ESSAYS ON PHYSIOLOGICAL SUBJECTS

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BY

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PREFACE.

The following Essays, with the exception of the last, were contributed anonymously to various Periodicals, during the years 1862—1864.

It has long been my intention to republish several papers so contributed, in a separate form, because they deal with subjects which, though by no means unimportant in themselves, at least in a theoretical point of view, have not received much attention from English physiologists.

I am now induced by special circumstances so far to anticipate my intention as to reprint, somewhat hastily, the few which follow, and which are concerned with questions affecting vegetable no less than animal physiology.

OXFORD, *Jan.*, 1868.

NOTICE.

This Volume was printed for private circulation early in the present year. The publication has been delayed in part by accidental circumstances, and partly also with the intention of adding to it some further Essays on kindred subjects. Since, however, the questions with which it deals have formed the subject of much recent controversy, it is thought advisable to allow it to appear at once.

Oct. 1868. G. W. C.

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ESSAY I.

Review of Mr. Darwin's Work on the Fertilization of Orchids [a].

We owe the appearance of this very interesting work, as the author informs us on the first page, to the demand which has been made for facts in proof of the proposition laid down in the 'Origin of Species,' that 'in no organic beings can self-fertilization go on for perpetuity.' (Ch. iv. p. 101.) It requires, therefore, to be examined from two points of view; firstly, as an account in a popular form of the reproductive system of a remarkable class of plants; secondly, in regard to the support which it affords to the above proposition, itself only a collateral branch of the author's great argument for the development of species. We have spoken of this work as giving a popular account of the fertilization of orchids, but we desire especially to avoid the

[a] 'On the various Contrivances by which Orchids are fertilized by Insects, and on the good effects of Intercrossing.' By Charles Darwin, M.A., F.R.S., &c. John Murray.

sense so often attached to the word 'popular' of something inaccurate or superficial. Mr. Darwin's book is popular only in so far as it makes no demand upon the reader for special or technical knowledge of botany, but only for an intelligent interest in the subject, and a reasonable amount of attention and thought.

It has been long known and still longer suspected by botanists, that the agency of insects is in some manner necessary to the fertilization of the greater number of orchids. Mr. Darwin's special design is to show, by a detailed description of what takes place in many of them, that the object of the whole arrangement is to secure the fertilization of each individual flower by pollen taken from another.

The main points in which orchids differ from most other flowers are, as many of our readers no doubt are aware, shortly as follows:—The reproductive organs are united into one central mass, called the 'column,' instead of being disposed in two circles. The anthers are in most cases reduced to one, and the pollen therein contained is united into masses by slender elastic threads, which are in many genera woven together and prolonged into a little stalk called the 'caudicle,' the three pistils are likewise joined together, and the stigma of one of them is modified into an organ peculiar to orchids called the 'rostellum.' This organ either consists of, or contains, viscid matter, and to a portion of it, called by Mr. Darwin the 'viscid disk,' which in some cases is prolonged into a 'pedicel,' the caudicles above mentioned are firmly attached. After this description, itself abridged from Mr. Darwin's introduction, we may hope to render what follows intelligible.

Commencing with the tribe Ophreæ, the author first describes the structure and action of the reproductive organs in the common orchis mascula, and it is worth mentioning that the description applies equally to the orchis maculata now[b] to be found in bloom in our woods. Like all the ophreæ its pollinia are furnished with caudicles congenitally attached to the viscid disk. The anther stands above the rostellum. The latter organ is nearly spherical in shape and overhangs the stigma. It consists of a membraneous bag, the upper and back part of which forms the disk, to which the caudicles are attached, and which contains a thick viscid matter. This membrane possesses the peculiar property of giving way upon the slightest touch, after the flower has expanded, in such a manner that while it retains its shape it sets the disk free. It resembles, in fact, a box of which the disk forms the lid; to the top of the lid is affixed the caudicle, and, to the internal surface, the viscid matter. The position of the parts is such that an

[b] July 1862.

insect, as it tries to reach the nectary of the flower, can scarcely avoid brushing against the rostellum, and, should it do so, the lower part, the body of the box, which opens with a spring, is pushed down, the viscid contents adhere to the insect's head, and, as the latter is withdrawn, the viscid matter, the disk itself, and the attached pollinia, will be withdrawn with it, and this done the box shuts again, *i. e.* the rostellum resumes its original form, and should the insect have removed but one pollinium and one half of the viscid matter, the remainder is again protected from the action of the air. That this wonderful set of contrivances really exists is proved by the fact that the pollen of one species of orchis, the pyramidalis, has been found adhering to the heads or probosces of no less than twenty-three species of Lepidopterous insects.

Moreover, our readers may easily satisfy themselves of its truth, as we have done, by trying the experiment for themselves, a needle serving as a rough substitute for the insect's proboscis. In order, however, to the accomplishment of the object, which this elaborate arrangement is to serve, three other conditions are required. Firstly, the anther must be previously open in order to permit the pollinia to be extracted, and this is always the case. Secondly, the viscid disk must adhere very firmly to the head of the insect, and this we find is brought about by a curious property of the viscid matter, that, namely, of becoming firm upon exposure to the air even for a very few seconds. The third condition required is, that the pollinia should be in such a position upon the insect's head as to enable it to strike the stigma of the next flower which the animal visits, and a special contrivance exists to bring this about. If our readers will look at Mr. Darwin's illustrations, or, still more easily, if they will examine with a needle a flower of orchis maculata, they will see that the pollinia when first removed stand almost at right angles to the needle, and that in this position they cannot possibly get through the narrow passage leading to the stigma of another flower; accordingly, it is found that the disk has a peculiar power of contractility, and that this affects only one side of it; the consequence is, that the pollinium, as it stands upon the needle, may be seen after the lapse of a few seconds to bend down until it gets into a horizontal position, with its top towards the point of the latter. In this position it is as impossible that it should miss the stigma of the next flower visited as that in the former one it should reach it.

We have given this sketch of the arrangements which bring about the fertilization of one genus of orchids at length, but our readers should remember that it is still only a sketch, and we must refer them to the book itself for other details not less interesting than those which we have given. Meanwhile, a shorter account must suffice for other genera. Passing on to the genus ophrys, we find arrangements very similar to those given above, but not altogether the same. Thus in ophrys muscifera, the curious motion of the pollinia after removal is substituted by a double bend in the caudicle itself. The curious ophrys apifera, again, has a different arrangement adapted for a different object, that, namely, of self-fertilization; thus the caudicles are flexible, and bent forward over the stigma, and the pollen is very light, and capable of being shaken out by the wind directly upon it. It is worthy of notice that ophrys apifera is found to be exceedingly fertile, and ophrys muscifera and aranifera to be peculiarly sterile; the two latter can be fertilized only by insects, and produce no nectar by which to attract them. These facts might lead to an opinion the very contrary of that at which Mr. Darwin arrives, were it not that the same contrivances exist in full development in ophrys apifera as in others, and it is difficult to believe that they exist for nothing. The only hypothesis, therefore, which remains is that they are intended to afford that *occasional* cross for which only Mr. Darwin at present contends. Contrivances similar to those already described are found in other species of the tribe, as in gymnodenia habenaria, &c., but varied according to the circumstances of each case. Thus, in the latter, an arrangement exists whereby the pollinia when fastened to the insect's head or proboscis, perform a double movement of convergence as well as depression in order that they may reach the stigma of the flower to which they may be next carried.

Passing on at once to the last group, with which the author concludes the account of British orchids, we may say that it consists of three genera, malaxis, listera, and neottia. They are remarkable in that in them no portion of the membraneous surface of the rostellum is attached to the pollinia. We shall notice the second genus as at once the best known and the most remarkable. In this genus, listera, the anther opens before the flower, and the pollinia rest upon the back of the rostellum, their lower ends remaining in the anther cells, and being supported by them. The rostellum has the peculiar power of exuding, when touched, however lightly, a drop of viscid fluid, and this fluid invariably catches the tips of the pollinia, and drying with extreme rapidity, glues them firmly to the insect or other object which touches the rostellum.

This experiment, again, we have ourselves been able to verify. The rostellum at this time is always somewhat bent over the stigma, but as soon as the pollinia have been removed it rises up, and thus leaves the latter open for the approach of any insect carrying pollen from another flower.

We must now bring to a close this very imperfect survey of Mr. Darwin's account of the British orchids, but we cannot do so without showing how the author intends all that has gone before to fit into his great plan for establishing the doctrine of natural selection. He says (p. 346) :—

'I have now nearly finished this too lengthy volume. It has, I think, been shown that orchids exhibit an almost endless diversity of beautiful adaptations. When this or that part has been spoken of as contrived for some special purpose, it must not be supposed that it was originally always formed for this sole purpose. The regular course of events seems to be that a part which originally served for one purpose, by slow changes becomes adapted for widely different purposes. To give an instance, in all the ophreæ the long and nearly rigid caudicle manifestly serves for the application of the pollen-grains to the stigma, when the pollinium attached to an insect is transported from flower to flower, and the anther opens widely that the pollinium may be easily withdrawn; but in the bee ophrys, the caudicle, by a slight increase in length and decrease in thickness, and by the anther opening a little more widely, becomes specially adapted for the very different purpose of self-fertilization, through the combined aid of the gravity of the pollen-moss and the vibration of the flower. Every gradation between these two states would be possible, of which we have seen partial proof in O. arachnites.'

Again, p. 348 :—

'In my examination of orchids hardly any fact has so much struck me as the endless diversity of structure, the prodigality of resources for gaining the same end, namely, the fertilization of one flower by the pollen of another. The fact, to a certain extent, is intelligible on the principle of natural selection. As all the parts of a flower are co-ordinated, if slight variations in any one part are preserved, from being beneficial to the plant, then the other parts will generally have to be modified in some corresponding manner. But certain parts may not vary at all, or may not vary in the simplest corresponding manner, and those variations, whatever their nature may be, which will bring all the parts into more perfect harmony with each other, will be seized on and preserved by natural selection.'

We proceed in the next place to examine the conclusion which Mr. Darwin draws from the facts which he has so carefully observed, and which he has expressed in the aphorism, 'Nature abhors perpetual self-fertilization.'

