

LETTERS TO THE EDITOR.

Deafness in white cats.

I AM engaged upon an investigation concerning the causes of deafness; and I have therefore naturally been much interested in Mr. Lawson Tait's paper concerning deafness in white cats, published in *Nature* (vol. xxix. p. 164), and in the letter of Mr. Joseph Stevens, published in the same journal, concerning his father's breed of deaf white cats (vol. xxix. p. 237).

I have myself come across three instances of white cats with blue eyes (two in Europe and one in America), and in each case the animal was deaf.

Mr. Tait's statement, that "congenital deafness is not known to occur in any animal but the cat" is a most extraordinary one, in view of the great prevalence of congenital deafness among human beings.

Of the 33,878 deaf-mutes in the United States, more than one-half are congenitally deaf;¹ and in Europe (excepting Germany) the proportion of congenitally deaf appears to be much greater,—about four to one, according to the late Dr. Harvey L. Peet (1854).²

Why should congenital deafness among the lower animals be confined to cats, and why only to white cats?

Mr. Tait notes also an apparent association between epilepsy and whiteness in animals. He says, "Every kind of white animal I have kept as a pet has been the subject of epilepsy; and the association is suggestive when we are told, as I have been frequently, that the disease is unknown among negroes."

It is worthy of note, that deafness also appears to be less common among negroes than among white people. According to the recent census, the total white population of this country amounts to 43,402,970, and the total number of white deaf-mutes is 30,661. The colored population is given as 6,580,793, with 3,177 colored deaf-mutes (not including Chinese and Indians).

Thus, while we have one deaf-mute for every 1,416 of the white population, we have only one deaf-mute for every 2,070 of the colored people. It would be interesting to know whether the proportion of congenitally deaf is less among the colored than the white deaf-mutes.

The pallid complexion of many deaf-mutes has often been commented upon by strangers as an apparent indication of ill health. While I cannot say that I have myself observed this as a common characteristic, still my attention has never been specifically called to the point. It would be easy to test the matter by collecting into one room all the congenitally deaf pupils of some large institution, excluding those pupils who became deaf from accidental causes. A cursory examination would probably show whether there is or is not, in the human race, an association between congenital deafness and the absence of coloring-matter from the skin and hair. I trust that some of your readers may be able to throw light upon these points.

ALEXANDER GRAHAM BELL.

Washington, D.C., Feb. 4, 1884.

Radiant heat.

In a letter to *Science* of Jan. 25, Mr. Fitzgerald thinks it is possible that I am misled as to the manner in which my rotating-screens work, by reason of the complication of the arrangement. I must nevertheless continue to assert, that I think I understand

¹ See Compendium of the tenth census (1880), part ii. p. 1664.

² See American annals of the deaf and dumb, vol. vi. p. 237.

how the process I have invented operates, and cannot admit that I am in error in this until such error is pointed out. Now, Mr. Fitzgerald's demand that I should show that the heat which originally came from *B* is returned to *B* in the same direction as the heat coming from *A*, would incorrectly lead the reader to suppose that I made some such statement or supposition in the original paper, and that consequently I was misled, as he suggests. But the most superficial examination of the paper shows that I have not for a moment supposed this; as I have simply proposed to so arrange the reflecting surfaces as to return radiations from *B* through some one or more of the apertures in the screen *b*, and not necessarily through the apertures from which they originated. It necessarily follows, that I did not suppose them to be returned in a direction parallel to the radiations from *A*.

I think, then, that Mr. Fitzgerald must certainly admit that I have not made the blunder which is implied in his letter.

Again: Mr. Fitzgerald takes it for granted, apparently, that this want of coincidence in direction would be fatal to the process; whereas, in my estimation, the only question is, whether the radiations which originally came from *B* are returned to *B* or not. What their direction may be appears to me entirely immaterial.

If it is possible to show that the want of coincidence in the direction of all the rays coming to *B* invalidates the process, as Mr. Fitzgerald implies, he will no doubt be able to give a direct proof of the fact. Such proof, however, seems to me impossible; for, after the energy reaches *B*, the path by which it has arrived is of no consequence.

It goes without saying, that in this view of the matter it is quite impossible to substitute the process proposed by Mr. Fitzgerald in place of mine; as in his process these directions necessarily coincide, which in mine cannot coincide.

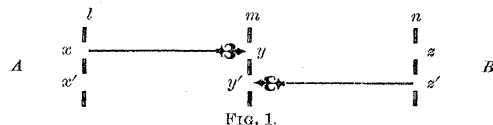


FIG. 1.

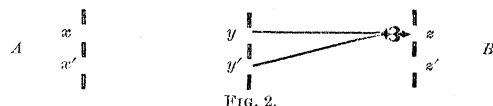


FIG. 2.

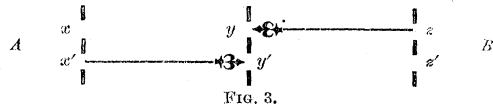


FIG. 3.

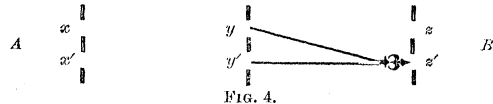


FIG. 4.

