Cras credemus.

A TREATISE

ON THE

CULTIVATION OF THE POTATO

FROM THE SEED,

HAVING FOR PROPOSED RESULTS

THE EXTINCTION OF THE DISEASE,

And a Yield of Thirty, Forty or more Tons of Tubers per Statute Acre.

(Sent, accompanied by a Packet of Seed, to each Member of the House of Lords; each Member of the House of Commons; and the Principal Landlords of Ulster.)

BY

JAMES TORBITT,

BELFAST.

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TREATISE

ON

THE CULTIVATION OF THE POTATO.

At the Meeting of the British Association at Belfast in the summer of 1874, I stated my opinion, that the sexual combination resulting in the birth of the seed, formed the true and only starting point of the life of the individual; that plants propagated by their buds were yearly growing older, and that the process of cutting them to pieces and planting them in the earth could not, therefore, be carried out for ever. And I illustrated my statement by the following experiment. In the spring of 1873, I planted the cutting of a Vine; in the spring of 1874 it had put forth its adventitious roots and leaves. I then took it up, divided it longitudinally into two equal parts, cutting down exactly through the centre of the pith, and leaving to each half an equal quantity of roots and leaves. I planted it in separate pots, and exhibited it growing healthily to the Biological Section of the Association. I argued that it was only one individual, as regards the duration of life; that neither half had received any accession to its original stock of life—obtained a new birth or new starting point of life—and that if cutting a plant into two equal parts did not effect a new birth, neither could cutting it into two unequal parts, and calling one the stem and the other the scion, do so. I pointed out that the potato, when propagated by the "set" for a series of years, undergoes a change; that in the season of its bloom, here and there a pedicel breaks at its junction with the calyx, the flower drops off, and a berry, which should have contained from seventy-five to a hundred seeds, is wanting in that cluster; that when so propagated for a further series of years, all the pedicels break, and the plant becomes utterly sterile, the condition to which the Skerry Blue is now reduced; that when so propagated for another series of years, it becomes unable to re-

produce its organs of reproduction, and the fields become completely flowerless, the state to which the Cruffle and others are now reduced; and I predicted that in a few years more the "sets" of these old flowerless plants would fail to germinate, they would rot in the earth; and that this death from old age would be supposed to be a disease, and be called as heretofore the "Miss."

I further inferred that as the potato became aged, it became so weak as to be unable to resist the attack of the parasite whose growth in the body of the plant forms the disease, and that in order to extirpate the parasite it would be necessary to grow young vigorous plants from the seed.

Upon this I was met by the assertion that from experiments made on "the Continent," it had been ascertained that seedling potatoes were quite as liable to the Disease as the old varieties; but I knew of my own knowledge that all the main varieties of the plant which were in cultivation in the North of Ireland in 1840 were dead, and that *all* potatoes were not—consequently that some at least of the new were hardier or healthier than the old: and I resolved to make the experiment for myself.

I had expended upwards of five thousand pounds in money, and perhaps fifty thousand in time, in perfecting an invention for the condensation and preservation of the potato; an invention which had met with the approval of the late Baron Liebig, and of a commissioner appointed to investigate the matter by the late Emperor Napoleon, to whom I had offered the French Patent. I had mounted a factory in the Vosges for the purpose of demonstrating the value of the invention (in which, in the opinion of the Emperor's Commissioner, I had succeeded perfectly); but this was in the Spring of 1870, and the war arrested my proceedings in France; whereupon I returned to Belfast, put up a factory there, and found myself again arrested by the potato disease. By it I was brought to a complete stand-still, and hence my attention became directed to the plant itself, its history, mode of cultivation, and the possible cause of the disease. At once it struck me like a thunderbolt—the plant had been propagated everywhere from time immemorial by merely cutting it to pieces. Cultivation from the bud and never from the seed, was the constant concomitant of the disease; everything else varied—soil, temperature, moisture, manure, everything, excepting only cultivation from the "set": it was

always there; and I at once suspected that it was, and is, and always will be, a condition precedent to fungoid disease in the plant. I suspect that even now, old age—too prolonged propagation from cuttings—is at the bottom of the vine grower's troubles, and several of them to whom I have explained the matter, fully agree with me; and one at least is preparing to grow the vine from the seed. Also the sugar planters, who it seems always propagate from cuttings, will of necessity have their own pestilence in due time; that is, if vegetative multiplication is not an equivalent to sexual reproduction.

Returning now to the assertion at the Meeting of the British Association, that seedling potatoes are as subject to the Disease as old varieties—in the Autumn following that Meeting (1874,) I advertised for potato berries, and obtained them (all from new varieties) by the ton, at Five Pounds per Ton. I lost about nine tenths of them by treating them as directed in the books, but I had millions left; enough to have re-stocked Europe in a few years.

In the spring of 1875, I selected a field as favourable to the development of the parasite, as could be found anywhere perhaps;—a piece of slob land on Belfast Lough which had been heavily manured as a kitchen or market garden for many years; in which since 1845, there has been probably every year more or less old potatoes planted, and more or less disease. This field is fat to rankness, full of immense worms, slugs, and larvæ, and I prepared it in the usual way for growing "sets" of the plant—that is, in drills; using as manures, here guano, there bone manure, in another place stable manure; and in another place no manure; a variation of treatment which produced no apparent variation in results. In these drills I planted here and there the "sets" of four or five old varieties of the plant, which were certain to become diseased, and did become diseased. In this same field, alongside and among these old "sets," I planted about five thousand plants, obtained this same season from the seed. When both were ripe (the seedlings and the old) I dug them up, and allowed them to lie side by side on the soil for a few days, in order that the *Peronospora Infestans* (the parasite) should have the fullest opportunity of making its attack. And what was the result? Exactly what I had expected. I found that

seedling potatoes *are* as liable as old ones to be attacked by the disease; but I found also, that that statement conveys only half the truth. I found that seedling plants of the potato are utterly impervious to the utmost force of the attack of the Peronospora Infestans; absolutely proof against the most extreme virulence of the infection; and that there need not be, if man chooses, a single tuber of the potato in the world, infested by this particular fungus. And the very simple solution of the paradox is this —there are seedlings and seedlings, just as there are men and men. One man out of two, perhaps, yields to the attack of Asiastic Cholera, the other is wholly unaffected; so with the potato: under conditions favourable to the parasite, about four plants out of five fail to resist the disease absolutely; from one to ten per cent. of the tubers become diseased slightly, and in a few plants, all the tubers become diseased. Thus of my five thousand seedlings of last season, four thousand became more or less diseased; but the lives of the fifth thousand, under the most adverse circumstances which I could devise, proved themselves to be impregnable to the attack of this fungus. Now, if they resisted the disease last year under conditions favourable to the parasite, why should they not resist it this year under conditions favourable to the plant? They have not yet become too old, and I propose to try it. I shall be disappointed if they do not resist the disease for some fifteen or twenty years.

On the title of this paper I have hinted at, or alluded to my belief that the plant may be made to yield 30 or 40 tons per acre. Well, it will be said that this means doubling the productive powers of the temperate zones; the production of a money value, not to be measured in units of a million; but in units, each of which represents the revenues of an Empire. It is perfectly true, that is the proposition—and the question is not, is the matter large; but is it true?

After much research, I have found one man, and one only, who seems to have comprehended the capacity of the plant. I mean the late Thomas Andrew Knight, Esq., "the venerable and talented proprietor of Downton Castle, surrounded by a princely domain of ten thousand acres of rich and beautiful country, who thinks of nothing, but of what may be useful to his fellow-creatures," and who was an original member, and was the second

President of the Horticultural Society of London. He says (I reprint from his works, as follows) :—

"PAPER ON VEGETABLE PHYSIOLOGY.—I. OBSERVATIONS ON THE GRAFTING OF TREES.

[*Read before the* ROYAL SOCIETY, *April 30th*, 1795.]

THE disease from whose ravages apple and pear trees suffer most is the canker; the effects of which are gennerally first seen in the winter, or when the sap is first rising in the spring. The bark becomes discoloured in spots, under which the wood, in the annual shoots, is dead to the centre; and, in the older branches, to the depth of the last summer's growth. Previous to making any experiments, I had conversed with several planters, who entertained an opinion, that it was impossible to obtain healthy trees of those varieties which flourished in the beginning and middle of the present century, and which now form the largest orchards in this county (Herefordshire). The appearance of the young trees which I had seen, justified the conclusion they had drawn; but the silence of every writer on the subject of planting, which had come in my way, convinced me it was a vulgar error, and the following experiments were undertaken to prove it so.

I suspected that the appearance of decay in the trees I had seen lately grafted, arose from the diseased state of the grafts, and concluded that if I took scions or buds from trees grafted in the year preceding, I should succeed in propagating any kind I chose. With this view, I inserted some cuttings of the best wood I could find in the old trees, on young stocks raised from seed. I again inserted grafts and buds taken from these on other young stocks, and, wishing to get rid of all connexion with the old trees, I repeated this six years; each year taking the young shoots from the trees last grafted. Stocks of different kinds were tried; some were double-grafted, others obtained from apple-trees which grew from cuttings, and others from the seed of each kind of fruit afterwards inserted on them. I was surprised to find that many of these stocks inherited all the diseases of the parent trees.

The wood appearing perfect and healthy in many of my last-grafted trees, I flattered myself that I had succeeded; but my old enemies, the moss and canker, in three years convinced me of my mistake. Some of them, however, trained to a south wall, escap-

ed all their diseases, and seemed (like invalids) to enjoy the benefit of a better climate. I had before frequently observed, that all the old fruits suffered less in warm situations, where the soil was not unfavourable. I tried the effects of laying one kind, but the canker destroyed it at the ground. Indeed I had no hope of success from this method; as I had observed that several sorts, which had always been propagated from cuttings, were as much diseased as any others. The wood of the old fruits has long appeared to me to possess less elasticity and hardness, and to feel more soft and spongy under the knife, than that of the new varieties which I have obtained from seed. This defect may, I think, be the immediate cause of the canker and moss, though it is probably itself the effect of old age, and therefore incurable.

Being at length convinced that all efforts to make grafts from worn-out trees were ineffectual, I thought it probable that those taken from very young trees raised from seed could not be made to bear fruit. The event here answered my expectation. Cuttings from seedling apple-trees of two years old were inserted on stock of twenty, and in a bearing state. These have now been grafted nine years; and though they have been frequently transplanted to check their growth, they have not yet produced a single blossom. I have since grafted some very old trees with cuttings from seedling apple-trees of five years old: their growth has been extremely rapid, and there appears no probability that their time of producing fruit will be accelerated, or that their health will be injured, by the great age of the stocks. A seedling apple-tree usually bears fruit in thirteen or fourteen years; and I therefore conclude, that I have to wait for a blossom till the trees from which the grafts were taken attain that age; though I have reason to believe, from the form of their buds, that they will all be extremely productive. Every cutting, therefore, taken from the apple (and probably from every other) tree, will be affected by the state of the parent stock. If that be too young to produce fruit, it will grow with vigour, but will not blossom; and if it be too old, it will immediately produce fruit, but will never make a healthy tree, and consequently never answer the intention of the planter. The root, however, and the part of the stock adjoining it, are greatly more durable than the bearing branches; and I have no doubt but that scions obtained from either would grow

with vigour, when those taken from the bearing branches would not. The following experiment will, at least, evince the probability of this in the pear tree :—I took cuttings from the extremities of the bearing branches of some old ungrafted pear-trees, and others from scions which sprung out of trunks near the ground, and inserted some of each on the same stocks. The former grew without thorns, as in the cultivated varieties, and produced blossoms the second year; whilst the latter assumed the appearance of stocks just raised from seeds, were covered with thorns, and have not yet produced any blossoms.

The extremities of those branches, which produce seeds on every tree, probably show the first indication of decay; and we frequently see (particularly in the oak) young branches produced from the trunk, when the old ones have been dead. The same tree when cropped will produce an almost eternal succession of branches. The durability of the apple and pear I have long suspected to be different, but that none of either would vegetate with vigour much, if at all, beyond the life of the parent stock, provided that died from mere old age. I am confirmed in this opinion by the books you did me the honour to send me; of the apples mentioned and described by Parkinson, the names only remain; but many of Evelyn's are still well known, particularly the red-streak. This apple, he informs me, was raised from seed by Lord Scudamore in the beginning of the last century.* We have many trees of it, but they appear to have been in a state of decay during the last forty years. Some others mentioned by him are in a much better state of vegetation, but they have all ceased to deserve the attention of the planter. The durability of the pear is probably something more than double that of the apple.

It has been remarked by Evelyn, and by almost every writer since, on the subject of planting, that the growth of plants raised from seeds was more rapid, and that they produced better trees than those obtained from layers or cuttings. This seems to point out some kind of decay attending the latter modes of propagation; though the custom in the public nurseries of taking layers from stools (trees cropped annually close to the ground) probably retards its effects, as each plant rises immediately from the root of the parent stock.

* Probably about the year 1634.

