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JUNE, 1911

The Fertility of the Soil

BEING THE

SEVENTEENTH ROBERT BOYLE LECTURE

DELIVERED BEFORE

THE OXFORD UNIVERSITY JUNIOR SCIENTIFIC CLUB

On the 3rd of June, 1910

BY

A. D. HALL, M.A., F.R.S.

DIRECTOR OF THE ROTHAMSTED AGRICULTURAL
EXPERIMENTAL STATION

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TO WHOM IT MAY CONCERN:

THE FERTILITY OF THE SOIL

WHEN I first received the invitation to deliver the lecture I found it very difficult to select a subject. The 'BOYLE' lecturer is expected to talk about his own subject; but I live so far away from the broad highways of science that I could expect very few of my audience to possess any knowledge of the country around me.

I do not admit that any branch of science can be dull if only it finds its competent expositor; yet I cannot hope to excite in others such interest as I have in the drama of the soil, a drama which tells of the strife between plants and their innumerable microscopic antagonists.

Remembering, however, that Oxford is the home of many movements, I shall endeavour to secure your attention by tracing the history of a science, and telling you of the successive stages of our knowledge, instead of giving an account of some rather technical piece of work. I have therefore chosen as my subject THE FERTILITY OF THE SOIL—a title vague enough to allow a little liberty of digression. But when I say I shall talk of the fertility of the soil, I do mean, however, to discuss the reasons why one piece of land, say, should yield large crops, whilst another piece near by only small crops, the differences being so real that a farmer will pay three or even four pounds an acre rent for some land, whereas he will regard other as dear at ten shillings an acre. The title of this lecture suggests

that my historical sketch must begin in the seventeenth century, as no Oxford man of science cares to miss an opportunity of recalling the fact that the birthplace of English science was this University. We need not fear, if we look back to the work of the little group of philosophers resident here at the close of the Commonwealth, that we shall find they ignored agriculture as a low and mechanic art unworthy of the attention of academies. On the contrary, the Thought they stood for demanded above all things a return to Experience, and they recognized that it was in the arts that experience had accumulated, the raw material of science had collected, and problems had arisen which would probably have escaped the notice of men confined to the study or the laboratory. It was a point of view which is worth remembering to-day; for, though the Philistine is so much with us that the enthusiast for learning is tempted to despise all useful knowledge, and attach a high value to a line of study only if it be exceedingly remote from practical ends, the fact remains that it is the men who work in a practical way and on a large scale who come across the most stimulating problems.

BOYLE and his contemporaries, we find, were concerned with two aspects of our subject: with how the plant itself grew and how its increased substance came to be, and, secondly, with the problem of what the soil does towards supplying that substance. The experiment which then held the learned world, the first we find recorded in vegetable physiology, is that of VAN HELMONT, who placed 200 lb. of dried earth in a tub, and planted therein a willow tree weighing 5 lb.; after five years the willow tree weighed 169 lb. 3 oz., whereas the soil when redried had lost but 2 oz., though the

surface had been carefully protected meantime with a cover of tin. VAN HELMONT concluded that he had demonstrated a transformation of water into the material of the tree. BOYLE repeated these experiments, growing pumpkins and cucumbers in weighed earth, and obtaining similar results (except when his gardener lost the figures). BOYLE also distilled his pumpkins and cucumbers, obtaining therefrom various tars and oils, charcoal, and ash, from which he concluded that a real transmutation had been effected: '*that salt, spirit, earth, and even oil (though that be thought of all bodies the most opposite to water) may be produced out of water.*'

There were not, however, wanting among BOYLE's contemporaries men who pointed out that the spring water used for the growing plants in these experiments contained abundance of dissolved material, but at that time the discussion as to the origin of the carbonaceous material in the plant could only be a matter of idle words. BOYLE himself does not appear to have given any consideration to the part played by the soil in the nutrition of plants, but some of his contemporaries carried out important experiments. Some instinct seems to have led them to regard nitre as one of the sources of fertility, and we find SIR KENELM DIGBY, at Gresham College, in 1660, at the meeting of the *Society for Promoting Philosophical Knowledge by Experiment*, in a lecture on the vegetation of plants describes an experiment in which he watered young barley with a weak solution of nitre and found how its growth was promoted thereby. JOHN MAYOW, that brilliant Oxford man through whose early death the young science of chemistry lost so much, went even further; for after discussing the presence of nitre in soils he

