670 km. and 2000 km., the mean diameter being $1''.9$, or about 1400 km. Figs. 2 and 3 illustrate the type of photograph on which the measures were made. These were taken with an interval of twenty-five seconds, and even a cursory glance will show that during that time the arrangement and forms of the granules have altered considerably; on this scale the diameter of the sun would be about 3 metres.

It seems probable that these researches will lead to most important conclusions concerning the nature and the periodicity of the changes in the granules themselves, their influence on the solar spots and faculæ, and, in general, to the resolution of many outstanding problems concerning the nature and action of the photosphere, which are at present unsolved. Prof. Hansky intends, therefore, to pursue this line of research, but, as he points out, it will only be during the comparatively rare moments of atmospheric calm, and with such a powerful equipment as he now possesses, that fruitful results are likely to be obtained. In any case, he is to be warmly congratulated upon those he has already published. W. E. ROLSTON.

PRACTICAL SCIENCE FOR SCHOOLS.[1]

PROF. PERRY said that in the early days of the society, when he had the honour of acting as a secretary, and when Guthrie and Foster, Kelvin and Fitzgerald were presidents, no presidential addresses were delivered, and he questioned whether we were not overdoing the business of requiring general addresses, which must almost always have as their theme the progress of science. Seldom did we find in such addresses new accounts of important original work, and he felt the inappropriateness of such an address in speaking before a society the Proceedings of which were more intense with original work of the best kind than any other society known to him with the exception of the Royal Society. He thought that every young reader of a paper before a scientific society made the mistake of assuming that his audience knew a great deal of the subject so familiar to himself, and hence his paper was not understood. Writers of books on physics assume their readers to be all truly logical students; they use words properly in a technical sense, and forget that many of their readers may use them in the newspaper writer's sense. For example, take the expression "adiabatic expansion." There are people who insist on finding that Rankine, Maxwell, and all others of our most exact writers are not only inconsistent with one another in the use of the expression, but that each is inconsistent with himself. If a portion of fluid expands slowly without gain or loss of heat, we know the way in which its p, v, and t alter as it changes state; this was originally called "adiabatic expansion," and the term has become a technical term for that kind of alteration of p, v, and t, however it may occur. Steam or air may be throttled through a non-conducting reducing valve, but the expansion is not adiabatic, although there is no gain or loss of heat. Steam or air passing along a pipe with friction, if it can only be made to lose heat through the metal of the pipe at exactly the proper rate at every place, is expanding adiabatically. When it is assumed that steam or air flows without friction from a vessel through an orifice, it is said that the expansion is adiabatic although it is rapid.

Referring to the teaching of physics to students entering upon the engineering profession, the president remarked that such teaching was nearly always slipshod. Many men enter a science college at the age of eighteen or more, knowing nothing of physical science. In the case of a great percentage of such men, it is impossible that they should acquire the scientific habit of thought. It is because so much of this kind of material is dealt with that much of our teaching is slipshod. Every pupil entering a science college ought to have been experimenting and working graphically and numerically on physical science problems from a very early age, and then our science classes would deal with them in a scientific way. The causes of the unfitness of the average student are two: one that his instincts and habits of thought were not trained from early youth; the other that his teachers in science colleges have absurd and uninteresting courses of study for him. In physics we are dealing with ideas which are not familiar to young students, ideas which can only become familiar in the laboratory. For example, such a simple mathematical idea as that of a decimal cannot be given in elementary schools in less than five or six years, whereas one week of weighing and measuring would give young children familiarity and clear ideas about decimals. Numerous examples could be given to prove that the principles of physics cannot be understood unless there has been early experimental training, and this is the reason why the professors of science in colleges of university rank and the professors in technical colleges obtain such poor reward for their labour. Referring to the many hundreds who every year take science degrees at the universities, and the thousands who pass the London University matriculation examination, Prof. Perry remarked that if that was the standard of excellence of those present, his address could serve no useful purpose. Nothing ought to be compulsory in schools except the study of English and of natural science. The object of a matriculation examination is to test whether a student entering a college will be able to benefit by the course of study there. The only language which ought to be compulsory in the science department of a university is English. A professor of science ought to be allowed to teach his students in the way that seems best to him, and he should examine his students himself. Hedge him round with rules and regulations framed by boards of studies; tie him down to a syllabus, and the work he will do might be much better, certainly much more cheaply, done by a grinder at low wages. There is no one general elementary course in physics which all students ought to take; neither by their previous training nor from the uses which they will make of the principles of physics are they fit to be taught together. What is wanted is more classes, more rooms, and more teachers.

