THURSDAY, FEBRUARY 12, 1891.

ESSEX AND THE TECHNICAL INSTRUCTION ACT.

THE distribution of the funds placed at the disposal of the various County Councils, in accordance with the Technical Instruction Act of 1889 and the Local Taxation Act of 1890, is now engaging attention throughout the country, and it is widely recognized, by those who are responsible for the proper administration of these funds, that the money cannot be better spent than in furthering the cause of technical education with special reference to the needs of their own districts. It is, in fact, an open secret that the Councils are expected to apply their funds in accordance with the Acts of Parliament, and the voices of public opinion and of the framers and supporters of those Acts have frequently been heard to this effect. The facts and figures which we have from time to time published in the columns of NATURE have helped to keep our readers alive as to the present state of affairs in the different counties. The unanimity which prevails, so far as concerns the general principle of applying the funds to the purposes of technical instruction, is certainly a most encouraging indication of the direction in which public opinion is moving. The main difficulties in the way of apportioning the grants are likely to arise, however, when the various claims come to be considered by the Councils to whom they are submitted. This particularly applies to counties like Essex, where no great manufacturing centres exist, and where the occupations of the rural population are agricultural or maritime. It may be difficult at first sight to see clearly how the grants can be applied in such cases, so as to satisfy the wants of a non-urban community, and at the same time to convey assurance to the Council that the money has been well spent in accordance with the spirit of the Acts. It may be pointed out, however, that agriculture clearly comes within the definition, and is, in fact, recognized as a branch of technical science, and no County Councillor who has the maritime interests of his district at heart would grudge the extension of a similar recognition to the claims of applied marine zoology, of navigation, boat-building, or any of the other industries carried on along our coasts.

Essex may be taken as a typical example of a county which is both maritime and agricultural, and the action taken by its Council will no doubt be eagerly watched by the Councils of other counties similarly constituted. As being one of the home counties, moreover, its case presents particular interest. The total amount at the disposal of the county is about £21,000, of which about £4000 goes to West Ham as the county borough, leaving £17,000 for the urban centres and rural districts of the remainder of the county. Numerous claims for grants have been sent in, and will receive attention in due course. Many grants for the carrying on of scientific and technical instruction in institutions already in existence in the larger towns will no doubt be justifiably made. But the means by which the ultra-urban districts can be provided for have yet to be developed, and the scheme put forward by the Essex Field Club, to which we have already briefly alluded in these columns, certainly seems to be sufficiently comprehensive to meet the wants of the case. Of the qualifications of the Club to carry on the work effectively it is not within our province to speak. It will suffice to say that the deputation from the Club, which was received by the County Council on February 2, comprised Sir Henry Roscoe, Profs. W. H. Flower, R. Meldola, and G. F. Boulger, Mr. F. W. Rudler, and others interested in the scheme. Lord Rayleigh, although unable to attend personally, had consented to allow his name to be added to the Committee.

The members of the deputation, whose scientific strength is unquestionable, expressed their approbation of the scheme, and spoke in the highest terms of the work hitherto done by the Club; if their expression of opinion is allowed that weight which it undoubtedly carries, there can be no doubt that Essex possesses in the Field Club an organization which the County Council would do well to avail themselves of. The scheme itself, which was formally submitted by the deputation, will be found in abstract in another portion of our columns. How nearly it falls in with the views of those most competent to speak authoritatively on the question will be gathered from the speech made by Sir W. Hart-Dyke at the recent Conference of the National Association for the Promotion of Technical and Secondary Education, in the course of which he said:—"To my mind the only practical way in which to carry on agricultural teaching is to have a central system. You must, I think, group together different villages and different schools, and have peripatetic teachers. If you do this you will find that the extra cost to school managers on the one hand, and to the ratepayers on the other, will be very small indeed, and yet you will be able to carry out an excellent system of agricultural education." An institution such as the Essex Field Club proposes to establish in connection with their Museum at Chelmsford would meet these views exactly.

MODERN BIOLOGY AND PSYCHOLOGY.

Animal Life and Intelligence. By C. Lloyd Morgan, F.G.S., Author of "Animal Biology," "The Springs of Conduct," &c. (London: Edward Arnold, 1890–91.)

THIS very interesting volume is nearly equally divided between the two subjects indicated by its title. In the earlier chapters we have excellent accounts of the nature of animal life and its relation to the environment; of the processes of life; of reproduction and development; of variation and natural selection; of heredity and the origin of variations; and of organic evolution. The later chapters deal with the senses and sense-organs of animals; the nature of mental processes in man, serving as a basis for our judgment as to the nature and amount of animal intelligence; the mental processes of animals are then very fully and carefully discussed in three long and very suggestive chapters; and this brings us to a final and very metaphysical chapter on mental evolution. It will be impossible here to do more than notice a very few of the interesting subjects which the author discusses with a fullness of knowledge and a judicial impartiality worthy of all praise.

In his chapter on "Variation and Natural Selection," Mr. Morgan deals with the question whether isolation, with no change in the conditions of existence, can lead to divergence of character, and thus to the formation of distinct species. He decides in the affirmative; and as he approaches the question apparently without any bias, it will be useful to see how far his arguments are well founded. He says:—

"Let us suppose that an island is divided into two equal halves by the submersion of a stretch of lowland running across it. Then the only possible cause of divergence would lie in the organisms themselves thus divided into two equal groups."

Before going further, it is well to note the utter impossibility of any such equality of conditions as is here supposed. Probably there is no island in the world but presents considerable climatal differences in its northern and southern, or in its eastern and western halves, even if its contour and geological structure were such as to admit of equal division. Neither is it conceivable that any division could separate every species upon the island into two equal portions, and without this the organic conditions would differ considerably. But, as we are considering a purely hypothetical case, we will allow the possibility of a sufficiently equal division in all respects. Mr. Morgan thus proceeds:—

"We have seen that variations may be advantageous, disadvantageous, or neutral. The neutral form a fluctuating, unfixed, indefinite body. But they afford the material with which Nature may make, through intercrossing, endless experiments in new combinations, some of which may be profitable. Such profitable variations would escape elimination, and, if not bred out by intercrossing, would be preserved. In any case the variety would tend to advance through elimination as previously indicated. But in the two equal groups we are supposing to have become geographically isolated, the chances are many to one against the same successful experiments in combination occurring in each of the two groups. Hence it follows that the progress or advance of the two groups, though analogous, would not be identical, and divergence would thus be possible under practically similar conditions of life."

Now this passage seems to me to be founded on a misconception of the true nature of the process of species production through variation and selection. The evidence we possess as to variation—and Mr. Morgan has given us some additional facts in this volume on the variation of bats—clearly shows that all the organs and parts of animals vary, in the two directions of greater or less development, about a mean value, which mean represents the typical or perfect character of the species for the time being. This typical character has been reached ages ago by all species in countries whose conditions are tolerably stable, and the remarkable fixity of character proved to exist, not only by the mummied animals of Egypt, but by those much older forms which were the contemporaries of Neolithic man, indicates that for such species, so long as conditions remain approximately unchanged, there are no "profitable variations" possible. They have long ago been brought into almost complete harmony with their environment; all possible combinations have long ago been tried; and the fact that though in every successive generation there are variations of every organ and every character in both *plus* and *minus* directions, these are not taken advantage of, but the mean assemblage of characters constituting the species remains unchanged, proves that changes in any direction without change of environment would be disadvantageous. There can therefore for such a species be no "profitable variations" so long as conditions remain, as they do *ex hypothesi*, absolutely unchanged; no "progress or advance" is possible without such change; and to suppose that there can be further divergence and specialization under the conditions assumed is to maintain that, what natural selection, acting on the innumerable variations yearly occurring in the whole species, has been unable to do in the past, will be done in the future by the same causes acting on the two halves of the species separately.

It cannot of course be denied that, however many combinations and variations have occurred in any species during the last ten thousand years, some few superior variations may occur in the next ten thousand, but there will probably be a corresponding number of cases in which the best variations of the year are below the usual standard, and there is no reason to believe that the one will do more than balance the other. The proviso which Mr. Morgan duly makes—"if not bred out by intercrossing"—would certainly require to be taken account of, since the idea that single favourable sports have had any part in the formation of new species is now rarely adopted by evolutionists. We must also always remember Darwin's maxim, generally admitted to be a sound one, that "Nature does not produce absolute perfection but only relative perfection,"—which again implies that when each species has reached an equilibrium with its environment there is for it no further perfection possible under the circumstances, no "profitable" variations tending to modify its mean specific characters of which natural selection can take account. For these various reasons it seems to me that any permanent modification of a species by mere isolation of a portion of it, and without some adequate change in the environment, is almost inconceivable.

Connected with this question is that of the existence of useless specific characters, which are not and never have been correlated with useful characters. Mr. Morgan here very properly suggests that the difficulty is as to what is to give such useless characters any fixity, and without fixity they will not be classed as specific. In a later chapter, on "Heredity and the Origin of Variations," he himself suggests a possible escape from this difficulty in the supposition that the intercrossing of individuals differing somewhat in character does not result in mere "hereditary mixture" but in "organic combination,"—meaning, I presume, that by such intercrossing new characters or the rudiments of new organs may be produced which were not present in any of the ancestral forms. He supposes that such combinations may initiate definite lines of variation, and that we may thus obviate the difficulty as to the origination of organs or structures whose first rudiments cannot be conceived to have been useful to their possessors. It seems to me probable that, however originated, there *are* such "lines of variation," and that some of the unknown laws of variation *do* lead to the initiation of the structures or organs which have been essential to the development of the varied types of the organic world ; but I nevertheless maintain that this does not necessitate the acceptance of the doctrine of useless "specific" characters, or that of the formation of new species by isolation in an unchanged environment. For, by the assumption, these lines of variation and these nascent structures are produced by favourable combinations within the limits of a species. They appear more or less sporadically ; they are at first of no utility ; there is therefore nothing to give them fixity or to lead to their general and uniform development in all the individuals composing the species. Thus they must remain, sometimes dying out, sometimes advancing, till under some changed conditions of the environment they become of use in the struggle for existence. From that moment they become subject to the law of natural selection. All individuals not possessing these characters, or possessing them in too small a degree, are eliminated, leading at once to the steady increase of the character and its constant presence in all individuals of the species. It has now become a "specific" character, but only because it has become useful. The definite "line of variation" is now followed because it is a useful line. But, the moment it reaches a maximum of utility, elimination prevents any further development in that direction although the tendency may still exist, and variations which are now injurious may still continue to appear though they cease to be preserved. This is the view I have already expressed in regard to Prof. Geddes's theory of variation in plants ("Darwinism," p. 428), admitting the tendency of vegetative development in the various ways he suggests to be highly probable, but denying that such causes can produce definite fixed characters without the eliminating agency of natural selection.

It has been objected that this view is inconsistent with the theory that the ornamental appendages and colours of male birds and insects have been produced by exactly such a tendency to development along certain lines of variation ; but it is forgotten that in this case such development is strictly correlated with the superabundant energy and vital activity of male animals, characters which are the subject of both sexual and natural selection. They are therefore increased within the limits of hurtfulness, while their utility as recognition marks, and as indications of sexual maturity, keeps them tolerably constant.

In discussing Weismann's theory of the continuity of the germ-plasm, Mr. Morgan gives reasons for believing that it is in some respects a retrograde step, and that the earlier view, of the continuity of reproductive cells, is the more probable ; and after discussing the various opposing theories—Darwin's pangenesis, Haeckel's perigenesis, Spencer's physiological units, and some others—he arrives at the conclusion that there is a continuity of reproductive cells ; that hereditary similarity is due to the fact that parents and offspring are derived eventually from the same germinal cells ; that there is no convincing evidence that in the Metazoa special modifications of the body so influence the germ as to become hereditary ; but nevertheless, he concludes, there is no reason why such influence should not be assumed as a provisional hypothesis.

The chapter on "Organic Evolution" is a most interesting one, discussing, as it does, in the author's suggestive manner, the various problems arising out of the theory of variation and natural selection. The greater part of this discussion is clear and convincing, but a few cases must be noticed in which the essential point of the argument appears to have been overlooked or insufficiently appreciated.

In considering the agency of natural selection it is urged that it can only act where there is a direct advantage to some individuals or disadvantage to others ; that the advantage must be immediate and present ; that the advantage must be sufficient to decide the question of elimination or non-elimination ; and that we must distinguish between mere indiscriminate destruction and selective elimination. Now, throughout this discussion, and especially in the last portion of it, Mr. Morgan fails to give due weight to the enormous scale on which Nature works, both as regards the number of individuals, the space over which they are distributed, and the time during which the process is going on. If we take all these factors fully into consideration, we shall, I think, see that there is really no importance in what seems to us fortuitous destruction, and that, sooner or later, every beneficial or injurious variation, such as we know are abundantly produced every year, must produce a corresponding effect. He says :—

"A hundred are born, and but two survive. It is a mistake to say that of the hundred born the two survivors are necessarily the very best of the lot. It is quite possible that indiscriminate destruction got rid of ninety of all sorts, and left only ten subject to the action of a true elimination."

Now, this would be quite true, and a valid argument, if a species usually consisted of a few hundred individuals, and the question of modification by natural selection had to be decided within ten or fifty years. But when the hundred individuals are multiplied by perhaps a million spread over a large area, and when the operations of accidental destruction and elimination go on during thousands and even millions of generations, we feel sure that, on the average of the whole, the worst will be strictly eliminated, the best as strictly preserved. A passage is quoted from Prof. Weismann about the destruction of eggs by weasels, cats, crows, &c., of the helpless young birds by the same enemies, of others by cold and hunger in winter, and yet others by the dangers of migration, and it is said: "There is here, first, a certain amount of fortuitous destruction ; secondly, some selection applied to the eggs, &c." But surely, as regards the whole species, and on an average over long periods, "fortuitous destruction"—that is, destruction which overtakes the very best as well as the very worst—must be totally insignificant, as compared with true selective elimination. The capacity of the parents to conceal the nest and eggs, and to protect the young birds, will have been constantly increased by selection, as well as every other faculty and character that is of value to the species, till a condition is reached by which the standard population of the species can be permanently maintained. In different years different qualities ensure survival, and thus some may often survive for a few years which are not so well fitted on the whole as some that have succumbed. But in considerable periods, including years of the severest trial, all these comparatively imperfect individuals will be destroyed, and only the very best be left to continue the race.

In discussing the origin of the beautiful forms and colours of birds, insects, and flowers, it again seems to me that there is some want of perception of the exact points at issue. Mr. Morgan argues, as I think very justly, that even the higher animals have no sense of beauty, and that "the word æsthetic should be resolutely excluded in any discussion on sexual selection." He urges, as I had myself done (NATURE, vol. xlii. p. 291), that a considerable portion of the beauty of flowers, as well as that of birds and insects, is due to symmetry, elegance of outline, surface texture, and other causes. It is, he says, the nectar, not the beauty of the flower, that attracts the bee; and in birds, "the mate selected has been that which has excited the strongest sexual appetence; his beauty has probably not, as such, been distinctly present to consciousness." All this seems to me to be excellent, but in another part of his work Mr. Morgan imputes to me opinions which seem to me erroneous, and which I am not aware of having expressed. Thus, at p. 206, speaking of flowers, he says:—

"And when we ask in this case, as we asked in the case of the beautiful colours and forms of animals, what has guided their evolution along lines which lead to such rare beauty, we are given by Mr. Wallace himself the answer: 'The preferential choice of insects.' If these insects have been able to produce, through preferential selection, all this wealth of floral beauty (not, indeed, for the sake of beauty, but incidentally in the practical business of life) there would seem to be no *a priori* reason why the same class, and birds and mammals, should not have been able to produce, through preferential selection, all the wealth of animal beauty."

I do not remember ever having used the term "preferential choice" as applied to insects and the special colours or markings of flowers. I have always held that these are merely signs of the presence of products which the insect needs and enjoys, and that there is probably no more preference for those particular colours and marks on the part of the insect than there is preference by us for a particular *number* because it indicates our friend's house, or for a particular *colour* because it is that of the seal of a favourite wine. Both number and colour may be in themselves either indifferent or even disagreeable to us, but they none the less serve their purpose of recognition marks. I see no more difficulty in the beauty of flowers and birds and insects being all incidental to the general laws of growth and development, subject always to the law of elimination, than in the beauty of landscape, of foliage, of crystals, of corals diatoms and shelly mollusks, of the exquisite forms and motions of the gazelle, the horse, or the kitten, which have all been produced without any question of preferential selection.

Again, after quoting my statement of certain facts showing that isolation is produced by the likes and dislikes of animals, Mr. Morgan says:—

"Mr. Wallace thus allows, nay, he lays no little stress upon, preferential mating, and his name is associated with the hypothesis of recognition marks. But he denies that preferential mating, acting on recognition marks, has any effect in furthering a differentiation of form or colour."

Now the passage Mr. Morgan quotes referred almost exclusively to preferential association, not to preferential mating, which I consider to be a result of the former. And this preferential association must certainly lead by elimination to a furthering of the differentiation of form or colour exactly so far as that differentiation was useful, and it even might be continued farther, as I believe it sometimes has been, till checked by absolute hurtfulness, if correlated with the extreme vigour and activity of male animals.

Mr. Morgan discusses at considerable length the question of whether the effects of use and disuse are hereditary. He admits the very imperfect character of the evidence in favour of the proposition that they are so, and he adduces, as in his opinion one of the best cases, "the instinctive avoidance" of nauseous and stinging insects by most birds. As neither the nauseous taste nor the stings are usually fatal, the avoidance of them is not of eliminating value, and cannot, therefore, have been produced by natural selection. Hence he thinks the inheritance of individual experience probable. But the "instinctive avoidance" is here assumed, whereas there is now good reason to believe that in the case of nauseous insects, and probably also of stinging insects, the avoidance is the result of individual experience or observation. Some of the most curious phenomena of mimicry can only be explained on this hypothesis.

