ON THEORIES CONCERNING SOILS AS MEDIA FOR PLANT GROWTH.

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These theories, together with the so-called laws of Liebig which I have just cited, were the cause of protracted controversies, some of which were not settled for a period of fifteen or twenty years. Indeed, one of the controversies (the nitrogen question) was not fully disposed of until 1885. The disputes in question were the result of experimental work in the field carried on by Lawes and later by Lawes and Gilbert in England, and beginning about the same time as the famous experiments of Boussignault in France. By actual results obtained on the analysis of the ash of certain plants and by the response of such plants to fertilizer ingredients, Lawes and Gilbert demonstrated that the composition of the ash of plants was no necessary index to the needs of plants for the mineral elements. They also showed that the air could not be depended upon to furnish sufficient nitrogen to plants when the soil was unequal to the task, reserving as incomprehensible the case of leguminous plants which usually proved to be exceptions to this rule. I wish that time and your patience permitted me to descant on the more detailed phases of those remarkable experiments of Lawes and Gilbert, wonderfully conceived, accurately planned, and scrupulously and ably executed, which served to answer and effectively dispose of Liebig's mineral theory and largely of his nitrogen theory; to tell of Liebig's patent fertilizer and the story and reason for its failure; and to describe the numerous investigations which these theories stimulated. It is impossible, however, to go into all these matters in the brief space of an hour. I must, therefore, content myself with what I have told you of Liebig's work and with the further comment that despite a number of errors which crept into Liebig's theories, the latter were essentially sound, and while they constituted very largely amplifications of de Saussure's theories and facts, they remain as great achievements from the point of view of scientific propaganda. The position in the annals of agricultural chemistry and in plant physiology occupied by Liebig, however, represents only a small part of the credit which is due him in the general field and particularly in the organic part of chemistry.

Before passing on to the progress of soil theory after 1860,

by which date the controversies between Lawes and Gilbert and Liebig were largely settled, there must be noted an old theory in regard to the cause of soil infertility, which has been of the greatest potency, directly or indirectly, in some of the most recent developments in soil theory. I refer to a modification of a very old idea of the selective powers of plant roots introduced into more or less modern literature by the distinguished French plant physiologist, Decandolle, in 1840. According to this theory, the roots of plants excrete certain substances which on accumulation become toxic to the same variety of plant, but not necessarily to other varieties of plants. Hence, the theory goes on to account for the benefits of crop rotation. It is interesting that this view should have been revived for the first time contemporaneously with the appearance of Liebig's report to the British Association, and particularly that it should have been regarded favorably by Liebig as the latter's more or less detailed consideration of it indicates. I shall return to a consideration of the Decandolle theory later in connection with a recrudescence of it in a period close to our own.

Great progress was being made in the sixties and seventies of the last century in all branches of science and much of it had its important relation to and effect upon soil theory. The brilliant work of Pasteur was going on and was opening the way for the development of modern microbiology, which dates from the discovery, by Robert Koch in 1882, of the plate method of isolating bacteria. Pfeffer and De Vries independently made their celebrated contributions to science in 1877 on the subject of osmotic pressure with particular reference to absorption by plants, thus rendering clearer the mechanism of salt action in the medium of plant growth. Many other pieces of outstanding work of a lesser magnitude were being accomplished. But none contributed anything to modify materially the theory of plant nutrition which may be regarded as a composite of the views of de Saussure, Liebig, and Lawes and Gilbert, on which all of those investigators would have agreed. Nor did any of them shed any light on the puzzling question of the legumes in connection with the nitrogen nutrition of plants. A radical departure from established experimental procedure was again needed as in several periods of the history of soil theory before. It was soon forthcoming, however, with the development of general, and closely on the heels therefore, of soil bacteriology. As far back as 1862, Pasteur suggested that nitrification was probably a bacte-

rial process. Up to then, and even for some time after that, it was regarded as a chemical reaction taking place without the intervention of living organisms. In 1877, however, the year in which Pfeffer and De Vries made their important contributions to the subject of the osmotic character of the soil, Schloesing and Muntz, two French investigators, definitely demonstrated the process of nitrification to be of biological origin. They found that when sewage containing ammonia was poured very slowly through tubes of sand, the ammonia would come through unchanged for a period of twenty days, after which it would disappear partly or wholly and its place would be taken by nitrates. When chloroform was poured into the tubes, nitrates would soon cease to appear in the filtrate and again ammonia would be found. When the chloroform was allowed to evaporate and the sand reinfected with some fresh soil, nitrates would again be found in the filtrate in a few days. This demonstrated beyond a peradventure that nitrification was a process due to living agencies. It took thirteen years more, however, and the patient attention and sedulous industry of a number of leading chemists and physiologists, culminating in the isolation of the nitrifying bacteria by Winogradsky in 1890, to make clear the process by which nitrogen in soil organic matter is rendered into nitrates.