It does seem possible, however, to employ two sets of openings such as Mr. Fitzgerald has proposed, in such a way that they shall together accomplish what neither of them can effect singly. For example: let there be three fixed screens, *l*, *m*, *n*, with two sets of openings, *x y z*, *x' y' z'*, which can be opened or closed instantly; and let them all be closed except when the contrary is explicitly stated. Let each of the four equal intervals of time which we shall speak

of be the time required by the front of a ray in moving from n to m , or *vice versa*. During the first interval let x and z' alone be open. The rays between the screens at the conclusion of the first interval are shown in the first diagram. During the second interval let y alone be open, and let the reflector at y' send the ray impinging on it towards z . The situation of the rays at the end of the second interval are shown in the second diagram. During the third interval let x' and z alone be open. The rays between the screens at the end of this interval are represented in the third diagram. During the fourth interval let y' alone be open, and let the reflector at y send the ray from that point toward z' . The next interval is a repetition of the first, and so on.

It is seen that, in fact, the difference in the directions of the two rays arriving at B can be made less than any assigned finite angle, however small, by sufficiently increasing the distance between the screens, or sufficiently decreasing the space between the openings, or both.

It is possible that the above process may, from its comparative simplicity, conduce to a clearer understanding of the relations involved, though it seems inferior to the one originally proposed in some important particulars.

H. T. EDDY.

University of Cincinnati, Feb. 2, 1884.

The Greely relief expedition.

In view of the comment upon the Greely relief expedition, it may not be out of place at this early date to call attention to a neglected principle of arctic navigation which bears with full force upon the navigation of the route in question. To its adoption may be traced the success of Nares with the Alert and Discovery, and of Nordenskiöld with the Vega; to its neglect, the wreck of the Jeannette and the Proteus among a host of others.

Simply stated, it is, under all circumstances, to cling to the coast, and among its islands find protection against the floating ice. To coast along the edge of the floe, and follow the openings it offers, is a veritable siren. Of course, the principle is not applicable till after Jones's Sound is passed; but here the course is usually free.

The Eskimo knew me as

TILLOTIANIAC.

New Haven, Conn., Feb. 2.

The red skies.

I have only time to-day to reply very briefly to your editorial inquiry on p. 30, as to previous instances of red skies and volcanic eruptions.

You will find a West India instance in 1831 on p. 165 of the *Meteorological magazine* for 1883; but the most striking parallel has been pointed out by Professor Karsten of Kiel as occurring in 1783, lasting about four months, and spreading over the whole of Europe, northern Africa, and eastern Asia.

Arrangements are being made for the concentration of all collectible information upon the subject, and I shall be proud to act as the receiver of copies of any notes or records which your readers may intrust to me.

G. J. SYMONS, F.R.S.

62 Camden Sq., London, N.W.,
Jan. 25, 1884.

[We shall publish Professor Karsten's article next week.]

Aeolian ripple-marks.

On the evening of June 11, 1883, after a severe rain-storm, during which large quantities of soil were washed from adjacent fields and deposited along the

roadside, I noticed near Brodhead, Wis., a peculiar phenomenon, which may be worth recording. The mud, deposited only a few hours before, was still very mobile, and, at the point where best seen, covered an area a rod or more wide and three or four long, presenting a perfectly plane surface. The steady force of the strong wind was interrupted by occasional gusts of greater violence, each of which raised on the plastic mud-surface corrugations, which, in every detail that could be caught during their momentary existence, resembled ripple-marks formed by water, being a beautiful and distinct series of parallel ridges slightly concave toward the direction of the moulding-force. The outlines of these aeolian ripples were no sooner defined than they began to dissolve, and a minute or two sufficed to obliterate all trace of them. The phenomenon was observed several times on the same surface, and also in adjacent localities, where the consistence of the mud, and therefore the duration of the ridges, varied slightly. The ripples were best defined in the thinnest mud, though this was most favorably situated for their production; and they disappeared less rapidly where formed in the more viscous material.

This, of course, is not a radically new phenomenon, but a rare phase of the familiar action of wind on liquid surfaces.

R. D. SALISBURY.

Winchell's 'World-life.'

Will you permit me to announce that a number of errata, attributable both to author and proof-reader, have found their way into my late work on 'World-life;' and I will be glad to mail slips of corrections to any who will kindly notify me by simple postcard that they so desire?

ALEXANDER WINCHELL.

Ann Arbor, Mich.

THE LABORATORY IN MODERN SCIENCE.

THE material circumstances under which scientific discovery is prosecuted have been completely revolutionized during the last forty years. Of the immense changes that have occurred, the majority have fallen within the last fifteen, one might almost say dozen, years. It is interesting and profitable to contrast the past with the present in this respect.

Forty years ago there were very few, more properly no laboratories which we of to-day would consider even tolerable. Now every university of importance and high repute, the world over, has large suites of rooms for each department of science, and often numerous great buildings within whose walls thousands and thousands of students are daily brought face to face with the facts and laws of nature. The generation that is now gone pursued its scientific studies in incommensurable quarters, and even those were destined for the use of the professors rather than the students. Many a small, dingy, and ill-lighted room is still to be seen, where some illustrious *savant* created new knowledge,—a small square chamber, with crooked walls, low ceiling, undulating floor,