pointed out that it must be this salt which feeds the plant, because none can be extracted from soils in which plants are growing. So general had this association of nitre with the fertility of soils become, that in 1675 JOHN EVELYN, of this college, wrote—*‘I firmly believe that where saltpetre can be obtained in plenty we should need to find other composts to ameliorate our ground’*; and HENSHAW, of University College, one of the first members of the Royal Society, wrote—*‘I am convinced indeed, that the salt which is found in vegetable and animals is but the nitre which is so universally diffused through all the elements, (and must therefore make the chief ingredient in their nutriment, and by consequence all their generation) a little altered from its first complexion.’* But these promising beginnings of the theory of plant nutrition came to nothing; the Oxford movement in the seventeenth century was not the real dawn of the science. At the end of the century the human mind, which had sought out of doors for some relief from the fierce religious controversies with which it had been so long engrossed, turned indoors again and went to sleep for another century. MAYOW’s work was forgotten, and it was not until PRIESTLEY and LAVOISIER, DE SAUSSURE, and others, about the beginning of the nineteenth century, arrived at a sound idea of what air really was and what its properties were, that it became possible to build up afresh a sound theory. By about 1840 it had been definitely settled what the plant was composed of, and whence it derived its nutriment: the carbon compounds which constitute nine-tenths of the dry weight (from the air), the nitrogen and the ash (from the soil). LIEBIG had contributed little to the realms of discovery, but his brilliant theories, and the weight of his authority, had

driven this theory of plant nutrition home to all ; a science of agricultural chemistry was founded, and such questions as the function of the soil with regard to the plant could be studied with prospect of success. By this time also methods of analysis had been so far improved, that some quantitative idea could be obtained as to the amount of the various constituents present in soil and plant, and naturally enough the first theory of soil fertility to be framed required that fertility should be increased by the addition of materials which were taken from it by the crop. As the supply of air, from which the plant derives its carbonaceous substance, is unlimited, the extent of growth would seem to depend upon the supply available of the other constituents which have to be provided by the soil.

It was DAUBENY, Professor of Botany and Rural Economy in this University, and the real founder of a science of agriculture in this country, who first pointed out the enormous difference between the amount of plant food in the soil and that taken out by the crop. In a paper published in the *Philosophical Transactions* in 1845, being the *Bakerian* Lecture for that year, DAUBENY described a long series of experiments that he had carried out in the Botanic Garden, where he cultivated various plants, growing some continuously on the same plot, and others in rotation. Afterwards he compared the amount of plant food removed by the crops with that remaining in the soil. The results DAUBENY obtained may be illustrated by the diagrams which show the amounts of nitrogen, phosphoric acid, and potash in some of our Rothamsted soils, compared with the amounts of the same materials removed by the crops they produced. Roughly speaking, we may say that any normal

soil contains the material for 50 to 100 field crops. If then the growth of the plant depends upon the amount of this material it can get from the soil, why is that growth so limited, and why should it be increased by the supply of manure which only adds a trifle to the vast stores of plant food already in the soil? For example, a turnip crop will only take away about 30 lb. per acre of phosphoric acid from a soil which may contain about 3,000 lb. per acre; yet unless to the soil about 50 lb. of phosphoric acid in the shape of manure be added, hardly any turnips at all will grow. DAUBENY then arrived at the idea of a distinction between the *active* and *dormant* plant food in the soil; the chief stock of these materials, he concluded, was combined in some form that kept it away from the plant, and only a small proportion from time to time became soluble and available for food. DAUBENY took a further step and attempted to determine the proportion of the plant food which can be regarded as active. He argued that since plants only take in materials in a dissolved form, and as the great natural solvent is water, percolating through the soil more or less charged with carbon dioxide, therefore in water charged with carbon dioxide he would find a solvent which would extract out of a soil just that material which can be regarded as active and available for the plant. In this way he attacked his Botanic Garden soils and compared the materials so dissolved with the amount taken away by his crops. The results, however, were inconclusive, and did not afford much hope that the fertility of the soil can be measured by the amount of available plant food so determined. DAUBENY's paper was forgotten, but exactly the same line of argument was revived again about twenty years

