On the other hand, the crowd of hydrogen or mercury atoms, by virtue of molecular motion and inter-collisions, act as gases. Whilst their *mean* free paths are conditioned by the degree of exhaustion, there may be amongst them a certain number of *actual* free paths differing widely on each side of the mean. Under the influence of the electromotive force, and at the right degree of exhaustion, these atoms arrange themselves in groups,[1] while the rushing swarm of electrons driven from the negative pole meet them and render them visible. According to J. J. Thomson, the mass of an electron is about the 1/700th part of that of the hydrogen atom, and as these masses start from the negative pole in a vacuum tube with a velocity of the order of half that of light, it is easy to see that their heating, phosphorescent and mechanical power must be stupendous.

The basis of the electron, as I foreshadowed in 1879 in the case of radiant matter, is probably the same in all cases—the protyle from which the chemical atoms were assumed to be formed.

On the two-fluid theory, the electrons constitute free negative electricity, and the rest of the chemical atom is charged positively, although a free positive electron is not known. It seems to me simpler to use the original one-fluid theory of Franklin and to say that the electron is the atom or unit of electricity. Then a so-called negatively charged chemical atom is one having a surplus of electrons, the number depending on the valency, whilst a positively charged atom is one having a deficiency of electrons. Differences of electrical charge may thus be likened to debits and credits in one's banking account, the electrons acting as current coin of the realm

SCIENTIFIC WORK OF THE GERMAN ANTARCTIC EXPEDITION.[2]

THE head of the German Antarctic Expedition, Prof. Dr. Drygalski, has sent from Cape Town to the home authorities a number of full reports on the work which had been carried on by the expedition up to the date of their despatch. As is well known, the ship, which had been specially built for the expedition, was long overdue at Cape Town, and her protracted non-appearance gave rise to some anxiety. We give the following extracts from the official report, which will shortly appear, in order to furnish evidence of the activity of the staff, and of the reasons for the great protraction of the voyage.

In the scheme of operations for the expedition, it had been arranged that visits to two land stations should be made during the voyage to Cape Town, in order to determine, by fresh comparisons with the absolute magnetic elements at those land stations, the changes in the magnetic character of the ship since its determination at Kiel before sailing. The magnetism of a new ship is always subject to changes in course of time, but these changes are more especially caused by change of magnetic latitude as the ship passes from one hemisphere to the other. With this object, the following places were selected as apparently desirable:—The Cape Verdes or Madeira, north of the magnetic equator, and Bahia or Ascension to the south of it.

[1] In an address delivered before the Institution of Electrical Engineers, January 15, 1891, I gave an outline of a theory of stratifications in rarefied gases. The following quotation renders my meaning clear:—"If, in any much-frequented street, at some time when the stream of traffic runs almost equally in both directions, we take our stand at a window from which we can overlook the passing crowd, we shall notice that the throng on the footway is not uniformly distributed, but is made up of knots—we might almost say blocks—interrupted by spaces which are comparatively open; we may easily conceive in what manner these knots or groups are formed: some few persons walking rather more slowly than the average rate slightly retard the movements of others, whether travelling in the same or in an opposite direction. Thus a temporary obstruction is created. The passengers behind catch up to the block and increase it, and those in front, passing on unchecked at their former rate, leave a comparatively vacant space. If a crowd is moving all in the same direction, the formation of these groups becomes more distinct. Hence mere differences in speed suffice to resolve a multitude of passengers into alternating gaps and knots. Instead of observing moving men and women, suppose we experiment on little particles of some substance, such as sand. If we mix the particles with water in a horizontal tube and set them in rhythmical agitation, we shall see very similar results, the powder sorting itself with regularity into alternate heaps and blank spaces. If we pass to yet more minute substances, we observe the behaviour of the molecules of a rarefied gas when submitted to an induction current. The molecules here are free, of course, from any caprice, and simply follow the law I seek to illustrate, and though originally in a state of rampant disorder, yet under the influence of the electric rhythm, they arrange themselves into well-defined groups or stratifications."—*Journ.*, of the Inst. Electrical Engineers, vol. xx, p. 10.

[2] Based upon an Article in *Der Tag*, Berlin, January 25.

After consultation with the magnetician of the ship, Dr. F. Bidlingmaier, I had selected Porto Grande in St. Vincent and Ascension. If, for any reason, Ascension proved to be inaccessible, it seemed advisable to adopt the usual plan, on board ship, and determine the deviation by swinging the ship on eight different courses in the open sea. During our stay at Porto Grande, which lasted until September 11–16, the magnetic observations were our principal business, and we succeeded in determining, on board the ship, the deviations in the Magnetic Declination, Total Force, and in Horizontal and Vertical Force due to her magnetism.

I myself landed, with two assistants, and set up a tent near the spot where the shore magnetic observations were being carried on, in order to secure time observations to rate our chronometers and watches, and also to make some observations of the force of gravity. Owing to the weather, we could make no astronomical observations.