Were a tree capable of affording an eternal succession of healthy plants from its roots, I think our woods must have been wholly overrun with those species of trees which propagate in this manner, as those scions from the roots always grow in the first three or four years with much greater rapidity than seedling plants. An aspen is seldom seen without a thousand suckers rising from its roots; yet this tree is thinly, though universally, scattered over the woodlands of this country. I can speak from experience, that the luxuriance and excessive disposition to extend itself in another plant, which propagates itself from the root (the raspberry), decline in twenty years from the seed. The common elm being always propagated from scions or layers, and growing with luxuriance, seems to form an exception; but, as some varieties grow much better than others, it appears not improbable that the most healthy are those which have last been obtained from seed. The different degrees of health in our peach and nectarine trees may, I think, arise from the same source. The oak is much more long-lived in the north of Europe than here; though its timber is less durable, from the number of pores attending its slow growth. The climate of this country being colder than its native, may, in the same way, add to the durability of the elm; which may possibly be further increased by its not producing seeds in this climate,—as the life of many animals may be increased to twice its natural period, if not more, by preventing their seeding."

"AN ACCOUNT OF AN IMPROVED METHOD OF RAISING EARLY POTATOES IN THE OPEN GROUND.

[*Read before the* Horticultural Society, *June 5th*, 1821.]

The destruction, in the present season, of early crops of potatoes by frost in this vicinity, (particularly in the gardens of those who could ill bear the loss they have sustained,) has led me to address to the Society the following account of some deviations from the ordinary modes of practice in the culture of that plant, which I have found successful in not only affording plants which more effectually recover when impeded by frost, but also in furnishing a larger and more early produce under ordinary circumstances.

It has long been known that abundant crops of late and luxuriant varieties of early potatoes may be obtained by planting very small

pieces only of their tuberous root: for the plants of those varieties always acquire a considerable age before they begin to generate tubers, and therefore do not too soon begin to expend themselves in the production of tubers; and the size which these acquire within any given period in the spring will be to a great extent regulated by the strength of the plants at the period when they first spring from the soil; and strong plants of such varieties can be afforded only by sets of considerable size. I have, in consequence, for some years past, selected in the autumn the largest tubers, and these nearly of an equal size, for planting in the spring; and I have found that these not only uniformly afford very strong plants, but also such as readily recover when injured by frost: for, being fed by a copious reservoir beneath the soil, a reproduction of vigorous stems and foliage soon takes place, when those first produced are destroyed by frost, or other cause.

When the planter is anxious to obtain a crop within the least possible time, he will find the position in which the tubers are placed to vegetate by no means a point of indifference; for these being shoots, or branches, which have grown thick instead of elongating, retain the disposition of branches to propel their sap to their leading buds, or points most distant from the stems of the plants of which they once formed parts. If the tubers be placed with their leading buds upwards, a few very strong and very early shoots will spring from them; but if their position be reversed, many weaker and later shoots will be produced; and not only the earliness, but the quality of the produce, and the size, will be much affected.

In the spring, when the young plants are just beginning to appear in the rows, I have often found it very advantageous to raise the mould over them in ridges by an operation perfectly similar to moulding the plants. Protection has been thus given against frost, and I have not found the period of maturity of the crop to have been in any degree retarded.

It has been contended that there is much waste in the practice above described of planting large sets; because the old tuber is often found to have lost little in weight, when an early crop is taken up in an immature state: and it has thence been inferred, that a very small part only of the matter of the old tubers enters into the composition of the new. But I believe a false inference

has in this case been drawn, and that, under ordinary circumstances, a very large portion of the soluble matter of the old tubers is employed in the formation of the new; for I have proved by experiments purposely made, that the vital union, and community of circulating fluid, between the old tuber and the plant which has sprung from it, is not so soon dissolved.

Some potatoes of rather large size and early habit were placed in such situations that the fibrous roots only of the plants entered into, or were in contact with the soil. Thus circumstanced, an abundant blossom appeared, and seeds would have been produced; but both the blossoms and the runners which would have formed young tubers, were alike removed.

The old tubers, though fully exposed to the sun and air, still retained life, and were obviously supplied with moisture by the stems, which had sprung from them; and the result was ultimately just what I had anticipated. The plants, after many frustrated efforts to produce blossoms and tubers upon every part of their branches, at last threw their sap back into the old tubers; and a numerous crop of young tubers was suspended from the buds, or eyes, of the old. This did not occur till autumn; and therefore the vital union must have subsisted through the whole summer; and I entertain but very little doubt, that such a union subsists under ordinary circumstances, till almost the whole of the soluble and organisable matter of the old tubers has been absorbed by the new. To what extent this occurs is, however, a point of little consequence: the important fact af the crop being increased by the employment of large sets has been proved by accurate experiments, in many successive seasons."

"ON THE CULTURE OF THE POTATO.

[*Read before the* HORTICULTURAL SOCIETY, *July* 1*st*, 1828.]

WHATEVER may have been the amount of the advantages or injury which the British Empire has sustained by the very widely-extended culture of the potato, it is obvious that under present existing circumstances it must continue to be very extensively cultivated; for though it is a calamity to have a numerous population who are compelled by poverty to live chiefly upon potatoes, it would certainly be a much greater calamity to have the same population without their having potatoes to eat.

Under this view of the subject, I have been led to endeavour to ascertain, by a course of experiments, the mode of culture by which the largest and most regular produce of potatoes, and of the best quality, may be obtained from the least extent and value of ground; and having succeeded best by deviating rather widely from the ordinary rules of culture, I send the following account of the results of my experiments. These were made upon different varieties of potatoes; but as the results were in all cases nearly the same, I think that I shall most readily cause the practice I recommend to be understood by describing minutely the treatment of a single variety only, which I received from the Horticultural Society, under the name of Lankman's potato.

The soil in which I proposed to plant being very shallow, and lying upon a rock, I collected it with a plough into high ridges of four feet wide, to give it an artificial depth. A deep furrow was then made along the centre and highest part of each ridge; and in the bottom of this, whole potatoes, the lightest of which did not weigh less than four ounces, were deposited, at only six inches distance from the centre of one to the centre of another. Manure, in the ordinary quantity, was then introduced, and mould was added, sufficient to cover the potatoes rather more deeply than is generally done.

The stems of potatoes, as of other plants, rise perpendicularly under the influence of their unerring guide, gravitation, so long as they continue to be concealed beneath the soil; but as soon as they rise above it, they are, to a considerable extent, under the control of another agent, light. Each inclines in whatever direction it receives the greatest quantity of light, and consequently each avoids, and appears to shun, the shade of every contiguous plant. The old tubers being large, and under the mode of culture recommended rather deeply buried in the ground, the young plants in the early part of the summer never suffer from want of moisture; and being abundantly nourished, they soon extend themselves in every direction till they meet those of the contiguous rows, which they do not overshadow, on account of the width of the intervals.

The stems being abundantly fed, owing to the size of the old tubers, rise from the ground with great strength and luxuriance, support well their foliage, and a larger breadth of this is thus, I

think, exposed to the light during the whole season than under any other mode of culture which I have seen; and as the plants acquire a very large size early in the summer, the tubers, of even very late varieties, arrive at a state of perfect maturity early in the autumn.

Having found my crops of potatoes to be in the last three years, during which alone I have accurately adopted the mode of culture above described, much greater than they had ever previously been, as well as of excellent quality, I was led to ascertain the amount in weight which an acre of ground such as I have described, the soil of which was naturally poor and shallow, would produce. A colony of rabbits had, however, in the last year done a good deal of damage, and pheasants had eaten many of the tubers which the rabbits had exposed to view; but the remaining produce per acre exceeded five hundred and thirty-nine bushels of eighty-two pounds each,—two pounds being allowed in every bushel on account of a very small quantity of earth which adhered to them.

The preceding experiments were made with a large and productive variety of potato only; but I am much inclined to think that I have raised, and shall raise in the present year, 1828, nearly as large a produce per acre of a very well-known small early variety, the ash-leaved kidney potato. Of this variety I selected in the present spring the largest tubers which I could cause to be produced in the last year; and I have planted them nearly in contact with each other in the rows, and with intervals, on account of the shortness of their stems, of only two feet between the rows. The plants at present display an unusual degree of strength and vigour of growth, arising from the very large size (for that variety) of the planted tubers; and as large a breadth of foliage is exposed to the light by the small, as could be exposed by a large variety; and as I have always found the amount of the produce, under any given external circumstance, to be regulated by the extent of foliage which was exposed to light, I think it probable that I shall obtain as large, or very nearly as large, a crop from the small variety in the present year as I obtained from the large variety in the last. I have uniformly found that, to obtain crops of potatoes of great weight and excellence, the period of planting should never be later than the beginning of March.

POSTCRIPT.

March 23, 1829.—Somewhat contrary to my expectations, the

produce of the small early potato exceeded very considerably that of the large one above mentioned; being per acre 665 bushels of 82 pounds. It is usually calculated by farmers that eighty pounds of potatoes, though eaten raw, after they have begun to germinate, will afford two pounds of pork; and I doubt much if the haulm, and the whole of the manure made by the hogs, were restored to the ground, whether it would be in any degree impoverished. I am not satisfied that it would not be enriched,—an important subject for consideration in a country of which the produce is at present unequal to support its inhabitants, and which produce is, I confidently believe and fear, growing gradually less, whilst the number of its inhabitants is rapidly increasing."

"ON THE POTATO.

[*Read before the* HORTICULTURAL SOCIETY, *February* 1*st*, 1831.]

IF the potato could only be employed, as it has chiefly been, to afford vegetable food to mankind, its improvement would be an exceedingly important object; for, circumstanced as this country is, it must necessarily constitute a large part of the food of the poorer classes; and it is consumed in large quantities at the tables of the affluent and luxurious. But I am convinced, by the evidence of experiments which I have been some years in making, that the potato plant, under proper management, is capable of causing to be brought to market a much greater weight of vegetable food, from any given extent of ground, than any other plant which we possess, with equal profit to the farmer. The Swedish turnip may, in certain seasons and when the soil is favourable, rival, and perhaps excel it; but a total failure of crops of that plant is an event of no unfrequent occurrence, and partial failures occur in almost every season; whilst by proper culture, and selection of varieties which vegetate and acquire maturity in successive parts of summer and autumn, there is not any crop which I conceive to be so certain as that of potatoes; and it has the advantage of being generally most abundant, when the crops of wheat are defective: that is, in wet seasons.* And, I think, I shall be able to

* Failures of crops of potatoes occur in Ireland, because the excessive poverty of the peasantry compels them to plant their ground generally with less than one-fifth of the proper quantity of potatoes; and all the Irish varieties which I have seen have been unproductive, though generally of exceedingly good quality; the Irish mode of culture is also, I have reason to believe, excessively bad.

adduce some strong facts in support of my opinion, that by a greatly extended culture of the potato, for the purpose of supplying the markets with vegetable food, a more abundant and more wholesome supply of food for the use of the labouring classes of society may be obtained than wheat can ever afford, and, I believe, of a more palatable kind to the greater number of persons. I can just recollect the time when the potato was unknown to the peasantry of Herefordshire, whose gardens were then almost exclusively occupied by different varietie sof the cabbage. Their food at that period chiefly consisted of bread and cheese, with the produce of their gardens, and tea was unknown to them. About sixty-six years ago, before the potato was introduced into their gardens, agues had been so extremely prevalent, that the periods in which they, or their families, had been afflicted with that disorder, were the eras to which I usually heard them refer in speaking of past events; and I recollect being cautioned by them frequently not to stand exposed to the sun in May, lest I should get an ague. The potato was then cultivated in small quantities in the gardens of gentlemen; but it was not thought to afford wholesome nutriment, and was supposed by many to possess deleterious qualities. The prejudices of all parties, however, disappeared so rapidly, that within ten years the potato had almost wholly driven the cabbage from the garden of the cottagers. Within the same period, ague, the previously prevalent disease of the country, disappeared; and no other species of disease became prevalent. I adduce this fact, as evidence only, that the introduction of the potato was not injurious to the health of the peasantry at that period; but whether its production was, or was not, instrumental in causing the disappearance of ague, I will not venture to give an opinion. I am, however, confident, that neither draining the soil (for that was not done), nor any change in the general habits of the peasantry, had taken place, to which their improved health could be attributed.

Bread is well known to constitute the chief food of the French peasantry. They are a very temperate race of men; and they possess the advantages of a very fine and dry climate. Yet the duration of life amongst them is very short, scarcely exceeding two-thirds of the average duration of life in England, and in some districts much less. Dr. Hawkins, in his Medical statistics, states upon the authority of M. Villerme, that in the department of Indre,

"one-fourth of the children born die within the first year, and half between fifteen and twenty, and that three-fourths are dead within the space of fifty years." Having inquired of a very eminent French physiologist, M. Dutrochet, who is resident in the department of Indre, the cause of this extraordinary mortality, he stated it to be their food, which consisted chiefly of bread; and of which he calculated every adult peasant to eat two pounds a day. And he added, without having received any leading question from me, or in any degree knowing my opinion upon the subject, that if the peasantry of his country would substitute (which they could do) a small quantity of animal food with potatoes instead of so much bread, they would live much longer, and with much better health. I am inclined to pay much deference to M. Dutrochet's opinion; for he combines the advantages of a regular medical education with great acuteness of mind, and I believe him to be as well acquainted with the general laws of organic life as any person living: and I think his opinion deserves some support from the well known fact, that the duration of human life has been much greater in England during the last sixty years than in the preceding period of the same duration. Bread made of wheat, when taken in large quantities, has probably, more than any other article of food in use in this country, the effect of overloading the alimentary canal; and the general practice of the French physicians points out the prevalence of diseases thence arising amongst their patients.

