

the parents are so remotely related that they are separated by seven or more "free generations," the chances are (unless one of the parents is highly prepotent) that, notwithstanding the great merits of the immediate ancestors and the expectations of Mendelians, the offspring will revert to mediocrity. Hence it is not enough that breeders should "mate the best with the best, avoiding close affinities"; they must avoid crossing distinct strains even when the members of one strain closely resemble those of another. This implies that, in addition to knowing the pedigree of their stock, breeders should know as much as possible of the wild races from which modern varieties and strains were originally derived.

J. C. EWART.

Arctic Plants from the Vall-y Gravels of the River Lea.

I HAVE recently found a plant-bearing bed in the Low Level River-Drift of the Lea valley at Ponder's End. It is exposed in an excavation worked by the Great Eastern Railway Company, and I am indebted to Mr. Horace Wilmer, engineer to the company, for permission to carry on my investigations.

The plant-bearing bed is found at a depth of 14 to 18 feet below the surface. It is embedded in stratified gravel and sand, which presents much evidence of tumultuous accumulation. In immediate association with it are found tusks, teeth, and bones of the *Elephas primigenius*, *Rhinoceros antiquitatis* (if we are no longer permitted to call it *tichorhinus*!), and other Mammalia.

The pit is situated on the present floor of the valley of the Lea at a level of about 35 or 40 feet above the Ordnance datum.

In correlation with the archaeological stages, the plant-bearing bed of Ponder's End is later than the Mousterien epoch. In fact, it is separated from this epoch by such a wide interval that it is in all probability post-Palaeolithic. On the other hand, it is unquestionably pre-Neolithic, although the interval in this case appears to be comparatively short. There is thus little doubt that it comes within the period of the archaeological hiatus between the Palaeolithic and the Neolithic ages.

It is by far the most important plant-bearing bed that has hitherto been found upon this horizon within the area occupied by Palaeolithic man in this country. It occupies a position not represented on the well-known sites of Hoxne or Hitchin. The only bed, so far as I am aware, which can be placed on the same horizon is that at the Admiralty Buildings, Westminster. This, however, only yielded two species of plants, one of them being the Arctic form *Betula nana*.

I am at present engaged upon working out the botanical material from this bed—a laborious task, occupying a large amount of time. I am submitting this to Mr. F. J. Lewis, who has very kindly undertaken its identification. A considerable amount of material has already been examined, and, so far, Mr. F. J. Lewis has succeeded in identifying ten species of plants, with four others doubtful. Three of this number, namely, *Salix herbacea*, *Betula nana*, and *Sibbaldia procumbens*, are distinctively Arctic, while most, if not all, of the remainder have a high northern range, although they are not confined to those regions. Mr. F. J. Lewis defines the assemblage as Late Glacial.

The researches of Mr. Clement Reid in beds associated with the Palaeolithic deposits have shown that there have been many oscillations of climate in the south of England since the deposition of the Chalky Boulder Clay. With the evidence of this new bed before us there can be no doubt that the Palaeolithic age was closed by a partial return to glacial conditions, succeeding an epoch, or epochs, when temperate conditions prevailed. This conclusion is in agreement with the results of recent work upon the mammalian fauna of the Pleistocene age.

This is not the place to enter further into this discussion. Enough has been said to indicate the importance of this bed in throwing further light upon the climatic changes of the Pleistocene age. It certainly suggests that the archaeological hiatus is to be directly associated in the south of England with a final return of glacial conditions of climate.

If this view be sound, as I believe that it is, it seems to

be perfectly justifiable to define Palaeolithic man as interglacial, even although the last glacial phase above indicated could not, of course, compare in severity with those which preceded it.

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A New Theory of the Descent of Man.

It is probable that some readers may fail to appreciate Prof. Klaatsch's "New Theory of the Descent of Man" at its proper worth owing to the technical terms and obscure descriptions used in the account published in NATURE of November 24 (p. 118). The theory is simply this. The Neanderthal man and the gorilla have continuous supraorbital ridges and similar markings for the insertion of muscles on their skeletons; the Aurignac man (who could pass as a fairly high type of modern humanity) has not a continuous supraorbital ridge, in which he presents a very superficial resemblance to the orang, and has certain muscular impressions on his skeleton somewhat similar to the orang's.

On this basis, which must be admitted to be "flimsy" in the extreme, Prof. Klaatsch builds his new theory and supposes that the gorilla and Neanderthal man are co-descendants of one branch, the orang and the Aurignac man of another. If one were to apply the principles used by Prof. Klaatsch to the canine in place of the human world, then we should say that the rough-haired Newfoundland is a co-descendant of a rough-haired bear, while the smooth-haired mastiff has arisen with the sleek leopard. An explanation is thus given of the points in which the Newfoundland and the bear, the mastiff and the leopard, have in common; but what of the hundred characters which the Newfoundland and the mastiff possess in common, and which separate them from the bear and leopard? Prof. Klaatsch ascribes these to "convergence phenomena." At least that is how he accounts for the fact that the Neanderthal and the Aurignac men have all the features common to humanity; one arose *via* the gorilla and the other arose *via* the orang, but both arrived at the same structural goal so alike that most of us regard them as the same species.

The theory, owing to the demand it makes on "convergence phenomena," passes somewhat beyond the limits of rational speculation. Prof. Klaatsch's theory has failed to gain the support of his able colleagues in Germany, and is not likely to receive serious consideration in this country.

A. KEITH.

Royal College of Surgeons, December 10.

The Cocos-Keeling Atoll.

IN reply to Mr. Wood-Jones's letter (NATURE, December 1), I would say that I still consider that his arguments against Sir John Murray's theory go in support of it.

Mr. Wood-Jones suggests the reason for the precipitation of calcium carbonate, when it has once begun, going on until the solution contains less than the normal quantity; what I wished to emphasise was that precipitation does not begin until more calcium carbonate than is normally present first passes into solution, *i.e.* that no crystals can be formed in the interstices of the massive corals in the lagoons until some of the dead coral is dissolved. There is, therefore, *proof* of solution in the lagoons of atolls.

In an early discussion on the same subject Sir John Murray pointed out that the processes of the solution of the carbonate of lime of dead shells and skeletons by sea water, and of its secretion by the living organisms, are going on side by side wherever there are life and growth, death and decay. In some regions secretion is in excess, and there is a formation of calcareous deposits; in others solution is equal to secretion, as in the red clay areas of the ocean; in others solution may be in excess of secretion, as in the larger and more perfect coral lagoons.

In small coral atolls the periphery is large relatively to the size of the lagoon, and the secretion of lime and the formation of coral sand are greatly in excess of the solution that takes place, hence the lagoon becomes filled up.