This conclusion Mr. Darwin proceeds to establish somewhat in the following manner:—Taking a particular tribe of plants, viz., the orchids, he finds that in by far the greater number of species the reproductive organs are so arranged as to preclude the possibility of the pollen from any individual flower ever reaching the stigma of the same flower; and, on the other hand, that peculiar and very beautiful contrivances exist for enabling the stigma of each flower to receive pollen from others through the agency of insects; and further, that insects are actually found with the pollinia of orchids attached to their heads in the precise manner necessary for effecting this object, and that when the visits of insects are artificially prevented, no fertilization whatever takes place. From this he argues that as self-fertilization would have been 'an incomparably safer process,' all these elaborate arrangements would not thus have been made, for the very purpose of avoiding it, had there not been 'something injurious in the process.' And having thus reached this conclusion in the case of orchids, he proceeds to infer the probability that a similar law may be found to exist throughout the organic world. Now, we desire to do full justice to the skill, patience, and acuteness manifested by Mr. Darwin in the researches upon which this conclusion is founded, but to our minds there appear to be insuperable obstacles to its admission. It seems to us that some of the facts as at present known are of far too contradictory a character to warrant it, and that others will bear an interpretation not altogether favourable to it. Thus, suppose an insect to visit a spike of orchid flowers, upon which several were about equally expanded, he would go from one flower to the next upon the same spike, conveying the pollen of one to the stigma of another; and it admits, we think, of very grave doubt how far two such flowers can be fairly considered as separate individuals at all. Again, we can hardly suppose that an insect will confine itself to one species of flower at a time; and indeed, Mr. Darwin himself gives us good reason for believing that it does not so (see p. 48); and it is clear that if an insect having visited an orchid flower and removed its pollinia in the manner described by Mr. Darwin, should then fly away to various other flowers, it would get the pollen grains rubbed off before its next visit to an orchid of the same species. We are, moreover, assured upon high authority, that it is but very rarely that any insect is found with pollinia attached to it. These and similar difficulties, although they may have their due weight in directing future observations, are not in our opinion of sufficient importance to counterbalance the evidence collected by Mr. Darwin, so far as regards the fact that the majority of orchids are fertilized by the agency of insects, and even that the special contrivances observed are so many provisions against self-fertilization in this case; but we cannot see any solid grounds upon which to extend this generalization, as Mr. Darwin has done at the close of his work, to the whole organic world. As long as any example can be found in nature in which self-fertilization can be shown to be the rule, we can have no right to pronounce the process essentially injurious, however many ingenious appliances for avoiding it we may discover in particular classes of beings. We can illustrate this point without going further than the tribe of orchids. The bee ophrys, as Mr. Darwin has shown with great clearness and candour, is possessed of special contrivances for self-fertilization, and such is its common method of reproduction, though provision is also made for occasional crosses by means of insects. Now, in this case, it seems clear to our apprehension that the bulk of the specimens of bee ophrys at any time existing are descended from many generations of self-fertilized flowers. The same reasoning may be applied to the barberry. In this plant, as many of our readers are aware, there exists a peculiar irritability of the stamens, which causes them, when touched, to bend forward in such a manner as to bring the anthers into immediate contact with the stigma. Here again we have a special contrivance for self-fertilization, and it is one which has not escaped Mr. Darwin's notice. In the 'Origin of

Species,' p. 98, he endeavours to meet the difficulty it presents by suggesting that as the agency of insects is required to bring this power of movement into action by touching the stamens, the insect so touching the flower would carry away some of the pollen attached to its body to other flowers; and by stating that, as a fact, where varieties of the barberry grow near each other they constantly intercross. We think all this may be admitted, but it will, nevertheless, remain the fact that the habitual mode of reproduction of the barberry is by self-fertilization, and that the majority of the existing plants must have been produced by this means. If we compare the chances in a single case we can hardly avoid seeing that it must be so. Suppose a bee to alight on a flower of barberry; he sets his foot upon a stamen, and it springs forward immediately, thus bringing its anther close down upon the stigma. This flower is then pretty certain to be self-fertilized. Meanwhile, a few grains of the pollen adhere to the insect's body, and he flies off with them. Is it equally certain that he will go to another barberry-flower and fertilize that? May he not drop the pollen on his way, or waste it on the stigma of a different kind of plant? In any case, it appears to us clear that, where there are two methods of reproduction in the same organism, of which one is the general rule and the other the rare exception, we are bound to suppose that the mass of existing specimens draw their descent from individuals produced by the ordinary means.

There exist in nature three forms of the reproductive function, viz., hermaphrodite and self-fertilizing, the hermaphrodite and mutually-fertilizing, and that in which the sexes are distinct. These three plans may exist in different proportions and may be variously modified in different classes of organisms, but they all exist in the vegetable kingdom, and they all exist in the animal kingdom.

What has now been shown in regard to the tribe of orchidaceous plants is that though the second method is that which they mainly follow, all three are to be found amongst them; in fact, that in respect of this function, they form as it were a microcosm corresponding to the macrocosm of the whole organic world; but we must confess that we are wholly unable to see that this affords the slightest ground upon which to establish the dictum that 'Nature abhors self-fertilization.'

ESSAY II.

MARRIAGES OF CONSANGUINITY [a].

IF we had to point out the tendency or habit of mind which, more than any other, has served, in modern times, to hinder the progress of real knowledge, we should fix upon that which impels not a few really able and competent persons, when undertaking an investigation, first of all to adopt a theory, and then to look at the facts which nature presents to them by its light exclusively. Such persons do not take up a hypothesis for its legitimate use, as a guide in experimentation, as any one pursuing an investigation in the science

[a] 1. 'On Marriages of Consanguinity.' Dr. Bemiss. 'Journal of Psychological Medicine' for April, 1857.

2. 'Hygiène de Famille.' Dr. Devay. Second Edition.

3. 'Comptes Rendus,' 1852–3 passim. Papers by MM. Boudin, Sanson, Beaudouin, Gourdon, &c.

4. 'On Marriages of Consanguinity.' By the present writer, in 'Medico-Chir. Review,' April, 1862; and 'Medical Times,' April 25th, 1863.

5. 'On the Fertilization of Orchids.' Mr. Darwin. London, 1862.

of light would in these days start upon the undulatory theory, but adopt it with a confidence in its absolute truth which renders them utterly blind to all facts which cannot be reconciled with it, and by consequence exaggerates out of all due proportion the importance of those which really make in its favour. Of the many inconveniences attendant upon the state of mind of which we speak, one of the gravest and quite the most paradoxical is to be found in the fact that its mischievous results always bear a direct ratio to the ability and industry of the person whom it affects. A man of real power who sets out upon a research into a complicated subject under such conditions as we have indicated, is sure to make out a good case in favour of his own preconceived view, and by so doing he will mislead others and hinder the advance of knowledge in a degree exactly proportioned to his own ability and reputation. Instances of the kind to which we refer will occur to any reader familiar with the history of almost any scientific question. But there is one feature in such cases which is especially worthy of remark; it is, that a man's preconceived notions upon any subject may take their rise from something quite distinct from, and external to, the subject itself; a religious opinion, a moral theory, a social predilection, a fact in his own family or personal history—any or all of these may, consciously or unconsciously, so modify his view of what ought to be a mere question of fact, as to render him a totally unsafe guide in any subject-matter which he has undertaken to examine and explain. The history of the scientific question forming the subject of this article will be found to illustrate these remarks even better than most others.

That there has existed, at least in all modern times, what is called a 'feeling' against the intermarriage of blood relations, is a fact that cannot be denied, but of which the scientific value cannot be rated very high. Before we admit the existence of such a feeling as even *primâ facie* evidence, we should remember how often such have been found to rest either upon no ground at all, or upon an entirely mistaken one. The biting cold of the winter months in England used to be called proverbially 'fine seasonable healthy weather,' until the Registrar-General's statistics had proved to the apprehension almost of the dullest, that mortality in our climate varies inversely as the temperature. In this case, doubtless the popular delusion took its rise from the sense of exhilaration and buoyancy felt by healthy, strong, and youthful persons on a bright frosty day, as compared with the dulness and languor experienced on a damp and warm one; but it entirely left out of the account the less obvious but more really potent influence of cold upon the old, the feeble, and the ill-provided. In the case before us, the following is, as I have elsewhere suggested, the probable history of the prevailing opinion [b]:—

'It should be remembered that all such marriages as those under discussion, were and are strictly prohibited in the Church of Rome. This prohibition was first removed in England by the Marriage Act of 1540, in the reign of Henry VIII. It is natural, therefore, that many people at the time should have looked upon this removal of restrictions as a somewhat questionable concession to human weakness, and upon the marriages made in consequence of it as merely not illegal, rather than in themselves unobjectionable; just as, should the Marriage Law Amendment Bill pass into law, there can be no doubt that many would now look upon marriage with a sister-in-law as a very questionable proceeding in a social and religious point of view, although they might possibly be unable to impugn its strict legality. Under such circumstances nothing is more natural, especially in an age when men were much more open to theological than physiological considerations, than that they should attribute any ill effects which

[b] 'Med. Chir. Review,' vol. xxix. p. 469.

might seem to follow from such unions to the special intervention of Providence. Such ill effects would be marked and noticed whenever they occurred, and would soon become proverbial; and when, in a later age, men began to pay more attention to the breeding of animals, and found that excessively close breeding seemed, in some cases, to produce similar results, they would be led to establish a false analogy between the two cases, and to infer the existence of a law of nature which close breeding and consanguineous marriages equally infringed.

'Something like this I conceive to be the true history of the common opinion upon this subject, an opinion which, as far as I can discover, rests on no satisfactory record of observed facts.'

I am induced to insist the more strongly upon this aspect of the question because the works even of modern and professedly scientific writers bear witness both to the universality of this popular prejudice, and to the probability of its theological or rather ecclesiastical origin. Thus Niebuhr [c] speaks of the Ptolemies, whose history certainly affords the most striking instance on record of close breeding in the human race, as degenerate both in body and soul. He seems to forget that their dynasty continued for some three hundred

[c] 'Lectures on Ancient History,' vol. iii. p. 471.

years, and that the history of Cleopatra, the last sovereign, though not the last descendant, of their line, is certainly not that of a person, in any intelligible sense of the words, degenerate both in body and mind. But the most remarkable instance is afforded by Dr. Devay, who, while writing specially on this subject in his work on Hygiene, which he professes to treat scientifically, occupies no small portion of the two chapters devoted to it with a long citation of the opinions of fathers and doctors of the church from St. Augustine down to the contemporary Archbishop of Tours. Truly it might be considered a rare treat for orthodox Frenchmen in these sceptical days to find such authorities polled to settle a scientific question, were it not that a few recent events, such as the late rejection of M. Littré by the Institute, threaten to make such triumphs commonplace.

I turn now from the consideration of the spirit in which inquiries into our present subject have been undertaken, and proceed to give a succinct account of the facts and arguments which have been brought forward on both sides of the question, that my readers may have an opportunity of seeing what real value belongs to them, and to which side the balance of the evidence inclines. This evidence is derived from two distinct sources, which differ in their subject-matter, in the method by which they can be investigated, and in the degree of certitude which attaches to them as far as they severally go, no less than in the conclusion to which they lead. These are, (1) experience derived from the study of mankind by means of recorded observation and statistics; and (2) that drawn from the study of the lower animals and even of plants, which admits of being brought to the test of strict experiment as well as of observation. The former of these methods has been pursued with much diligence by Dr. Bemiss, MM. Boudin, Devay, and others. We give a short summary of the results arrived at by these observers, in order that our readers may be able at a glance to comprehend the several points to which we shall have to direct their attention.

	Dr. Bemiss.	Dr. Howe.	Dr. Devay.
Marriage	34	17	121
Fruitful	27	Not stated.	99
Sterile	7	Not stated.	22
Total Children	192	95	Not stated.

This gives in Dr. Bemiss' cases an average number of 5·6 children to each marriage; in Dr. Howe's 5·58 to each. The average number of births to each marriage in England was recently 4·5. Of the 192 children born, 58 died in early life, and 134 reached 'maturity;' *i.e.* the number of early deaths was as

1 to 3·3. The average of deaths under 5 years old, as stated by Dr. West, is 1 to 3. It is thus clear that while the fertility of these marriages was much above the average, the infant mortality in their offspring was slightly below. In Dr. Devay's cases the total number of children is not given, and therefore no calculation on the point can be made.

In consequence of the different principles upon which these authors have arranged their statistics, it is impossible to exhibit them at length in a tabular form, or indeed to contrast them at all in detail; we must therefore content ourselves with stating that the relation of the principal forms of disease or defects mentioned by them varies as follows:—

	Dr. Bemiss. In 75 Cases of Disease.	Dr. Howe. In 58 Cases of Disease.
Scrofula and Consumption	38 or ·506	12 or ·207
Epilepsy and Spasmodic Disease	12 or ·16	0 —
Deafness	2 or ·026	1 or ·017
Idiotcy	4 or ·053	44 or ·758
Deformity	2 or ·026	0 —

From the loose form in which Dr. Devay's results are stated, we are able to contrast his statement with the above in one point only, namely, that of deformity, which appears in 27 out of 52 cases, or ·519 as against ·026 in one of the other cases, and 0 in the other.

