MacCullagh's Theory of the *Æther*.

MR. BASSET's criticisms in his letters in *NATURE* of October 17 (p. 595) and October 24 (p. 618) call for some reply. I willingly avail myself of the opportunity to attempt to make my meaning clearer.

(1) As regards the first letter, there seems to be some misconception. I have nowhere in the papers referred to given a proof, such as he supposes, of the theorem which he calls in question, viz. that a gyrostatic *æther* may be constructed which will function according to MacCullagh's optical scheme. That proposition is, I take it, Lord Kelvin's; and I simply gave references to his treatment, which occurs, at any rate implicitly, near the end of the third volume of his "Collected Papers" (pp. 442, 466).

The principal aim of the second of the papers referred to (*Phil. Trans.* A, 1894) was, assuming the existence of a continuous medium with kinetic and elastic energies given by MacCullagh's expressions, to examine how far such a medium would fulfil the functions that are required of the *æther*, as (i.) the transmitter of radiation, (ii.) the medium in which electric actions consist and are transmitted, (iii.) the underlying medium in which ordinary matter may itself consist, in the form of permanent configurations of strain or motion possessing mobile characteristics. In MacCullagh's own time it was recognised, by none more than by himself, that a medium like his was in no way analogous to ordinary elastic matter. Moreover it was held by many that it was an impossible constitution for any medium at all. This latter conclusion I combatted by quoting the fact that Lord Kelvin has actually shown how to make a model, representing MacCullagh's medium, by means of a cellular structure composed of portions of ordinary matter in spinning motion. This is the only way that I intended to introduce the gyrostatic *æther* into the argument. For I hold it to be more rational to take matter to be a structure of molecular type in the primordial medium (which is not itself matter, but is a *continuum* with simpler fundamental properties than elastic solid matter) than it would be to take the *æther* to be a molecular or cellular structure built up out of ordinary matter.

The functions required of the *æther* show that it must be a medium which can have kinetic energy involving inertia, and also elastic energy of some kind when strained. According to MacCullagh's scheme, its elasticity would consist simply in resistance to absolute rotation; so that an element of volume of the medium is taken to have relations to directions in space, of the same general type as the axis of a spinning gyrostic actually possesses. The analysis of the interaction of this inertia and this elasticity forms a dynamical theory of the medium, but the dynamics is not the dynamics of ordinary matter.

Mr. Basset easily arrives at inconsistencies by applying MacCullagh's energy formula directly to the structural gyrostatic medium of Lord Kelvin. The reason is that the problem is one involving ignored coordinates (in the phraseology of Thomson and Tait's "Natural Philosophy") corresponding to the latent spinning motions of the imbedded gyrostatics. Before the principle of least action can be applied after the manner of an ordinary continuous elastic medium, the actual energy function of the gyrostatic medium must be modified in the well-known manner, and it will thus assume a form equivalent to MacCullagh's. It would, no doubt, be interesting and instructive, as regards dynamical principles, to establish this in detail; but this is hardly the place to enter into a technical problem.

(2) As regards Mr. Basset's second letter, on the reflection of light from the surface of a magnet, the parallel which he draws between one type of theory which I provisionally uphold, and another which I reject, is, I think, not a real one. The latter theory retains the dynamical equations and surface conditions which belong to the luminiferous medium under ordinary circumstances, merely adding on to the electric force a new part of magneto-optic origin. This would hardly be open to objection if it worked; but it is admitted that it does not work, and in default of a specific reason being assigned for the discrepancy the theory fails. It is as if a machine, whose mode of working is thoroughly known under certain simple conditions, were observed to be working steadily under more complicated circumstances, while a mathematical analysis showed that it ought to get jammed under these new conditions. The inference would, I think, be that the machine has been reset, or some change made in its constitution, which obviated the jamming. Now the ordinary equations of the electric theory of light are, presumably, deducible from the energy function of the medium by the principle of least action. When the substance that

transmits the light is in an extraneous magnetic field, there is a subsidiary term in the energy function which arises from this field; therefore the application of the principle of least action will now give different equations of the medium, and different boundary conditions, from those which ordinarily hold good. The statement that the boundary conditions which held for non-magnetic circumstances are not now maintained, is not to the point; the question is rather, whether the boundary conditions which are appropriate to the actual formulation of the problem can all be maintained, and if they can the theory is consistent.

J. LARMOR.

St. John's College, Cambridge, October 25.

Lightning.—Chain Formation.

ON September 9, 1895, I was cycling near Pitlochry, N.B. The day had been extremely hot—80° F. in the shade—and as dusk came on it grew somewhat foggy, and flashes of distant lightning became frequent. At ten o'clock there suddenly came on a terrific thunder-storm. Crash succeeded crash, and the lightning, of all colours, blazed almost continuously. Objects fifteen miles off could be seen as plainly, if not more so, than in bright daylight. The rain soon turned the road into a torrent, and my electric lamp failed to act properly. But the chief peculiarity was the occurrence of eight strange flashes of a chain formation, with large elliptical links, and of a golden-yellow colour. These flashes were not rapid in their passage, as ordinary lightning is wont to be; but one of them took slightly over a minute to pour from the clouds to the edge of the valley opposite me. Two of these chains of living, burning gold passed between adjacent clouds, while the remaining six came to earth, one in the field just beside me. I then went off to seek for shelter; but the storm continued till 1 a.m.