To many readers, the latter portion of the volume, dealing with the senses and intelligence of animals, will be the most attractive. The chapter on the senses of animals is an admirable summary of the most recent observations and researches on this subject, and the explanation of the probable mode of vision of insects by means of their compound eyes is especially clear and very instructive. Then follow chapters on the mental processes of man and of animals, characterized by clear definitions and acute analysis. It is impossible to summarize these chapters, but some of the author's conclusions may be quoted. On the question of the psychology and intelligence of ants, bees, and other insects, after pointing out their widely different structure and sense-organs, he says:—

"Remember their compound eyes with mosaic vision, coarser by far than our retinal vision, and their ocelli of problematical value, and the complete absence of muscular adjustments in either one or the other. Can we conceive that, with organs so different, anything like a similar perceptual world can be elaborated in the insect mind? I for one cannot. Admitting, therefore, that their perceptions may be fairly surmised to be analogous, that their world is the result of construction, I do not see how we can for one moment suppose that the perceptual world they construct can in any accurate sense be said to resemble ours."

The following passage in like manner gives the author's conclusions as to the difference between the mental nature of man and the higher animals:—

"Furthermore, it seems to me that this capacity of analysis, isolation, and abstraction constitutes in the possessor a new mental departure, which we may describe as constituting, not merely a specific, but a generic difference from lower mental activities. I am not prepared, however, to say that there is a difference in kind between the mind of man and the mind of the dog. This would imply a difference in origin or a difference in the essential nature of its being. There is a great and a marked difference in kind between the material processes which we call physiological, and the mental processes we call psychical. They belong to wholly different orders of being. I see no reason for believing that mental processes in man differ thus in kind from mental processes in animals. But I do think that we have, in the introduction of the analytic faculty, so definite and marked a new departure that we should emphasize it by saying that the faculty of perception, in its various specific grades, differs generically from the faculty of conception. And believing, as I do, that conception is beyond the power of my favourite and clever dog, I am forced to believe that his mind differs generically from my own."

This seems a very fair statement of the case, and one to which, so far as it goes, I have no objection to make. But in the concluding chapter, on mental evolution, we have a serious attempt to overcome the difficulty of the relation between the physiological and psychical processes here stated to belong to "two wholly different orders of being." This is supposed to be done by the adoption of the *monistic hypothesis*, which assumes that these "wholly different orders" of things are really identical—that *neurosis is psychosis*. "The neurosis is the outer or objective aspect, the psychosis is the inner or subjective aspect." Then the subject is attempted to be made clearer by the adoption of other terms—"kinesis" for physical manifestations of energy, "metakinesis" for all manifestations of the mental or conscious order; and we have the following statement:—

"According to the monistic hypothesis, *every mode of kinesis has its concomitant mode of metakinesis, and when the kinetic manifestations assume the form of the molecular processes in the human brain, the metakinetic manifestations assume the form of human consciousness.*"

If this means anything, it means, what has been stated in simpler but equally exact and more intelligible language, that all force is will-power. But it goes further, and implies that there can be no mind like that of man, or superior to it, without a brain formed of similar materials and similarly organized with the brain of man. This necessary connection, and even identity, of the two is, however, what is not proved, and not even, in my opinion, shown to be probable.

The last few pages of the volume are devoted to a discussion of the causes which have led to the development of the higher intellectual faculties in civilized man, and a difficulty is found in explaining this development, except on the ground that the increase of intellectuality acquired by use of the intellectual faculties is inherited. The objection may be made that there is no proof of any increase of average intellect in Europe during the last two or the last twenty centuries; and, on the other hand, it seems probable that, although the unintellectual may generate more rapidly, a smaller proportion of the offspring survive. It is suggested that the development of the social habit, the mutual aid and protection thus afforded, may well have left a balance of the life-energy previously employed for individual self-preservation, available for the increase of pure intellect. The exceedingly sporadic character of exceptional intellectual power favours this view, which is analogous to that which I have suggested as having led to the development of the accessory plumes of male birds. Whether the very existence of such faculties can be adequately explained by increased brain-development alone, is another matter.

The present notice, necessarily confined to a few of the more salient features of the book, gives no fair idea of the great variety of topics treated, nor of the originality and clearness which are its great characteristics. The numerous woodcuts and diagrams are all illustrative of the text, and are fully explained, and the author is particularly happy in his use of diagrams and formulæ to illustrate the more obscure or difficult conceptions. The diagram, at p. 141, to explain Weismann's theory, as illustrated by the question, "Does the egg produce the hen, or does the hen produce the egg?" is one of these, and will render the problem intelligible to many who would otherwise have a difficulty in understanding it. On the whole, the work will prove a boon to all who desire to obtain a general knowledge of the more interesting problems of modern biology and psychology by the perusal of a single compact, luminous, and very readable volume. ALFRED R. WALLACE.

THE LAKE-DWELLINGS OF EUROPE.

The Lake-Dwellings of Europe; being the Rhind Lectures in Archæology for 1888. By Robert Munro, M.A., M.D. (London: Cassell and Co., 1890.)

IN this work on the lake-dwellings of Europe, Dr. Munro has carried out on a wider field the inquiry begun in 1882 in his book on those of Scotland. He has brought to his task qualities of a high order. He had a large share in the exploration of the lake-dwellings in his own country, and has recorded the results in a business-like fashion. He has prepared himself for dealing with the lake-dwellers of the Continent by a painstaking examination of the evidence on the spot, and by visiting all the principal collections. He further has read the voluminous literature bearing on the subject scattered through various journals and periodicals, as well as that which lies ready to hand in separate books. The result of all this labour—and how great it has been only those can know who, like the writer, have gone over the ground—is this work "smelling of the oil" in every page, well illustrated with numerous cuts and with good indexes, and a systematic list of references. It is, indeed, to be looked upon as an encyclopædia of matters relating to lake-dwellings, rather than as an ordinary book. It is little less than a miracle that the vast array of facts brought together should have been compressed into the narrow limits of six lectures.

The lake-dwellings of Switzerland and of the surrounding parts of the Continent were laid before the archæological public in 1866 by Dr. Keller and his English translator and editor, the late Mr. Lee, to whom, some twelve years later, we owe a second and enlarged edition. Since that time discovery has rapidly followed discovery in various parts of Europe. These have been carefully recorded by Dr. Munro in the work before us. The first three hundred pages, comprising the first three lectures and part of the fourth, deal with the lake-dwellings in Switzerland, France, and Italy. The rest of the fourth lecture is devoted to the discoveries in North Germany made principally by Prof. Virchow, and to the curious mounds which stud the marshes of Holland, and which occur also in those at the mouth of the Elbe.

The *terpen*, as they are called, of Holland, deserve more than a passing notice. They rise to a point above high-water mark, and break the monotony of the dead level which, before the construction of the sea-dykes in the twelfth century, must have been covered by every tide. Then they must have been islands, now they are mounds of various sizes, and distributed at sufficiently convenient intervals to afford sites for modern churches and villages, and even towns such as Leeuwarden and Leyden. A few years ago it was discovered that they were largely composed of domestic refuse, so rich in animal matter as to be of great value for manuring the land. The excavations for that purpose have shown that they are artificial islands formed of clay and of timbers, both upright and horizontal, which have been inhabited long enough to allow of great accumulations of refuse. Among the fragments of pottery found in them may be noted the Samian ware; and among the articles made of bone, combs identical with those found in association with Roman remains in lake-dwellings in Scotland. There are also glass beads, bronze dishes and tripods, and Roman statuettes and fibulæ, together with iron bridle-bits, shears, and hammers. The associated coins of the Roman emperors—Byzantine, Anglo-Saxon, and Frankish—fix the date of their occupation. They were inhabited from the time of the Roman attack on the Rhine as late at least as the reign of Lothair (A.D. 840–855), whose empire extended from Italy northwards as far as the Meuse and the Rhine. It is a significant fact that the evidence of the occupation of these mounds ceases with the break-up of the empire of Charles the Great, and the struggles between his degenerate descendants. They probably lay desolate towards the end of the ninth century, and became so completely forgotten that their story has only been made out by the recent diggings.

These mounds, however, apart from the light which they throw on the darkness of the written records of the first eight centuries after Christ, have a further claim to our interest in the fact that they were well known to the students of history in Rome as early as the first century after Christ. They are described by Pliny the Elder, in a passage worthy of being quoted:—

"We saw," he writes, "in the north the Chauci, who are called the Greater and the Less. In their country there is a great tract over which the ocean rolls in great volume twice each day, and raises for us the never-ending discussion as to whether it should be called land or sea. Here a miserable tribe lives on lofty mounds or artificial eminences raised to a height which they know by experience to be above the highest tide. These they occupy with their cabins. When the tide is up, they look like sailors in ships with water on every side; when it is down, like mariners who have been stranded. Their food is fish, caught around their cabins in the ebbing waters. They have no cattle, and do not use milk as their neighbours do, nor does it fall to their lot to contend with wild beasts, since there are no shrubs for cover. They twist cords of sedges and marsh rushes, and make them into fishing-nets. They dig turves, too, and, after drying them more by the wind than the heat of the sun, cook their food with them, and so warm their stomachs, frozen by the northern blasts. Rain-water is their sole drink, stored in ponds at the entrance of their houses. Even these tribes, were they conquered to-day by the Roman people, would say that they were slaves" (Pliny, "Nat. Hist., xvi. 1).

At a later time they possessed oxen and sheep and horses, and from the occurrence of Roman coins and of Samian ware—the Sèvres of the period—it may be concluded that they were in touch with the Roman civilization. They may have served in the Roman army, and they most probably shared in the attacks repeatedly made by the Germanic tribes on the Roman Empire. It would, indeed, be strange if none of the spoils of the legions of Varus, and if no traces of the conquest of their country by Germanicus, followed by his disastrous retreat, were met with in these ancient dwellings in the marshes of Holland.

The fifth chapter is devoted to the lake-dwellings of Britain and Ireland. The Irish crannogs date as far back as the Bronze Age, if not as far back as the Neolithic, and were inhabited as late as the time of Charles II., while those of Scotland begin with the Iron Age, and range as far down as the seventh and eighth centuries after Christ. Those on the south-west—such, for example, as Loch Lee, near Dumbarton—contain the same mixture of fragments of Samian ware and other articles of Roman civilization, with barbaric implements implying a rude manner of life, as we find in the *terpen* of Holland. The region between the Roman Wall and the Highlands was the scene of continual fighting between the Picts and Scots on the one hand and the Roman legions on the other, from the days of Agricola down to the time of the retreat of the Romans from Britain, and afterwards between the Celtic tribes and the English. With the conquest of the kingdom of Strathclyde by the Northumbrian Angles their history ceases.

Dr. Munro ascribes all the lake-dwellings in the British Isles to a Celtic source, and he believes that the Celts of Britain got the idea of protecting themselves by making artificial islands in lakes or morasses from "contact with the inhabitants of the pile villages in Central Europe." These conclusions seem to me not to be proved. It may be allowed that most, if not all, of our British lake-dwellings were built and inhabited by Celtic peoples, but it does not follow that they got the art of building them from Switzerland. Lake-dwellings would be naturally invented by any race living in a state of warfare in a land of lakes and morasses. In other regions the Celts constructed for the same purposes fortified camps where there were no lakes and morasses. The lake-dwellings, therefore, are most probably a mere local development depending on the geographical conditions. Seeing that pile-dwellings are now used by widely different races, ranging over an area from Central Africa to New Guinea, it is improbable that they should have been built by one race only in prehistoric times. In Switzerland itself they were used by the various invaders from the Stone Age down to the time when their Gallic inhabitants were conquered by the Romans.

Dr. Munro sums up with great care in his concluding chapter the principal features of the civilization of the European lake-dwellers. He accepts the Age of Stone, and rejects that of copper, pointing out that copper implements are probably the result of the local scarcity of the tin necessary for the manufacture of bronze. In treating of the Bronze Age, he considers the crescent-shaped earthenware articles rising from a flat base, found in several of the villages, to be "suggestive of religious ideas" instead of being head rests. The latter and more popular view seems to me to be most probable. Again, our author says that "the lake-dwellings of the Bronze Age are built in deeper water, and consequently further from the shore, than those of the Stone Age" (p. 538). Surely they were built further from the shore for purposes of defence against the better weapons, and consequently in deeper water. These, however, are minor points of criticism in a work which will be of great service to archæologists. Dr. Munro is to be congratulated on his success in completing a most difficult task.

W. BOYD DAWKINS.

OUR BOOK SHELF.

Colour in Woven Design. By Roberts Beaumont, Professor and Director of the Textile Department in the Yorkshire College. Pp. xxiv. and 440. (London: Whittaker and Co., 1890.)

AMONGST the merits which this book may possess (and we do not deny that they are considerable), elegance and accuracy of diction cannot be reckoned. This criticism is justified by the occurrence of countless phrases, such as these—"Non-luminous bodies are incompetent of emitting undulations that convey any coloured appearance to the mind"; "linear and curvilinear lines"; and "in the rose there is displayed in perfection all the various modifications in tint and shade to which this important colour (red) is susceptive." And we cannot endorse in all particulars the exposition of the theories of colour given by Prof. Beaumont. For instance, he contrasts what he calls the "light theory of colours" with the "pigment theory," and then, speaking of the latter, says: "Scientifically, it is no more a correct scheme than the light theory is applicable to the industries or to the mixing of paints." But surely the theory of Young, Maxwell, and Helmholtz is as applicable to the results obtained by mixing pigments or coloured fibres, as it is to the results of mingling coloured lights. Yet, while the author writes, on p. 20, "many of the mixtures obtained by this system (that adopted by Chevreul and Brewster) are diametrically opposed to the laws of physics," he proceeds to explain the chromatic phenomena of textiles by its aid. It is needless to urge how deeply Prof. Beaumont's acceptance of the red-yellow-blue triad of primaries vitiates his reasoning as to the effects of contrast, as to the question of the existence of tertiary hues, and as to the true complementaries.

When, however, we turn to the practical or technical sections of this hand-book, we find much information of sterling value. Here Prof. Beaumont is evidently at home. The numerous diagrams and photographs of checks, stripes, weaves, yarns, twists, twills, and diagonals, illustrate the descriptions in the text most satisfactorily. The analysis and synthesis of the various "weaves" are particularly well carried out, and constitute the largest and most important part of the volume before us. A scientific journal is, however, not the place for the discussion of such details of manufacture.

A few of the coloured plates are satisfactory; in others the garish hues and harsh associations may, we hope, be attributed to the failure of the chromolithographs to realize the intentions of the author. But some of the coloured figures are deplorably poor, or even thoroughly debased, in design; note particularly Plate xviii.; Plate xxviii., Fig. 2; and Plate xxxi., Fig. 2. A. H. C.

Constance Naden: a Memoir. By William R. Hughes. (London: Bickers and Son. Birmingham: Cornish Brothers. 1890.)

MISS NADEN was a writer of considerable freshness and ability, and all who knew her agree that she was also a woman of great charm of character. She did not, however, live long enough to produce anything of first-rate importance, and it was hardly advisable to make her the subject of a special memoir. Mr. Hughes appreciates thoroughly all that was most characteristic of his friend's intellectual and moral nature, but he does not possess the secret of presenting brightly and vividly facts in which he himself happens to be interested. Consequently, he does not succeed in conveying any adequate conception even of qualities which he is never tired of praising. The volume contains, besides Mr. Hughes's sketch, an introduction by Prof. Lapworth, and "additions" by Prof. Tilden and Dr. Lewins. The latter gentleman, who delights in the use of an extraordinary philosophical jargon, thinks it would be impossible to be satisfied with any memoir of Miss Naden "which should ignore the scientific hylo-ideal, or automorphic principle, or synthesis underlying and suffusing her whole intellectual and ethical architectonic." He proceeds to supply the necessary exposition, his chief difficulty being "the elementary *naiveté* and simplicity of the concept, or ideal, involved."

Euclid's Elements of Geometry. Arranged by A. G. Layng. (London: Blackie and Son, Ltd., 1890.)

IN this work, Euclid's Books I.-IV., VI., and portions of V. and XI., are dealt with. The enunciations and axioms are the same as those in Simson's edition, but the propositions have received many minor alterations. Only the more common symbols are employed, and some of the propositions have been considerably shortened by the adoption of other proofs based on Euclid's methods. Each proposition is accompanied with examples and in many cases with notes.

An excellent plan adopted throughout is that by which the student can see at a glance the enunciations, propositions, and figures, without the necessity of turning over a page. The appendix contains some simple theorems of modern geometry, a few alternative proofs of the propositions based on other methods than those of Euclid, and a collection of miscellaneous examples and examination papers. Beginners will find the book rather troublesome at first, owing to the use of the symbols; but after these are understood little difficulty ought to be experienced.

LETTERS TO THE EDITOR.

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Hermaphroditism of the Apodidæ.

THE reproduction of *Apus cancriformis* has been a much discussed subject. Although the animal has been well known since the middle of last century, it was not till 1833 that a male was reported to have been found, and not till 1856 that the occasional presence of males in small numbers was certainly established by Kozubowski. On the other hand, the fact that several generations of "females" could be produced without the presence of a male, was established as long ago as 1755 by Schaeffer, who concluded that the animals were hermaphrodite. Since that time authors have been divided in opinion between hermaphroditism and parthenogenesis (not to mention v. Siebold's theory of Thelytoky); the latter view has lately prevailed.[1]

[1] For the history of this subject see Bronn's "Classen und Ordnungen des Thierreichs," vol. v. On p. 810 the following words occur;—"Untersuchungen über die Gattungen Apus und Daphnia welche offenbar in dem bis zu voller Evidenz geführten Nachweis der Parthenogenetischen Fortpflanzung beider gipfeln." See also Lang's "Lehrbuch der Vergleichende Anatomie," p. 393.

