Washington, of uniform reports made from simultaneous observations taken daily at as many of the stations under your charge as it may be practicable for you to instruct or request to furnish such reports, or from other stations from which they may be voluntarily tendered, and of similar reports to be taken at the established stations of this Office throughout the United States. The reports to embrace, at least, pressure (reduced), temperature, wind, rain, relative humidity, and clouds, and to be made at 12.43 P.M., Greenwich mean time. The records to be printed or in manuscript, as you prefer, and to be mailed (as many of them as may be ready for exchange on the dates) in packages, on the 15th and last days of each month. Should circumstances render it inconvenient for your Office to furnish such reports without blanks for days on which they will necessarily fail to be taken, the records will be none the less gratefully received. Self-registered records will be very acceptable. In return exchange it is proposed to mail to your Office on the 15th and last days of each month the record of the simultaneous report prepared for that purpose in the form of which the enclosure herewith is a specimen for a single day.

"The data so to be exchanged are intended for any use either Office may wish to make of them.

"As an acknowledgment to those who may, upon your invitation, assist in a work so much wished for on the part of this Office, it is proposed to send to you monthly copies of the 'Official Monthly Weather Review,' with its Maps, for distribution to each of those so assisting, or other papers published by this Office, if so requested.

"In requesting this exchange as a part of a system to which it is hoped a very wide extension can be given, the Chief Signal Officer recurs with pleasure to the prompt encouragement received at your hands at the earlier steps for its adoption, and is gratified to announce that co-operation for similar exchanges of records, commencing on January 1, 1874, has been requested or is in progress with Prof. H. Wild, Director, Imperial Observatory, St. Petersburg; Prof. A. Coumbary, Director, Imperial Observatory, Constantinople, Turkey; Prof. Carl Jellinek, Director, Imperial Observatory, Vienna, Austria; Prof. Quetelet, Director, Meteorological Observatory, Brussels, Belgium; Prof. Buys Ballot, Director, Meteorological Institute of the Netherlands, Utrecht, Holland; and Prof. H. Mohn, Director, Meteorological Institute of Norway, Christiania, Norway. As time and its faculties will permit, this Office will seek additional aid. The advantages to accrue to the service in the United States are certain, and the hope is not unfounded that as the co-operation sought will be world-wide, so also will be the benefits resulting.—I am, &c.,

"ALBERT J. MYER

"Brigadier-General, U.S.A.

"R. H. Scott, Esq."

Remuneration of the Contributors to Milne-Edwards' "Mission Scientifique au Mexique"

EN vous remerciant de l'envoi d'un article (vol. ix. p. 260) relatif aux singulières assertions contenues dans une note de M. Gray, je vous demanderai la permission d'ajouter que ni M. Duméril ni aucun des autres naturalistes qui prennent une part, soit directe, soit indirecte à la publication de l'ouvrage sur la Zoologie du Mexique ne reçoivent pour ce travail une rémunération pécuniaire quelconque. C'est gratuitement et dans l'intérêt de la Science seulement qu'ils s'en occupent; par conséquent les renseignements fournis à mon estimable ami M. Gray, par je ne sais qui, sont faux.

Recevez, Monsieur, l'assurance de ma considération très distinguée.

MILNE-EDWARDS

Paris, ce 13 fevr.

Membre de l'Institut de France, et
Associé Étranger de la Société
Royale de Londres

Animal Locomotion

It is not my intention to go through the detailed proofs of the different statements in my review of his work to which Dr. Pettigrew objects, and which his letter of last week in no way falsifies, nor to show how he has quite missed the point of an observation of mine which he condemns as "utter nonsense," but simply to answer the question with which he ends his remarks. At first sight it might seem that the active dilatation of the

heart during diastole did depend on an inherent power in the muscular fibres of the ventricles to elongate, but the peculiarities of the coronary circulation are quite sufficient to explain the phenomenon without the introduction of so unnecessary a theory as that of Dr. Pettigrew. For in the heart when removed from the body, as in the living body during diastole, the injection of fluid into the coronary vessels causes the whole heart to open up from the congestion of the ventricular walls, and so produce the active dilatation which is well known to occur. This explanation was proposed by Brücke, and by myself some years later (Journal of Anat. and Phys.)

