

It has already appeared to me that the object to be attained was not the construction of a lifeship, but rather the fitting of lifeboats with steam machinery, thereby improving their efficiency and diminishing the risk of life, as a boat so constructed could be worked, and that more efficiently, by at most three men, instead of the large number now required to man them. The only means of propulsion which can be applied is, in my opinion, the hydraulic propeller, as, the turbine being enclosed, all risk of fouling pieces of wreck, weed, &c., is thereby avoided. To attempt to use a lifeboat fitted with a screw or paddle would only be courting danger and disaster. Such being the case, the boat designed by me consists of three tubes, the outer ones being circular, and the centre one in which the propeller works being semicircular, and placed underneath the platform grating connecting the two circular tubes. The three tubes would be turned up and unite at the ends, and would somewhat resemble a whaleboat. The peculiar advantage of the hydraulic propeller when applied in this manner is, that the boat could be turned round on its own centre, and sent ahead or astern by the man in charge by simply turning a handle, without issuing an order to any one, an advantage which I need hardly say is of the very greatest moment under such circumstances as those in which lifeboats are usually employed.

The system of towing lifeboats by means of steam tugs to some point as near as possible to the site of the wreck, is one attended with danger, and the lifeboat, when cast off, is deprived of its means of propulsion at the very time when engine power would be most effective in enabling it to contend with the broken water round a wreck. I remember a case at Bombay, when a lifeboat proceeding to a wreck was towed right under, and the Chinese crew swept out of the boat and nearly all drowned.

Tubular lifeboats, I need hardly say, are no novelty, and the addition of a centre tube to carry the propeller and the steam engine and boiler will certainly not diminish their efficiency.

JOHN FELLOWES

Naval and Military Club, Piccadilly, July 3

The Internal Structure of the Earth

ARCHDEACON PRATT'S letter in NATURE for June 22 calls for some remarks on my part. He communicates a few marginal notes written by Mr. Hopkins on a copy of the second part of my "Researches in Terrestrial Physics," which appeared in the "Philosophical Transactions," and seems seriously to regard these curt expressions as judicial utterances beyond which there can be no appeal.

In the first place, I am accused of incorrectly stating the nature of Mr. Hopkins's hypothesis as to the non-existence of friction between the fluid nucleus and solid shell of the earth. The words quoted from my paper as incorrect immediately follow a symbolical expression presented by Mr. Hopkins as the final result of his analysis, and my remark distinctly refers to this mathematical expression, and to nothing else. Remembering that the whole of Mr. Hopkins's mathematical investigations on the internal structure of the earth culminated in the deduction of this very expression, it is well to examine what are the words he uses in the course of his investigations which refer to the existence of friction between the shell and nucleus.

In his first memoir, "Philosophical Transactions," 1839, he says, "and since there will be no friction with the assumed perfect fluidity of the interior matter," p. 386. In his second memoir, I do not recollect that anything about friction is mentioned; but in his third, which summarises the whole of his preceding labours, after presenting the formula already alluded to, he states that it was established on the supposition of "the transition being immediate from the entire solidity of the shell to the perfect fluidity of the mass." He afterwards gives reasons for believing that a stratum of imperfect fluid probably exists between the shell and the perfect fluid, and he further uses the words, "Consequently the assumption made in our investigations of the absence of all tangential action between the shell and fluid will not be accurately true," p. 43. As my remark refers to these investigations and their immediate result, it is unnecessary to say to whom the charge of inaccuracy may justly apply. In affirming the existence of friction between the shell and nucleus to such an extent as to cause both to rotate as one solid mass, friction between the particles of the fluid is clearly implied; for if no such friction existed, the film of liquid touching the shell and moving with it might slip over the remainder of the nucleus.