I do not, however, think or mean to say, that potatoes alone are proper food for any human being: but I feel confident, that four ounces of meat, with as large a quantity of good potatoes as would wholly take away the sensation of hunger, would afford, during twenty-four hours, more efficient nutriment than could be derived from bread in any quantity, and might be obtained at much less expense.

I now proceed to give an account of the result of the experiment above-mentioned, which, I hope, will be found sufficiently interesting to attract the attention of the Members of this Society. It has been proved by many other persons, as well as by myself, that if all the blossoms of a potato plant be picked off, as soon as they become visible, the quantity of tubers will be considerably increased, particularly if the variety be one which produces seeds; and I have shown that the cause why early varieties of the potato do not afford

blossoms is the preternaturally early disposition of the plant to generate its tuberous roots. The early varieties are of dwarfish growth, and therefore improper for extensive field culture; but I have found that by cross-breeding between those and varieties of tall and luxuriant growth, I can communicate to the latter the habit of producing tubers only, without blossom; with, I have reason to hope, considerable advantages. I now possess a good many of such varieties, selected from a very great number, which prove totally worthless; but many of those varieties which do not produce blossoms, have other defects, which render them of little value. The stems of some of these are not strong and rigid enough to support themselves and their foliage; and they are consequently beaten down by rain and winds. The foliage of one stem consequently often becomes so placed as to shade the foliage of another; and as the whole material of the tubers is formed of living matter, which is generated in the leaves only, and as all leaves which are shaded become inefficient and useless, a sufficient degree of strength and rigidity in the stems to enable them to retain their foliage in its first position is very important; though I believe that this circumstance has not hitherto attracted the attention of any cultivator of the potato.

The tubers of other varieties, which were in all other respects apparently good, were defective in specific gravity, and consequently aqueous and worthless; and in others, veins of a red colour extended in to the body of the tubers, and gave an unpleasant colour to their meal, which was in some other respects of very good quality. But I have obtained several varieties which do not blossom, and which are, as far as I am at present capable of judging, without any particular defect; though I am far from thinking I possess any variety which has even approximated to the greatest state of perfection which the species is capable of attaining.

I have succeeded in obtaining, as I wished, some varieties which vegetate early, and others late, in the spring. Those of the first-mentioned habit will generally be found to afford the largest produce by having the advantages of a longer summer; but it is desirable to possess varieties of less excitable habits, because such usually remain good till a later period in the spring, when good vegetables are not always readily obtainable. I have also succeeded in obtaining varieties which do not vegetate till late in the

spring, and which, nevertheless, acquire perfect or rather early maturity in autumn, and there are probably climates in which such varieties would be peculiarly valuable; and the ductility and obedience of this species of plant to human will is so great, that I doubt whether, by the creation and selection of proper varieties, as abundant a produce might not be obtained within the limits of the frigid zone as in the torrid zone, of which the potato is a native. The weather in some parts of the coast of Norway, within the limits of the frigid zone, is very warm and bright during a period, which I believe to be quite long enough to ripen any early variety of the potato perfectly.

It is my wish to send in the spring one or two potatoes of each of the varieties which I think likely to prove valuable; and I shall be happy subsequently to send a quantity of any which may be approved.

In raising varieties of the potato from seeds it is always expedient to use artifical heat. I have trained up a young seedling plant in a somewhat shaded situation in the stove till it has been between four and five feet high, and then removed it to the open ground in the beginning of May, covering its stem during almost its whole length lightly with mould, and by such means I have obtained within the first year nearly a peck of potatoes from a single plant. But I usually sow the seeds in a hotbed early in March, and, after having given them one transplantation in the hotbed, I have gradually exposed them to the open air, and planted them out in the middle of May: and, by immersing their stems rather deeply into the ground, I have within the same season usually seen each variety in such a state of maturity as has enabled me to judge, with a good deal of accuracy, respecting its future merits.

I stated, in a former communication two years ago, that I had obtained from a small plantation of the early ash-leaved kidney potato a produce equivalent to that of 665 bushels, of 80 pounds each, per acre; and my crop of that variety in the present year was to a small extent greater. By a mistake of my workmen I was prevented ascertaining with accuracy the produce per acre of a plantation of Laukman's potato; but one of my friends having made a plantation of that variety precisely in conformity with the instructions given in my former communication to this society, I requested that he would send me an accurate account of the

produce; which I have reason to believe he did, for its amount very nearly agreed with my calculation, upon viewing the growing crop about six weeks before it was collected. The situation in which this crop grew was high and cold, and the ground was not rich, but the part where the potatoes to be weighed were selected was perfectly dry, and afforded a much better crop than the remainder of the field; which was planted with several different varieties. I calculated the produce of the selected part to be 600 bushels per acre, and the report I received, and which I believe to have been perfectly accurate, stated it to be 628. If this produce be eaten by hogs, or cows, or sheep, for all are equally fond of potatoes, I entertain no doubt whatever that it will afford twenty times as much animal food as the same extent of the same ground would have yielded in permanent pasture; and I am perfectly satisfied, upon the evidence of facts which I have recently ascertained, that, if the whole of the manure afforded by the crops of potatoes above-mentioned be returned to the field, it will be capable of affording as good, and even a better, crop in the present year than it did in the last; and that as long a succession of at least equally good crops might be obtained as the cultivator might choose, and with benefit to the soil of the field. Should this conclusion prove correct, a very interesting question arises, viz.—whether the spade husbandry might not be introduced upon a few acres of ground surrounding, on all sides, the cottages of day-labourers, to and from every part of which the manure and the produce might be conveyed without the necessity of a horse being ever employed.

A single man might easily manage four statute acres thus situated, with the assistance of his family; and if nothing were taken away from the ground except animal food, I feel confident that the ground might be made to become gradually more and more productive, with great benefit to the possessor of the soil, and to the labouring classes, wherever the supply is found to exceed the demand for labour."

"ON THE CULTURE OF THE POTATO.

Read before the Horticultural Society, March 19th, 1833.

The fact that every variety of potato where it has been long propagated from parts of its tuberous roots becomes

less productive, is, I believe, unquestionable. I have often witnessed the progressive decay of vigour, and the different effects of the influence of age, upon many different varieties. The quality of some has remained perfectly good, after the produce in quantity has become highly defective; whilst in others that has disappeared with the vigour of the plant. I brought to this place a single tuber of Lankman's potato soon after that was imported: the produce of that variety was then, and continued during some successive years, very great; but its vigour was gradually diminished; and in the last year its produce was at least one third (more than seven tons per acre) less than I obtained from the same soil, and under in every respect the same management, from other varieties of nearly similar habits, but which had recently sprung from seed. The propagation of expended varieties, therefore, appears to me to be one of the causes why the crops of potatoes generally have been found so much less than those which I have stated to have been produced here. I have received letters within a few months from persons in different parts of the kingdom, informing me that they have been unable to obtain by any mode of culture above two hundred and fifty or three hundred bushels of potatoes from an acre of good and well-manured ground. I have in answer desired to know the age of the varieties cultivated; but upon that point I have uniformly found my correspondents totally uninformed; communicating to me, however, the important intelligence that the same varieties bore more abundantly at a former period, and often that the quality of the former produce was superior. When I first stated, in a former communication, that I had obtained a produce equivalent to six hundred and seventy bushels of eighty pounds per acre, I found some difficulty in obtaining credit for the truth of my statement, though I then felt perfectly confident that by first obtaining varieties better adapted to my purpose, I should be able to raise much heavier crops; and the following statement, in support of which I am prepared to adduce the most unquestionable evidence, will prove that my confidence was perfectly well founded.

I planted in my garden, in the last season, some tubers of a variety of potato of very early habits, but possessing more vigour of growth than is usually seen in such varieties. The soil in which they were planted was in good condition, but not richer than the

soils of gardens usually are, and the manure which it had received consisted chiefly of decayed oak leaves, which I prefer to other manures, because it never communicates a strong taste or flavor to any vegetable. No previous preparation was given to the soil, and the spot where the plantation was made was not fixed upon till the day of planting; and no manure of any kind was then given. Owing to the variety being of a very excitable habit, I planted the tubers at least nine inches deep in the soil, and I subsequently raised the mould in ridges three inches high to prevent the young plants sustaining injury from frost; but no subsequent moulding was given. I anticipated from the previous produce of the variety, which I had raised by cross-breeding from two early varieties in 1830, a very extraordinary crop; and I therefore invited several gardeners and farmers to witness the amount of it; and I procured the attendance of the two most eminent agriculturists of the vicinity, who were tenants to other gentlemen. The external rows (two deep), and the external plants at the ends of all the remaining rows, were taken away, and the produce of the interior part of the plantation was alone selected; and that was pronounced to be fully equivalent to nine hundred and sixty-four bushels and forty-three pounds, or 34 tons 8 cwt. 107 lbs. per statute acre. Still larger crops may, I feel satisfied, be obtained, and my opinion is, that more than a thousand bushels of potatoes may, and will be, obtained from an acre of ground.

An opinion is, I believe, generally prevalent, that varieties of potatoes of very high and luxuriant growth are capable of affording per acre the greatest weight of produce; but this is certainly erroneous. Such will grow in poorer soil, and, requiring wider intervals between the rows, are better calculated for culture with the plough; and therefore, perhaps, their produce may be raised at as little or less cost per bushel, though that is, I think, very questionable. Much time and much labour of the plant must be expended in raising the nutriment absorbed from the soil into the leaves upon the top of a very tall stem, and down again to the roots and tubers.

The potatoes, in the extraordinary crop of which I have above spoken, were not washed, and therefore a deduction must be made for a portion of soil which adhered to them: but that was small, owing to the dryness and nature of the soil. Supposing a deduc-

tion of one hundred and sixty-four bushels be made in the above-mentioned account, and to afford potatoes sufficient to plant the acre of ground again, eight hundred bushels would still remain; and these, if judiciously given to proper animals, would certainly give twelve hundred pounds of animal food."

Further:—

Mr. Knight's first communication to the Royal Society was a paper "Upon the inheritance of decay among fruit trees, and the propagation of debility by grafting," read April 30, 1795; and, in 1797, he published a "Treatise on the Culture of the Apple and Pear, and on the Manufacture of Cyder and Perry." In this work he repeated the same opinions which he advanced in his paper, viz., that vegetable, like animal life, has its fixed periods of duration; and that however the existence of a variety of a fruit-tree may be protracted beyond the natural life of the original seedling plant, by grafting, or by unusually favourable circumstances of soil or situation, still there is a period beyond which the debility incident to old age cannot be stimulated; and to this he attributed the cankered and diseased state of the most of trees of the old varieties of cyder apples in the orchards of Herefordshire.

This hypothesis was so contrary to generally received opinions, that at first it met with considerable opposition; but the increasing decay of the old fruits, even where grafted on the most vigorous stocks, and the superior healthiness of the new varieties produced from seed, has caused Mr. Knight's theory to be now almost universally adopted. To remedy the ill-consequences that would have followed the decay of old fruits, he set about raising new varieties of apples and pears from seed; but instead of following the old method of merely selecting seeds from good kinds, it occurred to him, that by artificially impregnating blossoms with the pollen of a different variety, possessing qualities of a contrary nature, but calculated, if combined with those of the kind operated upon, to produce excellence, and by then raising plants from the seeds so produced, the chances of obtaining valuable varieties would be considerably increased; and though many of the apples at first raised from seed in this manner did not answer his expectations, he eventually succeeded in creating new varieties of many fruits and excellent vegetables, which have long been cultivated

and highly prized by the horticulturists of England, and probably by those of most civilized countries to whose climate they are suited.

The idea of improving fruits by crossing seems to have been entertained by Lord Bacon, though he was ignorant of the method of accomplishing it. After stating the effects of this course in producing mules in the animal world, he thus proceeds: "The compounding and mixture of plants is not found out, which, nevertheless, if it be possible, is more at command than that of living creatures; wherefore it were one of the most noble experiments touching plants to find this art; for so you may have a great variety of new plants and flowers yet unknown. Grafting doth it not: that mendeth the fruit, or doubleth the flower, but it hath not the power to make a new kind—for the scion ever overruleth the stock."

If to Lord Bacon must be assigned the merit of having first suggested the possibility of producing new fruits in this manner, it was reserved for Mr. Knight to discover the means by which those "most noble experiments" were to be rendered successful; and to his discoveries we undoubtedly owe the innumerable varieties of excellent fruits that supply our tables.—*From Life of Thomas Andrew Knight, Esq., prefixed to a selection from his Physiological and Horticultural Papers.*

Following this, I reprint *in extenso* Professor Asa Gray's article on the subject in the American Journal of Science and Arts.

