One of the qualities of the grass/legume sward is that it seems to improve soil condition and is acknowledged to increase the fertility of the soil, or at least to promote some benefit whereby arable crops in the rotation become more thrifty and more productive. Here again there seems to be an extraordinary lack of sound experimental evidence. We can infer, however, with some degree of confidence that the farmer who has been able to introduce the long ley into his farming system achieves a soil which works easier and better, and he raises the general level of his crop yields. One of the good qualities to be looked for in the grass crop, therefore, is abundant root development, which in turn fosters an improvement in soil condition and in level of crop production. Much work is urgently required in this connexion, not only in the agronomic field but also in associated fields of scientific endeavour. We need to know, for example, the influence not only of different grasses, legumes and herbs upon soil fertility, but also the influence of different systems of management of the grass crop and the part they play in building up the soil. Similarly, what is the long-term influence of different mineral products upon the soil, as well as upon the crops that are grown ?

No discussion on quality in grassland would be complete without reference to the qualities of the accepted fattening grassland of the Midlands and elsewhere in Great Britain. What is the fundamental difference between the so-called fattening and non-fattening land ? Clearly, any land is capable of fattening a bullock if that bullock can be maintained at a uniform level and reasonably high plane of nutrition throughout its fattening period. In the case of fattening land, the accepted practice is to turn the cattle beast out to grass in April and, without any purposeful management, that animal may be expected to be fat soon after midsummer almost irrespective of weather or season. In the case of non-fattening land, however, to achieve the same result in the fattened animal, one needs to plan the grazing in such a way that the animal is moved from pasture to pasture and assured of an abundance of high-quality feed which will maintain the animal at a high level of nutrition. From the scientific point of view, therefore, the distinction between fattening and non-fattening land is not a fundamental one and is in no way absolute. If the fattening animal is kept at a constant high plane of nutrition during the whole of its fattening period, then it should be possible to fatten on any land in Great Britain.

Dr. Curnow's paper on the œstrogens of grass was extremely informative. There is here clearly an important field of study which may have wide practical consequences. The technique of isolating the various œstrogenic compounds, and of relating each of them to the animal and its nutrition, remains to be worked out in detail. The observed facts are that, in Western Australia for example, excessive œstrogen occurring in a diet largely based on subterranean clover produced serious ill-effects in sheep of both sexes. Investigations in progress have shown that œstrogenic compounds are commonly found in a range of the native herbage of Great Britain, although normally not to the extent found in Western Australia. So far, little or no ill-effects have been observed in Britain, but work at the Courtauld Institute in London and at the National Institute for Research in Dairying at Reading is clearly important and aims in part at determining the role of the œstrogens in animal nutrition.

In the lively discussion which took place after the opening papers, Sir James Scott Watson indicated the practical difficulties of breeding in both the cow and the ewe so as to make use of the greater production which we can now expect from the grasslands of the world. This is a point of enormous importance, as it refers not only to our potential in lowland districts, but also to the problem of hill and marginal land. Dr. Norman Wright indicated, however, that the need for more production from grassland in Britain is immediate, if only to reduce our annual requirements of imported feeding stuffs. If properly utilized, grasslands in Britain can provide the bulk, if not the whole, of our protein requirements for cattle foods ; although we might have to import a certain proportion of low-fibre carbohydrates. Colonel J. A. Symon raised the important question of winter grass, and indicated that the problem on the farm is always to balance any extra summer carrying-capacity by an adequacy of good winter feed. Dr. R. E. Slade suggested that our level of manuring in general practice is not nearly high enough, and that with an improvement in our manurial practice and an increase in the amount of plant foods applied to the soil our grasslands would produce not only more dry matter per acre, but also more leaf, and leaf of better quality. He drew the important distinction between leaf protein and seed protein. Prof. H. D. Kay (president of Section M) closed the discussion by indicating that the animal protein factor might still prove a very important one, although speakers had suggested that a factor biologically equivalent to the animal protein factor may well be present in the leaf of high-quality herbage. William Davies

PLANETARY ATMOSPHERES

A GEOPHYSICAL Discussion was held under the auspices of the Royal Astronomical Society during its recent visit to Dublin. Prof. L. W. Pollak, of the Dublin Institute for Advanced Studies, presided, and the general subject considered was "Planetary Atmospheres", although, as will be seen, no very strict limitation to the problems discussed was really observed. Prof. Pollak, in his opening remarks, directed attention to the fact that the occasion was perhaps a historic one, since it was, so far as he knew, the first time a meeting of this nature had been held in Dublin.

