(*b*) WYMAN (J.) [Remarks on a duck's wing, etc.] *Proceedings Boston Society of Natural History*, v. 1855, p. 169. [As I say in my 'Bibliography,' *Bull. U.S. Geogr. Surv. Terr.*, v. 1880, p. 952, this is a paper "showing mechanism of flexion and extension, contributing to fixity of the limb, independently of muscular action." Wyman evidently discovered it himself, and was ignorant of Bergmann's discovery.]

(*a*) BERGMANN (Dr. C.), 'Ueber die Bewegungen von Radius und Ulna am Vogelflügel,' Müller's *Archiv f. Anat. u. Phys.*, vi. 1839, pp. 296–300. [This is an important, interesting, and so far as I know a novel paper on the peculiar mechanism of the fore-arm of birds, before mentioned in none of the works of Meckel, Cuvier, Tudemann, Wagner, etc. The sum of his paper is, that sliding motion lengthwise of the bones, whereby extension of the fore-arm upon the arm, and flexion of the same, respectively reproduce the same movements at the wrist.]

The last four paragraphs are extracted from my 'Bibliography of Ornithology,' most of which is still unpublished.

It is fortunate that the mechanism of the wing does not permit the primaries to lock in the manner that has been supposed, for, if it did so, birds could not fly.

One point more, and I hasten to conclude remarks that I wish were not necessarily so ungracious. The 'fixing of the wing' of a mortally wounded bird, in the manner described by Professor Newberry, does not bear on the case. It is simply a muscular rigidity, due to nervous shock, and of a part with the convulsive muscular action which, under similar circumstances, results in the well-known 'towering' of hard-hit birds. ELLIOTT COUES.

Smithsonian Institution, Washington, Dec. 21.

THE recent discovery of the power possessed by soaring birds to set their wings when fully expanded, and to remain locked independent of muscular action, explains to my mind a phenomenon that has puzzled me for many years. It has been my custom for many seasons to spend a few days each fall duck-shooting at the lakes bordering the Illinois River in central Illinois. The birds were almost invariably shot in mid-air, while flying rapidly by, and often, when not killed at once, they would set their wings and sail gradually down to the water or ground, which they would reach dead, the distance being from one hundred yards to a quarter of a mile, apparently corresponding to the height of the bird when shot. And it was a maxim with duck-shooters on these lakes, "That bird is killed, for he has set his wings."

Besides the ducks, I have seen this phenomenon illustrated in the wild turkey and prairie-hen. In wing-shooting the wild turkey, if it set its wings, and gradually came to the earth a quarter of a mile or more away, we always marked the spot, well expecting to find the dead body when we reached it. With Mr. J. S. Newberry, I trust that some student of anatomy will take up this subject, and demonstrate it to a certainty. W. S. STRODE, M.D.

Bernadotte, Ill., Dec. 22.

Eskimo and Indian.

CONSIDERING the intimate knowledge of the Eskimo language possessed by the two gentlemen who have passed their criticisms upon my remarks on the subject of the past relations of the Eskimo and the Indian, it would be of little avail for me to enter into any lengthy argument upon the matter, although I still consider that there is room for difference on many of the points raised. On a later occasion, I intend elsewhere to treat the subject, both in its ethnographic and philological aspects, on somewhat broader lines than in the article referred to. The evidence in favor of some relation in the past between the Eskimo and the Iroquois seems to me to be convincing, aside altogether from philological data. Kohlmeister and Kmoch (p. 37) state that there is a legend among the Eskimo that the "*Greenlanders* originally came from *Canada*, and settled on the outermost islands of the coast, but never penetrated into the country before they were driven eastward to Greenland." Dr. Brinton (in his *Myths of the New World*, p. 24, note) says, "It is curious that the traditions of the *Tuscaroras*, who placed their arrival on the Virginian coast at about 1300, spoke of the race they found there (called Tacci or Dogi) as eaters of raw flesh, and ignorant of maize." Dr. Rink (*Tales and Traditions of the Eskimo*, p. 11) has the following interesting passage *in rem*: "In the most remote ages the Eskimo, on their trading expeditions, appear to have overpassed their present southern limits. This may be gathered partly from *pure Eskimo words* being found in the language of more southern tribes, partly from the sagas of the old Scandinavians, who seem to have met travelling Eskimo, even to the south of Newfoundland." With regard to the general subject, M. Petitot ('De la prétendue origine orientale des Algonkins,' *Bull. Soc. d'Anthrop. de Paris*, vii. p. 248) expresses himself thus: "Ce qui est bien certain c'est que les *Inini* ne sont pas sans posséder de nombreux rapports de moeurs, de coutoumes, de physionomie, de traditions, et *même de langue* avec leurs voisins les *Pieds-Noirs* les *Tetes-Plates*, et même *avec les Esquimaux*." Elsewhere the same writer observes, "Il n'ai pu trouver dans l'esquimau du Mackenzie un seul mot qui provînt de l'idiome *dènè-dindjié*. Il aura plus de corrélation grammaticale avec le *cris*, dialecte algonquin . . . si dans cette langue les pronoms ne précédaient aussi la racine verbal comme en *déné*, au lieu de la suivre. La consonnance des mots y est à peu près la même. Dans les deux langues on remarque quantité de mots commençant par une voyelle et terminés en *ak*, *ik*, *ok*, *in*, *it*" (*Vocab. Français-Esquimau*, Introd. p. v.). This, to be sure, may not be strong evidence, but it points in a certain direction. From a comparative study of the Eskimo and Iroquois-Algonquin languages, it is certain there is much to be learned. If I have not succeeded in proving, from philological evidence, relations in the past between these people, I can only wait until others shall have done so. Mr. Murdoch has referred to the lack of phonetic vocabularies, and the errors consequent upon the use of such as are at present available. Surely, all the blame cannot be laid upon investigators, who endeavor to do good work with poor material. A glance at the 'Eskimo Bibliography,' lately compiled by Mr. Pilling, is sufficient to convince one that a very great portion of Eskimo linguistic material (and presumably the most valuable, because the most recent and scientific) is still in manuscript in the Library of the Bureau of Ethnology and other great institutions. When this shall have been published, and so distributed throughout the continent, so as to insure facility of access to students, then, I trust, the evidence of past relations between the Eskimo and Indian will be forthcoming, and the fact of their occurrence be capable of proof on scientific grounds. Elsewhere I have discussed the broad question of the pre-history of the Eskimo race, judging them to have been the dolichocephalic people who formerly extended over a great portion of the North American and perhaps of the South American continent. They have been intruded upon and pushed back by more warlike and aggressive races. Not a little interesting is the remarkable correspondence of the Botocudos and other South American tribes in many respects to the Eskimo; and the same remarks apply to some of the so-called 'fossil-men' of Brazil.