ago, and all over the world investigators began to try to measure the fertility of the soil by determining as *available* plant food the phosphoric acid and potash that could be extracted by some weak acid. A large number of different acids was tried, and although a dilute solution of citric acid is at present the most generally accepted solvent, I am still of opinion that we shall come back to the water charged with carbon dioxide as the only solvent of its kind for which any justification can be found. Whatever solvent, however, is employed to extract from the soil its available plant food, the results fail to determine the fertility of the soil, because we are measuring but one of the factors in plant production, and that often a comparatively minor one. In fact, some investigators, WHITNEY and his colleagues in the American Department of Agriculture, have gone so far as to suppose that the actual amount of plant food in the soil is a matter of indifference. They argue that as a plant feeds upon the soil water and as that soil water must be equally saturated with, say, phosphoric acid whether the soil contains 1,000 or 3,000 pounds per acre of the comparatively insoluble calcium and iron salts of phosphoric acid which occur in the soil, the plant must be under equal conditions as regards phosphoric acid whatever the soil in which it may be grown. This argument is, however, a little more suited to controversy than to real life; it is too truly logical, and depends upon various assumptions holding rigorously, whereas we have more reason to believe that they are only imperfect approximations to the truth. Still, this view does merit our careful attention, because it insists that the chief factor in plant production must be the supply of water to the plant, and that soils differ from

one another far more in their ability to maintain a good supply of water than in the amount of plant food they contain. Even in a climate like our own, which text-books describe as 'humid', and we are apt to call 'wet', the magnitude of our crops is more often limited by want of water than by any other single factor. The same American investigators have more recently grafted on to their theory another supposition, that the fertility of soil is also determined by excretions from the plants themselves, which thereby poison the land for another growth of the same crop, though the *toxin* may be harmless to a different plant which follows it in the rotation. This theory had also been examined by DAUBENY, and the arguments he advanced against it in 1845 are valid to this day. As, then, we have failed to base a theory of fertility on the plant food that we can trace in the soil by analysis, let us come back to MAYOW and DIGBY and consider again the nitre in the soil, how it is formed and how renewed. Their views of the value of nitrates to the plant were justified when the systematic study of plant nutrition began, but it was not until within the last thirty years that we obtained an idea as to how the nitre came to be formed. The oxidation of ammonia and other compounds of nitrogen to the state of nitrates was one of the first reactions in the soil which was proved to be brought about by bacteria, and by the work of SCHLOESING and MÜNTZ, WARINGTON and VINOGRADSKY we learnt that in all cultivated soils two groups of bacteria exist which successively oxidize ammonia to nitrites and nitrates, in which latter state the nitrogen is available for the plant. These same investigators showed that the rate at which nitrification takes place is largely dependent upon operations under

the control of the farmer ; the more thorough the cultivation, the better the drainage and aeration, and the higher the temperature of the soil, the more rapidly will the nitrates be produced. / As it was then considered that the plant could only assimilate nitrogen in the form of nitrates, and as nitrogen is the prime element necessary to nutrition, it was then easy to regard the fertility of the soil as determined by the rate at which it would give rise to nitrates. / Thus the bacteria of nitrification became regarded as a factor, and a very large factor, in fertility. / This new view of the importance of the living organisms contained in the soil further explained the value of the surface soil. / I remember how, when I was an undergraduate, the Master of Balliol told me that he had just been to DR. GILBERT'S first *Sibthorpean* lecture, and had been chiefly struck by the demolition of the fallacy which leads people instinctively to regard the good soil as lying deep and requiring to be brought to the surface by the labour of the cultivator. This confusion between mining and agriculture probably originated in the quasi-normal idea that the more work you do the better the result will be ; but its application to practice with the aid of a steam plough, in the days before bacteria were thought of, ruined many of the clay soils of the Midlands for the next half century. Not only is the subsoil deficient in *humus*, which is the accumulated débris of previous applications of manure and vegetation, but the humus is the home of the bacteria which have so much to do with fertility. The discovery of nitrification was only the first step in the elucidation of many reactions in the soil depending upon bacteria, for example, the fixation of nitrogen itself. A supply of combined nitrogen in some form or other

