

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 Jelinek, 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.,

"R. H. Scott, Esq."

"ALBERT J. MYER
"Brigadier-General, U.S.A.

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.

Paris, ce 13 fevr.

MILNE-EDWARDS
Membre de l'Institut de France, et
Associé Etranger 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 in the natural history of organ-pipes presents itself to