M. Boudin's statistics are of a different character and on a much larger scale. He takes merely the one defect of deaf-mutism, and finds, 1st, That while consanguineous marriages are 2 per cent. of all marriages in France, the number of deaf-mutes born of such marriages is, to all deaf-mutes,—

In Lyons	25 per cent.
In Paris	28 per cent.
In Bordeaux . . .	30 per cent.

He finds further: 2nd, That the danger of deaf and dumb offspring increases with the nearness of kinship between the parents; 3rd, That parents themselves deaf and dumb, do not, as a rule, produce deaf and dumb offspring, and that the defect is therefore not hereditary; 4th, That the number of deaf-mutes increases in proportion to the local difficulties to freedom of cross-marrying: thus it is in

France	6 in 10,000
Corsica . . .	14 in 10,000
Alps	23 in 10,000
Canton Berne .	28 in 10,000

Before entering upon any examination of these particular statistics, it is necessary to say a few words upon the application of the statistical method to subjects of this kind. It is scarcely possible to exaggerate the advantages which science, and especially biological science, has derived from the use of this method; but just in proportion to the benefit which accrues from the right use of any method, and to the consequent confidence which its application inspires, is the mischief which it can produce if misapplied, and the obstruction which it is capable of throwing in the way of the progress of knowledge when used upon a subject-matter to which it is unsuited. It may be applied, with every prospect of a successful result, in cases with which human volition has nothing to do, as it has been so applied to elucidate facts in pathology, such as the probability of death from a particular disease at a particular time of life.

Often too, when the will of man is an element in the calculation, but when that will can be shown to be swayed by conflicting motives the comparative power of which it is impossible to guage, a judicious application of the statistical method, if only the number of instances collected be sufficiently large, may enable us to arrive at a conclusion at least approximately true. But it does not follow from the full admission of all this, that the same method can be followed in cases such as that before us, and with a view to ascertain the causes as well as the circumstances of the phenomena to which it is applied. Thus, it may be true that we can arrive at the number of murders which will be committed in a population of a certain extent in a given time, but it does not follow that we can also tell what is the cause of all these murders, or that they all depend upon the same cause. Moreover, a murder is a fact which is usually discovered, quite independently of human testimony as to its mere occurrence; and if it is the interest of the perpetrator and his friends to conceal it, it is equally that of the friends of the victim to make it known. On the other hand, it is obvious that the value of statistics such as those the results of which we have just given, depends upon the truth of a number of family histories. These are all matters of testimony, and the motives to falsification thereof lie all on the same side. There is perhaps, as most lawyers and physicians are well aware, no point in which men are so morbidly sensitive and suspicious as one which touches a family secret, a family misfortune, or an hereditary disease. If a criminal could be convicted only upon the evidence of himself or his nearest relations, what would be the value of the statistics of crime?

These would form grave objections to any argument from statistics in a case such as that before us, and would justify us in questioning a conclusion founded exclusively upon them, even if the statistics themselves were irreproachable. Whether they are so or not in the present instance, I shall proceed next to inquire. In so doing I must beg my readers to bear in mind the purpose for which the statistics are brought forward. Their authors are all agreed that close breeding, whether in man or beast, tends of necessity to produce 'degeneracy' in some form or another; and this by some unexplained and apparently inexplicable law, quite apart from and independent of those ordinary laws of inheritance by the experience of whose action we are made aware that the diseases and peculiarities of the parent descend to his offspring, and this the more certainly if both the parents are similarly affected; and they present their several sets of statistics with the object of substantiating this view.

It is impossible not to be struck with the vague use of terms by all the writers who support this side of the question. They never seem able to escape as it were from the tyranny of their own phraseology, and appear to suppose that when they have introduced a long Latin word, with a perfectly indefinite meaning, they have gone a long way towards explaining a complicated series of facts. What is really meant by 'deterioration' or 'degeneracy'? Every variation from an original type, not to mention every disease, might, we suppose, be spoken of as degeneracy. Thus, adopting the hypothesis of the unity of the human race, if the first man was white, the black races would be degenerate, and *vice versâ;* and if he was intermediate in colour, like the Arab or the Brahmin, then would black and white both equally be degenerate. No one ever doubted the potent influence of close breeding in developing and perpetuating an accidental variety—it is indeed the one only means by which this can be done; and similarly, no one doubts that, given a degeneracy of any kind—a disease or a morbid tendency—already existing, close breeding will tend to develop and perpetuate it in exact proportion to the degree in which it is close. These are merely instances of the operation of the ordinary and well-known laws of inheritance, simple deductions from the time-honoured generalization expressed in the homely phrase 'Like breeds like;' and they are intelligible just in the same degree as are any other phenomena of nature which are referred to a general expression, which is for the existing state of science an ultimate fact. Breeders know well enough that the produce of two thoroughbred shorthorns, with whose pedigree they are well acquainted, will neither be a half-bred Alderney calf nor any other mongrel. But such facts as these are far too simple and well established to satisfy those writers who wish us to believe that if only the progenitors in this example be brother and sister, the produce might vary in the remarkable manner suggested. In the case before us, moreover, the most various and apparently unconnected forms of degeneracy are all attributed to the same cause. Exactly as a Scotch peasant puts every phenomenon of nature for which he is unable to render a reason, to the account of Sir William Wallace or the devil, so do these writers attribute every conceivable imperfection existing in the offspring of parents related in blood to the fact of consanguinity alone. Each observer, it is true, puts some one defect prominently forward, but in each case it is a different one.

The qualities of offspring at birth may be said to be the resultant of the reaction of the sum of those of the two parents upon one another, together with the modifications superinduced upon them by external circumstances. Now, as the antecedents upon which the condition of any offspring depends are thus extremely complicated, it is clear that nothing less than a very large and very unequivocal experience can justify us in asserting that, in a particular case, this, that, or the other phenomenon in the offspring is the result of this, that, or the other individual antecedent in the parents. Such experience in many instances we do possess. Hereditary gout and hereditary insanity are as clearly traceable through many generations in the families in which they are inherent as is the succession to the family estate, and very often much more so. They do not pass upon every member of such families, for many reasons, some of which we know, or are apt to think we know—such as emigration, change of external circumstances, habits of life, or even social position, and still more, the influence of successive intermarriages; but all this notwithstanding, the fact remains that such defects or peculiarities, once acquired, are, as a rule, transmitted to the offspring; and if the writers of whom we are speaking had contented themselves with showing that the marriages of blood relations are more likely, *cæteris paribus*, to produce unhealthy offspring than others where an hereditary taint exists, they would have made an assertion which, though neither very novel nor very interesting, could not well have been disputed. But what they really have asserted is something far different from this. It is, substantially, that if two persons marry, being related in blood, even at so distant a degree as that of second cousins, their offspring will, as a rule, be degenerate, or will themselves produce degenerate descendants. The following remarks by another writer are quoted by Dr. Devay, and adopted by him as accurately representing his own view. (Devay, 2nd ed., p. 246.)

'Ce qu'on reproche aux mariages consanguines ce n'est pas, dit le docteur Dechambre, de perpetuer dans les familles, par le moyen des alliances, les maladies susceptibles de transmission héréditaire, en certaines formes de tempérament, en certaines prédispositions organiques, comme l'étroitesse de la poitrine, ou quelque autre vice de conformation. *Il est manifeste que le condition de la consanguinité en soi n'ajoute rien aux chances d'hérédité morbide,* lesquelles dépendant de la santé des conjoints et de celle de leurs ascendants reciproques, ont la même source dans toute espèce de mariage. On accuse les alliances entre parents de même souche *d'amener de créer par le seul fait de non renouvellement de sang*, une cause spécial de dégradation organique, fatale à la propagation de l'espèce.'

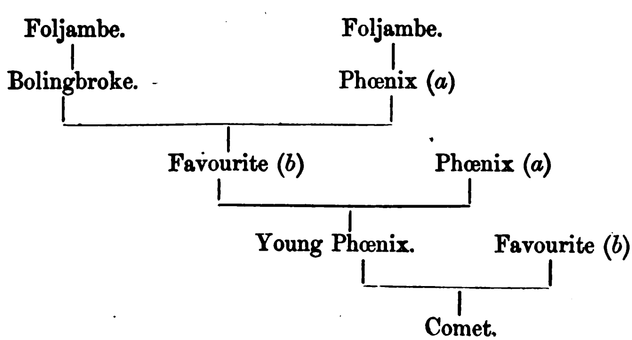
The questions, then, which we have to examine are as follows:—1. Is such a view as the above borne out by the facts which these writers have adduced in support of it? 2. Cannot these facts be equally well explained by the action of the ordinary laws of inheritance? and 3. Are there not other facts left out of view by these writers, which are not only left unexplained by their doctrine, but are quite irreconcilable with it? 1. The first reflection which occurs to a reader on looking at the statistics just quoted, is, as I noticed above, the extreme diversity of the effects which are in them assigned to one and the same cause, and that, too, in cases in which the antecedents and consequents are many in number, and consist of various elements, some known and more unknown, complicated and involved among themselves in every variety of combination. The old school definition of an efficient cause, '*Præsens effectum facit, mutatum mutat, sublatum tollit,*' is doubtless far too narrow to be rigidly applied in investigations into the phenomena of nature; yet we cannot but look suspiciously at an alleged cause which fails to conform to the definition in every single particular. In the case before us we all know perfectly well that the five principal consequences here alleged to follow upon consanguineous marriages—viz., sterility, mutism, idiocy, deformity, and scrofula—all occur in children when no such marriage has been contracted by the parents, and are all absent far more often than present when it has. The attempt to account for them all by the same cause reminds us of nothing so much as the similar attempt to explain all geological phenomena as the effects of the Noachian deluge, and can only lead to physiological absurdities, as that unlucky hypothesis did to geological. Moreover, in all but one of these cases we know of other well-established causes upon which the unhappy results are often found to depend, and unless it can be shown that these are excluded in the instance before us, we are not at liberty to introduce a new cause of which nothing is certainly known. This brings us (2) in the second place to the consideration of how far the facts adduced can be explained by the known laws of inheritance. There is a phenomenon well known to breeders of animals, and frequently observed also among mankind, which has been recognised by physiologists under the name of atavism. By atavism is meant a tendency, the laws of whose action are at present quite unknown to us, on the part of offspring to revert to some more or less ancestral type. Instances are not far to seek, and are familiar to many even who have not gone further than to remark the phenomenon itself. It is no uncommon thing to find a child born who grows up with but little resemblance to his immediate parents, but bearing a strong and remarkable likeness to some grandfather, or great-uncle, or other even more distant ancestor. This is a fact of common experience, nor is the likeness confined to figure or features, for similarities of disposition and temper, peculiarities both of mind and body, and even diseases, are found to descend in the same irregular and apparently unaccountable manner. Gout, one of the most hereditary of maladies, has even been supposed habitually to miss each alternate generation, and fall upon the next beyond. These things, we repeat, are known to happen among mankind, but from the length of human life, as compared with that of the domestic animals, it is among the latter that we find, as we might expect, that they have been most frequently observed, and in fact, the tendency to atavism is, we believe, habitually recognised and allowed for by the breeders of cattle. But though the fact is undoubted, no man can point out beforehand the individual case in which this reversion to the old type, this relapse, as we may call it, will take place, and many a time, doubtless, has its sudden occurrence frustrated the hopes of the breeder and wasted his labour and care. Now, if the known fact of atavism is fairly considered, it at once affords an answer to the objection of M. Boudin and Dr. Devay, that the various defects and diseases, the statistics of which they have collected, cannot be traced to the parents of those subject to them, and cannot therefore be looked upon as hereditary. The commonest acquaintance with the ordinary conditions of human life will enable any one to see that it is impossible for a medical man to investigate the family histories of any fifty of his patients, so far as to arrive at a clear notion of what has been the condition of health of even the four grandparents whom nature apportions to us all; and yet, without this, how can he pronounce with any certainty that a particular disease or infirmity is not inherited? It may be urged, no doubt with some force, that to bring into the discussion a phenomenon of which we know so little as we do of atavism, is to appeal not to our knowledge but to our ignorance; but the same is true, and true in a far higher degree, of consanguinity itself.