Even this splendid work on nitrification, including Winogradsky's classic contribution, however, would still have left unexplained the abnormal behavior of legumes in the experiments of Lawes and Gilbert. But the necessary explanation was made through the striking and epoch making discoveries of Hellriegel, and of Hellriegel and Wilfarth, made public in 1885, and soon confirmed by Lawes and Gilbert, and by many others. These investigators found that when planted in a sterile medium, legumes do not behave abnormally, but respond to soluble nitrogen like non-legumes, growth being more or less regularly increased with every increment of nitrate nitrogen. When, however, the medium was not sterile, they would at times behave like non-legumes and at times not. They found, further, that nodules containing large numbers of bacteria were always found on the roots of the plants behaving abnormally. They connected these various facts and decided that the bacteria in the nodules on the roots of legumes, by some mechanism, obtained access to the large store of nitrogen in the air, which was not available to non-legumes, or to non-infected legumes. The nodules had been known since 1687. and the bacterial bodies in them

had been known and described for a number of years before, but had not been directly and definitely connected with nitrogen fixation. Frank had suspected the connection and published his suspicions, but later retracted the idea. The organism of the legume nodules was isolated about 1888 by Beijerinck and named *B. radicola*.

The puzzling questions and erroneous theories regarding both the transformation of soil nitrogen and the fixation of atmospheric nitrogen and their relations with the growth of legumes and non-legumes were thus finally cleared up and disposed of in the decade between 1880 and 1890. Other important contributions to our knowledge of the more intimate nature of soils, and the process of nitrogen transformation and fixation and of many other soil biological processes, have been made since 1885, but none of them has, in my opinion, affected fundamentally our theories on plant nutrition and on soil fertility; except that in general, as will be commonly agreed, the knowledge of the soil organisms which we have gained since 1882 constitutes an indispensable link in the chain of ineluctable evidence which indicates that the flora and perhaps the fauna of the soil are directly or indirectly essential factors in the transformation of all elements of plant food in the soil into usable forms.

For purposes of giving the proper perspective and necessary introduction to the next period in the development of our subject, it is well to pause for a moment and consider the circumstances which have contributed to it and made it possible. The history of soil theory may be divided by an arbitrary method, which I have adopted, into three periods. The first period covers the large era from ancient times to the beginning of the eighteenth century, which was characterized by much speculation, by little experimentation, and by scarcely any progress in the object of attaining unto a knowledge of how the plant feeds and grows. The second period may be said to begin about the year 1700 and to end about 200 years later. In the first half of this period, the experimental idea was rapidly developing and the aim of thinkers and investigators was to determine what the requirements for plant growth are. In the second part of this period, speculation was rapidly giving place to theory tested by carefully devised experiments, beginning with de Saussure and culminating in the researches by Liebig and his followers, Lawes and Gilbert, Hilgard, King, Grandeau and many others. It was in this part of the second period that the object sought from the

beginning was successfully attained. The requirements for plant growth were finally determined. The parts played by the air, by water, and by the soil in plant nutrition were made clear and accurately appraised. But it must be noted that the school of de Saussure-Liebig-Lawes and Gilbert was one in which fundamental knowledge only of the *requirements* for plant growth were sought and found. They taught us little or nothing regarding the soil as a medium of growth to the point to which such teaching could be employed in formulating an idea of the conditions which control the power of the soil to yield heavily or lightly. In other words, it appears that the investigators of the nineteenth century assumed that the most difficult problem in plant production was solved when we learned what the requirements for plant growth are and their sources. The application of these principles had not progressed far enough to indicate to all concerned that it was necessary to know much more about plants and their media of development—soils—before we could hope to control the yields of crops on a thoroughly scientific basis. The investigators of those days could not see that it mattered little *cacteris paribus* how much of certain essential elements to plant growth were present in the soil at one time in the life of the plant, if they were not there in sufficient quantity at another period of its existence. They assumed that the soil constituted a static system in which, in some simple manner, water, aided by acids from decomposing organic matter and from root transudations, dissolved the minerals of the soil and maintained them at a more or less constant concentration. They assumed that the addition of soluble fertilizer salts to soils meant entire solution of the former and hence availability in toto to plants. With all these assumptions and many others in mind, the subject of the more intimate nature of soil reactions was ignored, merely analytical chemistry was applied to soils to determine total amounts of materials present, fertilizers were applied by empirical procedures; and in general, the outstanding work of Way, of the developing school of physical chemistry, and of Van Bemmelen on colloids were not considered as of much direct importance to a study of the soil. In other words, soil study *per se* was neglected. But the last years of the nineteenth century began to hear an occasional voice of protest from some of the investigators who were thinking hard on the subject of the soul-wearying soil analyses in vogue and their almost entire meaninglessness, on the riotous empiricism of fertilizer practices,

and on the possibility of learning something more about the chemistry of the soil itself.

