

laquelle j'avais recours est à peu près identique à la sienne. . . . Je m'empresse de satisfaire à cette demande légitime, et je laisse entièrement la priorité sur moi, à M. Pettigrew relativement à la question ainsi restreinte." (*Comptes Rendus* for May 16, 1870, p. 1093.)

The next point which Mr. Garrod takes up is the "induced currents" of the wing. I state that "the efficiency of the wing is greatly increased by the fact that when it ascends it draws a current of air up after it, which current, being met by the wing during its descent, greatly augments the power of the down-stroke. In like manner, when the wing descends, it draws a current of air down after it, which, being met by the wing during its ascent, greatly augments the power of the up-stroke." This is simply a statement of fact, and if Mr. Garrod causes a natural or artificial wing to vibrate he will find that the wing takes a greater catch of the air when a down- and up-stroke or an up- and down-stroke are made in rapid succession, than when a single stroke is made either in the one direction or in the other. This point becomes especially clear if a large artificial wing be constructed on the insect type and made to vibrate in a horizontal direction. If such a wing have its anterior margin slightly elevated and made to travel from right to left of the operator it draws after it a current of air which, being met by the wing when it is reversed and made to pass from left to right, acts as an autumn breeze to a kite. The wing literally flies on the current which it creates. It ascends at each thrust and carries the hand of the operator with it. Similar remarks are to be made of the tail of the fish. It is in this way that the *back air* and *back water* are utilised, and herein lies the excellence of the elastic reciprocating screw, as found in Nature, and as contra-distinguished from the rigid rotatory screw employed in navigation.

Mr. Garrod, adducing no proof in refutation of this and similar experiments, states "that these induced currents are of no real service in flight, because in their production there is as much force lost as there may be gained from their subsequent employment on the reversal of the action of the wing, if the bird's body has not advanced sufficiently far to be in each stroke beyond the range of their action, which is probably the case." On what authority does Mr. Garrod make this assertion? When a bird flies in still air, the wing of necessity must vibrate. The quicker it vibrates the more marked the reaction obtained from the air, and the greater the elevating and propelling power. The induced currents powerfully contribute to this reaction from the fact that the wing and the air are both moving, and moving in opposite directions. This, as explained, is a matter of experiment, and can readily be verified.

Lastly Mr. Garrod attacks my views on muscular movements. Here again he adduces no counter-proof, and, adhering to the old doctrine, contents himself by saying, "We are not ashamed to say that such has always been and still is our idea." This is not saying much. He takes exception to my statement that muscles have a centripetal or shortening power and a centrifugal or elongating power. Can he inform me how the left ventricle of the heart opens after a vigorous contraction, in which all the blood contained in the ventricular cavity is ejected and the ventricle converted into a solid muscular mass, if not by a spontaneous elongation of all its fibres?

Edinburgh, Jan. 27

J. BELL PETTIGREW

Specific Gravity of Sea-water

IN reference to Mr. Strachan's letter in NATURE, vol. ix. p. 183, calling attention to the discrepancy between Dr. Frankland's results and my own, permit me to state that they were not obtained from the same series of samples, and that the figures given by Dr. Frankland were, I believe, obtained by the use of a balance on shore, and also that from the way in which his specimens were packed, they were not liable to any appreciable loss by evaporation. They were not, however, taken from that part of the North Atlantic which was examined during the time that I was on board the *Porcupine* in 1869, to which alone my observations refer. My own results were obtained, as stated on p. 503 of "The Depths of the Sea," by delicate glass hydrometers, so graduated that the sp. gr. could easily be read to the fourth decimal place. Two instruments only were employed for the 105 observations made, and though they gave identical results, I had no opportunity of comparing their indications with the results obtained by a balance from the same specimen of water. I may remark here, however, that though the *absolute* results may not be quite correct, the relations between the sp. gr. of surface, intermediate, and bottom waters, pointed out on p. 505

of "The Depths of the Sea," as well as the range of variation, are probably very near the truth, since the same instruments were employed in all the determinations, and at the end of the series they indicated the same as at the commencement, when placed in a test solution, which was preserved for the purpose of detecting possible variations in the instruments themselves.

Clifton, Bristol, Jan. 17

WM. LANT CARPENTER

THE LINNEAN SOCIETY

WE regret to hear of an unpleasant event which took place at the meeting of the Linnean Society on Thursday last (5th inst.). So far as we have been able to gather the particulars they are as follows.

When the usual minutes had been read at the commencement of the meeting, a Fellow of the Society rose in his place and endeavoured to propose a motion reflecting upon the conduct of the President at the preceding meeting. The President (Mr. George Bentham, F.R.S.) ruled that the Fellow was out of order and that his motion could not be put, and requested the would-be mover of it to sit down in his place. In spite of frequent calls to order, however, this gentleman persisted in his endeavours to bring forward his grievances, and to address the meeting. At last Mr. Bentham, finding that his efforts to preserve order were vain, and that the mover of the motion (who had given no sort of notice of his intentions) was backed up by a body of clamorous friends assembled specially for the purpose, quitted the chair and left the meeting-room, followed by the Secretary and all the other members of the Council present.

As the chair of the Linnean Society can only be taken by a member of Council, the meeting thus came to a premature end, much to the disappointment of those who had assembled to hear Mr. W. K. Parker read his paper on the osteology of the woodpeckers.

We regret to have to add that, in consequence of this untoward event, Mr. Bentham has tendered his resignation as President of the Society. But we trust that the Fellows who caused the disturbance will, upon reflection, feel that however much they might have considered themselves aggrieved by the President's decision at the previous meeting, they were not justified in the course they pursued. In all meetings the decision of a chairman upon a point of order is held to be final, at all events for the occasion. More especially should this be the case in a learned society assembled for the discussion of scientific problems, and not for vulgar wranglings and disputes upon immaterial subjects.

We trust therefore that an ample apology will be offered to the President by these gentlemen, and that he will be induced to retain his chair until the approaching anniversary meeting of the Society, when he had already given notice of his intention not to accept re-nomination. The great services which Mr. Bentham has rendered to Science generally and to the Linnean Society in particular, are too well known to the readers of NATURE to render it necessary for us to descant upon them in these columns. The Linnean Society has just acquired a new and most convenient abode in the apartments at Burlington House, recently provided for it by the liberality of the country, and it would be a great misfortune if disunion should succeed in marring the work of those who are now endeavouring to make the Society still more useful and more prosperous than it has been in past times.

POLARISATION OF LIGHT*

IV.

THE phenomena exhibited by selenite are also produced by other crystals, but the facility with which plates of the former substance can be obtained, causes them to be generally used in preference to others. There is, however, a peculiar class of crystals, of which quartz, or rock crystal, is the most notable, which gives rise to effects different from those hitherto described.

* Continued from p. 205.