"DO VARIETIES WEAR OUT, OR TEND TO WEAR OUT?

By Professor Asa Gray.

This question has been argued from time to time for more than half a century, and is far from being settled yet. Indeed, it is not to be settled either way so easily as is sometimes thought. The result of a prolonged and rather lively discussion of the topic about forty years ago in England, in which Lindley bore a leading part on the negative side, was, if we rightly remember, that the nays had the best of the argument. The deniers could fairly well explain away the facts adduced by the other side, and evade the force of the reasons then assigned to prove that varieties were bound to die out in the course of time. But if the case were fully re-argued now, it is by no means certain that the nays would

win it. The most they could expect would be the Scotch verdict, 'not proven.' And this not because much, if any, additional evidence of the actual wearing out of any variety has turned up since, but because a presumption has been raised under which the evidence would take a bias the other way. There is now in the minds of scientific men some reason to expect that certain varieties die out in the long run, and this might have an important influence upon the interpretation of the facts that would be brought forward. Curiously enough, however, the recent discussions to which our attention has been called seem, on both sides, to have overlooked this matter.

But, first of all, the question needs to be more specifically stated if any good is to come from a discussion of it. There are varieties and varieties. They may, some of them, disappear or deteriorate, but yet not wear out—not come to an end from any inherent cause. One might even say, the younger they are the less the chance of survival, unless well cared for. They may be smothered out by the adverse force of superior numbers; they are even more likely to be bred out of existence by unprevented cross-fertilization or to disappear from mere change of fashion. The question, however, is not so much about reversion to an ancestral state, or the falling off of a high bred stock into an inferior condition. Of such cases it is enough to say that, when a variety or strain, of animal and vegetable, is led up to unusual fecundity or of size or product of any organ, for our good, and not for the good of the plant or animal itself, it can be kept so only by high feeding and exceptional care; and that with high feeding and artificial appliances come vastly increased liability to disease, which may practically annihilate the race. But then the race, like the bursted boiler, could not be said to wear out, while if left to ordinary conditions, and allowed to degenerate back into a more natural, if less useful state, its hold on life would evidently be increased rather than diminished.

As to natural varieties or races under normal conditions, sexually propagated, it could readily be shown that they are neither more nor less likely to disappear from any inherent cause than the species from which they originated. Whether species wear out, *i. e.*, have their rise, culmination, and decline from any inherent cause, is wholly a geological and very speculative problem, upon

which, indeed, only vague conjectures can be offered. The matter actually under discussion concerns cultivated domesticated varieties only, and, as to plants, is covered by two questions.

First, *will races propagated by seed*, being so fixed that they come true to seed, and purely bred, (not crossed with any other sort,) continue so indefinitely, or *will they run out in time*—not die out, perhaps, but lose their distinguishing characters? Upon this, all we are able to say is that we know no reason why they should wear out or deteriorate from any inherent cause. The transient existence or the deterioration and disappearance of many such races are sufficiently accounted for otherwise; as in the case of extraordinarily exuberant varieties, such as mammoth fruits or roots, by increased liability to disease, already adverted to, or by the failure of the high feeding they demand. A common cause, in ordinary cases, is cross-breeding, through the agency of wind or insects, which is difficult to guard against. Or they go out of fashion and are superseded by others thought to be better, and so the old ones disappear.

Or, finally, they may revert to an ancestral form. As offspring tend to resemble grand-parents almost as much as parents, and as a line of close-bred ancestry is generally prepotent, so newly originated varieties have always a tendency to reversion. This is pretty sure to show itself in some of the progeny of the earlier generations, and the breeder has to guard against it by rigid selection. But the older the variety is—that is, the longer the series of generations in which it has come true from seed—the less the chance of reversion: for now, to be like the immediate parents, is also to be like a long line of ancestry; and so all the influences concerned—that is, both parental and ancestral heritability—act in one and the same direction. So, since the older the race is, the more reason it has to continue true, the presumption of the unlimited permanence of old races is very strong.

Of course the race itself may give off new varieties: but that is no interference with the vitality of the original stock. If some of the new varieties supplant the old, that will not be because the unvaried stock is worn out or decrepit with age, but because in wild nature the newer forms are better adapted to the surroundings, or, under man's care, better adapted to his wants or fancies.

The second question, and one upon which the discussion about the wearing out of varieties generally turns, is,— *Will varieties propagated from buds, i. e., by division, grafts, bulbs, tubers and the like, necessarily deteriorate and die out?* First, Do they die out as a matter of fact? Upon this, the testimony has all along been conflicting. Andrew Knight was sure that they do, and there could hardly be a more trustworthy witness.

'The fact,' he says, fifty years ago, 'that certain varieties of some species of fruit which have been long cultivated cannot now be made to grow in the same soils and under the same mode of management, which was a century ago so perfectly successful, is placed beyond the reach of controversy. Every experiment which seemed to afford the slightest prospect of success was tried by myself and others to propagate the old varieties of the apple and pear which formerly constituted the orchards of Herefordshire, without a single healthy or efficient tree having been obtained; and I believe all attempts to propagate these varieties have, during some years, wholly ceased to be made.'

To this it was replied, in that and the next generation, that cultivated vines have been transmitted by perpetual division from the time of Romans, and that several of the sorts, still prized and prolific, are well identified, among them the ancient Græcula, considered to be the modern Corinth or Currant grape, which has immemorially been seedless; that the old Nonpariel apple was known in the time of Queen Elizabeth; that the White Beurré Pears of France have been propagated from the earliest times; and that Golden Pippins, St. Michael Pears, and others said to have run out, were still to be had in good condition.

Coming down to the present year, a glance through the proceedings of pomological societies, and the debates of farmers' clubs, brings out the same difference of opinion. The testimony is nearly equally divided. Perhaps the larger number speak of the deterioration and failure of particular old sorts: but when the question turns on 'wearing out,' the positive evidence of vigorous trees and sound fruits is most telling. A little positive testimony outweighs a good deal of negative. This cannot readily be explained away while the failures may be, by exhaustion of soil, incoming of disease, or alteration of climate or circumstances. On the other hand, it may be urged that, if a variety of this sort is

fated to become decrepit and die out, it is not bound to die out all at once, and every where at the same time. It would be expected first to give way wherever it is weakest, from whatever cause. This consideration has an important bearing upon the final question, are old varieties of this kind on the way to die out on account of their age or any inherent limit of vitality?

Here, again, Mr. Knight took an extreme view. In his essay in the Philosophical Transactions, published in the year 1810, he propounded the theory, not merely of a natural limit to varieties from grafts and cuttings, but even that they would not survive the natural term of the life of the seedling trees from which they were originally taken. Whatever may have been his view of the natural term of the life of a tree, and of a cutting being merely a part of the individual that produced it, there is no doubt that he laid himself open to the effective replies which were made from all sides at the time, and have lost none of their force since. Weeping willows, bread-fruits, bananas, sugar-cane, tiger-lilies, Jerusalem artichokes, and the like, have been propagated for a long while in this way, without evident decadence.

Moreover, the analogy upon which his hypothesis is founded will not hold. Whether or not one adopts the present writer's conception, that individuality is not actually reached or maintained in the vegetable world, it is clear enough that a common plant or tree is not an individual in the sense that a horse or man, or any one of the higher animals, is—that it is an individual only in the sense that a branching zoophyte or mass of coral is. *Solvitur crescendo:* the tree and the branch equally demonstrate that they are not individuals, by being divided with impunity and advantage, with no loss of life, but much increase. It looks odd enough to see a writer like Mr. Sisley reproducing the old hypothesis in so bare a form as this: 'I am prepared to maintain that varieties are individuals, and that as they are born they must die, like other individuals.' 'We know that oaks, sequoias, and other trees live several centuries, but how many we do not exactly know. But that they must die, no one in his senses will dispute.' Now what people in their senses do dispute is, not that the tree will die, but that other trees, established from cuttings of it, will die with it.

But does it follow from this that non-sexually propagated

varieties are endowed with the same power of unlimited duration that are possessed by varieties and species propagated sexually—*i.e.*, by seed? Those who think so jump too soon at their conclusion. For, as to the roots, it is not enough to point out the diseases or the trouble in the soil or the atmosphere to which certain old fruits are succumbing, nor to prove that a parasitic fungus (*Peronospora infestans*) is what is the matter with potatoes. For how else would constitutional debility, if such there be, more naturally manifest itself than in such increased liability or diminished resistance to such attacks? And if you say that, anyhow, such varieties do not die of old age—meaning that each individual attacked does not die of old age, but of manifest disease—it may be asked in return, what individual man ever dies of old age in any other sense than of a similar inability to resist invasions which in earlier years would have produced no noticeable effect? Aged people die of a slight cold or a slight accident, but the inevitable weakness that attends old age is what makes these slight attacks fatal.

Finally, there is a philosophical argument which tells strongly for some limitations of the duration of non-sexually-propagated forms, one that probably Knight never thought of, but which we should not have expected recent writers to overlook. When Mr. Darwin announced the principle that cross-fertilization between the individuals of a species is the plan of nature, and is practically so universal that it fairly sustains his inference, that no hermaphrodite species continually self-fertilized would continue to exist, he made it clear to all who apprehend and receive the principle, that a series of plants propagated by buds only must have weaker hold of life than a series reproduced by seed; for the former is the closest possible kind of close breeding. Upon this ground such varieties may be expected ultimately to die out; but 'the mills of the gods grind so exceeding slow' that we cannot say that any particular grist has been actually ground out under human observation.

If it be asked how the asserted principle is proved or made probable, we can here merely say that the proof is wholly inferential. But the inference is drawn from such a vast array of facts that it is well nigh irresistible. It is the legitimate explanation of those arrangements in nature to secure cross-fertilization

in the species, either constantly or occasionally, which are so general, so varied and diverse, and we may add so exquisite and wonderful, that, once propounded, we see that it must be true. What else, indeed, is the meaning and use of sexual reproduction? Not simply increase in numbers; for that is otherwise effectually provided for by budding propagation in plants and many of the lower animals. There are plants, indeed, of the lower sort, in which the whole multiplication takes place in this way, and with great rapidity. These also have sexual reproduction; but in it two old individuals are always destroyed to make a single new one! Here propagation diminishes the number of individuals 50 per cent. Who can suppose that such a costly process as this, and that all the exquisite arrangements for cross-fertilization in hermaphrodite plants, do not subserve some most important purpose? How and why the union of two organisms, or generally of two very minute portions of them, should re-enforce vitality, we do not know and can hardly conjecture. But this must be the meaning of sexual reproduction.

The conclusion of the matter from the scientific point of view is, that sexually propagated varieties, or races, although liable to disappear through change, need not be expected to wear out, and there is no proof that they do; but, that non-sexually propagated varieties, though not liable to change, may theoretically be expected to wear out, but to be a very long time about it."—*American Journal of Science and Arts*, *February*, 1875.

From the foregoing and indeed from all the text books on Botany it is clear that Mr. Knight's biographer was mistaken, when he imagined that his views were "almost universally adopted." However that may be, I hold fast by my own text, "that if cutting a plant into two equal parts, and growing it in different parts of space, do not constitute it two individuals, neither can cutting it into two unequal parts, and calling one the stem and the other the scion, do so." In neither case is there any new birth, nor consequently any rejuvenation of life, and all the analogies of nature point to this conclusion.

Notwithstanding alternate generation, and multiplication by fission, &c., the sexual union, I believe, is at the bottom of the life of the individual; and I also, like Mr. Sisley, am pre-

pared to maintain that varieties are individuals; and as they are born they must die, like other individuals; and I submit that I have proven it to demonstration by dividing a plant longitudinally into two equal parts, by cutting down exactly through the centre of the pith, by leaving to each half an equal quantity of roots, and by growing it in separate parts of space. I submit that the experiment cannot be got over, and that the question is settled; that plants *cannot* be propagated for ever by their buds.

In my judgment, a tree is not a multitude of individual lives residing in one common structure, but on the contrary a tree and its buds, and the forest which may be derived from them by cuttings, is only *one* individual life residing in a *multitude* of structures. I see it and I feel it, or at least it seems to me to be visible and palpable.

Let a branch of a plant be converted into a layer, it puts forth adventitious roots, and through them it obtains nourishment, at the same time setting up the commencement of a duplicate sap circulation, and it is still unquestionably a branch of the original plant, whatever a branch or a bud may be considered to be; for, sever the adventitious roots, and it resumes its original position as a common branch of the plant.

Now sever the connection with the stem, let it enter on a separate existence for itself upon its adventitious roots, and what is it? A new individual freshly born, endowed with a stock of life equal to that with which the plant from which it was severed was endowed, when it first started into life?

I think it is impossible; for where is the new birth, the new starting point of life? If anywhere, it must have been in the germination of the leaf-bud; but it could not have been there, because, if it had not been separated from the stem, it would have existed under the same conditions as the other unseparated buds, and all must have grown old together.

What then does this layer seem to me to be? Merely a separated branch of the original plant, animated by the same life, which derived its existence or rejuvenation rather, from the sexual union which generated the seed, which developed into the plant from which it was severed.