A. H. GARROD

WHILE admitting that Dr. Pettigrew appears to have made mistakes in his figures, and that he has not explained his views in the clearest manner, nevertheless it appears to me that, on the very important question of whether a bird's wing during onward flight moves downward and forward or downward and backward, he is right in asserting the former to be the fact.

The arguments of Mr. Garrod and Mr. Ward against this view seem to be founded on two assumptions—that the wing during its down-stroke is an inflexible plane, and that during its upward motion the quills open so perfectly that there is neither vertical nor horizontal resistance. But every feather of a wing is highly flexible towards its extremity, so that during the down-stroke the whole posterior margin of the wing must be curved up by the pressure of the air, thus forming a highly effective propelling surface owing to the rapid motion of this part of the wing. During the upward stroke the feathers open freely so as greatly to diminish, though not wholly to prevent, downward reaction; but the broad soft web of each quill will be bent down by the rapid escape of air between the quills, and this will necessarily give a forward motion, probably equal to that attained during the down-stroke, in which the small curved surface has a greater resistance and more rapid motion. If then the up- and the down-stroke both produce onward motion, the resultant of this motion will be in the direction of the mean position of the wings, which we may take to be about that of the body of the bird; but if the down-stroke were directed *backward* and the up-stroke *forward*, the resultant onward motion would be obliquely downward, and this downward angle of motion would tend to be so much increased by the continual gravitation of the body that the surplus vertical reaction of the down-stroke over the up-stroke would not be able to overcome it. A slight upward angle of the mean position of the wing-plane seems therefore to be essential to secure horizontal forward motion as a general resultant of the upward and downward action of the wings under the influence of gravitation; and to Dr. Pettigrew belongs the merit of showing that this is one of the most important characteristics of the flight of birds, and, probably in a still greater degree, of that of insects. A bird's wing is a highly complex apparatus, subject to a variety of flexures and motions in every feather; and it is only by a careful consideration of the action of the resisting medium on these variously curved elastic surfaces, both during the upward and downward motion of the wings, that we can arrive at any definite notion of their supporting and propelling effect. The experiments of Prof. Marey do not seem to contradict the theory of Dr. Pettigrew, as far as I can make out from an abstract of these given in the "Ibis" for 1870, p. 267; though, as his apparatus only gave the motion of the wing relatively to the body of the bird, they are not of very much value in determining the absolute angular position of the wings, which is what we want to arrive at. The highly-inclined position of a hovering bird is more to the point, as any less degree of inclination would lead to onward motion.

ALFRED R. WALLACE

On the Variability of the Node in Organ-pipes

THE variability of the node is an unrecognised phrase. Something similar in kind relating to the node will be remembered as having been mentioned by scientific writers in a cursory manner, then set aside as evidence of too doubtful interpretation to call for more extended comment.

From the time of Savart it has been known that the nodal division of the open organ-pipe does not take place at the exact half of the length, that the half nearest the embouchure is the shorter of these "unequal halves"—a contradictory term apologised for yet sanctioned, I believe, by the late Prof. Donkin.

The displacement of the node is perhaps the most significant fact that in the natural history of organ-pipes presents itself to

the attention of the investigator, be he student or teacher. Why it should have been passed by as though its meaning were not worth wrestling for is incomprehensible. Since Savart wrote no light has been thrown on this singular phenomenon, for the explanation which has been afforded (presently to be quoted), cannot be called in any degree satisfactory. In the illustrations of nodal division given in various scientific works there is a puzzling contrariety hardly to be accounted for except on the supposition that our engravers are as niggardly conservative in design as the buried Egyptians, or that the engravings themselves are the cherished heirlooms of our publishers. In one work a representation of the manometrical nodal division after Kœnig will be given, but *carefully* corrected and revised by the aid of a pair of compasses; in another a beautifully accurate copy of the original, so lopped to suit the size of the page that much mental effort and distortion of reasoning are incumbent on the reader in vain attempts to bring the engraving into harmony with the accompanying text, young eyes are mystified, it needs cold "well-worn eyes" to appreciate these fine economies of the publishing art; in another the manometrical nodal positions will be properly defined and, by some negligent inconsistency, on an opposite page an organ-pipe be depicted, admirably exact to theoretical localisation, in direct contradiction of knowledge. Faults of this kind should not be allowed to pass, they weaken faith in the teacher and are harassing to the inquirer.

