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LETTERS

TO THE

FARMERS OF SUFFOLK,

WITH A GLOSSARY OF TERMS USED,
AND THE ADDRESS DELIVERED AT THE LAST ANNIVERSARY
MEETING OF THE HADLEIGH FARMERS' CLUB.

BY THE

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IN THE

UNIVERSITY OF CAMBRIDGE.

1843.

LONDON:

R. GROOMBRIDGE, 5, PATERNOSTER-ROW.
HADLEIGH: H. HARDACRE.

PREFACE.

After a discussion on the "Fermentation of Manures," at a meeting of the Hadleigh Farmers' Club, a notice was entered on their minutes, to request me to give them a lecture, in consequence (as is stated in their Third Annual Report, lately published) of "the subject being considered important, and the want "of chemical knowledge severely felt." I had occasionally been present at the monthly meetings of the Club, and had given them a few popular lectures on scientific subjects connected with Agriculture. The opening sentences of my Address here published, will sufficiently explain the circumstances which led to its delivery. My very slight acquaintance with Chemistry not having qualified me for touching upon any but the more popular parts of the Theory of Manuring, has caused this Address to be more generally and favourably received than it deserves to be. It having been noticed in the local journals, induced me to think I might possibly be able to gain the attention of some of my practical neighbours, and persuade them to perform their experiments a little more systematically than they are in the habit of doing. With this view I began a series of letters, without any very definite plan in view; and I have carried them on for four months, amidst much occupation. They will consequently be found to contain several repetitions and redundancies, and (I doubt not) some crudities, which all who may read them, will agree in thinking, might as well have been suppressed. A judicious friend suggested the propriety of my curtailing a portion of the chemical discussions in Letters 3 and 5; and likewise of omitting the *drollery* (as he calls it) in several of the others, as being unsuited to the sedate character of a regular Pamphlet. But when I began to reflect upon how much might be advantageously omitted, I found I could not tell where to stop, and so I have resolved to let the whole remain, a few verbal corrections excepted. I must be content to lose caste in the eyes of the more discreet, if I have been unfortunate in selecting an improper "time to laugh;" but I shall not fear to give account of the few idle words in which I have indulged, seeing they were intended to serve a special purpose, which might not otherwise have been attained. In catering for the world's wide stage, it may not be amiss to remember that our audience are not all called to sittings in the dress circle; and a little sprinkling of broad farce may serve better to point a moral for some parties, than more sober comedy alone could have done.

J. S. HENSLOW.

Hitcham Rectory, 28th April, 1843.

ADDRESS

DELIVERED AT THE ANNIVERSARY OF THE

HADLEIGH FARMER'S CLUB,

DECEMBER 16, 1842.

You requested me to prepare for the present meeting, some sort of statement or report, on the theory of manuring. I have, in consequence, occupied all the leisure I could command since I met the Club in October last, in studying the opinions of various authors, who have treated the subject scientifically or practically. The opportunity which I possess of consulting publications more immediately devoted to the practice of husbandry, is very limited; and there are several authors, whose opinions I wish I could have seen, but which I have had no means of examining. Among those whom I have been able to consult, I may mention—Davy, Liebig, De Candolle, Sprengel, Payen, Daubeny, Johnston, the author of "British Husbandry" in the Farmer's series of the Library of Useful Knowledge, and the Edinburgh Encyclopædia. I have also read all the papers relating to this subject in the Journal of the Royal Agricultural Society, in the Gardener's Chronicle, and in some of the later numbers of the Gardener's Magazine. These are the chief materials from which I have endeavoured to form my judgment. You all know that I am entirely unacquainted with the practical details of husbandry, and I have no further acquaintance with a dunghill, than what I may have been able to cultivate by the sense of smelling. When I tell you that the most experienced chemical philosophers have pronounced the subject of manuring to be one of the most intricate, as it is one of the most important applications of their science, you must not expect that the exposition I am about to give you, can be otherwise than imperfect. I do not pretend that it will contain a thoroughly digested view of the great variety of opinion which I have met with, but I offer it to your notice, merely as the impression left upon my own mind of the present most plausible view of the subject. I bring to this enquiry, no greater knowledge of chemistry, than what any man of liberal education may be supposed to possess—who has attended two or three courses of chemical lectures in a university, and has occasionly burnt his fingers in attempting to repeat a few of the simpler experiments which he may have seen his instructor perform. I have, certainly, no greater knowledge of chemistry than what I conceive every one engaged in so important a pursuit as agriculture ought to have acquired, as a matter of course, and as part of his professional education, if he would hope to profit by those researches of professed chemical philosophers, which are calculated to throw light upon the science of husbandry, and enable him to turn to the best advantage the means which he has at his command. For myself, I must confess that I have felt somewhat in the position of the cock in the old fable, who, whilst scratching on a dunghill, stumbled on a precious stone, of which he could make no use, and professed that he would rather have found a single grain of barley than every such precious stone in the world. I do not mean that I have discovered any new fact of great value, whilst I was fulfilling the task you set me: new facts are not to be discovered without experiment, or personal observation. But I must declare there are a vast number of precious facts recorded in books, which are not sufficiently known to the generality of practical men. It is for you, gentlemen, to play the part of agricultural lapidaries, and to work up these precious jewels into more marketable shapes, and contrive to turn them into profitable commodities. I am sure that you are not likely to play the part of dunghill cocks, and to despise such jewels when you learn the real value of them.

The general impression left upon my mind by these enquiries is of a mixed character. I have felt cheered at finding the decided progress which has been made, and the good promise held out of further rapid advances; and I have been impressed with a feeling of regret that the want of chemical knowledge prevents so many practical men from either availing themselves of the information already acquired, or of adding any thing of real value to the common stock. I do not say of my countrymen, what the celebrated German chemist Liebig has declared of his, that they have no desire to avail themselves of the information which science proposes. I think I know the spirit of the English nation much better than to say this of any class of my countrymen. I will quote to you the opinion he expresses of the present race of German agriculturists:—"Agriculture has hitherto never sought aid from chemical principles, based on the knowledge of those substances which plants extract from the soil on which they grow, and of those restored to the soil by the means of manure. The discovery of such principles will be the task of a future generation, for what can be expected of the present, which recoils with seeming distrust and aversion from all the means of assistance offered it by chemistry, and which does not understand the art of making a rational application of chemical discoveries? A future generation, however, will derive incalculable advantage from these means of help."—Whatever may be the case in Germany, I much prefer the view which Sir Humphrey Davy took of the prospects of British agriculture in his day, and which appears to be now verifying to a very great extent:—"Science cannot long be despised by any persons, as the mere speculation of theorists; but must soon be considered by all ranks of men in its true point of view—as the refinement of common sense guided by experience, gradually substituting sound and rational principles for vague, popular prejudices. The soil offers inexhaustable resources, which when properly appreciated and employed, must increase our wealth, our population, and our physical strength. We possess advantages in the use of machinery, and the division of labour, belonging to no other nation—and the same energy of character, the same extent of resources, which have always distinguished the people of the British Islands, and made them excel in arms, commerce, letters, and philosophy, apply with happiest effect to the improvement of the cultivation of the earth. Nothing is impossible to labour, aided by ingenuity."

Before I proceed to offer you my remarks, I must express a hope that no one will be induced to adopt any suggestions which I may happen to make, without first experimenting for himself on a limited scale, before he ventures to operate on a large one. My wish is to stimulate to enquiry, not to dictate to practical men what it may be most expedient for them to adopt—I have no desire to direct, but to suggest. I fear that after dinner speeches are not very well calculated for conveying that description of information which is likely to be of real profit to the listener; but as I see some gentlemen of the press present, who are prepared to book my observations, I shall take the liberty of offering them my services in securing a correct report of what I may say. With all due deference to their ability to report me correctly, I know from experience that where persons are not perfectly familiar with the use of technical terms, important mistakes will sometimes creep in; and if my observations are to go abroad, I should wish to avoid any such inaccuracy, lest it might tend to mislead. With this preamble, then, I proceed to my task. But I must first beg you to have a little patience with me, and allow me to proceed in my own way. I find when an old woman with a long tongue has to give her evidence before me as a justice, that it is always best to let her tell her story in her own way, without interrupting her, or attempting to arrive at the conclusion by some short cut. And so, if you should think what I am about to state in the first part of this address, to be somewhat irrelevant, I trust you will bear with me, and perhaps you may find the second part of it a little more to the purpose. For I propose to divide this address into two parts; in the first of which I shall allude to a few of the general principles of nutrition; and in the second, I shall endeavour to shew how it is that manures are rendered serviceable according to those principles.

There exists so intimate a connexion between the different branches of Natural science, that it is impossible to treat of any one of them without alluding to some other. I find that I cannot well explain to you what are the chemical principles involved in the theory of manuring, without referring to the botanical principles upon which the nutrition of vegetables is supposed to depend. I need not, however, on the present occasion refer you to more than two of these principles, and they are of so simple a character that every one may easily comprehend the facts which they illustrate. All those plants which are the objects of attention to agriculturalists are possessed of roots and leaves; the roots are the parts, or "organs" of the plants by which matter in a liquid state is absorbed into the system; and the leaves are the organs by which matter in a gaseous state is exhaled, or discharged from the system. A portion of the crude matter absorbed by the roots, is modified in the leaves by a peculiar process depending upon the action of light,* and is thereby fitted for affording nourishment to all parts of the plant—but nearly all the rest is exhaled in the form of gas, especially the water (under the form of steam) and the oxygen. There are many striking analogies between the functions performed by plants and animals; only plants

*This process is fully discussed in *Letters*, 6, 7, 12, 13.

being more simply organized than animals, their functions are fewer, and not so complicated. Plants are without that internal sack which in animals we call the stomach, and with the practical use of which we have just been giving distinct evidence that we are well acquainted. Now, when animals have received food into this stomach of theirs, it is immediately acted upon by certain juices, secreted for the purpose, by means of which it soon becomes converted into a semi-fluid mass called "chyme." Whilst this chyme is gradually passing through the intestines, it is, in its turn, acted upon by certain absorbing vessels, called "lacteals," which take up from it a peculiar milky fluid which is named "chyle." This chyle is carried along the lacteals into the veins, where it is mixed with the blood; and after passing through the lungs, where certain changes are effected, it is itself converted into blood. The blood which has thus been derived from the food of the animal, contains the materials necessary for the nourishment of all parts of the system, and circulates, as we all know, through the whole body. In different parts of the animal frame there are certain glands, as the liver and the kidneys, &c., which are destined to prepare peculiar secretions from the blood; and to carry off such matters as are not required, or are no longer serviceable to the purposes of nutrition. These are discharged, as well as those superfluous portions of the food which are not essential to the formation of the chyle. In comparing the nutrition of plants with that of animals, we perceive some marked differences as well as some general resemblances in the two processes. Plants have no stomach, and there appears to be no very direct analogy between the first process in their nutrition, and in that of animals. Plants absorb, indeed, through the extremities of their roots, water holding many substances in solution; but it is hardly correct to consider the extremities of the roots as so many distinct mouths; there are no openings at those points, neither does anything in a solid state pass into them; they cannot imbibe even the most impalpable powders; they can absorb fluids only. This action then, seems to be more analogous to that of the lacteals in animals, which absorb the chyle from matter in the intestines. There is also this great distinction to be noted in the materials which afford food to plants, and food to animals, that whilst animals are fed only on matter which has been previously organized, (that is to say, which has formed part of a living being, either animal or vegetable,) plants are nourished by materials which they prepare out of inorganic matter. The water that enters their system at the extremities of their roots, contains a small per centage of various earths, salts and gases, of which I shall say a few more words presently. Having been subjected to a process analogous to the respiration of animals, the result is the formation of that "proper juice" of plants, which may be considered as their blood. So that what had entered in the form of inorganic matter, has now become changed into organic. Though chemists are able to imitate nature in compounding various inorganic substances out of the simple elements, they cannot directly prepare any portion of organized matter. It requires the agency of life, of vegetable life, to effect this in the first instance. Vegetable life is the power—if I may be allowed the expression—which the Creator applies to that engine or laboratory* which we call a plant, for the purpose of combining a few elements into those particular proportions in which they constitute "organic matter." It is upon the continued production of organic matter, out of inorganic, that the very existence of all animals depends. No animal can feed directly upon inorganic matter—upon earths, salts, or gases, &c. When beasts and birds of prey devour the flesh of other animals, they still feed on matter which was originally derived from the vegetable kingdom. So true to the very letter is the general declaration announced in the first chapter of Genesis:—"And to every beast of the earth, and to every fowl of the air, and to every thing that creepeth upon the earth, wherein there is life, I have given every green herb for meat."

A great variety of matters are to be found in different parts of organized beings, whether animal or vegetable; but several of these substances do not appear to be absolutely essential to their constitution. Several have been accidentally introduced with the ordinary food, and do not occur at all times in the system. Others, again, form only a very small per centage of organized matters, although their more constant presence seems to show us that they are absolutely necessary to the formation, or at least to the healthy condition of the individual. Setting aside, for the present, all considerations of such

* See a further allusion to this in *Letter* 13.

substances as these, we find the main bulk of that organized matter, of which animals and vegetables are composed, to be formed out of only three or four elements, united in a considerable diversity of proportions. In the three lectures which I have had the pleasure of delivering to the Club during the past year, I endeavoured to make you acquainted, by experiments and illustrations, with the nature of those four elements which enter most largely—though we cannot say exclusively—into the composition of organized matter. Those elements are carbon, oxygen, hydrogen, and nitrogen. The three first are the principal components of plants, though nitrogen is also essentially present in small proportion: not, as it should seem, in their very tissues, but in some of the organic products formed within them. Nitrogen enters more largely into the general composition of animal matter.

I can imagine there are practical persons who, not being familiar with chemical terms and ideas, may have seen no use in my attempts to explain them to you. Certainly to those who have no desire to become acquainted with the first principles of chemistry, such explanations can be of very little service; but to those who consent to turn their attention to the acquisition of such knowledge, the technical terms which it is necessary to employ, will soon become as familiar as household words; and you will find the words carbon, and oxygen, and hydrogen, and nitrogen, to bring to your recollection as distinct ideas as the words furrow and stetch. When I came to reside in Suffolk, little more than three years ago, I recollect that at our first village ploughing match, I was obliged to enquire what was the difference between a furrow and a stetch; but I have not forgotten what I was then told. Such information, however, is not very likely to be of much use to me in my pursuits, whilst a correct apprehension of such terms as those to which I have alluded, may certainly become of real service to you, provided you wish to advance a few steps towards the acquirement of chemical knowledge.

Since I have said that animals are wholly dependent upon the vegetable kingdom for their nourishment, and since plants contain less nitrogen than animals, we may perceive one reason why animals discharge so large a portion of their food in the form of excrement, without this having ever entered into the composition of the chyle.* I have stated that whilst all animals require previously organized matters for their food, plants are nourished by forming their nutritious juices out of un-organized matter. This assertion may seem to be contradicted by the fact, that the manures which are most frequently applied in culture, are organic. It is also true, that if plants are watered with weak solutions of certain organic products, as gum and sugar, they will thrive upon such nourishment. With respect to organic manures, I have undertaken to show you presently, how it is supposed they serve to nourish plants; but I am not prepared to say how such soluble substances as gum and sugar, when introduced by absorption at the roots, are acted upon by the system.† I do not think it has been clearly ascertained whether they are directly assimilated or not. In the case of parasitic plants, which absorb a nutritious juice directly from those plants to which they are attached, we see an example where it is not necessary that plants should prepare such juices for themselves; and so in the case of the fœtus, which depends for its nourishment on the blood prepared by the mother. But, even if it be possible to nourish plants by certain soluble organic matters, without these undergoing any previous decomposition, in the way we shall presently show to be necessary in all ordinary cases; still we must see that wild plants are never dependent for their nourishment upon the juice of others. They must cater for themselves out of the abundant natural supply of inorganic materials prepared for them. Some plant or other may grow on any spot of the earth's surface below the limits of perpetual snow, and not absolutely in the burning crater of a volcano, provided it can obtain a sufficiency of moisture. It is moisture that plants require in the first instance. Water itself can supply them with two out of the four elements essential to the formation of organized matter. Water is a compound of oxygen and hydrogen; but all water on the earth's surface naturally contains also carbonic acid, which is a compound of carbon and oxygen. This substance is every where present, in small proportion, in the atmosphere, and is readily dissolved in all

* I had not seen Liebig's important Work on Animal Physiology when I made this remark; or it would have been omitted.

† This is really a far more intricate problem to determine than some seem to be aware of. That jumping at conclusions, so commonly to be observed where persons are only partially acquainted with the difficulties in their way cannot be too strongly deprecated.

waters, so that the rains and dews cannot descend upon the earth without bearing with them some portion of it into the soil, from whence it may be absorbed by the roots, together with the water in which it is dissolved. Plants, it has been clearly established, derive their carbon by decomposing carbonic acid. The carbon is fixed, and the oxygen discharged, so long as their leaves are exposed to the influence of light. Although nitrogen forms the greatest portion of our atmosphere, plants do not obtain it directly from this supply. It should be remembered, as a fact of importance to the theory of manuring, that no element, in its free state, is directly assimilated by plants. Those elements out of which they prepare organic matters, are obtained by the decomposition of compound substances. The material which is now considered to furnish nitrogen to plants is ammonia. This is a compound of nitrogen and hydrogen, and is found dispersed through the atmosphere, though in very minute proportion, and in combination with carbonic acid. So that we ought rather to say, that it is the "carbonate of ammonia"—that substance generally called "smelling salts," and not ammonia itself, which is the source from whence plants obtain their nitrogen. There may, possibly, be other substances beside water, carbonic acid, and carbonate of ammonia, from which plants may derive one or other of the four elements of which they are mainly composed; but this has not been so thoroughly and satisfactorily shown to be the case, as with regard to the three above-mentioned. It will be observed that whilst water may supply two, and carbonic acid two of these elements, the carbonate of ammonia is composed of all four.

Having now given you this rapid and imperfect sketch of the mode in which vegetables are nourished out of certain inorganic compounds, whose elements they re-arrange into the form of organic matter, I shall pause a little, before I enter upon the second part of my address, that we may be able to proceed with the more regular business of an anniversary dinner.

I am now arrived at that part of my enquiry which must be considered of main importance to the agriculturist—the manner in which manures may be supposed to supply plants with materials for supporting the functions of nutrition. I propose first, to say a few words on the theory in general, and then I shall notice the composition of, and effects produced, by certain specific manures. By so doing I hope to be able to confirm and support the theory in a way which may make it more thoroughly intelligible.

If we regard manures as the actual food of plants, we must look upon the soil as the stomach of the vegetable kingdom. For it is necessary that certain changes should take place in all organic manures which are placed in the soil, before they can benefit the plants they are intended to nourish. The farmer, therefore, should as carefully watch the condition of this capacious plant-stomach, as a skilful physician would be attentive to the digestion of a dyspeptic patient. He must remember that his crops are not under the sole care of nature, who never would have placed them in the soil or situation where he chooses they shall live. When left to nature, plants, like animals, will range themselves spontaneously over the surface of the earth in those regions and localities, where each, after its kind, may have its peculiar wants supplied by the climate, soil, or other circumstances best adapted to its constitution. Whatever each receives from the soil, that it restores again in the natural progress of decay. But still, whatever a plant derives immediately from the soil, forms, as we have said, a very small portion of its entire bulk; being only that small per centage of ashes which remains fixed, after all the rest which can be dissipated by burning may have disappeared. The main bulk of every plant is derived from water, carbonic acid, and carbonate of ammonia, matters which are primarily derived from the atmosphere, and cannot be considered as any part of the soil, though plants absorb them from the soil after they have found their way into it. When a plant dies and is decomposed, these three ingredients are again formed during its decay, and are restored to the atmosphere. Still no practical man considers that his crops can feed on air alone; but he is careful to supply them with manures of various kind, either solid, or liquid, organic, or inorganic: and experience teaches him that his crops have relished such food. But, for all that, plants do not attack such food directly. Such food is often useless, even poisonous to them in its raw and unaltered state. It must be first digested (as it were,) either before it is put into the soil, or whilst it is beneath the soil; in order that those particular inorganic compounds may be formed out of it, which plants absorb in the way I have described, and from which they form organic matter. We may then, I think, consider the fermentation, putrefaction, and decomposition of organic manures, as a substitute for digestion, in the feeding of plants, when we compare their functions with those of animals. By this process, the elements composing an organic manure are restored to the condition of inorganic matter, and then they combine afresh, to form those inorganic compounds from which plants prepare their "proper juices." As for the several inorganic matters found in the ashes of plants, the presence of some of them is no doubt essential to the healthy condition and even to the existence of particular species; and therefore it is quite necessary that they should be restored to the soil. In many cases it is likely even more necessary that these inorganic matters should be replaced in the soil, than those organic parts of certain manures which serve to keep up a supply of the four most abundant elements of vegetation. For nature cannot readily restore to the soil the various inorganic matters that may be gradually abstracted by continued cropping; but she can always do so with respect to water, carbonic acid, and carbonate of ammonia—as for instance, during a fallow. When we supply organic manures capable of restoring the three last named substances to the soil, we are only hastening the operations of nature in this respect.

Now, with regard to one of these three substances—water; the aid we thus afford to nature, must be very trifling compared with the supply which she herself so bountifully furnishes. With respect to another of them—carbonic acid; her supply is at all times generous, since every drop of water that penetrates the soil, carries with it a certain amount of this ingredient, in solution.* Still it is very possible that the decomposition of organic manures, under particular circumstances, and indeed, in ordinary cases, does render very effectual aid, in directly affording a large additional supply of carbonic acid for the nourishment of plants. It was considered by Sir H. Davy that this supply of carbonic acid was the principal benefit derived from the decomposition of organic manures; but more recent experiments appear to have placed it beyond doubt, that the regular supply of the carbonate of ammonia is of far greater importance.

* The introduction of Carbonic acid through the leaf is referred to in Letter 13.

Although plants require little of this material for furnishing the small quantity of nitrogen necessary to their growth, yet it must always be present to a certain amount, or they cannot live. Little as they require, and ample as the supply may be for plants growing spontaneously, it appears that in removing crops from the soil, we abstract the carbonate of ammonia retained there more rapidly than nature can readily restore it.* Hence the necessity of furnishing the soil with organic manure, or at least with some material which may afford nitrogen to the ensuing crop. Such being the general theory on which chemists explain the manner how organic manures are considered to act, I shall now proceed to an examination of some of those specific manures which farmers are in the habit of providing for their crops.

That which is generally admitted to be the most important of all manures to the British farmer, and of which he is most careful to obtain a supply, is the common farm-yard manure from which dunghills are prepared. This is composed partly of vegetable matter, and partly of the dung and urine of animals. On putting such manure into the soil, we are evidently restoring the various inorganic matters which were taken from it, and in addition we are supplying it with a quantity of organic matter. When this organic matter is decomposed *(but not till then)* it affords, among others, the three inorganic compounds, water, carbonic acid, and carbonate of ammonia—essential to the nutrition of plants.

It is a subject of anxiety to agriculturists, to ascertain the extent to which they should allow the process of decomposition to be carried on in the dunghill, or whether they need allow the materials to ferment at all, before they are applied to the land. The question is not yet considered to be completely decided. Whatever I have to say on this subject must therefore be viewed as suggestions for further enquiry.