The animals, however, prove after all to be hermaphrodites. Since the last careful study of *Apus cancriformis*, as a whole, by Zaddach in 1841 (the works of Ray Lankester and others deal only with special points), new methods of research have been introduced into our laboratories which reveal details not easily discoverable by the older methods. Zaddach's figures of the ovaries and testes of Apus are thus naturally somewhat deficient—as deficient, indeed, as the best work we can do to-day will, we hope, be found to be fifty years hence.

As already announced in a preliminary note,[1] published in the current number of the *Jenaische Zeitschrift für Naturwissenschaft* (Band xxv., N.F. xviii.), a small species of Apus kindly handed me by Prof. Kükenthal, and presumably *Lepidurus glacialis*, Kroyer, proved on examination to be hermaphrodite. The specimens were found in East Spitzbergen during the expedition sent thither by the Bremen Geographical Society in 1889, under the conduct of Prof. Kükenthal. The species seemed to be new, as it did not agree with any of the descriptions of *Lepidurus glacialis*; not only was the whole animal much smaller, but its caudal plate was much smaller and not notched at the tip, and, most important of all, it possessed well-developed second antennæ, which have till now never been found in *Lepidurus glacialis* (Huxley's "Anatomy of the Invertebrata," p. 243). The new species, however, proved to be identical with the *L.* "*glacialis*" brought back from West Spitzbergen by Prof. Nathorst, specimens of which were kindly sent me by Prof. Leché, of Stockholm. It thus at first seemed likely that there were two species of Lepidurus in the Arctic regions—a *Lepidurus glacialis* and a *Lepidurus spitzbergensis*. I am now, however, inclined to look upon *L. spitzbergensis* as a stunted variety of *L. glacialis*, or, rather, as a precociously ripe young stage. My reasons for considering it merely a variety of *L. glacialis* are as follows:—

(1) I have succeeded in finding very distinct second antennæ in a large specimen of *L. glacialis*, from Greenland, kindly sent me by the Rev. Canon Norman, so that this supposed difference does not exist. (2) On examination of the genital tube, the sperm-forming centres are found in identically the same place in the two species, viz. at the posterior end of the genital tube, both, in this respect, differing from the other Apodidæ I have as yet had at my disposal to examine. (3) The other two differences—the small size of *L. spitzbergensis* and the undeveloped state of its caudal plate—are, on this supposition, easily accounted for.

We thus have, instead of two Arctic species of Lepidurus, a fully-developed *L. glacialis*, Kroyer, presumably in the warmer regions, and a small, precociously developed variety from the colder and more northerly regions—*L. glacialis* var. *spitzbergensis*.

Whether the variety is permanent or not, I have no means of deciding. It is interesting to find that Packard's measurements for *L. glacialis* make it smaller than *L. spitzbergensis* ("Monograph of the North American Phyllopoda," 1883), which shows that *L. glacialis* may be dwarfed by the unfavourable environment. As Packard's drawings are (judging from the development of the caudal plate) of the fully-developed animal, this leads one to think that perhaps, in rather longer summers, the Spitzbergen variety may develop into the typical *L. glacialis* without any great increase of size.

In my preliminary notice announcing the hermaphroditism of *L. spitzbergensis*, knowing how much the reproduction of the Apodidæ had been discussed, I ventured to assert that in all probability the other species of the genus would also prove on closer examination to be hermaphrodite. As above stated, I found the sperm-forming centres in *L. glacialis* in identically the same position as in the Spitzbergen variety. By the kindness of Prof. Möbius, the Director of the new Berlin Museum, and of the Rev. Canon Norman, I have also been able to examine *Apus cancriformis* and *Lepidurus productus*. In both these the sperm-forming centres were found scattered here and there among the rich branchings of the segmental diverticula of the genital tube. They occur either at the tips of such branches, where the eggs ordinarily develop, or as slight lateral bulgings of the same. In all cases the spermatogenesis is the same, the epithelium breaking up into sperm cells; these escape into the lumen of the tube, and are found in considerable numbers near the genital aperture, where the epithelial lining of the tube is hardly demonstrable, the walls of the tube consisting of a fibrous membrane, in the folds of which the sperm-cells lurk. The eggs are then fertilized as they stretch this membrane in passing out into the egg pouch. The whole richly-branched reproductive organ, with the eggs developing at the tips of the branches, and with here and there a testis, strongly reminds one of a monœcious plant, self-contained, and able to dispense with pollen from without.

[1] In this note, by an oversight, I stated that Schaeffer concluded the animals to be parthenogenetic.

I reserve the drawings and the more detailed description of the reproductive organs of the different species for a short comparative study of the Apodidæ which I hope soon to have ready for the press. By way of caution, however, I should here add that small yellowish sacs filled with minute cells occur here and there among the developing eggs. These must not be mistaken for the testes. They are the loci of discharged eggs, and the minute cells are the epithelium cells dislodged by the shrinking of the membrane of the genital tube, which is stretched some 100-fold by the ripening eggs.

The origin of this secondary hermaphroditism is not far to seek; it is clearly a protection against isolation, as in the case of the Cirripedia and certain parasitic Isopoda. The manner of life of all these animals is such that they are always in danger of being cut off from their kind; they would thus die out unless able to reproduce either parthenogenetically or by means of self-fertilization.

Some species of Cirripedia, as is well known, have dwarf males, the last remains of the original separation of the sexes. As already mentioned, small males of *Apus cancriformis* are sometimes found. Twelve finds of *A. cancriformis* and *L. productus*, recorded by Gerstaecker, give 4458 "females" (*i.e.* hermaphrodites) to 378 males; while sixteen finds, numbering 10,000 individuals, did not contain a single male. I have found no record of a male *L. glacialis*, and none of the twenty odd specimens of the Spitzbergen variety I have as yet examined have been males. It is probable that throughout the whole genus self-fertilization is taking the place of cross-fertilization, but that some species have gone further than others in dispensing with males. Two species, for instance, *L. couesii*, Packard, and *L. macrurus*, Lilljeborg, are reported to have more males than "females (?)," but the finds in these cases seem hardly large enough to allow us to judge; it may have been purely accidental that more males than "females" were caught.

The males of the Apodidæ, with the doubtful exception of *L. productus*, seem to be smaller than the hermaphrodites, otherwise there is no very pronounced sexual dimorphism as there is among the Cirripedia. We are perhaps justified in concluding from this that the hermaphroditism of the Cirripedia is of much older date than that of the Apodidæ. No comparison is here, however, possible, since the two have nothing further in common beyond the fact that they are both hermaphrodite, and that this hermaphroditism is in both cases an adaptation against extermination through too wide dispersion of the individuals.

Jena, January 30. H. BERNARD.

Stereoscopic Astronomy.

THE note on this subject in NATURE of January 22 (p. 269), regarding the perception of stereoscopic effect on examining properly-arranged photographs of Jupiter, recalls an observation which I published in one of a series of articles on physiological optics that appeared in the *American Journal of Science* in 1881 and 1882.

By taking advantage of the moon's librations, Mr. Lewis M. Rutherfurd, of New York, produced more than twenty years ago an excellent stereograph of this heavenly body. In examining this I found it possible to observe not merely the general convexity or concavity, according to the mode of stereoscopic combination, but also the inequalities upon the lunar surface. In an American text-book I have found the statement that Mr. Warren De la Rue had succeeded in obtaining a stereograph of the sun, from which, by stereoscopic vision, the ridges of the faculæ could be perceived in sharp relief. On application to Mr. De la Rue for a copy of this stereograph, I was disappointed to learn that the negative had, unfortunately, been destroyed, and hence no copies were attainable.

My own observations may be given by quoting from the article published in the *American Journal of Science* for May 1882. "On the stereograph of the moon, to which reference has been made, the elevation of mountain ranges and solitary peaks, and even the inequalities of the supposed dead sea bottoms can be clearly seen. The crater Copernicus, and the lunar Apennines, stand forth particularly boldly, and the ridge that divides the bed of the heart-shaped Sea of Serenity can be easily traced. Anyone who has undertaken the preparation of a stereograph with the pencil or pen, knows how very difficult it is to avoid the production of roughness in the combined image at places where smoothness is desired. No two impressions from the same type can be taken that will not present some inequalities when stereoscopically examined, and no two groups of type representing the same sentence can be so accurately adjusted as not to betray imperfection when subjected to this searching test."

For this statement regarding the moon, I was subsequently criticized by an English writer, well known in astronomical circles, who considered it to be extravagant. The test furnished by the photographs of Jupiter is probably even more delicate than that afforded by photographs of the moon's minor inequalities of surface. The observation of "W. J. H." is certainly very interesting. By experiments made in 1882 I found that a plane binocular image became noticeably convex or concave when the pair of diagrams under examination were so disposed as to produce an angular retinal displacement of only 47″ (*Philosophical Magazine*, October 1882). By comparing the photographs of celestial objects whose distance is known, it may be possible yet to show that the minimum displacement measured in 1882 is really not quite a minimum. W. LE CONTE STEVENS.

22 Universitätsstrasse, Strassburg, Germany, February 4.

Notable Palæolithic Implement.

DURING the last five or six years I have lived at Dunstable, and many persons in the neighbourhood now know that I notice old things a little. The consequence is that various objects are now and again presented to me for purchase. These things are mostly no good—common fossils, pieces of "petrified water," shells, coins of the Georges, &c., but at times something worth notice comes to hand.

Late last autumn a number of stones of no value were brought to me; amongst them was a good, flattish, sub-triangular, Palæolithic flint implement which had been picked up in 1830 by a farmer named William Gutteridge on Dallow Farm, near Luton—the late Mr. Gutteridge's own land. The implement had been preserved by the farmer as a curious natural stone, and he had affixed a label to it with locality and date. The person of whom I secured the stone knew nothing of stone implements. I soon ascertained the name and date to be correct from a relative of the late William Gutteridge. In 1830 the Gutteridges had held Dallow Farm for over 150 years.

Dallow Farm is in the valley of the Lea, and three-quarters of a mile west of the river at Luton. The ground is, I think, about 50 feet above the Lea, and from 400 to 450 feet above the Ordnance datum, but the heights on the large-scale Ordnance map are here insufficient. I have never found a Palæolithic implement at Luton, but I have picked up a few drift flakes there, and found a good number of Palæolithic implements a few miles off.

The Dallow Farm Palæolithic tool was found by Mr. Gutteridge seventeen years before M. Boucher de Perthes published his discoveries in France (1847), and eleven or twelve years before he began to notice such objects.

The famous Gray's Inn implement was found in 1690; Mr. Frere's discoveries were made at Hoxne in 1800; the Dallow Farm implement comes next in 1830; and the Godalming implement (Evans, "Stone Implements," p. 529) about 1842.

Dunstable. WORTHINGTON G. SMITH.

Stereom.

AMONG wants long felt, at least by animal morphologists, is some word that shall express for Invertebrata the idea that the word *bone* expresses for Vertebrata. Words such as *skeleton*, *shell*, *test*, and *carapace* express the whole structure, not the substance of which it is made. Words such as *nacre* and *stereoplasm* express some particular form of hard substance strictly defined from a physical or morphological stand-point. *Sclerenchyma* is the only word that has yet been used in anything like the required sense; but that is confined to corals, and, from its affinity with *cœnenchyma* and the like, it is well that it should be so. Driven back on cumbrous periphrases, I therefore venture to suggest the adoption of the word *Stereom* (στερέωμα, that which has been made solid). This word was used by Aristotle ("De Anim. Part.," ii. 9) for the hard as opposed to the soft tissues of the body, and may, for the purposes of modern science, be thus defined: any hard calcareous tissue forming skeletal structures in Metazoa Invertebrata, and in Protozoa. F. A. BATHER.

February 9.

Destruction of Fish by Frost.

IN regard to Prof. Bonney's letter of January 26 (p. 295), I would ask whether the fish were not killed by want of air due to the stagnancy of the water in the canal?

The moat here abounds in fish, and several holes were kept open for their sakes during the frost. The first partial thaw set our land-drains running. Where one of these began to pour a little water into the moat, though no fish had been visible since summer, now the largest pike and carp were seen crowding to the aperture, seeming to be gasping for air, and seeking the fresh flow. When the frost departed, scarce half-a-dozen fish—all small—were found dead. It would seem, therefore, that a very slight flow of fresh water would suffice to save fish from death. But this can seldom be wanting in any natural body of water, for few are even the tarns into which no brook runs. So such a cause of destruction can seldom have acted on a scale visible to a geological eye. E. HILL.

The Rectory, Cockfield, Suffolk.

A DEDUCTION FROM THE GASEOUS THEORY OF SOLUTION.[1]

BEFORE passing on, let me briefly recapitulate the chief points in Van't Hoff's gaseous theory of solution and the experimental laws on which it it is based.

(1) In every simple solution the dissolved substance may be regarded as distributed throughout the whole bulk of the solution. Its total volume is therefore that of the solution, the solvent playing the part of so much space; and its specific volume is the volume of that quantity of the solution which contains 1 gramme of the substance. To avoid confusion, it is best to speak of this as the *specific solution volume* (v) of the substance. It is obviously in inverse ratio to the concentration.

(2) In every simple solution the dissolved substance exerts a definite *osmotic pressure* (p). This is normally independent of the nature of the solvent. It varies inversely as the specific solution volume (or directly as the concentration), and directly as the absolute temperature (T). We may then write for solutions, as we do for gases, the equation $p \, . \, v = r \, . \, T$, where p and v have their specialized meanings, and r is a constant for each soluble substance.

(3) The *molecular solution volume* of all dissolved substances is the same if they are compared at the same temperature and osmotic pressure. If m be the molecular weight, $m \, . \, v = V$ is the molecular solution volume; and we can now write, as we do for gases, $p \, . \, V = R \, . \, T$, where R is the same constant for all substances.

(4) This constant R has the same value when the formula is applied to the dissolved state as when it is applied to the gaseous state itself.

(5) The gaseous laws, as I have stated them, are not absolutely true for dissolved matter in all circumstances. Dissociation often occurs, as it may occur in the process of vaporization, thus causing apparent exceptions. But apart from this there are and must be variations from the laws in the case of solutions of great concentration, just as there are in the case of gases and vapours of great concentration—for instance, in the neighbourhood of the critical point.

I wish now to ask your attention more particularly to

[1] Part of an address delivered by Prof. Orme Masson as President of Section B of the Australasian Association for the Advancement of Science, January 1891.

the actual process of dissolving, and then to lay before you a hypothesis which, as it seems to me, is a logical consequence of the general theory.

Imagine, then, a soluble solid in contact with water at a fixed temperature. The substance exercises a certain pressure, in right of which it proceeds to dissolve. This pressure is analogous to the vapour pressure of a volatile body in space, the space being represented by the solvent; and the process of solution is analogous to that of vaporization. As the concentration increases, the osmotic pressure of the dissolved portion increases, and tends to become equal to that of the undissolved portion; just as, during vaporization in a closed space, the pressure of the accumulating vapour tends to become equal to the vapour pressure of the liquid. But if there be enough water present, the whole of the solid will go into solution, just as the whole of a volatile body will volatilize if the available space be sufficient. Such a solution may be exactly saturated or unsaturated. With excess of the solvent it will be unsaturated, and the dissolved matter will then be in a state comparable to that of an unsaturated vapour, for its osmotic pressure will be less than the possible maximum corresponding to the temperature. On the other hand, if there be not excess of water present during the process of solution, a condition of equilibrium will be arrived at when the osmotic pressure of the dissolved portion becomes equal to the pressure of the undissolved portion, just as equilibrium will be established between the volatile substance and its vapour if the space be insufficient for complete volatilization. In such a case we get a saturated solution in presence of undissolved solid, just as we may have a saturated vapour in presence of its own liquid or solid.

So far we have supposed the temperature to be stationary, but it may be raised. Now a rise of temperature will disturb equilibrium in either case alike, for osmotic pressure and vapour pressure are both increased by this means, and a re-establishment of equilibrium necessitates increased solution or vaporization as the case may be.

Now what will this constantly increasing solubility with rise of temperature eventually lead to? Will it lead to a maximum of solubility at some definite temperature beyond which increase becomes impossible? Or will it go on in the way it has begun, so that there will always be a definite, though it may be a very great, solubility for every definite temperature? Or will it lead to infinite solubility before infinite temperature is obtained? One or other of these things must happen, provided, of course, that chemical change does not intervene.

Well, let us be guided by the analogy that has hitherto held good. Let us see what this leads us to, and afterwards examine the available experimental evidence. We know that a volatile liquid will at last reach a temperature at which it becomes infinitely volatile—a temperature above which the liquid cannot possibly exist in the presence of its own vapour, no matter how great the pressure may be. At this temperature, equilibrium of pressure between the liquid and its vapour becomes impossible, and above this point the substance can exist only as a gas. This is the critical temperature. And so it seems to me that, if we carry our analogy to its logical conclusion, we may expect for every substance and its solvent a definite temperature above which equilibrium of osmotic pressure between undissolved and dissolved substance is impossible—a temperature above which the substance cannot exist in presence of its own solution, or, in other words, a temperature of infinite solubility. This may be spoken of as the *critical solution temperature.*

But a little consideration shows that in one particular we have been somewhat inexact in the pursuance of our analogy. For we have compared the solution of a *solid* body to the vaporization of a volatile *liquid.* We can, however, do better than this, for volatile solid bodies are not wanting. It is to these, then, that we must look in the first instance. Now a volatile solid (such as camphor or iodine) will not reach its critical point without having first melted at some lower temperature; and a similar change should be exhibited in the solution process. At some definite temperature, below that of infinite solubility, we may expect the solid to melt. This *solution melting-point* will not be identical with, but lower than, the true melting-point of the solid; and for the following reason. No case is known, and probably no case exists, of two liquids one of which dissolves in the other and yet cannot dissolve any of it in return. Therefore there will be formed by melting, not the pure liquid substance, but a solution of the solvent in the liquid substance. Hence the actual melting or freezing point must be lower than the true one, in right of the laws of which I have spoken when discussing Raoult's methods in the earlier part of this address.