I have, therefore, been all along at issue with Mr. Hopkins on this point, when I concluded that the rotation of the shell and nucleus must take place as if the whole were solid. Mr. Hopkins declares this conclusion to be "a mechanical impossibility." It is this "impossibility" which has been reaffirmed by M. Delaunay in stronger terms than those I used. It has been shown to be not merely possible, but rigorously true, in a particular case, by an experiment of M. Champagneur, which I have myself recently verified, and it has been further so clearly illustrated in these pages by two correspondents A. J. M. and A. H. Green (May 18, p. 45) as to require no further observation. The coincidence of the axes of instantaneous rotation of the shell and nucleus necessarily follows if the whole moves as a solid mass; and to charge me with implying the coincidence in one of my formulæ is equivalent to charging me with being strictly consistent. On this point Mr. Hopkins is of course at issue with M. Delaunay as well as with myself. The next important question referred to on which I totally differ from Mr. Hopkins is that of the form of the inner surface of the shell. If the shell has been gradually formed by solidification from a fluid mass, it is evident that the rate of progressive solidification at the interior of the shell must depend on the rate of refrigeration of the surface of the nucleus. This takes place, and has probably taken place for ages, at an almost insensible rate of slowness, and therefore also the successive additions of matter to the shell's inner surface. Between the perfectly solidified and comparatively rigid part of the shell and the fluid nucleus, the matter on the point of becoming solidified is probably in a pasty or imperfectly fluid state (as Mr. Hopkins has admitted), and it is this matter which is subjected to a moulding action by the changes of shape of the nucleus, as I pointed out in the publication already alluded to. This pasty matter becoming slowly impressed with the shape of the nucleus, and freely yielding to the impression as it passes to the solid state, the more rigid part of the shell, precisely as the outer case of a mould, is saved from strain, and cannot undergo a corresponding change of figure. In the discussion which followed the reading of my communication to the French Academy of Sciences on March 6, it appears from the *Comptes Rendus* that M. Elie de Beaumont made some remarks which illustrate and support this view of the process of formation of the shell. The conclusion to which I was thus led, that the inner surface of the shell could not be less elliptical than its outer surface, was reaffirmed soon after the publication of my researches by an eminent mathematician, the late Baron Plana, of Turin. All this Mr. Hopkins considers as quite inadmissible, and very reasonably, too, in the opinion of Archdeacon Pratt, and all the results deduced therefrom are judicially pronounced to be "valueless." But my conclusion as to the interior ellipticity of the shell is only a necessary deduction flowing from the fundamental principles from which my inquiries start, a principle upon which I am as much at issue with Mr. Hopkins as upon anything referred to in his marginal notes. As this is the really vital divergence between us, a few words of explanation are desirable.

The hypothesis of the entirely fluid state of the earth anterior to its present state forms the groundwork of mathematical inquiries as to the earth's figure. The problem, as hitherto treated, always involved an additional hypothesis either openly or tacitly implied, namely, that the distribution of the particles composing the earth underwent no change by the earth's transition from a completely fluid condition to its present state. While Mr. Hopkins tacitly assumed this second hypothesis throughout his investigations, I have reason to believe that it was for the first time rejected in my paper on the "Figure and Primitive Formation of the Earth," which forms the first of my "Researches in Terrestrial Physics." By this step we are at liberty to investigate, with the aid of mechanical and physical laws and the known properties of the earth's materials, the probable arrangement and laws of density of the interior strata of the shell and nucleus. In attempting to do so, I was led to conclusions as to the earth's internal structure widely differing from those of Mr. Hopkins. I have great difficulty in believing that the crude comments on my researches communicated to Archdeacon Pratt, could have been intended to meet the public eye. Long before Mr. Hopkins sent these remarks to Archdeacon Pratt, he wrote to me promising to comment publicly upon my conclusions; and since then an opportunity occurred for pointing out in his presence at a meeting of the British Association what I conceived to be the inconclusive character of his results. Mr. Hopkins promised to reply, but neither this

nor his former promise was ever realised. He avoided public discussion, while as it now appears he *privately* depreciated results incompatible with his own. To Archdeacon Pratt I am grateful for producing evidence of the kind of weapon which I had long suspected to have been employed by my distinguished adversary.

HENRY HENNESSY

Dublin, July 1

Oceanic Circulation

MR. LAUGHTON treats an experiment which was only intended to be illustrative as if it had been advanced as probative, and tests it by a doctrine of "thermometric gradients" which does not correspond to the facts of the case. A uniform reduction of the temperature of ocean-water from the Equator to the Pole would doubtless give a "thermometric gradient" of infinitesimal minuteness. But the water of the circumpolar area, on which what Sir John Herschel truly designated the intense action of polar cold is exerted, brings with it so much of equatorial heat that a very decided increase of its specific gravity must be produced by the cooling process to which it is subjected within the polar area. This increase will be adequate, as I have attempted to show, to produce a continuous downward movement of the whole mass of water subjected to the cooling process; and such a movement, however slow, will make itself perceptible in a continuous outflow of the chilled dense water along the deepest floors of the great oceanic basins, and in a continuous indraught of warmer surface water into the polar area. The proof that such is the case seems to me to be afforded by the fact that temperatures not much above 32° seem to be uniformly met with at depths exceeding 2,000 fathoms, even under the equator; a fact of which Mr. Laughton and those who think with him have not, so far as I am aware, offered any account. That there is nothing in depth, *per se*, which produces this depression is shown by the absence of it in the Mediterranean.

It would be difficult, if not impossible, to carry out a probative experiment that should represent the actual conditions of the case. Taking the distance from the pole to the equator at 6,250 miles, and the average depth to which the chilled water would descend at $2\frac{1}{2}$ miles, we should require a trough having a proportion of 2,500 to 1 between its length and its depth, or (in round numbers) a length of half a mile to a depth of a foot. Let it be supposed that cold were continuously applied by a powerful freezing mixture to the surface of the water occupying one extremity of the trough as far as one-tenth of its length, and that heat were applied to the surface of the water occupying the opposite extremity to a corresponding extent, the intervening water being neither heated nor cooled artificially, would, or would not, a continuous circulation from the one end of the trough to the other come to be established? To me it seems that what Sir John Herschel calls the "common sense of the matter" teaches that the continuous descending movement given to the water at the polar end of the trough must in time propagate itself to the equatorial, provided only that the conducting power of the sides and floor of the trough were sufficiently bad to prevent the chilled stratum which falls to the bottom at one end from losing its cold before it reaches the other.