As a preliminary to the main discussion, Mr. R. Naismith, of the Radio Research Station, Slough, showed a research film entitled "The Ionosphere". The film illustrated the short-period changes in the heights and degrees of ionization of the various ionospheric layers, and showed clearly the great advantage of this method of presentation of scientific data in giving a coherent picture of comparatively slowly fluctuating phenomena.

The discussion proper was opened by Prof. J. H. J. Poole, who spoke on the origin of free oxygen in the earth's atmosphere. Prior to 1924, Jeffreys suggested that the constituents of the primitive atmosphere, including all the water of the hydrosphere, were originally held in solution in the once liquid earth, and only emitted to form an atmosphere during the final solidification of the crust. Harrison Brown's work on the terrestrial and cosmical abundances of the rare gases has recently confirmed this view. It is highly improbable, as Jeffreys points out, that the gases emitted from the crust contained any free oxygen, but consisted mainly of water, carbon dioxide and nitrogen. Three theories have been advanced to explain the subsequent production of :‹e oxygen, namely: (1) evolution of free oxygen from carbon dioxide by green plants during photosynthesis; (2) thermal dissociation of a small percentage of the dense water-vapour atmosphere present efo re the condensation of the ocean, and the subsequent escape of hydrogen into space due to the high atmospheric temperature (Tammann, 1924); (3) photochemical dissociation of water-vapour by solar radiation in the upper layers of the atmosphere, and the escape of hydrogen due to the high temperature of these layers (J. H. J. Poole, 1941; P. Harteck and J. H. D. Jensen, 1948).

Prof. Poole believes that while the bulk of the geochemical evidence shows that the main production of free oxygen was due to photosynthesis, it is necessary to assume that some free oxygen has been initially produced by a non-biological process to enable photosynthesis to start. He then briefly discussed the other two proposed mechanisms. It can be shown, if an isothermal atmosphere is assumed, that the period between the formation of the surface crust and the condensation of the ocean must have been very short, of the order of a few years. This need not be considered to be a fundamental objection to Tammann's theory, since the escape of hydrogen at the assumed initial temperature of 1,200° C. would be very rapid. A more serious objection can be raised, however, to Tammann's tacit assumption that the temperature of the exosphere of the earth was comparable with that of its surface during this period. Milankovitch, basing his argument on an equation due to Schwarzschild, has concluded that the original water-vapour atmosphere would have been in convective equilibrium, and consequently that when the surface temperature was 1,200° C., the earth was surrounded by a dense layer of clouds at a height of about 200 km. and a temperature of about 100° C. If this conclusion is correct, Tammann's theory must be invalid, since the rate of escape of hydrogen would be too slow. Prof. Poole considered that further investigation of this point is required, since Milankovitch's theory leads to the peculiar paradox that the hotter the surface of the earth was, the slower it cooled. This is due to the fact that, as the earth cools, the cloud-level falls and the cloud-temperature increases, and therefore presumably the loss of heat by radiation increases.