The supply of food through the roots of the original plant has been cut off, and the duplicate circulation of the sap has been fully

established. The one life now flows through two circles, but the origin is the same, and so by necessary consequence is its duration. The layer, therefore, and the original plant must both die of old age at the same time, on the expiration of the term of life alloted to the seed from which they both alike sprang; and the same law must apply to all the successive layers, suckers, grafts, &c., which may be derived from them.

They are all so many circulations of the same individual (and, as I think, indivisible) life; because they all spring from the same sexual union, the sole origin of all life—all individual life. That is, provided (as I assume) that vegetative multiplication does not afford a new starting point of life—is not equivalent to the sexual union. And what reason is there to suppose that it is? The duration of the life of plants is yet unknown, and therefore the periods of time through which they may be continued or multiplied by buds is also unknown; it is, no doubt, in some instances, thousands of years; but that fact is no evidence that another law of reproduction is in existence, or that the sexual union is not amply sufficient for the propagation and multiplication of individuals. It is more than sufficient, because it always eventuates in the production of surplus populations, and in the seemingly inequitable destruction of the vast majority of its products—the multitude of the *un*-fittest.

And relative to this law of vegetative multiplication, this incomprehensibly vast power of self-extension of plants, by virtue of their inherent vegetative energy, by which they absorb and organize enormous quantities of matter through immense ranges of time—it has not yet been measured, and therefore, as it seems, it has been assumed to be "unlimited." But I think it will be found to have a limit, and that that limit is the exhaustion of the stock of life generated by the sexual union. It appears to be wonderful that a seed of the potato should have the capacity of developing itself into an aggregate which, in one year's produce, might perhaps, cover the half of England, but if it be a fact, it must be accepted of as such, and after all, it is no more and no less wonderful than the law of *true* reproduction.

It is stated that "the sugar cane is propagated *naturally* by the stem; strawberries, by runners; potatoes by tubers, &c."; and of plants generally, that "their organizing forces are diffused through

their structures, whence arises the possibility of a *multiplication* of individual plants by simple subdivision of the vegetative structure of a single specimen—a process which is so frequently and abundantly manifested as to throw the proper *reproduction* by the seed into the background,—that the duration of herbaceous perennials may be regarded as unlimited, since they are always placed in a position to form new absorbing organs (roots) in the vicinity of their buds." But the buds of the *Wellingtonia Gigantea*, 300 or 400 feet high, are not in the vicinity of their roots, and the trees have already existed some thousands of years longer than any plants which have been derived from a succession of buds; and when the time comes when the vitality imparted to these derivative plants, and to the Wellingtonia Gigantea, by the sexual union, becomes exhausted, will not the force fail them which has heretofore enabled them to form new absorbing organs? And what does the above quoted phrase, "may be regarded as unlimited," mean? It means "is eternal." The limit of life—the exhaustion of the vital force engendered by the sexual union—is not perceived, and the defective perception is attempted to be concealed by the use of words of an indefinite signification.

It is true, nevertheless, and in an agricultural point of view it is a most important fact, that the original stem dies always, or almost always, many years, possibly thousands, before the plants which have been derived from it by a succession of buds;—and why? Not because these derivative plants have been endowed with an extension of the term of life allotted to the seed from which they and the original plant sprang; for from what source should that endowment come? And not because of the exhaustion of the stock of life of the stem, because the existence of these derivative plants proves that it is not exhausted; but because, as I imagine, of the exhaustion of the supply of food within reach of the roots, perhaps partly because of the noxious action of the excrements of the plant itself, of which the soil surrounding its roots must eventually be almost entirely composed, inconveniences from which the derivative plants, transplanted into fresh soil, are exempt: an exemption which enables them, or some of them, to reach the full term of existence. Also the stem of the old plant becomes in time almost all converted into heart-wood; the circulation of the sap is almost entirely arrested, and this in itself seems sufficient to account for death

before exhaustion of the full allotment of life, and explains why the plants derived by cuttings from the old stem live longer—*their* circulation is not destroyed by the growth of heart-wood. With regard to the potato,—the roots, stems, and foliage die annually, the seed is thrown away, the germs in the tubers are planted in fresh soil, and they are always placed in a position to form new absorbing organs (roots) and so to live out life to the end—a period, I think, of about 60 or 80 years. That is, they *may* live to the end of their term of life; but in point of fact, like other individuals, they are almost invariably cut off by disease or some adverse influence which their vitality, enfeebled by old age, is unable to withstand. Again, it is said that the vine has been handed down to us by a succession of cuttings from the time of the Romans. If so, this merely proves that the vine has a life of, at least, so many centuries, a matter of no moment in the consideration of this question, and nothing extraordinary in itself, seeing that some original plants have lived much longer, some thousands of years longer, than any succession of cuttings, the vine included. And this law of sexual reproduction, if the only true law of reproduction, must of necessity affect the vine and the sugar-cane as well as the potato. Grafted fruit-trees, &c., of course come under the law; but that is a matter of comparatively infinitely small importance.

We do not yet know the duration of the life of the potato, or of any perennial plant (and I submit that the potato is really a perennial). It varies no doubt in different individuals; but it seems absurd to suppose that its powers of vegetative multiplication are infinite, and that the process of cutting it to pieces and planting it in the earth periodically, will have the effect of conferring upon it immortality. On the contrary, the current vehicle of life—the existing plant—dies annually, subtracting one year of life from the original stock, while the life itself is carried forward in the successive annual buds and tubers, and undergoes annual depletion until exhausted.

Last spring (1875), by dint of advertisements, here and in England, and by procuring the names of large cultivators of the plant, I succeeded in inducing about 150 people to try to grow the potato from the seed which I sent them. In not one case out of the 150 was the result worth one farthing. Having never grown a potato from the seed myself, I could only give them such instructions as I

found in the books, and even these they did not attend to. I forgot or omitted to tell them that the produce of each seed, being a different individual ("variety") ought to be kept separate from the others, and in every instance which I know of they mixed them all up together, so that in place of a good selection to propagate by the tuber, they have only an average of the plant, average in yield, average in quality and average in liability to disease, not ripening together, and not being capable of being cooked together.

Forty or fifty of them reported to me, as they all had promised to do, the results of their experiments, and their reports would seem to show that the seedling potatoes grown under the common usual conditions are much less liable to the disease than old varieties.

Some of them grow as much as 200 acres each season; some of them agree with me that a branch of a plant is only a branch of an individual life, whether it be separated from the original stem or not;—almost all of them think that the potato in time "deteriorates," "degenerates," "wears out;" and they are unanimous in believing that new "varieties" should now be obtained from the seed.

My experiments were, however, more carefully made, and very naturally so, from the amount of money and time I had previously expended on the condensation and preservation of the plant, or the plant's tubers rather; a matter which I was led to go into, as follows:—Twenty years ago (1856), turning over the leaves of M'Culloch's Commercial Dictionary, I found it stated "that the rapid extension of the taste for, and the cultivation of, this exotic has no parallel in the history of industry; it (meaning the potato) has had, and will continue to have, the most powerful influence over the condition of mankind—but it labours under the disadvantage of being incapable of preservation, so that the surplus produce of a luxuriant crop cannot be stored over (like the cereals) to meet a subsequent scarcity; and from its great bulk and weight and the difficulty of preserving it on shipboard, the expense of carrying it from one country to another is so very great, that a scarcity can never be materially relieved by importing it from abroad."

Thereupon I set before myself the task of endeavouring to condense and preserve it.

After ten years' labour, suddenly at night, the truth flashed on me. I had perfected the invention five years previously, and I had

not seen it. I wanted to dry the potato like a Normandy pippin, keeping together all its nutritious matters; and all the time I had been separating it into its liquid and solid constituents. Use the separated solid constituents in the usual manner for the production of starch, sugar and spirit, and make use of the liquid flesh-forming constituents for cattle-food (heretofore lost) and the thing is done—and so it was. Or condense the liquid constituents by evaporation, like sugar-cane juice, or like the syrup of the potato itself, and the result is the "extract of meat," plus a little lime—the complete condensation of the whole of the potato in a form capable of preservation for ever—the absolutely perfect solution of the task I had set before myself ten years before. At first sight it seems curious, the extraction of the "extract of meat" from a vegetable; but a glance shows that it cannot be otherwise. The animal obtained its "extract of meat" from the vegetable; and these processes merely obtain the same substances direct from the vegetable, without the intervention of the animal; and I believe, when we shall have full crops of potatoes free of disease, (as we shall have I think in a few years more,) it may become available as food for man. The juice, when condensed under atmospheric pressure, has the flavour a little touched by the heat; but evaporate it in vacuo and I expect that that defect will be obviated. I may state that I have often eaten soup made of this extract, and have found it to have the same effect on my system as the extract of meat. Both of them taken in large quantities affect the action of the heart perceptibly, and in the same manner:—this result being due, no doubt, to the large quantity of potash which both contain. The smell and taste of both are the same:—the analyses are as follows:—

Results of the Analysis of a Sample of Vegetable Juice, received from Mr. TORBITT, of Belfast.

PER CENTAGE COMPOSITION.

	Grains.
Water	29·76
Organic Alcoholic Extractive Matter	24·32
Organic Extractive Matter, insoluble in Alcohol, containing 2·19 Grains of Starch	18·30
Mineral Matter	*27·62
	100·00

Containing 2·76 Grains of Nitrogen, equal to 17·66 Grains of Albuminous Matter.

*Containing—
2·13 Grains Sulphuric Acid.
3·89 „ Chlorine.
1·50 „ Lime.
1·39 „ Phosphoric Acid.
13·86 „ Potash.

Results of the Analysis of a Sample of Liebig's Extract of Meat.

PER CENTAGE COMPOSITION.

	Grains.
Water	18·56
Organic Alcoholic Extractive Matter	45·43
Organic Extractive Matter, insoluble in Alcohol, containing 8·56 Grains of Gelatine	13·93
Mineral Matter	*22·08
	100·00

Containing 7·19 Grains of Nitrogen, equal to 46·02 Grains of Albuminous Matter.

*Containing—
0·49 Grains Sulphuric Acid.
1·84 „ Chlorine.
8·22 „ Phosphoric Acid.
10·49 „ Potash.

(Signed)

ARTHUR HILL HASSALL,

LONDON.

Then the next step was to test the invention practically. I put up here in Belfast an experimental factory; I extracted 120 gallons of juice out of each ton of tubers, containing the whole flesh-forming matters of the plant; rasped up the compressed solid matters; separated rather more starch than is obtained by the usual processes, and I had then remaining that which is now lost—to wit, the 120 gallons of juice, and the pulp or raspings, which always contain a considerable quantity of starch, and which forms little mountains at all the potato starch manufactories. These two lost products, the juice and pulp, I mixed together, adding the necessary quantity of salt, and used as cattle food; and I found, as was to be expected, that it formed a food which they preferred to almost any other which could be given to them.

Having now reached the end of my arguments, and my narrative; I proceed to reprint some instructions for growth of the plant from the seed and a few results of experiments.

"In February, if you have the convenience of a hot-bed or hot-house, let the seed be sown thinly in flower-pots of rich light earth, and cover very lightly. In a month or six weeks, or as soon as the young plants are an inch high, they are to be raised carefully, and planted singly in small pots, and placed in a frame where they will have a very little warmth, and where they must have plenty of air and water as required until the middle of May, when they may be turned out of the pots into the open ground, without breaking the ball. By this treatment the potatoes will be so large the first year as to enable you to judge of their merits. If you cannot command artificial warmth, sow the seed in shallow drills of light rich earth early in April, and transplant the young plants in rich earth in June; raise the potatoes at the usual time, and treat them afterwards in the usual way: they will prove themselves the second year." (*Gard. Chron.* 1844, 806.)

"Zander, Count Arnim's gardener, at Boitzenburg (in Mecklenburg), has succeeded in producing a good crop of potatoes from seed the same year as it was sown, and as large as those from sets; and those seedling potatoes are entirely free from disease. His plan is as follows: he gathers the apples before the frost sets in (according to others a slight frost does not injure them), and keeps them in a dry place till the end of January. The apples are then crushed with the hand into a vessel, where they lie from six to eight days to rot, that the seeds may be easily separated from the pulp. Water is then poured on, and the seed is washed and dried like cucumber seed, and put away in a dry place. At the end of March, or beginning of April, the seed is sown in a hot-bed, and treated much the same as other culinary plants. If there is a convenient place for a hot-bed near a wall or house, exposed to the sun, glass is not necessary; the plants may be treated like tuberous plants, but as they are very susceptible of frost at night, they should be covered with straw or boards, which can easily be done, as the bed is surrounded with boards set in the ground, upon which the covering can be laid without injuring the plants. In May, if the plants are well grown, they can be planted out in a light soil about the usual distance that sets are planted. Zander,

in 1845, sowed early potato seed on the 11th of April, and planted them out on the 26th of May; and here we may remark, that vegetation is 14 days later in Boitzenburg than in Berlin. The plants produced from 1 to 1½ gallons (metre) of tubers. One plant even produced 280 tubers. There were of course a great many small tubers among them, yet the produce of large ones was, on the whole, equal to the produce from sets. As Zander has followed this plan for five years, he is now able to give seed to several gentlemen's gardeners and labourers. The potatoes used by them all proved healthy, while the disease was everywhere, and even in the neighbourhood. The space of half a square rood of land (7 feet) is sufficient to raise enough to plant one acre." (*Preussische Zeitung, Oct.* 1845.)

