Great Eagle Owl (*Bubo maximus*) European, presented by Mr. H. Godman; two Black Apes (*Cynopithecus niger* ♂ & ♀) from the Celebes, a Greater Sulphur-crested Cockatoo (*Cacatua galerita*) from Australia, deposited; two Alpine Accentors (*Accentor collaris*) European, purchased; a Coypu (*Myopotamus coypus*) born in the Gardens.

OUR ASTRONOMICAL COLUMN.

PHOTOGRAPHIC NEBULOSITIES IN THE MILKY WAY.—In the March number of *Astronomy and Astro-Physics*, and in several recent numbers of *Knowledge*, Prof. E. E. Barnard describes a number of wisps of nebulosity and diffused masses of luminous haze discovered upon photographs taken by him with a portrait lens six inches in aperture and having a focal length of thirty-one inches. A remarkable and large nebulous mass, situated about R.A. 21h. 34m. Decl. + 56° 50' appeared upon a plate exposed for seven hours. The picture shows a straggling group of bright stars in the centre of the nebula, which is more than two degrees in diameter. The group of stars is visible to the naked eye as a hazy spot, about three degrees north-west of the variable μ Cephei, the brightest star in the group being D.M. + 56° 2617. The star D.M. + 57° 2309 (mag. 6.5) is also shown by the photograph to be surrounded by a rather unsymmetrical dense circular nebulosity. This object was not previously known to be nebulous, though Prof. Barnard says that with the telescope the nebulosity can be seen as a hazy glow about the star. The region of the Milky Way lying north and east of Orion appears to be singularly rich in large diffused nebulosities. Photographs show that ω Orionis and λ Orionis are nebulous, while there is a faint and large diffused glow near the stars ν and ξ Orionis. There was a suspicion of a large nebulosity about α Orionis on one of the plates, but this has not yet been verified. The existence of the other nebulosities, however, has been established either by telescopic observation or new photographs. A photograph obtained at the beginning of last month shows two very singular fan-shaped patches of nebulosity close to γ Cassiopeiae. These are about 15' in diameter and point towards the star. They could just be seen by Prof. Barnard with the 12-inch of the Lick Observatory, but he thinks they would never have been detected if the photographic plate had not revealed them. Photographs of the region about 15 Monoceros show that this group of bright stars is mixed up with misty matter having a diameter of about three degrees. The place of 15 Monoceros for 1860 is R.A. 6h. 33m. 16s. Decl. + 10° 1' 43", and the condensation of the remarkable nebula in question is 12' south preceding the star. Prof. Barnard has now photographed the Milky Way from Scorpio to Orion, discovering many masses of nebulosity on the way. His pictures are not only beautiful views, but valuable records of the structure of the different regions portrayed.

MADRAS OBSERVATORY.—From the report of the Madras Observatory, just published in the Monthly Notices of the Royal Astronomical Society (vol. liv. No. 4), it appears that the Secretary of State for India has given his sanction to the proposals made by the Government of India regarding the future of the observatory. The observatory, which has hitherto been under the Madras Government, will, from April 1, be transferred to the Imperial Government. According to the report, a new observatory for solar physics will be erected at Kodarkanal, on the Palani Hills, under the direction of the present officiating Government Astronomer, who will, for the present, also have charge of the existing observatory. The new institution will undertake the work of solar photography now carried on at Dehra Dûn, and will also take up spectroscopic work on the sun, and actinometric researches.

A NEW COMET.—The first comet of this year was discovered by Mr. Denning on Monday evening in R.A. 9h. 55m. Decl. + 32° 15'. It was small and faint, and exhibited a short fan-shaped tail. The object was moving towards the east-south-east at the rate of nearly one degree per day.

RECENT INVESTIGATIONS AND IDEAS ON THE FIXATION OF NITROGEN BY PLANTS.

THREE totally different, though convergent, scientific controversies have arisen during the latter half of the present century concerning the rôle played in nature by nitrogen, as

met with in the air, rain, and soil, free or combined, in connection with the ordinary plants of agriculture and forestry; and, quite apart from their real relations to one another, these three controversies have at times been somewhat confused in their issues.