is absolutely indispensable to plants, and, in their turn, to animals; yet though we live in contact with a vast reservoir of free nitrogen gas in the atmosphere, until comparatively recent times we knew of no natural process except the lightning flash which would bring such nitrogen into combination. Plants take combined nitrogen from the soil and either give it back again or pass it on to animals. The process, however, is only a cyclic one, and neither plants nor animals are able to bring in fresh material into the account. As the world must have started with all its nitrogen in the form of gas, it was difficult to see how the initial stock of combined nitrogen could have arisen; for that reason many of the earlier investigators laboured to demonstrate that plants themselves were capable of fixing and bringing into combination the free gas in the atmosphere. In this demonstration they failed, though they brought to light a number of facts which were impossible to explain, and which only became cleared up when in 1886 HELREIGEL and WILFARTH showed that certain bacteria, which exist upon the roots of leguminous plants like clover and beans, are capable of drawing nitrogen from the atmosphere. Thus they not only feed the plant on which they live, but they actually enrich the soil for future crops by the nitrogen they leave behind in the roots and stubble of the leguminous crop. Long before this discovery experience had taught farmers the very special value of these leguminous crops; the Roman farmer was well aware of their enriching action, which is mentioned in the well-known words in the *Georgics*, beginning *Aut ibi flava seres*, where Virgil says that the wheat grows best where the bean, the slender vetch, or the bitter lupin had been most luxuriant.

Since the discovery of the nitrogen-fixing organisms associated with leguminous plants, other species have been found resident in the soil which are capable of gathering combined nitrogen without the assistance of any host plant, provided only they are supplied with carbonaceous material as a source of energy, whereby to effect the combination of the nitrogen. To one of these organisms we may with some confidence attribute the accumulation of the vast stores of combined nitrogen contained in the black virgin soils of places like Manitoba and the Russian Steppes.

Though numerous attempts have been made to correlate the fertility of the soil with the numbers of this or that bacterium existing therein, no general success has been attained, because we probably measure a factor which is only occasionally the determining factor in the production of the crop. Meantime our sense of the complexity of the actions going on in the soil has been sharpened by the discovery of another factor, affecting, in the first place, the bacterial flora in the soil, and, as a consequence, its fertility. Ever since the existence of bacteria has been recognized, attempts have been made to obtain soils in a sterile condition, and observations have been from time to time recorded to the effect that soil which has been heated to the temperature of boiling water in order to destroy any bacteria it may contain had thereby gained greatly in fertility, as though some large addition of fertilizer had been made to it. Though these observations have been repeated at various times and in different places, they were generally ignored because of the difficulty of forming any explanation—a fact is often not a fact until it fits into a theory. Not only is sterilization by

heating effective, but antiseptics like chloroform, carbon disulphide vapour, give rise to a similar result. For example, you will remember how the vineyards of Europe were devastated some thirty years ago by the attacks of *Phylloxera*, and though in a general way the disease has been conquered by the introduction of a hardy American vine stock which resists the attack of the insect, in many of the finest vineyards the owners have feared to risk any possible change in the quality of the grape through the introduction of the new stock, and have resorted instead to a system of killing the parasite by injecting carbon bisulphide in the soil. An Alsatian vine-grower who had treated his vineyards by this method observed that an increase of crop followed the treatment even in cases where no attack of phylloxera occurred. Other observations of a similar character were reported, and within the last five years the subject has received some considerable attention until the facts are now definitely established. Here are some illustrations of the sort of results which may be expected. The crop becomes approximately doubled, if the soil has first been heated to a temperature of 70 to 100 degrees for two hours, while treatment for 48 hours with the vapour of toluene or chloroform, followed by a complete volatilization of the antiseptic, brings about an increase of about 30 per cent. Moreover, when the material so grown is analysed, the plants are found to have taken very much larger quantities of nitrogen and other plant foods from the treated soil, hence the increase of growth must be due to larger nutriment and not to mere stimulus. The explanation, however, remained in doubt until it was recently cleared up by Drs. RUSSELL and HUTCHINSON,