So far as we have gone at present, it may be said that the two sides of the argument are on the whole pretty evenly balanced. The statistics of MM. Bemiss, Howe, and Devay may be left to answer one another, and even if they be considered to fail in doing so, the number of instances collected by these gentlemen is insufficient to afford more than the feeblest presumption in favour of their conclusion. But when M. Boudin comes forward, counting his instances by thousands, and tells us that in France the number of deaf-mutes who are descendants of consanguineous marriages is from ten to fifteen times what it ought to be when compared with the proportion which such unions bear to the whole number of marriages, we feel that we are on different ground. Such announcements cannot fail to produce in most men's minds a strong apprehension, at the very least, that the two phenomena which he is labouring to connect have, after all, some close mutual interdependence. On the other hand, when we fairly consider the difficulties, some of which we have just seen, which lie in the way of demonstrating that the defect is not in many cases inherited, the extremely complicated character of the phenomena with which we have to deal, and, above all, the fact that on M. Boudin's own showing, the alleged cause is absent in an absolute majority of the cases in which the effect is seen to follow, we are once again compelled to suspend our judgment, and to look further for new facts before we can arrive at a conclusion.

So far, then, we might conclude that the imperfect condition of our knowledge of the phenomena of inheritance, including in that term variation and atavism, precludes our coming to any decision upon the subject, but that the general consent of mankind, together with the positive evidence which has been given, is sufficient at any rate to arouse in our minds some misgivings lest the 'law of nature' which Dr. Devay and others contend for should really be found to exist: but before we can fairly yield, even to this extent, to the arguments of these authors, we must provide an answer to the third query, viz. (3) Whether there are not some facts which are quite irreconcilable with the theory in question? Now, in the case of the human race, the difficulty of obtaining trustworthy evidence is so great, that we should despair of ever attaining even to an approximation to the truth, did we depend on it alone. It consists almost exclusively of the published opinions of certain observers, more or less competent, as to the hygienic condition of certain small communities who from their isolated position are either supposed or known to intermarry frequently among themselves; and these opinions are found to be as contradictory in character as they are scanty in number. Fortunately, however, the evidence derived from the breeding of animals, and the record of that evidence preserved in the 'Herd-book' and the 'Stud-book,' is clear and decisive upon this point.

Mr. J. H. Walsh, well known, under the *nom-de-plume* of Stonehenge, as an authority upon sporting matters, says distinctly in his recent work, that nearly all our thorough-bred horses are bred in and in. M. Beaudouin also, in a memoir to be found in the *Comptes Rendus* of Aug. 5, 1862, gives some very interesting particulars of a flock of Merino sheep bred in and in, for a period of two-and-twenty years, without a single cross, and with perfectly successful results, there being no sign of decreased fertility, and the breed having in other respects improved.

In a former paper on this subject I have given the pedigree of the celebrated bull 'Comet,' and of some other animals bred with a degree of closeness such as no one who has not studied the subject would believe possible, and any approach to which in the human race would be quite impossible. In one of these cases the same animal appears as the sire in *four* successive generations. The pedigree of 'Comet' is as follows:—



Now, bearing in mind the argument of MM. Boudin, Devay, &c., that it is nothing but the mere nearness of blood relationship, and not any ordinary inheritance of parental defects, which produces the ill effects which they trace to consanguinity, such examples as these ought surely to have great weight. It is clear that even if it were established that such breeding as that from which 'Comet' was descended had invariably led to degeneracy and disease, we should not be thereby warranted in arguing from it that an occasional marriage of cousins among mankind had even the slightest tendency to produce similar results. But, on the other hand, we may certainly allege with some fairness, that if at the end of such a pedigree there is produced a remarkably fine specimen of the species to which it belongs, mere close breeding, independently of the qualities of the animals bred from, can have no ill tendency at all. At once so obvious and so forcible has this argument been felt to be, that the supporters of the opposite view have been at considerable pains to evade or destroy it. Four principal objections have been laid against either the admissibility or the value of the evidence derived from the lower animals. (1.) It has been said that prize-animals are not in fact perfect animals, but monsters, *i.e.* deviations from, or modifications of, the natural type of the species, induced by man with the object of fitting them for special purposes of his own. (2.) That pigs and other animals have been known to die out altogether after being bred in and in for several generations. (3.) That the evidence is valueless as applied to mankind, inasmuch as when animals are closely bred with success, the progenitors in such cases are carefully selected from among the stoutest and most healthy that the breeder can obtain. (4.) The last objection applies especially, or indeed exclusively, to M. Boudin's attempt to prove the prevalence of deaf-mutism in the offspring of consanguineous marriages; it is that the defect is one from which man, 'the talking animal,' alone can suffer, and one therefore expressly designed by Providence to punish man for a breach of nature's law. The special ingenuity of this objection lies in the attempt which it makes to draw a broad distinction between man and the lower animals, and thus to discredit the evidence derived from the latter in its application to the former. It is however more ingenious than conclusive, for deaf-dumbness means, as a rule, congenital deafness, and such a defect is almost as serious where it exists in the lower animals as in man.

As the settlement of this question of the applicability to man of the evidence derived from the lower animals, seems to be of great importance to the thorough understanding of the whole subject before us, I proceed to examine the above objections somewhat in detail.

(1.) The statement that prize-animals are unnatural, and therefore not perfect animals, nor fair types of their several races, contains undeniably a certain amount of truth. Those mere quivering masses of fat which appear from time to time in Baker Street under the title of prize-pigs, are doubtless no nearer an approach to the perfection of pig-nature than was the celebrated Daniel Lambert to the noblest standard of corporeal humanity; but it is no wise proved that they are in any intelligible sense degenerate. They are not only carefully bred, but also artificially fattened for a special purpose; and there is no more reason to doubt that they would have been quite different animals had they been differently treated, than there is that the same man who is spare and active as a Newmarket jockey, might become corpulent, pursy, and dyspeptic, if he entered on 'the public line' and spent his time dozing in his bar over rum-and-water and a pipe. This objection is therefore not proven even when most strongly put, and when a fairer instance is taken will be found to break down utterly. Such an instance is to be found in the English thoroughbred horse. Writers upon sporting matters are pretty generally agreed that no horse either bears fatigue so well or recovers from its effects so soon as the thorough-bred, and it is a subject upon which such writers are the best of all authorities. Thus 'Nimrod' concludes a comparison between the thorough-bred and the half-bred hunter in the following words: 'As for his powers of endurance under equal sufferings, they doubtless would exceed those of the "cocktail," and being by his nature what is termed a better doer in the stable, he is sooner at his work again than the other. *Indeed, there is scarcely a limit to the work of full-bred hunters* of good form and constitution and temper;' and yet these, as we have seen, are almost all close-bred.

(2.) With regard to the allegation that some animals have been known to die out after being closely interbred through a long series of generations, while we do not dispute the fact that such may have been the case, we are not aware of any instance of which the particulars have been noted in a satisfactory or really scientific form. We know neither after how many generations this result was produced, what was the degree of close breeding, nor what were the other conditions under which the animals were placed. All these particulars it is necessary to know before we admit the efficiency of *mere* close breeding as a cause of degeneracy, in the face of the evidence above adduced. The last, viz. the conditions under which the creatures were placed, is a matter of the greatest importance, inasmuch as if once any particular disease or defect be induced upon a stock, there is no doubt that it can be transmitted and intensified to an indefinite degree by close breeding. Just as a careful breeder can take dvantage of any accidental variety produced in his stock, and perpetuate it, if it be desirable to do so, so by careless close breeding may a disease be perpetuated, however undesirable or mischievous it be.

(3.) That the selection which is always practised in the close breeding of animals should ever have been brought forward at all, as against the applicability of evidence thence derived to the case of the human race, is a fact both curious and significant. It is so inasmuch as it shows at once how completely the few persons who have been at the pains to consider this subject at all have looked upon it not as a question of scientific physiology, but merely from a practical point of view. The question which really has to be decided is not whether under any particular circumstances close breeding is desirable or not, but whether any evil effect, or specific effects of any kind, are traceable to close breeding in itself, and independently of the condition, health, and perfection of the animals in whose case it is practised. We have seen this distinctly affirmed by Dr. Devay in the passage already quoted; if, therefore, we take his statement as it stands, it is quite clear that selection does not affect the question in the slightest degree. Dr. Devay states that the evils which he charges upon marriages of consanguinity are simply and solely due to the *non-renewal* of the blood, as he terms it, independently of any previous taint in the progenitors, which, he even ventures to assert, where it exists adds nothing to the chances of degeneration in the offspring. Now, the non-renewal of the blood is manifestly just as complete, if the degree of close breeding be the same, when the most careful selection has been exercised, as where none has; and if, as in some of the instances which we have cited (the bull 'Comet,' for example), close breeding, with selection, has been carried to an extent inconceivably greater than is possible in the human race, with no ill-consequences whatever, this constitutes a simple demonstration that mere non-renewal of the blood does not necessarily cause degeneracy, and that Dr. Devay's theory is therefore utterly untenable. In point of fact, what we may really learn by studying the effect of selection is that no law of nature whatever is infringed by close breeding, to whatever extent it be carried, but that precisely the same laws of inheritance obtain in it as in other cases.

The distinction which is now drawn between the study of this subject as a question of scientific physiology, and as a matter affecting practical life, is one of some importance. The consideration of it from the latter point of view might, if a sufficient number of trustworthy facts could be collected, be of some value, at least as a guide to indicate the direction in which investigation of a more scientific character could be carried on with the best prospect of success. Thus, the fact which M. Boudin has brought forward might profitably induce any one who should have the means of doing it, to investigate what are really the causes of congenital deafness. It is impossible to believe that mere non-renewal of blood is the cause, since the phenomenon is met with where the supposed cause is absent, and is itself absent in the great majority of cases in which the cause is in operation. The next step, therefore, should be to endeavour to learn what are all the antecedents in a mass of cases of deaf-mutism, with the view of discovering any one which is common to them all. When this is carefully done, it may not improbably be found that some other and quite dissimilar phenomenon has existed in the progenitors, having a tendency to bring about deafness in their offspring, and that this tendency has been developed with additional force by the marriage with the same family, exactly as is the case with other taints of disease. In order to illustrate our meaning, let us take, for example, one of those cases of correlation of growth brought forward by Mr. Darwin. He finds that all cats having blue eyes are deaf. Now, it has been found, and cases in proof of it have been published, that this is not absolutely true, though approximately so. It is evident that there is some causal connexion between these two phenomena, though what it may be is entirely unknown. Let us suppose, then, that previously to the announcement of this fact by Mr. Darwin, any one holding Dr. Devay's views on consanguinity had been making observations upon it on certain cats. He chances to have two cats with blue eyes, but not deaf, brother and sister we will suppose: upon these two breeding together the progeny produced are deaf. The observer in this case would almost certainly conclude that the deafness was a result of the consanguinity of the parents, whereas, had he known more of the antecedents of the case, he would have seen that the blue eyes of the parents indicated a strong tendency to deafness, and that this being the case in both, deafness had actually resulted in the offspring by the action of the ordinary laws of inheritance. Or, to give another example, which will be unhappily more familiar to many of our readers, and which deals more with actual and less with hypothetical facts than the above, let us take the case of hydrocephalus, or water on the brain, as it occurs in infants. This disease is now well known to be in one of its two forms a manifestation of the same constitutional disorder which produces consumption and other forms of scrofula; but this knowledge is a comparatively recent acquisition of pathological science. Had Dr. Devay then been conducting researches into the question of consanguinity, he might doubtless have discovered in certain regions where consumption was very prevalent, that the children of cousins were unusually subject to hydrocephalus, and not knowing of any connexion between two diseases superficially so different, would doubtless have announced that this was a special provision of Providence to restrain mankind from consanguineous marriages, with as much confidence as he has now declared the same of deaf-dumbness, deformity, &c.