A. F. CHAMBERLAIN.

Toronto, Dec. 17.

Weather-Predictions.

PERHAPS it can hardly be said that there is a science of weather-prediction at the present time; yet interest in the subject is increasing, and there are several persons in this country who are issuing daily scientific forecasts. While the basis upon which forecasts shall be issued admits of little discussion, yet it is far otherwise with their verification, and it would seem that much confusion has arisen on this account. The following comparison of weather-forecasts is given with the hope that others will enter the field outlined, and that a general discussion may clear up some of the misty points. The forecasts were made during October for Boston, Mass., by Mr. Clayton at Blue Hill, and by the writer at Washington, D.C. The predictions were for 'fair,' 'rain,' and halfway between, or 'threatening.'

The verifications were to be by the observations at Boston, made at 7 A.M., 3 and 10 P.M., each day. As there was no specific record of 'threatening,' the amount of clouds was to determine this condition. The prediction was made at Blue Hill at 2 P.M. each day from an examination of the Signal Service observations made over the country at 7 A.M., together with a study of the local conditions at 2 P.M. The Washington prediction was necessarily made from the 7 A.M. observation alone. The interval predicted for was from

midnight of each day to the succeeding midnight. The following table exhibits each prediction and the weather that followed:—

Prediction.		Weather.				Prediction.		Weather.			
(1)	(2)	7	3	10	rain	(1)	(2)	7	3	10	rain
rain	rain	cloudy	cloudy	clear	.05	fair	fair	clear	clear	clear	0
rain	rain	fog	clear	cloudy	.02	fair	threat.	fair	cloudy	cloudy	.01
rain	threat.	cloudy	fair	clear	0	fair	threat.	cloudy	clear	clear	0
fair	threat.	clear	fair	cloudy	0	rain	fair	clear	cloudy	cloudy	0
fair	threat.	clear	fair	fair	0	rain	rain	lt. rain	cloudy	cloudy	.96
fair	fair	clear	fair	fair	0	fair	fair	clear	cloudy	cloudy	0
fair	fair	cloudy	cloudy	cloudy	0	fair	fair	clear	fair	clear	0
rain	rain	lt. rain	cloudy	cloudy	.76	fair	fair	cloudy	cloudy	cloudy	0
fair	fair	fair	fair	clear	0	rain	threat.	clear	clear	cloudy	0
fair	fair	fair	cloudy	clear	0	fair	fair	fair	cloudy	cloudy	0
fair	fair	clear	cloudy	clear	0	th.	fair	cloudy	cloudy	fair	0
fair	fair	clear	clear	clear	0	rain	threat.	cloudy	clear	fair	0
						th.	fair	fair	cloudy	fair	0

It will be seen that the prediction was the same in fifteen cases, and eleven of these were fully verified. In order to obtain a fair comparative estimate for the remaining ten days, the predictions and the succeeding weather were referred to Prof. I. Russell, who decided that No. (1) agreed better with the weather twice, and No. (2) eight times. If these ten be regarded half verified, we shall obtain for No. (1) 48 per cent and No. (2) 60 per cent.