As regards the photochemical dissociation theory, the necessary conditions of high exospheric temperature and the presence of solar radiation of sufficiently short wave-length seem to be satisfied for the existing atmosphere. The danger of concluding, however, that this would also be true for the primeval atmosphere, before the start of photosynthesis, was emphasized. The temperature distribution above the tropopause is considered at present to be mainly due to the photochemistry of oxygen and the rarity of triatomic molecules, so that obviously conditions in an atmosphere consisting of carbon dioxide and nitrogen might be very different, and it seems probable that the exospheric temperature would be considerably lower. To give any estimate of the rate of production of oxygen even in the present atmosphere seemed to Prof. Poole to be impossible, owing to lack of knowledge of the amount of water present and the intensity of light of the requisite wave-lengths. Jensen and Harteck have tackled this problem by calculating, on certain assumptions, the rate of diffusion of water-vapour into the E-layer, and assuming that this figure gives the rate of production of oxygen. By this method they claim to have shown that the proposed mechanism could, in a period of 3,000 million years, have produced more than fifty times the existing amount of atmospheric oxygen. This result, however, is perhaps doubtful, and possibly only gives a major limit.

Prof. Poole concluded by remarking that improved $V2$ rocket technique might enable us to test this theory by actual investigation of the concentration of hydrogen in the high levels of the atmosphere above, say, 150 km.

Mr. F. Hoyle spoke on the origin of the lunar craters. He maintained that, on recent cosmological theory, the moon during its early life must have suffered numerous collisions with meteorites, and that consequently the lunar craters must be due to these collisions. If this deduction is true, the craters should be fairly uniformly distributed over the surface of the moon; but, as a matter of fact, certain areas of the moon, previously considered to be seas, have no craters. T. Gold has advanced a theory to explain this fact. He considers that these areas are regions of low gravitational potential, corresponding to the ocean basins of the earth. He then assumes that owing to the rapid changes of temperature of the moon's surface, very fine dust particles, of diameter about 100 A., are eroded from the higher levels of the moon. These particles, under the combined action of attractive gravitational, and repulsive electric forces, tend to collect in areas of low gravitational potential, much as water on the earth collects in the ocean. The consequence is that the so-called seas are really areas which are covered to a depth of perhaps four or five miles with a layer of dust effectively concealing the craters present beneath.

Perhaps the most interesting contribution to the discussion was given by Dr. D. H. Menzel, who spoke on the atmosphere of Mars. He prefaced his remarks by expressing the hope that by making a strategic retreat to this planet, he might ensure for himself a fairly peaceful passage in the ensuing discussion. Dr. W. Wright, of Lick Observatory, found, about twenty-five years ago, that photographs of Mars taken in ultra-violet light show practically no detail as compared with those taken in red light. The diameter of the planet in the ultra-violet photographs also appears to be larger. For many years astrophysicists have found difficulty in explaining this effect, since an atmosphere dense enough to obscure the centre of the planet's disk in the ultra-violet should scatter red light near the edge of the disk, where the effective depth of the atmosphere would be much greater. Consequently, the ultra-violet and red images should be of equal diameter, and the central area of the planet should appear bluish in colour. Dr. Menzel stated that he pointed out this fact twenty-five years ago, and then doubted the reality of Wright's observations. Despite his scepticism, however, he was afterwards admitted to Lick Observatory, and since then has become convinced of the validity of Wright's results. He now attributes the larger ultra-violet image to a thin cloud layer of carbon dioxide crystals which exists at a height of about 100 km. in the Martian atmosphere due to the temperature at this height being about 100° K. This layer would be capable of scattering ultra-violet light, but is transparent to red light, and thus accounts for Wright's observations.

Dr. Menzel then referred to the scarcity of water and the absence of oxygen in the Martian atmosphere. He considers that the polar caps consist of a very thin layer of ice crystals, perhaps only a fraction of an inch thick. During the long Martian summer the polar cap evaporates by sublimation without melting, and the actual surface temperature in the area of perpetual daylight may rise to about 20° C., the corresponding winter temperatures ranging from about −30° C. to −90° C. The atmosphere is usually very clear, but thin clouds have been observed to form in the late afternoon, and disappear during the early morning shortly after sunrise. These clouds are probably composed of fine ice crystals. An observer on Mars would see a deep blue sky with violet-coloured clouds forming at sunset.

Dr. Menzel concluded by referring briefly to the possibility of the existence of life on the planet. He considers that while the existence of higher animal types of life similar to terrestrial forms is unlikely, it is possible, as G. P. Kuiper has previously noted, that some simpler forms of vegetation, such as lichens, may do so. This would account for the seasonal change in colour observed in certain dark Martian surface features.