I shall here set aside all considerations of the extent of those benefits which may be afforded by the vast variety of matters to be found in a dunghill after it has been thoroughly rotted; and I shall

* This theory seems liable to objections. May it not rather be said that *artificial* crops need a more liberal supply of food than natural ones—the peculiar *races* under culture being required to furnish a more abundant yield of nutritive matter than the parent stocks when subjected merely to natural influences.

confine my attention to the consideration of a single ingredient, the carbonate of ammonia. Ammonia itself is said to exist frequently in small quantity in the excrement of animals, more especially in their urine. We must, however, look for our main supply of it to the decomposition of a variety of organic products, which are either dissolved in the urine, or more sparingly intermixed with the solid excrement. The whole of such materials, however, form a very small per centage of the entire mass. In urine, for instance, water alone forms above 90 per cent., and of the other materials which make up the remainder, some of them contain no nitrogen at all. In the progress of the decomposition of those products which contain nitrogen, this element is set free, and immediately unites with hydrogen, also set free, and the result is ammonia. But ammonia cannot exist long in a free state in the atmosphere. It enters into ready combination with any acid it may meet with. Now carbonic acid is another of the inorganic compounds, which is formed in abundance during the progress of the decomposition ; and some portions of it enter into immediate combination with the ammonia, and the result is a new substance—the carbonate of ammonia. Ammonia itself is highly volatile, it rises readily into the air and is dispersed. Carbonate of ammonia is also volatile, and escapes in a similar manner, though not with equal rapidity. In its solid state it is a white substance, looking something like a piece of marble ; and I will hand round the table a lump of it for your inspection. You will find that it emits a highly pungent odour, and if any gentleman present is unacquainted with the smell of a dunghill, he may obtain a notion of what it is like by smelling this lump of carbonate of ammonia. Though this substance is so volatile, it is readily dissolved in water, and will then be retained for a considerable time, the evaporation going on very slowly, except the temperature be somewhat elevated. It has been stated that gypsum may be advantageously employed in effecting the decomposition of the carbonate of ammonia, as fast as it is formed in the dunghill. The consequence would be, that we should have the sulphate of ammonia, instead of the carbonate ; and that salt is not volatile. But though plants may be able to obtain their nitrogen from ammonia, or the carbonate of ammonia, it does not follow that they may therefore do so from the sulphate, or any other of the salts of ammonia. This is a subject which chemists have not yet fully elucidated. It seems, however, to be quite certain, that all the salts of ammonia do produce a beneficial effect on vegetation, and therefore it is highly important to secure as many of them as possible, whether by retaining the liquid in which some may be dissolved, or by preventing the escape of such of them as can assume the gaseous state.

Perhaps I may be permitted to allude to a trifling experiment, which any of you can easily repeat, and which may serve to show how the ammonia may be fixed in the state of the sulphate of ammonia, upon decomposing the carbonate with gypsum:—If you place in a wine glass a little powdered carbonate of ammonia, with rather more than an equal quantity of powdered gypsum, (or of plaster of Paris, which is burnt gypsum) you will still perceive the strong odour of the carbonate of ammonia. But if you now add a little water to the mixture, and stir it well, the odour immediately ceases, and a slight effervescence in the glass shows us that a chemical action is taking place; and if we were to examine the result at the end of a little time, we should find the materials in the glass to consist of carbonate of lime and sulphate of ammonia.*

The affinities of lime and ammonia for the sulphuric and carbonic acids are so nearly balanced, that a mere difference in temperature will determine which of the two shall combine with each: and it is a singular fact, that if carbonate of lime *(chalk)* be mixed with sulphate of ammonia, and then moistened, a reaction takes place; and whilst the sulphate of lime *(gypsum)* is forming, the carbonate of ammonia (which is also formed) gradually escapes, till the water becomes pure, with nothing but the nearly insoluble sulphate of lime left in the glass. Without detaining you further with any disquisition on these effects, I recommend you to try such simple experiments as may seem to make the matter clearer to yourselves.

Since the whole amount of the salts of ammonia which may be procured, during the decomposition of the organic matters in a dunghill, depends upon the quantity of nitrogen which these may contain; it is evident that under ordinary circumstances, a portion only of that amount has been preserved at the end of the process—since there has been a constant escape of some part of the carbonate of ammonia,

* This experiment is more formally detailed in Letter 4.

in an invisible form, whilst the process of decomposition was going on. It should seem, therefore, a prudent step to get the manure into the ground as speedily as possible; and perhaps even before the decomposition of the materials has commenced; or at least before it is much advanced. But let us look a little more attentively at this important question before we come to any definite conclusions. Suppose I represent the quantity of nitrogen contained in the organic matter of a certain portion of unrotted manure, by 100. Suppose that this gradually combines with hydrogen, and forms 100 parts of ammonia, and this again combines with carbonic acid, and forms 100 parts of carbonate of ammonia. In such an estimate of the amount of these several materials, I make no allusion to their respective weights —I am merely looking to the relative proportion between the atoms of each substance, and this will be the same in the compounds as in the simple element, nitrogen. Whilst this formation of 100 parts of carbonate of ammonia was proceeding, let us suppose that half of this substance escaped into the atmosphere, and the other half was retained, some how or other, in the manure. We may suppose the moisture retains it; or that it has been decomposed by some acid, as the humic acid, or the sulphuric acid, and that the salts thus formed are dissolved in the liquid parts of the manure. At the end of the process, then, we shall find fifty parts of ammonia, in some form or other, in our rotted manure; the other fifty parts having been lost. Compared with unrotted manure, it is enriched by these fifty parts of serviceable material, whilst there is not an atom of any such in the other. Suppose we now put a certain quantity of unrotted manure into a patch of ground; and leave a like quantity till it has become thoroughly rotted, and put it also into another patch of ground of the same dimensions. In the latter case, we apply, suppose, fifty parts of the fixed ammonia directly to the soil, ready for the crop; but in the former case we have not any. Still, in the unrotted dung we have 100 parts of nitrogen, capable of forming 100 parts of ammonia. Provided, then, we can secure the conditions necessary for effecting the decomposition of the manure after it has been placed in the earth, we shall now have command over 100, and not 50 parts only, of ammonia, in some form or other. But, possibly the decomposition may not commence, or not proceed with sufficient rapidity, to benefit the crop to the same extent as the already rotted manure. Let us suppose, however, that it does begin immediately, and that whilst the crop is growing, 50 parts of the nitrogen have been usefully employed in keeping up a regular supply of ammonia. The unrotted manure will then have done its duty as efficiently, and perhaps more so, than the rotted; and when the crop is removed there will still remain 50 parts of the nitrogen, which has not yet been abstracted. It does not follow that the whole of this can be rendered available for the succeeding crop: for the decomposition will proceed, possibly with an increasing rapidity; so that before another crop can be benefitted by the nitrogen that was left, this may be greatly diminished. I do not mean to say that this can be considered a precise representation of what does really happen in any case: but it may serve to show you the complexity of the subject, and why it is that we cannot always expect so great an advantage from the use of unrotted manure as we might have fancied we should have done.

The process of putrefaction requires a certain degree of heat, of moisture, and the access of air, or it cannot take place. This process may, therefore, be very much advanced or retarded by particular circumstances. Thus, for instance, in a very dry, sandy soil, the vegetable matter of the manure may be preserved for some time, as effectually as the dried plants in my herbarium—here moisture was required. Or, in a stiff clay, the ready access of air may be cut off, and the decomposition, in consequence, proceed very slowly. Lastly, the weather may be too cold, and thus decomposition may be entirely arrested. We all know that in Russia the markets are supplied, during the winter, with frozen provisions, and that these will keep perfectly fresh as long as the frost lasts. Indeed, there seems to be no limits to the length of time which organized matter may be preserved in ice; and I dare say that most persons here present have heard of the entire mammoth which had been embedded in the ice of Siberia, in all probability, for thousands of years, until it was thawed out one hot summer, when the dogs and bears devoured the flesh. The skeleton and parts of the skin are still preserved in the museum at St. Petersburgh.

I have, perhaps, been unwarrantably tedious in these remarks, but I wish to impress upon you how requisite it is that every farmer, or at least every superintendant of agricultural proceedings, should be able to determine, upon correct principles, to what extent he ought to allow his manure to decompose before he makes use of it, in order that he may apply it to the very best advantage. He must distinctly understand that the longer he suffers any organized matter to continue rotting, the less nitrogen he can command for the formation of ammonia and the salts of ammonia. In cases where the greatest good may be expected from using the manure in its unfermented or very slightly decomposed state, there is an objection of a mechanical nature to its application, on account of the difficulty of getting the long straw into the ground. But, surely any mechanical difficulty may easily be overcome in an age of mechanical contrivances. I have no doubt that our Ipswich friends, Messrs. Ransome and May, would readily invent (if such an instrument should ever be required,) some smasher or crusher of long muck, which would masticate this plant-food as effectually as our own jaws have chopped up the excellent dinner we had lately on the table.

Science certainly advises against any long continuance of the process of decomposition, and her suggestions in this respect are backed by a recorded practice of the late Lord Leicester—to whose opinions all practical men will be willing to lend most serious attention. He is stated to have said, "that by allowing farm yard manure to be only half rotted, he found it went twice as far as when entirely rotted." I think I do not mis-state him; and that he also adopted the practice at the suggestion of Sir H. Davy.* I am careful not to give any very positive advice on practical matters, but I shall venture to forego a little of my caution in what I have to say further on the subject of dunghills. Much is yet to be learnt, as to the best mode of preparing and employing farm-yard manure; but I think, that as a general rule of management, farmers should be more anxious about retarding than accelerating its decomposition. Whenever they perceive a great heat arising, and a strong smell escaping, they may know that the chemical action is violent, by

* I find the statement in Sir H. Davy's own immortal work on agricultural chemistry. "I shall content myself with quoting that (viz. distinct proof) which "ought to have, and which I am sure will have, the greatest weight among agri- "culturists. Within the last seven years, Mr. Coke, (the late Lord Leicester,) "has entirely given up the system formerly adopted on his farm of applying "fermented dung; and he informs me that his crops have been since as good as "they ever were, and that his manure goes nearly twice as far." Page 291.

which the re-arrangement of the elementary substances it contains is taking place; and the carbonate of ammonia will be rapidly forming and escaping. They should take precautions to prevent this. Just attention should be paid to mixing the materials as equally as possible, and I will even venture to recommend (till good reason shall be shown against such practice,) that a little powdered gypsum be scattered from time to time over the materials as they accumulate. It can do no harm, and I believe may do much good. Let a sufficient quantity of water be sprinkled over it, to wet the gypsum thoroughly. If the manure is to be kept for any time, it may be covered over with the richest mould, which will retain* some portion of the escaping carbonate of ammonia; and if peat can be procured conveniently, it may be serviceable for the same purpose. But I must not trespass upon you by entering into details of this sort, which you will find better explained by practical writers. I should certainly avoid using lime on the dunghill; and I do not understand the use of covering a dunghill with chalk, which I see is sometimes the practice of this neighbourhood. Those who adopt it, no doubt find it beneficial; but I question whether they would not be equally benefitted by carting the chalk directly on to the land.

If some parts of our enquiry are encumbered with difficulties and uncertainty, there are some points upon which I find both scientific and practical writers appear most cordially to agree, and upon which, indeed I should have thought very little judgment was required. It should be no question with a farmer that he cannot be too careful in the preparation and preservation of his manure. It should at all times be under his immediate inspection and controul. The too common practice of leaving it exposed by the road-side is as impolitic as it is illegal. A proper place should be provided where it may be protected from undue influences of the weather, and where the farmer may secure from waste every drop of that rich brown liquid which I so often see oozing out and running away; but which may

* Practical men must distinguish between the *retaining* a volatile salt like the carbonate of ammonia, and the *fixing* its ammonia by changing it into a new salt (as the sulphate of ammonia) which is not volatile. Although the carbonate of ammonia may be partially *retained* in a dunghill by plastering it over with some sort of earth; this effect amounts to little more than *retarding* its escape for awhile. Whereas if it be changed into the sulphate it cannot get away, except in solution among the brown drainings which some liberal promoters of weed culture allow to run off by the road sides!

be considered as the very life-blood of his crops. There are sundry suggestions to be met with in professed treatises on the subject, and I shall not presume to decide which is the best mode of preparing a site for the dunghill. Perhaps a flooring of asphalt might be useful in some cases, instead of stone or brick. If the practice of leaving dunghills by the road-sides, is objectionable on a variety of accounts, so I consider there can be no doubt it is wrong to carry such manure on the land, in the manner generally adopted in this neighbourhood. I see it for a fortnight together scattered over the field in little hillocks. If there are any matters capable of rising in a gaseous state, their escape must be facilitated by such a process, and if there be any liquid matters oozing from the manure, they will penetrate the soil immediately under each hillock in undue proportion. I must incline to the practice of those districts where, (so I have been informed) whilst the manure is carted on the land in rapid succession of load after load by one set of men, another set of men attend to put it under ground as soon as possible.

Having spoken so fully on the subject of farm-yard manure, I shall not attempt to discuss very minutely the differences between this, and other organic manures. The chief objection to farm-yard manure is its bulk, and perhaps the day is arriving when means will be contrived for extracting from it all that is really essential to vegetation, in some very small compass.

I shall now pass on to what must be considered a still more important description of manure than that which I have just mentioned, although, in this country we are far behind other nations in understanding its proper management. I shall quote to you a passage from Sprengel on the subject—"Although there can be no doubt that night-soil is one of the strongest manures, it is still in most places managed with less care than any, and in many altogether neglected: yet the greater or less value attached to it in any country is certainly a proof of the degree in which the agriculture of that country is advanced. Where pains are taken with it, husbandry will be found in other respects excellent; where it is little thought of, the art in general will usually be less perfect."—In these observations I thoroughly coincide. Both theory and an intelligent practice unite in declaring that the mixture of fæces and urine, termed night-soil, is among the forms of animal excrement which abound the most in nitrogen. Besides this, such a material must contain all those inorganic substances to be found in the food of man, some of which may not be restored to the earth with the excrement of other animals, because they may not have formed an essential part of those plants upon which they fed. In the case of wheat, for instance, though we restore the straw to the earth, still the seeds may require that something should be extracted from the soil, essential to their development, which may not be retained in the straw itself. This, in fact, is known to be the case. Such a substance would not necessarily be restored to the soil, upon restoring the straw alone, or even the dung of other animals, which may never be fed on corn. But such a substance would be restored by the use of night-soil. The rapidity with which this manure enters into decomposition, renders its management more intricate; and fully justifies Sprengel's remark. If then, the farmers of England are prepared to avail themselves of what he tells them, I have no doubt they will *soon* find it to be well worth their while to establish some general system of saving every particle of night-soil they can command in towns and villages. I would go further in my recommendation, and though my advice may seem ironical, I believe it to be sound and wholesome advice; I would say to them, care less about the number of cattle you may require for securing an abundance of manure, and feed your labourers well, and better than they are now fed, and you will find it more to your interests to do so. I shall not dilate upon the manner in which night-soil should be prepared; but content myself with the statement that there are means for entirely depriving it of unpleasant odour. It can be no small convenience to be able to concentrate the useful part of twenty-five tons of farm-yard manure into thirteen bushels; and this is about the proportion in which a given quantity of prepared night-soil is said to excel the more bulky material. The liquid portions of animal excrement contain the greatest quantity of nitrogen; but the effects they produce are more transient, from the rapidity with which they decompose; so that great waste must occur unless they are applied in a very diluted state, and from time to time whilst the crop is growing.* If it were expedient to apply such

* This observation needs limitation. I find it recommended not to dilute urine with more than four times its bulk of water; but see a further reference to the subject in my second Letter.

manure little at a time, and frequently, the effects would probably exceed those of all other kinds. But the management of liquid manures requires tact and experience. Perhaps in places where it is secured from waste by being collected in tanks, the constant presence of gypsum would be found very serviceable.

With respect to other organic manures of animal origin, I need say scarcely any thing—they are all serviceable, and not a particle of them should ever be wasted. Every farm-yard might have some pit or general receptacle for all kinds of offal, blood, feathers, dead rats, mice, or whatever else is of animal origin. By covering such matters with earth (and again I would add, a little gypsum,) and mixing with them some vegetable refuse, a rich compost might be prepared of great use in the good cause.

There is one description of animal manure upon which I wish to make a few remarks. Every one is aware of the value of bone manure, of which such immense importations have taken place of late years. The effects produced by fresh bones must be considered of a two-fold character—one depending upon the decomposition of the organic matters in the bone, and the other on that produced by the action of the earthy or inorganic matter, which is chiefly phosphate of lime. Those who have never seen the experiment tried, will be surprised to find how large a portion of every bone, and even of the teeth, is composed of animal matter. I have here prepared two bones from a leg of mutton, which I will send to the right and left round the table. They have been steeped for a few hours in dilute muriatic acid, which has extracted every particle of earthy matter, without very materially altering their general appearance. But if you take them in your hand, you will find they are as flexible as if they were made of leather. This explains the use of bones in the manufacture of glue, or in making soup; so large a quantity of animal matter must be serviceable in its decomposition in supplying ammonia. Intermixed as it is with the earthy parts of the bone, it decomposes very slowly; and traces of it may be found in the fossil bones which have been buried in caves and dens of extinct animals which perished thousands of years ago. But when all this animal matter has been extracted from bones, whether by boiling, or burning, or by long decay, the earthy matter that remains is also a most valuable manure. It is chiefly composed of phosphate of lime, a substance, or at least one of the materials of which, seems to be very generally, if not universally essential to the structure of plants, or the perfecting of their seeds. Very little of it, however, is taken up by each individual plant. The same substance forms the greater part of that particular form of dog's excrement, called "album græcum," of the shells of crabs and lobsters, &c. It is highly advisable that all such matters should be added to the compost pit. Some simple contrivance may be employed for breaking them into small pieces, or for reducing them to powder. Perhaps something on the principle of a stamper—a vertically-placed beam of timber with an iron shoe, which may be raised and then allowed to fall on the materials to be crushed. It should be remembered that different animal manures enter into decomposition with very different degrees of rapidity. Those which are soft and juicy readily decompose, whilst such substances as horn, hair, feathers, and woollen rags, are sometimes not thoroughly decomposed till after the lapse of several years. These latter are generally much the richest in nitrogen.

With respect to organic manures of vegetable origin, I shall say but very little. They contain little nitrogen compared with animal manures. They ought all to be carefully collected, and added to the compost heap. Even the docks which are now pulled up in full seed, and placed in the middle of the road, I presume for the purpose of disseminating them in all directions, might be collected before they went to seed, and added to the general stock. The more juicy the vegetable matter, the more readily it decomposes; but even the decomposition of the more fibrous kinds may be secured by mixing them with other matters. In foreign countries, farmers seem to make much greater use of manuring with green crops, by ploughing them in before they have gone to seed, than is considered to be expedient among ourselves. Perhaps there may be cases in which attention to this practice would not be misplaced. If a fallow be required for improving the condition of the soil, and not merely for destroying weeds, then I can conceive that a green crop, which may be suffered to grow for the sole purpose of being ploughed in, might be of real benefit. At first sight, it might seem that such a crop could add no more to the soil than it had taken from it. But this is not strictly true. Some portion at least of its carbon has been derived from other sources, and thus an alteration must be effected both in the chemical composition and mechanical condition of the soil when the crop is ploughed in. The roots of the green crop are also employed in bringing up towards the surface, certain saline and earthy matters which lie at some depth, and are thus placed in a more convenient position for the crop which is to succeed.

I shall pass on to the subject of inorganic manures; though I cannot say much on them, after the length of time I have already occupied your attention. These substances are calculated to supply plants with some of those materials which enter into their composition to a much smaller extent than the four elements supplied by organic matter. Some of them, as the salts of ammonia, and the nitrates, may possibly be sources from whence plants are able to derive a portion of their nitrogen; but this is an undetermined question. Some of them seem to be serviceable to one kind of plant, and some to another. In general, I should consider that they ought to be applied in comparatively small quantities, and frequently, rather than much at once. I may remark upon a few of them, which seem to produce a specific effect upon particular plants. This appears to be owing to different plants having the power of selecting, to a certain extent, particular substances from the soil, which other plants growing in the same soil, either do not require at all, or only in much smaller quantity. For instance, all the grass tribe, take up a large quantity of silicious matter, the substance of which flints are composed. This it is which gives the polished surface to the straw of corn and grass. So much of this material is taken up by the gigantic grass, called bamboo, that lumps of it ooze out of the stem like masses of gum, and are contained in the hollow parts between the joints. I will send round for your inspection some fragments of this curious substance. Gypsum, again, is found abundantly in the plants of clover, and others of the same natural family. It is stated that as much as three or four bushels may be obtained from the plants which have grown on one acre. Where a soil does not contain gypsum, we may reasonably suppose that clover will not prosper; but the manner in which gypsum should be applied to the soil, admits of further enquiry. It is stated to produce most remarkable effects in some places by being merely scattered on the soil itself; whilst in other instances it produces no effeet unless it is scattered over the leaves of the plant in the form of plaster of Paris, or burnt gypsum. The nitrate of soda seems to be decidedly useful in invigorating the grass tribe (among others,) and in all probability will generally be found to increase the produce of a hay field, provided it be applied with other manures. For with all these inorganic manures, it is hardly judicious, in the present stage of our knowledge, to think of dispensing entirely with such as are of organic origin. That nitrate of soda accelerates the germination of seeds, I had an opportunity of witnessing in the effect it produced upon some wheat which I had steeped in it, and which came up more readily, and for a time, grew more rapidly than other seed sown with it. Salt, again, is another inorganic manure, about which there is great difference of opinion. If I should be asked, as a botanist, to what plants I should consider its application likely to be beneficial, I should say at once, try it upon all those which, in their native state, are found only growing near the sea side, or upon the very shore. I name asparagus, seakale, cabbage, all the forms of beet and mangel wurzel, perhaps celery also, as plants which nature shows us need salt for their healthy development, and consequently which might most probably be assisted in those more monstrous states under which culture has brought them. I dare say that most of you are well aware how grateful salt is to all cattle. It seems to be as essential to their perfect health as it is to our own, that they should obtain it with their food, as in fact they generally do to a greater or less extent. If a lump of rock-salt be fastened at one end of the manger, a horse will be found to lick it daily, and we may fairly conclude it is wholesome to him. If salt then is used as manure on grass lands, and it should be found to produce no very decided benefit, yet it is not unlikely that the grass will have become more grateful and serviceable to cattle, simply by its having absorbed some portion of it. A mixture of lime and salt has been recommended as a manure: perhaps chalk and salt would produce the desired effect still better. I know that one valuable member of this club once tried it, and succeeded in giving his field a coat of hard mortar, which he was afterwards obliged to scrape off again. But possibly he was in too great a hurry: for when lime and salt are left to the long continued action of the atmosphere, I can easily suppose that a carbonate of soda, and a muriate of lime will be the result—two salts which are said to have been found beneficial, and both of which are soluble in water. But I must really desist from further comment. What I have been saying will, I hope, assist in convincing the members of this club, or any others who may have patience to read this long-winded address, that practical men ought to be better acquainted with some of the leading principles of chemistry, than they generally are. Such knowledge is not merely required of them for the purpose of directing their own concerns; but without it they cannot hope to make chemists acquainted with the results of their practice in a form and shape which may be available for scientific purposes. A celebrated French Botanist, De Candolle, has pointed out the manner in which persons engaged in different departments of science, may co-operate in elucidating the general theory of vegetable physiology. On referring to Agriculturists, he is very particular in pointing out the necessity of their being far more accurate than they generally are in their details. I shall quote to you his remarks.—"The radical fault which so much detracts from the use that might otherwise be made of the experience of practical men, consists in the too frequent absence of all comparative experiment. I mean of such as are rigorously comparative. We read daily in works of husbandry, and we hear repeated in conversation, accounts of some particular process, which is pronounced to be good or bad without any reference to an exact comparative result. What a multitude of boasted processes we meet with in agricultural journals, which in reality can neither prove beneficial to practice, nor serve to throw any light upon theory. The first step towards curing this evil, is to bring practical men to understand that a single trial never proves anything. It is much to be wished that the host of agricultural and horticultural societies which now cover the face of Europe, would determine never to pay attention to the results of any but comparative experiments, detailed under a precise form of numerical expression." Provided agriculturists will turn over a new leaf in these respects, it is from them that science might expect to derive a considerable mass of facts by which she may hope to improve our knowledge of the laws of vegetation, and bring the art of agriculture to the dignity of a science. Sir John Herschel in his Discourse on Natural Philosophy(a book which every one should read who wishes to know what science means,) has some excellent remarks upon the possibility of almost every one who chooses it, co-operating in some way or other for the advance of knowledge. "It is an object of great importance to avail ourselves as far as possible of the advantages which a division of labour may afford for the collection of facts, by the industry and activity which the general diffusion of information, in the present age, brings into exercise. There is scarcely any well informed person, who, if he has but the will, has not also the power to add something essential to the general stock of knowledge, if he will only observe regularly and methodically some particular class of facts which may most excite his attention, or which his situation may best enable him to study with effect." May I then advise you to omit no opportunity of keeping an exact register of all the positive facts you can obtain in the cultivation of your crops. I would say register, register, register these facts, and in the end you will find that such a proceeding will be serviceable to yourselves in particular, and to science in general.