[1] Abstract of the presidential address delivered before the Physical Society on February 9, by Prof. J. Perry, F.R.S.

THE NEW ORLEANS MEETING OF THE AMERICAN ASSOCIATION.

THE New Orleans meeting of the American Association for the Advancement of Science, as stated in our issue for January 25 (p. 303), began on December 29, 1905, and continued for five days. At a meeting of the general committee it was decided to hold a special summer meeting at Ithaca, New York, to close on or before July 3, 1906, and an ordinary winter meeting in New York City to begin on December 27, 1906. The presidential and vice-presidential addresses will be omitted at the summer meeting and given at the winter meeting. The officers elected at the New Orleans meeting will, therefore, hold office until the close of the New York meeting. Chicago was recommended as the place of the winter meeting of 1907.

The following officers were elected for the Ithaca and New York meetings:—President: Dr. W. H. Welch, Baltimore, Md. Presidents of sections: A, Dr. Edward Kasner, New York City; B, Prof. W. C. Sabine, Cambridge, Mass.; C, Mr. Clifford Richardson, New York City; D, Mr. W. R. Warner, Cleveland, O.; E, Prof. A. C. Lane, Lansing, Mich.; F, Prof. E. G. Conklin, Philadelphia, Pa.; G, Dr. D. T. MacDougall, Washingtc D.C.; H, Prof. Hugo Münsterberg, Cambridge, Mass. I, Mr. Chas. A. Conant, New York City; K, Dr. Simo Flexner, New York City. General secretary: Mr. John F. Hayford, Washington, D.C. Secretary of council: President F. W. McNair, Houghton, Mich.

The following resolutions were adopted by the association:—(1) That the association instructs its president and secretary to communicate to the president of the Senate and to the speaker of the House of Representatives of the United States its strong conviction that Niagara Falls should be preserved as a natural wonder, and further expressing the earnest hope that the congress now in session will take prompt and energetic action looking toward an international consideration of the impending danger to Niagara Falls. (2) *An Appalachian Forest Reserve.*—That the association again respectfully calls attention to the rapid rate at which the forests of the

Appalachian Mountain region are being destroyed, and to the fact that, as a result of such destruction, the streams tributary to the Mississippi, as well as those flowing into the South Atlantic, are becoming continuously more irregular in their flow, and hence of less value for navigation and power purposes. (3) That the association respectfully petitions the Congress of the United States to make such provision as may be necessary for the protection of these mountain forests.

The following grants were made at this meeting of the association:—40*l.* to Messrs. Parsons, Kinnicutt, and Venable to assist in the publication of Prof. Parsons's "Bibliography of Beryllium," and 20*l.* to "The Concilium Bibliographicum Zoologicum."

The committee on electrochemistry reported as follows:—A study has been made of the behaviour of platinum and iridium in chlorine water and in dilute hydrochloric acid. Smooth platinum foil brought about no evolution of gas even after standing 168 hours in chlorine water. In precisely similar circumstances an iridium foil caused an evolution of 44·4 [? c.c.] of gas, 55 per cent. of which was oxygen. The oxygen results from the reaction

$$Cl_2+H_2O=2HCl+O,$$

while the chlorine came from the solution, the original vapour pressure having been about half an atmosphere. This series of experiments showed that iridium was a more powerful catalytic agent than platinum. A number of electrolytic experiments were made with hydrochloric acid of different concentrations. In all cases more oxygen was evolved from the iridium anode than from the platinum anode. The question as to the final equilibrium is still in doubt.

