this first trip could give, you may take more stations and investigate smaller areas more carefully.

I am glad to be able to say that in 1921 Dr. Russell, on the English steamer *John Bligh*, made the first trip across the North Sea with my bottom-sampler, guided by my assistant, Dr. H. Blegvad; they found some of the same communities between Lowestoft and Esbjerg as we know from the Kattegat.

Thanks to the bottom-sampler we can now speak about areas with a Venus mussel community, an *Amphiura filiformis* community, a *Brissopsis Amphiura chiajei* community, and so on, as we on land speak about a heath, a beech-wood, a meadow, etc.; we are also able to get a quantitative idea of the amount of animals on the sea-bottom, and are able to follow seasonal or other variations therein.

A dredge will sometimes give us, when well used, a bagful of animals, belonging to the epi-fauna as well as to the ordinary communities, and taken up from all the communities it has been towed over. The dredge is inclined, moreover, to take all animals *on*, not *in*, the bottom, and its content is therefore not a true illustration of what is living *in* or *on* the bottom, but a mixture mostly of epi-fauna from different communities, without giving the slightest idea of quantity per square metre. The content of a dredge and a bottom-sampler used on the same station will very often give quite different collections of animals.

The dredge has given excellent information to zoologists wishing to collect rare animals for preservation in alcohol, and for dredging oysters, and so on, but a true illustration of the fauna on the sea bottom it never has given and never will give.

I admit one thing: it is easy for me to speak and write about the bottom-sampler work, but it never will be well understood without seeing the work going on on board ship; many men of science from Europe have seen how quickly the sampler may be used, like an ordinary sounding machine, and how well it works. I should be glad to welcome many more visitors at the Danish Biological Station, not only to see the bottom-sampler working, but also to be able to discuss with them the problems which have arisen in my mind while using this method during the last 10 to 12 years.

It was a Dane, O. Fr. Müller, who first introduced the dredge in northern Europe for scientific use, and it will always be used by zoologists and for special purposes, but only the bottom-sampler is able to give a true and quantitative representative illustration of the bottom fauna.

Finally, I wish to say that to have a bottom-sampler and to use it is not enough to become a great marine biologist; it depends much upon the possession of working ideas. The bottom-sampler is not able to solve every question; it cannot, *e.g.*, take animals living very deep in a hard bottom, and the apparatus must be modified for special work, according to the size of the ship used, the depth at which you are working, etc., and it is necessary to supplement the investigation by means of other apparatus, fishing-gear, dredges, etc. But without quantitative work it is not possible to understand the principal features of the fauna of the sea-bottom.

It would be a matter of great scientific interest to have a bottom-sampler used down the slope of the continent at all depths, out on the very ocean floor, to determine all the communities living here, and to prove how barren the ocean floor really is. It would also be of great interest to follow our European communities from the North Pole down to Cape Town, to study their geographical distribution, to determine the perfectly unknown Arctic communities, and the unknown tropical communities. I have given a hypothetical chart in my Report No. 22, but it has to be verified. I am too old to do that, and my steamer too small. I hope other men will do it. I am sure the geologists would be glad to know something about these communities, based upon the common animals. I am certain that, like me, they care much more for common characteristic species and their distribution than for "rare" animals.

The productivity of the bottom fauna in European waters is by no means unlimited; it is, therefore, a matter of the greatest importance for some of the greatest fishery questions to know as much as possible about this productivity. The English fishermen are, as I often have heard, the backbone of the English navy; they depend upon the fishes, and these in turn depend upon the fish-food. Careful investigation of the latter is, therefore, a matter of great importance—particularly for Great Britain.

Adhesives.

By EMIL HATSCHEK.

THE treatise of Theophilus Presbyter, entitled "Diversarum Artium Schedula," and well known to all students of the history of painting, gives directions for the preparation and use of glues from leather and deers' antlers, of plum- and cherry-gums, and of mixtures of cheese and lime described as "cheese glues." This list of adhesives familiar to craftsmen at the end of the eleventh century covers practically all the types in use at the beginning of the twentieth century. A similar degree of old empirical perfection is shown by many arts employing colloidal material, and the student of colloid chemistry anxious to magnify his office is perpetually confronted with the task of explaining the *rationale* of traditional procedure and of suggesting improvements based on theoretical grounds.

The difficulties of this task are well illustrated by the first report of the Adhesives Research Committee.[1] Towards the end of the war a shortage of glue and of the chief substitute, casein, threatened to limit the output of aircraft, and the labours of the committee were accordingly directed, on one hand, to a close study of glue, and, on the other, to the discovery of possible substitutes other than casein. The report contains much interesting and novel matter under both heads.

The difficulties in the way of a rational study of glue seem to be twofold. The first is that the only criterion of its value as adhesive is a mechanical test of a glued joint between wooden test pieces of specified nature and size. The report describes the conditions of such a test, as finally adopted, and sets forth the possible sources of error. Both on theoretical and on practical grounds (about five days have to elapse from the soaking of the glue to the actual breaking test), it is desirable to find some easily measured constant which shows a simple quantitative relation with the breaking strength. No such constant is yet known, although empirically the setting time of the glue sol, the melting point of the gel and its "strength," *i.e.* roughly speaking, its modulus of elasticity, furnish some indication of its quality.