Another mode is the following:

"When the 'potato apple or plum' is ripe, preserve it from frost till it is shrivelled, when the pulp, &c. may be mixed with peat-mould, and preserved in that way till sowing time. About this season a slight hot-bed may be made, and when filled to about one foot from the glass a layer of turf should be placed above the fermenting material, with the grassy side down, and over this about four inches in depth of peat-mould and rotten dung, or fine rich mould, should be spread; then sow the seed in rows, four inches apart, adding a little soil between them as the plants advance. As the plants grow they should be freely exposed in fine weather, and gently watered; and when they have advanced to about six inches in height, in the beginning of May, or when all danger of frost is over, they may be planted out into rows of a suitable distance apart, placing the plants pretty thickly in the rows. Nothing more is now needed but to attend to them in the usual way. In a small bed, of three or four feet across, some thousands of plants may be raised." (*Gard. Chron.* 1845, 260.)

The following is Mr. Macartney's method of obtaining new kinds of potatoes from seed:—"Sow the seed in a hot-bed, about the mindle of February, in lines six inches apart, a quarter of an inch deep, and very thin. When water is necessary, sprinkle it between the lines, but avoid wetting the plants, as that would injure them. A little air must be given before they are watered.

"As the plants rise, rich earth, carefully put between the lines, will add fresh vigour to them; but the tops of the plants must not

be covered by these mouldings, which should be occasionally repeated, until they are fit for transplanting. To prepare them for this, about the end of April they must be plentifully refreshed with air; and, two hours before removing them, they must be plentifully watered all over, and the glasses covered with bass mats, to prevent the sun, if shining at the time, from scorching the plants. Take each plant up carefully, with a ball of earth attached to it, and plant them in trenches, as you would celery, only with this difference, the distance from plant to plant in the lines must be 18 inches; and if the sun should be shining out strong at the time of planting, a flower-pot should be placed over each to prevent flagging; for, with all your care in taking up, a good many of the fibres will be broken. After the plants have established themselves, remove the pots, and earth up occasionally, as long as the space between them will admit of it. The produce of new kinds of potatoes raised in this manner is generally prodigious for twelve years afterwards. The best manure is yellow moss and rotten horse-dung." (*Gard. Mag.* vi. 440.)

"It is to be remarked, that the tubers of every seedling should be kept separate, as scarcely two will be of a similar habit and quality, whilst many will be comparatively worthless, and but few of particular excellence. If the seed is obtained from a red potato that flowered in the neighbourhood of a white-tubered variety, the seedlings, in all probability, will in part resemble both of their parents; but seldom or never does a seedling resemble exactly the original stock. At all events, only such should be preserved as are recommended by their superior earliness, size, flavour, or fertility."

"The early varieties—if planted on little heaps of earth, with a stake in the middle, and when the plants are about four inches high, being secured to the stakes with shreds and nails, and the earth washed away from the bases of the stems by means of a strong current of water, so that the fibrous roots only enter the soil—will blossom and perfect seed."

Mr. Colin Macpherson, of Viewbank Terrace, Dundee, gives the following particulars, showing the practicability of perfecting potatoes from the seed in one season:—"From seeds of twelve potato 'plums' sown this season, in the first week of March, over 750 plants were produced, which I planted on a small

plot of ground (at Polmuir Nursery, Aberdeen,) measuring 8½ imperial poles; and, by carefully cultivating these plants, the stem attained to the height of from 2½ to 3 feet by the latter end of August, and bore a profusion of blossoms and 'plums.' And on lifting the tubers on Saturday, 29th November, the weight of the crop was found to be 9 cwt. 2 qrs, which is equal to 34½ Forfarshire bells 6 pks. and 71 lbs., or 8 tons 14 cwt. and 17 lbs. per imperial acre. It will thus be seen that seedling potatoes can be cultivated so as to yield the above returns the first year of their cultivation from the plum. I may also mention that the seedlings in question are of various varieties, are good in quality, and average-sized potatoes; and, further, that the ground they were grown on received no manure this season. In short, I have no hesitation in asserting that seedlings can be cultivated so as to yield at least from eight to nine tons of potatoes per acre the first year of their cultivation."

In the winter of 1875, I sent to the 150 people to whom I had sent seed the query sheet following, and give in full the following most valuable and interesting information: the succeeding reports refer to same query numbers.—*Please Say*—

1. At what distance were your seedling potatoes grown from the nearest diseased potatoes grown from the set? 200 yards.

2. Were there any walls or hedges between the seedlings and the sets? A hedge and belt of trees.

3. Had the disease previously developed itself in the field in which the seedlings were grown, and if so, how long since? No.

4. Of what description was the soil? Rich loam ground.

5. Was the locality well or ill ventilated? Well.

6. What manures were used? Leaf mould, with a little horse droppings.

7. Were the seedlings grown in a seed bed, and then transplanted? If so, were not some or even many of the tubers full size? Some in a frame, some in border facing south.

8. At what period or periods was the seed sown? And if at several, did the early or late sowings better, or equally, resist the disease? About 1st April.

9. How many plants per hundred absolutely resisted the disease? Cannot tell.

10. Of the plants infected (if any), about what proportion, by weight, of the tubers was infected? Nearly all.

11. What was the average weight of tubers yielded by a seed Cannot say.

12. Which do you believe gives the largest crop, the seed or set? Cannot say.

13. Supposing all seedling plants which had become infected by the disease to be destroyed year after year, and only those which had absolutely resisted it to be propagated from the tuber, what is your opinion as to the possibility of extinguishing the disease by steady persistence in this line of action? Cannot say.

14. Is it your opinion that such researches should be left to the unaided efforts of individuals? Have not formed any opinion.

December 9th, 1875.

"Rath Cottage, Ballymena, 20*th Dec.*, 1875.

Sir,—I hold no land. I am a cotter. The seed was sown in a close sheltered garden about 1st May, and a long frosty drought kept it from germinating. I had lost hope of it ever growing, so you see my faith is less than the small potato seed, which is less than a grain of mustard seed. When it showed signs of life I watered it when requisite. As it came up very thick I transplanted some of it when small, which was better than the other, some of the haulm was two feet high and proportionably thick and strong. My opinion is, if early sown, there would be a fair crop the first year.

There seems to be 4 or 5 kinds in mine, of different shapes and colours. I gave part of the seed to over twenty farmers, but the majority of them paid very little attention to it. But if it has done no other good, it has taught them their first lesson, and they may become apt scholars.

JOHN SLOAN, *ex*-H.C.,R.I.C.

1. Close by various early kinds that were diseased.

3. Yearly, and sets had disease a month before the leaves of seedlings were spotted.

5. Ill ventilated.

6. Farm yard manure.

7. Some transplanted, others not, none full size.

8 1st May, and too late.
9. With four exceptions they were only spotted on the leaf.
10. None of tubers but four were diseased.

JOHN SLOAN."

"*Newry Post Mark. (no date.)*

JAMES TORBITT, ESQ.,

SIR,—I am sorry that I have not a more favourable account to give of the produce of the seed you were kind enough to send me. Being too late in applying to you for it, the crop did not reach maturity before disease set in and checked the growth. My opinion is that the seed should be sown early in March, so that the plant might appear by the middle of April. I did not succeed in having any tubers larger than beans. My efforts have been principally directed to the rearing of new tubers of old varieties; that is new white rocks, and now the white rock seldom produces apples. In fact all our approved varieties very seldom produce seed, and it is very difficult to renew the sort when worn out by age. A neighbouring farmer has succeeded in rearing a new Skerry Blue identical with the old, and without the thick skin characteristic of that variety. He has experimented on the White Rock, but can only produce a variety of Red Rock therefrom. Might not something be done in the way of fertilizing flowers with pollen of approved varieties. Another important question to be answered, is, how are the spores of the disease propagated? Do they propagate in the ground, or only lie dormant there? What effect would be produced by destroying all the potato haulms in the country for several years before visited by disease? In my opinion all investigations with regard to this subject have been unproductive, because they were conducted by scientific men who knew nothing of the practical aspects of the question, or by uneducated persons who had no idea of the probable causes of the disease, and who could therefore only employ empirical remedies. Now that a gentleman of scientific attainments and practical experience [I regret that I cannot accept of the soft and flattering impeachment, J. T.] like yourself, sets about the work, there is every hope of eventual success. I am, Sir, yours very truly,

JAMES CRAWFORD.

1. In immediate proximity.
4. Rich loam.
5. Ill ventilated.
6. No manure.
9. All affected more or less except about 5 per cent.
10. Not more than one per cent. by weight were affected.
13. This seems the only possible method of extinguishing the disease.
14. Most certainly not. The question is a national one.

I am sorry this gentleman forgot to give his address, his questions are so pertinent: as it is, in the hope of him seeing it, and of others appreciating it, I take the liberty of reprinting a Note on Mr. W. G. Smith's discovery of the Potato-Fungus, by W. Carruthers, F.R.S., Consulting Botanist to the Royal Agricultural Society of England:—

"Since the preceding Report was in print the hiatus in the life-history of the potato-fungus has been filled up by the discoveries of Mr. W. G. Smith, F.L.S., which has been communicated to the Scientific Committee of the Royal Horticultural Society, and for which their author has received that Society's gold medal. The structure and life of the *Peronospora Infestans*, Mont., as found on the foliage, haulm, and tubers of infested potatoes, were previously well known and had been frequently described; but the conditions under which the life of the parasite is continued from the autumn to the following summer have been the subject of frequent, persevering, but hitherto unsuccessful research. At the instigation of the Royal Agricultural Society, Professor De Bary has renewed his investigations in this direction, and has arrived at important though yet unpublished results. So obscure has this part of the life of the fungus been, that some investigators have doubted whether the plant was a true *Peronospora* at all, and whether the desired information would not be discovered in some well known fungus parasitic on a different group of plants from the potato, and whose connection with the fungus of the potato disease had not been suspected.

The importance of Mr. Smith's discovery is all the greater that the subject was surrounded with so much obscurity.

It was in investigating the new aspect which the disease had assumed in some, especially in some American, varieties of potato

that Mr. Smith discovered the rest-spores. With the view of separating the tissues for more exact examination, he placed in water some of the diseased leaves obtained from plants grown at Chiswick. He observed that the mycelium grew with greater rapidity in the water, and after ten days he found it producing a large number of minute spherical bodies of two kinds, the one considerably smaller than the other. He further observed specimens in which the already known fruits of *Peronospora Infestans* were growing from the same mycelium as the newly-discovered bodies.

These bodies exactly correspond with the sexual organs that De Bary had already described in several species of *Peronospora* under the name of oogonia for the larger, and antheridia for the smaller bodies. Mr. Smith perceived that he had discovered the sexual organs in this species of *Peronospora*, and continuing his observations he traced the relation between the two bodies. He observed the small antheridia attaching themselves to the oogonia, and fertilising them, by discharging part of their contents into the larger cells through a small tube which was protruded into the substance of the oogonia. The growth of the fertilised oogonium, now called an oospore, was traced by him until it arrived at maturity, when it is a spherical body covered with warts or coarse reticulations and of a black-brown colour. It is but slightly larger than the cells of the leaf, being about one-thousandth of an inch in diameter.

When the rest-spores are mature, they separate themselves from the mycelium on which they grow, and lie as free bodies in the substance of the potato. And when in course of time the whole of the plant perishes, these small hardy bodies remain, able to endure through the winter, and ready to renew the life of the destructive fungus with the restored vegetable life of another year.

Mr. Smith has found the rest-spores in the haulm and tuber as well as in the leaf.

Having thus discovered the means by which the fungus maintains its life through the winter, we are able to look at the question of the possibility of doing something efficiently to mitigate if not destroy the evil. The malady which so extensively destroyed the silk-worms in the south of Europe some years ago, was unconsciously augmented by the producers throw-

ing the dead worms together and keeping them within the establishment, thus increasing the conditions favourable to the growth of the fungus, and to the unlimited development of the spores. It was not until, by the advice of botanists, they cleared away from their silk-worm houses every dead insect and withered leaf, and cleaned the walls, that they got any mastery over the disease. So we have been unconsciously harbouring the potato disease in permitting the haulm and foliage to decay on the field or in dungheaps, which left the undecaying oospores behind ready to start into life when the proper conditions were present. Every care now should be taken to destroy by burning all diseased haulm; and as diseased tubers also harbour the rest-spores, these should be utilised in some way in which the spores could not be injurious, as by employing them in the manufacture of starch or British arrow-root. A vigorous and universal attempt thus to deal with the fungus might now greatly reduce the future liability to, and extent of the disease, though it can never, I fear, deliver us entirely from it."—(*Journal of Royal Agricultural Society of England*, *Vol. XI.*, *Part II.* Murray, 1875.)

Before going into my own experiences I give a few more reports.—

"The Gardens, Campton Verney.