This was the status of the subject at the beginning of what I have classified as the third period in the history of soil theory and science, about the year 1900. The man to whom, in my opinion, we owe most for the inauguration of this new era is Cameron, whose work in turn was made possible through the fact of his connection with Whitney at the Bureau of Soils. Whitney's radical ideas on the functions of fertilizers, on the need for studying the soil's physical condition, and on the cause of soil infertility, made him rebel against the established order as early as 1892. He announced in that year that fertilizers function chiefly in improving the physical conditions of the soil and that the proper supply of water to plants by means of maintaining the proper physical conditions in the soil was the chief problem in crop production. Later on, he revived and amplified Decandolle's idea of toxic root excreta as being the chief cause of infertility in soils and the reason for the benefits of crop rotation by which means he believed fertility could be maintained. In these ideas Whitney was supported by Cameron, as above indicated, and by Schreiner and many other investigators at the Bureau of Soils. Cameron led the work on the physical and inorganic chemistry of soils, and Schreiner that on the organic chemistry of soils.

The longer Whitney and his associates studied the problem during the decade from 1893 to 1902, the more firmly convinced did they become of the validity of their theories. They appear to have regarded their theories subjectively, however, and made them the chief burden of the now celebrated Bulletin No. 22 of the Bureau of Soils by Whitney and Cameron, which was published in 1903. Since then, many papers have been published by the Bureau of Soils dealing with toxic root excreta and with studies on the physical chemistry of the soil or soil solution. The theory on the toxic root excreta has been made more inclusive and postulates also the presence in the soil of toxic organic substances produced in the decomposition of organic matter there. While nothing conclusive of practical significance has resulted from the search for such organic toxins, it has been demonstrated in those investigations, which are still going on, that toxic organic compounds are found in soils. Many such substances have been isolated from soil organic matter and much light has been shed on the organic chemistry of the soil, which,

until the investigations of Schreiner and his associates at the Bureau of Soils, had been scarcely changed from the crude ideas held by Detmer, Mulder and others from fifty to seventy-five years before. While, therefore, it still remains to be shown that crops on infertile soils are arrested in growth and depressed in yield by organic toxins, whether originating from root excreta or from the decomposition of soil organic matter by the soil flora, science owes a debt to Schreiner, Livingston, Whitney, and to many others working at the Bureau of Soils at one time or another, who have been directly or indirectly responsible for giving us a more intimate picture than we had ever had of the nature and complexity of the soil organic matter.

We must retrace our steps a little to return to the publications of Cameron, to whose ingenuity, clear and incisive thinking, and courage we owe, in my opinion, the development of our modern physico-chemical views of soils just as we owe largely to Schreiner the development of our modern ideas on the organic chemistry of the soil. As early as 1901, Cameron began the publication of papers which attempted to show the application of physico-chemical principles to soil reactions. He pointed out the fact that the soil is to be regarded as a physical-chemical system in which the various minerals and water form a solution of a certain composition and concentration when equilibrium is attained in the system. Since most minerals are only slightly soluble in water and since practically all soils contain the important rock and soil forming minerals, Cameron argued that when equilibrium is attained in any soil, other conditions being equal, the composition of its solution, as well as its concentration, must be very much the same as that in any other soil. He was so impressed with this idea that he proceeded to test it out by making a series of soil extracts with water after brief digestions of the soil with water. The differences which he obtained between the composition and concentration of different soil solutions were regarded by Cameron as being small and insignificant and he, therefore, concluded, as he stated in 1903 in Bulletin 22, in joint authorship with Whitney, that all soils contain extracts of the same general nature and concentration, the latter being great enough to supply everything needed for big crops; and that, therefore, infertility of soils was not caused by a lack of the mineral elements of plant food in soils; that fertilizers did not function to supply any deficiencies in that direction; that infertility in soils is caused by the accumulation of toxic organic compounds in soils; that