I shall here close my remarks on the subject you have requested me to look into; but before I sit down, I shall venture to say a few words upon another subject on which I feel myself much more qualified to give an opinion, than upon how your crops should be manured. There is a description of culture which requires its special manure, and in which I conceive you are as deeply interested as in any which you carry on in the fields. You have the proper cultivation of your labourers to look to. This is not the place, nor is this a befitting occasion for me to appeal to you on any higher grounds than mere worldly policy, for recommending attention to their moral, intellectual, and social condition. One of the best manures which you can provide for the description of culture I now allude to, is to secure your labourers constant employment. I shall not enter upon the wide field which this question embraces; but I put it simply to you as a matter of worldly policy to do so. I am no prophet; but it needs no prophet to foreshow you what will certainly come to pass, if your labourers are thrown out of employ. If profits are to depend in future upon increased produce, and not upon high prices, then must there be an increase of general intelligence among your labourers, to enable you to take advantage of improved methods of culture; and there must be increased labour also to carry them out. I recommend to your serious attention that glorious maxim of the wisest of earthly monarchs:—"There is that scattereth and yet increaseth; and there is that withholdeth more than is meet, but it tendeth to poverty."—Prov. 11. 24.

TO THE

FARMERS OF SUFFOLK.

LETTER I.

Gentlemen,

As our County Press has wished me to believe that my late Address to the Hadleigh Farmers' Club, on the Theory of Manuring, contained matter of interest to the agricultural portion of their readers, I venture to address myself to you, in general. You may remember that I did not profess to point out anything *new*: and that I merely attempted to draw a few inferences from recorded facts and the opinions of others. My object was to diffuse information, for the sake of some who might not be aware of the importance of a correct Theory of Manuring, or might be unwilling to believe that science can ever be expected to do much for the improvement of practice. Two or three of my practical friends have since then communicated with me on the subject; and certain enquiries and expressions in their letters have inclined me to think, that if I were to put on paper a few remarks upon some points more or less connected with your important pursuit, I might probably stimulate some among you to make further inquiry; by showing you what it is that science wants you to attempt, in order that she may be put into possession of a sufficient number of positive facts, for improving and perfecting her theories. I am told that very few practical farmers are sufficiently instructed to comprehend, or to profit by, the information which science puts before them, and in the excellent publication called the *Gardener's Chronicle*, this very week (published December 31) I find a confirmation of this statement. "It is "certain (says the Editor) that among the aids which cultivators "must now look to with most anxiety, is that of chemistry. It is "evident to all who understand the principles of tillage, whether in "gardens or fields, that the dear old empirical rules of action are "inadequate to the wants of the world; that if we are not to starve, "or be otherwise ruined, we must have more out of the land than "our fathers had; and that the common modes of cultivation will not "give us more. This is as plain as the sun at noon-day. Under "such circumstances, what is it that we English do? Why, our "Agricultural Associations talk of chemistry as a fine thing—listen "to lectures on chemistry, often, we fear, without understanding "them, and are contented to rest there, &c. The truth is, that "English education—including that of Cambridge and Oxford—does "not enable men to appreciate the value of such enquiries."

The misfortune of that incompetency of which the Editor here speaks is, that it not only disqualifies persons for availing themselves of every important discovery in science which may bear upon their practice, but frequently most fatally misleads them in their very desire to do so. In an account which I published twenty years ago, of the geology of Anglesea, I traced on the map a particular district under the name of the "Coal-measures," applying this term in its strict geological meaning, to a particular series of our strata, in which the principal coal-fields of England are situate. But some zealous speculators chose to fancy that coal must necessarily be hid somewhere in a district with so inviting a title; and, consequently, they very foolishly threw away their money in searching for it in places where any geologist would have told them they might have spared themselves the cost. I was informed that if I had visited the Island shortly afterwards, I should have run some risk of being ducked in the Menai. Now, I have no desire to run any risk of having my nose rubbed in a dunghill; if any one of you should so far either chance to misinterpret me or to trust me, as to be induced to put yourself to fruitless expense. I had rather find you all sceptical and over-scrupulous, than hasty and over-confident in adopting any suggestions of mine. *You must experiment for yourselves!* If you do not, you may yet be fifty years in determining some point of importance to you, which might be settled in fifty months or in fifty days.

I will tell an old college acquaintance, and worthy friend of mine, that he must *disprove*, and not *dispute* with me, Liebig's suggestion to add gypsum to your dunghills, if you wish to retain that important, but invisible ingredient, Ammonia,—which is continually escaping from them, and upon which plants mainly rely for one of those elements absolutely essential to their growth. My friend chooses to quiz me for having lately drawn attention to Liebig's suggestion, as though he did not value that tact which a knowledge of Natural Science confers upon her votaries, for making suggestions of this kind. " Talk of our making comparative experiments, (he thus writes " to me) and then tell us to mix gypsum in the manure heap! In " 1860, some such paragraph as this will appear :—' So, the absurd " practice of mixing gypsum in our manure heaps, for which no as- " signable reason can be given, took its rise from an incidental hint " given by a learned Professor, now no more.' "

My friend well understands how a mathematical education confers a power of comprehending a variety of natural phenomena, in a way which no popular view or exposition of them can possibly supply; and why should he refuse to an eminent chemist the right of expecting that the suggestions of his science should be fairly and fully tested? But I know my friends energy too well, not to believe that he intends trying the necessary experiments for testing Liebig's conjectures. In fact, he has just bought some gypsum! Only, I don't mean that he shall have all the credit to himself; and therefore, I shall challenge every farmer in Suffolk to compete with him. If only fifty among you can be found willing to accept the challenge, I shall hope to see the scepticism of my friend set at rest, by proof or disproof, I care not which, within as many months, as he has suggested years for the possible duration of my mortal career. My challenge is this—

The best comparative experiment for testing the value of Liebig's suggestion, that Gypsum should be added to manure heaps to fix the Ammonia. This experiment to be tried by every farmer in Suffolk who feels any interest in the progress of Agriculture.

The mode of trying the experiment should be as follows :—Two dunghills are to be prepared, as nearly alike in all respects as it is possible to make them; one with, and the other without, the addition of gypsum. Two separate and equal portions of the same field are to be manured with these. I shall say nothing about the respective quantities of the materials, or the time they are to be allowed to rot. A little variety, among the numerous trials which I expect to hear of, will be useful, and indeed necessary, to the speedy determination of the important problem to be solved. Let the gypsum be sifted over the several layers as they are deposited in one of the heaps. I would suggest there should be about enough to just cover the surface, without placing it on very thick.* The returns should give precise information of the following particulars, and any others that may suggest themselves. I shall be happy to report on them before this day twelvemonth; and I will plead for the thanks of all the Agricultural Societies in Suffolk being given to every contributor:—

1. The quantity of straw used in each heap.
2. The quantity of animal excrement.
3. On what day each was begun.
4. On what day each was completed.
5. On what day they were carried and deposited in the soil.
6. The number of loads, and weight of each.
7. The extent of land manured by each.
8. The quantity of gypsum used to one of the dunghills.
   N.B.—A comparatively small quantity is all that can be required.
9. Any difference in the coming up and appearance of the crops, on each piece of land.
10. When each arrived at maturity.
11. The *precise* amount of produce.
    N.B.—It is needless to reply to this question by guess or estimate. It must be answered by the *scales*.
12. Any peculiarity in the qualities of each produce.

Although these particulars may require nothing more than an ordinary degree of intelligence, and a little industry to note them, yet, if they are correctly stated by about 50 experimenters, without any attempt to coax the results into accordance with previous notions and prejudices, they may determine a very important problem in the present state of agricultural science.

Your obedient servant,

J. S. HENSLOW.

P.S.—If the farmers of Suffolk consider an occasional letter from me likely to be of service to them, perhaps they will endeavour to

* I have here omitted to say that the Gypsum should be well wetted.

prevail upon the editors of the several journals they may happen to take in, to copy from that one which I take in myself. The editors will not expect that I should furnish more than one copy of M.S.; and having ascertained from the editor of my own paper that he is willing to insert any communications of the sort, I shall send them to him.

Hitcham Rectory, January 7th, 1843.

LETTER II.

GENTLEMEN,

I stumbled yesterday upon the following notice in a newspaper, by whom sent I know not. Whether the suggestion it contains, of using common salt for fixing ammonia may be of equal value with that of Liebig, which I laid before you in my first letter, *it is for you to determine.*

"MANURE.—It is well known that in a close stable, where there are a good many horses, there is a very pungent smell affecting the nose and eyes, more particularly when the stable is being cleaned out. This smell is occasioned by the flying off of ammonia, which is the very essence and value of manure, and which volatilizes or flies off at a very low temperature—even the warmth of the dung in a stable will send it off, and it goes off in great quantities by the common heat of the dung in a farm-yard, whether thrown up in heaps or not. There is, however, a very cheap and simple remedy for this. Before you begin to farm out your stable, dissolve some common salt in water; if a four-horse stable, say 4 lbs. of salt, dissolved in two buckets of water, and poured through the nose of a watering pan over the stable floor an hour or so before you begin to move the manure, and the volatile salts of ammonia will become fixed salts from their having united with the muriatic acid of the common salt, and the soda thus liberated from the salt will quickly absorb carbonic acid, forming carbonate of soda; thus you will retain with your manure the ammonia which would otherwise have flown away, and you have also a new and most important agent thus introduced, viz., the carbonate of soda. As this is a most powerful solvent of all vegetable fibre, and seeing that all manures have to be rendered soluble before they can act upon vegetation, it will be at once apparent that the carbonate of soda so introduced must be a most powerful and valuable agent. If one cwt. of salt were used for these purposes every week for each 100 acres of land under cultivation, it would be quite ample to sprinkle frequently all the feeding places and the farm-yard; for the latter the salt may be sown by the hand in rainy weather. The weekly cost would be, say 1s. 6d. to 2s. 6d. The advantage to be derived from this simple measure is very great indeed, and can only be known by actual trial. Suppose the cost to be 2s. per week, the advantage may be safely estimated at ten times that amount."

In this account I observe, what appear to me, some inaccuracies. It may be true that a little free ammonia does escape in a stable; but I believe that chemists teach us it is rather the "carbonate of ammonia" which we smell there than ammonia itself. Ammonia cannot long exist in a free state wherever carbonic acid is present; and this acid is everywhere present in the atmosphere. Moreover, whilst ammonia is forming from the decomposition of certain organic compounds in the dung and urine of animals, carbonic acid is also forming abundantly, and will instantly unite with the ammonia. The abuse or mis-use of terms is too common, even among some of your best agricultural writers. This is very unwise. Precision in the application of technical terms is the very soul of science. But I have found chapters on the use of lime in agriculture, where lime is treated as existing under a variety of forms, when all the while the author is speaking of so many perfectly distinct mineral substances, of which lime happens to be one of the constituents. Nothing, again, is more common than to hear mention of a soil containing lime, or mild lime, when it is the "carbonate of lime" which is intended. Some will think me trifling in this matter, but really it is of great importance. The action of a particular substance may be very different in an un-combined state, from the action of another substance of which it may form a constituent part. For instance, both sodium and chlorine would act intensely upon the human frame, and rapidly destroy life if received into our stomachs; but when chemically combined, these substances, united with water, form common salt, one of the most harmless and most necessary articles of life. We might just as well call salt a form of sodium or of chlorine, as carbonate of lime a form of lime. We might as well call our flesh a form of carbon. Unless attention be paid to the application of terms, according to their strict meaning, a confusion of ideas is always to be dreaded. Thus, in the extract I am noticing, I presume we ought rather to consider that the carbonate of ammonia and the muriate of soda have mutually decomposed each other, than that the latter was decomposed by ammonia, and the soda afterwards united with carbonic acid. The result of such mutual decomposition would at once lead to the formation of muriate of ammonia (sal ammoniac) and carbonate of soda (commonly called soda). Now muriate of ammonia is described as being beneficial to vegetation, but what relation it may bear in this respect to the sulphate of ammonia, that form in which Liebig directs you to fix the ammonia, I am not prepared to say; *this must be left for yourselves to determine.* The author of the above communication directs attention more particularly to the carbonate of soda, which has, I believe, been found very serviceable on some lands; and I remember that I alluded to it in my address to the Hadleigh Club. But I must beg leave to doubt the explanation which he gives us of its mode of acting. It is stated to be "a most powerful solvent of all vegetable fibre;" whereas I have experimentally ascertained that we may preserve both animal and vegetable substances in solution of carbonate of soda, much better than in most other chemical salts. The precise manner in which the alkaline salts—of potash, soda, and ammonia—act on the vegetable system is at present an undetermined point; but that their action is more or less beneficial appears to have been fully proved. Provided, therefore, our informant is correct in saying that salt fixes the ammonia, his *advice* may be considered valuable, though his *theory* should turn out somewhat premature.*

* This question appears to be put in its proper light by Dr. Daubeny, in a letter appended as a P. S. to letter 9.

I shall here mention a little experiment I have lately tried, as it bears upon the subject I am discussing, though it absolutely proves nothing that was not previously known: because I was not careful about making the necessary *comparative* experiments essential for enabling me to draw positive conclusions. Two quarts of urine were saturated with salt and allowed to stand for a fortnight, when two quarts of water were added to them. A strip of grass in a grass plot which is kept constantly mown, in front of my house, was now watered with the mixture, on the 13th of December last. In about eight or ten days a visible effect had been produced, the strip became greener than the rest, and at this moment is very distinctly marked by its darker colour. I have another strip which was watered with a weak solution of nitrate of soda on July or August last, which is also of a darker green than the rest of the grass. No effect has been produced upon a strip that was watered with a solution of common salt at the same time. In these experiments I merely wished to see whether these solutions would produce any effect; and not to estimate the precise effect. But I am so satisfied with the effect produced by the urine, that I am intending to have an underground tank for the reception of all liquid "*rejectamenta*," such as soap-suds, scullery water, and urine. I think of dividing my tank into two compartments, and allowing the liquor to collect alternately in one or the other; and then, in the first week of every month, to clear out the compartment which has been filling, and carry the contents over a grass field, or apply them to the garden. Whether I shall place gypsum or salt in the tank will be for future consideration. I have been informed by practical men in this county, that the use of liquid manure has been tried and abandoned in several cases. Why it was not considered expedient to persevere I know not. The effects which it produces must be expected to be more transient than those of solid manure, and especially so on stiff soils, which may not readily imbibe it; but perhaps it may still be worthy your consideration, whether it would not be expedient to apply it, in certain cases, *very much diluted*,* and with frequency. But, however unadvisable

* I have since ascertained the probable cause why the use of liquid manure has been abandoned in this neighbourhood. It was collected in tanks where it became too much diluted by the undue access of rain-water. It was *mismanagement*, and no fault of the theory, which brought it into disrepute.

such a course may be thought for arable land, perhaps it may be recommended for pasture. It is the liquid parts of manure which are richest in materials suited for generating ammonia. It is to me a continued cause of surprise to witness the supineness of some of my neighbours, who persist in adhering to the bad and proscribed practice of preparing their dunghills by the roadside—a practice which not only scientific enquirers but all intelligent *practical* writers have denounced.

The preparation of manure should be watched and attended to at home, as carefully as the food you prepare for your cattle. My friends of the vegetable kingdom like good food as well as your stalled oxen; and they will not be put off with half allowance, and yet yield you double measure, you may depend upon it.

Your obedient servant,

J. S. HENSLOW.

Hitcham Rectory, January 14th, 1843.

LETTER III.

GENTLEMEN,

Mr. Bree* has correctly asserted that two comparative experiments *alone*, such as those I have suggested to you, cannot afford

* This reference relates to the commencement of a discussion, opened by this gentleman in the newspapers, on the improbability of gypsum acting in decomposing the carbonate of ammonia *under the circumstances of the case* I think it will be sufficiently understood, by Letter 5, what was the character of this discussion—one of perfectly fair argument between my friend Mr Bree and myself; but which we dropped on finding that it was likely to be too chemically abstruse for the purpose I had in view. I ought perhaps to have re-printed Mr. Bree's letters, and would have done so, if I had not been fearful that my pamphlet may already be considered too long.

conclusive evidence of the utility of gypsum in decomposing the carbonate of ammonia, formed during the decomposition of certain organic matters in a dunghill. It was not from inadvertency, as he supposes, that I proposed to restrict you to the consideration of those two comparative experiments. By referring to that Address to the Hadleigh Club, which has been the occasion of my commencing these letters, you will see that I have noticed the direct action of gypsum upon plants; and also, that I have briefly alluded to the anomalous relations under which lime and ammonia appear to stand with regard to each other, in their affinities for carbonic and sulphuric acid, to which Mr. Bree has likewise referred. But for the present I had rather not enter into these considerations. Let us only get the two experiments I have proposed to you, repeated by not less than fifty experimenters, in different parts of the county, and I think it very possible that we may need no further trials. Should any ambiguity remain, it may easily be cleared up by a few very simple experiments afterwards. I am very anxious, in a first effort, not to involve the question in greater complexity than may be absolutely necessary; and I am afraid that a third heap watered with sulphuric acid would not much assist us. If you are desirous of ascertaining whether free gypsum is capable of producing any effect in the particular soil in which the experiments are being tried, it may be applied at once to that soil, without the manure. If I had supposed that only one or two of you were likely to attempt the solution of the problem I have set you, I should have asked for a dozen different modes of preparing the dung-heaps; and I had thought of proposing that five pieces and not two only, of ground should be set apart; but afterwards, I considered the plan I have suggested would, upon the whole, prove to be the best. Only we are dependant upon *numbers* making the trial, if we are to hope for anything like success. Although I am still of opinion that the challenge may rest as it is framed, for the general sweepstakes; yet, as Mr. Bree has suggested an improvement, (for no doubt it would be one) for you to determine whether some free gypsum in the dunghill may not be producing a specific effect, I will suggest to the *doubly-energetic* among you, a second brace of experiments, viz.: To set apart two pieces of land of the same size as those you propose to manure with our two dunghills, and let one of these be prepared with gypsum, and the other left untouched. If the results should not be needed otherwise, it may be of importance to you to ascertain whether the particular field in which the experiment is made does require gypsum or not.

From a private communication which I have received, I fear I have been imperfectly understood as to the scale upon which it may be considered necessary to carry on these experiments. My correspondent expresses his fears that the necessary care, time, and expense required, will prevent many of you from attempting them. "To "comply with your terms, (he writes) it would be requisite to put "two equal bullocks into two equal yards, equally exposed to the "weather, equally drained, equally littered, equally fed; and this "carried on for three months at least; and the result carried out into "a heap and turned, and left for three months more, and then applied "to equally good land: the coming up and growth of the crop watched "for nine months; thrashed and marketed in three months more; in "all 18 or 20 months. The patience of no farmer would hold out so "long." Now, I certainly had no idea of taxing a farmer's patience with all these minutiæ. In leaving it open to the zeal or judgment of every one to proceed as far as he might choose, I have, perhaps, said too much about noting the time it may take in preparing the dunghills, though I did not suppose this would at most, include more than a few days. I will state, therefore, what may be considered as sufficient for those who have no inclination to go further.*

1. Mark out two pieces of land, each about one-eighth of an acre, in the same field; and settle before-hand how much manure you consider they may require.

2. Weigh out as much straw as may be required for preparing the requisite quantity of manure.

3. Measure out and mix well together as much fresh dung, of any or of all sorts, as may be required.

4. Place the straw and dung in alternate layers; sifting the gypsum over the dung in one heap as already directed, &c., &c.

All this need not occupy more than a good morning's work; though if larger dunghills are to be prepared, the time of preparation should be noticed, on account of the possible escape of the carbonate of ammonia. In those preliminary experiments which chemists or

* In letter 10, this experiment is proposed in a still more matured form.

botanists may attempt for the purpose of interrogating nature, there cannot be too minute or laborious attention paid to all the details; but when the results of such experiments appear to have ascertained some natural law of vegetation, which may be considered as bearing directly on the pursuits of agriculture, then the further experiments which the agriculturist himself is called upon to make, are of a much coarser and less elaborate character; and none of them need be made a tax upon his time, patience, or pocket, to any very formidable extent. An ordinary degree of attention to weights and measures is generally all that will be called for—little more, in fact, than the practice of the market itself requires. But success is mainly to be looked for *in the multitude of co-operators* accumulating a sufficient number of positive facts. All England might be converted into one great experimental farm, if our different agricultural societies would prepare accounts of the exact mode in which some hundred farmers might perform a set of easy comparative experiments at the same time, and send in the results of them. This is what is most needed for accelerating the present jog-trot progress of agriculture into something like a rail-road pace of advancing. A multiplicity of most important questions might then as speedily be settled, as you, gentlemen, farmers of Suffolk, mean to decide whether Liebig is right or wrong in recommending gypsum to you for fixing ammonia in your dunghills.

In concluding this letter, allow me, once for all, to thank the few gentlemen who have already, and those many gentlemen who intend hereafter, to assist in these discussions. The greater the number who enlist under the banners of free discussion the better. I, for one, shall be quite ready to acknowledge any error that may be detected in my statements, or to attend to any suggestions which I may feel convinced are improvements upon them; for you must understand that I am not an accomplished chemist, and that I have no experience as a practical agriculturist, and am therefore very likely to be caught tripping. But let us all agree to dispense with mutual compliments, and preserve our tempers, and truth in the end will not be far from us.

Your obedient servant,

J. S. HENSLOW.

Hitcham Rectory, January 21*st*, 1843.

LETTER IV.

GENTLEMEN,

Before I change the subject, I shall once more address you on the little experiment I have suggested to your notice. I was rather surprised, two or three days ago, to hear a practical agriculturist of this county declare that he was not acquainted with more than one farmer whom he considered *qualified* for undertaking the experiment I have proposed to you. I explained to him how very little was really required, and I hope I convinced him that even the tenant of any common cottager's allotment was quite as well *qualified* for conducting our experiment to a successful termination as the first chemist in the land. A scientific friend also, has written to me, and he says, "As for hoping to get results *registered* by the farmers, if "you succeed, you will either prove yourself a Magician, or else that "your Suffolk farmers are very different from the Shropshire ones "whom I have been accustomed to." Now, I have certainly no desire to be considered any way implicated in the "black art," but I shall most heartily rejoice to find the farmers of Suffolk thus far in advance of the farmers of Shropshire, as to be willing to attend to the suggestions of those chemists who would have them try a few simple experiments for themselves. All the world knows that the husbandry of Norfolk and Suffolk is generally in advance of that of every other county in England; I am afraid we may not say in Great Britain. But this will be very little to the purpose, if the Suffolk farmers have become unwilling to adopt the only sure steps which have *now* become necessary for their carrying to still greater perfection the craft they follow: and indeed, of converting the *art* of husbandry into the *science* of agriculture. In order that I may understand whether you are really inclined or not to co-operate in determining the question I have proposed to you; I shall take the liberty of requesting every one who is so disposed, to inform me of his determination. The penny postage, and the liberal conduct of our provincial journals, afford us such ready modes of communicating, that I do not hesitate to suggest the following arrangement. Let every gentleman who may be willing to co-operate, furnish me with his address by post. I will prepare precise instructions for the conduct of our experiment, leaving blanks to be filled up by each experimenter. Provided I shall have received not fewer than fifty applications for these instructions before the end of February; I will then get them printed, and will forward them by post to all who shall have favoured me with their addresses. In the mean time I will endeavour to persuade some neighbour to allow me to superintend the preparation of two small manure heaps; and I shall thus be able to form a more accurate judgment of the number of pounds of straw, bushels of dung and urine, and pints or pecks of gypsum which may be required for manuring a rood of land. I shall not think it worth while to proceed further in this question, unless fifty at least can be found to engage in it; but I shall hope to engage your attention by some other topic.

Although a knowledge of chemistry and botany is not necessary for any one who may be disposed to try the description of experiments which are called for from agriculturists; yet every farmer who will acquire a few precise ideas on such subjects will find himself in a better position for appreciating the value of these experiments, and will be able to reason for himself on the propriety of performing others. I shall therefore take the liberty of suggesting to those among you who may be utterly ignorant of chemistry, to make a beginning, by trying a little experiment illustrative of the subject before us. On the next market day enter any chemist's shop and ask for

One ounce of muriatic acid (spirits of salt) value 2*d.*

One ounce of sulphuric acid (oil of vitriol) 1*d.*

One ounce of carbonate of ammonia (smelling salts) 1*d.*

Procure also a small lump of pure chalk or marble, value nothing; and a teacup full of powdered gypsum, value about one farthing.