It is only by some really scientific investigation of the facts, some investigation, that is, which shall reduce them under the operation of a recognised, or at least recognisable law, that we can hope to obtain even such a knowledge of this subject as shall serve for a guide in practical life; and mere empirical generalizations such as those of Dr. Devay and M. Boudin are of little or no value even for this purpose, so long at least as the exceptional cases continue far more numerous than those which can be brought under the law. Such generalizations act more often than not as mere hindrances to the progress of science, or help it on only in so far as they provoke discussion, and thus, in the very process of being themselves overthrown, contribute to increase or correct our knowledge of the facts upon which they profess to be founded.

We have now, then, arrived at the end of another stage of our inquiry, and must consider that the question which was left in doubt by the near balance of the evidence obtained from the study of mankind, is settled decisively against the theory which attributes ill effects to the mere non-renewal of the blood by the much more extensive and less equivocal evidence which we derive from experiment upon the lower animals. And in this position we might have been content to leave the subject, had not Mr. Darwin recently entered the arena as a champion in the same cause as Dr. Devay. The whole of Mr. Darwin's most interesting and valuable volume upon the Fertilization of Orchids was written, as he tells us at the outset, in order to substantiate the assertion that 'it is apparently a universal law of nature that organic beings require an occasional cross with another individual.' This supposed law of nature is very ingeniously used in Mr. Darwin's previous work to serve as a support of the theory there advanced as to the origin of species, and at the end of the volume from which I quote, the author sums up his views upon the point in the following words, which will no doubt be fresh in the memory of many of our readers:—

'Considering how precious the pollen of orchids evidently is, and what care has been bestowed on its organization and on the accessory parts, considering that the anther always stands close behind or above the stigma, self-fertilization would have been an incomparably safer process than the transportal of the pollen from flower to flower. It is an astonishing fact that self-fertilization should not have been an habitual occurrence. It apparently demonstrates to us, that there must be something injurious in the process. Nature thus tells us, in the most emphatic manner, that she abhors perpetual self-fertilization. This conclusion seems to be of high importance, and perhaps justifies the lengthy details given in this volume. For may we not further infer as probable, in accordance with the belief of the vast majority of the breeders of our domestic productions, that marriage between near relations is likewise in some way injurious—that some unknown great good is derived from the union of individuals which have been kept distinct for many generations?'—pp. 359, 360.

It is not my present purpose to enter into any general discussion of Mr. Darwin's views, but I must take this opportunity of expressing the admiration which I feel for the marvellous diligence with which he has observed and recorded the phenomena of nature, the clearness of his descriptions, and, above all, the admirable candour with which he has admitted the full force and cogency of some of the objections which lie against his theory. My present business is with the very much narrower consideration of how far the inferences which he has drawn, in the very small portion of his subject which affects the question before us, are really borne out by the facts which he has adduced in their support, and whether there are not other facts of a precisely similar character which cannot be reconciled with them.

Mr. Darwin's argument, stated in a succinct form, appears to be as follows. If we examine the class of orchids, we find that the stigma and the pollinia, in most cases, exist in the same flower, and are in very close juxtaposition. We find also various indications that the pollen of orchids is precious, that is to say, it exists in small quantities, and various precautions, as we may call them, are taken by nature to prevent its waste. These facts, taken together, would naturally lead us to suppose that orchids would be self-fertilizing, but we find, on the contrary, that in by far the greater number of species the most curious and elaborate contrivances exist, whereby the fertilization of one flower by the pollen of another almost invariably occurs through the medium of insects, and that if the visits of insects are artificially prevented, no fertilization takes place. We may hence conclude that some evil must result to the species from the perpetual recurrence of self-fertilization, and may extend our inference so far as to suppose that close-breeding of any kind, even in so diluted a form as that practised among civilized mankind by the marriage of cousins, is in some unknown way injurious, and, in fact, that within certain limits the more remote is the connexion between two individuals who are to breed together, the better will it be for their offspring.

It is certainly curious that this should be the doctrine of one whose main theory leads directly to the conclusion that all organic beings are the lineal descendants of some one primæval monad. I do not mean for a moment to say that more than a mere apparent and superficial contradiction is here suggested, for intercrossing is merely one among many of the forces to which Mr. Darwin refers the gradual evolution of new forms of life, and it is one which we may easily suppose to have come into action at a period comparatively recent. But when we come to look into the argument more closely, the first tincture of distrust is imparted to our minds by the fact that, after all, it is but an argument from 'final causes.' Now, final causes have been looked upon with some suspicion ever since the time of Bacon; and it has certainly not been by the investigation of them that the chief discoveries of modern days have been made. In point of fact, in making use of an argument of this kind a man leaves everything like firm ground behind him, and sails out upon an ocean of uncertainties in which he has neither chart nor compass by which to steer. When he argues that such a phenomenon must exist for such a purpose, because there is no other purpose for which it can exist, it is obvious that his real meaning is,—because I don't know of any other purpose which it can subserve. But since the facts of nature which we understand, bear no very large proportion to those of which we are ignorant, these two propositions do not seem to bear any very necessary relation to each other. And after all, what has Mr. Darwin really proved? He has shown us that in the greater number of species of one class of plants certain arrangements which, on a superficial view, would seem intended to bring about constant self-fertilization, are found, when more closely looked into, to conduce to exactly the contrary result; but it remains upon his own showing that there are, at least in one species, the bee-ophrys, equally elaborate contrivances for the production of self-fertilization, as exist in the others for the prevention of it. If there were anything necessarily pernicious in the process itself, how is it that this exceptional case does not become extinct, instead of being, as Mr. Darwin admits that it is, one of the most prolific of our native orchids? We may admit what he also shows, viz. that *occasional* intercrosses are also brought about even in this case; but if we take the fact of the rarity of this event, together with that of the prolific character of the plant, it will be hard to arrive at a conclusion therefrom which will satisfy the requirements of Mr. Darwin's theory.

If we find that in the bee-ophrys, for instance, self-fertilization takes place fifty times while a cross occurs once, we are quite as well justified, to say the least, in arguing that it is a beneficial process because it is the rule, as that it is a pernicious one because it is a rule which admits of some few exceptions. Now, in point of fact, if we take the whole vegetable kingdom, instead of the one order of orchids, we shall find that the latter are almost as exceptional in their mode of fertilization, as compared with other plants, as is the bee-ophrys when compared with other orchids. In some cases, as that of the barberry, contrivances very similar to those described in the orchids exist for the very purpose of convenient self-fertilization; but such instances Mr. Darwin meets by the statement, that if several varieties of barberry are growing together, it is found that intermediate forms do in fact spring up, thus proving that mutual fertilization frequently occurs. Here, again, the same objection seems to lie, namely, that his inference is drawn not from the rule but from the exception. In the instance both of the bee-ophrys and of the barberry, self-fertilization is the ordinary mode of propagation, and it is therefore difficult to believe that in the vast series of past generations from which every existing plant has sprung, there have been any appreciable proportion of crosses. I am not here concerned to discuss the bearing of this matter upon Mr. Darwin's main argument, viz. the origin of species. It is, perhaps, possible that the supposition of a cross taking place once in fifty, or once in two hundred times, might satisfy the requirements of his theory. All which I have to do is to examine its bearing upon the questions which he has connected with it in the passage I have cited, and this certainly seems sufficiently remote. It is surely somewhat unsatisfactory reasoning to say,—'It appears necessary in all cases that there should be an occasional interruption to the perpetual series of self-fertilization, in all organic beings, *therefore* we may believe that a similar occasional intercross is necessary where breeding takes place between two individuals of very near blood-relationship, hence we may further infer that such intercrosses should be the rule; and finally, that even an occasional instance of interbreeding between two individuals very slightly related in blood is likely to be productive of serious degeneration in the offspring.' Yet this is really but a paraphrase of Mr. Darwin's reasoning in the above passage of his work. The difference of degree between the cases is so great as to destroy all analogy between them, and render the reasoning which might be sound in the one case totally inapplicable to the other. So great is their difference, that if, from the mere non-renewal of the blood, any appreciable degeneration took place in the offspring of a marriage of cousins, our finest breeds of sheep and cattle and horses would have long since become the most miserably degenerate beings on the face of the earth, if indeed any of them still remained upon it.

In conclusion, I will inquire shortly into the evidence which has been afforded by certain experiments recently made upon the growth of wheat, having for their object its improvement for agricultural purposes, and made, therefore, without any previous bias in favour either of close breeding or of crossing.

In pacing through the Great Exhibition of last summer, many of our readers may have noticed among the agricultural products in the Eastern Annexe some magnificent ears of corn, bearing the somewhat novel title of 'pedigree wheat,' which excited the admiration of all those interested in such matters. This wheat was exhibited by Mr. Hallett, of Brighton, who has given its history in the Royal Agricultural Society's Journal, vol. xxii. part 2. It appears that this gentleman having conceived the notion that careful breeding might produce some of the same advantages in cereals which it has been found to do in cattle and horses, commenced some years ago a series of experiments with the view of carrying out his idea. Having selected one ear of wheat of remarkably fine quality, he sowed the grains separately, at a distance of twelve inches apart. The next year he further selected the one finest ear produced from the former, and treated that in a similar way. The following table gives the result at the end of the fifth year from the original sowing :—

Year.		Length.	Containing	Number of ears on Stool.
		Inches.	Grains.	
1857	Original ear	$4\frac{3}{8}$	45	...
1858	Finest ear	$6\frac{1}{4}$	79	10
1859	Ditto	$7\frac{3}{4}$	91	22
1860	Ears imperfect from wet season	...	...	39
1861	Finest ear	$8\frac{3}{4}$	123	52

'Thus,' says Mr. Hallett, 'by means of repeated selection alone, the length of the ears has been doubled, their contents nearly trebled, and the tillering power of the seed increased five-fold.' By 'tillering,' I should perhaps mention, is meant the horizontal growth of the wheat-plant, which takes place before the vertical stems are thrown up, and upon the extent of which, therefore, depends in a great degree the number of ears which the single plant produces. Now there can be no doubt that a great deal of the marvellous improvement shown in the above table is due to the treatment which Mr. Hallett subjected his wheat; that is to say, to the fact of its being sown singly and apart, so that each plant has been allowed to develop itself fully; but we cannot attribute the whole to this cause.

The point in which we are especially interested is the fact that this wheat was, without any reasonable doubt, close bred throughout the whole of these five generations; and the result has been not deterioration, but most marked improvement. If we consider the structure of the wheat-flower, and the conditions under which it grew in these cases, we cannot entertain a doubt upon this question. Each individual flower is hermaphrodite, the flowers grow close together in a spike, and the number of stems thrown up from one seed all stand in a mass together. Hence it is hardly possible that the stigma of any one flower should receive pollen from any but either its own anthers or those of another flower on the same plant, which even Mr. Darwin himself admits can hardly be considered as a distinct individual. That Mr. Hallett himself has no doubt upon this point is proved by the following extract from a private letter of his, to myself, in which he thus answers a question upon this subject. 'As to crossing, I must in theory admit the *possibility* of its taking place, but have the fullest conviction that practically it has not taken place in my wheat and other cereals.'

Mr. Hallett had also found that the improvement in the sixth generation has been even greater than in any of the others. Now, though it is true that the result of a trial of six generations does not vouch for that of one of sixty or six hundred, it is still good as far as it goes, and since it has led to a marked and unprecedented improvement in the original stock, it certainly tends to throw doubt upon the opinion that mere close-breeding is of itself productive of degeneration.