crop rotation would overcome the latter difficulty; and that fertilizers function to neutralize the effects of the root and soil toxins and to improve the physical condition of the soil. Cameron developed these and minor arguments in bulletins of the Bureau of Soils and in a series of papers published in the *Journal of Physical Chemistry*, and brought them together in what is, in my opinion, his epoch-making little volume entitled the "Soil Solution," which appeared in 1911. The basic principles in Cameron's theory consisted in first considering the soil-water system as a dynamic one in which constant movement and change of the components obtains, and second, that regardless of the total quantities of the chemical elements present in soil, it is the amounts of them in the soil solution, or in the water films investing the soil particles, which is the nutrient medium of the plant proper, which constitutes the important and significant factor in soil composition. Considered in the light of brief study and the comparatively meager development of physical chemistry a decade ago, the essentials of Cameron's ideas were theoretically correct, but like all soils investigators who have preceded him did, and like all who may follow him are likely to do, he overlooked some of the very points which his novel method of attack should have rendered clear to him. The more secure basis attained by physical chemistry since and the more deliberate thought on the whole subject with Cameron's views as a basis makes it appear today that the soil system is a dynamic one; that all soil problems must be considered in that light and not in the light of the static system which has been largely assumed for it heretofore; that soil minerals in all soils when attaining equilibrium with the soil water may yield solutions of approximately similar composition and concentration. But, and this was overlooked by Cameron, that the attainment of equilibrium in soils is not a rapid and facile phenomenon, if it is ever accomplished; that the large variations in the partial carbon dioxide pressures in different soil systems, and the great disparity which characterizes activities of micro-organisms in various soils, for obvious reasons, vary the nature of the solvent to so large a degree that when the systems do attain equilibrium, their solutions may differ markedly in composition and concentration; that fertilizers do exercise important effects on the physical properties of soils, and may exercise anti-toxic effects to the root excreta and soil toxins, but that their principal function is probably to affect directly by addition and indirectly by changing the solvent properties and reactive powers

of the soil solution, the latter's composition and concentration. Cameron, as a physical chemist, should have appreciated that fact, but didn't, probably partly because of the results of his analyses of soil extracts obtained by a rapid method of digestion, which precluded the possibility of attaining equilibrium in the system. He does not seem to have appreciated sufficiently, moreover, the importance of the small differences in concentration between the different soil extracts which he obtained nor of the effect of season, and hence of temperature and moisture, in accomplishing marked changes in the dynamic system of the soil. He did not realize, as we do now, that the power of different soils to supply anew nutrients which are removed from the soil by rapidly growing plants varies widely.

All of these points have been developed in the last six or eight years and still constitute vital subjects of investigation in their different bearings and relationships today. While, thus, physical chemistry, a better training of our investigators, and better facilities, have permitted us to make rapid progress in our views concerning soils as media for plant growth and to modify considerably many of Cameron's important teachings, the latter loom large in the annals of our science. They represent a fundamental contribution which necessitated a keen mind, clear thinking, unusual gifts as a chemist, and more than the usual modicum of courage to enable their proponent to escape from the moorings of conservatism, and facing alone the scientific world to break the shackles which had held in bondage the ideas of soils investigators for sixty years. In other words, it is Cameron's attitude rather than his experimental data which constitute his contribution. It does not detract from the credit which is due him to point out, as I have above, some of the serious errors into which he was led by his theories and his disputes. His position is, in my opinion, destined to be secure and eminent in the annals of scientific investigations.

The theories of Cameron and those of Whitney which have been directly or indirectly responsible for the investigations of the Bureau of Soils did not, as may well have been surmised, go unchallenged. It would be impossible in so short a time as is now at my disposal to review the record of the controversies which have raged between the Bureau of Soils on the one hand, and the soil chemists and agronomists of this and other countries on the other, during the last fifteen year period, and particularly during the first ten thereof. Such well-known soils investigators

as King, Hilgard, and Hopkins, and such organizations as the Association of Official Agricultural Chemists, have been aligned against Cameron and Whitney, and opposed to their theories. Due to the progress which I have just briefly sketched for you, which has been accomplished by soil chemists during the last six or eight years, the protests of conservative men and organizations have nearly all been silenced, however, and the general attitude adopted by Cameron with the modifications described, are accepted in this country today. Soil analysis by the strong acid digestion method and by the fusion method have been discontinued very largely and most chemists realize that it does little service to farming interests, and certainly to science, in the great majority of cases to determine how much of the elements essential to plant growth are to be found in soils. We have learned that we must devise methods for determining the composition and concentration of our soil solutions, must study the phase phenomena in such solutions, must learn what factors regulate all these in the field, and to supply the means by which they may be kept in optimum relations for plant growth. We owe much of our ultra-modern ideas on the soil solution, on methods for obtaining and studying it, and on soils in general, to a rapidly growing body of workers who are enabling us to shape Cameron's theories to the pattern of a vast body of new and striking facts. At the risk of making invidious distinctions by mentioning some and not others, I feel constrained to mention the outstanding work in the last five years of Bouyoucos, Morgan, Jordan, Burd, Hoagland and Stewart, Gillespie, Sharp and a few others who are applying with great success a few simple fundamental principles of physical chemistry to the study of soils. We owe a debt, further, to the plant physiologists who have been profiting, like the others, from the application of physical chemistry to their work. The results which they have obtained and which we are applying to good advantage in soils work are largely due to the outstanding investigations of Osterhout, Livingston, Dixon and Atkins, Stiles and Jorgensen, Brenchley, Shantz and of many others who are following in their footsteps.