Before I propose the experiment which will serve to illustrate what may *possibly* take place in our gypsumed dunghill, I shall advise two or three preliminary experiments, for the purpose of your acquiring a little insight into the nature of the ingredients I have mentioned above. Take four wine glasses, A, B, C, D, and drop three or four drops of muriatic acid into A and B; and three or four drops of sulphuric acid into C and D. Add two or three times as much water as there may be acid in each glass. Scrape a little of the carbonate of ammonia, and drop it into the glasses A and C: You will perceive a strong effervescence take place. Continue adding more of the carbonate of ammonia till this effervescence ceases. The acids are then *neutralized*, as chemists say, by having combined with a sufficient quantity of the ammonia contained in the carbonate of ammonia. The effervescence was occasioned by the escape of carbonic acid set free, and which appeared under the form of a gas. In glass A we have now a muriate of ammonia (sal ammoniac) and in glass C a sulphate of ammonia. We do not *see* these new compounds, because they are both dissolved in the water; but if the glasses were set aside for a time, until the water could evaporate, we should then find both these salts of ammonia in a solid form.

Next, into glasses B and D drop a little powdered chalk or marble, and a strong effervescence will take place as before. These substances are composed of lime and carbonic acid united; and here also the carbonic acid has been set free, whilst the lime has entered into union with the muriatic acid in glass B, and formed a muriate of lime, a very soluble salt; and in glass D the lime has combined with the sulphuric acid, and formed the sulphate of lime, which is the same thing as gypsum. As sulphate of lime is not soluble except in a very large proportion of water, it will appear in this glass as a dense white powder.

When you have actually *tried* these experiments, and have thought about the changes which have been effected, you will find yourselves in possession of more accurate ideas of what chemists mean by decomposition, affinity, and neutralization, than you would have obtained by merely reading about them.

Having, thus, made these preliminary experiments, let us next attempt one which may serve to illustrate the manner in which Liebig intimates that the escape of carbonate of the ammonia may be arrested, during the decomposition of manure. Whilst dung, but more especially urine, is decomposing, carbonate of ammonia is continually forming and gradually escaping into the atmosphere. But you cannot see this, because the carbonate of ammonia escapes in the form of invisible vapour. If you smell at the pennyworth of this substance you have obtained from the chemist in a solid form, you may be made conscious that it is gradually evaporating, just as a piece of camphor would do. This essence of your pocket dunghill must, therefore, be kept close shut up in a box or jar, if you wish to retain it for any length of time. Ocular proof, as well as nasal, may be obtained, that the carbonate of ammonia is escaping; for if you unstopper the phial in which the muriatic acid is contained, and bring the mouth of it near the lump of carbonate of ammonia, you will immediately perceive dense white fumes floating about. These fumes arise from the muriatic acid (which is escaping in the form of vapour from the bottle) first decomposing the carbonate of ammonia (also escaping in vapour) and then uniting with the ammonia, and the result is the formation of this muriate of ammonia.

I shall now propose the little experiment I alluded to. Put about half a tea-spoonful of powdered carbonate of ammonia into a wine glass, and add about a tea-spoonful of gypsum to it; and stir them well together. Smell the mixture, and you will still perceive that the carbonate of ammonia is escaping. It is evident, therefore, that the gypsum has not yet acted in "fixing" the ammonia. Next add water enough to cover the mixture; stir it up and smell again. You will no longer perceive the odour of carbonate of ammonia. Now, water *alone* will act in *retarding* the escape of the carbonate of ammonia, though it cannot prevent this effect gradually taking place; as you may easily prove by dissolving a little of the carbonate of ammonia in water, and smelling it. But where the gypsum is also present, chemists tell us that a mutual decomposition of this substance and the carbonate of ammonia will occur; And at the end of a certain time you would find in the glass two new substances, viz.: carbonate of lime, and sulphate of ammonia. Here then, you see in miniature what is supposed may *possibly* be effected on a large scale by adding gypsum to a dunghill, provided we keep it sufficiently moist. But this is the very point I wish to see settled one way or the other. I have no decided opinion on the matter; and it is never safe to feel positive of success on a large scale, merely because an experiment succeeds on a small one. The same precautions are needed in agriculture as in the mechanical arts. Many a working model has been contrived which will act to admiration: and yet when the principle it was intended to illustrate, has been applied to machinery, the engineer has been disappointed by something coming into play which he had not foreseen, and he has failed in producing the results upon which he had calculated.

I am afraid that some persons will consider I am descending to matters which are far to elementary for the columns of a newspaper, and that I had better refer those who are ignorant of chemistry to some elementary work on that science. But my desire is to convince those who never opened a work on chemistry, how very little energy is required for attempting to gain some slight degree of information on the subject. I am an old hand at the art of teaching. For several years it was my lot to tutorize men at Cambridge in the rudiments of mathematics; and I know by experience how very necessary it is to place the first, and most elementary propositions, in various lights, before some men can be brought to catch the idea of what is intended. The first few difficulties fairly mastered, an advance afterwards becomes comparatively easy. "*Ce n'est que le premier pas qui coute,*" say the French—"The first step is everything." Let the first step be made to the chemist's shop for the four pennyworth of articles I have named above, and I suspect it will not be the last. But let not the first step be forgotten towards performing the experiment I have suggested, viz., that I be favoured with the addresses of those who are willing to try it.

Your obedient servant,

J. S. HENSLOW.

Hitcham Rectory, January 28th, 1843.

LETTER V.

GENTLEMEN,

I had intended to have turned over a new leaf, by discussing, in this letter, some of the functions of the leaf; but I must trespass once more upon you by a few remarks I wish to make upon Mr. Bree's comments in yesterday's paper. He has stated that Liebig has not recommended the addition of gypsum to *manure heaps;* and also that he himself considers the experiment I have proposed to you will be *useless*, because the carbonate of ammonia and gypsum cannot mutually decompose each other, under the circumstances of the case. I certainly did not turn to Liebig's work when I wrote my letter, to ascertain the precise words under which he had recommended the application of gypsum to farm-yard manure, but the impression which his statement had left upon my mind inclined me to believe that his advice was *general;* and that wherever the carbonate of ammonia might be forming in the materials of which dunghills are prepared, there it might be useful to apply gypsum to decompose it. I had sent to Dr. Daubeny, the Professor of Agriculture and Chemistry at Oxford, a copy of my Address to the Hadleigh Club, after it appeared in the papers, with a request that he would point out to me any errors into which I might very possibly have fallen, owing to my partial acquaintance with Chemistry. In his reply (Dec. 28) he assured me that he saw none, but added this remark; " I may notice that Mr. Pusey, in the last number of the *Agricultural Journal*, states " that the application of gypsum to stable-"yard dung has proved a failure. I know not why this should be so, " and therefore think it deserves to be enquired into further." I had previously written to Mr. Pusey to ask his reasons for the assertion here referred to; and in his reply (Dec. 15) he had stated, "A "farmer wrote to our Secretary desiring him to inform me that he " had used a considerable quantity of gypsum, and had found "it do no good—I did not mean that gypsum would not decompose " the carbonate of ammonia, but that it had not done so in a "farm-yard. Your experiment indicates the cause of failure to have "been the absence of sufficient moisture." The remarks of these gentlemen satisfied me that the question had not received that degree of attention which the interests of agriculture required; and this was the reason which induced me to propose the experiment in the form I did, (Jan. 7)—not as one which had been *suggested* in that form by Liebig, but as one which I thought would test the *general tenor* of his suggestions. I shall quote to you a few passages from Liebig, and Dr. Daubeny, which I consider fully justify my inference. Liebig says, at p. 192, "In *dung-reservoirs*, well constructed "and protected from evaporation, the carbonate of ammonia is re-"tained in solution." "When it (the carbonate of ammonia) is lost "by being volatilized in the air, which happens in most cases, the loss "suffered is nearly equal to one half of the weight of the urine "employed, so that if we fix it—that is, deprive it of its volatility, "we increase its action twofold;" and p. 193, "The carbonate of "ammonia formed by the putrefaction of urine can be fixed or de-"prived of its volatility in *many* ways. If a field be strewed with "gypsum, and then with putrified urine, *or the drainings of dunghills*, "all the carbonate of ammonia will be converted into the sulphate, "which will remain in the soil. *But* there are still *simpler* means of "effecting this purpose; gypsum, chloride of calcium, sulphuric, or "muriatic acid, and super-phosphate of lime, are all substances of "very low price, and completely neutralize the urine, converting its "ammonia into salts which possess no volatility." And p. 194, "The ammonia emitted from stables and necessaries is always in "combination with carbonic acid. Carbonate of ammonia, and "sulphate of lime (gypsum) *cannot be brought together, at common "temperatures, without mutual decomposition.* The ammonia enters "into combination with the sulphuric acid, and the carbonic acid "with the lime, forming compounds which are not volatile, and, con-"sequently, destitute of all smell. Now, if we strew the floors of "our stables, from time to time, with common gypsum, they will "lose all their offensive smell, and none of the ammonia which forms "can be lost, but will be retained in a condition serviceable "as a manure." And Dr. Daubeny, in speaking of the composition of *dung heaps*, thus comments upon Liebig: p. 83. "Hence Liebig advises the addition of sulphuric or muriatic "acid, both cheap substances, to the other materials of the "*dung heap*, which, forming with the ammonia present the "sulphates and muriates of that alkali, would at once prevent any "loss of it by evaporation. If these expedients be not adopted, it "should at least be borne in mind, that unless means are taken to "prevent it, the most valuable portion of the manure is constantly "escaping, during exposure to air and sun, by evaporation, and also "by draining off into the ground; whence, instead of a material "calculated to afford ready supply of nitrogen to the plant, we obtain "an *effete* mass, in which that element is in a great measure wanting, "and which, therefore, can only influence the growth of plants, by "virtue of the phosphoric salts and other fixed ingredients still "present in it;" and p. 84, "The above theory of its use, (viz. of "gypsum as proposed by Liebig) being admitted, we may be en- "couraged to extend its application to other crops besides the "Leguminosæ, and also *to mix it with the dung of our stables*, so as "to prevent the waste of this valuable material, which is constantly "occurring." Now, whether I am justified or not in supposing that Liebig intended to recommend the application of gypsum, and the other materials noticed by him, to your common manure heaps, I still retain my opinion that it will be highly expedient for you to try the little experiment I have suggested to your notice. All your manure heaps contain urine as well as dung; and surely they are as moist as the floor of the stable. No one can deny that a large quantity of carbonate of ammonia is driven off from them during the process of fermentation; and even the solid dung and the vegetable matter assist in supplying some of it. Whether gypsum will serve your purpose or not, is the question at issue. Mr. Bree is positive that it will not; because carbonate of ammonia and gypsum, *in a dry state*, do not mutually decompose each other. If you will refer to my address to the Hadleigh Club, you will see that I expressly stated that the gypsum should be *wetted*. "Let a sufficient quantity "of water be sprinkled over it to wet the gypsum thoroughly." Upon referring, however, to my first letter, I see that I have inadvertently neglected to repeat this caution; and hence I have misled Mr. Bree to suppose that I did not consider the presence of any moisture necessary to secure the decomposition of the two salts. But still, Mr. Bree has further argued, that as gypsum requires four hundred times its weight of water to dissolve it, we could never supply this menstruum in sufficient quantity to the dunghill, to enable the carbonate of ammonia and gypsum to act upon each other. But this, I think, is an incorrect inference. I conceive it cannot be necessary that all the gypsum should be in solution at the same time, and as fast as one portion is decomposed, the water would be able to take up another. If this be not admitted, I can still refer to the known principle, that the mutual decomposition of a soluble and insoluble salt may take place, provided the former be in a state of solution. Thus, the carbonates of potash and soda, in solution, are used to decompose the sulphate of barytes. I read in Ure's *Dictionary of Chemistry*, that in the manufacture of sal-ammoniac, an impure solution of the carbonate of ammonia (obtained from the destructive distillation of animal substances) is passed through a bed of pulverized gypsum, when mutual decomposition ensues, and the result is sulphate of ammonia in solution, and an insoluble carbonate of lime. In this case, the gypsum was not in solution; and why should we suppose it to be absolutely necessary that it should be so in the dunghill? Provided there is sufficient moisture to dissolve the carbonate of ammonia, it seems to me very likely that the mutual decomposition of the two salts will take place. Upon such points, however, I do not feel myself competent to speak positively. Where Liebig asserts that gypsum strewed over a field remains a long while before it is wholly decomposed, I had fancied that he meant to say, this was owing to the very gradual manner in which the carbonate of ammonia was supplied by the atmosphere: otherwise he appears to contradict himself. It was certainly an oversight on my part, in not alluding in my first letter, to the necessity of moistening the gypsum in the experiment I have proposed; and I am under obligations to Mr. Bree for having noticed the omission. I shall not dwell upon the further remarks of Mr. Bree, which are sufficiently interesting to you, as tending to illustrate the use of liquid manure, bût which do not apply so directly to the question at issue. Liebig's remark on this subject is worthy your most serious attention—p. 201. "When it is considered that with every pound of "ammonia which evaporates, a loss of 60 lbs. of corn is sustained, "and that with every pound of urine, a pound of wheat might be "produced, the indifference with which these liquid excrements are "regarded *is quite incomprehensible.* In most places, only the solid "excrements, impregnated with the liquid, are used, and the "dunghills containing them are protected neither from evaporation "nor from rain!"

If Mr. Bree is still unwilling to believe that the experiment I have suggested can be of any service, perhaps he will be so good as to propose to you an experiment for testing some other method suggested by chemists for fixing the ammonia in dunghills. The objection which occurred to me against the use of free sulphuric acid on a dung heap, was the probability of the acid entering into combination with other ingredients tnan the carbonate of ammonia, and thus not waiting till all of this salt had been formed, which might be afforded by the thorough decomposition of the organic materials. However, I shall be delighted to see some of you attending to Mr. Bree's suggestion, as well as some of you to my own. In order that he and I may not be jealous of any preference you may be inclined to show to one or other of us, I propose that all who intend to co-operate should toss up, and let heads or tails decide for each of you, whose experiment he is to try.

I have received a communication from a gentleman,* who informs me that he is a manufacturing chemist, and that he denies the fact asserted in the extract I quoted from a newspaper in my second letter, viz., that common salt can fix the ammonia formed in stables. I am inclined to believe that he may be correct; for, on dissolving common salt and carbonate of ammonia together, I do not find the smell of the latter to be destroyed. If you refer to my letter, you will see that I was merely attempting to correct the informant's *theory*, and not to dispute his *facts;* and that I cautiously remarked. "*Provided*, therefore, our informant is correct in saying that salt fixes the ammonia, &c." I should be prepared to assent more freely to the contradiction I have alluded to, if I had not perceived that Dr. Daubeny, in his lectures, inclines to the same opinion as the anonymous author of the extract—p. 86: "Chloride of calcium, *common salt*, sulphuric and muriatic "acids, phosphate of lime, and other salts, may, it should seem, on "the principles laid down, be substituted when gypsum cannot be

* See P. S. to letters 6 and 9.

obtained." Who shall decide when doctors disagree? This is precisely the sort of question which a person like myself, who am not a professed chemist, is unqualified to decide for you. My object in addressing you is not to dispute with chemists the facts they assert; but to endeavour to put before you the theory which explains their facts, in such a manner as may convince you how necessary it is *that you should experiment for yourselves, with some definite object in view*, and not in that hap-hazard sort of manner in which agricultural experiments are most frequently undertaken. My advice is, that you trust not implicitly to the suggestions of the most celebrated chemists; nor adopt their notions into your practice, without previously making *a set of comparative experiments* for yourselves, in order to test the value of their suggestions. I feel more and more convinced that some plan for securing co-operation, such as I have thrown out in these letters, might be made a most effectual means of greatly accelerating the progress of Agriculture. If you could enlist some first-rate chemist into your service, who should lay down for you precise rules for trying certain simple agricultural experiments, and you would then consent to act together *by hundreds and thousands* in attending to his directions, and in *registering* results, he would soon strike out for you such decided improvements in the art of culture, that your important interests would be able to maintain that state of prosperity in which it is so essential to the general well-being of the country they should exist. If the arrangements of your various agricultural societies were only as complete for securing abundant returns of *comparative experiments*, as they appear to be perfect for exhibiting fat cattle and fine roots, or even for discussing good dinners and promoting good fellowship, I should then hope to live long enough to see the farming produce of Great Britain double that which is now extracted from the soil. If you consider me too sanguine in this expectation, and that my assertions need some qualification, I will, for the present, fall back upon the declaration of the President of the Hadleigh Club, at their last anniversary.—"Though we have to compete with all the world, I firmly believe, "with the aid of science, that the energy of the British farmers will "carry them through every struggle and every difficulty."

Your obedient servant,

Hitcham Rectory, February 2nd, 1843. J. S. HENSLOW.

LETTER VI.

On stripping off the Leaves of Plants.

Gentlemen,

More than one person have fancied that they could increase the produce of their potatoe crops by pulling off the leaves. I have been told by an experimenter that he once pulled all the leaves from his potatoe plants, under an idea that by so doing he could divert the sap into the tubers, and thus greatly increase their size, by not allowing any portion of the nutriment to be wasted in developing such comparatively useless appendages as the leaves. The result of this experiment was a crop of tubers so minute that it almost needed a microscope to enable the disappointed experimenter to see them. The result of this single experiment renders it highly *probable* that it is not advisable to pull off the leaves of potatoes. *Possibly* it may be expedient to adopt such a practice in certain other cases. For instance, it may serve some good purpose to pull off the leaves from mangel-wurzel; or *possibly* it may be useful to strip a peach-tree or an apple-tree of its leaves, in order to divert the sap to the fruit, and thus increase its bulk or flavour. In the case of the mulberry-tree, indeed, it has been long ascertained by the experience of those who rear silkworms in the South of France and elsewhere, that such a practice by no means improves the growth of that tree. On the contrary, such constant pulling off the leaves, which they are obliged to have recourse to, produces a decidedly bad effect upon the health and stature of the trees. They become miserably stunted objects, and would die outright if the leaf-pullers did not take the precaution of allowing a few leaves to remain on the ends of the boughs. It should seem, then, to be sufficiently proved that neither potatoes nor mulberries rejoice in being despoiled of their leaves. But may we safely generalize from no more than *two* facts of this description, and conclude that no plant can be benefitted by abstracting its leaves? There are some opposing facts to be taken into consideration. There are certain plants, those for instance commonly called cactuses, which have no leaves; and there are, moreover, certain agriculturists (so I have been told) who strip the mangel-wurzel of its leaves; which we must presume they would not do unless they were well satisfied the practice was advisable. We have, then, potatoe and mulberry *versus* cactus and certain agriculturists. Ought we not to have recourse to further experiment, in order to test the value of the practice to which I have referred, since it has not been universally adopted, and (as I have also been informed) it is even objected to by some agriculturists as useless or perhaps injurious? Possibly I am under some error as to the extent and nature of this practice. I have been informed that in some places the leaves are pulled two or three times during the growth of the plant, whilst in other districts the practice is to pull them only a short time before the roots are dug up. I have, however, been asked my opinion, as a botanist, what result may be expected from pulling off the leaves of this plant. To which question I can only reply by alluding to those particular functions of the leaf which are connected with the nutrition of plants in general. What is your opinion, Mr. H., said a practical gentleman to me, about pulling off the leaves of mangel-wurzel? May I ask, was my reply, what may be your *motive* for doing so? I did it because my excellent neighbour, Mr. So-and-so, had adopted the practice; and Mr. So-and-so, it appeared, had seen it recommended in some journal. I asked whether the object was to *diminish* the size of the root-stalks; because I did not feel quite sure that this might not be advisable in some cases. But I was immediately informed that this was not to be *desired* in any case. There must, then, be some *expediency* in this practice which more than counterbalances whatever risk may be incurred of diminishing the size or weight of the root-stalks. I have no desire to interfere with any approved practice in agriculture; but it would be exceedingly desirable that science and practice should not be at issue; and as this practice, however modified, must still bear some relation to the functions of the leaf, perhaps you will not object to hear what botanists suppose they have ascertained concerning those functions; and then, possibly, some of you may feel inclined to try a little *comparative experiment** for the

* An experiment of this class is detailed in letter 14.

purpose of determining for yourselves whether their theory is correct or not. I need hardly invite so many experimenters to step forward in this case as for our projected gypsum experiment. An ordinary petty jury of twelve may be considered quite sufficient for adjudicating on such a subject as this; and I suspect they will find they run less risk of being puzzled in doing so, than they have sometimes been when sitting in a jury-box, mystified by the quirks and quibblings of opposing counsel. I shall not, in this letter, enter further upon the subject; but I will endeavour to prepare for you, in my next, some little account of the functions of the leaf, and perhaps I may then suggest an experiment by which you may test the utility of the practice of pulling off the leaves of mangel wurzel. In the meantime I shall be very glad to know to what extent the different factions of anti-pull-mangel-leaf and pro-pull-mangel-leaf practitioners prevail. Even if you should consider this question of too trifling a character for discussion, it may not be wholly unadvisable for me to offer you a few remarks upon the general functions of the leaf, which is so far from ever being an appendage of little or no value, that it is absolutely essential (wherever it is provided) to the health of the individual, to the full development of the tuber or the root-stalk, and to the perfecting of the seed. In leafless plants, a special provision is made for securing the purpose which leaves serve in other cases; the resources of Infinite Wisdom being as multifarious in providing the conditions essential to the well-being of each particular species of plant, as we find them to be in adapting the numerous forms of animated nature to the variety of circumstances under which they are destined to exist.

Your obedient servant,

J. S. HENSLOW.

Hitcham Rectory, February 11th, 1843.

P.S.—A few more last words, if you please, about our Gypsum Experiment. I do not wish to add to the arguments I have already brought forward in urging you to co-operate in attempting this little experiment; but I may state that I have received a second communication from the gentleman to whom I alluded in my fifth letter, and that he has allowed me to use his name in support of his testimony. It is Mr. Burgess, a manufacturing chemist, of Upwell, near Wisbeach, who informs me that he has had great experience in preparing both the carbonate and muriate of ammonia; and he states positively that the carbonate of ammonia in solution will completely decompose gypsum *without the necessity of this substance being first dissolved.* He has suggested the following experiment, which any one sceptical on this point may easily try for himself:

"Take an ounce of powdered carbonate of ammonia, and add to "it half a pint of soft water: put this in a phial and shake it occa-"sionally till it is completely dissolved. Take two ounces and a "half of finely powdered gypsum and add to the above: shake the "whole occasionally for two or three days, and the result will be a "solution of sulphate of ammonia, and an insoluble precipitate of "carbonate of lime."

I have been invited to extend the challenge I have offered you to the farmers of all England, under an idea that I shall not find fifty persons among yourselves who have *sufficient public spirit* to co-operate in the way I have suggested. I cannot believe this. But I can readily suppose that I may not be able to meet with fifty among you who feel sufficient confidence in myself to think it worth their while to attend to my advice: and, if so, I must be content to withdraw my challenge altogether. I should not think myself justified in extending it to all England. Such a proceeding might come with a good grace from a body like the Council of the Royal Agricultural Society, acting under the directions of a Liebig or a Faraday; but I am taxing my own opportunities (to say nothing of my presumption) to the utmost, in attempting to persuade you to help yourselves in that way which seems to me the most likely to secure a rapid improvement in the practice of agriculture; I mean by your *co-operating in performing comparative experiments, with a definite object in view.* It is not I, or any mere follower of science, but it is many among the best informed of your own body, who are daily assuring you of the absolute necessity that now exists for your attending to the suggestions of such chemists as the late Sir H. Davy, Liebig, and others who have directed their attention to agricultural chemistry. All will be of no use, or rather worse than useless, if you do not take the *right method* of testing the value of their suggestions. That method I believe I have pointed out to you, in advising extensive *co-*

operation for making *comparative experiments.* I am still far from having received the addresses of fifty willing to try our gypsum experiment; but there is yet a fortnight to the end of February. Those gentlemen who have already favoured me with their addresses must not take it amiss if they find that I decline accepting a less number than fifty. I am willing to devote time and attention to a useful purpose; but as I consider fewer results would lead to no satisfactory conclusions, I must decline proceeding any further if I find that my advice is not capable of influencing a sufficient number of you to make an attempt so simple and unexpensive as the one I have proposed.

LETTER VII.

