little volume. We think the idea of making such a collection a happy one, not only for scholastic purposes, but also for the use of those who wish to be able at any time easily to refer to any of the passages in Latin authors in which our island is referred to. Mr. Cayzer gives also translations of some of the chief references in Greek writers. We should think, if teachers and examiners could be persuaded to break through custom, the introduction of such a book into schools would add interest to the reading of Latin, and furnish, besides, the little fellows with a stock of valuable information. Most of the cuts are appropriate, several being old friends.

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.]

The Colour-Sense of the Greeks

MR. GLADSTONE has shown that the language of Homer is an inadequate vehicle for conveying precise and nicely distinguished ideas of colour. Whether the nation that was content to describe colours so imperfectly was also incapable of subtle perception of tones of colour is clearly another question. Language does not keep pace with perception unless a practical or æsthetic necessity arises for expressing what is perceived in words to other people.

Practical necessity gives names to pigments and bright objects, such as flowers and precious stones, rather than to tones of colour; the æsthetic necessity that lies upon the artist to utter what he has felt will naturally lead to imitative expression sooner than to an expression that is merely symbolical. In other words an early race will learn to use colour with nicety for decorative and pictorial purposes before it develops the distinctions of language requisite for accurate word-painting.

That this was actually the case among the Greeks appears, I think, very clearly in a passage of Ion which is preserved to us in Athenæus Deipnos., Lib. xiii. cap. 81 (p. 603 *seq.*). Ion, who was a contemporary of Sophocles, describes an evening which he spent with the great tragedian in Chios. Sophocles, admiring the blushing face of a little boy who served the wine, quoted, with high approval, a line of Phrynicus:—

"The light of love gleams on the purple cheek."

On this a certain pedantic grammarian breaks in—"In sooth, Sophocles, thou art skilled in poetry; but yet Phrynicus spoke not well when he called the cheeks of a beautiful person purple. For if a portrait-painter were to colour the cheeks of this boy with purple pigment he would no longer appear beautiful. It is not fitting to compare what is beautiful with what is not so." Sophocles laughs at the objection, and replies—"Neither, then, my friend, wilt thou be pleased with that line of Simonides which, to the Greeks, has appeared very well said:—

'The maiden sending forth her voice from her purple mouth;'

nor with the poet, when he says, 'golden-haired Apollo;' for if the painter made the hair of the god golden and not black, his picture would be less excellent. Nor wilt thou be pleased with him [Homer] who said 'rosy-fingered,' for if one were to dip the fingers in rose-colour, one would produce the hands, not of a fair woman, but of a dyer of purple." This retort produced a general laugh, and confounded the pedant not a little.

The Greeks, then, were perfectly aware of the insufficiency of the poetic vocabulary of colour; and accordingly they did not expect descriptive rendering of colour from the poet. This, it is plain, is a circumstance that must constantly be kept in view in any attempt to find in the poetry of the Greeks a measure of the development of their colour-sense.

Aberdeen, December 3 W. ROBERTSON SMITH

The Comparative Richness of Faunas and Floras Tested Numerically

IN his letter in NATURE, vol. xvii. p. 9, Prof. Newton has strongly brought out the absurdity of comparing districts of *very different areas* by the proportionate number of species to area in each. On this principle he shows that to be equally rich with the small island of Rodriguez, Madagascar ought to possess four times as many species of birds as exist throughout the whole world! It does not, however, by any means follow that the method thus exposed may not be of value in comparing regions of approximately equal area, as is the case with several of the primary regions, to determine the comparative richness of which Mr. Sclater first applied it. I have not Mr. Sclater's paper at hand, but it is my impression that he made no attempt to show—"that the proper mode of comparing the wealth or poverty of one fauna with another was to state the proportion which the number of species composing it bears to the area over which they range"—as Prof. Newton implies that he did, but that he merely adopted this method as the only one readily available for the comparison of his regions. Although I took the opportunity of making some corrections in the figures, I never committed myself to the principle; and I very soon afterwards found that it was not to be trusted. As, however, several later writers have made use of it without remark, it will be interesting to consider where the exact point of the fallacy lies, and with what modifications the method can be trusted to give useful and consistent results.

If we compare two islands of almost exactly equal areas, such as Ceylon and Tasmania, and find that the one has twice or three times as many species of mammals or birds as the other, it will be generally admitted that we express the fact correctly when we say that, as regards such a group of animals, the one is twice or thrice as rich as the other; and the same may be said of two countries or two continents of identical areas. For on the supposition that there is a general correspondence between the numbers of rare and common, of local and of wide-spread species in the two areas compared (and this seems probable), then the average number of distinct species to be met with on one spot, or to be seen during a journey of equal length, will be proportionate to the total number of species in the two areas. But now let us divide one of the two continents or islands which we are comparing into two or more parts. We know, as a matter of fact, that one-half the area will always contain much more than half the total number of species, while one-tenth of the area will contain immensely more than one-tenth of the species. To take an example: the county of Sussex is about one-eightieth part the area of the British Isles, yet it actually contains full two-thirds of the total number of flowering plants, both being estimated by the same flora (Babington's "Manual," fifth edition, British Isles 1,536 species, Sussex 1,059 species). If we now compare either Britain or Sussex with an *equal area* on the continent of Europe or North America, we may obtain an instructive estimate of the comparative richness of their respective floras; but if we compare *unequal areas*, and then endeavour to equalise them by getting the proportions of species to area, we shall obtain erroneous results, which will become literally absurd when the areas compared are very unequal.

