflected from the sun, or by phosphorescence. But there is another cause for luminosity of these particles, which may deserve a passing consideration. Each particle would be electrified by gaseous friction in its acceleration, and its electric tension would be vastly increased in its forcible removal, in the same way as the fine dust of the desert has been observed by Werner Siemens to be in a state of high electrification on the apex of the Cheops Pyramid. Would not the zodiacal light also find explanation by slow electric discharge backward from the dust towards the sun? and would the same cause not account for a great difference of potential between the sun and earth, which latter may be supposed to be washed by the solar radial current? May not the presence of the current also furnish us with an explanation of the fact that hydrogen, while abounding apparently in space, is practically absent in our atmosphere, where aqueous vapour, which may be partly derived from the sun, takes its place? An action analogous to this, though on a much smaller scale, may be set up also by terrestrial rotation giving rise to an electrical discharge from the outgoing equatorial stream to the polar regions, where the atmosphere to be pierced by the return flood is of least resistance.

It is also important to show how the phenomena of comets could be harmonised with the views here advocated, and I venture to hope that these occasional visitors will serve to furnish us with positive evidence in my favour. Astronomical physicists tell us that the nucleus of a comet consists of an aggregation of stones similar to meteoric stones. Adopting this view, and assuming that the stones have absorbed in stellar space gases to the amount of six times their volume, taken at atmospheric pressure, what, it may be asked, will be the effect of such a mass of stone advancing towards the sun at a velocity reaching in perihelion the prodigious rate of 366 miles per second (as observed in the comet of 1845), being twenty-three times our orbital rate of motion? It appears evident that the entry of such a divided mass into a comparatively dense atmosphere must be accompanied by a rise of temperature by frictional resistance, aided by attractive condensation. At a certain point the increase of temperature must cause ignition, and the heat thus produced must drive out the occluded gases, which in an atmosphere 3000 times less dense than that of our earth would produce $6 \times 3000 = 18{,}000$ times the volume of the stones themselves. These gases would issue forth in all directions, but would remain unobserved except in that of motion, in which they would meet the interplanetary atmosphere with the compound velocity and form a zone of intense combustion, such as Dr. Huggins has lately observed to surround the one side of nucleus, evidently the side of forward motion. The nucleus would thus emit original light, whereas the tail may be supposed to consist of stellar dust rendered luminous by reflex action produced by the light of the sun and comet combined, as foreshadowed already by Tyndall, Tait, and others, starting each from different assumptions.

These are in brief the outlines of my reflections regarding this most fascinating question, which I venture to put before the Royal Society. Although I cannot pretend to an intimate acquaintance with the more intricate phenomena of solar physics, I have long had a conviction derived principally from familiarity with some of the terrestrial effects of heat, that the prodigious and seemingly wanton dissipation of solar heat is unnecessary to satisfy accepted principles regarding the conservation of energy, but that it may be arrested and returned over and over again to the sun, in a manner somewhat analogous to the action of the heat recuperator in the regenerative gas furnace. The fundamental conditions are :—

1. That aqueous vapour and carbon compounds are present in stellar or interplanetary space.

2. That these gaseous compounds are capable of being dissociated by radiant solar energy while in a state of extreme attenuation.

3. That these dissociated vapours are capable of being compressed into the solar photosphere by a process of interchange with an equal amount of reassociated vapours, this interchange being effected by the centrifugal action of the sun itself.

If these conditions could be substantiated, we should gain the satisfaction that our solar system would no longer impress us with the idea of prodigious waste through dissipation of energy into space, but rather with that of well-ordered self-sustaining action, capable of perpetuating solar radiation to the remotest future.

FURTHER OBSERVATIONS ON THE FRESH-WATER MEDUSA, MADE DURING THE SUMMER, 1881

1. THE Freshwater Medusa—*Limnocodium Sowerbii*—reappeared in the lily-house tank in the Botanical Gardens, Regent's Park, during the summer of 1881, as the readers of NATURE were duly informed.

In spite of the renewed opportunities for study thus afforded, the life-history of this interesting organism still remains a mystery, and it is still exceedingly difficult to frame any hypothesis as to the original introduction of the jelly-fish into the tank where it was discovered by Mr. Sowerby in 1880.

The only general hypothesis which can be entertained as to the original introduction of the jelly-fish, is that it came "in some way at some time" with plants deposited in the tank.

It is *improbable* that the jelly-fish can have existed for many seasons in the tank unobserved, though *possible*, supposing that it first appeared in small numbers.

The last importation of an aquatic plant into the lily-house in Regent's Park, previous to the discovery of the jelly-fish in June, 1880, is that to which suspicion naturally attaches itself. This importation occurred early in March, 1879, when, as Mr. Sowerby kindly informs me, a Miss Tupper, whose address is not in his possession, presented to the Royal Botanical Society a specimen of a species of *Pontederia*. This specimen was wrapped in a piece of brown paper, was about one foot long, was crushed and as dry as hay, in fact the Garden superintendent and the man in charge of the tank thought it dead. The specimen is believed to have come from Brazil. It vegetated after being placed in the tank, and has given rise to a copious growth, part of which is now in the lily-house of the Botanic Garden at Oxford.

Mr. Sowerby cannot remember the introduction of any new plants into the tank at such time previous to this as would render it probable that the jelly-fish were introduced on such previous occasions.

It is clear, then, that if the jelly-fish were introduced with the *Pontederia*, either the animal itself or its eggs must have great power of resistance to partial desiccation. Of this power of resistance we have no further evidence, for the tank in the Lily-house is not completely emptied and dried in the winter, though the water is run off, a deep trough of water and mud remains permanently at one end of the tank.

At the same time it is in accordance with what is known as to many lacustrine animals that the eggs or young stages of the fresh-water jelly-fish should be able to resist partial desiccation. Hence the theory of its introduction with the *Pontederia* is, though far from demonstrated, yet quite tenable. Plants of this *Pontederia* were sent from Regent's Park to Kew and Oxford (where they are flourishing) some months before the discovery of the jelly-fish in June, 1880. But no jelly-fish made their appearance in consequence (so far as is known) in the tanks at Kew and Oxford. Hence the association with the *Pontederia* of the eggs or young of the jelly-fish cannot have been a very intimate one.

2. The history of the jelly-fish in the Regent's Park tank is as follows :—It was first seen by Mr. Sowerby on June 10, 1880. At that time there were some specimens nearly full-grown and a vast number of very young ones (apparently recently hatched) also. By the end of July not a specimen could be found in the tank. All the mature specimens examined by me in 1880 were males, numbering 150. I entirely failed to obtain any specimen which was female, either young or mature. Nevertheless Mr. Sowerby was of opinion that young were produced by adult individuals isolated and kept by him in a small glass jar. These young were those which I reported on in the *Quart. Journ. Micr. Sci.*, January, 1881. I could find only adult males in the jar with them, and think that it is possible (though not certain))that the young were hatched from eggs floating in the water when first introduced into the jar. They would thus be only *late-hatching* members of the same brood of which adults were discovered on June 10. It is, on the other hand, possible that they belonged to a second generation.

The males observed in 1880 discharged abundant motile spermatozoa from their genital sacs and were obviously ready for procreation.

Thus in 1880 we were left in ignorance of the female of *Limnocodium*, and in nearly complete ignorance as to the period and mode of reproduction.

3. In 1881 Mr. Sowerby observed the Medusæ again, only two days after the anniversary of their first appearance, namely, on June 16. He states, in a letter kindly written for me, that only a "few were seen, although the water appeared swarming with minute individuals just large enough to be distinctly seen with the naked eye. Many of these we determined by examination with a glass; they did not, however, appear to come to manhood, and about the 25th of June the whole of the Medusæ vanished."