Functions of the Leaf.

GENTLEMEN,

The crude sap introduced at the roots consists of nearly pure water, containing only a very small and variable per centage of certain saline, earthy, and gaseous matters in solution. It is a very common notion, and one upon which most erroneous conclusions are sometimes built by practical men (gardeners and others),* that this crude sap is directly employed in the nourishment and development of the various parts of the plant. There is much plausibility in such an opinion. Every one acquainted with the practice of pruning is aware that by cutting away some parts of a plant, he contrives to throw the rising sap into other parts; and he finds that, in consequence of such treatment, these latter parts are better nourished, and become more developed than they otherwise would have been. But,

* I have since ascertained this to be a very prevalent opinion among Agriculturists.

in spite of so plausible an experiment, the crude sap is not nutritious.* We might as well declare that we can receive nourishment ourselves from a weak dose of Epsom salts, or fatten our bodies upon soda water. Living bodies, whether vegetable or animal, can only be nourished and developed by *organic* matter. Now, crude sap may very possibly not contain a single particle of organic matter. The mystery is explained by the fact, which the science of physiological botany has ascertained, that plants are endowed with the peculiar faculty of preparing organic matter for themselves out of the materials which the crude sap contains. They are mighty agricultural chemists, surpassing Liebig himself in their power of effecting combinations and alterations among the elements of material substances. That justly eminent chemist, and many a one less eminent than he, can now manufacture sugar out of brown paper, and bread out of sawdust; and they can beat all the ablest conjurors of the good old times (when alchemy and astrology directed the undoubting faith of wondering admirers), by the marvellous extent to which they can carry their transmutations. But not one of all the chemists whom the world has yet seen, has been able to contrive how he might combine the elements of "inorganic" matter, so as to form out of them a single "organic" compound. They can change one organic body into another, as starch into sugar; but they cannot make either starch or sugar directly from the elements of which they consist. It is to the vegetable kingdom alone that this wonderful faculty belongs: and it is by the leaves of plants that the operation is carried on. By certain processes, which I hope to describe to you (so far as they are at present understood), all the leaves of a plant, whilst they are endowed with life, and *stimulated by light*, are enabled to combine three or four out of the fifty-six elements with which chemists are acquainted, into some one or other of the peculiar substances found in its juices, and distinguished as organic compounds. It may be gum, or sugar, or starch, or some other of the numerous vegetable compounds which the leaves prepare in the first instance; and then, such matters being carried into the system, and undergoing various modifications according to the constitution of different

* It is owing to an *admixture* with nutritious juice, prepared in the leaf and returned in the system, that the rising sap serves to nourish those parts into which it is specially directed.

plants, become subservient to their nourishment and development. The crude sap, then, is merely instrumental in supplying the leaves with the materials necessary for the formation of organic matter: it is the "proper juice" (as botanists call it) manufactured by the leaves, and in which a variety of organic matters are dissolved that forms the real nutritious fluid of the plant, as blood does of the animal. The importance of retaining all the leaves (whilst they are still living) on a plant, is sufficiently evident. Not one of them can be abstracted or injured without the plant being deprived of a certain amount of "power" for generating its "proper juice." A man might as well expect to live without lungs, or a fish without gills, as a plant without leaves. It would be ill-timed for me to dwell, in this letter, upon the important relation which the leaf bears to the animal creation, but it may be permitted me to observe how wonderful is the economy of nature—that all the myriad forms of animated beings, from the most minute invisible animalcule* to the gigantic whale of 90 feet length, are entirely dependent upon the vegetable kingdom for the organic materials of which their bodies are constructed. Among the various functions of leaves, I propose to direct your attention more especially to the two which are immediately concerned in the organization of brute matter. One of these is termed "Exhalation"—a function by which a large portion of the water introduced by the roots, as crude sap, is discharged from the system in a peculiar manner. The other function has been called "Respiration"—from an analogy it has been considered to bear to the breathing of animals. Before I can enter satisfactorily upon an explanation of these two functions, so as to render them thoroughly intelligible to those among you who have never turned their attention to these sort of enquiries, I shall beg permission to say a few words upon one or two of those inorganic substances out of which the leaf prepares the organic matter to which I have alluded. This will oblige me to postpone to another letter the actual discussion of the functions themselves. I

* The following extract from a letter, (Feb. 22,) of my friend Mr. C. May; may afford some idea of what observers have ascertained about many microscopic animalcules. "I have some new fossil infusorial shells, of which I measured a "beautifully corrugated one by means of my new micrometer. It was 1-1250 of "an inch long and 1-6000 of an inch wide; and taking the thickness to be the same "as the width (and it is probably less) a cubic inch will contain 45 billions of "them."

think, however, you will find my proposition of greater ultimate advantage than if I proceeded at once to that discussion. I shall, therefore, entitle the remainder of this letter

A Digression on the nature of Carbonic Acid.

The substance, above all others, with which you should make yourselves well acquainted for rightly understanding the functions of the leaf is carbonic acid. Acquainted, indeed, with it many of you have doubtless long been in a glass of soda water: the bubbling gas which escapes in that pleasant draught being carbonic acid, which had been forced by artificial pressure into the water, and there retained in solution till the pressure was removed by uncorking the bottle, when the gas again had a struggle for its liberty, and escaped. I mentioned, in my fourth letter, how you might cause an escape of carbonic acid to take place from chalk, or marble, or carbonate of ammonia. Carbonic acid always exists as a gas under ordinary circumstances, though chemists, by means of pressure, can cause it to assume a liquid form, and can even, by intense cold, reduce it to a solid state. The gas we call steam is much more manageable in these respects: we all know that it is easily condensed into water, and that water is easily reducible to the state of ice. It may not be impolitic for me to remark here upon a very common popular notion, that certain substances are essentially either gasses, or liquids, or solids: whereas, you are to understand, that every substance may be considered capable, under certain conditions, of assuming each of these three forms. Thus, quicksilver is a liquid at ordinary temperatures: it becomes a solid during the winters of more northern climates: it is readily converted by the heat of a common fire into an invisible gas. I must not dwell upon this subject, but you will find the remark I have made assist you in comprehending how it is that carbonic acid exists as a gas, whilst one of its elements (carbon) appears under a solid form; and that carbonic acid itself, when united to lime, forms the solid substance, chalk.

The two elements of which carbonic acid is composed, are carbon and oxygen. With the former of these you are well acquainted under the name of charcoal, which is very nearly pure carbon. With the latter you are still more familiar, though some of you may not be aware of it, since every breath of air you inhale is nearly half* composed of this element, oxygen. We are all great carbonic acid manufacturers. The oxygen we inhale is fixed in the blood through the instrumentality of the lungs; it is after a time united with carbon, and the carbonic acid thus formed is discharged from the lungs every time we exhale our breath. There is an easy mode of proving that we are all continually exhaling carbonic acid. Procure some lime-water; or rather prepare some by putting a few lumps of fresh burnt lime into a bottle of water, and cork it up for a little while, the water will dissolve a portion of the lime, and you will have lime-water. Pour some of this, when quite clear, into a tumbler, and so place a tube in it (a tobacco-pipe will do) that you may breathe through it, whilst the breath can rise in bubbles through the lime-water. As you continue breathing, the lime-water will gradually become milky. This is owing to the carbonic acid in your breath uniting with the lime dissolved in the water, and thus forming carbonate of lime (or chalk). As this substance is not soluble in water, like the lime, it appears in the form of a milky cloud, and gradually falls to the bottom of the tumbler. By the breath of your mouth you may thus call into existence a deposit of chalk.

It may seem strange that a solid black substance like charcoal should form an ingredient in the invisible gas, carbonic acid; but we may not argue from the condition of one element what will be the condition of a compound body in which it forms a constituent part. Charcoal itself cannot be rendered liquid, much less gaseous, by the most intense heat which chemists can command; but when carbon combines with oxygen, the new substance, carbonic acid, is gaseous. A chemist could decompose this substance before your eyes, and you would then see the carbon fall down in the form of a black powder, apparently called into existence out of nothing, though in reality coming from an invisible gas—a very striking experiment. The union of carbon and oxygen takes place when charcoal or other substances composed of it are burnt. The charcoal gradually disappears, but is not annihilated; it is now part of the compound which has been formed, (this same important material carbonic acid) and which is invisible to mortal eyes. When woody matter decays and

* This should have been written *one fifth.*

disappears, a similar union of carbon and oxygen takes place: only the process is now effected by very slow degrees, and no heat or light are developed, as in the case of actual combustion.

Your obedient servant,

J. S. HENSLOW.

Hitcham Rectory, February 18th, 1843.

P.S.—Mr. Bree and myself have come to the conclusion that it would be needless to discuss any further the abstruse chemical considerations involved in the fixation of ammonia by gypsum. In a letter from him, he enters, with every appearance of correct inference, into further explanation of his reasons for believing that we shall not be able to fix ammonia in dunghills by means of gypsum. Very possibly he may be perfectly right: we must wait the result of our experiments to know whether he is so. Mr. Bree has also recommended you to try certain experiments for fixing ammonia in liquid manures: and I most cordially second his recommendation. Not that I have much doubt, or any doubt, what will be the result of those experiments. I shall be very much surprised if they fail in fixing the ammonia generated during the putrescence of the urine. I have never scrupled to advise that gypsum should be applied to *liquid* manure for this purpose; for Liebig (as well as other chemists) has spoken so positively on this point, that if there is any error here, the value of his work would indeed be greatly diminished in the eyes of all who may be inclined to trust him. Still I am perfectly agreed with Mr. Bree in thinking that you ought to try these experiments also; and to test for yourselves the value of gypsum or some other substance as a fixer of ammonia in liquid manures. But I have been entirely misunderstood if any one has supposed that I have been advising how manure is to be prepared. This is quite another question from how it may be *improved* under the form in which it is almost universally prepared in England. Whatever may be the result of the experiment I have proposed, I conceive it will be of value to you. If we find that gypsum can fix ammonia in an open dunghill, then the long approved system of preparing muck in this manner may be continued: but if we find that such is not the case, then it will be a subject of most anxious and serious consideration to you, whether you ought not entirely to change your present system, and prepare your manures in dung-reservoirs, or in some other way in which you may be able to prevent the escape of that important substance, upon the presence of which the amount of produce so much depends. In proposing to try only gypsum, I have endeavoured to confine your attention to one of the simplest forms of experiment which I could devise. Should this substance fail, we must not forget that Liebig has named other substances, as I noticed in my fifth letter, which may serve the purpose—chloride of calcium (bleaching powder*) super-phosphate of lime, muriatic and sulphuric acids. It may be worth while to have these tried also. If we can only raise our volunteer corps of 50 Suffolk gypsum-powderers, we shall very possibly stimulate 50 or more in each of the neighbouring counties to try one or other of the remaining substances. We may then have a corps of Cambridge bleaching powderers, an Essex corps of superphosphate-of-limers, and a whole army of Norfolk sulphuric-and-muriatic-acidulators. All may be simultaneously attacking the common fortress—the old dunghill—with one or other of the weapons which Liebig has suggested as most likely to deter the ammonia from attempting its escape; and for retaining it in durance vile to serve a better purpose than merely offending the nostrils of such delicate constitutions as have no particular delight in the pungent odour of fermenting muck.

*Bleaching powder is Chloride of Lime; not Chloride of Calcium. See letter 9.

LETTER VIII.

GENTLEMEN,

I must allow the leaves to go on with their exhalations and respirations, without interfering further with these functions for the present, whilst I request your serious attention to a few observations I wish to make on the result of my appeal to you. When I announced my intention of calling for not less than 50 to co-operate in the way I have proposed, I found a very decided opinion expressed in all quarters that I should not be able to prevail upon so large a number. I had calculated on the well known spirit and energy of Englishmen, whenever fairly convinced that any particular course is the right course to be adopted: my only fear being that I should not succeed in persuading many of you to see, as clearly as I do myself, what your present course ought to be for securing the speedy improvement of agriculture. I had also witnessed, during the last two years, the great desire expressed by the members of the Hadleigh Farmers' Club for any description of information which might bear directly or indirectly upon your pursuits. I find that I have not miscalculated in the inferences which I drew from these premises. I have received the addresses of forty-three gentlemen who are willing to accept my challenge, and I find from two quarters that I can calculate upon receiving from ten to fifteen more. Before another week has expired I hope I shall have superintended the preparation of the proposed experiment on two or three farms in this parish. I will then print my promised circular, with precise directions how you are to proceed, and with it I will transmit a *Schedule A (for rotten dunghills!)* containing blank spaces, to be filled up by each experimenter. These schedules will be returned to me some months hence, when the results have been obtained.

Now that I consider we are fairly embarked, I must be allowed to speak a little more gravely than I have always found myself inclined to do. I must feel quite sure that you correctly understand all that I am aiming at. One point, certainly, is to ascertain whether the addition of gypsum to a common dunghill will improve the quality of the manure—which is putting this question in its simplest agricultural shape, divested of all chemical and theoretical notions. But the decision of this question I hold to be a very subordinate purpose to one of still greater importance which I have had in view. I am chiefly desirous of giving you an example of the manner in which your agricultural experiments ought to be conducted, if you would hope to render them available for scientific purposes. I believe our gypsum experiment to be only one of several which *must* be tried before the question at issue can be fully settled. But every such experiment, thus tried by a numerous body of experimenters, upon a correct principle, will add something so positive to the general stock of knowledge, that it is impossible it should not become of real importance to your interests. The little trouble, or very trifling expense, which each individual experimenter may happen to incur in such cases, is no way increased by his having forty-nine co-operators, whilst the value of his single experiment may possibly be augmented more than fifty-fold. It would be a ridiculously trifling tax upon any farmer to perform yearly *one* such experiment. We can all appreciate the advantages which flow from a division of labour; and many who may not be qualified for suggesting correct methods of experimenting for themselves, might be prevailed upon to act upon the suggestions of others, provided the precise mode of performing an experiment were laid down for them. The old proverb declares that "Lazy folk take most pains," and I almost question whether this proverb was not made prospectively, to apply to the manner in which certain so-called experiments have been conducted in our times. There is no lack of agricultural experimenters. It should seem that we are overstocked with them in England. A gentleman, high in your confidence, writes thus to me. "The fact is, that "whereas farmers were formerly averse to all change, many of them "are now ready to try every thing which is recommended, and it is "necessary to be cautious not to mislead them into expense." Now, this struck me marvellously as a very lamentable necessity. You are to be restrained in your anxiety to improve, and to be prevented from doing what science has been urging you to do. I have no doubt the remark was just, from the high authority from whence I received it; but it is equally obvious that the reason it was so, must be because farmers in general have not qualified themselves for experimenting in a safe and judicious manner. Most certainly there have been many experiments of late, conducted in a correct and skilful manner by eminent practical men; but too many even of these were undertaken with no other than some economical object in view, and not with any scientific aim. The results have mostly been tested by pounds, shillings, and pence, without regard to what might be the real cause of success or failure. Hence no new principles have been worked out. But it is only when these shall have been discovered, that it will be time to test their application to practice by the pocket-argument. For instance, the following comparative experiment was lately placed in my hands by an intelligent gentleman of this neighbourhood. It is performed in the approved way of obtaining comparative results; but from want of detail, and from standing single, very little can be inferred from it with respect to the nature of the influence exerted by the nitrate of soda.

Half an acre sown with, and half without the nitrate of soda.

	cb.	*bl.*	*pk.*		*lbs.*
With the nitrate, wheat - -	3	2	3	Straw - - -	840
Without ditto - - - - - - - -	3	1	2	- - - - - - -	746
Increase from nitrate - - -	0	1	1	- - - - - - -	94

	s.	d.
Cost of the nitrate of soda - - - - - - - - - - - -	13	6
Value of the 1*b*. 1*p*. of wheat, at 7*s*. 6*d*., 9*s*. $4\frac{1}{2}$*d*.;		
94 lbs. of straw, at 2*l*. per ton, 1*s*. 8*d*. - - - - - -	11	$0\frac{1}{2}$
Loss on the half acre - - - - - -	2	$5\frac{1}{2}$

Here the balance being against the pocket, no more attempts were made with nitrate of soda. The inspection of the above account induced me to ask the experimenter whether he had thought of trying only one half or one-fourth the nitrate of soda, to see whether the increase might not still be the same. If it should have turned out that such was the case, the pocket would then have pleaded in favour of its application. Before we can expect to understand the precise relation which nitrate of soda, or any other salt, bears to the effect it produces on a particular crop, there must be many well-digested experiments performed in many parts of the kingdom; and their results registered with a sufficient degree of uniformity to admit of their being compared together. Now, with the ready machinery of your numerous local agricultural societies, and the great central Society in London, together with the invaluable advantages offered by the penny postage, you might very easily organize a system of co-operation throughout Great Britain, which I am persuaded would work with a rapidity and an efficiency that would surprise you all. Of course, every one who may be desirous of promoting such a system must be at *some* trouble, but this will generally not be greater than is required to direct and superintend a labourer for two or three hours, whilst he is trimming a hedge or digging a ditch. The personal trouble to the farmer himself would literally be no greater than that. The intellectual part of these proceedings might be all transacted within the skulls of a few chemists, who would suggest, and within those of the more active members of your various Agricultural Societies, who might busy themselves in preparing and circulating, the different *schedules* to be filled up by the several experimenters. All that is now wanting to the adoption of such a scheme, is to convince the farmers of England generally, that unless they will consent to co-operate, they cannot expect to avail themselves (otherwise than by very slow degrees) of the information which modern science has placed at their disposal. I am no enthusiast in this opinion—I am speaking the words of sobriety. I am only repeating what others, wiser than myself, have continually asserted. I have already referred you in my address to the Hadleigh Club, to the opinion of De Candolle, and I have within these few days met with a similar declaration in an agricultural work in course of publication by Professor Johnston, of Durham. This gentleman is a chemist of high reputation; and, in his Lectures on Agricultural Chemistry and Geology, he has suggested several experiments in practical agriculture.* In speaking of the effects produced by special manures, either on particular soils or on particular crops, he remarks, "Now, neither of these subjects, which it is "so important to investigate, can be determined, either from theory "or from experiments, devised and executed in the laboratory of a "chemist. The aid of the practical farmer, of *many practical "farmers*, must be called in. *Numerous* experiments or trials must "be made *in various localities*, and by different individuals; all, how- "ever, according to the *same rigorous and accurate method*; in order "that, from the *comparison of many results*, something like a general principle may be deduced." The whole question, then, evidently turns upon this; whether the farmers of Great Britain are prepared to co-operate in making experiments under the guidance of persons qualified for directing their efforts. There is, in fact, an important link yet wanting in the chain which is to connect science with practice; and if science is only permitted to hold the pincers whilst practice plies the blows, that link may be speedily forged and fastened. My hopes are now much strengthened that this will soon take place. I shall in future listen with impatience to any desponding surmises about your not being sufficiently awake to the importance of co-operating for such a purpose. "My own experience (says a valued "correspondent) in this neighbourhood (Surrey) is against expecting "the present race of adult farmers to become much of readers; and "so my hopes rest on their sons." But I conceive it is not necessary for that description of co-operation which is required of you, that the present race of farmers should become "much of readers."

* Nos 28 to 31, have since appeared of Professor Johnston's work, to which are appended the results of a set of experiments undertaken in the North, in consequence of his suggestions. "No one (he observes) who studies with care the "experiments which follow, and the few remarks I have appended to them, will "hesitate in pronouncing them to be as a whole the most valuable contributions "to accurate experimental agriculture ever hitherto published. The results are "not all equally important, nor all equally instructive, but they are the first fruits "of a new line of research, which will lead us hereafter to the discovery of important general truths. They show that practical men are now on the right road, "and—spreading as scientific knowledge now is among the agricultural body,— "I trust there is no fear of their hereafter being prevented from pursuing it."

I have no pretension to criticize these experiments, at which, in fact, I have merely taken a most cursory glance: but I see enough in the remarks of Professor Johnston himself to satisfy me that he will be among the foremost to admit the advantage and the *necessity* of a more extended co-operation for accelerating the discovery of sound *principles*.

Let them *act* upon the suggestions of others; and then, whilst Liebig is uttering lamentations over his Germans, and consoling himself by talking of what will be done by a generation to come, the present race of British farmers will have forestalled that generation of foreigners yet to come, and have left the present generation behind. Why should we doubt that the farmers of Great Britain generally would be less prepared to co-operate than the farmers of Suffolk have shown themselves to be? No doubt there are some, and probably not a few, among you, as among every other class of men, who would indolently prefer a shorter road than that which science points out to them. Perhaps it would have been easier for me to have enlisted twice fifty co-operators if I had assured the agricultural world that my past studies in Botany had led me to the discovery of a marvellous composition, a certain incomparable vegetable elixir, in which it was merely necessary for the sower to steep his seed, and he would at once be able to double his crops! But to have found more than fifty willing to proceed in the legitimate spirit of philosophy, and without anything promised about reaping an *immediate* return, is an event which I must consider of most excellent omen. From what I have heard since my last letter, I believe a little further delay would have greatly increased our numbers; for I find that several farmers of this neighbourhood have not seen my challenge, and had only heard of it through others. If not more than twenty-five of you had come forward, I should still have asked each man to procure a double, and I make no doubt that we should thus have got our number completed. As it is, I think it will be advisable to increase our numbers, rather for the sake of experimental *instruction*, than for the issue of the experiment; and I shall have a hundred copies of *Schedule A* struck off, that I may be prepared to supply any additional recruits.

Whilst you are performing this experiment, I must call upon you to toss all prejudices and antiquated opinions to the winds; and to proceed as closely as possible upon the instructions with which you, will be furnished. There must indeed be a certain latitude allowed because it may not be convenient for one person to use any but littered straw, or for another to apply it to a particular crop. But the very object of requiring so many as fifty is to merge the inconvenience of such contrarieties as these in the general results obtained.

We are not in the position of chemists operating upon weights which it is necessary to test to the tenth of a grain; and we require no costly apparatus for determining our quantities. For the present experiment, your agricultural laboratory need be furnished with no more than the following instruments:—1. A dung-fork. 2. Pair of scales or steelyard. 3. Bushel basket. 4. Hair sieve. 5. Watering-pot. The gypsum must be finely powdered; and not burnt into Plaster of Paris.

Your obedient servant,

J. S. HENSLOW.

Hitcham Rectory, February 25th, 1843.

LETTER IX.

Gentlemen,

Since my last letter, I have been present at the preparation of our gypsum experiment on three farms in this parish, where the occupiers had volunteered to try it. I now feel myself a little better prepared than before for giving you precise instructions in the schedule I am about to circulate. I had intended this letter to have served as a comment upon that schedule, and to have been circulated with it. My engagements during the week have unluckily prevented me from putting my memoranda together (in time for the editor) under so precise a form as I should wish to see them, and, therefore, I must beg leave to defer communicating them publicly until next week. In the meantime, I hope I shall be able to forward both the instructions and the schedule to those gentlemen who have favoured me with their addresses, which now amount fully to fifty, without including some ten or twelve promised from the neighbourhood of Ipswich. I find the experiment even easier to be prepared than I had myself anticipated; and every farmer here to whom I have mentioned the amount of time and material consumed in preparing the three I have superintended, said with surprise, "Is that all?" On a *large* scale, the experiment requires less than two tumbrels of straw, and forty baskets of dung, and not more than twenty pints of gypsum!

I must request those gentlemen who are preserving my letters, as I find some are, to correct two errors which have crept into the seventh. In the *digression on carbonic acid,* I have carelessly stated oxygen to form nearly one-half, instead of one-fifth of the atmosphere; and in the postscript, I have supposed that chloride of calcium was the common bleaching powder, having in my eye, at the moment, the chloride of lime, and forgetting that chloride of calcium was the muriate of lime, by which name it was more familiar to me in my chemical days. I cautioned you, in the end of my third letter, that I should very likely be caught tripping in these sort of details: for my chemistry is very rusty; and though one gentleman, in the *Suffolk Herald,* has been pleased to refer to the *learned leisure* of Hitcham, I can assure him that my opportunities do not always allow me time for turning to a chemical author to ensure perfect accuracy about numbers and component parts, &c. I hope, therefore, that all such slips of pen and memory may be excused. A publisher has informed me that he intends to speculate upon your purchasing copies of my letters, which he proposes to print in the form of a pamphlet, and I hope he will not correct any of the errors of the sort I have just alluded to, further than by adding a little note at the bottom of the page. If he (or any one else) shall acquaint me when he means to reprint them, I will supply him with any critiques of this sort which I may happen to think necessary; and I shall be obliged to any correspondent who will be kind enough to point out such errors.