My orders for the next part of the voyage were to cross the equator on the 18th meridian, and then to make for Ascension. The object of the first position was to verify the sounding of 7370 metres (4030 fathoms) [the greatest depth on the line], which had been obtained by the French man-of-war *La Romanche* in 0° 11′ S. and 18° 15′ W. As this figure is not mentioned on the British charts of soundings, nor in the recent critical representation of sea depths, by Prof. Dr. Supan, I therefore wished to trace its possible connection with the great depths of the Brazilian basin.

The visit to Ascension was to attain the objects above named. It was evident enough that the carrying out of this plan would present some difficulties, for the usual sailing track to the Cape (which was best for our ship, owing to the low power of her engines, which would not allow of steaming against the S.E. trade with its accompanying sea) crosses the equator far to the westward, probably as far as the 25th meridian. A visit to Ascension would entail our steering a south-easterly course immediately on leaving the Cape Verde, so as to be able to make a south-westerly course to the island under sail. The course indicated was to be first tried and tested as to whether it would take too much time. We crossed the belt of calms under steam, between the two trade winds. In this swell the *Gauss* rolled, at times very heavily, so that much glass or other breakable articles in the laboratory came to grief, while the ship, under sail, even with a stiff breeze and a good deal of sea, had been remarkably steady. This swell retarded our progress considerably, as of course our speed was greatly reduced. This was aggravated by increased fouling of the bottom. As the ship was very low in the water, the screw well, through which the screw and rudder could be lifted, on meeting ice, so as to preserve them from damage, may have contributed to the prolongation of the voyage. In short, we proceeded very slowly along the prescribed track, where the wind failed us, and the currents at least gave no help, though the engines worked perfectly, and gave promise of a very satisfactory performance whenever we should come into a state of sea checked by the presence of ice.

All these impediments retarded us with enhanced insensity when we met the S.E. trade on the line. This was very fresh (and we should have liked to have had a similar force in the N.E. trade), but we could make no use of it, as it was dead ahead on our course for Ascension, and it brought with it a trying [swell and current. The rate of the ship got less and less, and at last stopped entirely on October 5. In these circumstances, as time was getting on (we crossed the line a few days after the entry of the sun into south declination on October 1, so that we ran directly from the northern into the southern summer), it seemed therefore desirable to reconsider our plan and give up the Ascension visit entirely, and so on October 6 I decided to do this and use the existing S.E. trade for a run to the Cape, starting on that day. As soon as we changed our course we made at once a speed of six or seven knots. On October 7 we disconnected the engines and made sail, but the wind did not last. On October 9 the S.E. wind died away, and these light winds continued, with some slight exceptions, up to Cape Town. The *Gauss* always made very short daily runs, so that we had a very long passage. The light winds and fair weather were the cause, as we had only one storm, November 18–20, just at the end of the voyage. We were naturally obliged to husband our coal so as not to lighten the ship too much.

On October 30, the after steam capstan was connected with Prof. Vanschöffen's vertical net, which was at a depth of

2000 metres, and above it hung my deep-sea thermometers and five or six buckets, at the depth of 1500 metres. As each bucket came up, Dr. Gazert and Dr. Philippi speedily emptied it of its contents to search for bacteria and determine the amount of contained gas. Dr. Bidlingmaier, on the captain's bridge, regulated his registering apparatus in the meteorological screen, while Captain Ruser, beside him, kept the ship heading the swell and watched that the deep-sea lines should not get foul and that the ship should not overrun them. The chief engineer, Stehr, on the after bridge watched the sounding apparatus with me. The first officer looked after the line as it came up and quickly dismounted the attached instruments. Vahsel saw to the running of the windlass itself, and Ott, in the small dinghy, picked up a huge albatross which Dr. Gazert had shot, and which was at once dissected by the practised hands of Dr. Werth. Then came up Dr. Stehr's question as to how many wheels were running on board at once, without actually counting them.

On Saturday, November 23, we reached Cape Town, having made some magnetic observations near the coast. On Saturday, December 7, the expedition will start again.

I can only say, in conclusion, that we shall never forget the warmth of the reception we met with, not only from the Imperial Consul-General von Lindequist, the members of his staff and the German colony, but also from the officials and scientific men of Cape Town, which rendered our stay there particularly pleasant.

THE USE OF ANATOMICAL CHARACTERS IN THE IDENTIFICATION OF WOOD.[1]

THE chief contributions to the study of the secondary wood of plants have been made by students of forestry, amongst which the names of Nördlinger, Hartig, Brandis, Gamble, and of many men connected with the Indian Forestry Department, deserve our respect. The school of Radlkofer (especially Solereder) has done good work in connection with the structure of the primary wood, which throws many sidelights upon that of the secondary wood, yet there is much less help to be derived from their studies than one would suppose, because there is frequently much difference in the structure of the two classes of tissue.

