(from the production of overtones of high pitch), and how it brings out the pure tones of the string instruments. The various instruments in an orchestra sound better. Everything is reduced in proportion, and, to use an illustration from art, it is like passing from one of Etty's huge pictures to a delicate and beautiful Messonier, in which one sees and appreciates every detail in an area of small dimensions.

JOHN G. MCKENDRICK.

A New Variety of Zebra.

Will you kindly allow me a little space to direct attention to a new and very interesting variety of Grant's zebra, shown in the accompanying photograph sent me by my friend Mr. C. W. Hobley, C.M.G., commissioner at Nairobi, East Africa? The specimen, writes Mr. Hobley, "was obtained by Mr. G. H. Goldfinch, assistant game ranger of the East African Protectorate, a few months ago in the neighbourhood of the Rift Valley. The

animal has a "big whitey patch in the middle of the back, and it came out of a herd which were all the same. I suppose it is a Mendelian 'sport,' which has become dominant in that particular herd, like the white waterbuck on the Euaso Nyiro, north of Kenya."

I propose to call this variety in Mr. Pocock's terminology, *E. quagga*, var. *Goldfinchi*, or in the old terminology, *E. Burchelli*, var. *Goldfinchi*. Mr. Hobley adds that the print is a little dark, as "the stripeless saddle on the body is very markedly white in the skin itself."

April 11.

WILLIAM RIDGEWAY.

Implements of Moustierian Type from the Rock of Gibraltar.

In a paper read before the Royal Anthropological Institute on March 7 (*NATURE*, March 16, pp. 100-101) I gave an account of recent cave-exploration at Gibraltar. In one of the caves thus described, the discovery of various mammalian remains was recorded, together with that of human bones, pottery, and stone implements.

In regard to the latter, a close comparison was made with cave implements, and the similarity of certain examples to implements of the Moustierian type was remarked. But a guarded opinion was given, and this caution, I am now glad to state, seems to have been excessive.

On March 31 Dr. Allen Sturge very kindly examined the specimens with me, and he allows me to record his opinion on four implements submitted to him. The con-

clusions were fully borne out by comparisons with specimens in Dr. Sturge's magnificent collection. Of the four implements, three (Nos. 2, 7, 13) are judged to be definitely of Moustierian type; the remaining one (No. 15) is either "Moustierian" or "early Aurignacian" (the next and following stage).

Thus out of eleven stone objects (from the cave in question), that are undoubtedly implements and not mere splinters, four are distinctly Palaeolithic, and of an early period. So far as I know, Palaeolithic implements have not been previously recognised or recorded in connection with the caves of Gibraltar. Moreover, those now mentioned were not accompanied by any polished implements or by any metal objects.

The recognition of Palaeoliths of the Moustierian type gives some ground for hope that eventually the whole series of cultural epochs may be established for the caves at Gibraltar, as has been done elsewhere. Further, the discovery of a human skeleton of that period might throw a flood of light on the significance of the Forbes Quarry skull. In any case, exploration will be resumed with increased zest in view of these possibilities.

In conclusion, I would point out that Obermaier seems to hold the opinion that the associations of culture with fauna will be found to differ in the Mediterranean area and in western or central Europe (*L'Anthropologie*, 1909, Tome xx., p. 520). My investigations have already suggested a marked similarity between the Gibraltar caves and some of those at Mentone. Probably the northern limit will be found to include Les Eyzies. Lastly, the preceding remarks are written with full appreciation of the weight of Compton's remarks (1910) as to the significance of isolated examples of implements referable to a particular age.

W. L. H. DUCKWORTH.

Anthropological Laboratory, New Museums,
Cambridge, April 5.

Damage done to Skulls and Bones by Termites.

THE extensive damage done to skulls and bones generally in many of the graves of Egypt and Nubia has been attributed to beetles, the bodies of these animals having been found in the earth which is invariably associated with the damaged area, the latter being, in fact, always covered with earth unless it has been knocked off during removal of the skull from the grave.