When such masters of Thermotics as Pouillet and Herschel consider that the doctrine of a general oceanic circulation sustained by differences of temperature is conformable to the facts at present known, I would suggest whether it would not be wise if those who are interested in the subject, instead of attempting to controvert their views on theoretical considerations, were to use their endeavours to collect additional data for practically testing them. By the kindness of the Hydrographer to the Admiralty I hope, in the course of the present season, to obtain some further information of a reliable kind; and I am doing my utmost to urge upon our Government a systematic inquiry into what the Secretary of the Scottish Meteorological Society has truly designated (in a recent letter to me) as "the most important problem in Terrestrial Physics."

July 3

WILLIAM B. CARPENTER

I SHOULD need Mr. Laughton's hint if I had ever supposed that the cause of the vertical circulation of the ocean could be determined by such an experiment as I suggested. The experiment was specially intended to throw light on the easterly and westerly oceanic movements. For this purpose it is only necessary that the rate of rotation of the shallow cylinder should be duly adjusted to the observed rate of the vertical motions. But

even in this respect the experiment would afford but an illustration, not a demonstration.

The subject of oceanic circulation is altogether too wide and too difficult for discussion in letters. Every point touched on by Mr. Laughton requires many columns for its full discussion. I just note that the infinitesimal nature of the thermometric gradients scarcely seems a sounder objection to the temperature theory of oceanic circulation than to the temperature theory of atmospheric circulation. In one case, as in the other, we must integrate the effects of the solar light on tropical and subtropical regions.

RICHARD A. PROCTOR

Day Auroras

LAST evening, about eight o'clock, being in the grounds belonging to the Radcliffe Observatory, I was exceedingly surprised at seeing what I *have no doubt* of being true auroral streamers, forming a little to the east of the south meridian, reaching an altitude of about 25° , and after travelling some distance in a westerly direction, vanishing. This lasted at least ten minutes, when the sky, which had been overcast nearly all day again became so. I pointed the streamers out to several people who were near me, some of whom watched them with me, as a proof of what I had before doubted, namely, that auroras are visible by daylight.

JOHN LUCAS,

Assistant at Radcliffe Observatory

Radcliffe Observatory, Oxford, June 28

The Solar Parallax

I REGRET that I have misinterpreted the severity of Prof. Newcomb's remarks respecting my chapter on the Solar Parallax. The fact is, that so far back as February 1 I was warned by an eminent astronomer that Prof. Newcomb had vowed here last November that he would annihilate all who upheld the finality or correctness of Mr. Stone's researches.

Prof. Newcomb must be sensible that his offer to supply information as to the history of inquiries into the solar parallax during the last few years is a very generous one; and that it will be immensely to my advantage to profit by his exceptional familiarity with the subject. I thank him very earnestly. I have an especial distaste for inquiries into the historical parts of scientific subjects, and shall rejoice to be saved the labour of looking up authorities, &c., in this particular matter. If I find my account requires alteration, I shall admit the fact without a particle of hesitation. It is indeed most desirable (though not, perhaps, for students of science, for whom I specially write, and who need trouble themselves little on the matter) that to each worker in the subject of the solar parallax his due proportion of credit should be assigned; and as in this case not only I, but Sir John Herschel, as well as the Council of the Astronomical Society, would seem to have done Prof. Newcomb less than justice, the sooner recantation is made the better.

Prof. Newcomb refers to "the kind spirit in which I have taken his remarks;" meaning rather, perhaps, the appreciative way in which I have spoken of his labours. His critique, regarded as a whole, was not, I take it, kindly meant; and though I by no means feel annihilated by it, I should be speaking untruly if I seemed to admit its justice. If I failed to note how I viewed his comments, it was only because I found a pleasanter subject to speak about in those important researches whereby he has advanced astronomy.

RICHARD A. PROCTOR

P.S.—I take this opportunity of noting that the remark in my former letter respecting the work of Mr. De La Rue and F. Secchi in 1860 must not be understood as implying that the account in F. Secchi's book *Le Soleil* is incorrect. On the contrary, I have no doubt it is strictly accurate. I was fortunate in securing a copy of *Le Soleil* before Paris was beleaguered, and derived considerable assistance from its perusal.

Lee Shelter

PERHAPS it is worth noting that a lee shelter is almost as effectual as a screen to the windward. The fact may be quite well known and understood; but I did not become aware of it till I was on Bognor Pier, when a strong gale was blowing directly on the broadside. There are seats backed and covered overhead and on the sides, alternately, on the one or other side of the pier, and on this occasion all the seats to the windward were occupied, so that, wanting a rest, I had to put up with one