besides its features as a botanical garden proper it is used as a nursery for propagating economic plants for distribution to the planters of the island and as an agricultural experiment station for the investigation of various agricultural problems.

The second paper was by Dr. Howe, on 'The So-called Flowering of the Adirondack Lakes,' a phenomenon caused by the growth of one of the minute blue-green algæ, specimens of which were exhibited. The substance of this paper appeared in the October issue of *Torreya*.

Dr. Britton spoke of the recent discovery by Mrs. Goodrich, at Syracuse, of *Phacelia dubia*, a plant new to the New York state flora. This discovery extends the known range of the plant several hundred miles farther north.

On motion, the thanks of the club were voted to Mrs. Cunningham for her interesting exhibition of flower paintings.

F. S. EARLE,
Secretary.

THE SCIENCE CLUB OF THE UNIVERSITY OF WISCONSIN.

THE club held its first meeting of the academic year on October 5, President F. P. Turneaure in the chair. The paper of the evening was given by Professor Victor Goldschmidt, of Heidelberg University, on 'Recent Developments in Crystallography.' Professor Goldschmidt discussed his recent work on the etching figures formed on calcite crystals and on spheres of calcite when subjected to the dissolving action of acids.

VICTOR LENHER,
Secretary.

DISCUSSION AND CORRESPONDENCE.

THE CHEMISTRY OF SOILS AS RELATED TO CROP PRODUCTION.*

The following quotations will best define the scope of this bulletin of seventy-one pages, and the theses which it is intended to establish and maintain.

Page 7. "The investigations made by the Bureau of Soils during the last ten years have shown that the economic distribution of crops is dependent mainly upon the physical characters of soils, and upon climate."

Page 13. "Briefly stated, the results given in the following pages appear to show, contrary to opinions which have long been held, that there is no obvious relation between the chemical composition of a soil as determined by the methods of analysis used and the yield of crops; but that the chief factor determining the yield is the physical condition of the soil under suitable climatic conditions."

Page 63. "The exhaustive investigation of many types of soil by very accurate methods of analysis under many conditions of cultivation and cropping, in areas yielding large crops and in adjoining areas yielding small crops, has shown that there is no obvious relation between the amount of the several nutritive ingredients in the soil and in the yield of crops."

Page 64. "It appears farther that practically all soils contain sufficient plant food for good crop yield; that this supply will be indefinitely maintained, and that the actual yield of plants adapted to the soils depends mainly, under favorable climatic conditions, upon the cultural methods; a conclusion strictly in accord with the experience of good farm practice in all countries."

The bulletin contains extended tables showing the results of the analytical work, and at the end, a full description of the methods employed therein.

The above four paragraphs, taken respectively from the beginning and the latter part of the bulletin, summarize the conclusions to which, as it states, 'the Bureau of Soils has been forced.'

These conclusions are certainly startling, to say the least; and perhaps not the least remarkable is the concluding one, which hardly agrees with the impressions left upon the mind of most of those who have made themselves acquainted with the history of agriculture, and its past and present practice in the most advanced civilizations.

Were such statements to emanate from a private laboratory, on a mere personal responsibility, it would be likely to be passed

* Bureau of Soils Bulletin No. 22, 1903.

over and allowed to run its course. But when it emanates from the head of the Bureau of Soils in the United States Department of Agriculture, and is expressly and persistently given as the opinion of that bureau, it can not be thus passed over unchallenged.

The above quotation from page 7 of the bulletin practically prejudges, or begs, the main question at issue. To any one outside of the bureau the cogency of this statement is far from apparent, except in so far as it may mean what has long been known and recognized, and need not, therefore, have been shown anew by the bureau.

If we examine the experimental basis upon which all these assertions are made, we find it to be the assumption that the aqueous soil solution is the exclusive source through which plants derive their food; and the fact, assumed to be demonstrated by a newly devised method of analysis, that that solution is practically of the same composition in all soils, so far as the mainly important plant-food ingredients are concerned. Throughout the bulletin the determinations thus made are considered and mentioned as constituting an 'exhaustive investigation of many types of soils, by very accurate methods of analysis.'