One of these controversies turned on the question of the transformations of combined nitrogen, as met with in the forms of ammonia, nitrites, and nitrates, and as organic compounds of nitrogen resulting from the decomposition of the remains of living beings—plants and animals—in the soil. The outcome has been the proof that oxidations and de-oxidations of these compounds are intimately bound up with the physiological activities of living organisms, especially bacteria, in the soil; the investigations of Giltay and Aberson, and Winogradsky's brilliant researches especially, have brought what had long been regarded as purely chemical problems into the domain of biology. "Nitrification" and "de-nitrification," to use the current terms, are phenomena incorporated with those of fermentation, respiration, &c., and therefore involve biological science for their elucidation.

Another of these controversies turned on the question whether the free nitrogen which forms so large a proportion of that huge gaseous ocean, the atmosphere, can be again directly employed by green leaves, and built up as combined nitrogen in plants; or whether, once having been disengaged from organic and other compounds, and passed into the air as gaseous nitrogen, it is for ever lost, except in so far as electric discharges and other energetic physical and chemical processes force this relatively inert element into combinations, which the rain then brings down as inorganic salts, and so help to restore the balance of nitrogenous substances in the soil.

This controversy, a long and involved one, started and for some time continued as a peculiarly chemical question, has passed through various phases and branched out into several subsidiary controversies, if we may so term them.

Thus the alleged "fixation" in the soil, especially investigated by Berthelot and André, became a scientific question apparently on definite lines of its own, and (so far as any such question can be independent) independent of the question whether ordinary green-leaved plants, such as peas, lucerne, wheat, &c. can assimilate the free nitrogen of the atmosphere by processes more or less comparable to those by which they are known to assimilate the carbon they wrench from the carbon-dioxide of that gaseous environment.

The latter question, again, became a divided one, chiefly owing to assertions that green leaves could directly assimilate the ammonia, if not the free nitrogen, of the air, and some time was occupied in arriving at the conclusion that ordinary green plants do not directly assimilate or fix either the gaseous ammonia or the free nitrogen of the atmosphere. This conclusion, in opposition to that arrived at by Ville, was regarded as so thoroughly established by the experiments of Boussingault and of Lawes, Gilbert, and Pugh, that it has been definitely accepted and taught for many years—and rightly so, from the evidence to hand.

The third of the three controversies referred to at the outset, is the more recent one concerned with the question whether certain of the higher green-leaved plants, particularly those known as leguminous plants (such as peas, beans, clovers, vetches, lupins, robinia, &c.), when living as they normally do in symbiotic association with certain microscopic and essentially parasitic fungoid organisms which invade their roots, are differently placed from other green plants as regards the power of "fixing," and assimilating, the free nitrogen of the atmosphere.

The present position of opinions on this last and most remarkable controversy is the subject of this article, so far as it can be done justice to in the short space at disposal.

It is now well known that leguminous plants are normally found to have certain nodosities or swellings on their roots, and that these swellings are caused by the activity of certain minute organisms which, as the writer of this article first proved, invade the roots from outside, after the manner of a parasitic fungus. The controversy as to the exact nature of these organisms—bacteria, according to Prazmowski, Beyerinck, and others, degraded allies of the Ustilaginæ, or some lower fungus, according to my observations, and the confirmatory evidence of Laurent—in no way affects the truth that these organisms do not kill the plants attacked, or even make them diseased, but incite them to more active life for a time. The evidence on

which these organisms (termed "bacteroids") have been taken to be bacteria—their growth in gelatine tubes, staining, and their minute size—is equally in favour of their being lower fungi, and is not sufficiently conclusive. Eventually the nutritious contents of these nodules, with the symbiotic "bacteroids," are absorbed, in whole or in part, by the leguminous plant, and their rich stores of nitrogenous material assimilated by the latter.

The experiments of Hellriegel and Wilfarth, of Lawes and Gilbert, and of others and myself, placed it beyond reasonable doubt that, taking the leguminous plant and its symbiotic organisms together with the pot of soil in which it is grown as a closed system, this system contains more nitrogen at the end of several weeks than can be accounted for by the nitrogen in the soil and the seed at the commencement of the experiment; and this was true in cases where careful precautions were taken to prevent the addition of any nitrogen further than the free nitrogen of the air. The only legitimate conclusion was that somewhere, and somehow, the system fixes free nitrogen from the air.