I have obtained Dr. Daubeny's permission to print the annexed letter from him to me. I think this advisable, as it contains an apology to the public for an error of the same class as my own; and it also contains an important caution to the public against an error of a much worse character. Whilst I am in the humour for apologising, let me express a hope that I have given no offence to any one by my seeming, on some occasions, to treat my subject too lightly or too frivolously; and on other occasions, to press it with too much officiousness or censoriousness. There is no body of men of such varied character as those who are engaged in agriculture. From the very subject nearest the throne, to many a subject very near the workhouse; from every occupation, from every party in the state, from every grade of intellectual cultivation, we have persons who apply their thoughts to enquiries connected with your most important profession; and though these letters have been more especially addressed to the farmers of Suffolk, I have considered the insertion of a few hints, which might apply in other quarters, not to be unadvisable. What may possibly have engaged the attention of some may not have been approved by others: but the end has been that our corps of co-operators has become more than fifty strong. It is with great satisfaction that I find the editor of the widely-circulated *Gardiners' Chronicle* taking the same view as myself of the importance of your attending to experimental co-operation. Let your example spread a little further, and let us only see a regular system of co-operation organizing itself through other counties, and I shall be very glad to make my bow as a chemist, and leave it to abler hands to prepare for you schedules B, C, D, &c., &c., to the end of the alphabet. Perhaps I may still be allowed to gratify an itching for discussing some botanico-agricultural question or other, as I may see occasion; but, though I believe it is good practice to concentrate one's fire upon a single point, and never to slacken till the breach is made, I cannot pretend to maintain so rapid a discharge as heretofore, now that the chief outwork has been gained.

Your obedient servant,

J. S. HENSLOW.

Hitcham Rectory, March 3rd, 1843.

TO THE REV. J. S. HENSLOW.

Oxford, February 24, 1843.

DEAR HENSLOW,

I fear I may have led the public into an error by mentioning, amongst other substances that *fix* carbonate of ammonia, common salt.

It is probable indeed, that when this substance in solution is mixed with carbonate of ammonia, also in solution, a partial decomposition of both salts ensues, the acids being divided between the two bases in the ratio of their respective affinities. But I doubt whether this principle can apply in the cases of manures, for when the salts become dry, the carbonate of ammonia would volatilize, and then the carbonic acid, united with the soda, would be divided betwixt this base and the ammonia, producing a fresh dose of carbonate of ammonia, which would fly off, and thus cause a repetition of the same process, until the whole of the muriatic acid reverted to the soda, and the whole of the ammonia was dissipated. I do not think, therefore, that any such advantages will be gained by adding muriate of soda, as by adding muriate of lime, and the other bodies I had enumerated, and, if you will mix the two salts together, and apply heat, or even dissolve them, and then evaporate, you will find that *carbonate*, and not *muriate* of ammonia will be (at least principally) evolved.

I regret, therefore, that I published this statement in a Lecture, which has gained so much publicity through the medium of the Journal of the Royal Agricultural Society. In other respects I see nothing to correct, either in my statements or in your comments upon them ; though, considering that practical men deny the efficacy of gypsum in fixing ammonia, it would be wrong to speak with perfect confidence on the matter, notwithstanding Liebig's authority.

Those, however, who intend to make experiments with a view of settling this question, should recollect, that the gypsum must be added, not in lumps, but in fine powder, and that a certain amount of water must be present, in order to bring about the decomposition.

Such hints as these may be considered trite and obvious, but I do not believe them unnecessary, when I find that a chemical manufacturer, who offers to farmers for manure, an article, which he has thought proper to call "Dr. Daubeny's Sulphate of Ammonia," appears to have so mistaken the directions I had given, (which I had conceived to have been plain enough) that, according to the statement of a chemical friend, who has examined this product, it does not contain a particle of ammonia.

Had he consulted me on the subject, before he made free with my name, I should have told him, that I had no wish to claim as my own, a process which was nothing more than the application of a well-known chemical process to the case of the ammoniacal liquor obtained at the Gas-works; but I can assure you, that I am quite as little accountable for this, as I am for the ill success that appears to have attended the process, and that I do not even know the name of the maker.

I am, Dear Henslow, &c.

CHARLES DAUBENY.

LETTER X.

Gentlemen,

Some persons will consider this letter to be a scene in "Much ado about nothing;" but experience has convinced me that I cannot be too minute in pointing out the precise conditions under which our experiment may be carried on; and therefore, if I am a little over-circumstantial in recounting to you the mode in which the three experiments referred to in my last letter were performed, I trust you will excuse me; and I believe you will be the better able to see how very little skill and trouble is necessary for conducting such experiments as these. What is really required of you, is to have a little faith in what chemists and others have declared to you, viz., that such experiments must be tried, if *you wish to help science*, in order that science may be able to return the compliment and *help you*. Perhaps I may find occasion for saying a few more words on this subject some other time; but to day I had better confine myself to an account of the manner in which our three Hitcham Experimenters have fulfilled their parts; and then proceed to give you a few instructions for the filling up of *Schedule A*. Provided this letter and the schedule are read attentively two or three times over, I think no one can possibly mistake what it is he is required to do: but if it should appear that I have not sufficiently explained myself, I beg there may be no scruples about applying for further information.

Mr. Hitchcock's Experiment.

I went to the ground accompanied by the experimenter, his brother, and my own gardener; and there found that preparations had been made by shooting down a tumbrel load of soil under the name of "good ditch stuff;" a tumbrel load of littered straw; a tumbrel load of clean and fresh horse dung, two pails of house urine, with a ditch full of water to dilute it, and a bushel of gypsum in a sack. Over these materials was presiding a labourer, apparently somewhat puzzled to account for our proceedings. The first step was to mark out two spaces, each five feet square, and upon them to place a bottom of the "good ditch stuff" half a foot thick. It should seem that clean straw is a scarce commodity in Hitcham just at present, and so we were obliged to put up with such as had been soaking in a cow-yard for two months amidst all the et ceteras of that description of locality. Some sort of rough calculation had been made of the amount of this straw, and also of the horse dung, by weighing a bushel skep of each; but I looked grave at the idea of our being satisfied with any estimate obtained by weighing only a single bushel; and so the steelyards were sent for. It is not necessary to weigh the whole of the materials used, but four or five baskets-full at least should be weighed in order to obtain an average for measuring out the rest. I suppose it is not necessary to remind any one not to forget to deduct the weight of the basket in making his calculations; but I find it is right just to hint that the weight of a basket-full should not be altered after it has been prepared, in order to coax the average. Let each be filled *by guess* as nearly alike as may be, and weighed ; and then the whole weight, divided by the number of baskets-full will give the average for further estimate. Our zeal, however, on the present occasion was sufficient to keep us warm enough for three hours, and through two or three snow storms, until we had weighed out the whole of the materials, and completed our task. We began with the dunghill to be gypsumed, and placed it to the S. of the ungypsumed, and finished that first before we began with the other. Two baskets-full of the straw were spread over the bottom, each weighing between 40 and 50 lbs. Then two baskets-full of the horse dung were spread out, each weighing somewhat less than the wetted straw. A pint of gypsum was then sifted over the horse dung; and the coarser particles of the gypsum which did not pass through the hair sieve, were preserved, in order that they might be weighed and deducted from the weight of a pint of gypsum, for the purpose of ascertaining the exact weight of the gypsum used. A gallon of liquid, half urine and half water, was then poured over this first layer of materials through a rose watering pot. Seven layers were thus prepared in succession, except that the topmost had only 2 gallons of water, there not having been any urine left. The whole was then covered over with two baskets-full of the wet straw. The other dunghill was then prepared in the same way, except that no gypsum was used—but the amount of materials employed was exactly the same. The two stood now about three feet high, and their appearance was such that it was presumed they would certainly attract any one curious in cucumbers to step out of his way to examine them. The experimenter was also of opinion that they would be objects of curiosity to his cows; and therefore he thought fit to enclose them with hurdles, lest these gentle quadrupeds should amuse themselves by poking them into the neighbouring ditch. I think it right to record his precaution here, because it seems that a cow is a very inquisitive and experimenting creature. I well remember that a cow once made a tolerably successful attempt at devouring a waistcoat of my own, which I had thrown off in a field whilst I was occupied in chasing the splendid swallow-tail butterflies, which no one with the feelings of a naturalist can ever possibly resist running after in the districts about Cambridge, where they are to be met with. My waistcoat looked very much as if it had visited the cow's first stomach, and required only a little further ruminating to render it sufficiently digestible for being re-swallowed. But, besides the precaution against cows, the experimenter found it necessary a day or two afterwards to protect our hillocks against the rooks, who had taken a fancy to them, and were doing their best to pull them to pieces. A covering of "good ditch stuff" stopped their proceedings. In this condition, then, the hills are intended to repose until next October, when they are destined to be applied to land reserved for a wheat crop.

Mr. Pilgrim's Experiment.

Here I found two bottoms, each seven feet by five, had been already prepared of fresh turf and mould. There were also on the ground two tumbrel loads of very wet litter from the farm-yard, two months old; two tumbrel loads of horse dung with a little litter also intermixed; a supply of mixed house urine and much rain-water, somewhat high in odour, which had been collected in a tank. Forty pounds of ready sifted gypsum was an improved mode of bringing this material to the field of action; but only 20 lbs. of it were used. Four large baskets of the wetted straw, averaging 62¼ lbs. each, were spread out as a bottom layer; and upon them were spread four baskets of the horse dung, averaging 57 lbs. each. An improvement was introduced in the mode of applying the gypsum, by continually sifting it over the straw and dung as they were spread out, so that it became more thoroughly incorporated with these materials than in the former instance. Two waterpots of the liquid, each holding ten pints, were then poured over the layer. The two dunghills were proceeding at the same time, as we did not weigh more than the materials for one layer, and could measure out the rest sufficiently rapidly to allow of this improvement also. Each was composed of five layers like the first, and covered over with a layer of the wetted straw. The experiment occupied about one hour and a half in preparing, under the engineering of five pair of hands. The hills were covered over with turf and earth; and are intended to be applied in June, to land which is to lie fallow till next spring, when it is to be sown with barley.

Mr. Carter's Experiment.

Here we found some clean straw in the barn-loft, and proceeded to tie up two large bundles, each of which weighed 61 lbs. Notwithstanding the very great difference between the weight of this clean straw and that of the wetted litter in the former experiments, we built two hills with it of the same size, or nearly so, as those of Mr. Hitchcock. The bottoms, of five feet square, were prepared with soil taken from a mangel-wurzel bed. The clean straw proved to be rather an awkward subject to deal with in preparing so small a heap; and the result sadly affronted those notions of propriety which good workmen possess about turning out a job in a workmanlike manner. It may therefore be as well in other cases, where clean straw can be got at, to chop it up into three or four lengths to make it more manageable. The straw was spread out into five layers, and on each were placed three bushel baskets of horse dung—the gypsum being continually sifted among the materials as in the last experiment. One water-pot of pure horse urine from a stable tank was poured on each of two of the layers, and two water-pots of much diluted drainings from the farm-yard were poured over each of the other three. The estimate for the whole quantity of dung used, was made by weighing out six baskets-full, at 274 lbs., which gives an average of $45\frac{2}{3}$ lbs. for each basket. The capacity of the watering pot used was ascertained to be 19 pints. These heaps were hurdled in and covered over with mould, and are to be applied in October for wheat. Although I consider this rough mode of performing the experiment will serve our purpose, I strongly recommend to all who possess the opportunity, to be careful in obtaining the separate materials as pure as possible. Clean straw, as in Mr. Carter's experiment; clean dung and pure urine as in Mr. Hitchcock's; sifted gypsum previously weighed out, as in Mr. Pilgrim's and Mr. Carter's. Attention to these sort of minutiæ always enhances the value of the experiment; and I dare say that the chemists who are preparing schedules B, C, &c., will be more positive on such points than I have been in this first attempt.

Instructions for filling up Schedule A.

There will be five separate occasions for referring to the schedule as the experiment progresses; and the memoranda have consequently been arranged under five heads. Most of the memoranda under the first head, seem to be too simple to need a comment, and a mere reference to the three experiments already detailed will explain them readily.

No. 3. may perhaps seem to be unnecessary; but once for all, I must request that experimenters will be content to submit! The noticing whether A. lies to the N., S., E., or W. of B., is to avoid any error or doubt that may hereafter arise, as to which of the two dunghills was gypsumed. Some people have very short memories.

No. 11. The whole should be secured of the same quality by mixing in one vessel.

No. 15. will need a little explanation. The chances of fixing the ammonia will be increased in proportion as the gypsum is more finely powdered. That which I have seen is rather coarsely so; and on sifting it through a flour sieve many large particles are left behind. These coarse particles may be thrown into a manure tank, where they will probably be useful. I should very much like to find the experiment has been tried by some persons with gypsum *artificially* prepared. I have been asked by more than one correspondent whether he could not prepare gypsum for his farm by dissolving chalk in diluted sulphuric acid, which is a very cheap substance; but I have replied that I should conceive there can be very few localities where the native material might not be purchased at a still cheaper rate. It would, however, be very easy to prepare artificially, the small quantity required for a dunghill; and I think it would be *well worth while* for some of you to try our experiment with gypsum procured in this way, because the particles would be in a much finer state than we can hope to procure them by mere grinding.

No. 19. I do not wish it to be supposed that I have been deluding the experimenters to attempt more than I originally proposed; but it has occurred to me that the value of the experiment will be very materially enhanced without much additional labour, by dividing each dunghill when rotted into two equal parts, and then proceeding to apply these halves in the manner set forth in the schedule. Nothing further need be done to the two halves of A. But one half of B. may be thoroughly mixed up with half as much gypsum as was employed on A., so that we shall then be able to compare the effect of applying gypsum (as in half of A.) *before* decomposition, with applying it (as in half of B.) *after* decomposition. Also there should be six pieces of land marked out of equal size, to one of which "nothing" should be applied; and whilst one half of A. is applied to one piece, the other half of A. might be applied through two pieces. Still I do not press this mode of performing the experiment, though I *most strongly* recommend it.

No. 20. The column headed "Nothing" means where nothing has been done to the land. If A. and B. are not divided into halves, the entries can easily be made to that effect; but if they are, then the "length" under (A. 2) will be twice as much as under any of the others, provided all the pieces of ground are exactly the same shape.

No. 21, 22. The usual terms in which these are expressed in practice is all that is required.

No. 25. It will be better to experiment on a seed crop, if convenient; but still a little variety in this respect is advisable.

No. 27, 28. The columns headed "leaves on" refer to a root crop; which should be given, if possible, both with leaves off (under the first columns) and leaves on.

No. 30, 31. In using the more general term "stalk," I mean it should apply either to the straw of a grain crop, or to the stems of flax, fodder, &c.

Your obedient servant,

J. S. HENSLOW.

Hitcham Rectory, March 9th, 1843.

SCHEDULE A.—EXPERIMENTAL CO-OPERATION.

QUERE.—THE EFFECT OF GYPSUM ON DUNGHILLS?

* *Preparation of Two Dunghills, one (A) with, and the other (B) without Gypsum.*

N.B.—Be very careful to make memoranda at the time, and *trust nothing to memory.*

1. Prepared by Mr. [ ] county [ ] parish [ ]
2. Date of preparation, year [184] day [ ]
3. Position of A, is to the [ ] of B.
4. Bottom for each is [ ] feet, by [ ] feet, and [ ] thick: composed of [ ]
5. Quantity of straw [ ]
6. Quantity of straw actually weighed is [ ] lbs. and hence the
7. Weight of straw in each dunghill is estimated at [ ] lbs.
8. Quantity of dung [ ]
9. Quantity of dung actually weighed is [ ] lbs. and hence the
10. Weight of dung in each dunghill is estimated at [ ] lbs.
11. Description of urine [ ]
12. Quantity of urine added to each dunghill [ ]
13. Quantity of water added to each dunghill [ ]
14. If 12 and 13 are uncertain, then state quantity of liquid (*described under* 11) added to each dunghill [ ]
15. Quality of the gypsum [ ]
16. Quantity sifted among A. [ ]
17. The dunghills covered over with [ ]

* * *Manuring the Land.*

18. Date of applying the manure. Year [184] day [ ]
19. State of the muck [ ]

N.B.—If possible (and unless *very* inconvenient) let each dunghill be now divided into two equal portions, viz. A. into (A. 1.)

(A. 2.); and B. into (B. 1.) (B. 2.) Let (B. 2.) be thoroughly mixed with half as much gypsum as was applied to A. Let (A. 2.) be applied to a space of land twice as great as (A. 1.) is.

20. Spaces manured	Nothing done.	A. 1.—A. 2.	B.1.—B.2.
Length - - -			
Breadth - -			

21. Character of soil [ ]
22. Condition of soil [ ]
23. Previous crop [ ]

* * * *Nature of Crop Manured.*

24. Date of sowing Year [184] day [ ]
25. Name of seed [ ]

26. Quantity of seed	Nothing done.	A. 1.—A. 2.	B.1.—B.2.
Sown where			

* * * * *Results Obtained.*

27. Date of gathering the crop. Year [184] day [ ]
28. Quantities actually measured and weighed.

As obtained	Meas.	Weight.	With leaves on Meas.	With leaves on Weight.
from "Nothing done"				
A. 1.				
A. 2.				
B. 1.				
B. 2.				

29. Estimate of whole measure and weight. As obtained

from "Nothing done"				
A. 1.				
A. 2.				
B. 1.				
B. 2.				

30. Quality of crop [ ]
31. Quality of stalk [ ]
32. Average length of stalk [ ]

* * * * * *General Observations.*

33. Any general memoranda may be inserted under this head.

LETTER XI.

GENTLEMEN,

There being no very direct communication* between Hitcham and the rest of the world, the arrival of the packet of Schedules from Bury has been accidentally delayed beyond the time when I had proposed to have circulated them; and I must beg to apologize, in the name of our unmacadamized bye-roads and tardigrade carriers'-horses, for so long trespassing upon your patience. I trust, however, before† this letter appears in print, that each of the *sixty-nine* gentlemen, who have already favoured me with their addresses, will have received his copy; and I shall be ready to distribute the rest of the hundred among those who may repent of their previous determination to have nothing to do with us. Mr. Potter, a practical chemist, in London, whose letters in the *Gardeners' Chronicle* prove him to be equal to the task he has offered to undertake for you, has publicly expressed himself so anxious that our experiment should have a fair trial, that he is willing to test every sample of gypsum, in order that you may be quite sure the material you are using is of the right quality. You are doubtless aware that this is a very liberal offer on the part of Mr, Potter, whose time is *professionally* occupied in performing chemical analyses. I have not the pleasure of knowing him, otherwise than by correspondence; but I feel obliged to him for the interest he has shown in volunteering his services. I shall, therefore, request every one who receives a Schedule, to send me as much of the gypsum he is about to use as can be conveyed in a penny letter; and I will then forward all the samples to Mr. Potter

* I am happy to say this observation is no longer correct; for among other advantages, the Eastern counties railroad has produced a stage coach which runs through the Village!

† Applications from a distance have exhausted my whole stock of schedules, and convinced me there are other counties at least as ready to co-operate as Suffolk.

for his opinion of them. These samples should be labelled with the addresses of the persons who send them; and this will also serve me for an acknowledgment that the Schedule has been duly received.

I do not know why it is that I cannot proceed a step in these endeavours to point out to you the necessity of experimental co-operation for the advance of agriculture, without being met (I won't say, obstructed) by sundry hints and surmises that my task is a hopeless one. I had thought the simple fact of your having accepted my challenge (and that in greater numbers than I had called for) would have been sufficient to have silenced all disputers about your readiness to co-operate, and all doubters about your being willing to bring our experiment to a proper issue. But I find I am now assailed with a fresh insinuation that it is not likely many of those who have engaged to co-operate will be *able* to fill up the Schedule in a satisfactory manner. Old proverbs are somewhat musty in these days of super-refinement, but I like them nevertheless, and so I say, I suppose these fresh doubtings imply that "I may lead a horse to the pond, but that I cannot make him drink." Certainly if the horse is unwilling, and the pond somewhat muddy, I should not imagine that he would drink—but if the horse is thirsty, and the pond clear, he will need no persuasion, and nature has supplied him with ability. I have mislaid a letter from a gentleman who lives at a distance, and who has expressed himself desirous of having a Schedule to put into the hands of his bailiff. I had intended to have quoted to you his unfavourable opinion of the capabilities of practical men, in a declaration he makes to me, "that he never yet knew a farmer who could correctly fill up a schedule of any sort!" I can only believe him to be very unfortunately placed. His estates are not in Suffolk! I am determined not to be a doubter; and I shall hope on till I see my hopes fulfilled, because I feel assured they will be. Of course I expect to find errors and inaccuracies in several of the returns; but still I am confident that the great majority of the schedules will be as fully and fairly filled up as may be necessary for our purpose. Let any farmer only look attentively through the schedule, and say whether it contains a single question which he is not likely to be able to answer with perfect ease, and far more readily (I suspect) than he found himself able to fill up certain schedules which he received from the parish assessor a few months back. Should any one feel doubtful about the meaning of any of the questions in our Schedule A, I would suggest that he should first apply to the parish assessor for his advice, and if, after all the training which that individual has so lately received in filling up schedules, he cannot satisfy the inquirer, I shall then advise that he will inform me of his doubts, and I will endeavour to remove them. The plea of idleness, of unwillingness, or of indifference, I can easily comprehend, but any plea of *incompetency* is to me incomprehensible. I am much more apprehensive lest another suggestion of evil surmise should prove partially true, and I must beg leave to be a little particular upon this subject. It has been suggested to me, that if our experiment should *fail*, such a result would produce a bad effect, by shaking whatever confidence you may at present be disposed to place in the recommendations of scientific men. "I hope it will succeed," said a gentleman who first suggested this possible contingency to me. Now really, Farmers of Suffolk, I am half inclined to say that I hope it may not succeed, if I am so little likely to be understood, that you have supposed I have formed any decided opinion about its success or failure. If I had wished to play the conjuror, or had cared to count upon your applause by foretelling success, I dare say I could have contrived some experiment in which success could have been secured, or at least not left very doubtful. Perhaps some of you might then have been persuaded to consider me an agricultural Solomon, and would have allowed me to dictate to you any wild schemes or plans for agricultural improvements, without any attention to common prudence or circumspection on your parts. But I hate all such manœuverings and under plots, as I would detest the pious lures and frauds put forth to prop or propagate an unstable faith. Look back through my letters, and you will see that the object and end which I have proposed to myself has been the suggesting to you the necessity of "experimental co-operation;" and that what I chiefly aim at in our present experiment is, to show you how "experimental co-operation" should be carried on. It would be very presumptuous and very unphilosophical in me to say beforehand, that I am positive of what will be the result of our experiment; but I do say that I am positive the experiment *cannot fail* in producing some good result; and that is all that a wise and cautious philosophy would have us look to. Let us first see what the result may be, and then let us speculate afterwards upon what that result may teach us. With this determination, the experiment *cannot fail*—it must teach us something. And so of every experiment conducted on correct principles—it never fails. However it may fail in producing that particular effect which the experimenter may *desire*, or fancy he can secure, yet, even if he should be completely baulked in his expectations, the experiment has assisted him in discovering *the truth;* and it will, in consequence, teach him either to cleave to the "old ways," or to turn to the "new ways," according as he finds that the one or the other practice will best serve his purpose. This discussion is not unnecessary; for I find that some persons have already been purchasing gypsum, and strewing it over their dunghills, just as if it were a settled point that such a practice *must* be correct. We shall never get on if we allow such hasty inferences to influence our determinations. I will quote to you three or four sentences from Sir John Herschel's Discourse on Natural Philosophy, and his word is worth a score of letters from me on such a subject. "To experience we "refer, as the only ground of all physical enquiry. But before ex"perience itself can be used with advantage, there is one preliminary "step to make—which depends wholly on ourselves: it is, the abso"lute dismissal, and clearing the mind, of all prejudice, from whatever "source arising; and the determination to stand and fall by the re"sult of a direct appeal to facts in the first instance, and of strict "logical deduction from them afterwards." .... "Not that we are so "unreasonable as to demand of the enquirer, an instant and peremp"tory dismission of all his former opinions and judgments: all we "require is, that he will hold them without bigotry, retain till he "shall see reason to question them, and be ready to resign them "when fairly proved untenable, and to doubt them when the weight of "probability is shown to lie against them. If he refuse this, he is "incapable of science." .... "With respect to our record of obser"vations, it should not only be circumstantial but *faithful;* by which "we mean, that it should contain all we did *observe*, and nothing else. "Without any intention of falsifying our record, we may do so un"perceived by ourselves, owing to a mixture of the views and lan"guage of an erroneous theory with that of simple fact—there is "no greater fault (direct falsification of fact excepted) which can be "committed by an observer." I have one passing word for the consideration of those worthy and excellent men, my seniors in years, who have lived and prospered under that system of farming into which they were initiated somewhere between fifty and ninety years ago! Some of them, I hear, think me a meddling fellow, and they laugh pretty loudly at the notion of my being able to advise them in anything worthy their attention. But will they tell me whether they have any idea how it is that an astronomer is able to measure the distances of the planets from the sun, and to obtain an accurate estimate of their bulk and weight, or how it is that he can foretel to a fraction of a second when particular celestial phenomena are to recur after the lapse of long periods of time? Now the mariner is indebted to this very astronomer (who perhaps never felt the discomfort of sea-sickness or possibly never saw a ship) for those nautical tables and calculations by help of which he can fearlessly traverse the broad ocean by night as by day, and tell exactly where he may be in the wide world, after sailing for weeks together out of sight of land. I do not say that the science of chemistry has yet attained to the same degree of excellency as the science of astronomy; but it has made, and is still rapidly making, very decided progress towards perfection. It is even now capable of lending great assistance to those who plough the land, though it may not as yet be quite so great assistance as that which astronomy can offer to those who plough the sea. If then some of our worthy seniors do not like to be put out of their old ways, at least let me advise our active juniors not to be daunted or discouraged by any of their pleasant banterings. Let these happy boys of the old school enjoy their humours, as long as they are good humours, and they will be amused (I doubt not) whilst I quote for their edification the account which a Roman poet, who lived more than 1800 years ago, has given us of the hapless old heathens of A. D. 1—so different from the hopeful old Christians of A. D. 1843! I will try whether I cannot roughly translate his verses for the sake of those who have forgotten their Horace, or possibly may never have known anything about him.