It is obvious that some process of reproduction had taken place between June, 1880, and June, 1881, giving rise to the Medusæ observed in 1881. Where were the females which produced the eggs from which this new generation was born? As in 1880, so in 1881, when first observed in June, *both minute young apparently just hatched*, and also *full-grown individuals were simultaneously detected.* In 1881 I examined about fifty of the full-grown individuals from the Regent's Park : as in 1880 they were all males. It seems probable that the adults observed on June 10 were merely early-hatched members of the same brood as the young (of various ages and sizes) which abounded with them.

From the experience of these two years it appears probable that the first specimens which hatch out must do so six weeks or two months before the middle of June. But as to the character of the eggs from which they hatch, we have as yet no idea. It would seem likely that those eggs were deposited before the emptying of the tank in December, and probably enough in the summer before the dying down of the males, so abundant until their total disappearance at the end of July.

But since the females have never been seen, it is possible that they have a slightly different form and habit from that of the males, or possibly a *very* different form —perhaps hydroid! In either case the females may have existed in the deeper parts of the tank either floating or attached, and so have escaped observation, whilst the high-swimming males were taken in abundance. The eggs may have been fertilised in August by the dying males, and have proceeded to a certain stage of development in the autumn—being then normally arrested in development in the winter (the period corresponding to the emptying of the tank and to a period of drought in their natural condition), as in the case of *Hydra viridis*. Then they would be ready to enter upon a new period of development and growth in the following spring and early summer.

4. The above suggestion is eminently hypothetical, and is somewhat difficult to reconcile with the result of the interesting experiment carried out at the suggestion of Mr. George Busk, F.R.S., in consequence of which some of the Medusæ were transferred in 1881 from the Regent's Park to the Victoria tank, in No. 10 house, at the Royal Gardens, Kew. This transfer was effected on June 16, four days after the second appearance of the Medusæ at Regent's Park. It is important to notice (as I am informed by Mr. Sowerby) that only six or eight full-grown Medusæ were so transferred—and I cannot feel much doubt that these were all males—similar to all those wh ch I have examined. But with these half-dozen full-grown specimens, a quart of water containing many hundreds of very minute Medusæ was also taken. The flask of water and the little and big Medusæ were poured out into the large tank at Kew on the same day—June 16.

Nothing was seen of them at Kew until *more than two months afterwards*, when (on August 18) the whole tank was seen to be swarming with full-grown *Limnocodium*.

The question which arises is: Were these Medusæ s mply the young Medusæ which had been transferred, now grown to maturity; or were they a new generation?

The chief objection to the view that the Medusæ swarming at Kew in August were the same Medusæ which were transferred in the young state from Regent's Park two months before, is found in the fact of their extraordinary abundance. I removed and examined myself at the end of September, from the tank at Kew, 200 specimens. Other naturalists also obtained numerous specimens. In the meantime, be it remembered, the whole colony had *died down or disappeared* (as early as June 25) *from the original tank in the Regent's Park!*

I am inclined to the view that the Kew Medusæ were actually the same specimens as those transferred in the young state, which were placed in more favourable conditions at Kew than their fellows experienced in the Regent's Park. The tank at Kew is larger than that in Regent's Park, food is therefore more abundant, and moreover the temperature was, when I observed it, from six to ten degrees Fahrenheit lower in the tank at the former than in that at the latter locality (75°–80° Kew, as against 85°–90° Regent's Park).

A fact which is strongly against the supposition that the transferred Medusæ had reproduced themselves is that the half-dozen mature specimens transferred were almost certainly males, and that the young specimens had not more than time to grow to full size, and were not observed to have arrived at maturity in the interval.

I made renewed and careful examination of the Medusæ at Kew at the end of September and in the beginning of October, when I had the advantage, through the kindness of Sir Joseph Hooker, of making use of the admirable laboratory recently erected in the Gardens. The specimens were often appreciably larger than any I had previously obtained from Regent's Park (fully half an inch across the expanded disc). All the specimens examined (200 in number) proved to be males. At the end of September there were no young or very small specimens in the Kew tank. In studying the genital pouches of specimens taken on September 27 I found below the ectoderm an abundance of ripe spermatozoa; these escaped through the wall of the pouch, which very readily ruptured. Below this layer of ripe spermatozoa, and between them and the "structureless lamella" separating endoderm and ectoderm, I observed (as I had observed previously in specimens from Regent's Park) a firm colourless tissue consisting of small nucleated cells. It occurred to me that these might possibly be ova, and the Medusæ accordingly hermaphrodite. They had not the appearance of ova at this stage, but still I thought it possible that they were very young ovarian cells. I treated specimens with osmic acid, alcohol, and picrocarmine in succession, and cut sections of the genital pouches, with the result of satisfying myself that this dense tissue beneath the loose spermatozoa was *not* ovarian, but consisted simply of the mother-cells of the spermatozoa.

I further tested this view of their nature by isolating a number of the Medusæ in large glass jars which were kept in the lily-house at Kew. After a fortnight (October 8) the gonads or genital pouches had increased in length and bulk both in my isolated specimens, and in those swimming in the tank. On examination, the dense tissue underlying the spermatozoa was found to have disappeared, or rather to have developed itself into additional crops of spermatozoids. Accordingly my hypothesis of hermaphroditism fell to the ground.

It is, however, remarkable that even when half-grown the genital pouches of *Limnocodium* will emit ripe motile spermatozoids upon slight pressure, and that they continue to form these bodies for so long a period of growth. Normally, I am inclined to believe, these spermatozoa are shed by rupture of the sac in incompletely grown individuals, from time to time, whilst new crops are produced from the as yet unexhausted spermatic tissue. This would imply that somewhere in the tank there are eggs or females producing eggs which are to be fertilised by the very abundant spermatic particles.

The tank at Kew was emptied and thoroughly cleaned out about the middle of October. The Medusæ were still to be found, though they had much diminished in abundance. Some of the sweepings of the bottom of the tank were preserved with the intention of replacing the material in the tank, so that the eggs of the Medusæ—if eggs there are—may have a chance of continuing the colony in the coming season.

My object in publishing these notes is that they may be of service to others who may feel disposed to investigate *Limnocodium*, and to search for the females should opportunity again be afforded. I also hope that some suggestions may be offered by other naturalists, which will be of assistance in solving the problems presented.

It is necessary to point out that the obvious plan of searching the sediment of either the tank at Kew or in the Regent's Park for eggs is not feasible. The bulk of the material to be examined is too great, since these tanks have a square area of several hundred feet. There would be a better chance of finding the females (supposing them to be deep-living or sessile) in such a search than the eggs, were it possible to empty the tanks and get at the sediment when a suitable period for such search arrives. But, as a matter of course, the proper treatment of these tanks in connection with the cultivation of plants cannot be interfered with. An obvious suggestion is that of isolating a number of both old and young Medusæ in small tanks, and thus obtaining the means of knowing exactly what becomes of them and of anything they may produce. I have attempted this both in 1880 and 1881, but without any success. The Medusæ isolated, even in large bell-jars holding 5 or 6 gallons of water, and maintained at a temperature of 80° F., do not thrive.