It is not the intention of the present writer to question the accuracy of the analyses, such as they are. But it is notorious that there are a great many methods that may and have been used for the chemical analysis of soils, each susceptible of great analytical accuracy, but in many if not in most cases having no practical bearing upon the agricultural value of the soils analyzed. The method of ultimate silicate analysis is one; and it is generally conceded that the results so obtained have but a very remote bearing upon the practical value of a soil. The method of extraction with distilled water is another; it is the opposite extreme, and unlike the silicate analysis, can certainly not be considered 'exhaustive.'

Now the criterion usually applied to the relevancy of soil analyses is whether they will stand the test of agricultural practice. Judged by this test, both the ultimate analysis and that by distilled water are, equally, failures, according to Whitney's own testimony. But his conclusion is that since his method fails as a criterion of rich and poor soils, therefore the chemical composition of soils has no bearing upon crop production; and that, therefore, 'the chief factor determining the yield is the physical condition of the soil under suitable conditions.'

To this assertion 'non sequitur!' is the obvious first answer. But before discussing it, it seems proper to recall, as regards the personal standpoint of the present writer, that he was the first one to undertake systematic physical soil work in the United States, in the early sixties; and has steadily pursued it ever since, as his publications* show. He has always held, taught and written that the physical soil conditions are the first thing needful to be considered in the estimate of a soil's practical value, the chemical composition second; since faults in the latter can in most cases be much more readily remedied than faulty physical conditions. But that chemical composition is the chief determining factor of phytogeography in the humid region, and inferentially of crop production within the same, became his conviction in the prosecution of the agricultural survey of Mississippi; and hence he made it prominent in his work in that state. In the arid region, where moisture is the dominant factor, and soil composition much less varied, soil physics has received his chief attention. It can not, therefore, be truthfully said that the writer has not fully recognized the enormous importance of physical soil conditions, both in his teachings and his publications.

Eleven years ago it fell to his lot to controvert the hypothesis then put forth by Whitney, to the effect that fertilizers act, not by conveying nourishment to plants, but by modifying the physical texture of the soil.† The recent enunciation of the chief of the Bureau

* *Proc. A. A. A. S.*, 1872, 1873; *Amer. Jour. Sci.*, 1872, 1873, 1879; *Proc. Soc. Prom. Agr. Sci.*, 1882–1898; 'Wollny's Forsch.,' 1879–1896; *Centralblatt für Agriculturchemie*, 1886; *Agr. Sci.*, 1892; *Jour. Amer. Chem. Soc.*, 1894; U. S. Weather Bureau Bull. No. 3, 1892; *Ann. de la Sci. Agron.*, 1892; Calif. Agr. Expt. Sta. Reports and Bulletins, 1877–1903.

† *Agr. Sci.*, 1892, pp. 321, 566.

of Soils, while still maintaining the preferential claim for the physical properties of the soil, at least admits the importance of the functions of plant food; but claims that fertilization is unnecessary because the supply will be 'indefinitely maintained.' He in fact takes us back to the times of Jethro Tull and the Louis Weedon system of culture, which also presupposed the indefinite duration of productiveness; but signally failed to realize it when the test of even as much as twelve years came to be applied. How can Whitney reconcile this predicted indefinite productiveness with the actual facts well known to every farmer, good and bad, who has ever taken fresh land into cultivation, and when pricing it is perfectly aware that after a period ranging from three years, *e. g.*, on the long-leaf pine lands of Mississippi to thirty or more years in the black prairies, he must needs resort to fertilization if he wants a paying crop; while in the Yazoo clay lands and the alluvial soil of the Houma country, hardly a diminution of production has occurred even yet? If, indeed, the soil solution is of the same composition in all these lands, then the common-sense conclusion is, obviously, that if the soil solution is the sole vehicle of plant nourishment, it must be supplied more quickly and continuously in the 'rich' than in the 'poor' soils. Certainly, *considering that both rich and poor soils are represented in the entire gamut of physical texture,* it is impossible to conceive that such changes in texture as would be brought about by poor cultivation should not occur in both. Yet the rich soils—those shown by the despised chemical analysis with strong acids to contain abundance of plant food, continue to produce abundantly, while the poor lands 'give out.' Hence, admitting for argument's sake that the soil solutions are really of the same chemical composition, it is clearly not the physical texture alone, or chiefly, that can account for these differences.