There is good reason, however, for believing that the damage is the work, not of beetles, but of termites, which still exist in these countries.

These animals, as is well known, never work without covering all their operations with a tunnel or ramification of tunnels composed of earth or grains of sand firmly stuck together by some secretion from the ants themselves. Under cover of these earthworks, they devour whatever substance they have built over, and the destruction is sometimes so complete that nothing but a shell of earth remains, the substructure having been entirely eaten away. In such cases the original form of the destroyed article may be distinctly seen, as the mud covers it in a fairly thin uniform layer, following all its lines and contours. A good example of this was seen at Koshtamna in Nubia, about seventy miles south of Aswan. Here a small wooden statue of a king was still standing in its original position in a tomb chamber, but the crown and more prominent features of the face, completely covered though they were with mud, still preserved the outlines of the form beneath. When, however, the mud was removed, it was found that the statue upon which it had been built was almost completely destroyed, only fragments of the wood being left here and there.

In the case of skulls and bones precisely the same thing happens. A skull is found covered with mud firmly stuck on, and with the traces of the white ants' tunnels running through. If the mud is removed, large areas of the cranial walls may be found to have disappeared altogether. In less exaggerated cases, holes will be seen with white, gnawed edges, or perhaps only the surface of the bone has been attacked. The cranial sutures are a favourite site for the commencement of the termites' operations.

The presence in some cases of the dead bodies of small reddish beetles embedded in the mud on such skulls led to the not unnatural conclusion that they were the authors of

the mischief. The following facts, however, show this to be incorrect.

In the first place, the tunnels, which run in all directions beneath the cake of mud, are too small to contain the beetles found embedded in it.

Secondly, the beetles are found in many other parts where there is no sign of damage to the bone, and quite commonly, in the case of mummies, beneath the linen wrappings where these are in contact with the skin, in which situation they are frequently embedded in the resin or bitumen which has been used in the mummifying process. In these situations there is, of course, no mud whatever, while the damage to the bones is *always* associated with earthworks and tunnels.

Thirdly, though the same earthworks appear in every direction in the grave containing affected bones, on the roof, walls, coffin, &c., no beetles are ever found anywhere in association with such workings except on the body.

Fourthly, in mummified bodies, where the wrappings, soaked in bitumen, are so hard as effectually to have excluded even the ravages of white ants, the works of which may, nevertheless, cover the *outer* surface of such wrappings, beetles are still found in and about the mouth and nose of the mummy, some stuck to the teeth, others to the linen with which the mouth is filled, but *not* in this case on the outer surface of the wrappings, where they ought to be if they were the authors of the earthworks which cover the mummy cloths.

From these facts it seems clear that the beetles were present before the process of mummification was complete, that they became covered over when the body was wrapped, or possibly were not hatched until this was complete, and so are found stuck to the resinous wrappings. In cases where less bitumen or other substance was employed, and the body was merely wound in cloths, the white ants were able to make their way through these with the greatest ease. While doing so they would come in contact with the beetles which had been included in the wrappings, and these would then perforce become embedded in the mass of earth brought up by the termites. It is noteworthy that *complete* bodies of beetles are seldom or never found in the mud. If carefully examined, their heads, legs, and under parts are usually gone, only the tough wing cases remaining, and these are so strong that to a certain point they will resist crushing with the fingers. On the other hand, beetles complete, so far as the naked eye can detect, in every part, even to the delicate antennæ, have been found under the wrappings, and particularly in the neighbourhood of the mouth and nose. The inference from this is, of course, that the white ants devoured the softer parts of the beetles when they found the bodies of these animals in their path, leaving the hard portions stuck in the mud of their buildings.

DOUGLAS E. DERRY.

Anatomical Department, University College,
London, W.C.

What is the genotype of *X. us* Jones, 1900, based upon a species erroneously determined as *albus* Smith, 1890?

Statement of Case.—Jones proposes a new genus *X. us*, 1900, type species *albus* Smith, 1890.

It later develops that *albus* Smith, 1890, as determined by Jones, 1900, is an erroneous determination.