WILLIAM CRAWFORD.

Personal Injury from a Fire-ball.

IN compliance with a wish expressed by several scientific friends, I place on record an instance of damage done by a fire-ball or globular lightning. About five weeks ago, when I was in Londonderry, the circumstances were related to me by Mr. James Harvey, of Northland Road in that city. Mr. Harvey was staying during the month of August at Culdaff, on the north coast of Donegal; and on the 24th of that month, at about 4 p.m., a little boy named Robert Alcorn, whose parents occupied a house near Mr. Harvey's, was desired by his father to go into the yard and drive away some fowls from the door. On going out of the house, the boy saw a large bright object in the sky about the size of the table in his bedroom (I give his own account, leaving out necessary considerations of distances, &c.), or apparently about six square feet in area. The object came towards his house from the west, or north-west; and when it came close, it partly burst with a report like that of a gun. He put his hands over his face to shield himself from "the spark," and after the explosion the bulk of the ball appeared to continue its course towards the east, low down. When it burst, however, it struck him, shattering the thumb and the first and second fingers of the left hand, cutting, scratching, and blackening the right hand and left cheek, and shattering into fragments several bone buttons on his coat. Very soon afterwards, Dr. R. Young, of Culdaff, and Dr. Newell, of Moville, attended the boy, and amputated the fingers and a portion of the thumb.

No one near the place saw the ball (except the boy, of course), but the parents and several others heard the report, and the boy's father rushed out immediately and caught his son as he was falling. Mr. Harvey soon afterwards examined the place, and could find no further trace of the fire-ball, except that a piece of bark had been knocked off a small tree within a few feet of the place where the boy was struck. The local police made exhaustive inquiry as regards the possibility of any one's having fired a gun at the boy, or of his having had any explosive in his possession; but nothing of the kind transpired.

It is well to add that at Redcastle (about eight miles away), one of the residents saw, on the same day, a bright object in the sky, which object he took to be a fire-ball. The day was stormy, with heavy showers, but no thunder.

M. Jamin relates ("Cours de Physique," tome premier, p. 470) several instances of globular lightning, and from these I select

the following as bearing, perhaps, the greatest resemblance to the above case as regards atmospheric conditions :—

“ À la suite d'un violent orage observé près de Wakefield, le 1^{er} mars 1774, lorsqu'il ne restait plus dans tout le ciel que deux nuages peu élevés au-dessus de l'horizon, M. Nicholson voyait à chaque instant des météores semblables à des étoiles filantes descendre du nuage supérieur au nuage inférieur.”

October 28.

GEORGE M. MINCHIN.

The Dispersal of Acorns by Rooks.

IN peat-mosses, on open chalk downs, and in ploughed fields, often a mile or more from the nearest mature tree, one constantly finds acorn-husks and also seedling oaks, which last a few months or, perhaps, a couple of years, and then die, the conditions being unfavourable. It has always seemed to me, while studying the origin of the existing fauna and flora of Britain, that this dispersal of acorns ought to give an important clue to the means by which this country was again clothed when the climate became more genial after the Glacial Epoch. The oak has the largest seed of any British plant, and if it can be carried distances of a mile or more, it is evident that the whole of our present flora may have spread more rapidly than is usually imagined, and may have crossed straits and wide rivers.

I have for several years noted the position of these seedling oaks, finding them in places where no mammal would take the acorns. For instance, they are common in any of the New Forest peat-bogs that are within a mile of an oak-tree. They are common also in some places on the top of the escarpment of the South Downs, half a mile from oaks, and 300 or 400 feet above them. They are always associated with empty acorn-husks, stabbed and torn in a peculiar way.

In October and November rooks feed in the oak-trees, and I have long felt convinced that they were mainly responsible for the dispersal of acorns, though it is not easy to catch them actually doing it. On October 29 of this year I was successful. In the middle of an extensive field, bordered by an oak-copse and scattered trees, a flock of rooks was feeding and passing singly backwards and forwards to the oaks. On driving the birds away, and walking to the middle of the field, I found hundreds of empty acorn-husks, and a number of half-eaten pecked acorns, which had not had time to change their colour—a cut acorn changes colour on exposure to the air like a cut apple, though not quite so fast. This showed that the birds had been disturbed in the middle of their feast, for the marks on the acorns were quite unlike those made by a rodent or any mammal. They were stabbed and pecked, and the husks were torn off in strips, usually starting from a puncture. It was also noticeable that many of them were not shed acorns, but were accompanied by acorn-cups, the stalks of which had been bitten to tear them off the trees. This was singular, for the ground beneath the trees was covered with shed acorns. The rooks, however, were in the trees, not under them, and the reason for the selection of acorns in cups is probably that they are easier to carry—a shed acorn must be an awkwardly large and slippery thing for a rook's beak, one with a stalk will be more convenient. Several uninjured acorns were found, and most of the remains occurred on smooth spots of short turf—places where a slippery acorn might conveniently be pecked without being lost. One almost uninjured acorn had been driven by a single peck deep into the soft soil of a mole-hill.

