"hundred god," and to the local deities of hill and river, the flesh being consumed only by the men, who must live apart from their wives during the rites; the cremation of the dead with subsequent interment of the bones, the ceremonies including an elaborate animal sacrifice and a tribal dance; the bachelors' club of youths associated for agricultural work, which is now passing into decay. Among the Meitheis may be noted the selection of a man who gives his name to the year, bears all the sins of the people during that period, and whose luck, for good or ill, influences the luck of the whole country. Sportsmen will be interested in the account of polo, with its primitive regulations. Introduced into Manipur from the Indo-Tibetan region about 1600 A.D., the possibilities of the game were suggested to British officers by Manipur teams which played at Cachar and Calcutta.

SOME SCIENTIFIC CENTRES.

No. XIV.—THE HORTUS BOTANICUS AT AMSTERDAM.

THE name of one of the most famous centres in the domain of biology conveys little idea of what goes on there to the average English-speaking man, unless he knows already. The Experimental Garden—as this centre is called—in the Hortus Botanicus at Amsterdam is a laboratory in which the results for which it is famous have been obtained, not by experiment, but by observation, as we usually understand these terms.

This is not the place to discuss the question whether a line can be drawn between experiment and observation; nor, supposing that one can, to attempt to arrive at some conclusion as to where observation ends and experiment begins. But it seems to us that the whole essence and significance of de Vries's work lies in the fact that it has been a work of observation. De Vries's name will be remembered as that of the man who saw what Darwin foresaw; who spent his life in carefully observing and accurately recording the process of the origin of species.

To appreciate the nature of the work which has been done in the Experimental Garden, it is necessary to take a brief glance at the main features of the previous attempts to deal with a problem which, until de Vries attacked it, resisted all attempts to solve it satisfactorily. This survey will also serve to explain more fully what is meant by the statement that de Vries's work was, in the main, one of observation.

The history of the efforts of biologists to deal with the problem of evolution, as told by de Vries in his "Mutationstheorie," is a history of the gradual improvement of the power of observation, which first saw in the genera the units of the natural system; then the Linnean species; and, finally, the elementary species of which the Linnean species are composed. At each stage in this history, the observer very naturally regarded as the ultimate unit of the natural system that unit which he saw by focussing his faculty of observation on it as finely as he could. In pre-Linnean days, the genera were regarded as the units; from then until now, the Linnean species have been so regarded, and the modern view, put forward by de Vries, is that the Linnean species are compound things, being, in fact, composed of the elementary species, which are the real units of the natural system.

In pre-Lamarckian days, the chief attribute of the real unit of the natural system was that it had been created and had not arisen by natural means. So that when Linnæus elevated the species to the rank of the unit of the natural system they acquired this attribute automatically. *Species tot numeramus quod diversae formae in principio sunt creatae* are Linnæus's words. It is a very interesting fact that Linnæus knew that his species were capable of further subdivision into what he called *varietates minores*; but these had arisen by natural means, and so were not worthy of the attention of the serious student. *Varietates levissimas non curet botanicus* were the words in which he forbade his students to pay any attention to them. The fads of genius are not buried with their authors. Prof. de Vries himself can remember pointing out on one or two occasions, when a student, curious abnormalities and instances of apparent subspecific characters to his professor, and being told by him not to pay any attention to them. He has occupied the rest of his life in doing so.

The nucleus of the Experimental Garden at Amsterdam was a certain potato-field near Hilversum, not far from Amsterdam. It had been bounded on its southern side by a canal from time immemorial. In 1870 the owner of the field, Mr. Six, had an extension of the canal dug along its western and part of its northern side; the result of which was that the original access to the field on its northern border was blocked, and that it could only be reached by its eastern side, where, however, there was, unfortunately, no road. Mr. Six found himself unable to let the field, and decided to plant it with trees. Rough paths were accordingly cut, and small trees planted.

Here was a wonderful opportunity for the wild plants, which had been kept in check with the hoe year after year, to establish themselves and multiply—an opportunity for the supercession of the horticultural by the cosmic process, to borrow Huxley's famous illustration. Yet, curiously enough, the fullest advantage of this opportunity was taken, not by an indigenous species, but by an introduced one which had spread over into the field from a small bed in a park close by, where a few annuals were grown every year. It was the beautiful evening primrose, *Œnothera Lamarckiana*.

De Vries first saw the field in 1886. The Œnotheras spread over a wide zone, the centre of radiation of which was the point at which the species had invaded the field. The centre of this zone was covered by a dense jungle of Œnotheras as tall as a man; outside this zone the adult plants gradually gave way to younger ones, whilst outside of all was an advanced guard of rosettes which did not lift their heads above the level of the ground.