From this solution melting-point upwards we shall then have to deal with two liquid layers, each containing both substance A and solvent B, but the one being mostly substance A and the other mostly solvent B. These may be spoken of as the *A layer* and the *B layer.* As temperature rises, the proportion of A will decrease in the A layer and increase in the B layer; and every gramme of A will occupy an increasing solution volume in the A layer (B being absorbed there) and a decreasing solution volume in the B layer. At each temperature the osmotic pressures of A in the two layers must be equal. The whole course of affairs, as thus conceived, now admits of the closest comparison with the changes which accompany gradual rise of temperature in the case of a volatile liquid and its saturated vapour. The liquid is like the substance A in the A layer; the vapour (which is the same matter in another state) is like the same substance A in the B layer. As temperature rises, the liquid diminishes in total qnantity, the vapour increasing; but the specific volume of the liquid increases, while that of the vapour decreases. The residual liquid is, in fact, constantly encroaching on the space of its vapour, just as the residual substance A in the A layer is constantly absorbing the solvent B from the B layer. Finally, in either case, the specific volume of the substance will become identical in both layers, which means that the layers themselves will become homogeneous and indistinguishable. Our system will then have reached its critical temperature—the temperature of infinite volatility in the one case and of infinite solubility in the other.

So much for hypothesis. Are there any facts in support of it? Well, in the first place the hypothesis demands that (in the absence of chemical change) increase of solubility with rise of temperature shall be as general a law as increase of vapour pressure; and we find that this agrees with the known facts, more especially since Tilden and Shenstone (Phil. Trans., 1884) cleared up certain doubtful cases. Secondly, the hypothesis seems to demand some connection between the true melting-points of salts and the rates of their increase of solubility; and such a relation has in a general way been established by the same observers. Thirdly, we have the fact, in complete accordance with the hypothesis, that while no case is known of a solid body having, as such, infinite solubility in any simple solvent, several cases are known of liquids of infinite solubility, and also of *solids which, after they have melted in presence of their own solution, become at some higher temperature infinitely soluble.* This last statement refers to the cases described by Alexéeff (*Wiedemann's Annalen*, 1886), of which I must say a good deal more directly. It would seem to apply also to the case of silver nitrate, which Tilden and Shenstone described as dissolving in water to the extent of 18·25 parts to one at so low a temperature as 130° C. The true melting-point of the salt is 217°; and I have seen it stated (but have been unable to find the published account) that Shenstone has himself shown it to be fusible in water, and of infinite solubility at quite reachable temperatures.

With regard to substances that are liquid under ordinary conditions, we have the well-known fact that some pairs are infinitely soluble in one another, while others exhibit the phenomenon of only partial solubility. The hypothesis would draw no hard and fast distinction between these cases, except the practically important one that such a mixture as that of ether and alcohol, which belongs to the first class, is usually above its critical solution point, while such a one as ether and water, which belongs to the second class, is usually below it. It should be possible, according to the hypothesis, to cool mixtures of ether and alcohol sufficiently to cause separation into two layers, similar to those observed at the ordinary temperature in the case of ether and water; but I do not know that this has yet been put to the test of experiment.

Alexéeff's experiments appear to me to be of the very highest importance, and to merit the closest attention in any inquiry into the nature of solution. As already stated, they afford the strongest support to the hypothesis which I have been discussing; indeed, had it not been for this support I should hardly have ventured to discuss it at all. They refer to solutions in water, below and above 100°, of phenol, salicylic acid, benzoic acid, aniline phenylate, and aniline, and to solutions in molten sulphur of chlorobenzene, benzene, toluene, aniline, and mustard oil. All these afford instances of reciprocal partial solution throughout a considerable range of temperature, leading eventually at a definite temperature to infinite solubility. Several of them afford instances also of solid substances with solution melting-points below their true melting-points.

Alexéeff experimentally determined the temperatures at which different mixtures of the same two liquids are just converted into clear solutions; or, in other words, he ascertained the strengths of the saturated solutions corresponding to different temperatures. For each pair of liquids he found that when a particular strength of mixture is reached, the temperature of saturation is lowered by further addition of either liquid. Thus a mixture of about 37 parts aniline to 63 parts water requires a temperature of 164°·5 to convert it into a homogeneous solution; but one of 21 of aniline to 79 of water assumes this condition at 156°, and one of 74 of aniline to 56 of water does so at 157°·5. He plotted his results in the form of curves, with temperature and percentage strength as the two co-ordinates. The curve for aniline and water is shown in Fig. 1; and this may be taken as a fair representative, the general form of all being similar. It is at once apparent that for every temperature up to a certain limit there are two possible saturated solutions, one of water in aniline and one of aniline in water. The limiting temperature at which there is but one possible saturated solution, and above which saturation becomes impossible, is called by Alexéeff the *Mischungs Temperatur*. It is what I have called the critical solution temperature. It is in the case of aniline and water about 167°, as nearly as one can judge from the curve without a greater number of experimental points than we have in this part; and the corresponding saturation strength is about 50 per cent. It is hardly necessary to say that this equality of the two ingredients is an accident which does not characterize all cases.

Now imagine a 50 per cent. mixture of aniline and water sealed up in a tube, shaken, and gradually heated. Let us assume that the tube is only large enough to contain the mixture and allow of expansion by heat, so that evaporation may be neglected as too small to materially complicate the result. The course of events will be exactly what I have already described with reference to the hypothetical A layer and B layer. There will be formed a saturated solution of water in aniline, which we may call the *aniline layer*, and a saturated solution of aniline in water—the *water layer*. Given the temperature, the percentage strength of each layer may be read off from the curve. As the temperature rises, the two layers will effect exchanges in such a way that the aniline layer will become poorer, and the water layer richer in aniline; and at about 167° the two layers will have attained equal strength and become merged into one. Were we to start with the aniline and water in any other proportions by weight, there would still be formed the two saturated solutions, but their relative amounts would be different, and one or other would be used up and disappear at a lower temperature than 167°. To attain the maximum temperature of complete solution you must start with the exact proportions which correspond to that temperature.

But it is possible to learn even more from Alexéeff's work than he himself has made evident. Let me call your attention to the curve shown in Fig. 2,[1] the data for which I have calculated in the following manner.

From Alexéeff's percentage figures was deduced the weight of water capable of dissolving, or being dissolved by, 1 gramme of aniline at each of his experimental temperatures, so as to form a saturated solution. Then from curves showing the expansion of pure water and pure aniline (the latter drawn from Thorpe's data, Trans. Chem. Soc., 1880) there were read the specific volumes of these substances at each of Alexéeff's temperatures; and from the combined information thus obtained, there was calculated the total volume of that quantity of the saturated solution at each temperature which contains 1 gram of aniline. This is what I have already called the specific solution volume. A slight error is involved by the fact that the volume of a solution is not exactly the sum of the volumes of its ingredients; but this error is necessarily small—too small to affect the general character of the curve or the nature of the lesson to be learned from it.

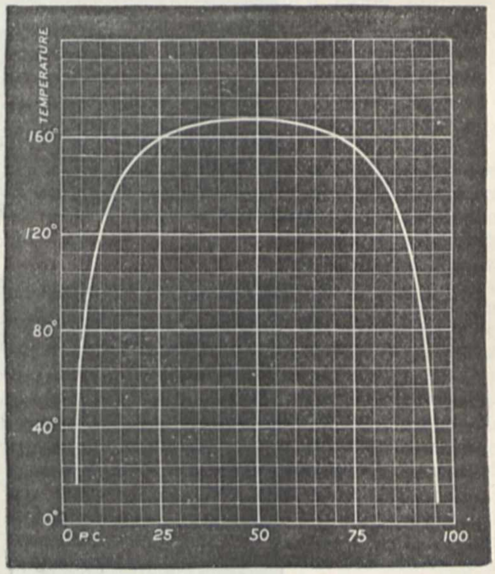

FIG. 1.—Percentage of aniline in its saturated aqueous solution (Alexéeff).

[1] In order to save space, only the upper portion of the curve is here represented, as it shows all that is essential to the argument. Of the twelve experimental points, one appears to be somewhat misplaced; but this does not affect that part of the curve shown in the figure.

The specific solution volumes of the aniline, calculated in this manner, were found to be as follows:—

Temperature.	Specific solution volumes of aniline	
	In aniline layer.	In water layer.
8°	1·015	—
16	—	32·16
25	1·036	—
39	1·053	—
55	—	28·27
68	1·087	—
77	—	19·55
137	1·297	—
142	—	7·696
156	—	5·248
157·5	1·498	—
164·5	—	3·412

These specific solution volumes are represented as abscissæ in Fig. 2, with the temperatures as ordinates. For the sake of comparison, I have placed side by side with it a specific volume and temperature curve (Fig. 3) for pure alcohol and its saturated vapour, plotted from the experimental data of Ramsay and Young (Phil. Trans., 1886). The reason that alcohol was chosen is simply that the data were convenient to my hand.

The two curves are strikingly similar in form and significance. In Fig. 3 we see the specific volume of liquid alcohol increasing slowly with rise of temperature, while that of the saturated vapour rather rapidly decreases. In Fig. 2 we see the specific solution volume of the aniline in the aniline layer slowly increasing, while that of the aniline in the water layer decreases more rapidly, with rise of temperature. In Fig. 3 we see that above the critical point the existence of liquid alcohol in presence of its vapour is impossible. In Fig. 2 we see that above the critical solution point the existence of an aniline layer in presence of a water layer is impossible. In Fig. 3 we see an inclosed area which represents

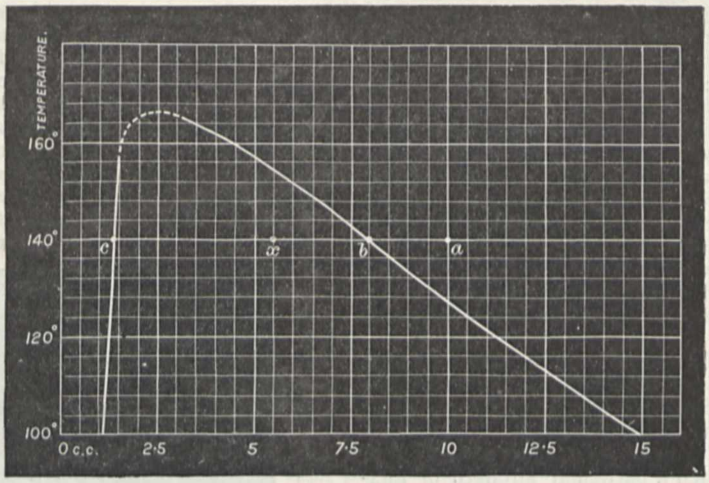

FIG. 2.—Volume of saturated aqueous solution containing one gram of aniline

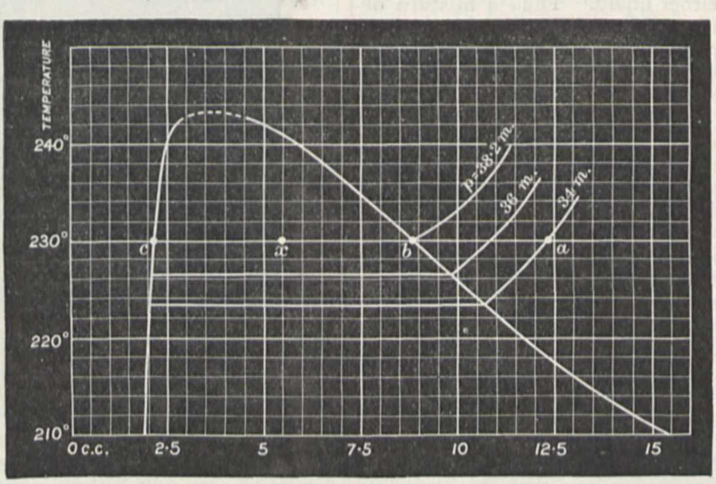

FIG. 3.—Volume of alcohol (liquid and saturated vapour) weighing one gram.

those temperatures and specific volumes which are mutually incompatible. In Fig. 2 we see an inclosed area which represents those temperatures and specific solution volumes which are mutually incompatible. In Fig. 3 we see that any two points on the curve which correspond to equal temperature must also, from the nature of the case, correspond to equal osmotic pressure. In Fig. 3 some of the pressures are indicated, as this can be done from Ramsay and Young's data. In Fig. 2 the value of the osmotic pressures cannot be given, as they have not been experimentally determined. In Fig. 3 any point outside of the curve and to the right, as at *a*, corresponds to the state of unsaturated alcohol vapour, whose temperature, specific volume, and pressure are indicated—the last by the isobaric line which passes through the point. In Fig. 2 any point outside the curve and to the right, as at a, must correspond to the state of an unsaturated aqueous solution of aniline, whose temperature and specific solution volume can be read, and whose osmotic pressure could be indicated by an isobaric line, had we the data for plotting it. A little thought makes it evident, too, that such isobaric lines would follow the same general course as those shown in the alcohol diagram.

Now consider what must be the effect of gradually decreasing the volume of the unsaturated vapour in the one case and the solution volume of the aniline in the unsaturated solution in the other, while temperature is kept constant. In the case of the vapour (Fig. 3), the point a will pass to the left across lines of increasing pressure, until the vapour becomes saturated at b. Then, if the diminution of volume continue, a portion of the vapour will condense to the liquid state, or be transferred to c, while the rest remains saturated vapour at b. With continued decrease of volume, the proportion condensed will constantly increase, but there can be no alteration of pressure till all is condensed; and after that nothing but a very slight diminution of volume is possible without a lowering of temperature. Well, how are we to diminish the solution volume of the aniline in the unsaturated aqueous solution? Clearly by depriving the solution of some of its water, so as to leave the same quantity of aniline distributed throughout a smaller space. And what will be the result of doing this while temperature is kept constant? Evidently, as in the other case, the point a (Fig. 2) will travel to the left, across lines of increasing osmotic pressure, until it reaches b—that is, until the solution is a saturated one; and after that, if more water be abstracted, some of the aniline will be thrown out or condensed, not as pure aniline but as a saturated solution of water in aniline, so that two layers will now co-exist—the aniline in one having the specific solution volume represented at b, and the aniline in the other having that represented at c. This tranference from b to c will continue, as water is abstracted, until the ratio of residual water to aniline is just enough to give the whole of the latter the specific solution volume shown at c. At this stage the water layer will disappear, and only a saturated solution of water in aniline will be left; and after that only a very small volume change can possibly result from further abstraction of water, as the specific solution volume is already not far from the specific volume of pure aniline itself at the same temperature.

To complete the comparison of the two curves, let me point out that, just as we can from Fig. 3 calculate the distribution of alcohol between its liquid and its vapour layers under given conditions, so can we calculate from Fig. 2 the distribution of the aniline between the aniline layer and the water layer under given conditions. In the former case, if the total volume of a tube containing n grammes of alcohol, at, say, 230°, be $n \times x$, and if x be marked off (Fig. 3) between b and c on the line of that temperature, then (x, b, and c standing for the volumes which can be read off on the horizontal base line) $n \cdot \frac{x-c}{b-c}$ is the weight of the alcohol in the vapour layer, and $n \cdot \frac{b-x}{b-c}$ is its weight in the liquid layer, and the volumes of the two layers in cubic centimetres are $n \cdot b \cdot \frac{x-c}{b-c}$ and $n \cdot c \cdot \frac{b-x}{b-c}$ respectively, which are together equal to $n \cdot x$. Just so also with the aniline and water mixture (Fig. 2). If $n \times x$ be the total volume of the mixture (both layers together) containing n grammes of aniline, at, say, 140°, and if x be marked off as it was in the other case, then $n \cdot \frac{x-c}{b-c}$ is the weight of aniline in the water layer, and $n \cdot \frac{b-x}{b-c}$ is its weight in the aniline layer, and the total volumes of the two layers are $n \cdot b \cdot \frac{x-c}{b-c}$ and $n \cdot c \cdot \frac{b-x}{b-c}$ respectively, together equal to $n \cdot x$.

If the actual weights of aniline and water in the mixture be given, the value of x can be calculated with a very fair approach to accuracy by the method adopted in plotting the curve; and thus all the facts with regard to the distribution at any temperature can be obtained.

Now if it be remembered that this case of aniline and water is not an isolated one, but typical of many cases experimented on by Alexéeff, and if it be remembered also that there exists no direct experimental evidence to show that the law which governs these cases is not the general law regulating all simple solutions, it must I think be granted that the facts do somewhat strongly support the hypothesis of a critical solution point which I deduced in the first instance from the general theory of solution. It may be summed up as follows:—

(1) In every system of solution which starts with a solid and its simple solvent, the solid has a solution melting-point which is lower than its true melting-point. Above this temperature the system consists of two separate liquids, each of which is a saturated solution.

(2) These two liquids become one homogeneous solution at a temperature which depends on the ratio of the original ingredients. There is one ratio which demands a higher temperature than any other. This is the critical solution temperature, above which either ingredient is infinitely soluble in the other.

THEORY OF FUNCTIONS.[1]

II.

WE now come to Dr. Schwarz's contributions to the theory of functions, which relate to the two theorems stated, and we begin with those which relate to the conform representation of various surfaces on a circle.

In the paper "Ueber einige Abbildungsaufgaben," we learn that these investigations date back to the time when the author was a student at Berlin. A fellow-student, Herr Mertens, observed to him how curious it was that Riemann had proved the existence of a function which would give a conform representation of the surface of a figure like a triangle on that of a circle, whilst the actual determination of the function in form of an expression seemed, on account of the discontinuities due to the corners, to be beyond the present powers of analysis. Dr. Schwarz thereupon tried to work out a special case, and selected the representation of a square on a circle, or, rather, first on the half of the plane which is bounded by a straight line; for the circle can be conformly represented on this half-plane by aid of reciprocal radii.