This matter has been since carried further, however, by Laurent and Schloesing, who, by growing various plants in an air-tight apparatus under such perfect control that they could analyse the quantity of nitrogen both in the plant and soil, and in the purified air, showed that the gain of nitrogen in the former during the progress of the experiments, is balanced by a corresponding loss in the latter. They further showed that only two kinds of plants could thus "fix" the nitrogen of the air. These are leguminous plants, and certain lower algæ (perhaps mixed with bacteria) or allied forms. This fixation only occurs under certain definite conditions, moreover. The leguminous plants must be infected with the symbiotic "bacteroids," and the algæ must be exposed freely to the air and light in the apparatus: even a thin layer of the sterilised sand employed sufficed to stop the action of the algæ.

Laurent and Schloesing found no fixation in the case of artichoke, oats, tobacco, mustard, cress, or any other plants experimented with; and their experiments, taken as crowning the edifice of evidence accumulated by them and numerous other observers, have been fairly regarded as proving that leguminous plants, at any rate, and perhaps certain lower algæ, do somehow "fix" the free nitrogen of the atmosphere and assimilate it.

Koch and Kossowitsch have recently claimed to confirm the above results of Laurent and Schloesing with algæ, and it should be mentioned that Frank had previously stated that such fixation by lower cryptogams occurs. Unfortunately we are as yet uninformed what species of algæ are exactly concerned here, and no one has cultivated them pure and confirmed the results.

It will be noticed that, so far, all that is established is that the infected leguminous plants, and the algæ of sorts, *plus* the known soil (usually sterilised sand to which known additions are made), somewhere and somehow gain in nitrogen at the expense of the free nitrogen of the atmosphere.

Now come the other aspects of the controversy, which is raging chiefly around the question as to exactly where and how this gaseous nitrogen is fixed.

Obviously several possibilities could be suggested.

(1) The gaseous nitrogen could be conceived as directly fixed by the plant which gains in nitrogen—as absorbed by the protoplasm of the living cells exposed to the air—*e.g.* the cells of the leaves of the leguminous plant, or those of the algæ on the surface of the soil. This view is actively maintained by Frank and a few supporters, who go as far as is possible in this direction, and really again raise the old question which originated with De Saussure, and was rightly regarded as refuted by Boussingault and Lawes and Gilbert.

(2) The gaseous nitrogen could be conceived to be fixed in the soil by means of bacteria or lower algæ (we have seen these are left indefinite), and, when it has been converted into nitrogenous compounds of some kind in the soil, eventually absorbed by the roots of the leguminous or other higher green plant in the ordinary course of events. The principal champion of this view is Berthelot, who claims to have proved that certain soil-bacteria, and also the organisms of the leguminous root-nodules, have the power of fixing the free nitrogen of the air, and so enriching the soil in nitrogenous compounds. In this connection, of course, the whole question of nitrification and de-nitrification in the soil will no doubt be involved with the question of the fixation of free nitrogen from the atmosphere.

(3) The fixation of the atmospheric nitrogen could be con-

ceived of as a powerful act of the machinery of the leguminous plant, urged to the necessary expenditure of energy by the stimulating action of the symbiotic organism in its roots. This view, held especially by Hellriegel, Prazmowski, and others, is also shared by Frank, who believes that it is only in their being thus stimulated to greater activity that the leguminosæ differ from many other plants, which, he says, also fix the atmospheric nitrogen directly, but to so much less an extent that the experimental proof of their power to do it is far more difficult.

(4) Another possible view is that the root-organisms act merely as accumulators of nitrogenous material, which has been derived from atmospheric nitrogen fixed and combined in the soil, by physical or chemical processes, or in the open ground by the action of soil-organisms; and the leguminous plant benefits by devouring (if we may employ this word) the bacteroids eventually, and profiting by their stores of nitrogenous material.

Let us now take these four possibilities in order, and examine them a little more in detail.

The first view rests almost entirely on the statements of Frank, of Berlin, who brings forward a number of experiments which in his opinion show that many higher plants, in addition to the leguminosæ, are capable of directly assimilating the free nitrogen of the atmosphere. For instance, Frank gives results showing that oats, buckbeans, spurrey, turnips, mustard, potatoes, and Norway maple are all capable of fixing atmospheric nitrogen.

Most of Frank's experiments were made in the open air, the pots of plants being simply sheltered from rain; but in some cases, he affirms that he got positive increase of nitrogen with mustard-plants under bell-jars, properly shut off from the outer air, and through which purified air was drawn.