All this seemed to offer to de Vries an opportunity which might never occur again of studying the phenomena of variation as exhibited by a plant multiplying, practically without restriction, in a state of nature. Moreover, he had been trying for some years past to find plants in a state of mutation (that is, of giving off new elementary species) but in vain. Œnothera broke the spell of failure. It was in a mutable period; new elementary species were arising; two had already arisen in the potato-field, *Œnothera brevistylis* and *Œ. laevifolia*. It very soon became evident that, to observe the process of the origin of mutations properly, it was necessary to grow the plants under direct personal observation in one's own garden. In the first place, only a very small proportion of the seeds that are shed in nature can germinate, and, of those that do, a very small proportion can attain maturity; so that if a mutation does arise the chance that the seed which contains it will survive to maturity is small. In the second place it is impossible to know the parentage of any of the plants in the field, partly because it is not possible to know from which plants the seeds which gave rise to them have come, and partly because, even if this were known, it would still not be known whether they were the result of a self- or cross-fertilisation.

Œnotheras were therefore transported from the field at Hilversum to the garden at Amsterdam. This was done in one of two ways. Either the young first year's rosettes were transplanted (*Œ. Lamarckiana* generally behaves as a biennial, flowering in the year after that in which it was sown), or seed was collected from the mature plants in the field at Hilversum and sown in the garden at Amsterdam.

The expectation, based on the appearance of two new species in the field, that more would arise in the garden was fulfilled. There arose altogether about a dozen new elementary species in the garden. The work of investigating the mode of origin of these new species consisted partly in finding out if the relative numbers of these species appearing every year were at all constant, and, if so, what the "mutation coefficient" (as this number was called) was; it consisted also in testing the constancy of each new species through several generations. Besides this, de Vries was continually on the look out for new species, and for this purpose large sowings of Œnothera seed were made every year. Moreover, crossings between the various elementary species were continually being carried out. The number of plants which had to be examined in the course of this work was enormous; and the number could not have been so great, nor the work so thorough, if de Vries had not paid special attention to the distinguishing characters of the seedlings of the various species.

His plan at first, *i.e.* shortly after the transportation of Œnotheras from Hilversum to Amsterdam, was to sow the seed directly in the bed in which the plants were to flower. The disadvantage of this plan was that all the seed did not come up in the first year; so that, in the first place, all the crop arising from a single sowing could not be recorded at one time, and, in the second, the bed could not be used for another sowing until it was certain that all the seed from the last had come up. Moreover, weeding and the minute examination of the seedlings was not by any means convenient in these circumstances. This plan was therefore soon forsaken, and that of sowing the seed in pans adopted in its stead. The pans were filled with soil which had been baked, a process which killed any seeds which might be in the soil, so that there was no possibility that any of the Œnotheras which came up could have arisen from any other source but the seeds deliberately sown in the soil, and the tiresome and difficult process of weeding was rendered unnecessary. The seedlings could be examined much more minutely and thoroughly in the pans than in the beds.

Prof. de Vries in his greenhouse.

The result of this innovation was that de Vries acquired a most intimate familiarity with the seedling characters of the various new elementary species; this is to a certain extent putting the cart before the horse. It is perhaps truer to say that the majority of the new species which were discovered after the introduction of this innovation owed their discovery to the fact that they differed from the parent form in the seedling stage.

Perhaps the most valuable improvement in the equipment of the Experimental Garden was one which was made possible by the practical way in which his former students and friends expressed their admiration for de Vries and his work, on the occasion of the twenty-fifth anniversary of his professorship at the University of Amsterdam. On this occasion de Vries was presented with a considerable sum of money, which was expended in the erection of a vast greenhouse, which enabled him to defy the climate of Holland, against which he had been contending for many years with anything but complete success.

The Experimental Garden at Amsterdam, as it now stands, is the result of an attempt to perfect a method of observing the origin of species. The success of this attempt will rank as one of the greatest achievements in biology.

THE SURVEY OF AFRICA.[1]

THE fifth volume of the account of the geodetic survey of South Africa, executed under the supervision of Sir David Gill, has now been issued. With the four volumes previously published the description of the whole work, from the southernmost point of the continent up to the Zambezi River, is thus completed. A sixth and final volume is promised, which will comprise that portion of the thirtieth meridian arc done by Dr. Rubin, carrying the survey northward from the Zambezi to a point 70 miles south of Lake Tanganyika. This will therefore round off the South African part of this great undertaking, the first idea of which was originally conceived by Sir D. Gill in 1879. To him, together with his able lieutenant, Colonel Sir W. G. Morris, the credit of thus carrying through this immense task, in face of many political and financial difficulties, must be ascribed.