He next looks for a function which has such discontinuities as are introduced by the corners of the square. The right angle has to be represented by an angle of twice the magnitude, and therefore here the representation cannot be similar in the smallest parts. The known properties of the exponential function help at once to find it. This function is thus obtained by a happy guess, not by prescribed rules. It still contains an infinite number of constants. To determine these it is observed that if $u = f(t)$ gives a conform representation of a surface T in the plane t on a surface U in the plane u, then also will $u' = C_1 u + C_2$ gives such representation, only the corresponding figure U′ is drawn to a different scale, and placed in a different position in the u plane. On differentiating this equation with regard to

[1] "Gesammelte Abhandlungen." Von H. A. Schwarz. Two Volumes. (Berlin: Julius Springer, 1890.) Continued from p. 323.

t, and eliminating the constants, it is found that the expression

$$\frac{d}{dt} \log \frac{du}{dt}$$

remains unaltered if we substitute u' for u, and is therefore independent of the absolute magnitude and position of the figure U in the plane. The value of this expression in the case under consideration is then shown to be a rational function of t, and on integration the value of u is got as an elliptic integral. On returning from the half-plane to the circle by the substitution $s = t - i/t + i$, we get ultimately

$$u = \int_0^s \frac{ds}{\sqrt{1-s^4}}, \quad s = \sin am\, u, \quad (k = i).$$

This lemniscatic function gives the required transformation, as is ultimately easily verified.

He also gives in form of a definite integral the function for the conform representation of a triangle on a circle, and the expression found at once suggests the function for the representation of any polygon.

These results he laid, in 1864, before the Mathematical Seminary of the Berlin University. They were, however, not published till 1869, and then with considerable additions. Meanwhile the objections raised against Riemann's theory, already mentioned, had been raised. These invalidated the results about the polygon; for the formula given contains a number of constants. The complete proof for the validity of the formula requires Riemann's theorem about the existence of a solution, or else a proof that the constants can in each case be determined. As the first had become doubtful, the author now states that he had been able to give the last proof in case of any quadrilateral, and adds that he has received a general proof from Weierstrass. But this proof is not given.

A few other examples are added, viz. the representation, always on a circle, of the part of a plane outside a square, of the space bounded by a parabola and by an ellipse. This last problem is more fully considered in a separate paper.

He next considers a polygon bounded by circular arcs. Such a polygon is transformed into another also bounded by circular arcs by aid of the substitution

$$u' = (c_1 u + c_2)/(c_3 u + c_4).$$

The differential equation obtained on eliminating the four constants, is of the remarkable form $\psi(u', t) = \psi(u, t)$, where $\psi(u, t)$ is the expression which Cayley has more suitably denoted by (u, t), and called the "Schwarzian derivative," and which, through one of Cayley's formulæ, has led to Sylvester's theory of reciprocants.

Some of the results obtained are extended to representation on a sphere by aid of stereometric projection, and finally the function is given which performs the conform representation of the surface of a polyhedron on that of a sphere.

The function obtained contains, again, a number of constants. The author states that these can at once be determined in case of a regular polyhedron, but that he has not been able to prove that this can always be done.

The case of a tetrahedron is considered and fully worked out in a special paper. The result is that the surface of a tetrahedron can always be conformly represented on the surface of a sphere, so that there is a one-one correspondence between the points, and this can be done in one way only if for any three points on the one surface the corresponding ones on the other have been arbitrarily selected.

The representation of a square on a circle is illustrated by a figure. The surface of the square is divided into smaller squares by lines parallel to the sides, and the curves representing these are drawn on the circle.

As this representation is performed by elliptic functions it gives interesting illustrations of these functions. In the paper attention is called to an illustration of a theorem of Abel, and in a note, added to the present reprint, a quotation from a paper on complex prime numbers, by Jacobi, is made, of which we repeat a part here. Jacobi says:—

"Eine ebenso interessante als schwierige Aufgabe dürfte es sein, dieser Theilung des Lemniscatenbogens in $a + b\sqrt{-1}$ Theile . . . einen geometrischen Sinn abzugewinnen."

This is done by the above representation.

The chief result of the papers considered is that the possibility of a conform representation of one surface on a circle has been proved for certain cases by the actual determination of the function which gives it. Especially it has been shown that a figure bounded either by straight lines or by arcs of circles can be thus represented. These results form the starting-point for the papers in which it is attempted to give strict proofs for Riemann's general theorems stated above.

In the paper "Zur Theorie der Abbildung," it is supposed that the plane surface U to be represented on the area of a circle consists of a simply connected single sheet whose boundary is everywhere convex. The plane is covered by a net of squares, and then that area is taken which contains all those squares that lie wholly or partly within the given area. For this new area U_1, a function u_1 exists, by aid of which the area of a circle is represented on that of the new figure, for it is bounded by straight lines. On halving the sides of the squares, a new area is formed, coming nearer the given one. After m repetitions of the process, we get a function u_m, representing the area of the circle on the last-formed area U_m. If m is now increased indefinitely, then U_m will become coincident with the given area U, and it is proved that the limiting value of u_m becomes an analytical function representing the area of the circle on the given area U. This proof, however, requires that the boundary of U is everywhere convex towards the outside.

It is worthy of notice that the boundary of the surface U is replaced by a broken line whose sides always remain at right angles, and which, therefore, never changes into a curve, which has, generally speaking, at every point a definite tangent. The boundary of U_m is and remains for an increasing m a broken line. This seems closely connected with Prof. Klein's speculations about the impossibility of representing $y = f(x)$ geometrically by a curve. All that a curve can represent is what he calls a *Functionenstreifen*. If we admit this, and consider the boundary of U as given by a *Streifen* (a strip of small but not vanishing breadth), we may divide the boundary into a finite number of parts, such that the strip representing each arc contains an arc of a circle or a straight line. The figure thus obtained can be conformly represented on a circle. Thus we get the theorem that every surface bounded by a *Streifen* can be conformly represented on a circle. This is all that is wanted for physical applications. In these, therefore, there need be no hesitation of applying Riemann's theorems. In fact, physicists never have hesitated, and have used Green's and Thomson's theorems, on which Riemann's depend, as if the boundaries of the solids and surfaces considered were continuous and without thickness, though neither can be true if matter itself is discontinuous in its smallest parts, if it is made up of atoms of small, but not infinitely small, size.

Having considered the papers which deal with the conform representation of one surface on another, we come to those in which the determination of an analytical function by aid of the partial differential equation $\Delta u = 0$ is treated.

We have seen that the determination of $w = u + iv$ as an analytical function of $z = x + iy$ can be made to depend on that of its real part u as a function of x and y, which satisfies $\Delta u = 0$.

According to Riemann, such a function, u, is completely determined if it has at the boundary of a given surface any prescribed values, whilst it is finite and continuous for all points within the surface, or has given discontinuities. It is required to prove that this assertion of Riemann's is true.

In the paper "Ueber einen Grenzübergang durch alternirendes Verfahren," Schwarz gives an outline of his method, but in so short a form that it is difficult to understand the reasoning, there being constant references to theorems which can only be known to those who are well acquainted with the literature of the subject. In the next paper, however, "Ueber die Integration der partiellen Differentialgleichung $\Delta u = 0$ unter vorgeschriebenen Grenz- und Unstetigkeitsbedingungen," we have the same problem treated in an exhaustive manner. All the propositions used are first explained, and only once reference is made to a theorem which Weierstrass has given in his lectures, but not published. This paper is very clear, and contains a complete introduction to the theory of analytical function, or, as Axel Harnack says, it contains "in gedrängter Kürze eine ganze Theorie der Potential functionen" (in a plane).

It is first pointed out that our problem, of determining u subject to given boundary conditions, can always be solved for a circle, and the function which solves it is given in form of a definite integral. The proof for this statement is given in a subsequent paper, "Zur Integration der partiellen Differentialgleichung $\Delta u = 0$," which contains, also, investigations about discontinuities, &c., and which should be read together with the one now under discussion.

It is next shown that the problem for any other surface is reduced to the above if the surface can be conformly represented on the circle. This greatly enhances the importance of such representation.

Three cases of surfaces for which this representation has been obtained are enumerated, viz. a *crescent* formed by two circular arcs, including a segment of a circle; a triangle bounded by arcs of circles (or by straight lines), provided two of the angles are right angles; and, lastly, such a triangle formed by circular arcs which has the third vertex a cusp.

Next an *analytical line* is defined, viz. if $z = f(t)$ is an analytical function, then the curve which the point z describes for real values of t is said to be analytical. Such a line is therefore, by aid of the function $z = f(t)$, conformly represented on a straight line, the axis of real t.

If now the boundary of a surface consists of a finite number of pieces of analytical lines, it will be possible to get, by aid of the equation $z = f(t)$, on the plane (t) a conform representation of one of these pieces, together with a part of the surface, and this may be bounded in such a manner that its representation on the plane (t) becomes, say, a segment of a circle. For this portion of the surface, therefore, our problem (of determining u) can be solved. From the whole surface we can thus cut off pieces next to the boundary for which the problem can be solved. It will therefore also be possible to place on the surface a number of figures such that for each of them the problem can be solved, and that no part of the given surface is uncovered, whilst none projects beyond the boundary. These figures will, of course, overlap.

Suppose, now, we could prove that our problem can be solved for a surface formed by such overlapping figures, if it can be solved for each of the component figures, the part where the figures overlap counting, of course, only once; then the proof would be complete that our problem of determining u as a solution of $\Delta u = 0$, so that it has prescribed values on the boundary of a given surface, has a solution if the surface is bounded by a finite number of pieces of analytical lines.

This is what Schwarz does by the "Grenzübergang durch alternirendes Verfahren." He supposes that T_1 and T_2 denote two surfaces, such that for each of them the problem has been solved, and places these so that they overlap, and their outer boundaries form a new surface, T.

The two figures have a region T^* in common; the figure T may therefore be expressed as $T = T_1 + T_2 - T^*$. Part of the boundary of T_1 is boundary of T; the other lies within T_2 and is boundary of T^*. The same is true of T_2. There is, first, a function u_1 determined for T_1, which has, on that part of the boundary belonging to T, the prescribed value, and on the other parts, within T_2, the value zero. Then a function u_2 is determined for T_2, satisfying the analogous boundary condition, only on the part of its boundary within T_1 it receives the values which the first function u_1 has there.

Then new functions are determined alternately for T_1 and T_2, each having on that part of the boundary which belongs to T the prescribed values, and on that part which belongs to T^* the values which the last function for the other area gives it. The effect is that, ultimately, two functions, u' for T_1 and u'' for T_2, are obtained, which coincide throughout the boundary of T^*, and which, therefore, must be identical for the whole common region T^*. But this implies, again, that they are values of the same function u, which satisfies, therefore, all conditions.

Riemann's principal theorem is thus proved, not for any surface, but for a case of very great generality. For the theory of analytical functions this seems sufficient, as it is difficult to conceive the necessity of having to use surfaces bounded by quite arbitrarily given boundaries.

In the theory of potential such boundaries might occur, but here Klein's investigations already referred to are pertinent. These, in fact, seem to brush away a great number of the difficulties which have been introduced by starting from too general definitions.

The theorems used by Riemann in his theory of Abelian functions, for instance, can all be proved by the above results of Schwarz, who, indeed, shows how the discontinuities which occur here can be treated by his method. He also extends some of his results to conform representation of surfaces on a sphere, and completes the investigation in his earlier papers.

Of the remaining papers we can only give a very short account. There are first two papers on developable surfaces of the first seven orders, and more particularly on those of order five.

The papers "Bestimmung der scheinbaren Grösse eines Ellipsoids für einen beliebigen Punkt des Raumes" and "Zur conformen Abbildung der Fläche eines Rechtecks auf die Fläche einer Halbkugel" contain applications of the use of Weierstrass's elliptic functions. Those who take an interest in the latter, which are in England known chiefly through various papers by Prof. Greenhill, will be glad to learn that Herr Schwarz promises in the preface of vol. ii. a new edition of his "Formeln und Lehrsätze zum Gebrauche der elliptischen Functionen."

In the important paper "Ueber diejenigen Faelle, in welchen die Gaussische hypergeometrische Reihe eine algebraische Function ihres vierten Elementes darstellt," we have applications of the theories just discussed, but we must forbear from entering into a discussion of it.

The investigation rests on a discussion of the well-known differential equation of the second order, of which the series is an integral. The results obtained are included in Forsyth's "Treatise on Differential Equation," though proved in a very different manner. They have enabled Klein to determine all differential equations of the second order which have algebraical integrals.

This paper, "Ueber algebraische Isothermen," contains this theorem:—

If a complex variable $w = f(z)$ has the property that the curves for which the real part u of w has a constant value are algebraical, then w is either an algebraical function of z, or μw, where μ or μi is some real number, is the logarithm of an algebraical function, or else μw is an elliptical integral of the first kind, with real modulus whose superior limit is an algebraical function of z.

All these investigations require a deep study of the very foundations of analysis. These cannot help to reveal a number of inaccuracies and gaps in the ordinary theories as contained in text-books. Accordingly we find several papers in the collection which contain such corrections. Thus there is one paper in which a complete system of independent conditions is given which underlie the proposition that $d^2u/dxdy = d^2u/dydx$.

In another paper the definition of the area of a curved surface in the first edition of Serret's "Calcul Différentiel et Intégral" is shown to be wrong. In another long paper it is proved that of all solids of given volume the sphere has the smallest surface. All previous proofs depend on the supposition that *one solid exists* which has a minimum surface; of this the present proof is independent.

The first volume contains papers relating to surfaces of minimum area. The original problem is one of the calculus of variation, viz. a given closed curve in space being given, it is required to determine that surface bounded by it which has the least area. One of the chief properties of such surfaces is that the principal radii of curvature at each point are equal but opposite, or the mean curvature is everywhere = 0. Hence all surfaces which have this property are called "surfaces of minimum area," though it is not any longer true that surfaces of this kind have the original property for every part cut out by any curve drawn on them, just as the arc of a great circle on a sphere ceases to be the shortest line between its ends as soon as the arc becomes greater than a semicircle. The question to decide whether this is the case for a given closed curve on the surface is considered in the paper "Ueber ein die Flächen kleinsten Flächeninhalts betreffendes Problem der Variationsrechnung." It is, of course, a problem about the second variation.

There is an interesting connection between the surfaces of minimum area and the conform representation of one surface upon another, viz. every such representation of the whole surface of a regular polyhedron on a sphere gives rise to a surface of minimum area, which in this case contains an infinite number of straight lines.

In the first paper the surface is considered which is thus obtained from the cube. This paper, when communicated to the Berlin Academy, was illustrated by models. In the present reprint, nicely executed shaded figures of these are added. The first gives the surface corresponding to one face of the cube. It is bounded by four edges of a regular tetrahedron. In the second we have the surface corresponding to all six faces of the cube. It forms one continuous sheet. If this surface be still further continued, a surface is obtained which extends throughout the whole space. Of this the third plate gives a part. Analytically the problem depends on elliptical functions.

These investigations are continued in the second paper (and several others), which obtained a prize of the Berlin Academy. The prize problem required the complete solution, by aid of elliptical or Abelian functions, of some important problem taken from almost any part of pure or applied mathematics. Herr Schwarz treats of the surface of least area bounded by any skew quadrilateral.

In "Fortgesetzte Untersuchungen über specielle Minimalflächen," a new problem is proposed, viz. there is given a closed chain consisting of straight lines and planes, it is required to find a surface of minimum area bounded by the lines and perpendicular to the planes.

Of the other papers we mention the "Miscellen aus dem Gebiete der Minimalflächen." It contains a highly interesting review of the whole subject, including Plateau's investigations, and is full of suggestions.

The volumes, which are dedicated to Weierstrass, are well printed on octavo pages sufficiently large to give room for the formulæ required, and not so large as to be unwieldy, as is the case with a recently published "Collection." But there is one point in which the edition might have been improved, trifling as far as editing and printing are concerned, but of great benefit to the reader. It is very desirable that in all editions of collected papers the examples set by Sir William Thomson and Prof. Cayley should be followed, of placing the date of the first publication of each paper both in the table of contents and at the head of each paper. O. HENRICI.

GEOGRAPHICAL EXPEDITIONS.

M. GROMBCHEVSKY, now at St. Petersburg, has given the Russian Geographical Society a most interesting account of his last expedition. It is known that the Expedition left Marghelan in June 1889, and that having found the Alai Mountains deeply clothed in snow, they went to Kala-i-khumb through Karategin and Vakhia. They found that the khanate of Shugnan was at war with the Afghans, and as the latter refused to let the Expedition go further, M. Grombchevsky returned to Vakhia, after having crossed the Sytarghi Pass, which has on its western slope a great glacier, six miles long. In August, after having made a long circuitous journey over the Pamir (the well-known Pamir robber, Sahir-Nazar, being the guide of the Expedition), they reached the frontier of the Pamir khanates now occupied by the Afghans, and waited there for the Ameer's permission for further advance. A refusal was received in October, when the temperature already was from 20° to 24° C. below zero, and the Expedition could find no fuel of any kind. So they crossed the Mus-tagh ridge (yaks being used for the transport of provisions), and reached the valley of the Raskem River, where they met with Mr. Younghusband. During their fifty-five days' stay on the banks of the Raskem, they explored the passes of Shimshal, Mustagh, and Balti-davan, leading to Kashmir, as well as the passes across the Raskem ridge leading to Kashgaria. In November, M. Grombchevsky was at the Kashmir fort Shahidulla-kodja; but the fort was abandoned, and, the Expedition having no provisions, they asked permission to enter Kashmir and to winter there. But Colonel Nisbet refused admission to Kashmir, so that the Expedition had nothing to do, M. Grombchevsky says, but to move eastward, across the desert plateaus of Tibet, in order to reach some inhabited spot. Moving up the Kara-kash, the Expedition ascended the Tibet plateau. The thermometer fell as low as − 33° to − 35° C., all water was frozen, and two-thirds of the horses died; so that all natural history collections were abandoned, and, notwithstanding a frightful snowstorm, the Expedition re-crossed the mountains and went to Kashgaria. The first settlements were reached in February. Next month M. Grombchevsky went to Khotan, and thence to Niya, where he met with the commander of the Tibet Expedition, M. Pyevtsoff. At the end of March, he visited the Sourgak gold-mines in the south of Niya—where he found 3000 men busy in gold-washing—and Polu, whence he again ascended the Tibet plateau, and after some explorations he returned to Kashgaria again. In the autumn he visited the middle course of the Raskem River, making acquaintance with interesting tribes of mountaineers, and thence returned to Russia. The geographical results of the expedition seem to be very important. Surveys were made over a length of 5000 miles, and latitudes and longitudes were determined at 73 different spots; heights were measured throughout the journey, and photographic views taken; and rich geological, botanical, and entomological collections were secured.