It might be thought that it would be much simpler for the rooks to feed on the ground beneath the trees. Some of them apparently do so; but the majority seem always to carry the acorns into the open. The rook is a suspicious bird, quarrelsome, and a born thief. He seems particularly to object to a comrade watching him from any post of vantage, and the rooks when among the oaks, for some reason or other are always quarrelling, notwithstanding the abundance of food. An acorn dropped on rough ground or in a peat-moss would stand a great chance of being lost in some crevice or soft place; but the oak seeds so freely, that the bird need not waste time trying to recover the lost acorn—there are plenty more on the tree.

In this way oak-woods must spread rapidly. But we still want observations as to the distance to which acorns can be carried. I have seen seedling oaks at a distance of a mile from the nearest mature tree (not necessarily the tree from which the acorn came), and have found the characteristically torn husks somewhat further away. Do rooks roosting in elm-trees ever carry home acorns for supper? There used to be a number of

rooks which roosted in elms near Brighton in the autumn and winter, but crossed the Downs to feed in the Weald. I have often watched them returning at dusk. Do they ever bring acorns from that distance? This flock may have been responsible for the seedling oaks near the edge of the Downs; and if it could occasionally bring an acorn still further, to Brighton, it is evident that the oak may have crossed the Strait of Dover, when it was somewhat narrower, and that Britain, as far as the oak shows, may have been continuously an island since the Glacial Epoch.

CLEMENT REID.

On the Audibility of Fog Signals at Sea.

IN NATURE of August 8, attention was called to some recent investigations, published in *Hansa*, on the inaudibility of fog-horns at sea within certain zones surrounding the signal, although the horn is distinctly heard outside of such regions. It seems strange to me that I can find nowhere suggested that this may be a phenomenon of interference similar to that suggested in light by Dr. Lloyd of Dublin (*Trans. Roy. Irish Acad.*, vol. xvii.).

If we let x equal the distance of the observer from the signal, h and y the heights of the signal and the observer, respectively, above the level of the sea, and δ the linear difference between the paths of the reflected and the direct rays of sound, then

$$x = \frac{1}{2\delta} \left\{ \delta^4 - 4\delta^2 (y^2 + h^2) + 16 h^2 y^2 \right\}^{\frac{1}{2}} = \frac{2 h y}{\delta}$$

approximately. An attempt to apply this formula to the observations recorded in the Report of the American Light-House Board, published in 1894, was foiled by the lack of sufficient data for substitution in the above formula. However, if $h = 100$ ft., $y = 30$ ft., and δ be taken for a wave-length of 2 ft. (which are probable values for the variables), then we would expect minima of sound at 1.1 and 1.3 miles, the maximum between these being at a distance of half a mile from the source of sound, which quantities are of the right order of magnitude. These distances might be modified considerably by refraction, the wind, and to some extent by the tide. When there are two minima, as in the *Hansa* experiments, this seems a much more probable explanation than that by refraction alone generally offered, and it explains the phenomenal loudness outside the silent area. The above equation shows that the boundaries of the silent regions in vertical planes are hyperbolas, which is essentially different from what the refraction theory gives.

F. E. FOWLE.

Washington D.C., U.S.A., October 21.

To Friends and Fellow Workers in Quaternions.

IN NATURE for October 3, 1895, there is a letter, signed by P. Molenbroek and Shunkichi Kimura, on promoting the study of quaternions and allied systems of mathematics. I notice that this has not, as yet, been responded to in NATURE. I do not think that the subject should be allowed to drop; but that some permanent good should be done to science by making this branch of mathematics part of the compulsory course of study for students for the highest honours in mathematics in our universities and university colleges, in the hope that more workers may follow the subject up afterwards.

Unquestionably the calculus is of very great value in the higher natural philosophy, and in every sense will repay the trouble bestowed upon it, though I speak in all meekness and not as an eminent authority on the subject. May we hope for some information as to what form the literature of the International Association will take?

G. H. J. HURST.

Eton College, Windsor.

The Colours of Mother-o'-Pearl.

HAD Mr. C. E. Benham given his address when writing to you on this subject (*NATURE*, vol. lii. p. 619), I should merely have taken the liberty of sending to him direct a copy of a paper entitled, “Prof. Blake and Shell-growth in Cephalopoda” (*Ann. and Mag. Nat. Hist.*, ser. 6, vol. i. pp. 421–427, June 1888), in which similar arguments to those of Mr. Benham were adduced. Now, however, perhaps you will permit me to refer Mr. Benham to Dr. W. B. Carpenter's report on shell-structure (*Brit. Assoc. Rep.*, 1844, p. 11). As for the text-book writers, who usually support their explanation of the iridescence of mother-o'-pearl by reference to the theory of