After a time they die. Their ill-health under these circumstances is apparently due not merely to the want of food, since with sufficient trouble the small Entomostraca on which they feed can be supplied to them, but to the very fact of isolation in a small receptacle. They require a large bulk of water. Fluviatile organisms can be kept in a small vessel by means of a constant stream passed through the vessel, and organisms which inhabit small ponds present no difficulty. But lacustrine forms are very difficult to deal with. Should the Medusæ reappear this year, it is my intention (with the permission of the authorities) to partially submerge a vessel with freely perforated sides in the large tank, the bottom of such vessel to be imperforate, and the vessel itself two feet in diameter and three feet in depth. If a sufficient number of the first brood of young Medusæ can be cultivated in this vessel through the summer, both males and females (unless the females have some altogether unsuspected history) will in all probability arrive at maturity, and reproduce in it as they clearly enough have done in the Regent's Park tank between 1880 and 1881. It will then be possible from time to time to examine carefully the contents of this experimental vessel. I need not say that I should be very glad if others would carry out a similar experiment.

E. RAY LANKESTER

ELECTRICITY AT THE CRYSTAL PALACE

II.—*Edison's Electric Light*

THE centre of attraction at the exhibition of electricity in the Crystal Palace, formally opened on Saturday by the Duke and Duchess of Edinburgh, will unquestionably be the show of Mr. Edison. His electric light in the Entertainment Court and the Concert Room is by far the finest ever yet made, and is of itself a spectacle to be remembered. No expense has been spared to demonstrate the power and beauty of his incandescent lamps, and the divisibility of the current to meet the wants of domestic lighting; while Messrs. Verity and Sons have seized the occasion to illustrate their skill and show how eminently adapted the electric light is for ornamental purposes. The heated filament of carbon inclosed in a vacuous bulb of glass is well fitted for all kinds of domestic illumination by reason of its pure and absolutely steady glow, its healthiness and freedom from noxious fumes, and its comparative coolness. But in addition to its superiority over gas, oil, and candles in these respects, the Edison exhibit also proves in the most striking manner its superiority as a decorative light, and its unrivalled capacities for enhancing the artistic pleasures of our homes. Besides giving off no deleterious gases to tarnish gilding or dim the most delicate colours, the incandescent lamp lends itself to the designer's fancies in a way which no other illuminant can; and we may expect something like a revolution in household decoration by its introduction, as well as a new development of the brass-worker and the glass-blower's art.

Before considering the apparatus employed by Mr. Edison at the Crystal Palace for the production and distribution of the light, we shall briefly describe the results. To begin with the Entertainment Court, which is in reality a small theatre, the principal object of interest is a magnificent chandelier suspended from the middle of the ceiling. This beautiful object is in itself a work of art, and sustains ninety-nine incandescent lamps. It is conical in general shape, and is about fifteen feet in height by ten feet in diameter at the lower end; while its weight is half a ton. In device it represents a tapering bouquet of flowers rising out of a golden basket. The stem of each flower springs from a circular brass plate within the basket, and bends over towards the spectator, presenting to him its calyx of coloured glass, in which is fixed an incandescent lamp. The foliage is all of hammered brass, richly gilt, and here and there is mingled with the sun-flower or tiger-lily, and some rambling sprays of fern. The corollas of the flowers containing the lamps, and acting as their shades, are in the form of heaths and harebells, made of glass, and tinted with a variety of colours—pearl, white, ruby, clear olive, and clouded blue. Each lamp projects from the heart of the flower like an enlarged pistil, and throws its light outwards and downwards into the room below. The lights are controlled in three sections by turncocks, like gas, and thus a graduated effect can be obtained, or all the lights may be put on or off at will.

On each side of the stage, which is furnished with a row of twenty-four footlights, there is a pretty candelabra[1] mounted on a short marble column, and representing a rose-bush springing from a golden urn. The stem of the bush is entwined with China roses, and crowned with five upright lamps or candles, like the fruit of the tree. On the left of the stage is hung an exquisite little chandelier or lustre of Venetian glass, which, though far less imposing than its gaudier neighbour in the centre of the hall, is chaster and more elegant, and better fitted for an ordinary drawing-room. It is about four feet high, and consists of loops and festoons of crystal drops on gilded chains, encircled at the bottom by a ring of fourteen lamps; and inclosing higher up a single incandescent bulb of ruby glass under a bell shade of the same material. The use of coloured glass for the vacuous bulb itself is illustrated here, and shows how the light can be tinted to harmonise with any interior furnishing, or suit the taste and eyesight of individuals. The brilliance of the glowing carbon in a transparent bulb is not too strong for the ordinary eye to look at with impunity; but persons of weak sight may have it reduced by the use of clouded bulbs, and students, or those suffering from diseases of the eye can employ bulbs of green or blue glass. Photographers, too, can have recourse to ruby lamps in the development of their negatives.

On the right side of the stage there is a third chandelier of gilt brass, with twelve naked bulbs, a number of single lamps on stands or movable brackets, like gas-jets, with turn-cocks, and either naked or shaded by flat conical reflectors of opal glass. Specimens of these are shown in Figs. 1 and 2. Then there are hall-lanterns of brass, inclosing clusters of bulbs, window-lights, a very handsome billiard lamp, containing six set of twin lamps, shaded from the eyes of the players by opal glass reflectors and crimson fringes, and two handsome drawing-room shade-lamps of the same pattern, each containing a cluster of eight bulbs inside, and one being supplemented by four pairs of naked bulbs outside. Bulbs are also shown burning under water, either clear or tinted, to illustrate the use of the incandescent system in fiery mines, and there is a specimen of a regulator lamp by which the power of the jet can be graduated at will like a gas-flame, by simply turning the cock. This lamp is shown in Fig. 3, the lower being the regulator, which acts by inserting the resistance of a series of vertical carbon rods into the circuit. This is done by turning the screw-piece at the base of the cylinder inclosing the bars. The cylinder is perforated to allow the air to circulate and keep them cool.

In the top of the lamp the novelty is the form of the contact surfaces to prevent sparking or breaking the circuit. These are conical, the small cone seen on the top of the figure being forced away from a conical cup on turning the screw plug. The large surfaces of the cones prevent simultaneously separating, and prevent a large spark. A safe-guard for the lamp against a too powerful current is provided in a short lead wire, seen running across the left of the figure. When the current is too strong this wire fuses, and the current of the lamp is interrupted.

[1] Electrolier and electrolabra would be the corresponding terms.

In addition to these lamps Mr. Edison also exhibits some very handsome sconce mirrors supplied by Messrs. Verity and Sons. One of these is a novelty in its way, since the bulb lighting it is inside the frame, and therefore out of sight. The interior of the frame is, however, whitened, and reflects the light out through narrow panes of clouded glass which flank the central mirror, and the face of the spectator thus illuminated can be seen in the mirror. This is another effect which could not be produced by gas. The other sconces are lit by naked bulbs, supported by in front of the mirrors, curving brass brackets in which the ruling idea of foliage and flower or fruit is elegantly worked out.

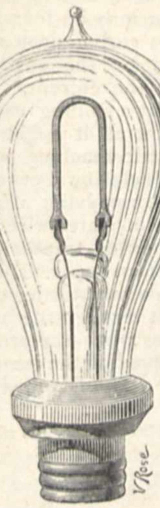


FIG. 1.