Whitney states in this connection (p. 51) that I have 'called attention to an apparent exception to this rule (that production is sensibly proportionate to the water supply) in heavy adobe (heavy clay) and sandy lands in California, which bear equally good crops of wheat.' It happens that this 'exception' holds good throughout the somewhat extensive arid region of the United States; and my explanation is not only, or mainly, that the roots go deeper, but that in the arid region, sandy soils are as a rule quite as rich in plant food (again by chemical analysis of the rejected sort) as the clay soils. Hence the abundant and lasting production of the arid sandy lands (even drifting sands) when irrigated.

Whitney's argument that even the rich arid soils can not yield more than the maximum crops of the humid region can hardly be taken seriously.

It is a striking fact that in the entire bulletin only a single full soil analysis (*i. e.*, one made with strong acids) is quoted. There is a table giving the results of determinations of available plant food, determined by the official method, alongside of the distilled water extract; and it is apparent that the two differ widely. But there is no definite agreement among soil chemists as to the 'available' determinations, whether as to value or method; the matter is still in the tentative stage, and I wholly dissent from the 'official prescription.' The table in question proves nothing. But it would have been instructive, so long as Whitney wishes to disprove the value of soil analysis as usually made, to have at least some of the soil classes he adduces as proofs, analyzed by the usual methods; if only in order to show that these soil types—the Cecil clay, the Sassafras loam, Norfolk sand, etc., are really, as alleged by him, the same soils over the area assigned to them. How have these soils been identified in the mapping? We are informed (p. 8) that 'the classification of soils in the surveys made by this bureau is based mainly on physical differences, apparent to a trained observer.' It is apparent from the annual reports that the mineralogical and geological data which are elsewhere considered as essential to a definite characterization of a soil, and which certainly are to be counted among the physical characteristics, are in most cases wholly ignored. Instead, we have local names by the thousand, conveying no meaning whatever to those not acquainted with the localities; since nothing but a scantily inter-

preted physical analysis is ordinarily given. Even when the mineral composition of the soil is obvious, these meaningless local names are retained as against preexisting local or descriptive designations. Thus we have, *e. g.*, a 'Fresno sand' appearing also in the report on Orange and Monterey Counties, California —localities hundreds of miles apart. To the uninitiated only the physical analysis is offered as a mark of their identity by the trained observer. It seems a pity that that training should not have extended to calling that material a granitic sand, which would have rendered the designation intelligible all over the world, at the same time conveying important practical information in view of the well-known cultural characteristics and value of granitic soils. It is given out that these studies will be made later in the laboratory. But it may be seriously questioned whether it would not be better to cover less ground more thoroughly, and be content with less extended and hasty mapping. This superficial method of work naturally excites criticism, not only at home, but also abroad.*

Until some better proof of identity is shown we can not accept Whitney's conclusions based on the similarity of the soil solution with widely varying production on 'the same soil'; and his entire argument suffers seriously from the absence of any convincing proof that 'rich' soils do not supply plant food, even in aqueous solution, *more rapidly* than does 'poor' land.

But is the aqueous solution the only source of supply? Whitney rejects *in toto* the idea that anything but the carbonic acid secreted by the roots aids the solution of plant food; but his method of analysis practically ignores even this solvent, the use of which was suggested and actually carried out by David Dale Owen, and tried by myself, in the early fifties. I found it unsatisfactory and abandoned it; but it would seem to have been incumbent upon Whitney and his coworkers to introduce this inevitable agency into their soil extractions, if it was intended to represent natural conditions. This is a fundamental, not to say fatal, defect.