The Popular Conception of the Scientific Man at the Present Day.[1]

The traditional scientific man has disappeared almost as completely as the traditional Yankee of the stage. The change came gradually, but the proof that it had come was brought before us suddenly. In 1902 there was called in New York a meeting of those who were designated by the picturesque expression captains of industry. To that meeting representatives of science were invited, not as lions to be stared at, but to sit with the leaders of the industrial and commercial world as representatives of science, and not only of applied science, but of pure science. As the captains of industry were supposed to be men of force in organising and to have a keen insight into men and things, we had a right to feel that science was honoured, perhaps not more than ever before, but for a reason for which it had not been honoured before in the United States. The fact that since that date the reputation of some of the captains of industry has suffered an eclipse does not alter the fact that to be considered a captain of industry was, in the eyes of the public, enviable. The conception of a scientific man as a captain of industry means simply the acknowledgment that science has a practical relation to the world, and that fortunately the public have advanced far enough to see, although perhaps somewhat dimly, that pure science sooner or later develops into applied science. The leaders of science are to be placed in the class of organisers, managers of a sort of scientific trust. This is science up to date, and the public are right when they regard science as an organisation. But they are only partly right. There is a good deal more than that in science, and, although good managers and directors are necessary, it is true that the power of organising and the power of investigating are two different things, and often exist in inverse ratio to each other, and it is the latter which is at the basis of science. An organiser is of no use until there is something to organise, and the materials on which the organiser in science must work are not made by machinery, but by the brains of individual workers, and it is important that they should be placed under the most favourable conditions for work. If hitherto there has been perhaps too little organisation, there is a danger that in the future there may be too much. In a mill many men are doing the same kind of work, but in science one man should not duplicate the work of another. The object of organisation in the one case is to secure uniformity of product, in the other to encourage diversity of work.

[1] From the address of Prof. W. G. Farlow, president of the Association.

The ways in which the public may aid scientific men are directly by endowments for paying salaries and indirectly by providing properly equipped laboratories and other necessary equipment, and especially for paying for the services of assistants. Both forms of help are necessary, for a man capable of managing and getting the greatest amount of good work out of a well equipped establishment deserves more than a meagre salary. On the other hand, those with what appears to be a respectable salary may have to spend a good part of it to make good the deficiencies in their equipment. In deciding whether a man is well paid or not, it is necessary to ask not only what salary he receives, but what are the means of work provided for him. It is not my intention here to direct attention to the special ways in which scientific establishments would be benefited by gifts from the public nor to discuss the question what is a proper salary for a scientific man. The latter depends upon too many complicated conditions, and cannot be separated from the more general question of what those in equally important positions in other walks of life are paid. The question of proper equipment, including the question of assistants, has already been brought before the public on a good many occasions and in a good many ways, and a good deal has been given in recent years, although by no means enough.

If, as it appears, the public have reached a better conception of the position of the scientific man and of his pecuniary needs, it may be added that he has the right to hope that he can appeal to the public not only for pecuniary, but for moral support, for, in many cases, the public are the final arbiters where differences arise, and unfavourable conditions often disappear quickly as soon as it is felt that one side or the other is backed by public opinion. It may, therefore, be well to state somewhat explicitly some of the conditions which are unfavourable to the progress of science in the United States or which tend to retard it. Here it is not so much a question of money as of a just appreciation of the true position of scientific men in their relation to those for whom their work is undertaken. That work, using a rough classification, may be considered as that done in technical and commercial concerns, that done for the Government, and that done in universities, including under that general term all colleges, scientific schools, and similar institutions which have a permanent endowment of some kind.

The Relation of Mechanics to Physics.[1]

We find the physicists of the beginning of the nineteenth century still very strongly attached to the idea that all natural phenomena not only may, but must, be explained on the basis of Newton's laws[2] by central forces acting instantaneously at a distance. Newton's mechanics had done such admirable service in astronomy that it had come to be regarded as the only possible means of describing and discussing the actions of nature. The gradual abandonment of this position and the change to the modern view, according to which all actions in nature are transmitted through a continuous medium and require time for their transmission, was accomplished only after a long struggle that occupied the greater part of the nineteenth century.