Apart from these latter, and in spite of Frank's assertion that the quantities of combined nitrogen in the air are so immeasurably small that they may be neglected, it seems fair to object that, in the present state of science, we cannot trust experiments in the open air to decide such a point; while, with regard to the experiments with mustard, it must not be forgotten that not only the old results of Boussingault and Lawes and Gilbert are entirely and emphatically opposed to them, but the exceedingly careful recent experiments of Schloesing and Laurent, made with all modern appliances and methods, showed the contrary—no signs of fixation of nitrogen could be obtained in oats, tobacco, cress, mustard, cabbage, spurrey, and potato, the very plants Frank used.

Frank replies that completely normal plants cannot be grown under such closely covered glass vessels as these experimenters use, but he accepts their positive results in all cases. Frank's contention is that the plant must be very vigorous, and near its maturing point, before it has power to energetically seize and "fix" the atmospheric nitrogen; but (without denying that it is possible that the utmost vigour may not be as yet attainable under the conditions necessary for culture in closed glass receptacles of limited capacity) it is impossible to overlook the danger that in experiments in the open air, the time which must necessarily elapse before Frank's critical period of maturity on the part of the plant is reached, is long enough for all sorts of disturbing influences to come in, especially if any kind of "fixation" in the soil, such as Berthelot asserts, really occurs: the root-hairs would take up, and the plant absorb, nitrogenous bodies as fast as they were formed in the soil around them, while there would be ample time for the development of many generations of micro-organisms in the medium.

In view of the tenacity with which the belief in a direct absorption of atmospheric nitrogen is cherished by many foresters and agriculturists, it seems imperative that critical experiments should be persevered in; as matters stand, we cannot accept Frank's position as proved, or even as rendered probable.

The possibility mentioned above as an explanation of the danger of accepting Frank's results would be rendered a certainty if the recent researches of Laurent and Schloesing, Koch and Kossowitsch, and Berthelot, in part supporting earlier statements by Frank himself, turn out to have been properly interpreted.

Laurent and Schloesing—and their results are confirmed by Koch and Kossowitsch—declare that sterilised sand, devoid of nitrogenous material, when covered with a growth of certain green and blue-green algæ, probably mixed, however, really does "fix" the atmospheric nitrogen, and gains in nitrogen-

compounds, but only if the algal growth is freely exposed to the atmosphere in the closed chambers employed. These statements confirm earlier, but less definite, experimental results by Frank; and the latter has recently expressly stated that certain fungi—e.g. *Penicillium cladosporioides*—can flourish in a medium to which no nitrogen but that of the atmosphere has access.

Berthelot goes further, and claims to have established that several species of soil-bacteria and fungi, including the fungoid organism of the leguminous tubercles cultivated separately, can "fix" free nitrogen; and if the analyses of the small quantities of materials in his flasks survive the criticism of the chemists, it seems difficult to refuse credence to the views he puts forward; but, as in most of these cases, it is the enormous difficulties of analyses which lie at the root of the matter.

Moreover, different observers differ considerably on this question. Beyerinck, while regarding it as probable that the nodule-organisms "fix" atmospheric nitrogen, admits that he does not prove it; and in Laurent's special investigation into this question, he left it also uncertain; while Immendorf failed to satisfy himself that these organisms can flourish without organic compounds of nitrogen; and Frank insists that they do not thrive at all without organic nitrogenous food-materials. Moreover, it must not be overlooked that other observers, e.g. Gautier and Drouin, have given evidence pointing to possible phenomena of "fixation" of nitrogen by compounds of iron and other substances clinging to particles of the sand employed, which may interfere with the accuracy of conclusions drawn from experiments where sterilised soil in the open air is concerned.

When we reflect how very minute these organisms are, and what excessively small quantities of nitrogen they need for their life-purposes, we cannot be surprised at the difficulties met with in these investigations. But, however far from proved we may regard the question of fixation of free nitrogen by soil organisms, it is perfectly clear that here is a most pressing question for further experimental research, and agricultural and forest practice are alike keenly interested in having the question definitely answered.

The third possible view—that the leguminosæ are able to force free nitrogen into combination with other elements, owing to the energetic action of their protoplasmic machinery stimulated by the symbiotic fungoid organism—deserves more consideration than may at first sight appear, especially to those who are not familiarised with the remarkable phenomena of symbiosis generally.