The problem remains, how to compare unequal areas of which we possess the zoological or botanical statistics. We can only do so by equalising them, and this may not be so difficult as at first sight appears. For example, let us take the Palæarctic and North American regions, in which the species of birds are nearly equal in number, but the areas are as about seven to three. The number of the Palæarctic species have, however, been proportionately increased of late years, and if we take the western half of the Palæarctic region so as to include North Africa and Persia we shall have an area about equal to the Nearctic region, and a number of species perhaps one-sixth or one-eighth less, which will thus represent the comparative richness of these two areas. The eastern half of the region, including Japan and North China, is probably as rich as the western; while the intermediate portion is poorer in species. Combining these three portions, and taking the average, we should perhaps find the Palæarctic region about four-fifths or five-sixths as rich as the Nearctic, instead of less than one-half, as shown by the method of proportionate areas.

Whenever we know how many *peculiar* species any district contains, we can deduct its area from the total area of the region to be compared, and this number of peculiar species, from the fauna of the region; and by this means we may reduce two unequal regions to comparative equality. Again, all detached portions or islands should be omitted in estimating the comparative richness of regions, because they affect these regions very unequally. By adding Britain to Europe you increase the area without adding to the fauna, and thus make the region seem poorer; while by adding Madagascar to Africa, or New Zealand

to Australia, you add to the fauna in a greater proportion than you increase the area, and thus make the region seem richer. For a fair comparison continents should be compared with continents, and islands with islands, and these should in every case be brought to an approximate equality of area by lopping off outlying portions with their peculiar species. We shall then get results which will be instructive, and which will afford us a true estimate of the comparative richness of different countries in the several classes of animals and plants.

ALFRED R. WALLACE

Mr. Crookes and Eva Fay

IN Dr. Carpenter's eagerness to show that his statements about Mr. Crookes and Eva Fay had some basis of fact, he seems entirely to have forgotten the real issue which he has himself raised, and which is of great importance to all engaged in the study of these tabooed subjects. The question simply is, whether *any* investigation of the alleged abnormal powers of individuals, however painstaking and complete it may be, and however decisive its results, is to be branded with opprobrious epithets, without any proof of error or fallacy, but merely on the dicta of newspaper writers and alleged "exposers."

In the case before us Mr. Crookes made certain experimen's in his own laboratory, in which the greatest refinements of modern electrical science were employed; and of these he published a detailed account. That is the sum total of his acts and deeds in regard to Eva Fay. Yet because these experiments have been referred to in America as indorsing Eva Fay's remarkable powers, and because some persons charge her with being an impostor, and go through an alleged imitation of her performances, Dr. Carpenter accuses Mr. Crookes of encouraging "disgraceful frauds" and indorsing a "notorious impostor." Now it is clear that, to support this accusation, Dr. Carpenter must *prove* that Eva Fay was an impostor in respect to what happened *in Mr. Crookes's house*, and that, to use Dr. Carpenter's own words, she evaded his "scientific tests" by a "simple dodge." He must prove that Mr. Crookes exhibited culpable carelessness or incapacity in accepting, as conclusive, tests which were really fallacious; for, otherwise, how can Mr. Crookes be held responsible for anything which happened afterwards in America? Dr. Carpenter has promised to do this in the forthcoming new edition of his lectures; but as the accusation against Mr. Crookes has been made in the pages of NATURE, and the question is a purely scientific one—that of the absolute completeness of the test of "electrical resistance"—I call upon Dr. Carpenter to explain fully to the readers of NATURE the exact particulars of that "simple dodge" which is to destroy Mr. Crookes's reputation as a physical experimenter, and to sustain the reputation of his accuser. Unless the explanation is so clear and conclusive as to satisfy *all* the witnesses of the experiments that Eva Fay *did* evade the scientific tests, and that what they saw was simple conjuring, then Dr. Carpenter is bound to find a conjuror who will submit to the same tests as Eva Fay did, and produce the same phenomena before the eyes of the witnesses, so as to show "how it is done." Mr. Maskelyne, who professes to have exposed Eva Fay, will of course be ready to do this for an adequate remuneration, which I feel sure will be forthcoming if Dr. Carpenter is proved to be right and Eva Fay's "simple dodge" is clearly explained.