It is well known how the ideas of Faraday, of Maxwell, of Hertz, gradually gained ascendency over the older views and led to the abandonment of the idea of central forces acting instantaneously at a distance, in almost all branches of physics except in the theory of gravitation. It is also known that Maxwell, by a brilliant analysis, succeeded in establishing the connection between his electromagnetic theory and the analytical mechanics of Lagrange. Thus, at the end of the nineteenth century we find a general attitude toward physical phenomena essentially different from that prevailing at the end of the eighteenth century.

With the rise of the electron theory in the course of the last twenty-five years a new element has been introduced

[1] From the address of Prof. Alexander Ziwet, president of Section A, Mathematics and Astronomy.

[2] See, however, Laplace, "Mécanique Céleste," livre i., chap. vi. ("Œuvres," vol. i., 1878, pp. 74-79), a passage to which E. and F. Cosserat have recently directed attention.

into this development, an element which seems destined to affect very radically, not only our interpretation of physical phenomena, but also our general views about the principles of theoretical mechanics.

There seem to be two things underlying all the phenomena in the physical world—the ether and matter. To attain the unification of physical science, shall we consider the ether as a particular kind of matter? Or shall matter be interpreted electromagnetically? The older mechanics dealt exclusively with matter; and when it first became necessary to introduce the ether, this new medium was often endowed with properties very much like those of matter. The hydrodynamic analogy by which the apparent mass of the moving charge was interpreted illustrates this tendency. The physics of the ether has, however, reached so full a development that the properties of the ether are now known far more definitely than those of matter. These properties are contained implicitly in the fundamental equations of Maxwell and Hertz which in their essential features are adopted in the electron theory of Lorentz.

It is now pretty generally recognised that Newton's "laws of motion," including his definition of "force," are not unalterable laws of thought, but merely arbitrary postulates assumed for the purpose of interpreting natural phenomena in the most simple and adequate manner. Unfortunately, nature is not very simple. "As the eye of the night-owl is to the light of the sun, so is our mind to the most common phenomena of nature," says Aristotle. And if since Newton's time we have made some progress in the knowledge of physics, it is but reasonable to conclude that the postulates which appeared most simple and adequate two hundred years ago cannot be regarded as such at the present time.

This does not mean, of course, that the mechanics of Newton has lost its value. The case is somewhat parallel to that of the postulates of geometry. Just as the abandonment of one or the other of the postulates of Euclidean geometry leads to a more general geometry which contains the old geometry as a particular, or limiting, case, so the abandonment or generalisation of some of the postulates of the older mechanics must lead to a more general mechanics. The creation of such a generalised mechanics is a task for the immediate future. It is perhaps too early to say at present what form this new non-Newtonian mechanics will ultimately assume. Generalisation is always possible in a variety of ways. In the present case, the object should be to arrive at a mechanics, on the one hand sufficiently general for the electron theory, on the other such as to include the Newtonian mechanics as a special case.

After the searching criticism to which Poincaré, especially in his St. Louis address,[1] in 1904, has subjected the foundations of mechanics and mathematical physics, almost the only one of the fundamental principles that appears to remain intact is the principle of least action. It seems, therefore, natural to take this principle as the starting point for a common foundation of mathematical physics and of a generalised mechanics, but with a broader definition of "action," or what amounts to the same, with a generalised conception of "mass" so as to make the latter a function of the velocity.

The Partition of Energy.[2]

The general theorem which I wish to discuss may be stated by saying that the kinetic energy of the body is so distributed among the degrees of freedom, by which the state of the body as a dynamical system is described, that an equal share is, on the average, allotted to each degree of freedom of each type of molecule.

The questions which have always been raised about this important theorem of the kinetic theory at once come to our minds. First, is the theorem true, or rather, does it state what would be true for an ideal system of particles moving freely within a containing vessel? second, is the proof of the theorem impeccable? third, is there any experimental evidence that it applies to real bodies?