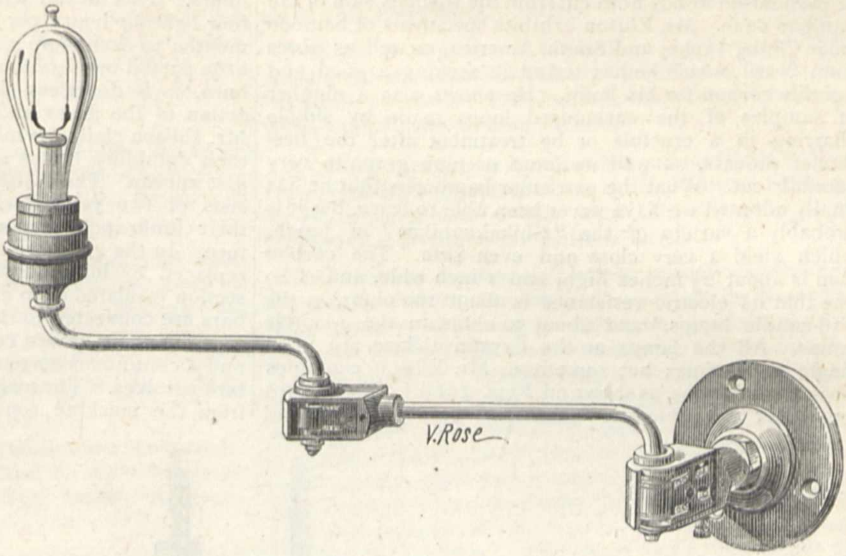


FIG. 2.

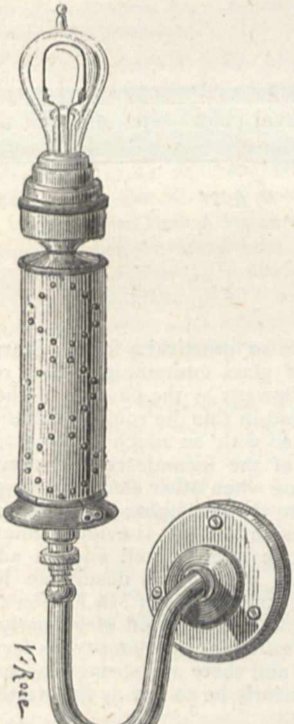


FIG. 3.

In the Concert Room, opposite the Entertainment Court, there are some 280 lamps, about forty of which are termed "half-lights," that is, giving 8 candle-power, or a light one-half of the full 16 candle-lights. Of these 120 are hung in festoons between the pillars of the galleries, the rest being suspended in sets of four under the galleries, or fixed within a large crystal lustre suspended from the roof, and looking like a nest of diamonds. In the Entertainment Court and Concert Room together there are nearly 500 lights, and the stalls in the wide avenue leading to the railway station, have yet to be lighted. In all there will be about 700 lamps required when the exhibit is complete. To drive the 500 lamps now going there are eight dynamo-electric machines at work, and four more are being got ready for the remaining 200 lights. Three Robey engines of 25 horse-power nominal are planted to work these machines, one engine to every four machines. It is usual to allow ten lamps to each horse-power, but what the actual power consumed may be is difficult to state.

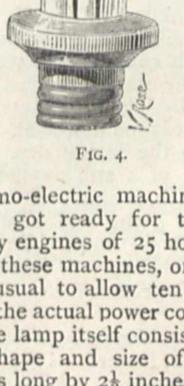


FIG. 4.

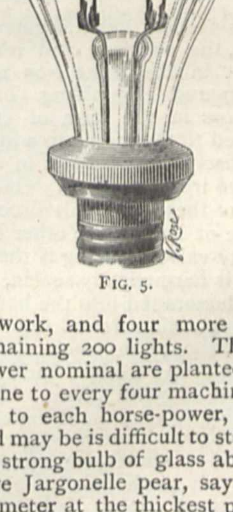


FIG. 5.

The lamp itself consists of a strong bulb of glass about the shape and size of a large Jargonelle pear, say $4\frac{1}{2}$ inches long by $2\frac{1}{2}$ inches in diameter at the thickest part. From the narrow end a tube of glass projects nearly half-way into the bulb, and contains [the ends of the copper conducting wires or electrodes. The inner end of this tube is closed by a flat keel of solid glass, but the wires pass through this into the upper part of the bulb, where they are connected by an electrotype of copper to a fine loop or arch of carbonised woody fibre cut from the silicious skin of the bamboo cane. Mr. Edison exhibits specimens of bamboo from China, Japan, and South America, as well as fibres from Brazil, which he has tested in seeking a good and durable carbon for his lamp. He shows also a number of samples of the carbonised loops made by simple charring in a crucible or by treatment after the Berthollet process, as well as loops of pure graphite very carefully cut. What the particular bamboo is that he has finally adopted we have never been able to learn, but it is probably a variety of the "Shikakuahikee" of Japan, which yield a very close and even skin. The carbon loop is about 2½ inches high, and 1 inch wide, and is so fine that its electric resistance is about 100 ohms. in the "16-candle lamps," and about 50 ohms. in the 8-candle lamps. All the lamps at the Crystal Palace are plain single loop lamps; but sometimes Mr. Edison combines two or more loops, as shown in Figs. 4 and 5. These loops can either be coupled up "in series," or "quantity," and instead of making the loops plain they may be curled into a spiral form. The air being exhausted from the bulb there is no oxidation of the carbon after a short time and Mr. Edison claims that his lamps will last 1000 hours. This at an average rate of between three and four lighting hours per night would give a life of nine months to each lamp; but the estimate may be found a little partial in practice: for though the carbon does not burn, it is doubtless slowly dissipated by the wasting action of the gases and the energy of the current. As Mr. Edison claims to make the lamps at a shilling each, their durability is not so very important as it might at first appear. The Edison dynamo electric machine consists of two vertical electromagnets inclosing between their lower pole pieces of soft iron, a revolving armature. In the armature the usual coils of insulated wire are replaced by longitudinal bars of copper of trapezoidal section insulated from each other by brown paper. These bars are connected to the slips of the commutator in such a manner as to give a continuous circuit through the bars and a continuous current to the brushes when the armature revolves. The main conductor conveying the current from the machine consists of a solid rod of copper in

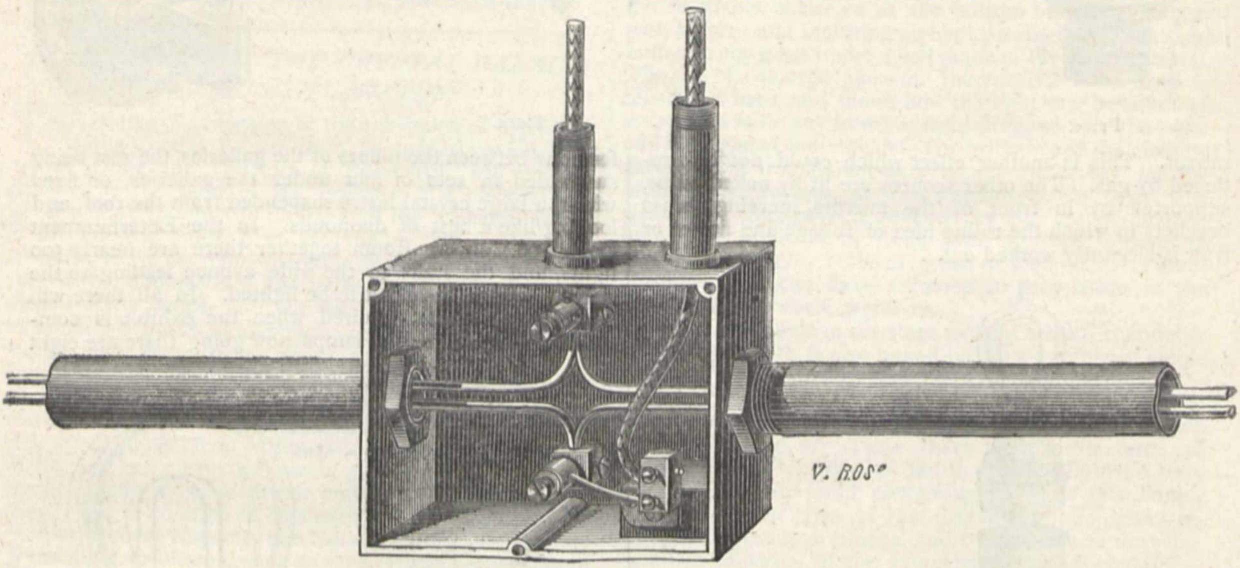


Fig. 6.