The grouping of the vessels and the medullary rays and the arrangement of the wood-parenchyma are frequently so characteristic that various genera can be recognised by a glance at the transverse section, *i.e.* horizontally as the tree stands; and, further, it is by no means rare to find the same structure running through a whole genus or, less frequently, through a whole order. A hundred genera could be cited which exhibit a strong family likeness, and of the Proteaceæ and Sapotaceæ it may be said that the description of the structure of the wood of one species will practically serve for the whole order. On the contrary, there are orders which appear to consist chiefly of exceptions, as in the case of the Celastraceæ, where it is difficult to find two genera with any important feature in common. The structure of the woody portion of cryptogams has been employed for years in the study of fossil plants; that of the monocotyledonous trees and of the conifers is notoriously uniform, and is as sure a guide to their position in the natural system as any external character. Why then should not the same rule apply to the angiospermous dicotyledons, and for what reason should the thread be lost as soon as we pass from one division of the vegetable world to another? It seems a by no means extravagant idea that, inasmuch as it is quite indifferent to the welfare of a plant what the structure of its woody portion may be so long as it performs the mechanical duties imposed upon it, ancestral traits should be preserved undisturbed in the wood more than in any other part.

Ignoring this debatable question there is no doubt whatever of the economic importance of this study. There are not only so many kinds of timber in use in Europe and elsewhere, but there are great numbers which are destined to become useful, together making a variety with which no timber dealer can keep *en rapport* by the old method of rule of thumb. It is still more difficult in the colonies and in new countries to tell one wood from another, because the number of persons possessing the necessary training is smaller than at home. The popular and vernacular names are in many places so frequently duplicated or misapplied that they are useless as guides unless the structure of the wood be taken into account. Instances could be multiplied in which wrongly named timbers have been referred to their proper titles, and of inquiries for unknown woods being directed into the proper channel, and of cases in which attempted deception has been frustrated by the anatomical method.

For practical purposes it is rarely necessary to use high powers of magnification or to study the sculpture upon the walls of the cells. A pocket lens or a two-inch objective will frequently suffice to display the special character of the structure. If higher powers be used this individuality, as I may call it, is lost, as it is dependent upon the arrangement or complex of the elements. For instance, the radial or tree-like arrangement of the vessels in the wood of all the trees of the genus Quercus is recognisable by the naked eye, but it fails to be striking when viewed under a half-inch objective. This particular feature may be traced through the genera Corylus, Castanea, Ostrya, Castanopsis and Carpinus, but not in Fagus. The concentric undulating lines of vessels characteristic of the elms are also usually visible to the naked eye and can be traced in every

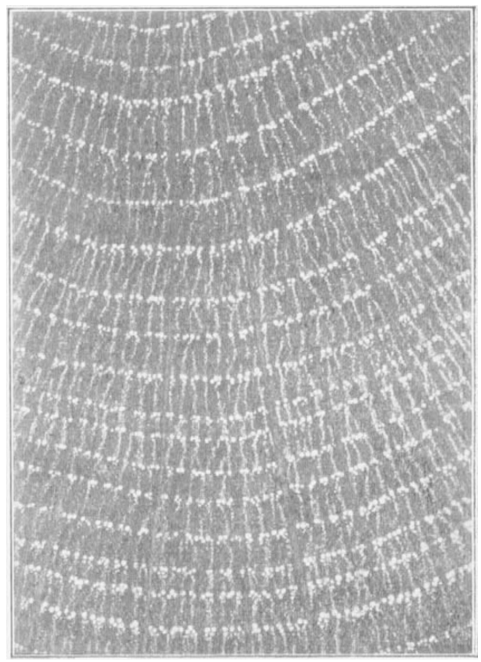

FIG. 1.—Oak. Transverse section × 3½.

species of Ulmus and, in a modified form, in Celtis, also in Ficus, Morus, Artocarpus, Maclura and Urtica.

It may at once be conceded that anything like a natural system of classification of woods by their structure is quite impossible at present. There are too many glaring exceptions and there is too little recorded information. Out of sixteen species of Caprifoliaceæ examined,[1] fifteen have the same type of structure, while the remaining one, *Viburnum Tinus*, L., is quite different; while out of nineteen species of Celastrus there were found no less than seven distinct types of structure.

Nevertheless, amongst such a number of different woods a guide to enable one to trace the name of any wood is a crying need, and several authors have attempted with more or less success to satisfy it. There are several by which the European woods may be identified, notably those by Mathieu, Hartig, Schwartz and Nördlinger, that of the latter embracing exotic woods also, to the number of 1100. Unfortunately, Nördlinger, whose work is otherwise unrivalled, relies upon the definiteness or indefiniteness of the boundary of the year's growth of wood in too great a degree, hence the student is led astray. Alfred Ursprung has recently shown how elusive this

[1] Based upon a paper read before the Society of Arts on December 4, 1901, by Mr. Herbert Stone.

[1] Sambucus, 2 species. Viburnum, 6 species. Lonicera, 8 species.