[1] "Bulletin des sciences mathématiques" (2). 28, pp. 302–324; English translation in the *Bulletin of the American Mathematical Society*, vol. xiii., February, 1906.

[2] From the address of Prof. W. F. Magie, president of Section B, Physics.

I would remark about the first question that the theorem is so distinguished by its simplicity, and by its aspect as a sort of unifying principle in nature, that few men can set it fairly before their minds without at least desiring to believe it true. Most of those who have recognised that Maxwell's original demonstration was not flawless are still convinced of the truth of his conclusion, or at least believe his conclusion to be so probable as to make it worth while to try for a more accurate demonstration. Their state of mind is like that of Clausius and of Lord Kelvin, when they perceived that Carnot's theorem respecting the efficiency of a reversible engine could not be proved in the way in which Carnot tried to prove it.

With respect to the second question, it was very soon pointed out that Maxwell had made in his proof an assumption that could not be justified by immediate inspection, and which was itself in need of demonstration or of avoidance. The later demonstrations of Maxwell and Boltzmann have been likewise subjected to criticism, and can be shown to involve assumptions that will not be granted on inspection. The difficulties that arise in these proofs come from the necessity of applying in them the calculus of probabilities, and centre around the question of the legitimacy of the application of that calculus. It is commonly agreed that Maxwell and Boltzmann have assumed a condition of the system of moving particles, as a requisite for the application of the calculus of probabilities, which is contradicted by many systems of which we have certain knowledge, and cannot without proof be admitted as likely to obtain in other systems, about which less is known. In the method employed by Jeans the application of the calculus of probabilities is made in a different manner, and does not necessitate the introduction of the hypothesis of Maxwell and Boltzmann. It seems to me that, in this last form of the theory, the difficulties which have environed the subject have at last been mastered.

In respect to the third question, that concerning the experimental evidence for the truth of the theorem, it is well known that, in general, Boyle's law follows as a consequence of the general principles of the kinetic theory, that Gay-Lussac's law is an immediate consequence of a relation plausibly assumed between temperature and the kinetic energy of the molecule, that the motion of the radiometer and the laws of transpiration and many other properties of gases can be deduced from the general theory, and, in particular, that Avogadro's law follows from the simplest form of the theorem of equipartition. But further proof of this theorem in its general form is still needed. Such proof as we have will be discussed in this address.

Considering the bearing of the relations that have been adduced upon the general question of the equipartition of energy, it seems to me that their general consistency with that principle, especially the way in which the heat capacities of the organic compounds can be portioned out among the atoms by means of simple assumptions about their degrees of freedom, does afford some confirmation of the principle. Mere chance can hardly account for so large a number of successful coincidences.

The Sanitary Value of a Water Analysis.[1]

Though much valuable information can be obtained from the careful study of the nitrogen content of a water, the water analyst does not depend alone upon these factors in forming an opinion as to the source of the organic matter, and turns to other chemical as well as to bacterial data to substantiate or modify the opinion thus formed. From the chemical point of view the most important of these data is the combined chlorine that a water contains. This is due to the fact that though chloride of sodium occurs in rain-water, especially near the sea, and in small amounts is found in all soils, it is a characteristic constituent of sewage, the animal body expelling the same amount of salt as it absorbs.

A careful study of the amount of combined chlorine in normal waters, made by Prof. Thomas M. Drown, showed that in Massachusetts, where salt-bearing strata do not occur, the amount of chlorine in a surface water depended on its distance from the sea, and that for Massachusetts

[1] From the address of Prof. Leonhard P. Kinnicutt, president of Section C, Chemistry.

it was possible to establish normal chlorine, or, as they are commonly called, iso-chlor lines.

