

weight of zirconium is 91.3 with an accuracy of about 0.1 unit.

While the presence of 1 per cent.  $\text{HfO}_2$  in a preparation of  $\text{ZrO}_2$  influences the apparent atomic weight by 0.6 unit, the presence of 1 per cent. of  $\text{ZrO}_2$  lowers the apparent atomic weight of hafnium by not less than 1.4 units. One must thus obtain a hafnium preparation containing a very small amount of zirconium in order to fix the atomic weight of hafnium with an accuracy similar to that reached for zirconium. It was possible some time ago to supply Prof. Hönigschmid with such preparations from this laboratory. The values found by him, when analysing  $\text{HfBr}_4$ , for the best preparation were 178.32 and 178.35, and for a less pure fraction 177.78 and 177.80. The samples used by Hönigschmid and Zintl were recently very thoroughly investigated by Mr. Thal Jantzen by means of the method of X-ray analysis, which when proper precautions are taken is able to give values of high accuracy and offers a simple method of estimating the zirconium content of hafnium preparations. The values for the zirconium content of these preparations were found to be 0.16 and 0.57 per cent. The values for the atomic weight found by Hönigschmid and Zintl have thus to be raised to 178.57 in the first, to 178.64 in the second case, and we may, therefore, with a probable error of less than 0.1 unit fix the atomic weight of hafnium at 178.6.

G. HEVESY.

Universitetets Institut for teoretisk Fysik,  
Copenhagen, February 5.

#### Late Palæolithic Art in the Cresswell Caves.

I WRITE as chairman of the British Association Committee, now resuming, by permission of the Duke of Portland, the exploration of the Cresswell Caves where it was dropped some forty years ago by the Rev. Magens Mello and myself, to prevent your readers from being misled by the following passage in the third edition of Prof. Sollas's book on "Ancient Hunters," p. 536.

"There is a singular absence of any attempt at art in all the Palæolithic stations of England. The horse figured here [Fig. 299] is, I am assured, a forgery introduced into the cave by a mischievous person."

The Cresswell horse was the first proof of the range into Britain of the wonderful art of the French caves, and the discovery made in the 'seventies by myself was published—after a careful scrutiny by Sir John Evans, Sir Augustus Franks, Lord Avebury, General Pitt-Rivers, and other leaders—in the Quarterly Journal of the Geological Society of London. It has remained unchallenged for more than forty years, and has passed into the literature of anthropology. *Res judicata est.* The charge of forgery is not now to be made without clear evidence. In answer to a letter asking for this evidence Prof. Sollas writes to me that it is based on what he was told "some years ago, I think 1919," by a clergyman since dead, who declined to give names or other particulars. This means that the charge of forgery is founded on gossip without a shred of evidence, and is unworthy of further notice.

The Cresswell horse is engraved in fine lines in a style similar to that of the figures of animals found since in the late palæolithic caves of France and Switzerland. It is not accurately represented by Prof. Sollas in his Fig. 299. This figure is copied from Evans's "Ancient Stone Implements" (2nd edition, p. 524), in which my woodcuts were used. If the copy be compared with the original it will be seen that the details have been omitted, leaving merely an outline useless for the study of the art of the caves.

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Prof. Sollas is equally unfortunate in his sweeping statement that there is no attempt at art in the palæolithic caves of England. Our Committee is now at work at Cresswell, and Messrs. Garfitt and Leslie Armstrong have already recorded the discovery of incised figures of bison and reindeer along with other late palæolithic finds. As the work proceeds it will probably result in further proof that the picturesque gorge of Cresswell Crags was a hunting station of the artistic tribes who followed the wild animals in their migrations from the south of France into Britain, then the north-western region of the great Pleistocene Continent.

W. BOYD DAWKINS.

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Fallowfield, Manchester,  
January 29.

#### The Ages of Peat Deposits.

THE wide interest now taken in the study of peat will, I think, justify further reference to the subject of Dr. Pearsall's article in NATURE of December 6 and the letters from Mr. Tonks and Mr. Forbes which followed on January 24. It is satisfactory to note that Dr. Pearsall has withdrawn from the obviously fallacious correlation he at first put forward. He now, however, makes a second correlation based on the identification of the birch scrub on the peat-covered 25-foot beaches as Lewis's Upper Forest, making the latter Neolithic in age and therefore climatically in conflict with the evidence in the Pennines. This yields him the same result, namely, that climatic deductions from peat and forest beds are untrustworthy.

This second correlation, however, has no more to recommend it than the first. Mr. Forbes will, I am sure, bear me out when I say that a few isolated occurrences of birch scrub at a low level are no proof of a forest period. On the contrary, the evidence of the submerged forests indicates that the period of greatest tree growth in the British Isles, *i.e.* Lewis's Upper Forest, antedates the 25-foot beach. Dr. Pearsall will, therefore, have to try still another correlation if he wishes to establish his point.

May I add to that of Mr. Tonks my appreciation of the admirable work recently done in the Pennines by Dr. Woodhead, Mr. Buckley, and Mr. Holmes.

W. B. WRIGHT.

Manchester.

IN directing further attention to the question of peat deposits, Mr. Wright seems to add little to the questions raised. He accuses me, however, of basing a hypothesis on the occurrence of isolated patches of birch scrub. I have repeatedly (and publicly) expressed the opinion that the presence of timber in peat can have little significance unless the wood layer is continuous over a very wide area, and I have, indeed, criticised Lewis on the grounds that his "forest layers" did not always fulfil this condition. I may, therefore, be forgiven for finding Mr. Wright's accusation a little amusing. It appears to me, however, that his criticisms can only be seriously urged by disregarding completely the use of the words "may" and "might" in my letter (although one of them is italicised), and by failing to attach any significance to the sentence which expresses the opinion that, on whatever they are based, these hypothetical correlations throw doubt on the climatic hypothesis of peat stratification. This is the gist of the letter, to which Mr. Wright's attention may be redirected.

W. H. PEARSALL.

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