The predictions were also referred to Professor Upton, who suggested two schemes for verification, by one of which he computed No. (1) 67.2 per cent, and No. (2) 69.6 per cent; and by the other, No. (1) had 61.0 per cent, and No. (2) 65.0 per cent. As Professor Upton preferred the second scheme, I give it in detail. His plan was as follows:—

Arrange all possible weather-combinations in a table, and give to each prediction a certain weight according to its position in the table, as follows:—

Weather.			Predictions.		
			fair	threatening	ra
clear	clear	clear	3	0	0
clear	clear	fair	4	1	0
clear	fair	fair	4	1	0
fair	fair	fair	4	1	0
clear	clear	cloudy	3	2	0
clear	fair	cloudy	3	2	0
fair	fair	cloudy	3	2	0
clear	cloudy	cloudy	2	3	1
fair	cloudy	cloudy	2	3	1
cloudy	cloudy	cloudy	1	4	2
trace	of	rain	0	3	3
	rain		0	2	4

In this scheme it is possible that too much weight has been given 'fair,' and too little 'threat.' However, as the prediction 'threat.' seems of doubtful utility, it should have less weight.

This discussion has brought out one fact of great interest regarding methods of verification. Mr. Clayton verified the same predictions by the observations at Blue Hill, a station very near Boston. He makes the percentage 85. This great difference of 24 per cent seems very surprising, and can hardly be due to the difference in weather at the two places. It seems probable that this difference is due to the method of verification, and that a mere percentage obtained from an arbitrary verification cannot be relied on for comparing the relative merits of two predictions. It is to be hoped that a further discussion of this question may lead to clearer light and understanding of the methods of prediction and verification best suited to the needs of the public.

H. A. HAZEN.

Washington, D.C., Dec. 14.

The Chinese Wall.

THE note on the Chinese wall in a late issue of *Science* (x. No. 253), calling attention to Abbé Larrieu's assertion that the wall does not exist, recalled to mind Abbé Huc's account. Turning to it, I find that he was a believer in it, and with good reason. In Vol. II. of his 'Journey through Tartary, Thibet, and China,' p. 31, he gives the following account, which may interest some of your readers, and serve to correct an erroneous impression:—

"The part of the wall immediately to the north of Pekin . . . is really fine and imposing; but it must not be supposed that this barrier is equally large and solid throughout its whole extent. We have had occasion to cross it at more than fifteen different points, and have often travelled for days together without ever losing sight of it; and instead of the double battlemented stone wall which is seen at Pekin, it is sometimes a very humble-looking wall of clay; and we have even seen it reduced to its simplest expression, and composed only of stones piled up together."

Thus, though the wall may not and does not have the magnitude and solidity often attributed to it, yet in one form or another it certainly seems to exist, and is not, as we are told Abbé Larrieu says, 'a huge Chinese lie.'

JOSEPH F. JAMES.

Miami Univ., Oxford, O., Dec. 20.

Tornado Force.

I SEND you some facts in relation to tornado force and its peculiarities of action, which may not be uninteresting to your readers, on either side of the question, involving the nature of the force or forces.

The tracks examined by me did not present continuous lines of destruction, but areas of destruction separated by intervals entirely or almost entirely exempt from destructive forces, from which it is inferred, that while the storm, in its common and ordinary features, pursued its way steadily onward by bodily transferrence, the tornadic action was developed interruptedly, and progressed by successive transplantings.

The first area examined, tornado of April 23, 1883, was composed of two distinct parts. The first was a long rectangular space of about half a mile in length, from west-south-west to east-north-east, and a hundred and fifty to two hundred yards in width. Within this space the trees were prostrated from south-east, south, south-west, and west, and intermediate points; and, wherever two or more were found lying across each other, the one thrown from the direction nearest to east, or farthest round from west, was always at the bottom. Thus, those thrown from south always lay on top of those from south-west, those from south-west on top of those from south and south-east, and those from west were always on top of all other directions. This order was without an exception. The rectangular area terminated at the east end in an irregularly circular area of about eight hundred yards diameter, either east and west or north and south. Bisecting this area both ways, and dividing it into four quadrants, the south-west and south-east were found to correspond in all respects with the rectangular area, except that in the south-east there was a greater proportion of trees thrown down from east-south-east and south-east than in the other sections; and in the south-west quadrant, near the centre, a tree thrown from south-west was overlain by one from south, the single exception to the order noted above. In the north-east quadrant the destruction was less than in either of the others, and trees were thrown down from east, north-east, north, north-west, and west. In the north-west quadrant the trees were thrown from north, north-west, and west, chiefly from north-west, west-north-west, and west; and in the instances where they crossed each other, the order in relation to the west was similar precisely to that of the other parts, progressing from east round by north to west, as, on the other side, the progression was from east round by south to west; so that in these, the north-east and north-west quadrants, trees thrown from north-east lay under those from north, those from north under those from