In the first place, the fact that leguminous plants amply provided with the root-nodules do "fix" the atmospheric nitrogen, under conditions in which the same plants devoid of the nodules fail to increase their supplies of nitrogen, is far better established than any of the other cases discussed, and must now be accepted as proved by the experiments of Frank, Hellriegel, myself, Lawes and Gilbert, and especially by the recent splendid investigations of Laurent and Schloesing.

It is true that Frank says the symbiosis is not absolutely necessary for the fixation to proceed, but even he declares that the leguminosæ are stimulated to greater powers of nitrogen-fixation by the nodule-organisms.

A curious and significant confirmation of the symbiosis theory comes from the experiments of Nobbe, Schmid, Hiltner, and Hotter, who find that *Elaeagnus* plants, the roots of which develop nodules due to the invasion of a fungus totally different from the one causing the leguminous nodules, also "fix" and assimilate the free nitrogen of the atmosphere, as shown by their growing and flourishing much better and more rapidly than *Elaeagnus* plants side by side with them, but not infected with the root organism. It will be interesting to see if further research shows similar results with any of the physiologically similar root-outgrowths, due to very different fungi, met with in *Taxodium*, *Podocarpus*, *Alnus*, *Funcus*, and many other plants, including some vascular Cryptogams.

Now comes the question, in what part of the leguminous plant does the actual "fixation" of the free nitrogen occur? Frank stands practically alone in claiming the leaves to be the organs concerned. Nearly all other observers regard the roots as the region, and the nodules themselves as the actual seat of fixation.

Kossowitsch has even attempted the heroic task of deciding between leaves and roots, by enclosing the former or the latter respectively in air-tight receptacles, shut off from the non-enclosed

parts, in which gases devoid of nitrogen were circulated. He could not always keep the apparatus perfectly gas-tight, however, and this and other failures met with in these exceedingly difficult experiments, undoubtedly weakens the force of his conclusions that it is in the roots and not in the leaves that the process occurs, though it does look as if the balance of evidence obtained fairly support his conclusion so far as it goes.

There are facts, however, to be gathered from the microscopic analyses of the root-nodules, as furnished by myself and others, which have been in great part overlooked in the discussions on this subject, and which, although not conclusive, seem to support the view that the seat of fixation may be in the nodules themselves. For instance, the nodules are supplied with a regular system of conducting vascular bundles, communicating with those of the roots; then their cells, during the period of incubation of the symbiotic organism, are abundantly supplied with starch; further, the cells in which the fungoid organism is vigorously flourishing are evidently exceedingly active, as may be deduced from their large size, brilliant nuclei, protoplasm, and sap-vacuole, all of which show signs of intense metabolic activity, lasting for considerable periods. The fact that the sap expressed from these active tissues is alkaline, has been interpreted as in accordance with Leew's suggestion that the living protoplasm, in presence of an alkali and free nitrogen, can build up ammonium nitrite, or some similar body. Be this as it may, there can be no question as regards the infected nodule-cells being centres where intense physiological activity is going on; and it seems impossible to avoid the conclusion that the vascular supplies from the roots into the nodules bring to these cells water in which various salts, carbo-hydrates, &c. are dissolved, and carry off from them the soluble products of metabolism.

Presumably these products of metabolism include nitrogenous bodies.

In the ordinary course of events, theory teaches that these nitrogenous bodies—e.g. amides, preceded by simpler compounds—are built up by the machinery of the ordinary living cell-protoplasm from carbo-hydrates and nitrates, the energy necessary for the metabolism being derived chiefly (if not entirely) by the oxidation of part of the carbo-hydrates supplied.

This constructive metabolic work of the protoplasm is an act which we cannot explain in detail. We can only dimly perceive that it must be due to some remarkable power the protoplasm possesses—and in virtue of which it is an illimitable machine much more economical in its actions than any apparatus we can construct—of so placing the atoms and molecules of the nitrate, carbo-hydrate, water, &c. with which it works, that they are enabled to undergo movements into which we cannot as yet force them in the laboratory.

The whole matter seems to depend on some peculiar mode of presentation of the atoms and molecules concerned; and we can see no further than that this can be done in the living cell, because the protoplasm is a suitable engine for thus bringing the combining elements into the necessary positions in space.

