with certainty that a particular type of implement was present. Mr. Reid Moir's collection appeared to him to present all the characteristics which one would expect to find in a group of implements belonging to the Le Moustier culture.

Mr. Kennard said that palæontologists were firmly convinced that the Boulder Clay was of late occurrence. An examination of a continuous series of examples of the fauna exhibited no traces of the variation between warm and cold types, which would have been expected to occur if the theory of alternating warm and cold periods were correct. Fauna of the cold period were always last in the series. Mr. H. Bury said that the evidence brought forward by Mr. Reid Moir made it necessary to raise the question whether Chalky Boulder Clay was always, and wherever it occurred, of the same age. The evidence from Hoxne was diametrically opposed to Mr. Reid Moir's results, and this, together with the doubts which had been expressed as to the character of the evidence obtained from Hoxne, made it desirable that the borings on that site should be repeated.

RECENT ENGLISH MARINE BIOLOGY.

RECENT English papers on marine biological research include one by Dr. E. C. Jee on the hydrography of the English Channel during the years 1904–17. This forms part i. of the Fisheries Investigation Series III., the publication of which is now resumed by the Board of Agriculture and Fisheries. Periodicities in the physical properties of the Channel water are discussed, and correlations between these and the pilchard fisheries are apparently established. A most interesting "Contribution to the Quantitative Study of Plankton" is published by Dr. E. J. Allen in part i., vol. xii., of the Journal of the Marine Biological Association.

Plankton investigations, in so far as they have been quantitative, have been a series of approximations to a complete determination of the number of organisms of all kinds contained in a unit volume of sea-water. Hensen's original method consisted in the use of a net made of fine-meshed silk cloth which was lowered in the sea and hauled to the surface. Experiment and calculation gave a coefficient for each net, from which the area of cross-section of the column of water filtered could be approximately determined. It has been found latterly that the greater number of microscopic organisms in the water escaped through the meshes of the cloth, and more refined filtering methods were introduced by Lohmann.

Finally, it was thought that by centrifuging small quantities of water a complete enumeration of the organisms present might become possible, and this method did, indeed, largely increase the numbers inhabiting unit volume of sea-water. Why it should not enable the investigator to determine *all* is not easy to see, but it certainly under-estimates them, as Dr. Allen's results show. In his experiments small quantities of water (10 c.c.) were centrifuged and the contained organisms counted. From four such trials a mean of 14·45 per c.c. (or 14,450 organisms per litre) was obtained. The same water sample was then examined by inoculating ½ c.c. in a sterilised sea-water containing the culture solutions used by Allen and Nelson for the study of marine diatoms. The inoculated medium so prepared was then distributed into seventy small flasks, each containing about 20 c.c. of the liquid, and the latter were allowed to stand for several weeks. The colonies (mainly Diatoms and Flagellates) growing in the flasks were then identified and counted, giving an estimated number of at least 464 organisms per c.c. (or 464,000 per litre). Even then it is evident that the result is an under-estimate of the actual population of the water sample, for the medium is apparently selective, and organisms that appeared in the centrifuged samples did not grow (and were therefore unrecorded) in the cultures. Bacteria did grow, but were not identified and estimated. The result is therefore another, and closer, approximation to a biological value which is of extraordinary interest.

J. J.

THE RED COLOURING MATTER OF PLANT GALLS.

DURING recent years our knowledge concerning plant colouring matters has rapidly increased, and quite a large number of pigments have been subjected to careful and full investigation. A further interesting contribution to our knowledge in this field of research is contained in a recent paper by Dr. M. Nierenstein, in which he deals with the colouring matter of the "red-pea gall" (Trans. Chem. Soc., 1919, cxv., pp. 1328–32). The galls that were examined occur on the leaves of various British oak-trees when galled by *Dryophanta divisa*, Adler.

It has been generally assumed that the red colouring matters of these and similar galls belonged to the anthocyan class, and one of the objects of the investigation was to ascertain whether the anthocyan assumed to be present was related to quercetin. By this means it was hoped to obtain some light upon the relationship between the products present in the normal plant and those pathologically produced as the result of the formation of the galls.

The investigation resulted in the isolation of a red pigment, to which the name "dryophantin" has been given. Dr. Nierenstein concludes that this colouring matter is not an anthocyan, but a diglucoside of purpurogallin (the first derivative of purpurogallin to be found in Nature), and that, like gallotannin, it is of pathologic origin. He is of the opinion that the other so-called anthocyans obtained from plant-galls are in all probability not anthocyan colours at all, but related to "dryophantin." In view of this he proposes to classify these red pigments under the class-name "gallorubrones."

This paper is of considerable interest, particularly if further investigation confirms the presence of purpurogallin derivatives as regular constituents of these and other red galls. In respect of the conclusion implied as to the absence of pigments of the anthocyan group, the present paper is not sufficient evidence of such absence, for the process whereby the colouring matter has been isolated is such that there is a very considerable doubt whether many anthocyan pigments would survive the treatment.

IONS AND IONISATION.

THE Faraday Society, though a small body, is very active. One of the most useful features of its activity is the holding of general discussions on matters of scientific and technical interest, and the publication of these discussions in its Transactions. "The Present Position of the Theory of Ionisation in Solution" was the subject of a discussion held on January 21, 1919, and the report is now issued in the form of a separate reprint (pp. 178, Faraday Society, 10 Essex Street, Strand, W.C.2, price 12*s.* 6*d.*), thus making it available to a larger public than the members of the society.