" Multa senem circumveniunt incommoda; vel quod
" Quærit, et inventis miser abstinet, ac timet uti;
" Vel quod res omnes timide gelideque ministrat;
" Dilator, spe lentus, iners, pavidusque futuri;

"Difficilis, querulus, laudator temporis acti
"Se puero, castigator, censorque minorum."

"Many are the disadvantages under which an old man labours; "either he is niggard and timid about making use of whatever he "has acquired by dint of his own exertions; or else, if he does ven- "ture to act, he sets about everything with cold and hesitating cau- "tion. He is full of delays, of very faint hope, inactive, a professed "alarmist, hard to please, a determined grumbler, ever praising the "good olden time when he was himself a lad; always correcting and "scolding his juniors."

Whilst the active members of a rising generation continue to pay all due respect to grey hairs and old prejudices, and are ready to allow "that there is nothing new (that is worth knowing) under the sun," they may still be trusting to their own judgment, and be striving to keep pace with others in all matters relating to their own times and to their own affairs.

Now that I consider our gypsum adventure is fairly afloat, it will probably not be requisite that I should refer to it again for many months to come. May it prosper to the improvement of your *judgment*, and tend to assist you in discerning what is most requisite for hastening the progress of agriculture! I shall hope to resume and carry on our dis ussion of the "Functions of the Leaf" through another letter or two; and by that time I shall be called to duties in Cambridge which will occupy me too closely for a few weeks to allow of my troubling you any further, for the present, with a continuation of these letters.

Your obedient servant,

J. S. HENSLOW.

Hitcham Rectory, March 9th, 1843.

LETTER XII.

Function of Exhalation.

GENTLEMEN,

Every farm-house in Suffolk can doubtless furnish forth half-a-dozen clean tumblers; and we shall need no other apparatus for illustrating one of the two functions of the leaf to which I alluded in my 6th and 7th letters. I have just been trying three little comparative experiments in a very simple form, and find they will be sufficient for our purpose. Let us call the three experiments A, B, C, respectively. Each will require two tumblers, one of which is to be filled about two-thirds with water, and the other to be left empty. Place a card over each of the three tumblers which contain the water, and large enough to completely cover it; and then place an empty tumbler, mouth downwards, upon the card. You might use a piece of stiff paper (as I did), varnished with a solution of shell-lac in rectified spirits of wine, which will prevent its imbibing moisture, and more effectually cut oft all communication between the two tumblers. Now make a cap to one of the tumblers (say B) by rolling round it (in two or three folds) a piece of thick paper, sufficient to prevent the light from penetrating: and by giving it a twist above it may be kept rolled up. Lastly, drill a small hole in the middle of each card, just large enough to admit the stalk of a leaf—I used large ivy leaves. The stalks of three leaves are to be passed through the three cards respectively, and the apparatus must be so adjusted that we shall have the bottom of each stalk dipping into the water in the lower tumbler, whilst the blade (or *limb* of the leaf, as botanists term it) is enclosed under the inverted empty tumbler: the card cutting off the communication between the two tumblers in each experiment. Place A and B in the direct rays of the sun; B with its cap on. Place C in clear day-light, but removed from the influence of the direct rays of the sun. In less than five minutes the empty tumbler of A will have become coated on the inside with a cloud of dew; but on lifting up the cap from the empty tumbler of B it will be seen that no dew has been deposited; neither, *as yet*, will any be found in the empty tumbler of C. If B be closed over again with the cap, and matters left as before, for a little time longer, the dew will be seen to increase rapidly in A, and after some time a little will also be found deposited in C. In B none will be found, except that, *occasionally*, there may be seen a deposit on the side of the empty tumbler, opposite to the side on which the sun has been shining. I proceed to explain the causes of these results.

The rapid formation of the dew in A arises from the strong "exhalation" of steam, proceeding from the leaf when exposed to the direct rays of the sun—a continued supply being kept up by the water in the lower tumbler imbibed by the leaf-stalk. This exhalation of steam is not kept up very rapidly in C, exposed only to ordinary day-light; and in the dark, in B, it ceases altogether. The dew which makes its appearance on one side of the tumbler in B arises from another cause. We all know that a wet or moist substance gradually becomes dry by exposure to warmth, and a dry atmosphere. Now a fresh leaf is a moist body, and the evaporation of its juices will gradually take place until it becomes thoroughly dried. The warmer the surrounding atmosphere the more rapidly does this evaporation take place. The very card also, between the tumblers, contains some moisture, which will be given off when the experiment is placed in the warmth of the sun-beam, and will rise in steam into the empty tumbler. The side towards the sun being the warmest, and the opposite side the coldest, the steam in the tumbler becomes condensed on the colder side only; in short, the appearance is strikingly different from what we witness in A. The little moisture in B has clearly arisen merely from the effect of common evaporation, and subsequent condensation. You will find the very same effect taking place in the upper part of the lower tumbler immediately over the water; the steam which rises there condenses on the side opposite the sun, which is kept cool by being shaded by the card above it. In A the moisture continues to accumulate over the whole inner surface of the tumbler; and is occasioned, not by the heat, but by the *light* of the sun, exciting or stimulating the leaf to the performance of that vital function which is termed "exhalation." Even in C, the same action is carried on by the stimulus of common day light; but so feebly, that it is scarcely rendered apparent by our mode of conducting the experiment. With proper apparatus, an exact estimate may be obtained of the quantity of moisture absorbed and exhaled by a plant; and it has been ascertained that a sunflower, three feet high, will absorb as much as 30 ozs, and exhale 20 ozs, in the course of the day. When the first rays of the morning sun rouse the dormant energies of the leaf, they set it off exhaling at a rapid rate; but at that time of day the temperature of the air is so cool that the steam condenses as fast as it is discharged from the myriads of untaxed stills which are there manufacturing a more healthy beverage than gin or whiskey. I dare say that many a sportsman, who at an early hour has been wading through a field of coleseed, never thought of attributing the ducking he received to this "exhaling" power of the leaves, but has concluded that it all arose from the ordinary effect of common dew precipitated from the moisture in the atmosphere. If a plant be completely excluded from light it soon becomes dropsical, because the roots continue slowly to absorb moisture, but the leaves have no power to exhale it, and it therefore accumulates unduly. Perhaps I cannot make you perfectly understand the nature of that *contrivance* by which the function of exhalation is carried on, because it requires you should first take a peep into that hidden world which is exposed to us only by the microscope; but I think that you may obtain a good general notion of it, by fancying you can see the skin on both sides of the leaf perforated by minute pores. Through these invisible pores the steam is exhaled much in the same way that the insensible perspiration is continually escaping from the surface of our bodies, and which is often rendered uncomfortably demonstrable to persons who encase themselves in Mackintosh coverings. Imagine these invisible pores to be formed by means of a pair of invisible sausage-shaped bladders, filled with vegetable juices; these bladders lying side by side, and attached only at the two ends. By pressing the ends towards the middle point, the bladders would curve outwards, and thus an opening, like a slit or pore, would be formed between them. We may consider this description of apparatus (forming the pores) as so many invisible floodgates, which can be opened or shut at the command of the Sun, in order that the steam may be exhaled or retained accordingly. Now I do not pretend to assert that this is *precisely* the way in which the function is carried on by the mechanism I have described; because some of my physiological acquaintances may possibly have better notions about the manner in which glandular structures perform their functions; but I think this is perhaps the most popular and plausible manner in which I may hope to give you some general idea of the subject. Under the microscope we may distinctly see these peculiar arrangements by which the pores are closed and opened; and they are studded over both surfaces of the leaf, more especially on the under surface. The beauty and regularity in the texture of the skin, (or *epidermis)* of a leaf, as much excels those of the most delicate web that can be wrought by human skill, as the gorgeous clothing of the lily surpasses the most glorious array of Solomon. The number, disposition, and form of these pores (to which Botanists have given the name of *stomata*, or "little mouths") vary greatly in different species of plants; succulent plants, or those which have very juicy leaves and stems, have very few pores on their surface. Such plants might seem, on first thoughts, to require a larger supply of moisture than others, but the very reverse is the case. They are destined to occupy dry and barren spots, imbibing the little moisture which the night dews may convey to them; and then husbanding their resources, they refuse to part with it even in the scorching heats of a sandy desert. It has been ascertained, that the amount of water distilled by exhalation is about two thirds of the quantity imbibed by the roots; and consequently that one-third remains fixed in the plant. The conditions under which it is fixed will be more particularly detailed when I come to speak of the other functions of the leaf, which I hope to describe in my next letter. There is another result to be noticed, which has been obtained by the exhalation of this two-thirds of the water imbibed. Whatever matters the water may have held in solution—salts, earths, or metallic oxides—these have all been retained in the plant. Thus we may understand how such substances may be gradually accumulated in considerable quantities, though they may be present in the soil in too minute proportion to admit of the Chemist detecting them. The plant may have been exhaling off ounces after ounces of water before it has been able to accumulate so much as a single grain of a particular substance essential to its health, and consequently essential to its successful cultivation. To give you an example, noticed by Liebig, of the patient perseverance of common seaweeds, in collecting an element named Iodine, but which is furnished to them in such sparing proportions that it does not form so much as one millionth part of the weight of a given quantity of sea-water. Yet they obtain it from this storehouse in sufficient abundance for the chemist to extract it from them as an article of commerce.

Your obedient servant,

J. S. HENSLOW.

Hitcham Rectory, March 23*rd*, 1843.

P. S.—A desire has been expressed that I would myself re-publish and edit my letters to you, under the form of a pamphlet. The following considerations have determined me to do so. Although I believe I have not allowed the interest I have felt in this correspondence very materially to trespass upon my attention, to the exclusion of any of the more important duties attached to my position as a parish priest; yet it is evident that whatever of thought, time, or money I have allotted to this subject during the past three months, *might* have been otherwise devoted in some way advantageous to my parish. It seems to me, then, that I may possibly be able to make some *amende* for such deficiencies by appropriating to some parochial utility or other, whatever may accrue from their publication in these letters. I have, therefore, no very delicate scruples about requesting you to promote their circulation, if you think them calculated to excite the farmers of other counties to co-operate, after the example of the Farmers of Suffolk. I see no reason why we should not have *at least* 100 distinct experiments performed every year; and each carried on by between 30 and 50 practical men, whilst each man need not be called upon for more than a single experiment annually. I propose inserting in this pamphlet my address to the Hadleigh Club, and to add a few explanatory notes, or some sort of commentary and glossary, which may give them a more general character, and enable them to serve as a *precursor* for inducing the farmers of other counties to listen to any future suggestions which may possibly (and will probably) be made to them, for organizing a complete system of experimental co-operation throughout England. My hopes that such will be the case are greatly strengthened now that I find it publicly asserted by a superior authority that he considers such a scheme both *advisable and practicable.**

LETTER XIII.

Function of Respiration.

GENTLEMEN,

I shall be obliged to draw a little more deeply upon your faith than was necessary in my last letter, whilst I endeavour to explain to you that function of the leaf which has been named "respiration." Still, I do hope to be able to set before you two or three very simple experiments, which may serve to convince you that Botanists are not very far wrong in what they have asserted concerning this function. The function may be thus expressed—"that leaves decompose carbonic acid under the stimulus of light, the oxygen of this substance being discharged from the plant into the atmosphere, and the carbon fixed as an ingredient of that 'proper juice' by which all parts of the plant are nourished and developed." I shall first ask you to try a little experiment which may serve to remove a cause of

* In an article devoted to the consideration of this subject in the Gardener's Chronicle of 18th March, it struck me as rather singular that its scientific Editor, (Dr Lindley) should have selected, as worthy of general attention, two remarks contained in my 5th Letter, which an Editor of a local Journal had just been noticing, for the purpose of ridiculing a suggestion which he evidently considered to be unadvisable. I shall prefer quoting from the former. "The subject of "experimenting *on a large scale* is one, the *importance and possibility* of which "become more evident the more we consider it in all its bearings —We may fairly "doubt whether the coarse operations of Gardening and Agriculture will ever "bear to be examined in any other way; and if they would bear it, the "new results of new practices will certainly fail of carrying conviction to the "minds of the mass of the people, unless supported by evidence accumulated "upon evidence."

error that might otherwise warp our judgment in estimating the actual effects produced by the function we are about to consider.

Preliminary Experiment.

Place a tumbler full of spring or pond water before a fire, and another in the direct rays of the sun. As the water becomes warm, in each case, you will find a number of little bubbles collect on the bottom and sides of the tumblers; and if you place in the tumblers a piece of stick, or metal, or other substance, bubbles will also collect upon these. If you place several tumblers of water in the direct rays of the sun, and put into them differently coloured substances, you will find that bubbles will first make their appearance in those tumblers which have the darkest coloured substances in them; because the darker the substance the more rapidly it will become heated and so heat the water sooner. It is very evident that these air-bubbles have made their appearance in consequence of the heating of the water. But from whence do these bubbles proceed? They are derived from air which we may describe as being *dissolved* in the water, just as we find a piece of salt or sugar may be dissolved in water, and thus rendered invisible. When we boil water, all the air which is thus dissolved in it is driven off; and upon allowing it to stand exposed to the air it will again dissolve a certain quantity. Various gases, besides common air, may thus be dissolved in water —as carbonic acid, &c.; and many springs upon issuing from their subterranean courses give off, into the atmosphere a portion of the gases dissolved in their waters, and which are retained in them so long as they remain under-ground. Whoever has been to Harrowgate is acquainted with the offensive odour of the mineral waters of that place of public resort; and whoever wishes to have a good idea of it without going there, need only smell at a rotten egg, or wash out a recently discharged gun-barrel into a tumbler and apply his nose to it. I have already observed in my seventh letter that common soda-water contains a large quantity of carbonic acid. I have used the term *dissolved* in these cases in rather an improper sense, but it will sufficiently serve to convey a general idea of what takes place. Carbonic acid is thus dissolved in all waters, and is also present everywhere in the atmosphere. Supposing, therefore, that leaves can so act upon it, as to decompose it, under both circumstances, we may then be able to exhibit the result of their action upon that which is dissolved in water, even though we might not be able to do so upon that which is dispersed in the air; because, in the former case, the oxygen which is set free would rise through the water in bubbles, and we shall see these, whereas, in the latter case, it would be discharged as an invisible gas into the air. Let us try whether we can make this clearer by a simple experiment.

Illustration of the Function of Respiration.

Fill two or three tumblers with pond or spring water, (which we may be sure) in either case, will hold some carbonic acid in solution. Place a leaf or two under the water in each tumbler. I find that common laurel leaves are well adapted to experiments of this sort, only from their size they require large glass jars, instead of tumblers, unless we cut them in halves. In order to place the leaves conveniently under the water, I make use of a split shot, or small piece of lead, with which I nip the edge of the leaf and leave it fixed there. This causes it to sink and yet retain a vertical position. Place one tumbler in common daylight, and the others in the direct light of the sun. In the former you will observe no effect produced; but in the latter you will soon find numerous little bubbles making their appearance upon the surface of the leaves. I suspect these bubbles have not unfrequently been considered to be oxygen escaping through the pores of the leaf, owing to the decomposition of the carbonic acid contained in the water; but I now feel satisfied that we must ascribe them to the separation of the dissolved air, by the heating process described above. Still I hope to show you a partial effect produced by the decomposition of carbonic acid by the leaf. Arm yourselves with a little of that gift of patience, so essential to all farmers and experimenters; and of course doubly essential when farmers become experimenters. In due time (perhaps in less than half an hour) you will be able to distinguish certain bubbles, which will make their appearance at the cut end of the leaf, or else here and there upon its surface. These will increase in size more rapidly than the rest, and at length rise to the surface. After one has risen, another will immediately begin to make its appearance at the same spot. A little attention will soon convince you that these bubbles have proceeded from the interior of the leaf, and we may, I think, allow them to be the oxygen which the leaf has separated from the carbonic acid introduced into it. The interior of the leaf is composed of little invisible bladders filled with fluid; and these bladders are so packed together as only to touch each other here and there, and thus to leave open spaces between them, which are filled with air or some other sort of gas. Especially in the under part of the leaf the bladders are so loosely packed that a very large portion of air or gas is retained there. This is the cause why the under surface of leaves is generally the palest; but if the air is expelled, as I shall presently describe how it may be, and water be allowed to enter, the under surface will then become as dark as the upper. Plunge a leaf into hot water, and the air which it contains will escape in bubbles from the cut end, and also from a few points on its surface. Sometimes a bubble thus driven out will be seen to adhere to the surface for a few seconds and then be suddenly re-absorbed into the leaf. When we place leaves in water, under an air-pump, the air they contain escapes in bubbles with great rapidity, from the cut end of the leaf-stalk. If you allow the leaves to soak for a few hours in the tumblers, the water will gradually insinuate itself, either through the cut end of the leaf, or through the part pinched by the split shot, or through some accidental rupture in the skin. When the water has thus displaced the air, the under surface will first appear to be blotched with dark spots, wherever it has penetrated, and at length the whole will become uniformly dark coloured. I refer especially to laurel leaves. It is after these leaves have been soaking for two or three days that I have witnessed the stimulating effects of light in exciting the function of respiration in the most illustrative and convincing manner. The oxygen will often issue in a continued stream of little bubbles, even with a distinct noise, from the cut end of the leaf, or from some point or other upon its surface. If the hand or any solid substance be interposed between it and the sun, so as to cast a shadow over the leaf, the effect ceases; but it is immediately renewed upon the removal of the obstacle. Even whilst the leaf continues to be thus shaded, the stream of bubbles may be caused to issue afresh by merely throwing a gleam of *reflected* light upon it from a looking-glass. When we happen to get a leaf into such good humour for these experiments, they become very amusing and instructive; indeed, the information you may thus draw from a common tumbler is far more satisfying and enduring than any fleeting gratification that could be afforded you by the best shilling's-worth of brandy and water which it ever yet held. Only *try these simple experiments, and think* a little about them, and the knowledge you will thus acquire will wonderfully assist your conceptions of the manner in which these functions of the leaf are carried on. When the leaf has become thoroughly saturated with water, it is very curious to see the under surface gradually recovering its pale tint as soon as the tumbler is placed in the light of the sun. When I wish to give the water a good dose of carbonic acid, I fill a phial with this gas, twist a piece of lead round its neck, and let it float in the tumbler or glass jar, with its mouth downwards. In a few hours the water will have taken up all the carbonic acid; a large portion of which will, however, soon escape from it, into the air; still, there will be enough of it retained to cause the function to be renewed with vigour, after it had ceased from want of sufficient supply. I have had a little aquatic plant growing in a glass jar full of water for the last six months, on the table of my study, and whenever the sun falls upon it, up rise several little streams of bubbles of oxygen from the edges of some of the leaves which happen to be torn; but I do not perceive that any oxygen escapes from the surface of the uninjured leaves.

I begin to feel a little sceptical about the manner in which it is commonly asserted that carbonic acid is admitted within the leaf. That some portion of carbonic acid must be introduced with the moisture imbibed by the roots seems to be undeniable; but that other portions are introduced through the very pores by which the steam is exhaled, and the oxygen discharged, appears to me to be still open to further enquiry. This, however, is a subject for Botanists to look into, and one that is not suited to these letters: only you will observe that what I have said in illustration of the functions of respiration is no way affected by this part of the theory.

It is usual to treat the effects we have ascribed to "exhalation" and "respiration" as the results of a two-fold action, both excited by the same stimulus of light; but I question whether it would not be preferable to consider them as the common effect of a single function which might be appropriately termed the "function of organization." What we really observe is the simultaneous discharge of water and oxygen from the leaf. This effect is attended by the fixation of the elements of water (oxygen and hydrogen) with carbon, in the form of "organic matter." But I must suppress an inclination I feel to discuss the subject more fully. I have before noticed the fact, and I cannot resist again alluding to it, that plants are engines or laboratories employed by the Creator in the great work of combining a few elements into a nutritious material, which is to serve for the development of the whole animal creation. I am not acquainted with any natural law which impresses me with a greater sense of wonder than this "function of organization." Here we see mere crude matter brought under subjection to the living principle. It is wonderful to view the effects of gravity retaining the planets in their orbits! it is wonderful to witness the magic changes which chemical affinity can work in compounding and decomposing a variety of mineral substances! but to me it seems far more wonderful, that man and all other animals, are entirely dependent upon plants for manufacturing materials for those bodies of corruption in which, for a little while, they are destined to live and move and have their being. As the hopes of the agriculturist are so much dependent upon the healthy performance of this important function, surely he ought to know at least as much about it as the present ignorance of science may be able to teach him, if he would expect to be able to shape his practice in accordance with the laws of nature, and not needlessly to set himself in hopeless opposition to them. He will ever find nature a kind handmaid, and ready to minister to his wants whilst he woos her skilfully; but a very termagant wherever he resists her rudely. I have perhaps been dwelling too minutely through the last two letters upon these functions of the leaf, but I have not had time to condense my materials into smaller compass. I have still a few observations to make with respect to their practical bearing on some points of agricultural interest, but I shall have the Editor looking more gravely than yourselves at the length of my letter if I prolong it any further. I must therefore reserve for next week what I have yet to say upon this subject.

Your obedient servant,

J. S. HENSLOW.

Hitcham Rectory, March 31*st*, 1843.

LETTER XIV.