Space can be found here for mention of only a few of the many interesting points raised by the subsequent speakers. Dr. E. Opik considered that owing to infra-red absorption, the temperature of the carbon dioxide cirrus must be nearer that of the Martian surface (190° K.) than the postulated 100° K. required for its formation. Dr. Menzel agreed that this seemed an objection, but pointed out it might also apply to temperature problems in the earth's atmosphere. Dr. H. H. Poole and others raised the question of the possible origin of repulsive electric fields on the moon. Friction between the dust particles and the moon's surface was suggested, but an attractive, not a repulsive, force would be produced in this way. It was finally decided that a repulsive force could only be produced by both the particles and the moon's surface becoming positively charged through the emission of photo-electrons. Several speakers doubted the meteoric theory of the formation of the craters on observational grounds, and also maintained that the seas present the appearance more of extensive basaltic lava flows, rather than of presumably featureless dust surfaces.

Prof. W. M. H. Greaves raised the problem of the absence of free oxygen from other planetary atmospheres. Prof. Poole considered that for the major planets this is due to the fact that these planets have been able to retain hydrogen, and that for Mars and Venus it is caused by the absence of water. Dr. Menzel stated later, however, that he is not convinced that water does not occur on Venus, and he attributed the negative spectroscopic evidence to the occurrence of water as hail in a convective atmosphere. He pointed out that Lyot's observations on the polarization of the light from Venus seems to indicate that the clouds are composed of water, and mentioned that further work on this problem is being undertaken at the Lick Observatory.

Mr. Hoyle considered that the absence of free oxygen in the original atmosphere would not prevent the formation of an ionosphere. He further maintained that Tammann's theory must be rejected, because it would have supplied too much oxygen; and hence the theory really proved that no water was present in the original atmosphere, and that all the hydrogen contained in the existing oceans has been captured later by the earth from space, the oxygen required to form water being derived from the atmospheric carbon dioxide.

THE WORK OF THE MOTOR YACHT *MANIHINE*

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SINCE the summer of 1947, Major H. W. Hall, owner of the motor yacht *Manihine*, has placed this vessel at the frequent disposal of the British Museum (Natural History) for the purpose of marine biological investigations. In British waters work has been almost entirely confined to the English Channel, more particularly to the lesser-known eastern half. Besides adding to the national collections, these cruises over a period of four years have done much to increase our knowledge of the composition of the bottom fauna and summer plankton of the eastern Channel. Samples of this plankton have been made available to the Marine Biological Laboratory at Plymouth, while from time to time work has been undertaken for the Fisheries Laboratory, Lowestoft.

During the winter of 1948–49, the *Manihine* was concerned with a hydrological and biological survey of the little explored Gulf of Aqaba in the northern Red Sea. Reports of this survey are now ready for publication. It is planned to extend this work during the coming winter to the Sudanese Red Sea. The biological work will mainly consist of routine plankton hauls, trawling and dredging (if the character of the sea floor permits), fishing by line and collections along the coral reefs. As in the earlier survey of the Gulf of Aqaba, observations will be made on surface and sub-surface temperatures and salinities. Records from a deep-water bathythermograph will also be taken.

It may reasonably be expected that the results of the forthcoming work in the Sudanese area, together with those obtained earlier in the Gulf of Aqaba, will add considerably to our knowledge and understanding of the marine fauna of the Red Sea. In particular, it is hoped to learn something of the nature and extent of the deep-water fauna, which is at present not well known. There are, for example, records of only three species of bathypelagic fishes from the Red Sea. How far the shallow sill at the southern end may act as a barrier to the immigration of bathypelagic forms has yet to be determined.

It is the problem of barriers and the possible limitation of genetic interchange between Red Sea and Indian Ocean forms which makes a study of the fauna particularly interesting. The Red Sea is, geologically speaking, a relatively young sea. Its present connexion with the Indian Ocean, from which the greater part of its fauna has been derived, is thought to be about nine to ten million years old.