Seeds sown April, 1875. The season has been most unfavourable for testing these seedlings. I transplanted them 16th June, to one of the best situated borders in the garden, giving them every chance. I found by the middle of August that the tops were slightly affected by the disease. I raised them 24th October. I found the majority of the tubers large, to my surprise. I found the blues very slightly affected by the disease. The largest tuber weighed 6 oz. The white varieties were very much affected, two thirds of them bad. Not very promising in appearance. I intend to sow some of the promising kinds and give them a fair trial next season.

W. WARD."

Report by Alfred L. M'Calmont, Esq., Highfield, Uplands, Southampton. 24th December, 1875.

"Plants were grown within ten yards of diseased potatoes grown from the set in a garden in which the disease appeared last year.

Ground well ventilated and drained. Stable manure used.

Grown in hot-house and transplanted. Some of the tubers were full size. Sown in April, transplanted in June. No disease; but a few tubers were eaten by earth-worms and slugs. I think the tubers will take the disease in the course of two or three years (see Mr. W. G. Smith's report on the potato fungus.) [This report is reprinted in full above, J. T.] Some of the plants had very small foliage, and almost all flowered. A few had seed-apples, but they did not come to maturity. One, a white kidney, (the best of the fine sorts) was very small in the foliage.

ALFRED L. M'CALMONT."

"Moy, 10*th December*, 1875.

Seedlings were grown just beside diseased potatoes. Locality well ventilated. Farm yard manure used. Those planted in March grew larger, and resisted disease better. About eighty or ninety plants per hundred absolutely resisted the disease. Of the plants infected, about one tenth by weight of the tubers was infected. I have seedlings of three years' age which give as large or a larger yield than the set.

SAMUEL CORRIGAN."

"Grange, Portadown, 15*th December*, 1876.

Soil good rich clay with sand. Well ventilated. Stable Manure. Grown in field and thinned out. Sown 17th March and 15th April. 17th March, no disease; 15th April, disease hardly perceptible. Unimportant portion of the diseased tubers was infected. I believe that the seed when properly cultivated gives a larger yield than the varieties now grown from the set. I believe the disease may be extinguished by the means you propose. I think the Government should take it up.

RICHARD ALLEN."

"The seedlings were grown in boxes in my green-house in March, afterwards potted, afterwards planted out. About 9 out of 10 plants absolutely resisted the disease. Of the plants infected most of the tubers were infected. There were about 20 to 30 tubers per root (or seed) about as large as walnuts. I believe the set gives a larger crop than the seed. I think the disease is atmo-

spheric, and do not believe growing potatoes from the seed will prevent it, any more than using sets.

J. W. ROBINSON.

Tetton Hall, Wyberton, Boston, 10*th December*, 1875."

"*December*, 1875.

The seedlings were grown in next drill to diseased potatoes. Disease had always developed itself in the field since I got it five years ago. Soil old, rich and heavy. Ill ventilated. Farm yard manure used. Sown in April, and tubers attained about half size. I don't know how many plants per hundred absolutely resisted the disease, but the greater number were affected. Of these, about one eighth by weight, of the tubers, was affected. I believe the means you propose is the only possible way of extirpating the disease, and I believe they will be successful. It is not my belief that researches such as these should be left to the unaided efforts of individuals.

HENRY MATHERS, *Newforge House, Maralin*."

"Ballynegh, 14*th December*, 1875.

J. Torbitt, Esq.

Sir,—I enclose you the paper you were so good as to send to fill up. I will pay more attention in the future as I am greatly interested. I am conscious I did not give half the attention I might have done to the culture of the potato seed you so kindly sent me; nor did I give it the observation I know now it is entitled to. All the excuse I can make to you is, my experience was young. I was ignorant altogether, not knowing even what the seed of the berry was like; and people I showed it to laughed at the idea of any sized tubers growing from such a small seed. It was only when the crop was coming to maturity we had any belief, and the results at raising time surprised us; and then one is so interested in the different sorts, and the different sorts of haulm likewise. I only sowed three perches of a drill and there were four sorts of haulm and the same of tubers. We had rather better than 21lbs. and more than we deserve, for I had no faith in them; but now I see my error, and that your time, trouble, expense and research, is a noble and good work, and deserves every consideration. Yours respectfully,

ALEXANDER NAPIER."

Report referred to by Mr. Napier, *in foregoing letter.*

In the same field with the seedlings, we had Skerry Blues, which have been more diseased this year than in any of the previous eight years. In my opinion, in about six or eight years they will be as bad as the Rocks are now. The soil was kindly and dry, the field well ventilated, and we used farm-yard manure, sowed the seed in beginning of April; but weather turned in dry and cold, and it did not braird till the rain came, which was about a month after. I sowed the seed like turnip, in drills with deep track in centre; and when commencing to have good strong tops, raked down the shoulders of the drills, thinning afterwards about 9 inches apart [*about half the proper distance, J. T.*], and there were a good number about the size of eggs, but far more numerous than any potatoes I have ever seen. About one plant in ten absolutely resisted the disease, and they kept quite green for weeks longer than any potatoes in the country, and this is interesting; for this year the blight withered all sorts at the same time. Of the plants infected, I dare say about one pound of tubers in fourteen was affected; but on looking through them since, I find a very small per centage more affected. I find the plants that withered soonest are most diseased, in my opinion. I did not know what seed was till I received it from you. There was not one I showed it to, would believe I would have any tubers from it. They were as ignorant as myself; and the very persons who laughed at me were surprised to see the crop I had.

I have no hesitation in saying, and I believe it from my heart, that you will likely be the means, either directly or indirectly, of, at any rate, introducing a potato that will resist the disease for a time, and take the place of the Skerry, as I believe it is commencing to fail; and I believe it will be a great national good. My opinion is, our Government should give every encouragement, pecuniary and other, to researches of the kind. No one for a moment would think you would get one individual in a thousand would do the same as you are doing, unaided, for the public good. Is not the potato everything, in a sense, to Ireland? When it completely failed, did not it cause famine in the land? I believe it to be a noble work of you, and that you are a benefactor to your country.

"The Leuchold, Dalmeny Park, Edinburgh, *December* 13*th*, 1875.

Dear Sir,—I regret to say that I have been most unfortunate in my experiment with the potato seed. I sowed about a quarter of an acre, in the centre of a large white crop field, and not one plant ever came up. I gave some seed to Lord ————'s gardener for him to sow, he was equally unsuccessful. I sowed also a small bed in my own garden, with great care, and only three plants came up. These three grew very well—*immensely to shaws* at least four feet in length; the tubers were, however, very small. One of the plants is of a distinctly different kind from the other two. The two appear to be coarse varieties, and would not suit us here. I am keeping the seeds carefully, and will plant them another season, which will better prove what they are, in respect to kinds. I think the ground being too dry must have been the cause of the seed not coming up last season. I remain, dear Sir, yours truly,

PETER GLENDINNING."

"Brown Farm, Biggleswade Beds, (*no date.*)

Seedlings and diseased potatoes were grown close to each other, in field diseased last season. Soil, gravel. Locality well ventilated. Manure, farm yard. Seed sown in rows in the usual manner [evidently in the field, like turnips, J. T.] in the middle of April. A little disease in the tops, none in the potato. I think your proposed plan of destroying all seedlings which become diseased, and propagating only from the tubers of those which do not become diseased, would make little or no difference in extinguishing the disease. It is my opinion it comes after thunder storms; rain alone will not produce it. I am very sorry to inform you of my loss. I had taken the greatest care of the tubers grown from the seed you sent me; they were large enough to plant next season, but I had them stored in the cellar, and the water came in very sudden and I could not get them out, so I have lost all my produce. Yours faithfully,

JOHN KING."

[I wrote Mr. King I should send him more seed, and I shall supply his loss with tubers grown from the seed last year, which were disease-proof here. I want to know if they will continue disease-proof in Bedfordshire, J. T.]

"Ballyclare, (*no date*).

Sir,—My garden contains 3 roods. It is a deep rich soil, which is heavily manured every year, with farm-yard manure. Every spot of it lies to the sun. I have laboured it for eleven years, and every year my potatoes were badly diseased. The seed you sent me, I sowed in the middle of March, and planted them in drills, with farm-yard manure, in the beginning of May. I never had stronger, healthier, or more beautiful plants. They were the admiration of every one; they could scarcely believe they were from the seed of the first year, and I thought they were going to resist the disease altogether, but alas it made its appearance on the leaves; but not very rapidly, and the haulm did not decay away like that of the old potato. The greater part of them blossomed and produced some seed, but not much. I raised them at the end of November. I had from 16 to 32 tubers from each seed. Not more than one-eighth were diseased, and those very slightly. The disease was almost confined to one variety, which, when cut, has black streaks. Yours respectfully,

MARY M'FADDEN."

"Castlewellan, *December* 18*th*, 1875.

Sir—I sowed your seed in a garden sheltered by trees, where potatoes had been grown for the last 20 years, and with the half of the crop invariably bad. I transplanted the largest of the plants, and the produce amounted to about $\frac{3}{4}$ cwt. to the square perch; and what was singular, I hadn't a bad potato in the whole lot. I gave some of the plants to my neighbours, and they all report most favourably of them. Some of the plants blossomed, but none produced apples. Yours respectfully,

JAS. O'FLINN."

"Well House, Saltney, *December* 20*th*, 1875.

Dear Sir.—The potatoes from your seed are blue-red in colour; nearly all looking like an early variety, very small, but no disease. Seeds from Patterson's Victorias cropped rather better than yours, and are nearly all apparently the same kind as Patterson's. Yours very truly,

JOHN ROBERTS."

"Mount Hamilton, Clough Mills, 20*th December*, 1875.

Dear Sir,—In the middle of April I sowed your seed in drills

like turnips, only very thin, and thinned out the plants to a foot or so apart. In the whole there were not twelve diseased plants, although they grew in a garden, closely adjoining Black-hearts, which are attacked by the disease earlier than perhaps any other variety. I am, very truly yours,

THOMAS GREGG."

"DUNFANE, 1*st January*, 1876.

SIR,—Miss Walker of Crebilly received a package of your potato seed from me on the 5th April. Some days after, she sowed it on a kind of hot-bed, and it came forward soon. She had it then transplanted to drills in a kind of boggy ground, proverbial for disease. The leaves of her seedlings were diseased, but the potatoes which were of various kinds, all resisted it; as did the haulms, and they were near diseased kinds without ditch or wall between them; and she states she had an average crop; tubers weighing 2, 3, 4, 5 ounces.

JOHN SLOAN."

[This is the ex-Head Constable, R.I.C., writer of second report].

"THE GRANGE, GOOLE, YORKSHIRE.

DEAR SIR,—I have saved some of the best of those that entirely resisted the disease, and I trust the effort you are making, will be successful. If you cannot, by your plan, entirely eradicate the disease, you will, I have no doubt, raise some new and valuable varieties. My plants were sown in April, about two yards from diseased potatoes, on sandy loam, pretty well ventilated, and yard manure used. They were drilled on ridge, and thinned out. About 40 plants out of 100 absolutely resisted the disease. Of the infected plants, at some roots nearly all, at others half, and at a great many only a few—say one or two tubers at a root, were affected. I think the disease might be extinguished by your method, and I think it ought to be tried. I think the Government, or some of our great societies, ought to take up the matter. Wishing you every success. I am, yours respectfully,

W. SMITH."

"*December* 25*th*, 1875.

SIR,—The plants grew very well until long after the potatoes in the neighbourhood were diseased, and then the foliage of the

seedlings became diseased also. They blossomed, but bore no 'plums.' They were sown on 11th and 13th March, close beside diseased potatoes, in a field in which the disease has always shown itself since it commenced; soil, loam; part well, part ill ventilated; manure, farm-yard and a little guano. A few tubers were nearly full size; about 70 plants per 100 absolutely resisted the disease. I believe that the disease might be extinguished. I think these researches should not be left to individuals. Your obedient servant,

W. LAMONT."

Report from J. Bailey, Esq., *Shirley, Southampton, per* Mr. Blundell.

"Seedlings were grown within about 30 feet of old diseased potatoes, and with nothing between them. Disease had developed itself in the garden each year; soil gravelly, well ventilated; seed sown 7th April. A few tubers attained full size, and about 80 per 100 plants resisted the disease absolutely."

"*Report.*

Soil light sandy, pretty open though well sheltered; liberally manured in Autumn, with farm-yard manure; disease in the garden the year before; grown on a seed-bed about middle of April; transplanted, and only a few of the tubers were of a good size. Very few of them were at all touched by the disease, and those few were all white varieties. The plants being too close, they did not give this year so large a crop as the set. I think it is a likely way of extinguishing the disease. W. G."

"Crossburn, Troon, 10*th January*, 1876.

Dear Sir,—I sowed the seed on a hot bed about the beginning of March, and planted out in a garden, on good horse manure, on the 4th of June, about 1,200 or 1,500 seedlings. I think the ground and the manure had been too strong, as the potatoes, which were not ready to lift till November, were so badly worm-eaten that it was impossible to say much about them; there seemed a great number of varieties amongst them, and a red kind in particular appeared more susceptible to disease than any of the others. Whereas a white kind which bulked well, though small, did not disease at all. I am, yours faithfully,

W. GUTHRIE."

Report, AUTHOR *not known*.

