

by no means the only culprit—is that, even amongst scientific men, botany has come to be regarded in this country as scarcely a serious branch of science at all, and as little more than a kind of biological equivalent of the "Use of the Globes," suitable, indeed, for ladies' schools, and useful, also, like "Materia Medica," for the purpose of occupying the attention of medical students so as to keep them out of mischief during their first summer session.

LETTERS TO THE EDITOR

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Visual Phenomena

IN support of Mr. Arnulph Mallock's conclusion (vol. xiv. p. 350) as to the cause of the star-shaped appearance presented to the naked eye by a bright point, perhaps the following epitome of some notes which I made last January may have interest:—

1. Looking at a distant lamp, with both eyes, I see a radiant corona round the lamp.
2. I find that this corona is composed of two coronæ superposed, one due to the right eye, the other to the left.
3. Each corona has distinctive features of its own, which are recognised in every observation and have remained the same for years.
4. The radiant beams (which are the conspicuous feature) are not exactly radial, but are forked once or twice.
5. The corona is bounded by a peripheral fringe of blue succeeding to red.
6. The diameter of the corona varies with the diameter of the pupil. The distal portions of the radiant beams are concealed or revealed by the contraction or dilatation of the pupil.
7. Any given part of the radiance may be cut off by advancing an opaque body in front of the eye, from the *same* side as the given part of the radiance.

An obstacle which cuts off only the central rays of the entering pencil only dims the central image, but does not affect the radiance.

The radiance, therefore, is due to the outside rays of the entering pencil becoming excessively refracted so as to be thrown across the visual line before reaching the retina.

8. There is nothing in the *cornea*, *aqueous humour*, or pupillary margin of the *iris*, that can cause such refraction.

9. It appears most likely that this excessive refraction of the outside rays of the entering pencil is caused by something in the crystalline lens—probably, in the first place, by undue convexity of the more marginal parts of the lens, which are uncovered by dilatation of the pupil.

10. The *radiant* appearance of the corona is probably due to the radiate structure of the crystalline lens.

The agreement between them is pretty close, extending to the furcate character of the rays.

11. On examining my own eyes by a pencil of diverging rays (admitted so as to throw a shadow of anything inside the eye upon the retina) I find that the main beams of the radiance correspond pretty well to the main radii of my lenses, in the opposite direction.

This confirms that the radiance is due to the radiate structure of the lens.

In one particular my observations differ from Mr. Mallock's. He finds that the length of the rays depends "on the brightness of the point" as well as on the size of the pupil. I find that points of different degrees of brightness have rays of the same length, that length being limited only by the size of the pupil, but that the *breadth* of each ray varies with the apparent size of the lamp-flame or other luminous point under observation. Indeed, if the lamp-flame is so bright as to stimulate the iris to contraction, the rays become shortened in like measure.

The radiate structure of the lens of the human eye is well shown in Kölliker's "Manual of Human Microscopic Anatomy" (ed. 1860, p. 568). The "non-fibrillated" or "central planes" there described are possibly of greater refractive power than the

wedge-shaped fibrillated portions between them. This would cause the phenomenon of radiance by excessive refraction of the outside rays of the entering pencil.

HUBERT AIRY

Edensor, Kidbrook Grove, Blackheath, Aug. 28

Species and Varieties

IT may be taken, I presume, that the description and naming of "species" has now a great value as material for studying the laws of the evolution of species and of geographical distribution and variation; and that the question is not so much to know what name to call a "species" as to account for its presence and form in the economy of nature. And it will, perhaps, also be granted that the study of geographical distribution does not consist alone in acquiring a knowledge of the fauna of a district, so much as in investigating the laws of the special differentiation of that fauna. It thus becomes evident that the slightest modification tending to persistency requires the most careful record, as it is only by the knowledge of first slight modifications that we can expect to understand the process of larger divergencies.

In faunistic catalogues, should the author be inclined to the "lumping method" (perhaps in one sense correctly), a form may be considered as only a variety of some well-known species, and recorded under that name. We thus learn nothing of its modification, and are led to think of it as agreeing with the typical form, thus losing one of the most important facts in our study of variation and distribution. Another element of error seems also co-existent. When a newly-discovered form is designated a variety of another species, it would lead us to suppose that that species is the original type from which the other form has branched. But it is quite possible to suppose on such reasoning that the newly-discovered form may have been the parent from which the previously described species may have been derived. The offspring may have been collected and described before the parent; or we may be filling a space with planets to which there is no sun. This I have frequently suspected in the study of exotic lepidoptera, a study which is principally confined to the perfect form of the insects and of whose larval changes we know in most cases nothing. It rests generally on the judgment of individual entomologists as to the amount of difference to be considered specific.

Without venturing on the vexed question as to species and varieties, would it not be furthering the cause of science that what are considered as merely local varieties should, *as such*, have as careful a description as though they were ranked as species.

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Antedated Books

I CANNOT allow that our *Transactions* are antedated, as is asserted by "Another F.Z.S." As is the case with the *Philosophical Transactions* of the Royal Society and the *Linnean Transactions*, no date beyond the year of issue is given on the cover, but there is the additional advantage of the date at which each number goes through the press at the bottom of the sheets. There can, however, I think, be no objection to giving a still more exact date of publication on the cover, and this, in accordance with "Another F.Z.S.'s" suggestion, for which I am much obliged to him, I propose to do in future.

P. L. SCLATER

I AM sorry to see that "F.Z.S." still prefers to write under the signature of his first letter. I was in hopes that when he saw the gravity of the accusation which he was bringing against me, he would in his own person have disclaimed any intention of seriously laying such a charge to my door, but as no word of apology escapes him, I regret that in making such an injurious statement he has not added the weight of his name to the accusation. I am, therefore, at liberty to suppose that he is no stranger himself to the "evil practice" (*honi soit qui mal y pense*) which he deprecates, otherwise I cannot imagine such a thought occurring to anyone; but it is evident that on receiving the book his only impulse was to write a letter to this journal instead of charitably endeavouring to discover some feasible explanation of what he calls the "false" date. As most naturalists in England are in the habit of following the rules of the British Association in regard to nomenclature, "F.Z.S." can find more profitable