On January 31, in the great amphitheatre of the Sorbonne, Paris, the French Geographical Society held a special meeting for the reception of M. Gabriel Bonvalot and Prince Henry of Orleans, whose travels in the heart of Central Asia have won for them an honourable place in the ranks of modern explorers. The chair was taken by M. de Quatrefages, who warmly congratulated the explorers on their achievements, and announced that the Society had conferred on the Expedition its large gold medal, the highest reward at its disposal.

M. Bonvalot, the chief of the Expedition, gave a full and interesting account of the journey. He left Paris with Prince Henry on July 6, 1889, and arrived on September 1 at the Russo-Chinese frontier, near which their caravan was organized. At Kuldja they met Father Dedékens, a Belgian missionary, who, to their great satisfaction, consented to accompany them, and rendered them important services. Having crossed the mountains of Tian-Shan, they arrived at Kurla, in Chinese Turkestan, where M. Bonvalot engaged fresh camels. At Lake Lob-Nor they reorganized their caravan, and laid in stores for six months. They then crossed the chains of Altyn Tagh, the Tshimen Tagh, and the Columbo Mountains, travelling sometimes at heights of more than 4000 metres. The region was wild and desolate, and the cold intense; and M. Bonvalot found it necessary to limit the Expedition to fourteen men, forty camels, and eighteen horses, the rest being sent back. Having followed for some time the traces of a caravan in the direction of Lhassa, he decided to keep to the same route as far as it could be made out; and in his address at the Sorbonne he gave a vivid description of the difficulties the party encountered in trying to discover the way the caravan had taken. On December 31, at a height of more than 5000 metres, a terrible storm caused them to lose sight of the marks by which they had been guided; whereupon they journeyed along the 90th degree of longitude. They found great chains of mountains, vast lakes, extinct volcanoes, geysers, and a Pass at an altitude of 6000 metres. Below 5000 metres they met with herds of wild yaks, antelopes, and other animals. Birds had wholly disappeared, and there was no vegetation. The only water they could obtain was melted ice, and cooking was impossible. Two men died, and the animals perished one after another. At last the traces of the route were discovered, and the Expedition arrived at Lake Ten'gri-Nor, where they met certain Tibetan authorities, who were accompanied by numerous horsemen. They had great difficulty in proving that they were Frenchmen, but after forty-five days of negotiation, at Dam, near Lhassa, the Tibetans provided them with the means of continuing their journey, as they had lost all their own means of transport.

The travellers followed what is called "the little route" from Tibet to China—a route still unexplored. They crossed the territory of independent tribes, who, in accordance with the wishes of the Llama, furnished them with yaks and horses. They were now in a region of valleys, and of wooded grounds well supplied with game and with large wild animals. In the course of three days they saw twenty-two bears. Some of the valleys are cultivated and occupied by villages. The Expedition followed the upper courses of the Salouen and the Mékong, and that of the Yang-tse-kiang, the sources of which they thought they recognized on the southern side of a colossal chain of mountains which they called "Monts Dupleix."

At Batang, which they reached on June 7, 1890, they met with Chinamen. They rested for a month at Ta-Tsien-Lou, on the Chinese frontier, where they received a cordial welcome from French missionaries; and on July 29, they started for Tonkin, arriving at Yunnan on September 5, where they found a letter from Europe, dated September 5, 1889. Reaching Manghao, on the Red River, they hired Chinese junks, and entered Tonkin at Lao-Kaï. Soon afterwards they were at Hanoï. Altogether, they had traversed 2500 kilometres on an unknown route.

Among the more important of the geographical results of the journey is the discovery of volcanic regions. On December 22, 1889, they observed on the plateau they were crossing a *coulée* of lava; and, looking towards the horizon, they saw in the west an isolated volcano, to which they gave the name of Mount Reclus, in honour of the well-known geographer. Further on, they came to other volcanoes, near which they saw great blocks of lava, which at a distance they took for yaks. One small chain reminded them of the mountains of Auvergne.

In the great chain of Dupleix they found fossils (bivalves), belonging to Tertiary strata, at a height of 5800 metres. In the same region they discovered various minerals, especially iron and lead. At the foot of the Dupleix chain, among rocks, they met with grey monkeys, with rather long hair and short tails. These creatures appeared to be isolated, as they had not been seen before, and were not seen afterwards.

At the meeting of the Royal Geographical Society on Monday, Mr. E. G. Ravenstein gave some account of the British East Africa Company's Expedition, under Mr. F. J. Jackson, from Mombassa to Uganda. The route up to Machako's, about 250 miles north-west of Mombassa, is already pretty well known from the narratives of Mr. Joseph Thomson and others. The portion between Machako's and Uganda had also been traversed to some extent by Mr. Thomson, as well as by Count Teleki and the late Dr. Fischer. Captain Lugard found that the plateau, which rises to about 6000 feet at Machako's, is much broken up by ravines, while there are numerous waterless stretches, where, however, water can generally be found by digging. There are numerous valleys and glades, with abundant vegetation; many patches of forest, mostly of soft-wood trees, and even several perennial streams. Iron and copper are abundant in some places, and indications of gold were found by Captain Lugard. From Machako's, Mr. Jackson's caravan had to make its way up the steep face of the Kinangop escarpment, 9000 feet in altitude, below which, in the valley between that and the equally steep and high Mau escarpment lay lake Naiwasha, and several other lakes, all without outlets, and yet all fresh. A descent of some 3000 feet has to be made to the lakes. These two escarpments, which may be said to extend more or less continuously from Abyssinia to Ugogo, are, Mr. Ravenstein pointed out, two of the most remarkable physical phenomena on any continent. The plateau between Machako's and Lake Victoria Nyanza is even more broken up by deep ravines than that between Machako's and the coast, so that travelling becomes of the most trying character. While the country here is to a large extent of a steppe character, still there are some districts of the highest fertility. In some cases the forest has been cleared away, and the country cultivated by the natives, some tribes being great cattle-rearers. Many of the gorges are still densely clad with forests, and beyond the Mau escarpment is a perfect network of rivers. Game was plentiful and buffaloes were seen in large herds. The north-east corner of Lake Victoria Nyanza has been laid down more accurately than on existing maps, and the contour given to it by Mr. Stanley is in all essential respects confirmed. Usogo, where the Expedition received a cordial welcome, is evidently one of the richest countries in Africa; a marked contrast to Uganda, which, owing to the strife which has prevailed since the death of Mtesa, has been converted into a wilderness

Before entering Usogo, Mr. Jackson made a detour to the north-east of Mount Elgon, but did not succeed in reaching Lake Rudolph, visited by Count Teleki. The country in this direction is of a barren steppe character, sparsely covered with bush, and with a few heights rising above the general level. On his way back, Mr. Jackson and his caravan travelled right across the summit of Mount Elgon, one of the most remarkable mountains in Africa. It is an extinct volcano, the crater of which is eight miles in diameter, its appearance reminding one of the great craters seen in lunar photographs. This mountain is over 14,000 feet high, and, taken in combination with Kilimanjaro, Kenia, and Ruwenzori, seems to indicate that at one period this must have been a region of intense volcanic activity. High up on the face of this mountain Mr. Jackson came upon the caves of which Mr. Thomson told us. These he found to be entirely natural, and not the work of man. One is so large that on its floor has been built a village of huts; for the caves are inhabited by natives who have been compelled to take refuge here from their enemies in the plains. Mr. Jackson's natural history collections are very extensive; very many new species of birds and insects have been sent home. Mr. Bowdler Sharpe stated that these collections have revolutionized existing notions as to the zoological geography of Africa. In the Mount Elgon region types are found similar to those of Abyssinia on the one hand and the Cape on the other; and Mr. Sharpe stated that the region most resembling that of Elgon is that of the Cameroons Mountains in West Africa; but this is based mainly on the ornithology of the two regions, the entomology leading to somewhat different conclusions. On the whole, the geographical and natural history results of the expedition are of high importance, and credit is due to the British East Africa Company for encouraging work of this kind.

NOTES.

PROF. HELMHOLTZ, as we have already stated, will celebrate his seventieth birthday on August 31. In honour of the anniversary, a marble bust of Prof. Helmholtz will be prepared; and it is proposed that there shall be a Helmholtz Medal, to be bestowed on the most eminent German and foreign physicists. An international committee has been formed for the purpose of carrying out these schemes.

AT its last meeting, January 28, the Russian Geographical Society awarded its two great gold Constantine Medals to Prof. A. Potebnya for his numerous ethnographical and philological researches, and to Prof. Th. Sloudsky for his geodetical work. The Count Lütke's medal was awarded to S. D. Rylke, also for geodetical work, and especially for the mathematical discussion of the results of the recent exact levellings. Gold medals were awarded to P. Rovinsky for his geographical and ethnographical description of Montenegro; to N. Filipoff for his work on the changes of level of the Caspian Sea; to V. Obrutcheff for a work on the Transcaspian region; and to V. Priklonsky for his manuscript, "Three Years in the Yakutsk Region." A number of silver medals were awarded to several persons for many years' meteorological observations, and various ethnographical works of minor importance.

THE ninth German Geographentag, which will meet in Vienna on April 1, 2, and 3, will deal chiefly with the present state of our geographical knowledge of the Balkan peninsula, and with the investigation of inland seas. A geographical exhibition will be held in connection with the meeting.

A ROYAL Commission has been appointed to inquire into the effect of coal-dust in originating or extending explosions in coal-mines. Mr. Chamberlain is to be the Chairman, and his brother Commissioners are Lord Rayleigh, Sir William Lewis, Prof. Dixon, Mr. Emerson Bainbridge, and Mr. Fenwick, M.P. A small committee of experts has already investigated the subject.

ON Friday last, in the House of Commons, in answer to Sir H. Roscoe, Mr. Plunket said it had been decided to proceed at once with the completion of the buildings in connection with the Science and Art Department on the east side of Exhibition Road. They would ultimately be devoted wholly to art collections, although for some years it was probable that some portions of them might be temporarily available for science collections. The buildings would, however, take several years to complete, and he was in communication with the Science and Art Department as to the best means of providing for the science requirements, and he hoped soon to be able to submit a proposal to the Treasury. As the buildings on the east side of Exhibition Road would cost some £300,000 or £400,000, it was obvious that any further immediate demands on the Chancellor of the Exchequer must be confined within as narrow limits as possible.

SOME time ago Sir Joseph Fayrer announced his intention of retiring from the presidency of the Sanitary Assurance Association. At the tenth annual meeting of the Association on Monday, Mr. Rutherfurd referred to the great services Sir Joseph had rendered to the cause of sanitary improvement, and proposed a resolution expressing warm appreciation of the valuable services rendered by him during the ten years in which he had held the office of President, and assuring him of "their high admiration for the zeal and energy manifested in his disinterested and gratuitous discharge of those services (amidst numerous other public and private duties) to the undoubted promotion of the general health and public good." This was seconded by Surgeon-General Cornish, and warmly supported by Prof. Smith, and adopted. Sir Joseph Fayrer, in acknowledging the resolution, said he should continue to take a lively interest in the Association and its work, and would retain his seat on the Council. Surgeon-General Cornish, late Sanitary Commissioner with the Madras Government, was elected President; and Sir Joseph Fayrer and Prof. Roger Smith were elected Vice-Presidents.

A CAPITAL paper on decimal coinage, weights, and measures was delivered before the Society of Arts on February 4 by Mr. J. Emerson Dowson, and is printed in the current number of the Society's Journal. He urged that there is pressing need for a thorough investigation of the whole subject by a Royal Commission; and, as evidence of the opinion of important bodies of commercial men on the subject, he mentioned that all the seventy-two Chambers of Commerce of the Association of the United Kingdom have repeatedly pronounced themselves in favour of the decimal system, and that the four large Chambers which are not members of the Association (Edinburgh, Glasgow, Liverpool, and Manchester) have taken the same ground. Sir Henry Roscoe, who presided, remarked that he himself had largely benefited by the use of the metric system, which was employed of necessity by men of science in all countries. He had also seen the simplicity and ease with which arithmetic was taught in foreign schools, and could bear testimony to the way in which the old German system of weights and measures was entirely swept away in a few months, when the new system became the law of the land, and how readily it was adopted by the people. Mr. Goschen told Mr. Leng, in answer to the memorial sent from the Dundee Chamber of Commerce, that he was well aware of the strong case which the advocates of a decimal system had made out, but the difficulties were very great, and he could not undertake to bring in a Bill. They did not expect him to do so at present; the matter required further consideration; for although several Committees and Royal Commissions had sat upon it, the whole question was not then considered, and they wanted a new Royal Commission to consider the whole subject. He could not help thinking that if pressure were brought to bear on the Government by Chambers of Commerce, and by all interested in the simplification of the present confusion, not only would a Commission be granted, but the public would be ripe for a decisive step being taken that would put us on a level with all the rest of the civilized world.

AT the meeting of the Institution of Civil Engineers on February 3, a valuable paper on electric mining machinery, by Messrs. Llewelyn B. and Claude W. Atkinson, was read. The authors maintained that electric power was destined to become an important factor in mining mechanics, on account of (1) the facility with which it could be used with machines which required to be moved from time to time; (2) the great economy in first cost and reduced cost of working owing to its efficiency being higher than that of compressed air or any other medium of power transmission; (3) the smaller cost of maintaining the cables, as compared with piping on shifting floors in roadways, &c. Methods were described which the authors considered sufficient to obviate all objections to the use of electric motors in coal-mining, whether by excluding inflammable gases or by constructions which would allow of their safe combustion. Experiments, trials, and practical work, extending over four years, showed, it was contended, that—(1) electrical pumps might be used with advantage and economy for mine-draining; (2) electrical coal-cutters could replace hand-labour with saving in cost, and increased production of coal; (3) electrical drilling-machines were available in place of machinery worked by hand or compressed air.

AT the meeting of the Royal Botanic Society on Saturday last, a gift of seeds of the Parà rubber-tree suggested to Mr. Sowerby, the Secretary, some interesting remarks on india-rubber and gutta-percha. In the Society's museum was a specimen of the first sample of gutta-percha imported to Europe—viz. in 1842—and it was shortly after that date that it was used to insulate the first submarine telegraph cables. No substitute had been found to take its place. From some papers lately published in the *Electrical Review*, he gleaned that from the "wholesale cutting down of adult trees" and the "reckless clearing and burning of the forests" the trees furnishing the most valuable kinds of gutta-percha had become exceedingly scarce, and in most localities utterly extirpated. This was also rapidly becoming the case with the trees which supply the many varieties of india-rubber, and, sooner or later, all natural vegetable products used by man would have to be artificially cultivated, as the natural supply never kept pace with the artificial demand. Some few attempts had been made to cultivate india-rubber, but as yet not very successfully.

THE latest report of the Oxford University Extension Delegacy, ending with the last summer meeting, is highly satisfactory. It appears from it that the number of courses of lectures has grown from 27 in 1885–86 to 149 last year, that the number of centres at which lectures were delivered has risen from 22 in the same year to 100 in the last session, that, whereas in 1886–87 the average number of students attending the different courses was 9908, now no fewer than 17,904 receive in the different centres the instruction given. History seems from the return to be the favourite subject—85 courses were given upon it; on literature and art 28; on various branches of natural science 25; on political economy 10. "In August, 1889," the report continues, "the second summer meeting of Oxford University Extension students was held in Oxford. I was attended by 1000 students, and lasted rather more than a month, this period being divided into two parts to meet the convenience of those students whose duties prevented them from being present during the whole meeting. During the year the system of training lecturers on their appointment has continued." Finally the report acknowledges the receipt of various scholarships intended to enable poor students to avail themselves of the oppor tunities afforded by the summer meeting, Lord Ripon and Mr. J. G. Talbot, M.P., being the most considerable donors.

THE other day the members of the Manchester Geological Society attended a special meeting, which was held in the Geological Museum, Owens College, to hear an address by Prof. Williamson on the vegetation of the Carboniferous age. The special object of the meeting was to afford information to those engaged in the superintendence and working of coal-mines, which would enable them to assist in the collection of specimens likely to be of scientific interest. Prof. Williamson gave a lucid and interesting description (in which he was aided by some beautiful diagrams and specimens) of the families of plants represented in the coal measures. Speaking of the assistance he had received, and still hoped to receive, from geologists residing in Oldham and elsewhere, in working out their structure, he said that one great desire he had was to enrich the museum with as complete a collection as possible. All his own specimen would shortly be transferred to that building, and he therefore felt all the more confidence in asking the help of others. He invited members of the Society to preserve and send to him any fossil plant, however fragmentary, which came into their possession from the Carboniferous strata, taking care to note the character and position of the bed in which it was found. They wanted the most exact information obtainable with reference to the vertical range of species, and also as to their range horizontally. In getting the specimens together it was of importance that they should avoid mixing those from different seams. In this way most valuable information might be gained with regard to the life-history of many at present little-known plants of the coal measures.

THE Agricultural Department of Victoria has applied to the U.S. Minister of Agriculture for the services of an expert in the growth and manufacture of tobacco. According to the *Australasian*, there is a large field for the growth and manufacture of tobacco in Victoria, but so far the efforts made in that direction have met only with indifferent success, owing to the defects arising from want of knowledge in the drying and treatment of the leaf after it has been cut. It is hoped that, if the services of a thoroughly competent expert were secured, these defects would be removed, and that before very long tobacco would form one of the chief products of the colony.