The work begun by Prof. Drown has been carried on by other investigators, and to-day the iso-chlor lines for all the New England States and New York and New Jersey have been determined. The result of this work is that the amount of chlorine occurring in the surface waters of the above named States gives most valuable information. Chlorine above the normal of the region shows pollution. It does not indicate whether the pollution is direct or indirect, but does show that sewage, from which the organic matter and the germs of disease may or may not have been removed by filtration through soil, has had access to the water. Chlorine above the normal is, therefore, always a suspicious sign which must be investigated. I know that it is claimed that in many of the western States, owing to geological conditions, very little information can be obtained from the determination of chlorine. I believe, however, more careful and thorough work is necessary to prove that such is the case, and that further investigation may show that though it is impossible to construct iso chlor lines running through the State, the normal chlorine of different localities in a State can often be determined.

Another factor that is often used in the attempt to decide whether or not a water contains an excessive amount of organic matter is the oxygen consumed. The oxygen consumed is not, however, a measure of the organic matter in a water, but only a measure of the amount of mineral reducing salts plus a certain amount of the organic matter, the amount depending on the method of determination used. It gives, in my opinion, very little information as to the character of the organic matter, and is only valuable when different surface waters are to be compared with each other, or when used in filtration experiments.

The same may be said as regards colour, turbidity, and the amount of mineral matter that a surface water contains, that, though of essential importance in deciding on the value of a normal water as a potable water, they give little information as to pollution.

In the early days of bacteriology it was claimed that the final criterion as to pollution of a water would be furnished by aid of that science, and though this hope has not been fulfilled, the information that can be gained by a bacterial analysis is often of the highest importance. It not only aids in the interpretation of the chemical data, but may of itself show, almost without question, that a given water is polluted, for though attempts to isolate special pathogenic germs have generally failed, even in waters known to contain these forms, characteristic sewage forms, like the colon bacillus, can be isolated if they occur in any number in a water. Occurrence of numerous characteristic sewage bacteria can point only to one thing, pollution, and if such forms are found there is no question that the water receives sewage drainage. Bacteriology, however, cannot determine, except very roughly, the amount of pollution or the present condition of the polluting matter, nor does it give but very little, if any, information as to past pollution. If the pollution is recent and of any considerable amount, a careful bacterial examination will show the fact, and probably better and more convincingly than any chemical analysis. If the pollution is more remote, more information can, as a rule, be drawn from chemical than from bacterial data. If the polluting matter has filtered through the soil before entering the water, bacterial work will not indicate the fact.

As a general statement, it may be said that a bacterial analysis, while giving information as regards recent and continuous pollution, gives no information as to the past history of a water, and in this respect differs from a sanitary chemical analysis.

To form a judgment as to the wholesomeness of a water, the data of a sanitary water analysis, the source of the water, whether surface, ground, or artesian, must be known; a survey, even of a surface water, though it may show whether or not the water is polluted, does not give information regarding the amount or condition of the polluting matter; with ground and artesian waters it often gives very little information, and an opinion regarding the character of such waters must, as a rule, depend on the sanitary analysis.

The Generic Concept in the Classification of the Flowering Plants.[1]

The difficulties of classifying plants in a really natural and logical way are somewhat increased by the involuntary and well-nigh necessary admission of a certain historic element into our systems. There is another source of this artificiality, besides the temptation to allow poor genera to stand, on the ground of long usage. The relation of a genus to its name is a matter which exerts no small influence in this regard. The attempt to determine which of several names is to be retained for a given genus constantly forces us to consider the historic basis on which the genus rests, and to attach its name to some species or group of species to which it was first applied, to determine, in other words, what was the type of the genus, and to maintain the genus in such a way that it may always be true to its type. While sympathising to a considerable extent with those botanists who desire to place our nomenclature upon a more secure basis by attaching the names to recognised types, I feel that the methods employed will have to be very cautiously applied, or they will tend greatly to increase the artificial element in our system. The historic type is not a natural thing; it is merely that particular form of plant life which was, often quite by accident, first discovered and, therefore, first received the name which it bears. Later discoveries often show that this first species of a genus is by no means of a typical, or, as one may say, central character. It is often quite peripheral, perhaps even an aberrant or outlying member of the group to which it belongs. However important the historic type may be in nomenclature, it is obvious that it is of no particular significance in classification, and any employment of the type method in the determination of proper names must not on any account be permitted to exercise any influence in classification. The word type itself is decidedly unfortunate as thus applied to what is often very far from being typical. In this as in some other phases of taxonomy it is of the greatest importance to keep it clearly in mind that nomenclature, although very necessary to classification, is a thing wholly apart from the classification itself. It is, furthermore, quite evident that nomenclature should be subservient to classification, and that the clearness and accuracy of classification should never be sacrificed in order to give beauty or symmetry to any system of nomenclature.