I have already shown (in this month's *Fraser*) that the supposed *exposure* of Eva Fay in America was no exposure at all, but a clumsy imitation, as will be manifest when it is stated that the exposer, Mr. Bishop, performed all his tricks by *stretching the cord* with which his hands were secured to the iron ring behind his back! There is hardly a greater exhibition of credulity on record than Dr. Carpenter's believing that *such a performer* proved Eva Fay to be an impostor and Mr. Crookes's experiments valueless. But what can we expect when we find a *Daily Telegraph* report quoted as an authority in a matter of scientific inquiry?

I venture to think that, whatever may be their opinions as to the amount of *fact* in the phenomena called "spiritualistic" (by Dr. Carpenter, but never by Mr. Crookes), all men of science will agree with me that Dr. Carpenter is bound to *prove by direct experiment* that Mr. Crookes and his coadjutors were the victims of imposture on the particular occasion referred to; or if he fails to do this, that he should in common fairness publicly withdraw the injurious accusations he has made against Mr. Crookes and all who are engaged in similar investigations. If this is not done it is equivalent to deciding that no *possible* proof of such phenomena is admissible—a position which is not that of Dr. Carpenter, or, as far as I am aware, of the scientific world generally.

I beg to take this opportunity of apologising for my involuntary appearance under false colours in this month's *Fraser*. The letters "F.R.S." were added to my name after the corrected proofs left my hands and wholly without my knowledge. I have desired the editor to make a statement to this effect in his next issue, but in the meantime wish to set myself right with the readers of NATURE.

ALFRED R. WALLACE

Nocturnal Increase of Temperature with Elevation

WITH reference to the article in NATURE, vol. xvi. p. 450, on the above subject, allow me to place on record the following facts. On the night of January 7, 1874, in Lucknow, the temperature fell considerably below the usual. The minimum thermometer on the grass at the observatory registered 5° below freezing point. The destruction of plants in the Horticultural Gardens was great. Plantains, pine apples, sugar-cane, mango trees, casuarinas, pomsettias, colvilleas, bugainvilleas, &c., &c., were all injured; some killed outright. The remarkable fact which I observed on that occasion was, that the destruction of vegetation was only up to a certain height, viz., up to between seven and eight feet from the ground. Above that, not a leaf was touched by the frost. On the mango trees especially, which were planted close to each other, it was very remarkable to see a distinct line of destruction along the trees, of seven or eight feet from the ground. This, I think, distinctly showed that the temperature on that night, above eight feet from the ground, was decidedly warmer, and thus protected all vegetation, while all below it was more or less injured, or killed by frost. Other observations, I made lately, corroborate the result of the direct observations made by Mr. Glaisher. During the commencement of October there were several rainy days, with an easterly wind; the total rainfall was under 2¼ inches. When it ceased, and the clouds cleared away, I observed the following:—Before seven o'clock in the morning there were only a few low-lying clouds to be seen. As the sun rose, the wind still in the east and almost a calm, clouds began to form in all directions; about noon, and till about 3 P.M., the sky was thickly studded with cumuli of various sizes. After that hour, wider and wider gaps began to form between the clouds, and the dissolving of the cloud-masses continued as the sun approached setting. About two hours after sunset there was scarcely a cloud to be seen, and the twinkling stars came out in their full brilliancy. This melting of the clouds after a certain hour, and completely so after sunset, would, I think, indicate that the cloud region after sunset became decidedly warmer than it had been during the day.

E. BONAVIA

Lucknow, October 22

Expected High Tides

MR. EDWARD ROBERTS in his letter has, I think, missed the chief object I had in addressing you. I did not complain that the authorities had not taken pains to calculate the heights of the tides, but that while one could take up almost any paper on the coast and find the heights of the tides of the place for the coming week, not one of the London papers, so far as I could find, supplied this information for its readers. What I felt to be a desirable thing was that the Meteorological Office, or some other constituted authority, should send to the daily papers warnings, when necessary, that on such a day a dangerous tide might be expected with a wind from such a quarter and with such a barometer, as the tide would be unusually high under even favourable weather—in fact, give a forecast of the tide.

It is almost useless to ask the public or vestries to put two or three facts together and think out the matter for themselves; they require some authoritative announcement to prepare for danger. And this is the more necessary as an overflow of the Thames at above-average spring tides is, as Mr. Roberts says, now a matter of meteorological circumstances only, and on account of the increased range of the tide in the river.

I was not aware that Captain Saxby had predicted high tides so far back as 1869. If, as Mr. Roberts says, the Astronomer-Royal wrote re-assuring the public that there was nothing extraordinary in the November 3 tide, and as, on the contrary, that tide rose 3 feet 3 inches above Trinity high-water mark, this incident may possibly have had something to do with the establishing of Captain Saxby's reputation with the public as a predictor of tides,