On the whole evidence before us, then, I cannot conclude otherwise than that the very general opinion, that there is some special law of nature which close-breeding infringes, is founded rather on a kind of superstition than on any really scientific considerations. If we look upon the question as one of science, we find that of the facts given as evidence in favour of this opinion, all except those adduced by M. Boudin can without difficulty be reduced under the ordinary laws of inheritance; and even those which he has brought forward, though at present not accounted for by the same laws, cannot be shown to be exceptions to their action, and remain quite equally unaccounted for by the introduction of the hypothesis under discussion. On the other hand, the known facts brought to light by investigation among the lower animals and plants, are such as positively to disprove this hypothesis as regards them; and it would require much more stringent proof than any one has ever yet attempted to bring forward, in order to justify us in believing that man is under the action of physiological laws differing from those which obtain in the rest of the organic world. The aspect of the question before us from the practical point of view is, however, somewhat different. Here further evidence is still required, and will no doubt be collected. It is of course conceivable, whether probable or not, that there may exist at the present time in civilized communities, so few families really free from all taint of disease or imperfection, as to render intermarriage of blood relations unsafe by the action of the ordinary laws of inheritance. I am indeed strongly disposed to disbelieve, in the absence of strict evidence, in any such degenerate condition as the normal state of modern humanity; but it is this point, and nothing further, which observation and statistics are capable of deciding; and in order even to do this, the observations must be more careful and the statistics far more extensive than any which have yet been recorded.

NOTE ON ESSAYS I AND II.

A VERY elaborate Essay upon the above subject, by Mr. William Adam, wherein it is discussed both in its physiological and its legal bearings, appeared in the Fortnightly Review, vol. ii. p. 710, and vol. iii. p. 74. Mr. Adam's conclusion on the physiological question is as follows, vol. iii. p. 81 :—'That there is no physiological law against consanguineous unions: by which it is meant to be affirmed that there are no injurious physical consequences which necessarily and universally follow them. In the vegetable kingdom self-fertilization is common and salutary. In the animal kingdom close-breeding does not deteriorate, and often improves, the breed. In the human race the alleged bad effects are not proved, and they are disproved by the occurrence of the alleged cause without those bad effects, and of the bad effects independent of the alleged cause. Further, there is no proof of the physical deterioration of those divisions of mankind amongst whom consanguineous unions are known more or less to have prevailed. Ancient history furnishes no ground for supposing that the Persian and Egyptian nations suffered any physical degeneracy from that cause.' It is certainly worthy of remark—and it is a point referred to by Mr. Adam—that, if we suppose all mankind to have sprung from a single pair, all mankind must be consanguineous, and in the earlier portion of its history *all* marriages must have been marriages of consanguinity; and it is not easy to understand the conception of a 'law of nature' which visits with condign punishment, now, a kind of union the formation of which was in former times the indispensable condition of the continuance of the human race upon the earth's surface.

For the following observations in confirmation of the views maintained in the above Essay, I am indebted to the kindness of Dr. John Davy:—

'There are some facts which lead me to think that if animals coming together to breed are quite healthy, free from any taint of disease, that their offspring will be healthy.

'In small isolated societies there must be much breeding in and in unless special precautions be taken to prevent it. I shall mention a few instances, in which I believe no precautions of this kind have been attended to.

'Some forty years ago I visited the secluded little dale, Glenfinlass, in the Highlands of Perthshire, and I there learnt that, with one exception, there was no instance in the memory of man of a Stuart marrying out of the glen. The few families I believe were healthy: at the house we were entertained, our hostess was remarkable for beauty, and was above the average size of women. They lead a pastoral life; milk forming a good part of their diet.

'In the Scottish islands and islets, in many of which the inhabitants are few in number, and have little intercourse with the mainland, the same kind of marriages must often occur: yet the people are supposed to be, and I believe are, nowise degenerate. We are told on good authority that pulmonary consumption is comparatively rare amongst them. In Cornwall there are small fishing villages so situated, so in a manner isolated, that marrying of blood-relations must be common,—such villages as Mousehole and Newlyn in the Mount's Bay; yet these people bear no marks of degeneracy, but the contrary: they are remarkable for their good looks, and, I believe, good health; to which the active habits of both sexes, and their fish diet, may greatly conduce,—and their living so much in the open air.

'In the Mediterranean there are many similar examples of small isolated societies, amongst whom there appears to be the enjoyment of more than ordinary health and freedom from hereditary disease. They occur in the Lipari islands, in the Islands, or rather islets, belonging to the Ionian Islands, such as Fanno, Maganisi, Vido, Cerigo. As well as I could judge when I visited these spots, their inhabitants were peculiarly favoured as to health, and as to good constitutions. The population of Stromboli, one of the largest of the Lipari, amounted, when I was there some 35 years ago, to about 1500. There was not a medical man or a lawyer in the island. Agriculture was the main occupation of the inhabitants—the culture of the vine. No man there was idle, and all seemed in easy circumstances favourable to health. One of the most considerable of them told me that the only precaution he took to keep himself in health was to change his shirt after working in his vineyard. In the island of Fanno towards the entrance of the Adriatic, the inhabitants lead much the same kind of life, and are, I believe, equally healthy, and as personable in their appearance. As to size, they are not less than the average; they are singularly contrasted with their cattle—a small breed, smaller by far than that of the neighbouring Corfu, or of Italy on the Calabrian coast, yet of delicate make, and very active. Their peculiarity as to size may be owing—such was my conjecture on the spot—to a scanty pasturage, and purity of blood; and in relation to health and form, goodness of climate and wholesomeness of food, though scanty.'

Again Dr. Davy writes:—'About ten years ago, a pair of red deer were taken from the herd and put into a paddock of twenty or thirty acres adjoining Stornoway Castle, Isle of Lewis, the property of Sir James Matheson, Bart. There confined, there has been a yearly increase; the number now (1862) is about 23, not including several killed. They are all descendants of the original pair; and I was assured by my informant, the keeper, under whose observation they had been the whole time, that compared with the deer of the forest, they showed a marked improvement.'

And once more:—'Looking over one of my note-books I find a fact which may be interesting to you. Conversing with an intelligent Cumberland sheep-farmer about dogs,—he said that breeding in and in did not, he thought, do them any harm. He gave the instance of 'a dog the father of a bitch, and in succession of her puppies, and also of the puppies of one of the latter,—and the dogs were all good dogs.'

Dr. Davy's conclusion is as follows:—'The principle, if I may use the expression, that seems to me most in accordance with facts is that, if there be vigour and health and no taint of blood, the offspring of parents however nearly allied need not be degenerate.'

I should add that a very similar conclusion was maintained by no less an authority than Von Baer at a congress of anthropologists held at Göttingen as long ago as 1861—a fact with which I did not become acquainted until after the publication of my first Essay on the subject, and for the knowledge of which I am indebted to Professor Rolleston. Von Baer's words are as follows:—'Gegen eine weit verbreitete Ansicht dass die nahe Verwandtschaft der Eltern eine schwächliche oder unfähige Nachkommenschaft erzeuge, wurde der specielle Nachweis von der kräftigen Gesundheit einer Familie gegeben, in der seit langer Zeit die ehelichen Verbindungen unter nahen Verwandten gewöhnlich waren, und der ohne Zweifel richtige Schluss gezogen dass nicht die nahe Verwandtschaft an sich schädlich sei, wohl aber eine Krankheitsanlage sich mehrt wenn sie in beiden Erzeugern sich findet und aus ihnen auf die Nachkommenschaft wirkt.'

Thus the conclusions of these distinguished physiologists coincide entirely with my own, while those arrived at by Mr. Adam go, if anything, further than I should be prepared to follow in the same direction.

ESSAY III.

Recent Researches on the Production of the lowest forms of Animal and Vegetable Life [a].

It is not the intention of the present paper to enter into the whole history of the speculations of physiologists on the subject of the spontaneous generation of living organism, a subject which has been one of the vexed questions of biological science from almost the earliest times, and which is even now, as I shall have abundant opportunity of showing, far from being finally decided. Any of

[a] 1. 'Hétérogénie.' Par F. A. Pouchet. Paris, 1859.

2. 'Mémoires sur les Corpuscules organisés qui existent dans l'Atmosphère.' Par L. Pasteur. ('Annales des Sciences Naturelles,' vol. xvi.) Paris, 1861.

3. 'Etudes Expérimentales sur la Genèse Spontanée.' Par F. Pouchet.

4. 'Papers in the Comptes Rendus for 1859–64.' By MM. Pouchet, Pasteur, Joly, and Musset, Schaafhausen, &c.

5. 'Journal of the Royal Institute of Lombardy.' ('Giornale dell R. Instituto Lombardo,' 1851, p. 467.) Communicated by P. Mantegazza.

6. 'Sulla Generazione Spontanea note Sperimental.' Del Paolo Mantegazza. Milano, 1864.

7. 'Nouvelles Expériences sur la Génération Spontanée, &c.' Par F. A. Pouchet. Paris, 1864.

my readers who desire such a history will find a good summary of it in the early chapters of the first two treatises just referred to; and should they wish to go still further and deeper into the question, will find in the same place abundant references to original sources of information. All that I can attempt at present is to give a *résumé* of the most recent experiments and investigations which have been undertaken, and to indicate the present position of the question. The long controversy in the last century between Needham and Spallanzani left the question in dispute much where it was before. For whereas it was made clear, by the experiments of these observers, that putrescible matter if boiled for a length of time in closed vessels could be preserved without alteration, it remained doubtful whether the somewhat rough process to which the experiment was submitted had merely killed the germs of organisms contained in the vessel, or whether it had produced some such change in the constitution of the contained air as rendered it incapable of sustaining organic life. The latter of these views, though apparently the less probable of the two, obtained great credit from the support of Gay-Lussac, who found that the air in the 'Conserves d'Appert' made by this process contained no oxygen.

Here, then, the matter was left until taken up by

Schwann in 1837, and from that time till the present it has been the subject of constant experiment and research.

Schwann, with a view of removing the ambiguity which arose from the doubtful purity and questionable composition of the air contained in the vessels upon which he experimented, resolved to introduce fresh portions of air into them during the progress of the experiment. This he did by permitting the interior of his vessels to communicate with the external air by means of tubes maintained during the whole experiment at a temperature nearly equal to that of boiling mercury, a current of air being kept up by means of another tube, with an aspirator attached. On repeating this experiment, however, over a mercury-bath, side by side with similar ones to which air was admitted in its natural state, the results were not found to be free from ambiguity. Sometimes organisms made their appearance in both series of comparative experiments, sometimes in neither. The only real advance, therefore, made by Schwann towards the settlement of the question was the complete refutation of Gay-Lussac's assertion as to the part played by oxygen in the development of organisms or the process of putrefaction. These experiments were followed up by others conducted by MM. Ure and Helmholtz, whose processes were like those of Schwann; by Schultze, who passed the air through strong chemical reagents instead of the heated tubes; and by Schroeder and Dusch, who employed a filter formed of a large tube filled with cotton for the same purpose. All these observers, however, failed to get beyond the point to which the experiments of Schwann had led them. Thy all came to the same conclusion that there was 'something' in the air besides oxygen, the presence of which was necessary to the production of putrefaction; but whether this 'something' consisted of the germs of minute organisms, or of some gas or fluid, or of miasmata or what not, was a point which the respective partisans of 'heterogenesis' or 'pan-spermism' were left to dispute at their leisure.