cross-section, like a segment of a circle. Two of these rods, the outgoing and return wire, are inclosed a little apart in the same iron pipe and insulated by a black compound resembling Thomson's wax. Branch-conductors in the form of cables for side-streets are connected to the mains in a joint-box shown in Fig. 6. This consists of an iron box in which the mains are connected to two iron terminals. One branch cable is connected to one of these terminals direct, and the other through a short piece of lead to the other terminal. The lead acts as a safety-valve in fusing if the current is too powerful. The box is hermetically sealed, to keep the inside dry. The conductors led into the houses are of a still smaller size, having a diameter of from two to three millimetres; but throughout the whole system the going and returning wires keep together, and the lamps are simply connected across between them. In each lamp, too, there is a similar safety connection of lead to protect the carbon if the current is too strong.

The incandescent system has evidently been brought to great perfection by Mr. Edison, backed as he is by plenty of capital and skilled assistance. Although the idea of it is not new, and was patented in England by Mr. Starr in 1845, Mr. Edison deserves great credit for working it out in so practical a form. Starr described a vacuous bulb of glass containing a thin rod of carbon rendered incandescent by the passage of the current, and Mr. Edison found in this the rough pebble which he has cut and polished with so much success. Moreover, he saw the merits of the incandescent system for domestic lighting at a time when other electricians were giving all their attention to the arc light; and therein showed his genius and foresight. For it is evident now to electricians that while the arc light is well enough adapted for the lighting of large areas, it is unsuitable for small interiors. The practical success of Mr. Edison's system is not thus far a complete justification of his early promises, for the cost is still an unknown quantity, as far as the public are concerned, and there are strong reasons for believing that it will not nearly be so low as the startling figure held out in 1878.

NOTES

At the annual meeting of the Geological Society the medals were presented as follows:—The Wollaston Gold Medal to Dr. Franz Ritter von Hauer, Director of the Austrian Geological Survey; the Murchison Medal to Prof. Jules Gosselet, of Lille;

the Lyell Medal to Dr. John Lycett, of Scarborough; the balance of the Wollaston Fund to Dr. G. J. Hinde; the balance of the Murchison Fund to Prof. T. Rupert Jones; a moiety of the Lyell Fund to Prof. Charles Lapworth, Mason College, Birmingham, and to the Rev. Norman Glass; a portion of the proceeds of the Barlow-Jameson Fund to Baron Constantin von Ettingshausen, Professor of Botany at Graz. Mr. J. W. Hulke, F.R.S., was elected President, in succession to Mr. Etheridge.

THE Rev. Thomas Romney Robinson, D.D., died on Tuesday, after a short illness, at his residence, The Observatory, Armagh, at the patriarchal age of eighty-nine years. He retained his mental faculties in surprising activity and vigour to the last.

M. DESOR, one of the last companions of Agassiz in his great Alpine excursions, which led to the discovery of the theory of glaciers, has just died in Neufchatel. M. Desor, although born in Germany, was of French extraction, and had been a naturalised Swiss citizen, and became the president of the National Council. He bequeathed all his fortune to the city for scientific purposes.

DR. W. R. HODGKINSON, Senior Demonstrator at the Royal College of Chemistry, has been appointed to the Professorship of Chemistry and Physics at the Royal Military Academy, Woolwich.

PROFESSORS ROSCOE AND ABEL, as presidents respectively of the Chemical Society and Institute of Chemistry, will hold a reception on the 22nd inst. at the Crystal Palace in connection with the Electric Exhibition.

THE Sanitary Institute is to hold an Exhibition of Sanitary Apparatus and Appliances at Newcastle-on-Tyne, this year, from September 26 to October 21, in connection with the fifth Autumn Congress of the Institution.

ON April 18 next a Congress of Greek physicians and naturalists from all parts of the world will meet at Athens.

THE International Congress for Ethnographical Sciences, called together by the Paris Ethnographical Institution (founded 1859), will meet at Geneva on April 10 next under the presidency of M. Carnot. Besides all the European States, India, Egypt, Japan, Canada, the United States, the Argentine Republic, and Australia will be represented. The Institution includes amongst its main objects the facilitation of the personal intercourse between men of science of all countries, and also the support of exploring travellers. All information regarding the Congress is furnished upon application by M. G. Becker, Lancy, near Geneva.

WE learn from No. 13 of the Johns Hopkins University Circulars (February, 1882) that Prof. Cayley, F.R.S., has commenced residence as Lecturer in Mathematics. He read a paper at the January meeting of the "Mathematical Seminary" entitled "On Two Cases of the Quadric Transformation between Two Planes."

WHILST this winter has been remarkably mild in Western Europe, it has been of quite unusual severity in South-Eastern Russia. The main chain of the Caucasus is covered from the top to the lowest valleys with snow. The great depression of the Kura and Arako rivers looks like a Siberian plain covered with snow. The bright sun of the south seems unable to warm the cold soil, and in the night the small streams and irrigating channels freeze. Even the Mikhael Gulf of the Caspian, south of Krasnovodsk, was frozen from December 19 to January 7, and the thickness of the ice was 4½ inches.

COLONEL BRINE and Mr. Simmons started on Saturday morning from Canterbury in their balloon trip across the Channel, the wind being considered favourable. After getting about thirteen miles out from Dover the aëronauts discovered that the wind backed to the south-west, and thinking discretion the better part of valour they lowered their car into the sea and were picked up by a passing steamer, after having been in the air for about three hours.

INSTEAD of allotting the surplus from the Electrical Exhibition to the new School of Chemistry and Physics, M. Cochery has kept it for the establishment of a laboratory of electricity, which will be under his administration.

A COMMITTEE is being formed at Neuss on the Rhine, with the view of erecting a monument to the late Dr. Theodor Schwann, in the public gardens of that town. Dr. Schwann, as our readers will remember, was a native of Neuss.

THE February number of *Naturen* contains an interesting notice of the changes of movement observable in the Norwegian glaciers, which, as is proved by well-attested local records, have repeatedly advanced and receded within the last two centuries. It would appear that the vast system of the Justedal glaciers has been especially affected by these variations, for here, where the ice has been diminishing since 1750, it had previously been advancing so rapidly, that in 1742 the local magistrate was summoned by the occupants of a hamlet known as "Ni Gaard," Nine Farms, to inspect the damage that was being done, and to grant them remission of their taxes on such lands as no longer admitted of cultivation. The official report states that the glacier had then approached within one hundred ells of the nearest farm, and that in the following year the buildings were thrown down and crushed under the advancing masses of ice. Gradually the other farmsteads disappeared, leaving nothing but the name of the spot to attest that it had once been cultivated. Since this period the Justedal glaciers generally, have been retreating, a fact which was first noticed by Prof. Smith, of Upsala, who, writing in 1817, draws attention to the milder winters which in Scandinavia had characterised the latter half of the last century, while the years 1740-42, which succeeded several hard winters and bad summers, had been so especially inclement that they are known in Norway as the "Green years," from the unripe condition of the corn. This period coincides with the date of the devastations of the Nigaard glacier which, after a prolonged process of diminution has, according to De Seue, been again steadily advancing since 1869. The Folgefon glacier, near the Sörfjord has, as we learn from the report of Sexe, who visited it in 1864, been subject to similar alternations. At the present time it is advancing, its extremity having between 1860 and 1878 been projected 40 metres further forward, bringing it within 200 metres of cultivated fields.