But there is still a wide difference of opinion in this matter of the acid root secretions, and the investigators quoted by Whitney have by no means settled the matter. Among others, Kossowitch,* when observing the fact that calcic bicarbonate leached from his vegetation pots, failed to establish the absence of other organic acids from the solution. The old etching experiments have not, to my mind, lost their force; and in my experience I find it difficult to overcome the evidence of litmus paper reproducing a faithful image of citrus roots (in the soil) filled with a .83 per cent. solution of citric acid.† If the *paper* can take up the acid from the root surface, surely the much stronger capillary action of the soil can do so, according to Cameron's experiment quoted on page 54.‡ But if so, *Whitney's entire argument based on watery soil solutions falls to the ground.*

Not the least remarkable part of the bulletin is that in which Whitney discusses the use and action of fertilizers. He does admit that 'there is no question that in certain cases, and in many cases, the application of commercial fertilizers is beneficial to the crop.' But he calmly brushes aside, as so many cobwebs, the enormously cumulative evidence of all the practical experience of three quarters of a century in the use of commercial fertilizers, as well as the carefully guarded culture experiments made during that time by numerous scientific workers; and announces the truism that climatic and seasonal conditions *may* neutralize the beneficial effects of any and all fertilizers used. This has been long and often said, experienced and foreseen; every one

* *Biedermann's Centralblatt*, February, 1903, p. 143.

* 'Ann. de la Sc. Agron.,' 2 ser., 1, 220, 1903.

† Report Calif. Expt. Sta. for 1895–6 and 1896–7, p. 181.

‡ "When a porous cell having deposited in it a semipermeable membrane through which water can pass freely, but through which salts and certain organic substances like sugar can not pass readily, is buried in a soil short of saturation, but yet in fair condition for plant growth, the soil will draw water from the cell against a calculated osmotic pressure in the cell of 36 atmospheres, or about 500 pounds per square inch."

knows that deficiency of moisture or heat, or imperfect cultivation, as well as the improper manner of application of fertilizers, may render them wholly ineffective. We have also long known that soluble fertilizers soon become insoluble (but not necessarily unavailable) in the soil, in a manner fairly well understood, and that hence they can not long influence the watery soil solution to which Whitney pins his faith. But since the same conditions influence the unfertilized soils to even a greater degree, manifestly because of the slower and less vigorous development of the plants, it is not easy to see what special corroboration Whitney's hypothesis can derive therefrom. He calmly discards, as having been made under 'abnormal conditions,' the elaborate and conclusive experiments made by the best observers in pot culture, in which the physical factors were so controlled as to eliminate them from the problem of the action of special fertilizers; and we are told that 'very little effect is obtained in field culture in attempts to increase the value of crops showing inferior growth, by the application of fertilizers.' A trip through the malodorous turnip fields of the Low Countries or of Switzerland in autumn would convince even the Bureau that the thrifty inhabitants know that when a fertilizer is made to reach the feeding roots its action is invariably most strikingly beneficial. That a top dressing of insoluble fertilizers on a growing crop can do but little good needs no discussion; and it is but too true that a great deal of the fertilizers used in the arid region remains wholly ineffective for a long time because of the deep range of the feeding roots and the shallow application of insoluble fertilizers.

In the classic water-culture experiments of Birner and Lucanus, quoted in the bulletin (p. 15), the well water was supplied continuously and in indefinite amounts. It is thus no wonder that the results were so good, for at no time was there a lack of plant food supply, nor would such changes as would injuriously affect the growth occur. But for these frequent renewals of the water the result would doubtless have been very different, if only as a consequence of changes in the *reaction* of the solution. It is singular that this important point is not even casually mentioned in the bulletin with respect to the soil solutions. The deleterious effects of soil acidity upon most culture plants, long known in general, has been well and thoroughly investigated by H. J. Wheeler.* Yet neither in the tables nor in the text of this bulletin do we find any evidence that this point has had any attention with respect to its possible bearings on the differences in production on what are held by the bureau to be identical soil areas. We are not informed whether the large amounts of lime present in some of these solutions were sulphate or carbonate; yet the importance of this difference is enormous, as is well shown by the contrast between the natural vegetation as well as the cultural value of gypseous as against limestone lands, which are everywhere among the most productive. An excellent illustration of what this omission may mean exists on the Gulf Coast of Mississippi, where (as I have shown in the 'Report on Cotton Culture,' Tenth Census, Vol. 5, p. 69) the soil of the infertile 'sand hammocks' differs from the highly and lastingly productive soil of the 'shell hammocks' almost alone in the proportion of lime (calcic carbonate) and phosphoric acid present, and in having an acid reaction; the percentages of plant food being very low in both, and both equally of great depth. This observation, together with others, led me very early (1860) to the conclusion that mere *percentages* of plant food were not in all cases proper criteria of soil fertility; and also to the enunciation of the statement which I have repeated many times in both my teaching and my publications, to wit: 'While all fresh soils of high plant food percentages are highly productive under all but very extreme physical conditions, the reverse is by no means true; since soils with low percentages may be highly productive if the relative proportions of the several ingredients be good, and the soil mass deep.' I have for some years carried on an investigation to determine the limits of dilution within which plants will do equally well in soils of high fertility (and plant food percentages) when these are diluted