But though, during this time, the results of investigation failed to support positively one or the other doctrine, there is no doubt that the general opinion of scientific men inclined to the latter of the two. The current of scientific progress seemed to have set uniformly in that direction; and from the time when Van Helmont gave directions for the manufacture of mice, and when people believed that maggots were spontaneously generated in putrid meat, each successive discovery—almost each successive observation—had served to confirm the view that every organism, of whatever kind, is the immediate product of a previously existing organism. Infusorial plants and animals alone now occupied the mysterious and unexplored realm of nature, in which it was still believed by some that organisms were constructed by the action of some undiscovered laws directly from matter which had formerly formed a part of some more highly endowed creature than themselves. Such was the state both of knowledge and of opinion when, in the year 1858–9, M. Pouchet of Rouen presented to the Academy a series of papers detailing observations and experiments, by which he professed to have demonstrated the existence of spontaneous generation as a fact; and the Academy, struck no doubt with the astounding nature of the discovery, proposed in 1860 as a prize subject—'Essayes, par des Expériences bien faites, de jeter un Jour nouveau sur la Question des Générations Spontanées.' It is with the investigations which have been undertaken since this time that we shall be mainly concerned in the remaining portion of this essay.

M. Pouchet, then, in his first work on this subject, appears as the professed champion of Heterogenesis, puts forward his views in a systematic and elaborate treatise, and supports them upon a basis of very extended and laborious experimentation. Having in his first three chapters given a history of the previous progress of the question, and certain general views of the metaphysics of the subject, into which it is not my present purpose to enter, he proceeds in chapter four to give an elaborate experimental disproof of the theory maintained by the 'Pan-spermists,' as he calls them. This he does somewhat as follows. There are certain points on which all observers, or nearly all, are agreed; thus, all admit that in order to the production of infusoria the conditions required are—(1) decomposible organic matter; (2) water; (3) air. And it is also generally admitted that the mixture must be maintained within certain limits of temperature, and that all organisms are destroyed by boiling the fluid in which they subsist. Now, if all the organisms produced in a given infusion are, as the 'Pan-spermists' say, the produce of 'germs,' it follows that these germs must exist either in the decomposible matter, or in the water, or in the air employed in the experiment. M. Pouchet therefore undertakes a series of experiments, with the view of systematically eliminating each of them in turn. He premises, however, that the real point at issue is the existence of germs in the air, as most even of his adversaries are ready to admit that the other two elements of the combination are easily within the management of the experimenter. It is well that this is the case; for, as I shall immediately show, M. Pouchet's position in respect to some of these points is far from being unassailable[b].

The fact upon which M. Pouchet chiefly relies as demonstrating the possibility of the production of

[b] Pouchet: Hétérogénie, p. 225.

infusoria, where no germs can have been contained in the putrefiable matter which is made the subject of the experiment, is, that they are found to appear in infusions of organic matter which have been previously submitted to an extremely high temperature. Thus he took 10 grammes each of maize, pease, lentils, and beans; and having literally, as he says, carbonized them, he put them in separate vessels, containing each 500 grammes of distilled water, and placed the vessels themselves under a bell-glass. In twenty days, at a mean temperature of 20° Cent., infuso ia were produced in all of them. In similar infusions, in which the grain used was not carbonized, animalcules higher in the scale and in greater abundance were produced in three days. In this instance M. Pouchet's experiments appear to prove his point; and as it is not a point of very cardinal importance, the proof will probably not be challenged. But in regard to the next step—that of eliminating the water as the element in organic infusions, which may carry the germs of animalcules—he is not equally successful. The experiment upon which he principally relies is one in which, after using water artificially formed by the combustion of hydrogen in air, he obtained organisms in his infusions. The method in which this artificial water was obtained was by burning a stream of hydrogen in the open air, having placed in a convenient position a metal plate, upon which the watery vapour might condense as fast as it was formed, and trickle down into a vessel prepared to receive it below. This process was continued for three days, at the end of which time sufficient water (500 grammes) was obtained for the experiments. Now, to this ingenious and painstaking process it may, I think, reasonably be objected that it is not easy to see how the air of the laboratory or other apartment in which it was carried on could be sufficiently excluded to avoid all chance of ambiguity. No doubt, when the vapour first condensed on the metal plate, the temperature would be high enough to destroy such of the germs floating in the air around as might be deposited in it; but the trickling of the water down into the vessel must have caused some slight current of air to set in the same direction, and it may fairly be doubted whether the apparatus could be, or was, so arranged that the temperature at the mouth of the receiving vessel should still be high enough to effect the same purpose. In point of fact, the water thus trickling down into the vessel below would act in its degree precisely as an aspirator intended for the express purpose of collecting germs from the surrounding air, as M. Pasteur actually did in an experiment which I shall have to notice by-and-bye. It is a pity that M. Pouchet has not introduced into the plates at the end of his work an engraving in illustration of this experiment. The much simpler process of using fresh distilled or even merely boiled water, would have been less liable to objection.

We come now to what may be called the central portion of M. Pouchet's work—namely, his elimination of the air as the source of the germs from whence infusoria are produced. It is well, before entering upon an examination of M. Pouchet's arguments, and the experiments upon which they are founded, to point out, as clearly and shortly as possible, what are the points required to be proved in order to establish either 'heterogeny' or 'pan-spermism,' and also what are the peculiar difficulties which seem to render the problem before us almost, if not altogether, insoluble. The grand difficulty, which affects both sides of the question equally, is that confessedly the 'germs' which are the matter in dispute, are incapable of being brought to the test of our senses. No magnifying powers which we yet possess can show them, nor can we ever say, whatever may be the degree of optical perfection at which our microscopes may hereafter arrive, that some particles do not exist which they may fail to show us, and that such particles may not be the germs of organic beings. Until we can see certain particles and watch them developing into vibrios or bacteriums, as we can watch an egg developing into a chicken, we can never settle this question of spontaneous generation by a direct appeal to the evidence of our senses. Besides this antecedent difficulty, which affects all investigation into the subject, there are, however, others no less formidable, which apply to each of the two views under discussion. The impassable barrier in the way of M. Pasteur and his fellow 'pan-spermists,' is the fact that they have to prove a negative. A man may say, 'I made such and such a decoction or infusion of organic matter, and placed it in such and such circumstances, and I found no organisms developed in it;' but he is always open to the objections that he examined it too soon, and the organisms were not yet produced, or too late, and they were dead and decomposed; or that the precautions taken to avoid the extraneous introduction of germs were such as to destroy the conditions required for their developement; or, finally, that the organisms being few in number, and extremely minute, might easily exist, and yet escape the acuteness of the observer—in short, that the latter is justified in asserting only that he has failed to find them, and not that they are positively absent.

The heterogenist, on the other hand, struggles against a weight of *à priori* reasoning, which renders the success of anything short of demonstration impossible. Spontaneous generation is essentially an 'old-world' creed, and having been driven from every part of the organic world which can be made the subject of strict investigation and experiment, takes a last refuge among the very lowest of all organic beings, just for the very reason that their minute size renders accurate observation of them all but impossible. Such being the difficulties which stand in the way of each of the opposite views of this perplexed question, it follows that the heterogenist has far the easier task before him of the two. If the truth be on his side, he has only to show that organisms are produced under conditions which exclude the possibility of the introduction of germs from without, and he has proved his point; *à priori* objections will, in these days, avail nothing against a positive experimental demonstration. This M. Pouchet believes that he has accomplished, and in putting forth such pretensions he places his opponent in a position of some difficulty; for, in order to establish his views, the latter must not only be able to show that his own experiments are fair ones, that is to say, are made under such conditions as do not militate against the development of organic life, but he must be also prepared to show that M. Pouchet's are inconclusive. Until both their conditions are fulfilled, however strong or general may be the *disbelief* in heterogeny among scientific men, its *disproof* is an achievement which yet remains to be accomplished.

The following then, to return from this digression, are those amongst M. Pouchet's many experiments undertaken for the purpose now in question, which have appeared to me the most worthy of remark. The well-known experiment of Schultze, in which he isolated a decoction of organic matter from the surrounding air between two Liebig's bulbs, in one of which was contained concentrated sulphuric acid, and in the other solution of potash, and no organisms appeared in the decoction though the air was renewed day by day, is one of those upon which, previously to the investigations of M. Pasteur, the opponents of heterogeny chiefly relied. To this M. Pouchet raises three somewhat formidable objections: (1) He remarks, that when the bulbs were at length removed and the air freely admitted, Schultze found that the organisms were developed in three days—i. e. somewhat sooner than they ordinarily appear in similar decoctions left open to the air from the first. (2) This experiment, even if accurate, would only prove that air transmitted through sulphuric acid is incapable of developing or sustaining life. (3) He has himself repeated the experiment, using sulphuric acid in both the bulbs and drawing the renewed air through by means of an aspirator, and in from twenty to twenty-five days he found both animal and vegetable organisms developed in his decoction.

Similarly, M. Pouchet states that he has repeated Schwann's celebrated experiment, in which he supplied fresh air to a decoction through a tube heated to redness, and found that no organisms were produced, with results the very opposite to those which Schwann obtained. Not satisfied, however, with thus destroying the credit of the evidence previously brought forward in favour of the 'pan-spermist' view, M. Pouchet has also proceeded to establish his own hypothesis by independent experiment. As examples I select the following:

Having first filled a large vessel with boiling water, M. Pouchet introduced into it oxygen and nitrogen in the proportions in which they exist in the atmosphere, in quantity sufficient to displace about two-thirds of the water, and then placed in it a small quantity of hay previously heated to the temperature of boiling water. After the lapse of a month the infusion was found to be peopled with infusoria. This experiment I have noticed for two reasons—viz. because there are two points in which it is evidently open to objection. In the first place, it is a matter of great doubt whether the temperature of 100° Cent. *in air* is sufficient to destroy any germ which might be attached to the hay. It is admitted by both the disputants in this case that such a temperature in water is fatal to all germs, and upon this point we shall have more to say hereafter; but it is not equally certain that all will admit that air of this temperature has the same effect. Then, again, the experiment was performed over a mercury-bath, and M. Pasteur believes that the mercury in all ordinary baths, as they exist in laboratories, contains germs which have fallen into it from the air. Hence the use of a mercury-bath always introduces an additional element of uncertainty.

In another experiment M. Pouchet makes the following arrangements, which we can only render intelligible by employing letters to indicate the various parts of the apparatus. He takes two bottles, one with three and the other with two necks, A and B, and two open glass vessels, C and D, and places them alternately, the three-necked bottle, A, being on the right-hand; the open vessel C, the two-necked bottle B, and lastly, the other open vessel D. These vessels are connected as follows: Through the middle neck of the bottle, A, a siphon tube passes from half-way down this bottle to nearly the bottom of the open vessel, C. From the left-hand neck of A another tube passes, reaching nearly to the bottom of the bottle B, through the right-hand neck of the latter. Again, through the left-hand neck of B a similar tube passes from half-way down B to nearly the bottom of the open vessel D. The right-hand neck of the three-necked bottle remains to be accounted for: through this passes almost to the bottom of the bottle another bent tube connected with a further tube of porcelain maintained at a red heat. The bottle A is filled with boiling water; B, with a decoction of hay, also at or near a boiling temperature; C and D contain merely a little mercury, sufficient to cover the mouths of the tubes which enter them from the bottles. The apparatus being thus arranged, a stream of air is pumped into it through the red-hot tube connected with the bottle A. The effect of this is as follows: The supply-tube going to the bottom of A, the air entering forces the boiling water through the tube in the middle neck into the open vessel C, until the fluid in A has fallen to the level at which the tube was placed. This point being reached, the air continues to enter, bubbles up through the fluid in A, and passes through the tube in its left-hand neck to the bottom of the two-necked bottle B. From thence, in a similar way, the decoction of hay in the latter bottle is forced over into the remaining open vessel, until it also falls to the level of the branch of the siphon which connects them—i.e. until B, like A, is half emptied of fluid. The apparatus was then left alone for six weeks, and the fact of its remaining air-tight through this time was proved by the fluid in all the vessels remaining at the level at which it was left when the air was originally pumped in. On examination, M. Pouchet found in the bottle which contained the decoction of hay, several tufts of penicilium and the remains of vibrios. In this case it would certainly seem as if all ambiguity were removed. The air with which alone the apparatus was supplied had to pass not only through a heated tube, but also through a body of water almost at the boiling point, before it reached the decoction in the bottle, and the mercury used can in this instance have introduced no element of uncertainty, since the siphons connecting the bottle with the open vessel must, we suppose, have been empty, and as no current of air or stream of fluid set from one into the other, it is impossible that any organism springing from germs in the latter could penetrate into the former.