"Sowed seed in a drill between cruffles and flounders on the 18th March, 1875. Had potatoes of different kinds, of which about one cwt. of twenty was diseased."

Report from S. THOMPSON, ESQ., *Muckamore Abbey, Antrim*.

"Sowed, part in seed-bed, part out, some tubers full size, grown in rich garden soil, in which there was disease this year, locality well ventilated, manure common. Fully 90 per cent. of the plants resisted the disease absolutely."

"KILLINCHY, CO. DOWN, 12*th March*, 1876.

DEAR SIR,—I think it right to let you know how the potato seed you kindly sent me last year has turned out.

I sowed some in hot-beds and hardened them off, then transplanted them, in drills two feet apart, some I sowed in the open air, thinned them out to proper distances, and planted the thinnings; from all I had a tolerable fair crop, *not one diseased*; but the seed from the hot-beds were the largest; I suppose because they had an earlier and better season to grow.

I have twelve distinct sorts. I have long ones as black as jet, something like what was called 'Black Lords,' long ago. I have evidently Kidneys, and round ones of various colours. I shall plant them all whole this season again; and if you have any hint to throw out for my guidance, it shall be carried out by yours very truly,

JAMES M'CANN.

P. S.—You might send me a very small sample of seed. J. M'C.'

It is now 28th of March, the printer wants copy, and the seed should be in the ground at once; I must therefore record my own experience rapidly and without revision.

I sowed seed in a hot house 1st January, 1875; on a seed bed in the open, 24th March; in a conservatory without artificial heat on 4th April; again, some time in May, in the open air. The sowings of 1st January and 4th April were planted out about middle of May. The 4th April gave the better result, both as regards yield and resistance to disease. The sowing of 24th March, owing to cold winds (I suppose) did not give so good yield as the sowing of

middle of May; they were both planted out after those of 1st January and 4th April. All had large and vigorous stems and foliage. Some 5 per cent of the plants produced short thick black stems, and no tubers at all; some others a small number of worthless small tubers; others a small number of full sized tubers; others an immense number of small tubers; others again, a very large number of medium sized beautiful tubers of all shapes, sizes and colours; for instance, some of them salmon coloured, some of them shaped like the one half of a ring of three or four inches in diameter; the thickness of the ring being half an inch to an inch. One of the plants yielded 145 tubers, and from their weight, and the small space the foliage occupied, I believe, with Mr. Knight, that crops of 30, 40 or more tons per acre may be obtained. That is to say, I believe, as I stated before, that the productive powers of the temperate zones may be doubled. When the plants were a few inches high, I observed that one in a thousand or so, possessed a slender, dark red, almost black stem Leaves of a much darker green, of smaller size, more deeply corrugated, with a larger number of hairs, of a stronger growth. These I fixed upon at once, as something worth looking into. I separated them, and they all produced dwarf dark foliage, with the most beautiful white, shapely, smooth skinned tubers I have ever seen. The space of ground occupied by the foliage was so small, and the produce so large, that I will not venture to put a name upon what I believe they may be found to yield. But I want every one who grows the seed to watch for them, separate them, and see the result. Two plants of vigorous looking growth I allowed to ripen in the conservatory. Sown on the 4th April, one of them ripened about September, and being supported, it reached the height of 15 feet, with 15 or 20 branches. The other, springing from a single stem, divided into about 25 branches about 16 feet high. Off these plants I took "cuttings," small shoots thrown up at the axils of the leaves—incipient branches—these all produced a few almost full sized tubers. Off these cuttings I took other cuttings, in the hope that the plant could be propagated in this manner for the same length of time that it could be propagated by the set. I find it cannot; these second cuttings failed to grow. The yield of these two plants was pretty good, about 8 lbs. each; but nothing in proportion to the foliage.

The largest grew on till December, when I killed it by throwing water on its leaves. None of the plants in the field gave more than 2½ lbs. of tubers; but the field was so unsuitable to the plant, that skerries grown in it last year were uneatable. The diseased plants I allowed to lie on this field, and I think it is the field in the world the best saturated with the spores of the *Peronospora*. In it I am preparing to grow a fresh crop of seedlings; and in it I have no doubt, I will find hundreds of individuals (varieties, as we are pleased to call them) upon which the *Peronospora* cannot bite. It has been done before, why not again? And now I shall proceed to give what information I can as to the best means of growing the plant. The seed should be sown at once, and if it be desired, to make the most of it, one seed to the square inch. The bed, any sort of good light earth, sifted or throughly pulverized; seed to be covered with same fine earth, ⅛ or ¼ inch deep, well watered with rain water, and kept properly moist. Temperature from 45 at night to 50, 60, or 70 Fah. by day. I used boxes filled to within one inch of the top, and then filled up to the top with rain water, the surplus water filtering through. When the plants are two inches or so high, let them be transplanted without breaking the roots, into spaces of at least 4 inches square. When the frosts are over, and the temperature genial, they may at any time be planted out, and in my opinion the following method is the best.

Let lazy beds be made 5 feet wide, with spaces for trenches between them 20 inches wide. Let the beds be well trenched with the spade, 18 inches deep. Within 10 inches of the margain of each side of the bed, place a line of seedling plants distant from each other 15 inches. In the middle of the bed place another line 15 inches apart of seedlings and sets alternately. Let the sets be of such "varieties," as are most certain to catch the disease.

But let the plant have a *chance* for its life—a fair field, but no favour—let the ground be free from the shade of tree, walls, or anything else. Let the plant have light and air, and let the *Peronospora* do its worst. Then every plant which is not immaculate when ripe, which does not resist the shock absolutely, destroy it. No matter how beautiful; no matter how protific, destroy it. It is not to be depended on for a moment. It is not a "staff of life," and why cultivate it; when any number of millions as

prolific and as palatable as it, can be obtained with the utmost facility? And of those which resist the first year, and fail in any succeeding years—destroy, destroy, destroy. It is the principle of "natural selection" artificially carried out, and it is done every day in animal life, and with results beyond, far beyond, what could possibly have been anticipated beforehand. If it were applied to the human race, how the mattter of the better cultivation of a plant would fade into nothingness, by comparison. As to manures, let the sets in the middle row have a full dose of farm-yard manure, if possible containing seeds of the parasite. For the seedlings I cannot say; if the soil is good, I think, perhaps, they are as well without it. Before planting out the seedlings, they should be "hardened off." At all times except in frost, the conservatory should have a full supply of air. I do not think, with Mr. Knight, that artificial heat is requisite, at least in the latitude of Belfast, when the seed is sown after first of April. Where a conservatory is not available, a hot-bed, or cold frame, or warm border will answer, only the plants must be protected from frost. My plants, all or almost, all blossomed, not one in 500 bore fruit; neither in the conservatory, hot-bed, cold frame or field. I do not understand this. I know all the old "varieties" are seedless, and I thought the young would have produced their seed freely the first year. There seems to be some analogy or relationship here to the perennial plants; some of which do not blossom until after many years' growth. Red purplish flower occurred very rarely—the skerry blue has it, and it is one of the most nearly disease proof varieties now in cultivation; but its yield has fallen off to almost nothing. Cream coloured, pale, blue, pink and purple flowers are common. The second, third,—several pairs of the lower leaves, attain a length of 14 to perhaps 18 inches from axilla to tip of blade. Blade of terminal leaf 4 or 5 inches broad, 6 or 7 inches long. The upper and later produced leaves are of the usual size.

In regard to this matter, every one will ask, what does Mr. Darwin say? He says, and it will be generally admitted that there is no higher authority in the world—he says:—(*The Variation of Animals and Plants under Domestication* Vol. II. page 362. Murray, 1868.

"We may conclude that the difference between sexual and asexual generation is not nearly so great as it at first appears; and we have already seen that there is the closest agreement between gemmation, fissiparous generation, the repair of injuries, and ordinary growth of development. The capacity of fertilisation by the male element seems to be the chief distinction between an ovule and a bud; and this capacity is not invariably brought into action, as in the cases of parthenogenetic reproduction. We are here naturally led to inquire what the final cause can be of the necessity in ordinary generation for the concourse of the two sexual elements.

"Seeds and ova are often highly serviceable as the means of disseminating plants and animals, and of preserving them during one or more seasons in a dormant state; but unimpregnated seeds or ova, and detached buds, would be equally serviceable for both purposes. We can, however, indicate two important advantages gained by the concourse of the two sexes, or rather of two individuals belonging to opposite sexes; for, as I have shown in a former chapter, the structure of every organism appears to be especially adapted for the concurrence, at least occasionally, of two individuals. In nearly the same manner as it is admitted by naturalists that hybridism, from inducing sterility, is of service in keeping the forms of life distinct and fitted for their proper places; so, when species are rendered highly variable by changed conditions of life, the free intercrossing of the varying individuals will tend to keep each form fitted for its proper place in nature; and crossing can be effected only by sexual generation, but whether the end thus gained is of sufficient importance to account for the first origin of sexual intercourse is very doubtful. Secondly, I have shown, from the consideration of a large body of facts, that, as a slight change in the conditions of life is beneficial to each creature, so, in an analogous manner, is the change effected in the germ by sexual union with a distinct individual; and I have been led, from observing the many widely-extended provisions throughout nature for this purpose, and from the greater vigour of the crossed organisms of all kinds, as proved by direct experiments, as well as from the evil effects of close interbreeding when long continued, to believe that the advantage thus gained is very great. Besides these two important ends, there may, of course, be others, as yet unknown to us, gained by the concourse of the two sexes."

He says, in effect, as I understand it, that there *is* some difference between sexual and asexual generation; and I for one have the "courage of my opinions"; and my opinion is, that that difference consists in this, that the sexual union affords a fresh starting point of life; and that no other process or system of generation does.

Besides advertising locally, on the 9th February, 1876, I sent £10 to the "Times" with the following advertisement:—"Extinction of Potato Disease, with Doubled or Trebled Crops.—Modus Operandi.—Growth from the Seed. Exposure of plants to full force of infection. Destruction of those which succumb. Propagation of the rest by the Set. (In all places some plants will repel the attack of the parasite; in some, all.) Seed supplied gratis. Address Robertson, Brooman, & Co., 166 Fleet Street, London; or James Torbitt, Belfast, Ireland."

It elicited applications from a few ladies and clergymen only;—hardly a dozen from men of the world. Under these circumstances I made up my mind at once; I divided the seed into packages, each containing nine thousand seeds—one dram. (150 seeds of the potato weigh one grain Troy.) Each packet therefore possesses the "potentiality" of producing nine thousand new varieties of the plant. Of these, I think it will be found impossible to infect—to subject to the action of the *Peronospora*—more than eight thousand. Each packet therefore, is probably capable of producing one thousand new disease-proof varieties; out of which, surely there is room for selection and propagation from the tuber.

And with great respect, I now send one packet of this seed, accompanied with this paper, to each Member of the House of Lords, and each Member of the House of Commons; besides about a thousand others, applicants for the seed; and if the intrusion be considered unwarrantable, I beg leave to apologise.

But I do not think it will be looked on in that light, and what I would earnestly request, would be, that half-an-hour be devoted to the study of this paper, and that whatever conclusion may be arrived at, it shall be an independent one, perfectly unbiassed by the theories of Botanists, and if that opinion be favourable, I would suggest, that the seed be sown and orders issued that the instructions be carefully carried out, and the result noted.

I do not know if I might venture to suggest, that government

should be pressed to take action in this matter; but I am much inclined to believe, that if seed were bought in the Autumn, and distributed all over the country, with instructions how to grow it —in Ireland the Constabulary could do it admirably;—and if substantial prizes were offered for disease-proof potatoes, I am much disposed to believe that the disease might be eradicated in, perhaps, three years.

In the event of something like this not being done: I propose again to buy berries by the ton, and issue seeds by the hundred millions, accompanied by this paper (at present, I send out fourteen millions, each a new "variety" of the plant,) and I shall next select the Magistrates and the Clergymen of the kingdom, as the parties before whom to lay the matter, and I propose to continue the process till the disease be extinguished. It will cost money no doubt. Well, I am prepared to spend a thousand a year on the work, and I believe it will be judiciously expended. For myself, I intend to go on growing from the seed, and destroying all which fail; and also to commence the "most noble experiments" of cross breeding; not from worn-out varieties, but from healthy young plants, grown by myself, from the seed; and I see no reason, if Mr. Knight could raise 34 tons on an acre, why I should not.

Any proposition, suggestion, or enquiry from any quarter, shall meet with respectful attention, and any reports which may be vouchsafed, as to results of experiments, shall be much esteemed.

NOTE.—Accompanying seed was all obtained from one variety. Origin and age unknown, yield large, tubers cylindrical and large, colour dark reddish, quality coarse, suitable for cattle, almost 1[illegible]ease, yields berries profusely.

J. TORBITT.

58, NORTH STREET,
BELFAST, 31*st March*, 1876.

ALEX. MAYNE PRINTER, CORPORATION STREET BELFAST.