A RECENT communication of Herr Büchner to the Imperial Academy of Sciences of St. Petersburg announces that, among other objects obtained in the Chinese Province of Kansu by Herrn Potanin and Beresowski, during their expedition of 1884–87, was a skin of *Ælluropus melanoleucus*. This very remarkable Bear-like animal is hitherto known only from the specimens which were procured by Père David in the principality of Moupin, in the north of Szechuen, and which are now in the Paris Museum. Herr Beresowski met with it in the mountains of Southern Kansu, at an elevation of 10,000–12,000 feet, where it inhabits the Bamboo-bushes, and is known to the natives as the *Pei-ssjun* or *Chua-ssjun*, *i.e.* White, or Spotted, Bear. Few presents, we imagine, would delight the heart of the Director of the British Museum of Natural History more than examples of this rare and little-known Mammal. As France and Russia can now both boast of specimens, England, whose interests in China are so predominant, surely ought to be able to obtain some likewise.

An interesting paper on the destruction of wolves in France appears in the current number of the *Revue Scientifique*. The law in virtue of which rewards are given for the killing of wolves was passed on August 3, 1882, and during the last four months of that year 423 were destroyed. In 1883 the number killed was 1316, the sum paid in rewards being 104,450 francs. The number was 1035 in 1884; 900 in 1885; 760 in 1886; 701 in 1887; 505 in 1888; 515 in 1889. The departments in which most animals have been slain are Dordogne and Charente. It is believed that very soon no specimens will be left in France except those which occasionally reach it from neighbouring countries.

It has been a practical difficulty in the use of electrical accumulators, to know, at any given time, to what extent they are charged or discharged. M. Roux, we learn from *La Nature*, has devised an indicator of charge, based on the principle that the mean density of the liquid varies according to the quantity of electricity stored. He uses a flattened glass tube, of length about equal to the depth of the liquid, and weighted so as to be heavier than the thrust it receives. It is hung by means of a platinum wire to one end of a short lever, pivoted in the middle, and having a weight on the other arm; with full charge, this lever is horizontal. At right angles to the lever is a weighted pointer, which gives indication on a horizontal scale above, with 100 divisions; the pointer, vertical at 100 when the accumulator is charged, stands at 45 after discharge. The readings on the scale indicate directly the percentage ratio of the quantity remaining in the accumulator to that of saturation. By means of electric contacts, &c., the state of charge can be announced. M. Roux finds by experiments the differences between indications of his apparatus and those of an ampere-meter placed in the circuit to have been always less than 3 per cent., which is considered very satisfactory, the object being industrial.

The *Annals of the Astronomical Observatory of Harvard College*, vol. xxi. Part ii., contains a summary of the meteorological observations made in the year 1889 under the auspices of the New England Meteorological Society, and three investigations dealing respectively with types of New England weather, land and sea-breezes, and the characteristics of the New England climate. These essays are of a very exhaustive nature, and should be of much use to meteorologists.

The recent earthquake in Algiers did little damage in the town itself. The first shock, which occurred at 4 a.m. (on the morning of January 15), was followed by a more violent one at 4h. 10m. lasting 12 seconds. Slight shocks were also experienced at 5 a.m. and at 7 a.m. The direction of the shocks was from south-east to north-west. The inhabitants fled from their houses to the open places. The shocks were most violent at Gouraya, near Cherchel, which also suffered. In Philippeville two persons were killed and many injured by the fall of a house. In Villebourg many houses are in ruins; and numerous lives were lost there, and at Blidah, Medeah, and Orleansville.

The journals of Nismes record three deaths in that neighbourhood resulting from poisoning by *Amanita citrina*, which appears, at least in the south of Europe, to be one of the most deadly of Fungi.

A "Flora of Palestine" is in progress edited by the Rev. G. E. Post, and is now completed as far as the end of the order *Umbelliferæ*. Several new species are described.

The death is announced, on January 24, of Dr. Philip Carl, Professor of Physics at the Munich Royal Military College, and late editor of the *Repertorium für physikalische Technik* and the *Zeitschrift für Elektrotechnik*.

Captain Abney will deliver at the Society of Arts a course of five popular lectures on "The Science of Colour," on the following Friday afternoons, at half-past four o'clock:—February 13, 20, 27, March 6, 13. The lectures will be popular and elementary, and fully illustrated by experiments.

The atomic weight of rhodium has been redetermined by Prof. Seubert and Dr. Kobbé, of the University of Tübingen, and an account of their results is published in the current number of *Liebig's Annalen*. During the last few years the atomic weights of the other five metals of the platinum group, ruthenium, palladium, iridium, platinum, and osmium, have formed the subjects of most careful investigations, mainly at the hands of Prof. Seubert, with the result that these values are considered as among the best determined of all atomic weights. The remaining metal, rhodium, must now be added to the list, for the present redetermination leaves no doubt whatever that the real value of this atomic weight has been arrived at within the ordinary limits of inevitable experimental error. The two principal former determinations afforded widely different results. That of Berzelius, in 1828, from an analysis of the salt $4KCl \cdot Rh_2Cl_6$, gave the number 104·07; while Jörgensen, in 1883, employing the ammoniacal salt, $Rh_2(NH_3)_{10}Cl_6$, deduced the value 103·00 ($O = 16$) or 102·74 ($O = 15·96$). In the present redetermination, Seubert and Kobbé made use of the same ammoniacal compound as Jörgensen. Fine crystals of this salt are readily prepared in a state of purity, they are very stable in the air, and the salt lends itself readily to accurate analysis. It was thus in all respects eminently suitable for the purpose of an atomic weight investigation. The experimental method of treatment which was found to be attended with least possibility of error consisted in reducing the heated salt in a current of pure hydrogen to metallic rhodium, and thus determining the ratio of the weight of salt to its content of rhodium. The finely powdered crystals, after drying at 100° C., were weighed in a porcelain boat; the boat and its contents were then placed in a combustion-tube lined internally with a cylinder of platinum, the tube was gradually heated in the furnace, and a stream of hydrogen passed through. After complete reduction, and displacement of the hydrogen by a current of carbon dioxide, the metallic rhodium was found in the boat in the form of a bright grey rod. The boat and metal were then again weighed. Ten such experiments yielded values ranging, when $O = 15·96$, from 102·61 to 102·81. As the final mean, the number 102·7 is adopted; or, if $O = 16$, the round number 103·0. These values confirm those obtained by Jörgensen in an exceptionally satisfactory manner, thus setting the question finally at rest. Rhodium, therefore, retains the place in the periodic system marked out for it by its chemical behaviour, between ruthenium, of atomic weight 101·4, and Pd 106·3, and in the same vertical group as its analogue iridium.

The additions to the Zoological Society's Gardens during the past week include two Malbrouck Monkeys (*Cercopithecus cynosurus* ♂ ♀) from West Africa, presented respectively by Mr. J. P. Heseltine and Mrs. Newton; two Common Peafowls (*Pavo cristatus* ♀ ♀), bred in Scotland, presented by Mr. Richard Hunter; two Globose Curassows (*Crax globicera* ♂ ♀), two Mexican Guans (*Penelope purpurascens*) from Central America, a Daubenton's Curassow (*Crax daubentoni* ♂) from Venezuela, deposited.

OUR ASTRONOMICAL COLUMN.

"Annuaire du Bureau des Longitudes."—This extremely useful and unique *Annuaire* for 1891 has just been published. The astronomical information is as complete as could be desired. The tables of physical and chemical constants are of the same comprehensive character. MM. Lœwy and Schulhof give an account of the comets that appeared between 1800 and 1826, and in 1889. This list completes those given from 1882 to 1890, and forms with them a catalogue that contains references to every published comet observation made this century. A table of sixty-two double stars, of which the elements are known, is given for the first time by M. Glasenapp. Another table, constructed by M. Bossert, contains the proper motions of sixty-nine stars. M. Cornu contributes a succinct description of three types of stellar spectra, and an interesting article on the astronomical applications of Doppler's or Fizeau's principle. M. Janssen gives an account of his ascent of Mont Blanc for the purpose of studying the telluric spectrum. M. Tisserand points out the uses of the minor planets, and discusses the communications made at the International Conference on Degree Measurements, held at Freiburg on September 15, 1890. In the portion of the work devoted to terrestrial magnetism, mention is made of anomalous disturbances similar to those found by Profs. Rücker and Thorpe in England. Many other important points are brought together, and the whole stands forth as a *vade mecum* having no equal.

UNITED STATES NAVAL OBSERVATORY.—Captain F. V. McNair, the Superintendent of this Observatory, has recently issued his report for the year ending June 30, 1890. Prof. A. Hall has used the 26-inch equatorial for observations of double stars, Saturn and its satellites, and Mars. The reduction of these observations is now in progress. Prof. Harkness has employed the transit circle in observations of the sun, major and minor planets, and certain stars for clock and instrumental corrections. The 9·6 inch equatorial has been used by Prof. Frisby for observations of comets, minor planets, and occultations of stars by the moon. Lieut. Hodges has made observations with the meridian transit instrument. Mr. Paul has continued the observations of his new Algol-type variable, S AntilIæ, referred to in the last report. The reductions show that the period deduced by Mr. Chandler in the *Astronomical Journal*, No. 190, viz. 7h. 46m. 48·0s., will be changed by only a very small fraction of a second. The magnetic observations and testing of chronometers have been continued as usual. As in many other Observatories, it is complained that the staff is insufficient to reduce and discuss the astronomical, geodetic, gravitational, and tidal observations that are made.

A NEW THEORY OF JUPITER AND SATURN.—In the Astronomical Papers prepared for the use of the American Ephemeris and Nautical Almanac, vol. iv., Mr. G. W. Hill develops a new theory of the movements of Jupiter and Saturn. The guiding principle of the investigation was "to form theories of Jupiter and Saturn which would be practically serviceable for a space of three hundred years on each side of a central epoch taken near the centre of gravity of all the times of observation: theories whose errors in this interval would simply result not from neglected terms in the developments, but from the unavoidable imperfections in the values of the arbitrary constants and masses adopted from the indications of observation." It is seven and a half years ago since the necessarily laborious computations were commenced, and many years must elapse before any verdict can be given as to their accuracy. The theory appears, however, perfect, and the author is to be sincerely congratulated upon its development.

ARGELANDER-OELTZEN STAR CATALOGUE.—Dr. E. Weiss has completed his comparison of the stars contained in the catalogue constructed by Oeltzen from Argelander's southern zones and reduction-tables, with those contained in Schönfeld's Southern *Durchmusterung* and the Cordoba Catalogue. The work forms a supplementary volume to the Annals of the Observatory of Vienna University. The places of 18,276 stars are given for 1850, and the value of total precession which will enable them to be determined for the mean epoch 1875·0. About 1000 stars had to be examined with the Vienna equatorial, because they were not contained in the Cordoba zones, and 200 others for verification of position. Very few differences of magnitude have been found, and these were only of small amount. Argelander's Catalogue has always been of extreme value to astronomers. Prof. Weiss's revise renders its usefulness inestimable.

TECHNICAL INSTRUCTION IN ESSEX.[1]

THE Council of the Essex Field Club proposes to establish, at the cost of the Club, in Chelmsford (chosen not only as the county town, but also as being a central position in Essex), a public Museum to illustrate the natural productions, the geology and physiography, and the industries and manufactures of Essex, together with an educational series of specimens and preparations, which may be employed for teaching purposes. The Museum will also contain a library of books, maps, parliamentary papers, pictures, &c., treating of the natural history, geology, topography, history, and industries of Essex, as well as a general library of books necessary for the study of the before-mentioned subjects.

The Council of the Club is very desirous that the Museum shall be of the greatest possible service in promoting the study and love of science and its applications to industries and manufactures, and as a subject of general education. In the endeavour to carry out these objects, the Club most respectfully asks for the aid of the County Council of Essex, in accordance with the powers given by the above-mentioned Acts.

The leading features of the technical education scheme of the Essex Field Club are as follows:—

(1) The establishment of a central institution in Chelmsford in connection with the Club's Museum, with large laboratories and class-room, furnished with apparatus and preparations for practical teaching, and in which, as occasion may arise, examinations could be conducted; the institution being also amply provided with lecturing and class-teaching appliances (lanterns, slides, diagrams, apparatus, models, materials, and specimens) so arranged in travelling cases that they could be easily sent to any part of the county for use at the local lectures and classes.

(2) The arrangement of peripatetic courses of classes and lectures, conducted by specially qualified teachers (either supplemental to local efforts, or at the sole instance and cost of the institution) for imparting instruction in science and technology in any parts of the county, particularly in rural and maritime districts. The teaching given to be either elementary or more advanced, but always, as far as possible, of a thoroughly practical character, and such as will give a knowledge of things rather than words, and develop the faculties of seeing and doing.

The most important of the subjects proposed to be taught may thus be grouped:—

(*a*) Elementary drawing, practical geometry, carpentry, modelling, &c., and their applications in the study and practice of the following subjects.

(*b*) Practical elementary physics and chemistry, and their applications in agriculture, industries, &c.

(*c*) Biology, including practical botany and the principles of vegetable physiology, and their applications in agriculture, gardening, &c.

(*d*) The principles of geology and mineralogy, and their applications in agriculture, water-supply, &c.

(*e*) Human physiology and the laws of health or hygiene.

(*f*) Geography and physiography, including practical meteorology.

(*g*) The principles and practice of agriculture and agricultural chemistry, live-stock management, fruit-growing and preserving, dairy management, &c.

(*h*) Forestry, arboriculture, and gardening.

(*i*) The structure, life-histories, diseases, distribution, &c., of fish, molluscs, crustacea, &c., with special reference to the Essex fisheries, oyster culture, &c.

(*j*) Courses of instruction on the diseases of plants and animals, and on beneficial and injurious birds, insects, injurious fungi, &c.

(*k*) Special courses of instruction on the scientific principles and practice of any local industries.

(*l*) Navigation, fishing, &c.

(*m*) Cookery and minor domestic industries.

The stock of apparatus, models, preparations, maps, specimens, &c., in the central institution would allow of the teaching in these lectures and classes being illustrated and made practical in a way that would be impossible in the case of rural centres and villages under any other system. In schemes of elementary, scientific, and technical instruction hitherto put forward, towns and populous centres have alone been considered. The present scheme would permit of the best kind of instruction being given not only in towns but also in rural and maritime districts, and that at a minimum cost. It should be noted also that if in the future an extension of the institution in any direction should be considered desirable by the County Council of Essex, the plans proposed will readily allow of such development without any interference with the work then being carried on.

The museum, laboratories, and class-room would also be serviceable:—

[1] Abstract of scheme put forward by the Council of the Essex Field Club (under the Technical Instruction Act, 1889; the Local Taxation Act, 1890; and in accordance with the regulations of the Science and Art Department).

(1) As a means of giving general scientific information and practical education in the county, and as a local centre of nstruction for Chelmsford and its neighbourhood.

(2) As a place for instruction in the higher branches of any subject for advanced pupils, and as giving opportunities for individual practical work; the laboratories and class-room would also be of general advantage as an examination centre for any science or technical classes held in the county, whether under the Club's scheme or otherwise.

(3) It is submitted also that the museum, laboratories, and library at Chelmsford will be of considerable utility to the inhabitants of the county at large, to farmers, gardeners, fishermen, &c., and to members of the County Council, county officers, and others desirous of obtaining accurate information about Essex, its natural productions and industries, and also as affording facilities for any special technical investigations in the subjects above mentioned.

The Club would become affiliated to the Science and Art Department, so that Government examinations could be held, prizes and payments on results earned, and grants claimed towards the building fund, and for the purchase of apparatus, examples, &c. This affiliation would bring the Club clearly within the terms of the Technical Instruction Act, 1889.

In the work of carrying out the above scheme, the Essex Field Club would have special facilities; it would be in fulfilment of one of the highest objects of the Club, and the Council and members would have every incentive to carry out the scheme well and energetically. The ordinary meetings, serial publications, and circulars of the Club would also aid much in making the work widely known and appreciated, and in attracting students likely to receive benefit from the teaching afforded.

The grants from the County Council would be supplemented by (*a*) local contributions; (*b*) fees from students; (*c*) grants earned from the Science and Art Department; (*d*) special aid, both in money, specimens, and assistance by the Club and its members, the scheme being really complementary to the existing work of the Club.

The management of the classes would be in the hands of a special committee or committees, appointed by the Council of the Club, not necessarily chosen from the members, which committee or committees would have control over the apparatus during the continuance of the grants, and the Council of the Club would undertake, on its part, to carry out the above stipulations also during the continuance of the grants.

The Council claims that the scheme above set forth is of a wide-reaching character, embracing the whole county and not any particular district; that it will supplement in a very useful way the work of existing educational centres; and that it is calculated to be particularly serviceable in those districts not provided for by urban educational institutions. It has been formulated under the advice of some eminent practical educators; it is in accordance with the recommendations of the National Society for the Promotion of Technical and Secondary Education, and above all, it is perfectly workable provided sufficient funds are available for the purpose.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—The following is the speech delivered by the Public Orator, *Dr. Sandys*, Fellow and Tutor of St. John's, in presenting for the complete degree of M.A. *honoris causâ* Mr. J. A. Ewing, B.Sc. (Edinburgh), F.R.S., recently elected to the Professorship of Mechanism and Applied Sciences, vacated by the resignation of Professor Stuart, M.P.:—

Dignissime domine, domine Procancellarie, et tota Academia:—

Uni e professoribus nostris, Britanniae senatoribus adscripto, nuper valediximus, cuius merita de Academiae praesertim finibus Britanniae in oppidis magnis late prorogandis, animo grato in perpetuum recordabimur. Successorem autem eius hodie salutamus, qui, vitae humanae spatio dimidio vixdum decurso, quindecim iam annos, primum solis orientis inter insulas, deinde patriae septentrionalis in litore, professoris munere egregie functus est. Interim opera eius insignia, partim machinis vapore actis explicandis, partim scientiae magneticae investigandae dedicata, non modo doctrinae Britannicae inter thesauros, sed etiam Societatis Regiae inter annales relata sunt. Quid dicam de pulcherrimo eius invento, quo terrae motus etiam levissimi accuratissime indicantur? Nonnulli certe vestrum audivistis orationem eximiam, quâ nuper, munus suum auspicatus, scientiae machinali Academiae inter studia locum vindicavit, supellectilem ampliorem ei deberi arbitratus. Croesi divitiis si forte frueremur, Archimedis scientiam apparatu amplissimo libenter ornaremus. Interim civium munificorum liberalitatem exspectantes, his studiis in hac arce doctrinae denuo instaurandis (ut Vergili utar versu) *Dividimus muros et moenia pandimus urbis.* Quod si quis hodie loci eiusdem verbis male ominatis abuti velit, *Scandit fatalis machina muros*; omen illud in melius statim convertimus, recordati ex equo Troiano viros fortes, meros principes, exstitisse. Tali igitur viro, scientiae tantae inter principes numerato, non iam manus nostras velut devicti dedimus; foedere potius novo utrimque devincti, dextram dextrae libenter iungimus. Duco ad vos Professorem Ewing.