* Reports of the Rhode Island Expt. Sta.

with quartz sand. While not yet completed, this investigation has already shown that a rich adobe (clay) soil, as well as an equally rich sandy soil, diluted to an extent of four to one, shows equally good growth, but that when in these soils the dilution reaches five to one, development is quite slow, and in a short season would mean a crop failure. The moisture content was in all these cases maintained at one half the maximum water capacity of each diluted soil. Photographs show clearly that here the roots made up by their extension for the lack of concentration of the food supply; but at the dilution of one to five they were unable to make up that deficiency, at least within a reasonable time, although the same total *amount* of food ingredients was always present in the increased bulk. Other things being equal, it is the *proportion,* then, between the several soil ingredients, quite as much as the absolute quantity at hand, that determines production. Incidentally, this experiment shows the wide variation of physical composition (from a soil containing 35 per cent. of colloidal clay to one with only 8.75 per cent., and in the sandy soil from 7.6 per cent. to 1.9 per cent.) within which plants will do equally well, provided the plant food ingredients are rightly proportioned; and provided also that a proportionally large soil mass is available to each plant.

In the foregoing discussion, only the salient points of the bulletin in question have been taken up, and their most obvious weaknesses briefly considered. To do more would involve the writing of a paper as long as the bulletin itself; and it is to be hoped that the matter will be taken up by others, also. Thus, for instance, the Rothamstead Station might have something to say regarding the singular interpretation here put upon the splendid work of Lawes and Gilbert.

In conclusion, it seems to the writer that the verdict upon the main theses put forward so confidently in this paper must be an emphatic 'Not proven!'

E. W. Hilgard.

Berkeley, California,
November 11, 1903.

ABSORBED GASES AND VULCANISM.

To the Editor of Science: The descriptions of the spine of Mont Pelé by Hovey and Heilprin remind me of the phenomenon I observed some ten years ago, when my mind was on the subject of the part which the original absorbed gases play in vulcanism, as discussed in my paper in the *Bulletin of the Geol. Soc. Am.,* March 3, 1894. I had a bottle of Werner's grape milk packed in the place of the tin of an ice cream freezer, the same having served its purpose, in order to cool it. I presume any other carbonated beverage would work similarly. Though chilled well below 0° C. the beverage remained clear and unfrozen, as long as it was corked, but upon removing the cork the gas began to escape and freezing to set in rapidly. Sometimes nearly the whole contents of the bottle would freeze. Upon one occasion, however, I remember seeing a 'volcanic plug' of frozen matter forced out in a round cylinder from the neck.

I am inclined to think that there may be a very close analogy with the Mont Pelé spine. I think it would not be very difficult to reproduce this phenomenon, though I can not tell the exact temperature at which it occurred.

Alfred C. Lane.

SHORTER ARTICLES.

THE HEREDITY OF 'ANGORA' COAT IN MAMMALS.

That Mendel's law is a fundamental principle of heredity becomes daily clearer as new illustrations of its workings come to light, either through a reexamination of the older observations on heredity or through the performance of new experiments. One of these new illustrations it is the purpose of this note briefly to describe.

The writer has already pointed out, in the columns of Science, two pairs of alternative, or Mendelian, characters pertaining to the hairy coat of guinea-pigs. (1) A pigmented coat of any sort is dominant over an unpigmented, or albino, coat. Accordingly when a pure-bred pigmented guinea-pig is mated to an albino, the young are invariably pigmented. (2) The rough, or 'rosetted,' condition of coat found in so-called Abyssinian and Peru-