The present volume is replete with interest both to the scientific surveyor and to the student of public policy on the questions of survey and map-making. The main interest naturally centres about the introduction by Sir D. Gill, and the introductory report on the trigonometrical survey of the Transvaal by Sir W. G. Morris. The former gives a succinct history of the triangulation of South Africa, recapitulates the now well-known proposal to extend the thirtieth meridian arc through the continent, and concludes with a detailed *résumé* of the negotiations between the Imperial Government and the colonial authorities for the formation of a federal survey department. These extended, with intermissions, from 1901 to 1904, and finally ended abortively, one colony after another deciding that they could not afford the expenditure necessary for the construction of an accurate map of their territory. The expenditure ultimately and implicitly involved by the existence of inaccurate maps or by the complete non-existence of any maps at all, being an item which does not come on the estimates for the year, is, we must perforce conclude, a subject of little concern to the politician. Otherwise, unless we are to assume that public memory is so short that a period of three or four years is sufficient to drive the most striking events out of mind, it is difficult to see how one of the main object-lessons of the South African war, *the extreme costliness of bad maps,* should have been so soon and so completely set aside. Sir D. Gill's account not unnaturally gives special prominence to those parts of these proceedings which took place in Africa, with which he was directly concerned. The result is that he does less than justice to the part played by the War Office, and is apparently unaware that the proposal to carry out a complete survey of South Africa, by cooperation between the Imperial and the colonial authorities, was put forward by that office long before the date of the similar suggestion by Colonel Morris, referred to on p. 16.

The whole history of this geodetic work is a curious inversion of the general order. Usually it is the complaint of the map-maker that, whereas it is not difficult to get money from a Government department for the immediate, practical work of mapping, it is a more laborious task to persuade them of the necessity for a liberal expenditure upon the fundamental geodetic triangulation. In South Africa the exact reverse of this has been the case, and we have the anomalous position of a complete triangulation system without the resulting maps; even as yet it is only in the case of the Orange River Colony and partially in Cape Colony that any of the maps of the country are based upon the positions of the geodetic points.

Of the technical part of the report the most interesting is undoubtedly the account of the base measurements carried out with invar wires hanging freely, at a constant tension, between low tripod supports. Five bases in all, totalling a length of 70 miles, were measured. Each was gone over with the wire three times, and the apparent probable error varied from 1 part in 1,000,000 at the Belfast base, where the staff was inexperienced, down to nearly 1 part in 7,000,000 in the most favourable case. Sir D. Gill maintains that, with a trained staff, a base can be measured in this way with an actual final uncertainty of less than 1 part in 1,000,000—say, 1 inch in 15 miles—a contention apparently justified by the figures. The rate of progress, including the time spent on the wire comparison with the standard bars, averaged 475 yards per day, and the cost was high—153*l.* per mile of base. In view of this, and in view of the fact that a limiting error of 1 part in 1,000,000 implies a much higher degree of precision than that attained by the angular observations, it would seem more practical, for similar work in the future, to make the bases both shorter and less accurate, and, therefore, cheaper and more rapidly executed. This would have the effect of preserving that balance between the degrees of precision of the different parts of the work so essential to the economical conduct of a cycle of physical operations.

The horizontal angles were observed with the 10-inch Repsold theodolite, the probable error of a single angle being found to be $0''.30$ with eight changes of zero, or $0''.39$ with four only. It is remarked that as these figures closely coincide with those previously reached in Cape Colony and Natal with the same instrument, they probably represent the highest possible degree of precision attainable under the special climatic conditions and with the instrumental means available. So far as the observing end of each line is concerned this is possibly true, but it is questionable whether the results might not have been improved, with no sacrifice of time or money, if a better pattern of beacon had been employed. The tripod or quadripod beacon, forming from any distant point a double cone, with vertex at the centre, of sufficient height to enable the theodolite or heliostat to be centred without disturbing the legs, is an altogether preferable form to the pole beacon actually used.

E. H. H.

[1] Geodetic Survey of South Africa. Vol. v. Reports on the Geodetic Survey of the Transvaal and Orange River Colony, executed by Colonel Sir W. G. Morris, K.C.M.G., C.B., and of its connection by Capt. H. W. Gordon, R.E., with the Geodetic Survey of Southern Rhodesia, with a preface and introduction by Sir David Gill, K.C.B., F.R.S. Pp. xxxvii+463+16 plates; 6 maps. (London: Harrison and Sons, 1908.)