The Council of the Senate report that, in view of the dissent of ten of the Colleges therefrom, they have resolved to proceed no further with the proposed statute for relieving distressed Colleges from the contribution to the University funds.

E. A. T. Wallis Budge, M.A. of Christ's College, the distinguished Egyptologist of the British Museum, has been approved for the degree of Doctor in Letters.

A portrait of Prof. A. Newton, F.R.S., painted by Mr. C. W. Furse, has been presented to the University by the subscribers, and will probably be hung in the New Museum.

Mr. S. J. Hickson, M.A., the author of a recent work on Celebes, has been appointed to the Lectureship in the Advanced Morphology of Invertebrates, vacant by the resignation of Prof. Weldon, F.R.S., now of University College, London.

Mr. G. F. C. Searle and Mr. S. Skinner have been appointed Demonstrators of Experimental Physics at the Cavendish Laboratory.

Dr. Anningson, University Lecturer in Medical Jurisprudence, and Medical Officer of Health for Cambridge, announces a course of lectures and demonstrations in public health, suitable for candidates for the University diploma. The course will be given in the Long Vacation.

The Annual Reports of the Fitzwilliam Museum Syndicate and of the Antiquarian Committee contain long lists of valuable gifts and acquisitions of archæological and ethnographical interest received during the past year.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, January 22.—"The Passive State of Iron and Steel. Part II." By Thos. Andrews, F.R.SS.L. and E., M.Inst.C.E.

The experiments of Series III., in this paper, relate to the effect of temperature, and the observations of Series IV. refer to the influence exerted by nitric acid, of varied concentration, on the passive condition of iron and steel.

Series III.—Effect of Temperature on the Passivity of Iron and Steel.

The bars selected for these observations were unmagnetized polished rods, which had been previously drawn cold through a wortle; a pair of bars of each metal were cut adjacently from one longer bar, and then placed securely in the wooden stand; each bar was $8\frac{1}{4}$ inches long, 0·261 diameter. The U-tube, containing $1\frac{1}{4}$ fluid ounce of nitric acid, sp. gr. 1·42, was rigidly placed in an arrangement as shown on Fig. 3 in the paper. One limb was surrounded by a tank containing water, the other limb by a tank of the same capacity containing powdered ice; the arrangement was such that the water-tank could be heated by a Bunsen burner, and its temperature slowly raised, whilst the ice-tank was kept full of powdered ice.

The bars were in circuit with the galvanometer, and soon after immersing them in the nitric acid heat was applied to the water-tank, and the temperature of the nitric acid in that limb of the U-tube slowly raised to the temperatures required, whilst the acid in the other limb of the U-tube was meanwhile maintained at a temperature of 32° F.

The arrangement will be understood on reference to Fig. 3 in the paper, and the electro-chemical results obtained are graphically recorded on diagram I.

These electro-chemical experiments indicated that the wrought iron was less passive in the warm nitric acid than the soft cast steel: the average E.M.F. of 94 observations with wrought iron was 0·030 volt; whereas, in the case of the 94 observations on cast steel, the average E.M.F. was only 0·010 volt.

It was noticed that the behaviour of the steel, under the conditions stated, was more irregular than that of the wrought iron.

In the whole of the series of experiments on diagram I. the nitric acid was raised to a temperature of 175° F.; the cold nitric acid in the limb of the U-tube A remained perfectly colourless, and the steel or iron therein absolutely passive; but the steel or iron in the warm nitric acid in tube A, commenced to be gradually acted upon as the temperature increased, a pale yellow tint beginning to appear in the solution in the tube A, shortly after commencement.

The results showed that iron or steel does not fully lose its passivity up to a temperature even of 175° F., though the passivity is shown to have been considerably modified by temperature. The critical point of temperature of transition from the passive to the active state is therefore higher than 175° F., and is shown in the experiments of Part I., Series II., Table II., to have been about 195° F.

Series IV.—The Passivity of Iron and various Steels increases with the Concentration of the Nitric Acid.

Scheurer-Kestner considered that the passivity of iron was not dependent on the greater or less degree of saturation of the acid. In connection with this aspect of the subject, the electro-chemical experiments of Series IV. indicate, however, that the property of passivity in iron is not absolutely fixed or static, but that its passivity is modified to a certain extent in relation to the strength of the nitric acid used.

A current was observed between two bright "passive" wrought iron or various steel bars of the same composition, one in cold nitric acid, sp. gr. 1·5, the other in cold nitric acid, sp. gr. 1·42; the electro-chemical position of the bar in the weaker acid was positive. The mode of experimentation was generally similar to that previously employed. The chemical composition and general physical properties of the various steels used in the experiments are given in Tables IV. and V.

The results, the average of many repeated experiments in each case, are given in Table III., and show that the passivity of iron increases considerably with the strength of the nitric acid.

The results of the experiments of Series IV., Table III., show that wrought iron was less passive in the weaker acid than most of the steels, the soft Bessemer steel being found similar in passivity to the wrought iron.

The average E.M.F. was as follows:—With wrought iron, 0·054 volt; soft cast steel, 0·028 volt; hard cast steel, 0·036 volt; soft Bessemer steel, 0·059 volt; tungsten steel, 0·039 volt.

Geological Society, January 21.—A. Geikie, F.R.S., President, in the chair.—The following communications were read:—On the age, formation, and successive drift stages of the valley of the Darent; with remarks on the Palæolithic implements of the district, and on the origin of the chalk escarpment, by Prof. Joseph Prestwich, F.R.S. i. *General Character and Age of the Darenth Valley.* The river is formed by the union of two streams, the main one flowing east from near Limpsfield, the other west from near Ightham, parallel with the ranges of Lower Greensand and Chalk, and flows northward into the Thames. The first indent of the valley was subsequent to the deposition of the Lenham sands, and, indeed, to the Red Clay with flints, and the old implement-bearing drift with which this is associated; and the same remark applies to a system of smaller valleys starting near the crest of the escarpment and running into the Thames. ii. *The Chalk Plateau Drifts and Associated Flint Implements.* Since the publication of the author's Ightham paper, Mr. Harrison and Mr. De B. Crawshay have found implements mostly of rude type (though a few are as well finished as those of Abbeville) in numerous localities on the plateau, where, owing to the gradients, the difference of level between plateau and valley-bottom is much greater than at Currie Farm. Evidence derived from the character and conditions of preservation of these implements is adduced in favour of their great antiquity. iii. *The Initial Stages of the Darent Valley.* The author has previously shown that in early Pliocene times a plain of marine denudation extended over the present Vale of Holmesdale, and that in pre-glacial times the plain was scored by streams flowing from the high central Wealden ranges. These streams centred in the Darent, and the excavation of the present valley then commenced. There is a gap in the sequence between the pre-glacial drifts and the earliest post-glacial drifts of the valley, which is probably covered by the extreme glacial epoch. It was a time of erosion, rather than of deposition in this area. Of the earliest drift of the Darent valley, little has escaped later denudation. The bank of coarse gravel on the hill on the west side of the valley between Eynsford and Farningham, certain flint-drifts in the upper part of the valley, and a breccia of chalk-fragments on the hill west of Shoreham, may be referred to this period. iv. *The High-Level or Limpsfield Gravel Stage.* The gravel at Limpsfield occurs on the watershed between the Darent valley and the Oxsted stream, but the author agrees with Mr. Topley that the gravel belongs to the Darent system, and Westheath Hill may be part of the original ridge separating the two valleys. This gravel is post-glacial, and the denudation of the area had made considerable progress at the time of its formation, for the chalk escarpment rises 200–300 feet, and the Lower Greensand 100–200 feet above the gravel-bed. The author traces outliers of this gravel down the valley at lower and lower points to the Thames valley at Dartford, and correlates it, not with the high plateau-gravel, but with the high-level gravel of the Thames valley, and shows that its composition indicates that it is derived from the denudation of the Chalk and Tertiary beds. Mr. A. M. Bell has discovered numerous implements in it, mostly of the smaller St. Acheul type, and the author hopes that they will soon be described by their discoverer. These implements agree in general type with the "Hill group" of the Shode valley, and not with the older group of the Chalk plateau, or those of the lower levels of the Thames and Medway. v. *Contemporaneous Drift of the Cray Valley.* Implements of this age have been found by Mr. Crawshay and by Mr. P. Norman, near Green Street Green, in gravel which is more than 100 feet below the Red Clay of the plateau. vi. *Brick-earths of the Darent Valley.* These are traced along the upper course of the valley from near Limpsfield. They seem to show glacial influence, and Mr. Bell has discovered a few implements in them. The Limpsfield deposit is from 10 to 30 feet below the adjacent gravel. Brick-earth, possibly of somewhat later date, also occurs near Dartford. vii. *Other Gravels of the Darent Valley: the Chevening and Dunton Green Drifts.* The relations of the gravels grouped under this head are more uncertain than those of the Limpsfield stage. Various features in the gravels point to the temporary return of glacial conditions during the period of formation of these and the brick-earths; and these are described in detail. viii. *The Low-level Valley-Gravels.* The correlation of these is also uncertain. West of Dartford is a bed corresponding with that at Erith, in which Mr. Spurrell found a Palæolithic floor. It contains land and fresh-water shells. The surface of the Chalk is here festooned under a covering of the fluviatile drift. The author attributes this festooning to the effects of cold. ix. *The Rubble on the Sides and in the Bed of the Valley.* The author describes this rubble, and rejects the view that it is rain-wash or due to sub-aërial action, and discusses the possibility of its having been produced by ice-action. x. *Alluvium and Neolithic Implements.* These occur chiefly between Shoreham and Riverhead. xi. *On the Chalk Escarpment within the Darent District.* The author, after discussing and dismissing the view that the escarpment was formed by marine denudation, criticizes the theory that it was due to ordinary sub-aërial denudation, and lays stress on the irregular distribution and diversity of the drift-beds in the Darent area; these do not possess the characters which we should expect if they were formed by the material left during the recession of the Chalk escarpment owing to sub-aërial action; and he believes that glacial agency was the great motor in developing the valleys, and, as a consequence, the escarpment, and that the denudation was afterwards further carried on in the same lines by strong river-action and weathering, though supplemented at times by renewed ice-action. By such agencies, aided by the influence of rainfall and the issue of powerful springs, he considers that the escarpment was gradually pared back and brought into its present prominent relief. After the reading of this paper there was a discussion, in which Mr. Topley, Dr. Le Neve Foster, Mr. De B. Crawshay, the President, and the author took part.—On *Agrosaurus Macgillivrayi*, Seeley, a Saurischian reptile from the north-east coast of Australia, by Prof. H. G. Seeley, F.R.S.—On *Saurodesmus Robertsoni*, a Crocodilian reptile from the Rhætic of Linksfield, in Elgin, by Prof. H. G. Seeley, F.R.S.

Royal Microscopical Society, January 21.—Annual Meeting.—Dr. C. T. Hudson, F.R.S., President, in the chair.—Mr. Swift exhibited and described a new form of petrological microscope which he had made under the instructions of Mr. Allen Dick. It differed from the ordinary patterns in having no revolving stage, but was so constructed that whilst the object remained fixed the eye-piece and the tube below the stage could be revolved.—Mr. E. M. Nelson exhibited a new apochromatic condenser, by Powell and Lealand, which gave a larger aplanatic solid cone than it had hitherto been found possible to obtain.—The report of the Council was read, showing an increase in the number of Fellows and in the revenue of the Society.—Dr. C. T. Hudson delivered his annual address.—Dr. R. Braithwaite was elected President for the ensuing year.

PARIS.

Academy of Sciences, February 2.—M. Duchartre in the chair.—The death of General Ibañez on the 29th ult. was announced. An account of his life and works was given by M. J. Bertrand.—On the approximate development of perturbing functions, by M. H. Poincaré.—The photography of colours, by M. G. Lippmann. The conditions said to be essential to photography in colours by M. Lippmann's method are: (1) a sensitive film showing no grain; (2) a reflecting surface at the back of this film. Albumen, collodion, and gelatine films sensitized with iodide or bromide of silver, and devoid of grain when microscopically examined, have been employed. Films so prepared have been placed in a hollow dark slide containing mercury. The mercury thus forms a reflecting layer in contact with the sensitive film. The exposure, development, and fixing of the film is done in the ordinary manner; but when the operations are completed, the colours of the spectrum become visible. The theory of the experiment is very simple. The incident light interferes with the light reflected by the mercury; consequently, a series of fringes are formed in the sensitive film, and silver is deposited at places of maximum luminosity of these fringes. The thickness of the film is divided according to the deposits of silver into laminæ whose thicknesses are equal to the interval separating two maxima of light in the fringes—that is, half the wave-length of the incident light. These laminæ of metallic silver, formed at regular distances from the surface of the film, give rise to the colours seen when the plate is developed and dried. Evidence of this is found in the fact that the proofs obtained are positive when viewed by reflected, and negative when viewed by transmitted, light—that is, each colour is represented by its complementary colour.—Observations by M. E. Becquerel on the above communication. M. Becquerel called attention to the experiments made by him on the photography of colours in 1849. His researches, however, dealt more with the chemical than the physical side of the question.—General Derrécagaix read a memoir on a table of centesimal logarithms to eight decimal places, issued by the Service Géographique.—M. Faye presented the *Connaissance des Temps* for 1892 and 1893, and *L'Annuaire du Bureau des Longitudes* for 1891, and described the additions to the latter.—On the distribution in latitude of solar phenomena observed at the Royal Observatory of the Roman College during the latter half of 1890, by M. P. Tacchini. The results, in conjunction with those previously presented, show that in 1890, as in 1889, the prominences were more frequent in the southern hemisphere of the sun than in the northern hemisphere. The maximum frequency occurred in the zone $-40°$ to $-50°$. Faculæ and spots have been most frequent in the northern hemisphere.—Remarks on the displacement of a figure of invariable form in all the planes that pass through some fixed points, by M. A. Mannheim.—Complementary note on the characteristic equation of gases and vapours, by M. C. Antoine.—On the basicity of organic acids according to their electrical conductivity; monobasic and bibasic acids, by M. Daniel Berthelot.—On the reactions of the oxyalkyl derivatives of dimethylaniline, by M. E. Grimaux. A new class of colouring-matters is described.—On the composition and properties of *levosine*, a new substance obtained from cereals, by M. C. Tanret.—On the quantity of oxygen contained in the blood of animals living on elevated regions in South America, by M. Viault. The results indicate that the proportion of oxygen contained in the blood of men and animals (indigenous or acclimatized) living in the rarefied air of mountainous regions, is sensibly the same as that which is contained in the blood of men and animals living at lower levels.—On the amount of hæmoglobin in the blood according to the conditions of existence, by M. A. Müntz. The author, like M. Viault, finds that animals living at great altitudes—that is, in a medium where the pressure of oxygen is low—have the proportion of hæmoglobin in the blood increased, and it consequently acquires an absorbing power for oxygen which compensates the effect of rarefaction. It is also concluded that altitude is not necessary to produce these modifications, and that the same results may be obtained if, instead of diminishing the amount of oxygen, the quantity of combustible matter is increased.—On the larvæ of *Astellium spongiforme* and on the *Pœcilogonia* of *Ascidiæ*, by M. A. Giard.—On the anatomy of *Corambe testudinaria*, by M. H. Fischer. A new species of mollusk is described. The author notes: "J'ai recontré dans l'oreillette et dans les muscles de la radule des fibres striées transversalement." This confirms the idea that the striation of muscles is intimately connected with the mechanism of their contraction.—On the *Acridium peregrinum* from the extreme south of Algeria, by M. J. Kunckel d'Herculais.—On the influence of the nature of soils on vegetation, by M. G. Raulin.—On the respiration of cells in the interior of masses of tissue, by M. H. Devaux.—Influence of the hygrometric state of the air on the position and functions of the leaves of mosses, by M. E. Bastit.—On the siliceous clays of the Paris basin, by M. A. de Lapparent. The formation of *glaçons-gâteaux*, by M. F. A. Forel. These ice formations are said by the author to be "les *pan-cakes* des Anglais."—Remarks on the temperature of Marseilles, by M. J. Léotard.

STOCKHOLM.

Royal Academy of Sciences, January 14.—Further notices on the molecular weight of the earth (oxide) of gadolinite, by Baron A. E. Nordenskiöld.—On the structure of the transversely striated muscular fibre, by Prof. G. Retzius.—The altitude of the clouds measured in the mountains of Jemtland during the summer of 1887, by Messrs. Hagström and Falk.—Photographs of the spectrum of iron taken with the aid of the Voltaic arc, and one of the "gitters" of Rowland, exhibited by Prof. Hasselberg.—On dinitro-diphenyl-disulphine, i., by Dr. A. Ekbom.—A short relation of a zoological tour to North Greenland during last summer, by Dr. D. Bergendæl.—Some observations on the water of the Gullmar fiord, by Dr. A. Stuxberg.

CONTENTS.

PAGE