What is the genotype of *X. us*, 1900; *albus* Smith, 1890, or the form erroneously identified by Jones as *albus* in 1900?

Discussion.—The nomenclatorial problem expressed in the caption of this note is solved in two diametrically opposite ways by different authors.

Some writers maintain that the original *albus* Smith, 1890, is the genotype, while others maintain that the genotype is represented by the species actually studied by Jones and misdetermined as *albus* Smith.

Cases of this general nature have given rise to considerable confusion in nomenclature, and several such cases have been referred to the International Commission on Nomenclature for opinion.

At the last meeting of the Commission, the principles involved came up for discussion, but it was impossible to reach a unanimous agreement. On account of the differences of opinion, the secretary was instructed to make a

careful study of a number of cases, and to report upon the same to the Commission.

It is not difficult to foresee that, no matter how the cases are finally decided, great dissatisfaction will arise among zoologists, because the opinion rendered is not the direct opposite of what it eventually will be.

Recognising that this is one of the most difficult cases that has ever been submitted to the Commission, and recognising the fact that, regardless of our action, we shall probably be criticised more on the basis of our decision on this case than because of any other opinion that we have rendered, I am desirous of studying at least 100 cases, if possible, that would come under such a ruling, before my report is formulated.

In view of the foregoing premises, I respectfully request zoologists in different groups to direct my attention to as many instances of this kind as possible with which they are acquainted in their different specialities. Further, since the arguments on both sides of the problem appear to be almost equally valid, it does not seem impossible that the final decision will have to be based upon the arbitrary choice between the two possible rulings, and on this account I am desirous of obtaining all possible arguments on both sides as they occur to different zoologists, and also any personal views based upon convenience or inconvenience, or other grounds, which may be held by different colleagues.

I will hold the case open at least until September 1, for the presentation of arguments by any persons who may desire to submit their views.

C. W. STILES,

Secretary of the Commission.

April 4.

A Kinetic Theory of Gravitation.

As one who for many years has been attracted by the problem of gravitation, I was greatly interested in Mr. C. F. Brush's "Kinetic Theory of Gravitation" (*NATURE*, March 23), and in Sir Oliver Lodge's letter relating thereto (*NATURE*, March 30).

About three years ago I made an attempt to examine how far gravitation might be accounted for by waves of compressional type propagated through the æther (cf. *Phil. Mag.*, January, 1909). Before any such theory can be admitted, even as a working hypothesis, it must be shown by rigorous dynamical methods to be capable of accounting for gravitational attraction. This in itself involves no elaborate analysis, though questions arise as to the fundamental nature of matter and of its motion with respect to the æther.

It appears, in opposition to what might readily be supposed, that Mr. Brush's assumption of a directionally indifferent (isotropic) distribution of waves is not needed; a single progressive train of plane-waves would answer equally well. The real difficulties of the theory are encountered when we consider the several effects, other than gravitational attraction, which might arise from the impact of compressional æthereal waves upon atomic matter. It has to be shown that, under admissible assumptions, the *direct* action of the waves would not give rise to any observable phenomena of motion, and that the heating effect might be *nil*, or small enough to escape observation. Other points no less important have also to be considered; they are dealt with at length in my paper.

I fully concur in Sir O. Lodge's objection to regarding the atom "as a foreign substance—a sort of 'grit' in the æther," and, in the paper referred to, matter was treated as of purely æthereal constitution, the motion of a material body through the æther being regarded as unaccompanied by any bodily transference of *ultimate* matter through finite distances. As to whether the gravitative property of matter is essentially bound up with its constitution, or is due to something external, I think Sir O. Lodge will agree that, notwithstanding metaphysical prepossessions (in which I largely share), we should yet keep an open mind. The real solution of the question is perhaps very different from what we are reasonably entitled to expect!

It may be mentioned, however, that some experiments now in progress seem likely to add very considerably to the difficulty of accepting a compressional-wave theory of gravitation.

C. V. BURTON.

Boar's Hill, Oxford, April 2.