The limitation of genera has always in the past rested on individual judgment, and it must continue to do so in the future. Although the genera of the flowering plants have now been scientifically studied for about two centuries, there is at present in America, at least, a degree of diversity in their interpretation which is rather discouraging. It is disheartening because it is impossible to see in it any real progress toward a well rounded and satisfying system, which will win the confidence of the professional botanist, give uniform training to the student, and command the respect of our colleagues in other branches of science. From this, I think that it is perfectly clear that botanical systematists have certain imperative duties in regard to this subject. These duties are, in the first place, great caution in making changes, and, in the second place, a feeling of obligation, when these changes seem necessary, to state the reasons for them so clearly and forcibly that they will appeal to all thoughtful and discriminating workers in the same field. The burden of proof should always rest upon the writer suggesting the change.

What we need in botanical classification is a series of constants, a number of graded categories which can be generally endorsed and properly respected. Standards as definite as those of the physicist are, of course, quite unattainable in dealing with the variable and often intergrading groups of organic creation. But where absolute accuracy and uniformity are impossible, we should the more diligently seek to preserve such standards as exist. As has been pointed out, there are but few families of flowering plants which have not been comprehensively treated by monographers who, so far as their particular group was concerned, have been in a position to see pretty

[1] From the address o. Prof. B. L. Robinson, president of Section G, Botany.

clearly where it was best to draw generic lines. While it must be admitted that there are many minor differences in the generic concepts exhibited in the scholarly and monumental works to which I here refer, yet they establish a good usage, which on the whole has a considerable measure of uniformity, and goes far to establish the rank of such categories as genus, species, and variety.

Let me urge that, while we remit no effort to secure further light on this subject, there should be a general agreement to treat the accepted and traditional interpretation of large and important genera as sacred and binding until we can furnish definite and convincing evidence that change is needful, and that for the welfare and dignity of our science all should unite in opposing changes of the artificial sort, which consist merely in the shifting of ranks and modification of standards.

Investigations and Commercial Tests in Connection with the Work of an Engineering College.[1]

In any school it is necessary, in securing the best efficiency in instruction, that the professors shall be able to speak with authority on the subjects which they teach. In technical schools those who teach the practical engineering subjects cannot speak with authority unless they have had practical experience. Investigations and commercial tests may serve to give them this practical experience, and the question naturally arises, Is it a good policy for professors to conduct such work in connection with their regular college duties?

Let us consider the various ways in which a professor in an engineering school may acquire the practical experience which is necessary in his work.

First, he may be called to a professorship from the practical field.

Second, after teaching for a time and finding how necessary a practical experience is in his work, he may turn to the practical field, and then return to teaching.

Third, he may undertake practical work in connection with his college duties, and gain his experience in this way.

Each method possesses its own advantages and disadvantages. Starting with the first, it must be admitted that many of our best instructors have entered the teaching line after they have had experience in the practical field. Such a man has an advantage in being able to make use of this experience immediately, when he starts at his teaching work. There is a disadvantage, however, in the fact that should he have secured a mature experience in the practical field, he will necessarily be no longer a young man, and it may be hard for him to teach and properly to adapt himself to the theoretical part of his course.

The advantages of the second system of securing a practical experience, where the professor leaves the teaching field, takes up outside work, and then returns to teaching, are that during his practical career he will be very much alive to the points he should look into, and, furthermore, if he returns to teaching he will possess the advantage of having experience both as a teacher and as a practical engineer.

We will now take up the third method, where a professor obtains his practical experience by conducting outside work in connection with his college duties. The outside work undertaken by a professor should be that of a scientific or strictly engineering type.