working in the Rothamsted Laboratory. In the first place they found that the soil which had been put through the treatment was chemically characterized by an exceptional accumulation of ammonia, to an extent that would account for the increased fertility of the soil. At the same time it was found that the treatment did not effect complete sterilization of the soil, though it caused at the outset a great reduction in the numbers of bacteria present. This reduction was only temporary, for as soon as the soil was watered and left to itself, the bacteria increased to a degree that is never attained in the soil under normal conditions. For example, one of the Rothamsted soils employed contains normally about seven million bacteria per gramme, a number which remains comparatively constant under ordinary conditions. Heating reduced the numbers to 400 per gramme, but four days later they had risen to six million, after which they increased to over forty million per gramme. When the soil was treated with toluene, a similar variation in the number of bacteria was observed. The accumulation of ammonia in the treated soils was accounted for by this increase in the number of bacteria, because the two processes went on at about the same rate. Some rearrangement was effected also in the nature of the bacterial flora; for example, the group causing nitrification was eliminated, though no substantial change was effected in the distribution of the other types. The bacteria which remained were chiefly of the class which split up organic nitrogen compounds into ammonia, and as the nitrate-making organisms, which normally transform ammonia in the soil as fast as it is produced, had been killed off by the treatment, it was possible for the ammonia to accumulate. The

question now remaining was what had given this tremendous stimulus to the multiplication of the ammonia-making bacteria ; by various steps which need not here be enumerated, the two investigators arrived at the conclusion that the cause was not to be sought in any stimulus supplied by the heating process, but that the normal soil contained some negative factor which limited the multiplication of the bacteria. Examination along these lines then showed that all soils contain unsuspected groups of large organisms of the protozoa class, which feed upon living bacteria. These are killed off by heating or treatment by antiseptics, and on their removal the bacteria which partially escape the treatment, and are now relieved from their attacks, increase to the enormous degree that we have specified. According to this theory the fertility of a soil containing a given store of nitrogen compounds is limited by the rate at which these nitrogen compounds can be converted into ammonia, which in its turn depends upon the number of bacteria present effecting the change, and these numbers are kept down by the larger organisms preying upon the bacteria. The larger organisms can be removed by suitable treatment, whereupon a new level of ammonia production, and therefore of fertility, is rapidly attained. Curiously enough one of the most striking of the larger organisms is an *amoeba* akin to the white corpuscles of the blood—the *phagocytes* which, according to METCHNIKOFF'S theory, preserve us from fever and inflammation, by devouring such intrusive bacteria as find entrance into the blood. The two cases are, however, reversed ; in the blood the bacteria are deadly, and the amoeba therefore beneficial, whereas in the soil the bacteria are indispensable, and the amoeba

becomes a noxious beast of prey. Since the publication of these views of the functions of protozoa in the soil, confirmatory evidence has been obtained from various sources. For example, men who grow cucumbers, tomatoes, and other plants under glass are accustomed to make up extremely rich soils for the extensive culture they practise, but despite the enormous amount of manure they employ, they find it impossible to use the same soil for more than two years. Then they are compelled to introduce soil newly taken from a field and enriched with fresh manure. Several of these growers have observed that a good baking of this used soil restores its value again ; in fact it becomes too rich and begins to supply the plant with an excessive amount of nitrogen. It has also been pointed out that it is the custom of certain of the Bourbay tribes to burn vegetable rubbish, mixed, as much as possible, with the surface soil, before sowing their crop, and the value of this practice, though forgotten in European agriculture, is still on record in the books on Roman agriculture. We can go back to the Georgics again, and there find an account of a method of heating the soil before sowing, which has only received its explanation within the last year, but which in some form or other has got to find its way back again into the routine of agriculture.

My time is out, and I fear that the longer I go on the less you will feel that I am presenting you with any solution of the problem with which we set out : 'What is the cause of the fertility of the soil?' Evidently there is no simple solution, there is no single factor to which we can point as *the* cause ; instead, we have indicated a number of factors, any one of which may at a given time become a limiting factor and determine the

growth of the plant. All that science can do as yet is to ascertain the existence of these factors, one by one, and bring them successively under control, but though we have been able to increase production in various directions, we are still far from being able to disentangle all the interacting forces, whose resultant is represented by the crop.

One other point I trust my sketch may have suggested to you. When science, a child of barely a century's growth, comes to deal with a fundamental art like agriculture, which dates back to the dawn of the race, it should begin humbly by accepting, and trying to interpret, tradition. It is unsafe for science to be dogmatic; the principles upon which it relies for its conclusions are often no more than first approximations to the truth, and the want of parallelism, which can be neglected in the laboratory, gives rise to wide divergencies when produced into the regions of practice. The method of science is after all only an extension of experience; what I have endeavoured to show in my discourse is that the traditional practices of agriculture and the most modern developments of science are linked by a continuous thread.

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