Now, if this is so, there seems no exclusion of the possibility, at any rate, that the cell-machinery may be so stimulated into greater activity that it can even force the notoriously inert nitrogen molecules, properly presented, into combinations with other molecules, resulting in the production of nitrites, amides, or similar bodies in ascending order.

The whole matter no doubt resolves itself into some such question of a properly adapted engine sufficiently supplied with energy. The matter seems capable of explanation, in some degree, if we remember that carbo-hydrates and oxygen are present in abundance; the real difficulty is with the machinery, for we cannot as yet picture the exact construction or working of such an engine, as physiology nevertheless impels us to suppose the cell-protoplasm must be.

It may be remarked, by the way, that the likeness of the living protoplasm to an engine, in the sense implied, may hold good whether the former is an "emulsion," in the sense of the defenders of that hypothesis, or a "structure," in the sense of those who refuse the emulsion hypothesis.

The fourth of the possible views as to the means by which free nitrogen becomes available to the leguminous plant, however, reminds us that, although the evidence points to the stimulated leguminous plant as the best established example of one capable of doing this work, there are other possibilities.

Berthelot's recent insistence that certain soil-bacteria can fix free nitrogen, taken with Frank's, Laurent and Schloesing's, and

Koch and Kossowitsch's experiments, make it impossible to deny that the above hypothesis as to the powers of the protoplasmic machinery may apply to the cells of some lower organisms, without symbiosis coming into play at all. The remarkable facts brought to light regarding sulphur-bacteria and iron-bacteria by Winogradsky, and the still more unexpected results this observer obtained with nitrifying organisms, show that the machinery of the cell can avail itself of sources of energy undreamt of by earlier observers. If, by the oxidation of sulphur or sulphuretted hydrogen, or of lower iron-compounds, or of ammonia, certain of these organisms can obtain the energy necessary to set going machinery capable of so presenting other molecules of the elements they take up to one another that organic compounds result, it is by no means inconceivable that, at the cost of carbon-compounds which they oxidise powerfully, the necessary energy can be obtained to force even free nitrogen into combinations.

It is equally conceivable that in the case of the leguminosæ, the symbiotic organism is really more of a parasite (it is necessarily a parasite in some degree) than is assumed in the third view, and that, at the expense of the carbo-hydrates so richly furnished to it by the host plant, the fungoid organism alone supplies the machinery for forcing the nitrogen into combination, and that when it has stored up relatively large quantities, owing to its activity in the incubators—the root-nodules—provided for it by its host-plant, and is diminishing in resisting power, the latter at length turns round and absorbs the stores.

The chief objection to this view is that the gains in total nitrogen seem to be greater than would be thus explained, unless the organisms in the soil outside the roots are also fixing free nitrogen.

Such then, put too shortly as regards the numerous experimental facts, are some of the chief ideas agitating the scientific world on this question, a question which, be it emphatically stated, promises to be of more importance to agriculture in the future than any legislation as to prices, &c. that we can conceive; for if it turns out that the acquisition of free nitrogen by the land, or, what amounts to the same thing, the plants growing on it, can be economically promoted, the farmer and forester may have the control of sources of real wealth not yet dreamt of. Unquestionably there is an enormous amount of careful and very difficult experimental work to be done before we arrive at the solution of the various vital questions raised; but the astounding results obtained during the last decade by a few earnest workers promise brilliant results in the future.

H. MARSHALL WARD.

THE HAR DALAM CAVERN AND ITS OSSIFEROUS CONTENTS.

IT is now about half a century since Admiral Spratt first recorded the discovery of the Maltese ossiferous cavern deposits in which, at a later period, Prof. Leith Adams found so remarkable an assemblage of mammalian and other remains.

The fauna which was then brought to light was of a most unique and interesting character, consisting of three species of elephants, one of which *E. falconeri* was, when full grown, no larger than an average size Newfoundland dog; two species of hippopotamus, one *H. minutus*, about the size of a small donkey, several species of gigantic swans, large dormice, land tortoises, cranes, vultures, &c.