The discussion was opened by Dr. Senter, who briefly reviewed the position with regard to such outstanding problems as the hydration of ions, the deviation of strong electrolytes from the mass-action law, and the chemical activity of ions and non-ionised molecules respectively. Communications on these and other topics were afterwards read and discussed, amongst the contributors or those taking part in the discussion being Arrhenius (the originator of the theory), Acree, McBain, Bousfield, Sand, Partington, Porter, Newbery, Lindemann, Philip, and J. C. Ghosh.

Fundamental differences of opinion with regard to the main problems discussed were very marked. The evidence for the hydration of ions is by some held as final, by others as having only a limited application, and by yet others as quite inconclusive. Most attention was given to the problem of the abnormality of strong electrolytes. At extreme dilutions these electrolytes are regarded by many as behaving normally, the dissociation constant for uni-univalent electrolytes being about 0·02, but owing to the magnitude of the water-correction, and the difficulty of exactly fixing the molar conductivity for infinite dilution, this result must still be looked upon as uncertain. At ordinary dilutions the law has no application, and the most promising explanation is that elaborated by Dr. J. C. Ghosh, who proceeds on the assumption that the strong electrolytes are practically completely ionised in all dilute solutions, but that there is an electrostatic equilibrium between mobile ions, which contribute to the conductivity, etc., and inert ions, which do not. An electrical dilution law is therefore substituted for the chemical mass-action law in the case of strong electrolytes. For weak electrolytes this electrical action would only enter as a negligible disturbing factor of the mass-action law. The further development of this idea may be awaited with interest.

THE SPECIES CONCEPT AMONG FUNGI.

IN the Transactions of the British Mycological Society (vol. vi., part ii., September, 1919) Mr. W. B. Brierley protests against the practice of mycologists in describing as species the forms which are presented to them in Nature or as pathological growths, especially on cultivated plants. The description of new fungal species is based on the assumptions that the distinguishing characters are of a morphological nature, and that the essential specific characters are constant and hereditary and may be determined in one specimen of one generation. But the laboratory and field experience of the experimentalist shows that under changes in the environment the whole structure and facies of the organism may be transformed, while under identical conditions there is considerable evidence that the morphological variation of a particular fungus is definite and constant. The so-called species of the mycologist is comparable with the "ecad" of the ecologist, and is the resultant of the organism and its environment. "Ecads" indistinguishable from each other may be produced from two distinct organisms interacting with one and the same environment, or with two different environments. Two precisely similar fungi growing on a potato and a decaying tree-stump respectively may really be different species, though the systematic mycologist would consider them identical. The true organism is a physiological equilibration, a metabolic entity, the interaction of which with the environment results in the growth-form or "ecad." It follows that the morphological species concept must be given up in favour of the physiological species concept. The only exact method of determining species is by means of quantitative data derived from cultural treatment under standardised physico-chemical conditions, for this method alone reveals the physiological condition of the organism. The author suggests that even the apparently stable forms of the higher fungi, Agarics, Polypores, etc., are merely "ecads," and that two precisely similar morphological entities of, for instance, *Agaricus melleus* may conceal totally different physiological constitutions which under other conditions of growth would diverge characteristically.

Mr. Brierley also attacks another concept of mycology, namely, the "educability" of fungi, or the induction by suitable treatment of permanent modifications in their biochemical, morphological, or other properties. This concept is widely held by microbiologists, but if it implies a possibility of a change in the physiological constitution of an organism, it follows that with fuller knowledge and improved technique a rapid change of one species into another is possible. The author affirms that the condition of knowledge and the available evidence are not such as to warrant an hypothesis so subversive of the foundations of biological science. The presumed mutations may be due to the presence of mixed populations in supposed pure cultures, or merely the expression of a developmental stage previously unrecognised; further, no organism in which sexuality exists or is conceivable must be used unless its gametic constitution and genetic behaviour under all the conditions of the experiment are known.

THE UPPER LIMIT OF UNPLEASANT BEATS.

IT is well known that Helmholtz traced all discordant effects of two or more musical notes when sounded together to the presence of beats occurring between the prime tones, between the prime of one and an upper partial of the other, between the upper partials of each, or to beats occurring in some other way. Further, to produce the unpleasantness in question the beats must lie between certain limits of frequency, which limits vary with the pitch in use.

In this connection it is of interest to note that Mr. Narendranath Chatterjee, of Chittagong, India, has recently given a formula expressing the upper limiting frequency for beats for which the roughness vanishes. This formula he writes as follows:

$$B = \frac{N}{0{\cdot}7 + n + i}, \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where B is the number of beats per second for which the roughness vanishes, N is the frequency of the lower of the two tones sounded simultaneously, n is the number of the musical scale containing N and beginning with 32 per second as the fundamental of the first scale (the octave of this being the fundamental of the next scale, and so on), and, finally, i is the interval between N and the fundamental of the scale in which it is contained.

Thus N, n, and i are connected by the equation

$$N = 32 \times 2^{n-1} \times i \quad . \quad . \quad . \quad . \quad . \quad (2)$$

The results of the law compared well with Mayer's experimental values, as shown in the following table:

Frequency N of lower tone	Frequency B of beats when roughness vanishes	
	Mayer's values	Results of law
64	16	17
128	26	27
256	47	45
512	78	76
1024	135	133