Gentlemen,

I am indebted to Mr. R. Rand, of Hadleigh, for the following account of an experiment tried by him last year, in consequence of a remark I had made at a meeting of the Hadleigh Club, and which (if the truth must be told) you may find in letter 6. Three separate portions, each 7 yards long and one wide, (that is seven square yards) of mangel wurzel were selected, where the plants on the ridges appeared to run as nearly alike as possible. Four or five of the under leaves were stripped, upon three separate occasions, from the plants on one of them (A), viz., on July 8, August 6, and first week in September; from another of them (B), on two occasions, viz., August 6, and first week in September; and none were stripped from the third ridge (C). The roots were carefully cleaned and weighed, and the produce was as follows:—

	lbs.		ton.	cwt.	qrs.	lbs.	
From A . . .	47½	neat weight, or	14	13	0	27	per acre.
B . . .	52	. . . .	16	1	0	2	
C . . .	61	. . . .	18	16	2	9	

So far this experiment speaks for itself, and I dare say its language is sufficiently intelligible not to need an interpreter. Now I do not even yet presume to allow science to boast herself over practice from the result of this single experiment; for I do not know to what extent the practice alluded to, any where prevails. I merely produce the experiment in illustration of what I have been saying of the function of the leaf, and in contradiction to the very commonly admitted notion that the root *directly* nourishes the leaf, and not the leaf the root. If each member of the jury whom I have invited to test the utility of pulling leaves off mangel will only consent to play the part of executioner, and, after torturing a certain number of plants, will be so good as to favour me with the result in the form of comparative experiments, more or less like those of Mr. Rand, I shall be happy to report upon them, and state to what extent you may possibly expect to increase your produce by the practice, or to what extent you may perhaps be able to succeed in diminishing it. It was not an unfrequent practice in the unhappy days of the feudal ages (may their like never return!) for our bold Barons to put unlucky Jews to torture whenever they could catch hold of them, and could not prevail upon them by fairer means to produce a certain amount of cash from their pockets; and so, perhaps, we shall find that some non-baronial experimenter of these our own days, of somewhat advanced and advancing intelligence, may discover a harmless mode of torturing a mangel, so as to force it to yield more nourishment by foul treatment than it may be disposed to do by fair means. But I will not be so unphilosophical as to anticipate the result of any projected experiment. If the experiments detailed in Letters 12 and 13 have any way tended to convince you that the leaf really does prepare all the nutriment upon which root, and branch, and every other part of a plant depends, I fancy you will never see a leaf in future without thinking it ought to remain upon the stem as long as it is alive. As plants are without leaves during the winter, they receive no fresh food at that time; but are content with what has been already stored up for them in their stems and roots, to support the feeble action which is going on throughout that period of torpidity. Thus, also, when bears take up their winter sleep, they are well lined with fat, which gradually wastes in supporting the vital functions; but they do not trust to licking their paws (as some suppose) for nourishment to keep them alive. It would occupy me too long to dwell upon the various inferences which may be drawn in a practical point of view from the establishment of a correct principle like the one I have called the "function of organization." The very commonest occupations of the garden and the field—watering, pruning, transplanting, &c., &c., ought all to be considered with reference to this important function, in order that a correct judgment may be obtained on the best modes of carrying on these operations. I will, however, just venture one or two remarks, by way of suggestion, on points of practical interest. Since the amount of exhalation depends upon the supply of water introduced at *the root*, it seems to be a plain deduction of common sense (until some stout practitioner shall produce his strong reasons for refuting it) that all who water their plants should apply the water immediately to the root, and in good doses—and not to the herbage, and in small sprinklings only: unless where it is necessary to clean the leaves. It is true, indeed, that the whole tissue of plants is capable of imbibing moisture, and so the leaves will be refreshed and look well satisfied immediately after they have been wetted, though the root itself may not have been watered; but this only produces an undue stimulus, inviting them to an activity which, unless it can be sustained by a sufficient supply in the right direction, will soon cause the leaf to become exhausted, wither, and die. A similar effect is produced when a botanist brings home the plants he has obtained in his rambles, in that peculiar description of tin box which he dignifies with the classic name of a Vasculum, and then straps it across his shoulders, to the admiring ridicule of country gazers. If his plants were quite dry when gathered, even though they seem to be half faded on being taken out of the vasculum, they may readily be revived on being placed in water; but if they were gathered wet, or were wetted in the box, however fresh they may look when first taken out, they will soon fade afterwards. Light being the great stimulus to this vital function, sufficiently accounts for (what every one is well aware of) the propriety of temporarily shading plants when their roots have been weakened by transplanting; because they cannot then imbibe the moisture with sufficient rapidity to supply the requisite discharge at the leaves. In this insular climate of our high latitudes, the direct light of the sun is seldom likely to be an unwelcome visitant upon the leaves of our ripening crops, and therefore the shadows of all trees planted in the hedges towards the South of fields, must always be saying stop—not so fast—to any very rapid elaboration of the "proper juices" of those plants upon which they may chance to fall. The tree will probably have been closely trimmed to the very topmost branches, in the most approved fashion of ugliness; but whilst it has any head left, its shadow must still fall somewhere, and there it will cause a certain amount of relaxation in the manufacture of "proper juice," which it will be most improper to sanction—unless it cannot be disallowed without producing some greater inconvenience. This remark may be considered trifling, and possibly it may be unnecessary; but I would ask whether the actual amount of the bad effects thus produced has ever been estimated by direct experiment? The greater rapidity with which corn ripens, after flowering, in still higher latitudes than our own, is owing to its longer daily exposure to the light of the sun in regions where, at midsummer, it hardly dips at all below the horizon. After the "proper juice" has been prepared and carried into the system, a variety of chemical changes take place, according to the nature of the plant; and in some crops it is of the utmost importance to the success of their culture that these changes should be closely watched, lest the time should pass by when it is of most consequence they should be gathered. This is well known to those who in France cultivate beet for the manufacture of sugar. Are our own cultivators of mangel wurzel equally well acquainted with any facts that may teach them when it may be most to their interest to pull up these plants: which are only a variety of beet? As the leaves can prepare only a certain amount of nourishment for all parts of the plant, if we are desirous of supplying one part with it rather than another, it may then be advisable to remove the part which we have no desire to cherish. Thus we may pull off the tubers from potatoe plants when it is considered more desirable that the flowers and fruit should be left for ornament; or we may pull off the flowers, as soon as they are about to expand, if we should happen to think it most advisable to give the tubers a good blow out of "proper juice," for the sake of afterwards robbing them of their stores for our own particular eating. But I shall not pursue further the practical inferences which may be deduced from the firm establishment of correct principles—that is rather your province than mine—and possibly some of my attempts might excite a smile at my own expense. I must try, before my next letter, whether I cannot muster up courage sufficient to take a somewhat bolder step than I have hitherto done, and speak out a little plainly to other parties as well as to yourselves. If we are ever to have a general scheme for experimental co-operation properly organized through all England, this can only be effected by the more influential parties attached to the agricultural interests lending the farmers a helping hand—not so much as regards expense (for there need be little or no expense incurred by most of you), but in respect of example, advice, and counsel. Hosts of advisable experiments are importunately thrusting themselves forward for trial; and many of them (I doubt not) only need a little cross-questioning, previously to their being engaged by one or other of you on advantageous terms, and in the capacity of faithful and trustworthy land-agents.

Your obedient servant,

J. S. HENSLOW.

Hitcham Rectory, April 5th, 1843.

LETTER XV.

GENTLEMEN,

For the sake of uniformity I address myself to you, though I intend to direct my observations in this last letter mainly to our country gentry, and to our rural clergy. This intention has been awakened by a remark of the Editor of the *Gardener's Chronicle*, in his comments on some of my letters to you. "There is not (he says) "that universal stir among the agriculturists which their political "position, and the imminency of their danger, loudly call for; and "for hundreds who are helping themselves, there are thousands who "trust to others for assistance. We are not, however, among those "who blame them; on the contrary, their bad education disarms the "critic, and we are convinced that if they do not move, it is rather "because they do not know how, than because they are unwilling. "We would, therefore, represent *to country gentlemen, to the rural* "*clergy*, and to the people of good education throughout the country, "that they should all bestir themselves, each in his own circle, "in order to induce the farmers to try whether they cannot "better their condition by bettering their husbandry, rather than "by waiting helplessly upon Parliament. We are convinced that "if this were generally done an immense deal of good would "result." From all political allusions I have scrupulously abstained. I have only been considering the possibility of *improving* agriculture by the adoption of an improved system of *experimenting.* To this question I continue to restrict myself, and proceed without further preface to appeal to the two classes named above.

To the Country Gentry.

I feel the full responsibility of appealing to persons so much better qualified than myself to form a correct estimate of what is most desirable for the interests of Agriculture; and who (I know) are perfectly aware of the importance of uniting science with practice. Still, I claim for scientific men, the right of declaring what may be the steps which they consider best for speedily attaining so desirable an object. Now, I need only refer you to several of those reports which have been written by our most eminent men of science, for the British Association, for decided expressions of regret at the scanty distribution of *strictly scientific* information throughout England, compared with its wider spread in foreign countries. Popular notions of science are prevalent enough; but these cannot convey sufficiently accurate ideas of the precise description of evidence which is needful for the further *advance* of science. If I mistake not, such popular notions often throw sad obstacles in the way of immediate advances, by directing well meant efforts to purposes of little or no value: or they lead to the suggesting of heavy schemes, and the planning of expensive machinery, for effecting what might be more readily and rapidly accomplished by easier and less costly contrivances. I should be parading the notion of a general system of experimental co-operation with oppressive officiousness, if I found myself standing alone in recurring to this suggestion. I have already quoted the opinions of De Candolle, Liebig, Lindley, Johnson, Sir H. Davy, and Sir J. Herschell, all tending to shew the immense importance to be derived from an extensive co-operation between experimenters, when they would wish to accumulate facts for the purpose of building up any science like the one you are desirous of seeing established for agriculture. The last named excellent and judicious adviser has promulgated a few maxims which may be adopted by all persons who would attempt the planning of any such scheme. "In short (he says) there is no branch of science "whatever in which, at least if useful and sensible queries were dis- "tinctly proposed, an immense mass of valuable information might "not be collected from those who, in their various lines of life, at home "or abroad, stationary or in travel, would gladly avail themselves of "opportunities of being useful. Nothing would tend better to attain "this end than the *circulation of printed skeleton forms*, on various "subjects, which should be so formed as, 1st, to ask distinct and "pertinent questions; 2dly, to call for exact *numerical* statement on "all principal points; 3dly, to point out the attendant circumstances "most likely to prove influential, and which ought to be observed; "4thly, to call for their transmission *to a common centre*."

There is also an important advantage to be expected from a scheme of extensive experimental co-operation, beyond the actual discovery of sound agricultural principles—viz., the certainty of its raising the general intelligence of our agricultural population. Simple as the experiments may be which the farmers will be called upon to try, they will all inevitably serve to promote *discussion* wherever they are attempted. I am told that our gypsum experiment has been a universal topic at the market tables in this district; and an intelligent farmer, whom I happened to ask whether he considered his neighbours were really likely to turn their thoughts to speculations of the kind, replied to me, "Well, sir, I never saw them so much in earnest before." This, depend upon it, would soon be the case everywhere, if a little trouble were taken to prepare schedules on the plan recommended by Sir J. Herschell. Of course, there are certain persons—mere lumps of apathy—who have not more than the two ideas in their heads of sowing and selling; but we have no more right to judge farmers from these samples than to condemn any particular class of men because there are certain persons who prove themselves unworthy the position they occupy in it. We might as well stigmatise our clergy as hypocrites, our nobility as blackguards, our military and navy as bullies, our physicians as quacks, our lawyers as cheats, and our *scavans* as fools. It is a bad spirit which harps on partial evil only to condemn the general good. It is very unfair to censure a whole class, or even any large portion of a class, from looking at the least worthy or efficient members of it. There is plenty of intellect, plenty of intelligence among our farmers; but it is (as Dr. Lindley has well observed above) their bad education only which disqualifies them for helping themselves. May I then earnestly invite you to take a little trouble in setting them on the right road, and in encouraging them how to proceed along it. If you will do this, I suspect that men of science will be likely to run some risk of being overwhelmed with schedules, and smothered, like Tarpeia, beneath the weight of the presents they have called for. I feel convinced of this from the applications I have received from various distant quarters for copies of schedule A, of which I have not only now dispersed all that were printed, but have been asked for several more. In the name of the excellent authors to whom I have referred, I venture to call upon you to think about, and to act upon, some scheme for extensive experimental co-operation. At present your more enterprising tenants are wasting their strength like ill-disciplined troops in a desultory warfare. A little drilling will soon convert them into regular campaigners; and then, if you will only cheer them on, their advance upon ignorance will be as steady, and their success as certain, as when our brave fellows, in the day of danger and duty, pour their deadly fire into the ranks of a stubborn foe.

To the Rural Clergy.

I may appeal to my brother Clergy a little more confidently than to any other class, because with them I am upon ground which we can firmly tread. Many of you, indeed, are yourselves occupiers, and so far you are personally interested in the improvement of agriculture. But now that we are most of us only pensioners on the land, and no longer tithe-owners, we have surely all increased stimulus for exerting ourselves in any way which may seem to promise advantages to the landed interest. We cannot now be personally profitted, as heretofore, by any agricultural improvements: for so far as they may tend to increase the produce of the land, they evidently tend also to keep down prices—which must act disadvantageously upon *our* incomes, whilst both landlord and tenant will be profiting by the improvements introduced. We may now, then, fairly hope that every little assistance we shall chance to have it in our power to give the farmer, in his experimental researches, will be viewed by him as a disinterested act. Among much that is excellent, generous, and confiding, in the character of our British farmers, it must be allowed that there are some serious exceptions of case-hardened selfishness to be met with, which appear to be perfectly incapable of comprehending the possibility of our being influenced by any other than secret motives of self interest in whatever we undertake. Under our new relationship with the occupier, and now that we are no longer so closely linked with him in his daily anxieties, I do trust that even the most sceptical of our real motives will allow us to serve our generation in these worldly matters, without risking the reproach that we cannot possibly be seeking the welfare of our neighbours, independently of all considerations of personal advantage.

Your obedient servant,

J. S. HENSLOW.

Hitcham Rectory, April 20th, 1843.

A GLOSSARY

OF

TECHNICAL AND OTHER UNUSUAL TERMS,

WHICH OCCUR IN THE LETTERS.

ACIDS. Compound substances, many of which (but not all) are formed by the union of oxygen with some other substance. Those most important to the agricultural chemist are the Carbonic, Muriatic, Nitric, Phosphoric, and Sulphuric. They are distinguished by a sour taste and the property of changing vegetable blues to red. Make a blue infusion from the flowers of violets or cornflowers, &c., by steeping them in water, and pour into it a little vinegar to witness this effect.

AFFINITY, or Chemical attraction; is the power or property by which dissimilar substances unite, and form new substances. Thus Lime unites with Carbonic acid, and forms a new substance, the Carbonate of Lime, which under different forms is called Marble, Limestone, Chalk, &c. &c.

AIR. The mixture (without chemical union) of Oxygen and Nitrogen, which forms our atmosphere. It contains also a certain per centage of watery vapour, carbonic acid and other substances.

ALKALIS. A few compound substances which possess the property of changing vegetable blues to green, and readily unite with acids, and thus forms salts. If we except certain vegetable products now called alkaloids, there are only four alkalis, Potash, Soda, Lithia, and Ammonia.

AMENDE. A French word signifying "reparation."

AMMONIA. An Alkali, composed of the two elements Nitrogen and Hydrogen. It exists under ordinary circumstances as a gas; but unites very readily with water, when it forms the "liquid ammonia" of the shops. The salts which it forms with different acids appear to be very serviceable in vegetation—more especially the Carbonate of Ammonia.

Assimilation. The property by which fresh matter is added to the substance of plants or animals.

Azote—see Nitrogen.

Calcium. An element of a metallic nature, which united with oxygen forms lime.

Carbon. An element, which is very nearly pure in the form of Charcoal. United with oxygen it forms carbonic-acid. It is a constituent part of all vegetable matter.

Carbonates. Compound substances formed by the union of Carbonic-acid with some other substance—as in the following examples:

Carbonate of Ammonia. A salt, formed by the union of Carbonic-acid and Ammonia. There are three different combinations of this nature; the one most familiar to us, is the common "smelling-salts" of the shops, which gives the pungent odour to dunghills—where ammonia is formed by the decomposition of organic matters containing nitrogen.

Carbonate of Lime. A compound body formed by the union of Carbonic-acid and Lime. According to its various degrees of purity, hardness, or other qualities, it bears different names—as Chalk, Limestone, Marble, &c. By heat, the Carbonic-acid is driven off as a gas, and the lime remains.

Carbonate of Soda. A particular salt formed by the union of Carbonic-acid and Soda.

Carbonic-acid. A compound formed by the union of Carbon and Oxygen. The combustion (or common burning) of charcoal, wood, or coal, &c., arises from the rapid union of the oxygen of the air with carbon, thus forming carbonic-acid; whilst the effect is accompanied by the exhibition of heat and light.

Chalk. Carbonate of Lime, generally very pure, forming a soft white rock.

Chloride of Calcium. A compound of Chlorine and Calcium—otherwise called Muriate of Lime: because it scarcely exists without combining with water, and then it changes its nature to the latter salt; which see.

Chlorine. An element, which under ordinary circumstances exists as a gas with a greenish tinge. United with Hydrogen it forms the Muriatic or Hydrochloric-acid.

Chyle. A milky looking fluid, separated from the chyme, in the intestines and conveyed by particular vessels called lacteals into the veins. It serves for the formation of blood.

Chyme. The pulpy mass into which all animal food passes in the stomach, by the action of the gastric juice. The formation of chyme is called "digestion."

Crude. Raw and unaltered.

Develop. The act by which the different parts of organic bodies expand or grow.

Dyspeptic. Subject to indigestion.

Effete. French word signifying worn out or spent.

Elements. Simple substances, of which Chemists are acquainted with 56 or 57. Many of them are of a metallic nature; as Gold, Copper, Iron, &c. Some appear under the form of gases, as Oxygen, Hydrogen, Nitrogen, and Chlorine. The Agricultural Chemist has to deal with only a few of these elements. The main bulk of all plants consists of only four, viz., Carbon, Oxygen, Hydrogen, and Nitrogen; the latter being present in small proportion.

Epidermis. A delicate skin which coats over the whole surface of plants.

Exhalation. A peculiar vital property of the leaves, and other green parts of plants; by which water, in the form of steam, is continually discharged (under the influence of light) through minute invisible pores called stomata.

Fœces. Excrements.

Fœtus. The young animal before birth.

Function. A vital action performed by some part or other of the system. Thus the heart has the function assigned it of keeping up the circulation of the blood. Leaves have the function assigned them of concocting organic matter.

Gas. That peculiar state of matter in which its particles are kept far asunder by mutual repulsion. This condition depends upon certain relations between the heat and pressure to which different bodies may be subjected. Some few require a more intense heat to bring them to the state of gasses, than can be commanded by man; whilst others cannot be brought out of a gaseous to a liquid much less to a solid state, under the highest pressures, and exposed to the greatest cold to which we can subject them.

Gastric-juice. A peculiar fluid formed in the stomach, by the action of which upon the food, this becomes changed into Chyme.

Glandular. A peculiar structure of certain parts of organic bodies which serves for secreting particular substances. Thus, the liver secretes bile.

Gum. A substance formed in the proper juices of plants, and which serves to nourish and develop them. It consists of Carbon, Oxygen, and Hydrogen, and is one of the simplest forms of organic matter.

Gypsum. The form in which the Sulphate of Lime occurs in nature, viz., as a soft mineral substance. When burnt, the water naturally combined with it is driven off, and it becomes reduced to a powder called Plaster of Paris. When water is added to this powder it unites with it very rapidly, and the mass quickly hardens; or *sets* as it is termed—thus forming stucco.

HERBARIUM. A collection of specimens of dried plants, preserved between paper, for purposes of botanical study.

HYDROGEN. An element, the lightest substance in nature, which has never yet been reduced from the state of a gas to that of a liquid. It is a constituent of all vegetables.

INORGANIC. See organic.

IODINE. An element, which readily assumes by heat the state of a gas, of a beautiful violet colour. It is obtained from certain sea-weeds.

LACTEALS. Particular vessels which lead from the intestines, and convey the chyle into the veins.

LEGUMINOSÆ. A particular family of plants which includes Beans, Peas, Clover, Lucerne, Saintfoin, &c., &c.

LIME. A compound of oxygen and calcium. This is generally procured by heating some kind of carbonate of lime; and by thus expelling the carbonic-acid, the lime is left pure. If lime be exposed to the air, it gradually attracts carbonic-acid from it, and returns to the state of Carbonate of lime, such as Chalk, &c.

MAMMOTH. A description of fossil Elephant.

MARBLE. A form of very compact limestone, i. e., a Carbonate of Lime.

MURIATES. Compounds of Muriatic-acid and other substances; as the following.

MURIATE OF LIME. A particular salt, formed by the union of Muriatic-acid and Lime. This is the same substance as Chloride of Calcium with the addition of water, one element of which (Hydrogen) uniting with the Chlorine, forms the muriatic acid, and the other element (Oxygen) uniting to the Calcium, forms the Lime.

MURIATIC-ACID. A compound of Chlorine and Hydrogen, which exists as a gas; but which is readily taken up by water, and then forms the liquid muriatic-acid, or "spirits of salt," of the shops.—See Salt.

NEUTRALIZE. When an acid combines with an alkali, so that the compound thus formed ceases to possess either acid or alkaline properties.

NITRATES. Compound substances of which Nitric-acid is an ingredient. As in the following example:

NITRATE OF SODA. A particular salt formed by the union of Nitric-acid and Soda.

NITROGEN. An element, which under ordinary circumstances appears as a gas. It forms four-fifths of the Atmosphere; and in small proportion enters into all plants.

ORGAN. Any particular part of a plant is so called. Thus the leaf is an organ, the root is an organ.

ORGANIC. Belonging to, or which has belonged to, a living body, whether plant or animal. All other forms of matter are termed inorganic.

OXIDE. A combination of oxygen with certain substances, and which does not possess acid properties.

OXYGEN. An element, which under ordinary circumstances assumes the form of a gas. It is considered to be the most abundant substance in nature, and to form nearly one-half of the materials of our globe. It is one of the constituents of all plants.

PARASITE. A particular description of plant or animal, which derives its nourishment from the substance of some other to which it attaches itself in the living state. Thus the Misseltoe is a parasite on the branches of apple and other trees. A species of Orobanche is a destructive parasite on the roots of Clover.

PHOSPHATE OF LIME. The combination of phosphoric-acid and lime, which enters largely into the composition of the bones of animals.

PHOSPHORIC-ACID. A compound of Oxygen and Phosphorus.

PHOSPHORUS. An element which possesses a wax-like appearance and consistency. In the air it slowly unites with oxygen, and appears luminous—when heated, it burns with intense violence, and emits a most brilliant light.

PHYSIOLOGY. The science which treats of the laws by which the different functions of living bodies are performed, whether plants or animals.

PLASTER OF PARIS. See Gypsum.

POTASH, OR POTASSA. An alkali formed by the union of oxygen and Potassium. The common Potash of the shops is a combination of Potash and Carbonic-acid, i. e., a carbonate of potash.

POTASSIUM. An element which possesses a metallic appearance, but is lighter than water.

PROPER-JUICE. The nutritious fluid formed in plants out of the crude sap introduced by the roots. It is first formed in the leaves, and thence distributed through the system.

REJECTAMENTA. A Latin word signifying "refuse."

RESPIRATION. The act of breathing in animals. The term is also used to express the decomposition of carbonic-acid by the leaves of plants, which separate its carbon and set the oxygen free.

ROCK-SALT. Common salt, as it occurs in solid beds in the earth.

SAL-AMMONIAC. See muriate of ammonia.

SALINE. Of the nature of salt.

SALT. This term is applied to many compounds between acids and other substances; as well as to " common salt," which is the Muriate of soda.

SILICEOUS. Composed of the earth called Silica; which is the ingredient of flints, and a variety of other hard stones, characterized by the property of striking fire with steel. Sand is flinty matter in a finely powdered state.

SODA. An alkali composed of Oxygen and Sodium.

SODIUM. An element of a metallic appearance, but lighter than water.

STOMA, (in the plural STOMATA.) Small invisible glandular pores scattered over the surfaces of leaves; through which the moisture introduced by the root is largely exhaled, or driven off.

SUGAR. A substance formed in the proper juices of plants, composed of Oxygen, Hydrogen, and Carbon.

SULPHATE OF AMMONIA. A compound of Sulphuric-acid and Ammonia.

SULPHURIC-ACID. A compound of Oxygen and Sulphur, commonly known as oil of Vitriol.

SUPER-PHOSPHATE OF LIME. A compound of Phosphoric acid and Lime, in which the acid is in excess.

TARDIGRADE. Slow-paced. A name which naturalists give to a particular group of animals, including the Sloths.

WATER. A compound formed by the union of the two elements Oxygen and Hydrogen. In its solid state it becomes " Ice," and when gaseous it is " Steam."

FINIS.

H. HARDACRE, PRINTER, HADLEIGH.