These remarkable discoveries in an area so circumscribed attracted much attention in the scientific world, and considerable interest was evinced in them; and this the more so as that Dr. Falconer was at that time engaged in determining certain organic remains which had been found in similar deposits, and under similar conditions, in Sicily. The result of the explorations in the two islands was to demonstrate that Malta had, at no very remote period, been directly in connection with the continental areas on the European side, and indirectly with those of Africa. Brilliant, however, as were the discoveries made by Spratt and Adams, many difficulties of vital interest to the archaeologist, the geologist, and the physical geographer were left unsolved when Adams departed from these islands in 1872. Dr. Caruana, a Maltese archaeologist, then carried on the work for a short time, but finally abandoned it; and from that time to the arrival of Dr. John Murray, in 1889, nothing further was done. In that

and the following year, Dr. Murray undertook a thorough investigation into the marine strata of the islands, and recognising the value of the work that still remained to be done in the Pleistocene beds, he urged the desirability of a careful examination of them being made. Several interesting discoveries of Pleistocene deposits had already been made by Mr. John H. Cooke, in the island of Gozo; and accordingly, at the suggestion of Dr. John Murray, and with the assistance of the Royal Society, Mr. Cooke undertook to carry on the work.

In the spring of 1892 the excavation of a large cavern situate in the Har Dalam Gorge was commenced, the results of which have materially assisted in clearing up many of the more debatable problems which had been left unsolved by previous workers. These results have been embodied, by Mr. Cooke, in a memoir which was communicated to the Royal Society.

The gorge in question is one of many gorges of erosion, which are to be found in the low-lying and denuded areas of Malta and Gozo. It is situated in the eastern part of Malta, and debouches on the broad, extensive bay of Marsa Scirocco. It forms the drainage channel of a catchment basin of considerable area, but owing to the very small annual rainfall of the islands the stream that now courses through it for a few occasional hours in the winter season is insignificant in size, and impotent as an eroding agent.

But it was not always so. The rounded boulders, the water-worm debris, and the curvilinear undercuttings with which the sides of the gorge are scored, as well as the character of the caves in the vicinity and of their deposits, all attest to the former action of torrential volumes of water such as could only have been formed during the existence of climatic conditions of a much more severe and humid nature than those that now endure. The cavern, which has been named after the gorge in which it occurs, is situated 500 yards from the shore on the northern side of the gorge, and consists of a main gallery, 400 feet in length, when it ramifies in various directions, forming smaller tunnels and chambers, which follow the jointings and bedding planes of the rock. One branch fissure is 250 feet in length, 15 feet high, and just wide enough for a man to pass along it, widening out at intervals into dome-shaped rock chambers.

Two of the other galleries are of considerable height, but do not exceed 20 feet in length; and they were filled with debris to within 1 ft. 6 in. to 2 ft. of the roof with a reddish plastic clay, kept moist by percolation from the roof, the sides of all the galleries being encrusted with a stalactitic lining.

The mouth of the main gallery is 26 feet wide and 10 feet high, and has been used during late years as a cattle shelter, the entrance being walled up and provided with a doorway. It widens inwards into a spacious chamber, 60 feet wide and 17 feet high, having a branch on the right hand, which was filled to the roof with alluvial soil and boulders.

The roof of the cavern was formerly covered with stalactites, but most of them have been broken off by the action of the torrents that invaded the cave, and they were afterwards buried in the floor deposits.

These stalactites and their corresponding stalagmitic bosses were observed at three different levels, each being covered by fresh alluvial deposits, indicating the intermittent character of the floods that invaded the cavern and the long periods that elapsed between them.

The deposits met with vary considerably in different parts of the cave. At the furthest extremity they are mainly composed of red loam; in the middle of large boulders, broken stalactites, and old pottery enclosed in clay; towards the entrance of a grey indurated marl with abundant remains of land shells, roots of plants, bones of deer, and boulders.

The cave is everywhere strewn with enormous quantities of waterworn boulders, similar to those met with so abundantly in the valleys and gorges of the islands.

From the differential characters exhibited by the earlier and later layers, it was evident that the deposits belonged to two distinct epochs, each of which was characterised by a special fauna.

In the lower series were found the remains of *Ursus (arctos?)*, *Elephas mnadrensis*, *Hippopotami pentlandi*, *Canis* (equalling a wolf in size), *Cervus elaphus*, var. *barbarus*, and *Human remains*; while the upper series was found to abound in the remains of man and domestic animals—pig, goat, sheep, *bos*, small land chelonian, and *Cervus elephas*, var. *barbarus*.

The discovery of the carnivore was specially significant. As early as 1859 Spratt had observed that many of the bones that