The advantage to a college in having its professors do research and outside work is that what reflects to the credit of the professor will reflect to the credit of the college. Furthermore, the college will be looked to as a source from which an unbiased opinion can be obtained, and in maintaining this standard it will be fulfilling a high and useful mission. The results of the investigations may be made the subjects of scientific papers to be read before the various societies, and any reputation that a professor gains in this way will benefit his college.

The day is past when there can be a strict line drawn between the work of the consulting engineer and that of the professor who teaches in the same field. The ideal professor in a given line should be able to take up the work of the consulting engineer in that line, and the ideal consulting engineer should possess enough technical knowledge to fit him for being a professor. There should be no jealousy, but rather a bond of friendship in that the fundamentals which each should master are the same.

[1] From the address of Prof. D. S. Jacobus, president of Section D, Mechanical Science and Engineering.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

OXFORD.—The results of a census undertaken each year by the *Magazine* show that there are 2722 undergraduates actually in residence this term, as compared with 2621 in Hilary term, 1905. The increase is probably due to the Rhodes scholars and to the fact that a larger proportion of undergraduates now complete three years of residence than was the case a few years back. The three largest colleges are Christ Church, New College, and Balliol, with 211, 210, and 181 members in residence.

A long vacation course in geography will be held in Oxford between August 7 and 25, provided that sufficient names are sent to the Reader in Geography, Old Ashmolean Building, Oxford, by June 1. The course will include lectures and demonstrations in the School of Geography, and surveying and map-drawing in the field.

At a meeting of the Junior Scientific Club, held on Wednesday, February 14, at the museum, papers were read by Prof. Miers on "Spontaneous Crystallisation," and by Mr. C. G. E. Farmer on "The Use of Finely Divided Metals in Organic Chemistry."

CAMBRIDGE.—The regulations for the diploma in mining engineering were passed by the Senate last Thursday. Among the chief of these regulations is that the candidate may take such parts of the natural sciences tripos and of the special examination in mechanism and applied sciences as bear upon the subject of mining engineering, or a candidate may take honours in the mechanical sciences tripos. Details of the examination and the schedules in the art of mining and in metallurgy will be found in the *Cambridge University Reporter* for December 5, 1905.

The Smith's prizes for 1904 have been adjudged as follows, the names being in alphabetical order:—C. F. Russell, Pembroke, for his essay on "The Geometrical Interpretation of Apolar Binary Forms"; F. J. M. Stratton, Gonville and Caius College, for his essay on "A Problem in Tidal Evolution Suggested by the Motion of Saturn's Ninth Satellite."

Mr. J. W. Nicholson, of Trinity College, has been elected to the Isaac Newton studentship in astronomy and physical optics, of the value of 250*l.* for one year, for study and research in astronomy.

Mr. R. H. Rastall, late scholar of Christ's College, Harkness scholar in 1903, has been elected to a junior fellowship at Christ's College. Mr. Rastall has worked chiefly in the Geological Museum at Cambridge, and has written on the Blea Wyke beds of Yorkshire and on "The Buttermere and Ennerdale Granophyre" of Lakeland.

DR. C. H. LEES, lecturer in physics and assistant director of the physical laboratories of the University of Manchester, has been appointed professor of physics at the East London College.

THE King's Speech, read by His Majesty at the opening of Parliament on Monday, promised that, at the earliest possible moment, a Bill would be introduced "for amending the existing law with regard to education in England and Wales."

THE Lancashire County Education Committee has recommended the council to make a grant of 100*l.* a year to the fund for the establishment of a department in economic botany in the University of Liverpool. The cost of the proposed department has in consequence now been completely guaranteed.

THE Senate of the University of St. Andrews has resolved to confer the following honorary degrees, among others, at the graduation ceremonial on April 3:—LL.D., Dr. A. C. L. G. Gunther, F.R.S., in appreciation of his lifelong and distinguished labours in zoology, Prof. J. C. Wilson, Oxford, and Prof. A. H. Young, Manchester.