The second difficulty is of a more fundamental nature. It is known that pure gelatin is not a good

[1] Department of Scientific and Industrial Research. "First Report of the Adhesives Research Committee," pp. iv+129. Price 4s.

adhesive, so that the superiority of glue must be due, directly or indirectly, to the presence of other substances of which, so far, little is known. Investigations on this point are proceeding; in the meantime the committee have evolved a novel and highly promising test, that for "diffusible nitrogen." A gel of standard composition is immersed in a known volume of water, and after a fixed time the nitrogen content of the latter is determined by Kjeldahl's method. This is, of course, due to compounds of much lower molecular weight or aggregation than gelatin, and—apart from some exceptions—the amount of diffusible nitrogen is roughly inversely proportional to the tensile strength. While this result is of great interest, it can scarcely be said to simplify the problem stated above, namely, what factors cause the difference between pure gelatin and glue. Speaking, however, quite generally, we know of no connexion between constitution and adhesive properties; the striking fact is how sparingly the latter are distributed between a very few materials even among highly hydrated colloids.

Lack of space forbids detailed reference to the very interesting investigations on the extraction of gelatin from various raw materials, but the committee's successful attempt to find a strong vegetable adhesive must be mentioned. A protein was prepared from castor bean residues—which are poisonous and therefore useless as cattle food—and this protein forms a strong adhesive with calcium hydroxide and alkaline salts in various proportions. From the data given regarding the solubility of this protein, it appears to be related to casein, and the mechanical properties of the adhesive prepared from it are not much inferior to those of casein glues.

The report is supplemented by an appendix—which greatly exceeds in length the report itself—giving a "Descriptive Bibliography of Gelatin." This is a very complete, lucid, and impartial summary of the vast literature, in which no paper of any interest seems to have been overlooked. Those from English sources—though important—are remarkably few in number, and this state of things suggests questions which are none the less curious for being familiar. One is whether the development of a very promising discipline is going to be left to workers of other nations as completely as was (to take an unhackneyed instance) that of the theory of functions; the other, whether such cases of neglect arise from deep-seated national tastes or idiosyncrasies in research, or merely from inadequate opportunities for tuition and experimental work.

The Decomposition of Tungsten.

THE September issue of the Journal of the American Chemical Society contains an account of the preliminary experiments made by Drs. Wendt and Irion on the decomposition of tungsten at extreme temperatures, with the production of helium, a report of which appeared in the daily press, to which reference has already been made in NATURE (April 1, 1922, vol. 109, p. 418). The authors regret the exaggerated early report, given wide publicity by the press after its oral presentation, and emphasise the preliminary character of the work. They describe fully the apparatus used for attaining temperatures above 20,000° by passing heavy currents through metal wires, and state that when tungsten wires are exploded in a vacuum at such temperatures the spectrum of helium appears in the gases produced. When the explosion is conducted in carbon dioxide, 0·713 milligram of tungsten gave rise to 1·01 c.c. of gas not absorbed by potash solution. The authors remark that their method "includes factors, both of cause and of error, analogous to those operative in the voluminous and inconclusive controversy on the evolution of helium in various types of low pressure electrical discharge tubes, extending from 1905 to 1915."

The electrical apparatus provided for currents of 40 amperes at 100,000 volts during the brief period necessary to charge the condenser, which was then discharged through a tungsten wire 0·036 mm. diameter and 4 cm. long. The wires were stretched between heavy copper terminals in a special spherical glass bulb of 300 c.c. capacity, which was capable of withstanding momentarily an enormous outward pressure, and had a small discharge tube sealed on for examination of the spectrum of any gas produced. The wire was heated to well above 2000° for 15 hours in a high vacuum before the explosion was made, and the tube before explosion showed no spectrum or fluorescence when connected with a 50,000-volt coil. No dust, smoke, or solid residue was left after the explosion. Gas was present, which showed the faint presence of the strongest green line of mercury, probably from back diffusion of the pumps, and the only other line uniformly present and positively identified was the strong yellow line of helium. It would seem that both hydrogen and neon were absent. The absence of hydrogen is of interest, since the atomic weight of tungsten is exactly 46 times that of helium, and this element would therefore not be expected to give hydrogen on disruption of its atom.

The explosion in carbon dioxide seems to have been less conclusive, as the authors do not seem to have been quite sure of the absence of unabsorbable impurities. They point out that if the entire weight of 0·713 milligram of tungsten had been converted into helium, 4 c.c. of this gas should have been obtained. The much smaller volume found would point to the production of heavier gases. Altogether the work is of very great interest, although the authors emphasise the necessity of complete analysis of the gas obtained before anything conclusive can be stated. This chemical test is to be made in the continuation of the work.

The Belt of Political Change in Europe.

IN a paper contributed to Section E (Geography) of the British Association at Hull, Prof. J. F. Unstead commented on the striking fact that the new states of Europe, or those which have gained or regained independent existence during recent years, lie in a relatively narrow belt of country extending across the whole of Europe from the Arctic Sea in the north to the Mediterranean in the south. West of this belt changes have been slight, while east of it a final settlement has not been reached. Of this belt no part has been exempt from change. It contains about 100 millions of people or about one-fifth of the inhabitants of Europe, and covers about one-fifth of the total area of the continent. The new states have been formed mainly by the break-up of three great empires, the disintegration of which was one of the results of the world war.

Prof. Unstead pointed out that the belt of change is a region caught between east and west, marginal to each and influenced by each, and he showed how