From the combined results of modern plant physiologists and soil chemists, we are beginning to accord more importance to the role of the concentration of the soil solution as a medium of plant growth. In my opinion, a proper understanding of the effects of differences in concentration of the soil solution as affecting plants will probably go a great way toward solving some of the

most difficult problems in plant physiology with which we are confronted today. Particularly the problems involved in the so-called physiological diseases of plants would, it seems to me, be made easier by an understanding of the effect of the concentration of the nutrient medium on the diseased plants. For example, in such diseases as die-back and mottled leaf in citrus trees, it is not unlikely that we are dealing with concentrations of salts in the soil solution which are many times higher than those to which the roots of citrus trees are accustomed in their natural habitats, and hence derangement of the normal function of the leaves may result. This is particularly possible when, in addition to a high concentration of salts, a state of balance between the constituents of the solution obtains which is inimical to plants. This will serve as an illustration of the great possibilities which are in store for us in studies of the concentration and balance of the soil solution. The experimental data and observations which support such a theory cannot be reviewed here.

For the sake of completeness, another theory on the infertility of soils, which has been advanced in the last seven or eight years, should be mentioned. It is the one proposed by Bolley, to account for "flax-sick" and "grain-sick" soils. Bolley believes that such soils are rendered infertile through the accumulations, in large numbers, of parasitic fungi, introduced with poor seed, and self-propagating in the soil, which develop with the young plants and depress their growth. This theory has not received much serious attention, however, in spite of the fact that all agronomists and soil chemists seem to agree that the fungi in question may contribute to the depression in yields of those soils. With the fungi removed by the use of good seed, crop rotation, and tillage, the essential problem of maintaining the soil's power to produce still remains.

With the more intense study of the soil itself, which has characterized the last few years, there has gradually grown a more and more insistent demand for accurate methods in such study. One important basis for such demands should be referred to here, in view of its overwhelming significance for the future of all soil studies. I refer to the very marked variability which characterizes soil samples gathered in any field, no matter how apparently uniform, and no matter how close together the samples are taken. Only within the past several months, Frear has called attention to this great variability, and, independently and contemporaneously, one of my colleagues, Waynick, has

actually made a statistical study of one such case, the results of which not only are in agreement with Frear's view, but, in addition, lend mathematical precision to it. Moreover, it provides a basis by which such variability may be determined and a way is made open for the correct appraisal of the significance or insignificance of differences in the results obtained in experimental work.

I have given you in brief, and I fear only in fragmentary form, a survey of the important theories in the history of soils studies. Had I appreciated the true difficulty of the task of discussing them in so brief a space, I should probably not have undertaken it, since I realize how inadequately I have been able, for lack of time, to discuss some very interesting and important points which are involved. I trust that I have succeeded, however, in picturing to you the epochal monuments left by the great figures in the procession of thinkers and workers on soils, from the time of Virgil until today; to interpret for you the successive steps by which they have progressed; to appraise for you the relative importance of the contributions as I view them; and to give you a general idea of the status of our theories today. Above all, I desire to emphasize the fact that now, more than ever before, are we sadly wanting in the best kind of human material for our investigators in soils. The great triumphs of the past have indeed explained much, but, like all investigations, they have served to indicate much more that must yet be known and much that holds in store as great reputations and as many splendid victories as any of the phases of our problems which have been solved. But the problems which remain are of an intricacy and a difficulty probably surpassing anything which we have been obliged to face heretofore. We therefore need men who are second to none in imaginative capacity, in sheer mental vigor, and in training in physics, mathematics, and physical chemistry. To such, I can promise problems of an innate beauty, complexity, and fascination which are not surpassed in any other branch of science. I am optimist enough to believe that, though they have been slow in coming to us, we shall gradually impress such men with the importance of the work and enlist them in a service of the greatest interest to science, and hence of the greatest importance to mankind.