THE meteorological report of the weather in 1881, as observed in Christiania, exhibits the same anomalies that have been recorded in other countries. Notwithstanding periods of exceptionable mildness, the mean annual temperature was 1° R. lower than the normal. The highest temperature (20°·4 R.) was recorded on May 31, the lowest (−18°·7 R.) on January 14. In November and December the temperature was higher than usual, the excess amounting in the latter month to 3°·6 R., which was largely influenced by the abnormal heat of December 28, when the thermometer at noon marked 9°·4 R., a temperature that has never before been reached since the opening of the observatory in 1837; while since 1857 the mean for December had not risen above the freezing point. The rainfall was marked by equally great irregularity in the manner of its distribution, only 5 millimetres being recorded for April, and 102 for August, the former being 19·3 mm. below the average, and the latter 24 mm. above it.

A SERIES of scientific lectures in Chinese to the Chinese schools in Peking, commenced by the American mission, is said to be attracting wide attention in the Capital, and to have drawn large audiences. Two of the course have already been delivered; the first by Dr. Edkins, the well-known Sinologist, on astronomy, and the second by Dr. Dudgeon, head of the Missionary Hospital at Peking, on the heart and circulation of the blood. Both lectures were copiously illustrated by the magic lantern. It is gratifying to find the missionaries, who are among the small number of people capable of teaching the Chinese in their own language, working thus for the spread of elementary western knowledge amongst the Chinese.

AT the ordinary meeting of the Meteorological Society, to be held at 25, Great George Street, Westminster, on Wednesday, the 15th inst., at 7 p.m., there will be an Exhibition of Anemometers and of such new instruments as have been invented and first constructed since the last Exhibition. During the evening the President, Mr. J. K. Laughton, M.A., F.R.A.S., will give a historical sketch of the different classes of Anemometers, and will also describe such forms as are exhibited.

ACCORDING to the *Annales de l'Extrême Orient*, at the commencement of the year there were in Japan 3929 miles of telegraph line with 9345 miles of wire. The telegrams sent during the year numbered 1,272,756, of which about 96 per cent. were in Japanese, while there were 22,695 international messages. A school of telegraphy has been founded in the capital, the pupils receiving a practical knowledge of English and French. During the year this institution sent 227 young men out to the various telegraph offices. The average cost of sending twenty words in Japanese for a distance of sixty miles is about three *sen*, or one penny, taking as a basis of calculation the line from Tokio to Nagasaki. The average for shorter distances is much greater, being about seven *sen* from Yokohama to Tokio, a distance of only twenty miles. There were 112 offices open to the public, and 70 attached to departments of State, the police, &c.; 53 remained open night and day; 848 Morse instruments were in use at the end of the year, and 29 Bell's telephones.

A VIOLENT earthquake is reported from Tongatabu in the Pacific on November 24 last. The whole island was so shaken that it was almost impossible to remain standing erect. A strong earthquake shock, which lasted twenty seconds, was felt at Bellinzoni, Olivone, and other parts of the Canton of Lesser, Switzerland, on February 27.

THE Russian botanist, M. Smirnoff, who is now in Turkestan, writes to the Russian Geographical Society that the vineyards of the country are quite destroyed by the small parasitic fungus *Erysiple*. He says that he never saw such a dreadful and widely-spread destruction of vineyards as he witnessed in Turkestan. It can only be compared with the destruction by a heavy hail-storm.

HERR HAKONSON-HANSEN draws attention to a remarkable phenomenon due to refraction, observed by him at Trondhjem, on January 17, and similar in all respects to one witnessed by him at the same place on November 15, 1881. On both occasions, at 2.50 to 3 p.m. in the day, a rose-coloured stripe was seen to stretch across the sky from about north-west to east. From the middle of this rose a vertical column of a somewhat lighter red colour, and inclining on its western side to a shade of yellow, the whole being intensely luminous. After remaining visible for about ten minutes, the bright reds and yellows gradually faded away, leaving nothing but a blackish gray streak across the heavens. The sudden and striking apparition of this vertical column recalled, as Herr Hansen observes, the descriptions given in past ages of bloody crosses seen in the heavens, and regarded as prophetic of coming wars and pestilence, and he remarks that if it had been seen at a later period of the day, it might have been taken to be a specially brilliant aurora.

STEAMERS recently arrived at New York report that they encountered immense fields of ice in lat. 45° 48′ N., long. 47° 48′ W. The *Circassian* had to steer south two days to clear them, and the *St. Germain* was fast for seventeen hours in the same pack.

M. SALIGNAC, one of the most active electricians of Paris, has discovered a new regulator which will be one of the curiosities of the next *grande soirée* given at the Observatoire on the 13th inst. Each of the two carbons is supplied with a parallel rod of glass, to which it is attached in a solid manner. These two rods being placed horizontally, are pushed by a spring, and the spark is lighted between them. But between the two glass rods there is a glass stopper which is warmed by the light in such proportion that the rods yield gradually to the pressure of the springs, and the carbons can approach each other, as is required for the constancy of illumination. Our correspondent witnessed preliminary experiments which he states have been a wonderful success.

THE additions to the Zoological Society's Gardens during the past week include a Macaque Monkey (*Macacus cynomolgus* ♂) from India, presented by Mr. G. Richardson; a Common Peafowl (*Pavo cristatus*) from India, presented by Mrs. Walter Crane; a Greater Sulphur-crested Cockatoo (*Cacatua galerita*) from Australia, presented by Mr. S. Sidney; a Common Jay (*Garrulus glandarius*, white var.), British, presented by Lieut.-Col. Birch Reynardson; twelve Pink-lipped Snails (*Helix hæmastoma*) from Point de Galle, Ceylon, a nest of Cocoons from Kadur District, Mysore, presented by Mr. J. Wood Mason; a Four-horned Antelope (*Tetraceros quadricornis* ♀), born in the Gardens.

OUR ASTRONOMICAL COLUMN

THE VARIABLE STAR U GEMINORUM.—The following interesting note upon a recent maximum of this apparently capricious variable, is by Mr. G. Knott, who writes from Cuckfield on March 6:—"On February 20 I noted it 13·7m. At intervals it seemed to flash out brighter, and there was every indication of a probable rise to maximum. Clouded skies night after night prevented my observing again till March 1, when in a clear interval of short duration I found U, 9·7 mag. *Disk large and ill-defined.* I have observed it since as under:—

March 2 ... 9·9m. Light rather unsteady. Colour, white or bluish white.
,, 3 ... 10·1m. Bluish white.
,, 4 ... 10·5m.

I suppose we may take the maximum to have fallen *not later* than February 28.

The previous maximum observed by me fell on April 3, 1881, the interval in days being 331, which would give 110 days as the period, if we suppose three maxima to have occurred in the interval. The period appears to vary between 75 and 126 days. The star appears to form a kind of connecting link between the *ordinary variable* and the so-called *new stars*, and as the causes which presumably underlie the phenomena are *physical* rather than *geometric*, perhaps we ought not to be surprised that the period has a wide and somewhat irregular range. *The star is a most interesting one.*

It is due to Mr. Knott to add that so far as the published observations of variable stars elsewhere enable us to judge, he has been much more successful than other observers in following the maxima of this difficult variable of late years; such success could only result from very assiduous and careful observation.