Kœnig in his own illustrations represents the displacement of the node as it is indicated under experiment, for this one condition of truth to nature had been too often before him in his manometric flames to allow of his disregarding its faithful portraiture. The difference he shows to exist as to position corresponds very closely with that we arrive at by other means, by calculation of scales and by the practical teachings of experimental study of the relations and arrangements of organ-pipes. Of the cause of the displacement Kœnig offers no elucidation.

The following explanation is quoted from Prof. Airy's treatise on "Sound and Atmospheric Vibrations." In the section on open organ-pipes he says:—

"It was found by Mr. Hopkins that the node next the open mouth of the pipe was somewhat less distant from it than that given by theory, or, which amounts to the same thing, that the place where the air has always the same density as the external air is not exactly at the pipe's mouth but somewhat exterior to it."

The extent of the disparity would be but very imperfectly comprehended under this vague delineation. Other authors have attempted explanation, in substance the same as the above, to account for the disparity; the summary of the whole is, that science brings forward no better plea than the surmise of a probable place, somewhat exterior to the mouth, which the air-wave of the *lower half* of the pipe has to attain before it can be properly said to be completed in length. Truly an illogical conclusion if this line of reasoning is carried out. In common fairness the *upper half* of the pipe may claim to be credited with a reasonable amount of wave-prolongation, seeing that at the higher orifice the internal column of air pulsates the atmosphere with far greater vigour than at the mouth, and consequently that for a similar attainment of density the due addition of wave-length would only serve to increase the disparity in relation to the *half below* the node.

A displacement of some sort thus receives acknowledgment although as yet the variability of the node is unsuspected.

The actual extent of the disparity between the "unequal halves" can be ascertained. It is subject to laws of relation as of definite a character as are found in other dynamical problems when the elements of calculation are delicately defined. An approximate estimate will be sufficient for the present purpose. For avoidance of the inconvenient "unequal halves" it will be permitted me to coin two simple terms as more distinctively representative, and to speak of them as super-nodal and sub-nodal.

If a standard open diapason pipe be made for some designed pitch, whatever that pitch may be, it may safely be predicted that the pipe will stand considerably short of the full theoretical length; æsthetically judged for musical quality, it ought to be about one-eighth less, a difference much affecting the veracity of scientific argument.

Doubtless it would be somewhat a novelty for a scientific lecturer to tell his audience that one-eighth of the whole wave-length was lost by conversion into organ-pipe vibrations, yet, unless he innocently accepts the ironical reply of Galileo on the pump question, that "perhaps Nature is indifferent to a few

feet," he is strictly in this dilemma: if the pipe is a natural standard of wave-length, the velocity of sound in air computed on the basis of the pipe's length falls very far short indeed of the philosophical estimate, 1,125 feet per second; on this ruling the latter should be pronounced to be irreconcilably wrong, *or else* the frank admission made that there is no "necessity of relation" that the wave-length in an organ-pipe, giving a defined pitch, and the wave-length in the free air corresponding to that note should be identical.

Taking the several classes of pipes, from the Diapason to the Vox Angelica, ranging from the pipes of the most vigorous to those of the softest intonation, the amount of difference from full measure varies from one-eighth to one-twelfth *less* than that which theory demands. The loss is mainly due to the cause which enforces nodal displacement.

Our immediate inquiry is, what is the extent of displacement of the node, and what its variability? Divide the length of the already reduced pipe into seven equal parts, and the unequal halves will be in the ratio of 4 to 3. Four parts belonging to the super-nodal half, and three parts to the sub-nodal half, subject to a relative variability, according to the position of the pipe in the range of octaves, and subject to a fluctuating variability determined by force of wind, diameter of pipe, character of scale, relative size of mouth, mode of voicing, and other details, changing the proportion, perhaps, to 6:5, or even to 7:6. Whatever the extent of the variability, change in result rigidly follows change in details, with a calculable value. When, instead of the fundamental note, the pipe vibrates in harmonic nodal divisions, the lowest half-segment takes upon itself almost the whole difference, and not merely a proportional share in comparison with its segmental relation to the whole pipe. A remarkable fact, but one fully accounted for in that which I have termed the *aëro-plastic reed theory* (NATURE, vol. viii. p. 25) for it is easy to me visibly to demonstrate that the harmonic-independent and the harmonic-concomitant are originated in the pipe by totally different natural processes.

