

as we know it in the London Basin. This will be seen on the publication of a paper which was sent in to the Secretary of the Geological Society on October 10 last, but has not yet been put down by the Council for reading.

A. IRVING.

Wellington College, Berks, December 2.

The Ffynnon Beuno and Cae Gwyn Caves.

MR. WORTHINGTON G. SMITH's letter in NATURE of December 1 (p. 105), is so misleading that I hope I may be allowed to reply to it. As is usual with highly prejudiced observers, he has attempted to prove too much for his case, as he might have seen had he taken the trouble to refer to my papers. The scraper which he mentions was submitted to Dr. John Evans for his opinion, and his conclusion as given in my paper in the Proceedings of the Geologists' Association, vol. ix. p. 17, is as follows. The scraper "is not of a river-drift form, so far as at present known, but is precisely like many from the French caves of the reindeer periods, such, for instance, as La Madelaine." Mr. Worthington Smith's contention therefore that it agrees exactly with "the Neolithic scrapers of Icklingham and Mildenhall" can only prove that there is no chronological value in the classification of such implements. I must explain, however, that we have based no argument on the scraper referred to, since it was found, before the explorations were properly commenced, in an open part of the cavern, and, as stated by me in the paper referred to, "it would be improper to dogmatize on this evidence." I may say at once that I entirely demur to any classification based on the form of the implements rather than on the fauna associated with them, and I see no reason whatever to suppose that the worn, roughly-trimmed implements usually found in river gravels are older than the better-preserved flakes and trimmed implements found in caverns, which would be used for a different purpose from the rougher ones. The implements discovered subsequently belong to the so-called oldest types found in caverns, and were associated with Mammalian remains, equally characteristic of the oldest river gravels as of the caverns. Mr. Smith's statements in regard to the drift "in front of the Denbighshire caves" are of so extraordinary a character that I am tempted to ask him, before I criticize those statements, whether he ever visited the Ffynnon Beuno Caves during the course of the explorations, whether he ever saw the section of the drift exposed at the Cae Gwyn Cave, and what evidence he can bring forward to support his statements that the drift "is not in its original position, but distinctly and obviously relaid," and that he doubts "whether before it was relaid it was a true Glacial gravel at all?" I think the members of the British Association Committee, who have carefully conducted the explorations, and have the strongest evidence in support of their conclusion that the caverns, which are now about 400 feet above sea-level, were occupied by man and the animals before the marine drift and boulder-clay covered them over, have a right to ask for the data upon which such statements as those above referred to are based. These relate to facts, and must be dealt with in a different manner from those statements which are made clearly from a bias against the idea of Glacial and pre-Glacial man. Mr. Smith says that he has not been able to read up the literature of the subject, therefore he is probably unaware of the fact that Prof. Prestwich has recently (Quart. Journ. Geol. Soc. for August last) stated that he has arrived at the conclusion that the high-level gravels, with implements, in the valleys of the Somme, Seine, Thames, and Avon date back to Glacial or pre-Glacial times; and that "the great masses of gravel in the neighbourhood of Mildenhall and Lakenheath, also containing flint implements, are certainly not of fluvial origin"; and that they seem to him "to be part of the phenomena connected with the passage of the great ice-sheet over the eastern counties, and in that sense pre-Glacial."

HENRY HICKS.

Hendon, December 2.

Cloud Movements in the Tropics, and Cloud Classification.

A FEW months ago I called attention to the fact that the general movement of the upper clouds in the tropical regions of the Atlantic was from a westerly point; since then I have worked up all my observations (which extend over a period of

331 days spent in these regions in all months of the year except June) with the following results:—

Between latitudes	Upper layer of cloud comes from	Middle layer of cloud comes from
N. 23° and 17°	S. 67° W.	S. 45° W.
N. 16° " 11°	S. 56½° W.	S. 83° W.
N. 10° " 6°	S. 1° W.	S. 17° W.
N. 5° " 0°	N. 41° W.	N. 35° E.
S. 1° " 5°	N. 32° W.	N. 78° E.
S. 6° " 10°	N. 45° W.	S. 58° W.
S. 11° " 15°	N. 53° W.	N. 16° W.
S. 16° " 23°	S. 86° W.	N. 55° W.

Taking a general mean for the whole region, this gives for the upper layer of clouds N. 86½° W., and for the middle layer of clouds S. 73° W. These results are from observations taken by myself, and no observation was registered if there was the slightest doubt as to the cloud movement. The ordinary ship register of upper cloud movements is worse than useless, a propagatory movement of the upper clouds being constantly mistaken for their real movement, and the names being hopelessly mixed, the cirro-cumulus being the source of most mistakes.

The cirro-cumulus exceeds all other forms of cloud in extent, ranging from the delicate fine mottles at a great elevation to the large flaky masses quite low down, and until it is considered a middle layer cloud we are certain to have some confusion.

It is quite time that cloud classification was placed on a more satisfactory basis. Now one observer will call a certain form of cirro-cumulus, a cumulo-cirrus; a moderately high (middle layer) stratus of uniform texture, a cirro-stratus; again, one form of low stratus, a pallio-stratus. Another observer will even call a detached fragmentary stratus, cirro-cumulus; and lots of observations will be useless from one observer failing to understand the particular form of cloud A calls pallio-stratus or B calls cirro-cumulus. Far better to keep to Luke Howard's simple nomenclature till some classification is definitely fixed to which all can agree.

To be satisfactory the classification must be founded on the physical and morphological (if I may use the word here) structure of clouds. I find no difficulty in making observers understand the difference between a stratiform and a cumuliform cloud; this is the first step, and once the distinction is thoroughly grasped the rest is comparatively easy. I propose something of this sort. Two orders, the "Stratiforms" and the "Cumuliforms," these to be subdivided into types, and these again into species; e.g. taking the ordinary dull-looking stratus commonly seen in anticyclonic areas, it would be described as—

Order	...	Stratus.
Type	...	Low-stratus.
Species	...	Pallio-stratus.

Or take that form of cirrus which appears as lines or threads right across the sky; it would be described thus—

Order	...	Stratus.
Type	...	Cirrus.
Species	...	Cirro-filum.

By using this system an observer would be gradually brought to recognize first the broad distinctions and then the minute distinctions in clouds.

DAVID WILSON-BARKER.

THE FORMS OF CLOUDS.

SO much attention has been given of late years to the study of clouds, and so many names have been suggested by different writers for the same form of cloud, that the whole question of cloud forms and cloud names must soon be referred to an International Congress. A few remarks on certain broad facts connected with the shapes of clouds, and on the fundamental principles by which weather forecasts are deduced from these forms, may therefore be acceptable to those who have not given special attention to the subject.

The two most important facts which must never be forgotten are: (1) that cloud forms are essentially the same all over the world; and (2) that there are only five or six distinct structures of clouds.