THE TOTAL SOLAR ECLIPSE OF MAY.—By way of reply to several inquiries as to the most probable track of the central line in the eclipse of May 16, we subjoin the following points which have been interpolated down from those given for five-minute intervals in the "American Ephemeris." As already remarked in this column, the difference in the place of the moon employed in that work, from Hansen's place, happens, on this occasion, to correspond very closely with the amount of Prof. Newcomb's empirical correction of Hansen's Tables, and hence the path calculated is likely to be as near to the true one as any prediction we are able to make.

Greenwich mean time, May 16. h. m. s.		Longitude E. ° ′		Latitude N. ° ′		Duration of totality. m. s.
18 22 30	...	30 18·2	...	25 50·9	...	1 9·2
18 23 45	...	30 58·0	...	26 11·6	...	1 10·6
18 25 0	...	31 37·0	...	26 31·9	...	1 12·0
18 26 15	...	32 15·3	...	26 51·8	...	1 13·3
18 27 30	...	32 52·9	...	27 11·2	...	1 14·6
18 28 45	...	33 29·9	...	27 30·2	...	1 15·9
18 30 0	...	34 6·1	...	27 48·9	...	1 17·1
18 31 15	...	34 41·9	...	28 7·2	...	1 18·3
18 32 30	...	35 17·2	...	28 25·2	...	1 19·5

Thus in longitude 31° 37′ E., latitude 26° 32′ N., a point close upon the Nile, the duration of the total phase is 1m. 12s., and the middle at 20h. 31m. 28s. local mean time. The central line crosses the Nile about a degree north of Luxor, one of the stations occupied for the observation of the last Transit of Venus.

A NEW ASTRONOMICAL MAGAZINE.—M. Flammarion has commenced the publication of a monthly periodical intended to give an account of the progress of astronomy and allied subjects in popular language. His first number contains an article on the history of the Observatory of Paris, with illustrations showing the establishment as it existed in 1672, from the frontispiece to Lemonnier's "Histoire Céleste" (a work which has now become somewhat rare), and in its actual state, with the additional grounds to the south of the main building, extending to the Boulevard Arago. The number also includes M. Flammarion's observations upon the brightness of the great comet of 1881, as compared with stars, from June 23 to September 4, commenced at Paris and concluded in the Alps at an altitude of 2000 metres. Referring to Prof. Winnecke having observed this comet as late as January 8, 1882, M. Flammarion remarks: "On n'a probablement jamais suivi une comète à une pareille distance." This, however, is a mistake. The distance of the comet of 1881 from the earth at the time of Prof. Winnecke's observation was 3·08 (the earth's mean distance from the sun being taken as unity): but the following comets were observed at greater distances:—Donati's comet, 1858, to 3·14, Colla's of 1847 to 3·18, the great comet of 1811 to 3·50, Mauvais' Comet, 1848, to 4·40, the great comet of 1861 to 4·70, and the extraordinary comet of 1729 to 5·23, notwithstanding the inferior telescopes of that day. The magazine is well printed and illustrated, and will doubtless be popular, especially with amateurs in France, who appear to be a much more numerous class than formerly.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

OXFORD.—There will be an election in next June at Magdalen College to at least one Scholarship in Natural Science, the conditions of election being subject to any new Statutes which may be made by the University Commissioners. The examinatian in Natural Science will be held in common with Jesus College, at which an election will be made to one Natural Science Scholarship, and possibly one Exhibition. Questions will be set relating to General Physics, to Chemistry, and to Biology; but candidates are recommended not to offer more than two of these subjects.

The value of the Scholarship is 80*l.* a year, and of an Exhibition 40*l.* Neither Scholarships nor Exhibitions will be awarded unless properly qualified candidates offer themselves.

Candidates for the Scholarship and Exhibition at Jesus must be natives of Wales or Monmouthshire, or persons who shall have been educated for the four years last preceding their election (or last preceding their matriculation if already members of the University) at a school or schools in Wales or Monmouthshire . . . if any such persons be found of sufficient merit and fit to be scholars of the College in the judgment of the electors.

Mr. J. R. Wynne-Edwards, of Giggleswick School, has been elected to a Junior Studentship for Natural Science at Christ Church, Oxford. At the same examination a second studentship was awarded to Mr. W. H. Pendlebury, of Manchester School, and an Exhibition was awarded to Mr. R. W. Lancaster, Commoner of Christ Church. There were thirty-five candidates.

CAMBRIDGE.—From the University accounts for 1880-81 just published, it appears that the disposable income of the year amounted to about 30,500*l.*, of which only about 3300*l.* was from property, while 27,200*l.* was derived from matriculation, examination, and degree fees, and quarterly payments from members of the University. Of this sum over 11,300*l.* was expended for strictly scientific purposes, to which also further receipts from special endowments, amounting to 2950*l.*, were devoted. It cannot be said that the University as such spends sparingly for scientific purposes in proportion to its income.

Mr. Donald McAlister has been approved as a teacher of medicine, and Mr. A. Sedgwick as a teacher of comparative anatomy, for the purpose of giving certificates to medical students.

SOCIETIES AND ACADEMIES

LONDON

Geological Society, February 8.—R. Etheridge, F.R.S., president, in the chair.—Ridley Henderson, William John, and James Robert Millar Robertson, M.D., were elected Fellows, and Prof. S. Lovén, of Stockholm, a Foreign Member of the Society.—The following communications were read:—Description of some Iguanodon remains discovered at Brook, Isle of Wight, indicating a new species, *Iguanodon Seelyi*, by J. W. Hulke, F.R.S.—On a peculiar bed of angular drift on the high Lower Chalk Plain between Didcot and Chilton, by Prof. J. Prestwich, F.R.S. In making a railway from the main line to Chilton, this bed of drift was cut through for a depth of about 1¼ mile. It lies on a flat plain extending from the foot of the escarpment of upper chalk to the top of that of lower chalk. In places it is full 28 feet thick. At first a fine chalk rubble, it becomes after a while coarse, and s divided by clay beds into an upper and a lower deposit. Here small boulders and bones occur, the latter much shattered; but *Elephas primigenius*, *Rhinoceros tichorhinus* (?), *Bison priscus*, *Cervus tarandus*, *Equus*, &c., have been identified. The boulders are Sarsen-stone, and there are small fragments of flint. Shells of *Pupa marginata*, *Helix hispida*, and *H. pulchella* have been found. The drift (which is widely spread) is from 150 to 260 feet above the Thames, at highest 407 feet above the sea. The author compares it with the rubble-beds overlying the raised beaches of Sangatte and Brighton. It is unconnected with any river-course, is not of marine origin, and its materials, where not local, are derived from the southward.

Anthropological Institute, February 7.—F. G. Hilton Price, F.S.A., treasurer, in the chair.—It was announced that the following new Members had been elected since the last meeting:—Dr. Brabazon Casement, F. T. Hall, Miss Marshall, R. M. Connolly, Mrs. R. M. Connolly, T. Dixon, Mrs. T. Dixon, W. K. Foster, T. Ridgway.—Mr. Edward C. Hore read a paper on the twelve tribes of Tanganyika. The author described the distribution of the tribes in East Central Africa: A narrow margin of a doubtful civilisation on the east coast—one to two hundred miles of small native tribes fast losing their distinctive nationalities and tribal customs and arts, and mixed with semi-civilised half-castes—then a narrow interval, more or less desert, seems to be as well the refuge of robbers and renegades, as a natural boundary between the first-mentioned tribes, and the next tract of from two to four hundred miles occupied by tribes of uneasy and apparently warlike aspect, and retaining to more considerable extent the original arts and customs; another narrow border of debateable country again separates these from the more prosperous, peaceful, and civilised tribes of the equatorial lake regions, a few of which the author described. The more northern tribes on the lake are an active and handsomely formed people, with obvious traces of the Abyssinian race, but many distinct differences are noted amongst the twelve tribes. The writer laid stress upon the fact of having lived and travelled among these tribes for four years, and never having failed in making some friendly negotiations with those visited. Three stations have already been occupied by the London Missionary Society, who will shortly send out a steel vessel to navigate the lake and maintain more stations on its shores.—Mr. George W. Bloxam read a note on a Patagonian skull brought from Carmen, at the mouth of the Rio Negro [lat. 44°], by Capt. Hairby.—A paper on the Napo Indians, by Mr. Alfred Simson, was read.