The nodal difference detected by Mr. W. Hopkins was much smaller in extent, but there is an important distinction not to be overlooked: his experiments (recorded in the Transactions of the Cambridge Philosophical Society, vol. 5) were not made with organ-pipes, but with glass tubes supported in position over a glass plate, the plate being set in vibration by friction. He expressly rejected organ-pipes by reason of their intractability and of the difficulty of obtaining results from them of the nature desired.

In like manner we continually find experimentalists rejecting organ-pipes as insubordinate pupils; they prefer dumb pipes and the artificial speech by tuning-forks, and having obtained such negative evidence, make a clean transfer of their conclusions to all argumentative reasonings and expositions of the nature and functions of the original, living, speaking organ-pipes. The Hon. M. Strutt, in his paper on the Theory of Resonance, printed in Phil. Trans. Nov. 1870, says:—

"Independently of these difficulties, the theory of pipes or other resonators made to speak by a stream of air directed against a sharp edge is not sufficiently understood to make this method of investigation satisfactory. For this reason I have entirely abandoned this method of causing the resonators to speak in my experiments, and have relied on other indications to fix the pitch."

Prof. Airy is as evidently dissatisfied with the state of theory and experiment, using such phrases as these: "the matter, however, demands more complete explanation;" "that obscure subject, the production of musical vibrations in a pipe by a simple blast of air;" "possibly when the mathematical calculus is farther advanced, this may be shown," &c. Beyond the province of mathematical analysis his survey is keen, and with foresight of the results of possible experiments.

At the present date our best authorities are in effect repeating the assertion of Biot that "the particular properties of the vibrations of confined air in tubes are not yet sufficiently explained." The disturbing influence of some unknown agency may be discerned in Dulong's experiments of filling organ-pipes with various gases, and estimating the velocity of sound in these gases by the pitch produced. Similar experiments on this method are referred to by Herschel, and he, noticing how the results gave for hydrogen gas a velocity differing by one-fourth from that which theoretically had been calculated, could only account for it by supposing an impurity in the gas used for the experiments. There is little need to resort to the supposition of

an impurity; it is quite sufficient to know that an agreement in length of organ-pipe and aerial wave-length was assumed which does not exist, and that, moreover, the mechanical nature of the organ-pipe, and its delicate apparatus so wonderfully balanced for the attainment of its ends, had escaped observation. The admirable method of experiment for ascertaining the velocity of sound in gases, devised by M. Kundt, by means of glass tubes and lycopodium seed, is free from the same source of error; and, as might be anticipated, comparison shows a marked difference in estimates. In respect of carbonic acid gas and hydrogen gas, for instance, Dulong differs from Kundt, his estimate in the one case being less by one-fifth of the whole, and the other more by one-fourth; the divergence interprets itself, indicating the relation of their densities to the compelling force, the unseen mechanical action at the mouth of the organ-pipe. This will be clear when the "air-moulded reed" is fully understood in its nature and functions. When the magnetism of the earth is perceived, the dip of the needle to the north or south of the equator in accord with its localisation is explained.

The confession of "obscurity" amounts to a concession that the old theory has been found wanting, that it is inadequate to deal with facts. Whether in dealing with the larger questions here brought into discussion, or with the simpler class, the mere modifications of structure, it is equally incapable. If, for instance, a stopped pipe is pierced through the stopper and a short open pipe inserted, say a third or fourth the diameter and a third or fourth the length, what will be the effect of this on the pitch? The old theory would reply, the added length would cause a flattening of pitch, and then will come a proviso for safety's sake, that if the change converted it into an open organ-pipe then the pitch would be raised in accordance with the open length. We go to Nature for her say in the matter, and find that the pitch is raised not flattened, and that the extent is about a quarter of a tone, and that *further lengthening* of the smaller pipe takes back the pitch again just its quarter tone. If another stopped pipe is drilled at the back with a hole of a diameter a third or fourth of that of the pipe, but so that it shall be at a higher level than the lip or edge of the mouth, in effect shortening the air column by admission of external air at a higher point, what will be the result? On the old theory we should expect the pitch to be higher in consequence. Appealing to the ear we know that, on the contrary, it is flattened. These results cease to be anomalies when viewed under the new theory, and indeed they would be predicted with confidence as the necessary outcome of the conditions.

