winds over the Arctic basin do not move outward, but, on the contrary, inward. The wind components throughout have been computed by Mohn, and these appear strikingly set forth upon two of his charts.³ Evidence has piled up along the same direction from stations on the Arctic coasts of Siberia and British America for the long winter season, during which winds from the north are accompanied by elevation of temperature and those from the south by a fall of temperature. Kindle, with much personal experience in Arctic America, has rendered a service through assembling the observations to prove this generalisation.⁴

A more complete answer to the objections raised by Mr. Bonacina will be found in a monograph now in press and soon to be obtained through the General Library of the University of Michigan.⁵

WILLIAM HERBERT HOBBS.

The Palæolithic Drawing of a Horse from Sherborne, Dorset.

IN entitling his account of this object "On an apparently Palæolithic Engraving on a Bone from Dorset" (my italics) Sir Arthur Smith Woodward displayed his habitual caution, and it is with great regret that I now find myself obliged to differ from one for whose judgment I have so great a respect. For I still believe the Sherborne drawing to be a forgery and a clumsy one at that. It was perpetrated as a practical joke, such as delight the heart of boys of fifteen. How far the finders of the bone were involved in the affair there is nothing to show, one or other of them may have been innocent of it. But that some of the boys in the school were not quite so ignorant as Mr. Araldo Cortesi professes himself to be is shown by the fact that they were familiar with "Early Man in Britain" and the illustration of the Creswell Crag horse given there by Sir W. Boyd Dawkins.

Mr. Bayzand, who was intimately acquainted with all the facts—which he communicated to me—acutely remarks that a tracing of the Sherborne drawing when superposed on that of Creswell reveals an identity in size and as well a remarkable correspondence in outline. The odds against such a correspondence arising as a mere coincidence are sufficiently great, but it is easy to understand how a tyro in the art of forgery would have found it difficult—unless he happened to be a skilled artist—to make a sufficiently plausible copy with a change of scale. Further, those parts of the Sherborne copy which differ from the Creswell original do so in just those details which are likely to betray the inexperienced observer.

I do not fully understand the term "semifossilised," but I imagine it is intended to mean less altered than the mammalian teeth found in the gravels of the neighbourhood, *i.e.* in just such a state as might be expected of a bone which had lain in a refuse heap exposed to the air.

University Museum,
Oxford, January 22.

W. J. SOLLAS.

PROF. W. J. SOLLAS has directed my attention to a letter in the issue of NATURE of January 16, by Sir Arthur Smith Woodward, on the supposed Palæolithic drawing of a horse from Sherborne.

At the time of the "discovery" of this drawing I

³ *Op. cit.*, vol. 3, pls. xviii. and xxviii.

⁴ E. M. Kindle, "Observations on Ice-borne Sediments by the Canadian and other Arctic Expeditions," *Am. Journ. Sci.*, vol. 7, April 1924, pp. 253-257.

⁵ William Herbert Hobbs, "The Glacial Anticyclones, the Poles of the Atmospheric Circulation," *University of Michigan Publication*, Ann Arbor, Mich., pp. 200, 3 pls., 53 text figures. 3 dollars.

was engaged at the Sherborne School in arranging the museum collections and then learnt something of the history of the find.

The whole affair, as I gathered, was a trick played solely for the benefit of the science master without any idea that it would go any further. Its success was a source of much merriment at the school, particularly amongst those boys who were under this master, and I was even invited by some to inspect the fake.

I may mention that a copy of "Early Man in Britain" was lying in the Museum while I was there, and I should add that I was informed that the bone was discovered near a rubbish heap on the Bristol road, where some of the refuse of the town had been deposited.

This, so far as I can remember, was in the year 1912 or 1913, and I dismissed the whole matter from my mind until I learnt, not without surprise, that Sir Arthur Smith Woodward had communicated the "discovery" to the Geological Society. When later I read his account I at once informed Prof. Sollas of all that I knew, and so am directly responsible for the statement made by him in the third edition of "Ancient Hunters."

C. J. BAYZAND.

Oxford, January 21.

Spermatogenesis in Spiders and the Chromosome Hypothesis of Heredity.

PROF. WARREN, in NATURE, January 16, writes a second letter in support of his previous contention that the spermatogenesis of spiders is different from that of all other animals, and that it is "a good corrective to the view of the all-sufficiency of chromosomes, and to the tendency to substitute cellular structure in the place of the directive vital activity of the organism."

It is difficult to understand what Prof. Warren means by this statement, because it is universally allowed that modern cytological observations have thrown a clear light on nearly every problem of genetics and heredity.

In the past ten years I have probably carried out more investigations than any one else on the cell cytoplasm and its organelle, yet throughout I have been obliged to admit that the only cell organs which fulfil the necessary conditions which a study of genetics lays down, are the chromosomes.

At the same time, while I have been forced to believe the chromosome theory in general, I recognise clearly that there are still many facts which the chromosome theory does not explain. The spermatogenesis of spiders, as we know it in this laboratory, provides no stumbling-block to a belief in any part of the chromosome theory, any more than does the presence of atypic spermatozoa in molluscs, for example.

The amitosis question has long been a subject which the anti-chromosome theorist has toyed. Prof. Warren is not the first rebel; he will not be the last. When I was a student at Oxford I caused some concern to my teachers by developing a bad attack of amitosis-itis. It was brought on by reading Prof. Child's interesting paper on "Moniezia." However, that is "past history."

Now Prof. Warren is in the middle of a bad attack of the same disease. Like all the evidence for amitosis in germ cells, Prof. Warren's is going to be unconvincing. Even if this investigator does demonstrate division or fragmentation of nuclei, this does not necessarily mean that the cell is dividing amitotically. I can only refer Prof. Warren to Macklin's