Royal Horticultural Society, February 14.—Sir J. D. Hooker in the chair.—*Proliferous Acorn-cups:* Sir J. D. Hooker exhibited malformed cups of *Quercus Ilex*, the evergreen Oak, received from Mr. F. Moore, of the British Museum. The tree grows on the cliff's edge, in the Isle of Wight. Minute acorns appeared to have been produced in the axils of the bracts which formed the cups.—*Carnation Disease:* Mr. W. G. Smith exhibited specimens of carnations received from Dr. Hogg, attacked by the nematoid worm, *Anguilula*.—*Root Malformation:* Dr. M. T. Masters showed a specimen of elm-root much distorted in places, in consequence of meeting with obstructions in growing in Lias limestone rock. It was lately figured and described in the *Gardener's Chronicle* (p. 147), and was received from Mr. Ingram, of Belvoir Castle.—*Variation in Pear-Leaf:* Mr. R. D. Blackmore exhibited a three-lobed leaf which had been produced after root-pruning, such being in this case a reversion to the primitive character of the plant. Mr. Henslow remarked that in some cases the change from a simple to a lobed and compound state is the result of further development, as may be easily traced in blackberries and raspberries. —*Apparent Superfœtation in the Pea:* Mr. J. Laxton, of Bedford, forwarded a communication, describing some experiments in fertilising a flower with the pollen of six other varieties. The conclusions he drew from the appearance of the peas and flowers subsequently produced by the seedlings, were that (1) pollen of more than one variety of pea used to cross-fertilise the same flower, may influence more than one ovule in the same ovary; (2) that there is some evidence of the pollen from more than one variety affecting the same ovule.—*Report on Winter Losses, &c., in Plants:* The secretary, the Rev. G. Henslow, gave an account of the progress he had made in compiling statistics for a report on the meteorological phenomena of, and consequent injury to plants in severe winters. He had obtained particulars of severe winters from A.D. 220 to 1881; but those during which destruction of, and injuries to plants had been specially recorded, were the following eight:—1851-52, 1852-53, 1859-60, 1860-61, 1864-65, 1878-79, 1879-80, 1880-81. He had collected all the information he had at present been able to find with reference to these winters, and had drawn up, first, a short account of the principal meteorological phenomena of the year *preceding* each winter, as well as of the winter itself—as the behaviour of a plant under frost so much depends upon its previous conditions; in each case such was followed by details of injuries to and losses of plants over as many places in the British Isles as possible. The importance of registering meteorological phenomena and the losses in several winters lay in the fact that the conditions of the winters respectively differed in many ways from one another. The consequence was that *the immediate cause* of plants succumbing to frost was not always the same. There would be an Introduction dealing with several interesting matters bearing on meteorology and plant-injuries, and he proposed completing it with copious indices, so that no difficulty would be met in finding the exact behaviour of every plant in any country and in any winter. A discussion followed, in which the great importance of elaborating the report as fully as possible, and of speedily publishing it, were insisted on.

Victoria (Philosophical) Institute, March 6.—A meeting of this Society took place at its house, 7, Adelphi Terrace, when a paper was read by Mr. J. E. Howard, F.R.S.

PARIS

Academy of Sciences, February 27.—M. Blanchard in the chair.—The following papers were read:—On double salts of mercury, by M. Berthelot. This relates to chlorobromides, iodo-bromides, and chlorocyanides of mercury, iodocyanide and bromocyanide of mercury and potassium, &c.—On the action of strong doses of strychnine on the motivity of nerves in mammalia, by M. Vulpian. They abolish the motivity in mammalia as well as in frogs. The quantity of strychnine necessary is greater than that of curare for the same result. (Nicotine, too, in sufficient dose, abolishes the motivity of motor nerves).—Induced currents of polar interversions, by M. Du Moncel. The currents from displacement of a coil on an iron bar, through a fixed magnetic field, are not of the same nature as those from displacement (in a fixed magnetic field) of this bar, reacting directly on the coil. In the former case those generated by each half of the magnetised bar are in contrary directions, whereas in the other case they are always in the same direction, and their intensity increases with the amount of displacement, but it becomes almost nil in a complete movement of the coil in the former case.—Colouring-matter formed in flour-paste, by M. Lecoq de Boisbaudran. Violet is sometimes formed by a small organism in the surface-cells of paste kept long in moist air. Different atmospheres were tried with the (fertilised) paste, and acetic acid vapour seemed the most favourable to production of the colour. The colouring matter is insoluble in water, but soluble in alcohol and ether; in the dry state it has a metallic lustre, like aniline colours. The author describes its spectral and other properties.—Geological and zoological relations of Campbell Island with neighbouring southern portions of land, by M. Filhol. The two principal geological elements of Campbell Island are a band of limestone, and lavas (the former anterior as a formation). The island seems to have appeared in the end of the Pliocene epoch. The New Zealand Eocene, Miocene, and early Pliocene lavas are quite different from those of Campbell, which contain mineral anorthite (a known characteristic of post-pliocene lavas). The geological age of the island is determined by the epoch of the volcanic eruptions. M. Filhol finds his conclusion confirmed by zoology.—On the physiological character of tendinous contraction, by M. Guérin. This contraction has been supposed of *reflex* order, a return action of the spinal cord, provoked by direct excitation of the nerves in the tendon. M. Guérin here contends that it is absolutely of the same order as muscular contractility, tendons showing both voluntary and involuntary contraction, like muscles.—On the employment of bitumen of Judæa against vine diseases, by M. de Lafitte. He recalls earlier observations on the subject than those noticed by M. Abric, by Count de Bertou. —M. Maumené, in a provisional note, said he was able to offer incontestable proof of the individual existence of H_3N, and its decided alkalinity with regard to active colours and acids. He hopes to do the same for HN.—Observations of the comet $g =$ VIII. 1881, and of planets (221) and (222) at the Paris Observatory, by M. Bigourdan.—Observations of the planet Palisa (221) at Marseilles Observatory, by M. Borrelly.—On the successive differentials of functions of several variables, and on a property of algebraic functions, by M. Darboux.—On the integration of differential equations by series, by M. Poincaré.—On certain uniform functions of two independent variables and on a group of linear substitutions, by M. Picard.—Gastric microzymas and their digestive power, by M. Bechamp. He isolated some from the juice obtained with artificial fistulas. Their action on fecula, cane-sugar, fibrine, caseine, and primov-albumen is described. They do not act on albuminoid matters in neutral matter. By their power of action in an acid medium they are distinguished from pancreatic microzymas; the latter, too, give crystallisable compounds of decomposition, as leucin and tyrosin, while the former do not.—New observations of apparent death in new-born infants treated successfully with a bath at 50°, by M. Campardon.—Analysis of a volcanic ash ejected by Etna on January 23, 1882, by M. Ricciardi. This contained silica 37·82, sulphuric acid 20·57, alumina 9·97, protoxide of iron 14·05, lime 11·98, with a little magnesia, chlorine, soda, and potash. The recent appearances of Etna seem to indicate a fresh paroxysm of the volcano.

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