The proposition that in an organ-pipe there is no constant wave-length for an ascertained pitch, will no doubt be discountenanced as novel and revolutionary, but it is true and will have to be acknowledged. A further proposition that in an open organ-pipe there are three different velocities speeding at different rates, concurring in every vibration, and essential to the synchronic time of its note, has a still more aggressive aspect defiant of law. Not so. It is because law—"known law," does not cover the facts, is unstable in its applications, and is deficient in prevision, that there is room for new hypothesis which does not play fast and loose with nature; the utmost exactitude of length in an organ-pipe is as indispensable in this as in the older theory, but the relation is one of proportion to a system, and the least and every variation will make imperative suitable or corresponding modifications in other portions of the structure. Only a whistle, yet with more to marvel at for delicacy of organi action and beauty of adaptation "than is dreamt of in philosophy."

As regards "fixity of wave-length," that characteristic reappears in a new relation, and we shall find that, allowing for retardation by friction, the super-nodal half-wave of the pipe corresponds very closely with length in atmosphere. The cause of the displacement of the node is involved in the physical action taking place at the mouth of organ pipes, the consideration of which is reserved for a further communication.

HERMANN SMITH

Auroral Display

As a few remarks on the aurora of the 4th may be of interest to some of your meteorological readers I append the following notes:—

At 6.15 P.M. on Wednesday, the 4th inst., an aurora commenced in the northern part of the sky which gradually went down towards the south.

7.15.—Semicircle from W. to E., streamers shooting up from it.

- 7.25.—Light more diffused, a few streamers at N.W.
- 7.30.—A semicircle of diffused light from W.S.W. to E.
- 7.35.—Bright line of light from W.S.W. to E.; no streamers.
- 7.40.—A very faint irregular line of light from W.S.W. to E.
- 7.45.—Diffused light.
- 7.50.—Same as at 7.45.
- 7.55.—Streamers shooting down from zenith all round. Very fine.
- 8.—Bright at N.N.E. Streamers N. and N.N.E. A sharp S.E. breeze.
- 8.5.—Bright light at N.W. No streamers.
- 8.10.—Streamers at N.E.
- 8.15.—Streamers at S.S.E.
- 9.—No aurora perceptible.

From the above, we note one peculiarity, namely, that the aurora was chiefly in W. + E. or W.S.W. and S.S.E.

WILLIAM H.V. WATSON

Braystones, near Whitehaven, Feb. 9

[We have received letters concerning this aurora from several other parts of the country.]

THE PHOTOGRAPHIC SOCIETY

ALTHOUGH we published last week a letter from Mr. Baden Pritchard, Hon. Sec. of the Photographic Society, impugning the justice or accuracy of our strictures on that Society, our esteemed correspondent has not caused us to change our opinion.

We have now before us the Journal of the Society for the past year (a summer vacation of three months excepted), and certainly it furnishes *prima facie* evidence of the most apathetic and inefficient condition which is consistent with continuous existence. The numbers contain eight pages each, the page little more than half the size of that of NATURE, and in the whole year's proceedings there are twelve pages devoted to science, half of this being a lecture by Prof. Stokes; three or four papers of considerable value on technical points of photographic interest, and much which the charity of any semi-learned society would be largely strained in giving paper and ink to.

There is no mention of scientific or other committees, no provision for them in the laws, no reports of investigations made or to be made, no notice of scientific discovery abroad or recognition of discovery at home. Mr. Pritchard has no need to assure us that the body "does not profess to be a purely scientific one"—the scientific element in it, so far as its own record shows, is purely fortuitous.

But without demanding scientific labours from a body not "purely scientific," we do not even find evidence of common activity in the research of practical problems, and if any of its members are, as Mr. Pritchard suggests, engaged in researches on the process and nature of film best suited for transit of Venus observations, they have not had faith enough in the countenance of their Society to place their labours before it, or ask its assistance in performing them.

Since our article appeared, the revolution alluded to has taken place, and that part of the Society in favour of reform having a majority at the meeting appointed for the discussion of the question, have carried an amendment to the laws providing that henceforward the Society at large shall select its council, and that the majority of the actual council shall not have the power to select for retirement such members as it sees fit and to decide who shall replace them, as has actually been the case hitherto; it has also been decided that the presidency shall rotate. These measures were, as we learn from the photographic papers, strongly opposed by the council, and upon being carried by a majority of 30 to 23 (the council itself voting in the minority) the entire body resigned.

As the meeting at which this stroke of singular policy was made, was that for the election of the new members of council, these were enabled to assume the reins of government and prevent the, otherwise in-