Another experiment which, though not so conclusive as the last, is certainly not altogether without force, was this: M. Pouchet took eight Wolfe's bottles, each having two necks. A bent tube in the right-hand neck of the right-hand bottle communicated freely with the air by one end, the other reaching nearly to the bottom of the bottle. This first bottle was then united to the second by another bent tube, inserted only just below the neck of the first bottle, and passing almost to the bottom of the second. The rest of the series were similarly connected, the bent tube from the left-hand neck of the last bottle passing into an open vessel containing a little mercury. All the eight bottles being then filled with decoction of hay, air was drawn through the whole apparatus. It is evident in this case that the air must have been bubbled through the fluid in seven other bottles before it reached the last, yet M. Pouchet found that organisms appeared in *all* the bottles at the end of sixteen days, and that the last was just as fruitful as the first. Hence he argues, and certainly with some apparent justice, that were the germs of all these organisms floating in the air, they would certainly have been deposited in some of the earlier bottles, most of them in the first, and the last ought to have been comparatively, if not entirely, free from any manifestations of life.

The last experiment of M. Pouchet which we propose here to mention, is one in which he took two similar glass vessels, and filled them with equal portions of the same infusion, or decoction (for the experiment was tried with each) of hay; one of these he left in the open air, the other was covered with a plate of glass, and placed under a bell-glass in a dish of water. In both glasses, after eight days, the same organisms were found in equal profusion. Certainly, as M. Pouchet remarks, on the supposition that the germs of these organisms were supplied by the air, it seems strange that the very limited quantity of air in contact with the decoction in the first vessel should have contained sufficient germs to people it in the same time as fully as that in the second, which was freely exposed to the atmosphere of the room.

By a large series of experiments, then, of which the above are but a small selection, M. Pouchet considers that he has proved that whereas, when a putrefiable body, air, and water are placed in contact, within a certain range of temperature, certain low organisms are produced, this production does not come from germs conveyed in either of the three elements of the combination, and that, consequently, it must be held that the organisms in question are generated spontaneously, and not produced, as are all other creatures, *ab ovo.*

We leave all criticism upon these experiments until we have given a slight sketch of those which have since been performed, both by M. Pasteur, in opposition to this view, and by M. Pouchet himself and others in its defence. This sketch we shall be compelled to make merely in outline, as the space which it would occupy, if given in detail, would leave no room within the dimensions of any ordinary article to estimate the comparative value of the experiments on both sides, and determine the actual position of the question at issue.

M. Pasteur tells us that he was led to grapple with this question as an almost necessary consequence of his researches upon the kindred subject of fermentation. His study of that subject had led him to the conclusion that all ferments, properly so called, are really organic beings, and that fermentation, instead of being an action set up by the contact of albuminous matter in a certain state of decomposition with a fermentible body, is really an action produced by the life, and growth, and reproduction of certain low kinds of organism. The mode of production of these low organisms in the various forms of fermentation became naturally the next subject of investigation. Are they produced immediately by the decomposition going on, or mediately by the development of germs, for which the fermentible matter forms a suitable element, and, if so, whence do the germs proceed? This was the question which presented itself at this stage of the inquiry, and thus the experimenter found himself face to face with the problem of spontaneous generation. With him, as with M. Pouchet, the main points requiring to be determined have reference to the air. Does the air, as a fact, contain the germs of living beings? If so, can these be excluded from the infusions which are subjected to experiment? And will such exclusion prevent all development of life in them, while it can be shown that the other conditions of the experiment are such as to be favourable to life, and the addition of germs only is required to enable it to break forth in abundant quantity?

An objection constantly urged against the views supported by M. Pasteur is, that no one has ever *seen* the germs in the air, which are assumed to play so important a part in the processes of putrification and fermentation. This M. Pasteur has triumphantly met by means of the following very ingenious experiment: he obtains a tube in the shape of the letter T having a stopcock in each of its limbs; this being suspended in a horizontal position, the upper end of the cross-piece is attached to a cistern, and, by means of the cock, it becomes possible to regulate a stream of water passed through the cross-piece of the T tube to any quantity that may be desirable. By this means a current of air is constantly drawn at a regulated pace through the long limb of the T. The apparatus, in fact, becomes an aspirator capable of acting as long as the cistern contains any water. The long limb of the T is then connected with a piece of glass tube, passing through a hole in a shutter, and open to the air outside. In this glass tube a plug of gun-cotton is placed, and the apparatus set in action. Thus any given quantity of the external air can be drawn through the tube containing the gun-cotton, and literally filtered of whatever it contains. This process having continued for a considerable time, M. Pasteur withdrew the plug of gun-cotton, and placed it in a precipitate glass; then dissolving the cotton in a mixture of alcohol and æther, he left the dust which would not dissolve to collect as a precipitate at the bottom of the glass, to be afterwards examined microscopically. By this process he was able to discover that in dust collected in twenty-four hours there were contained a considerable number of small, round, or oval bodies, quite undistinguishable from the spores of minute plants and the ova of infusoria, though the number of them differed greatly according to variations in the temperature, the moisture and the stillness of the air, and the distance above the soil at which the air-filter was placed. The experimenter was, moreover, enabled by this method to preserve the dust collected for further experiment in a way which led, as we shall presently see, to important results.

This point, then, being established, M. Pasteur proceeded next to repeat Schwann's experiment with air passed through a heated tube, which he did in the following manner. Having taken a flask, and bent the neck almost horizontally, he placed in it 100 parts of water, 10 of sugar, and from 0·2 to 0·7 of albuminoïd and mineral matter obtained from yeast. The neck of the flask was then drawn out, so as to be capable of being sealed, and connected with a platinum tube passing through a furnace, and maintained during the experiment at a red heat. The contents of the flask were now boiled for two or three minutes, and then suffered to cool completely. The flask was then refilled with ordinary air, all of which, however, had been raised to a high temperature. Finally, the neck of the flask was sealed, and it was put aside in a temperature of 30 cent. (86° Fah.). No organism whatever appeared in this decoction, or in any of a similar character. M. Pasteur's words are as follow: 'J'affirme avec la plus parfaite sincérité que jamais il ne m'est arrivé d'avoir une seule expérience, disposée comme je viens de le dire, qui m'ait donné un résultat douteux.' Other experiments, performed with equal care, had, however, led to diverse results; but in these either a mercury bath had been used, or the liquid made the subject of the experiment was milk; and M. Pasteur informs us further on in his researches that he always found the results unsatisfactory under those circumstances, and is led to believe that mercury baths, remaining exposed as they do to the air, become themselves the vehicles of germs, and that milk, in common with other alkaline fluids, in some way protects the germs contained in it, so that a greater heat or a longer exposure to the boiling temperature is required for their destruction.

To complete M. Pasteur's chain of proof, one link only is now required. He has shown us that germs, or bodies not distinguishable from germs, do exist in the atmosphere, that they can be collected, looked at, preserved. He has shown us also that if means be taken to supply a decoction of organic matter with such air only as has been submitted to a degree of heat capable of destroying all germs contained therein, no production of life takes place in it. In his fourth chapter, he proceeds to supply this link, by relating how he has been able to take a decoction upon which the last experiment had been performed, and which had remained for a length of time unchanged, to sow within it some of the dust and germs obtained by his ingenious air-filter; and how within a very few days the hitherto barren fluid has become fruitful and abounded in simple forms of life.

By adopting an apparatus which I fear it would be impossible to make intelligible to my readers by mere verbal description, but which is a modification of that last described, M. Pasteur has been able, without admitting any air except such as had been previously heated, to break off the neck of one of his flasks, and to introduce a small tube of glass, containing a bit of the cotton from an air-filter, and once more to seal up the neck as before. The constant result has been, that in from thirty-six to forty-eight hours organisms have been developed in the flask, in about the same time, that is, as would have been required to produce them in the same fluid if left freely open to the air. The same result followed if asbestos was used instead of cotton to form the filter, thus removing any ambiguity that might be supposed to be introduced by the use of an organic body for this purpose. For this series of experiments, as for the last, M. Pasteur claims invariable success and absolute infallibility.

'Il ne m'est pas arrivé une seule fois,' he says, 'de voir réussir les expériences *à blanc*, comme je n'ai jamais vu l'ensemmencement des poussières ne pas fournir des productions organisées. . . . . En présence de tels résultats, confirmés et agrandis par ceux des chapitres suivants, je regarde, comme mathématiquement démontré, que toutes les productions organisées qui se forment à l'air ordinaire dans de l'eau sucrée albumineuse, préalablement portée à l'ébullition, ont pour origine les particules solides qui sont en suspension dans l'air . . . . ces corpuscules sont dont les germes fécondes de ces productions.'

M. Pasteur further proceeds to corroborate his views by several series of experiments—all *invariably* successful—by means of which he establishes, very much to his satisfaction, the following propositions : (1) That the conclusion above drawn from the case of yeast can be extended to various other organic fluids, as urine, milk, &c; but that when, as in the case of milk, the fluid employed is alkaline, either a slightly higher temperature than that of boiling water, or a longer exposure to that temperature than was required in the other experiments, was necessary, in order to the destruction of all the germs contained in the fluid. (2) That if an infusion similar to those above used be simply placed in a flask with a long drawn-out and bent neck, and it be then boiled until the steam passes off freely by the neck, it may be left with the neck unsealed, and will even then remain quite unchanged. There will, of course, in this case, be no current of air setting into the vessel, and what few germs enter the neck will be arrested in the curves of it. If, on the other hand, after it has thus remained unchanged for many weeks, the neck be cut off, the decoction will become peopled in from twenty-four to forty-eight hours. (3) That germs do not pervade the atmosphere equally, but, while plentiful in common localities, are almost or altogether absent from others. Thus, decoctions enclosed with due precautions in deep cellars, or on the tops of mountains, such as the Jura or Montanvert, were found frequently to remain quite unchanged, exactly as do those which are supplied with air artificially deprived of its germs.

(4) Finally, that the mercury bath introduces, as we remarked above, a new element of uncertainty into all experiments on this subject in which it is employed. He further finds that he is able to produce organisms in a mixture quite free from all albuminous constituents, and containing only water, sugar-candy, tartrate of ammonia, and a little ashes of yeast—a fact proving, as he considers, that the part played by the albuminous matter in an ordinary organic infusion is simply that of an aliment for the growing and multiplying organisms in it. M. Pouchet's reply to this very elaborate and ingenious monograph of M. Pasteur is certainly not a success. It appears in the 'Ann. des Sciences Naturelles' for 1862, p. 277. Those of our readers who care to refer to it will find that it contains much more vituperation than argument, and that what arguments are to be found in it have been, for the most part, met by M. Pasteur by anticipation. There is, however, one curious experiment therein mentioned which requires some discussion and explanation. M. Pouchet finds (p. 297) that if the same quantity of the same fluid be placed in two vessels, one deep and narrow, the other shallow and wide, the more highly-organized and larger infusoria will appear in the former vessel, but not in the latter; and he argues, if the germs dropped into both vessels from the surrounding air,

you ought to have a much larger number in the wide vessel than in the narrow one. But the difficulty here is more apparent than real. In each case the organisms first to appear are the lowest of all—viz. the monads, bacteriums, and vibrios; and it is only after the appearance, the life, death, and decay of these, and after a thick stratum of their dead bodies has been formed at the top of the narrow vessel, that the more highly-organized creatures are produced. It is this stratum of bodies of vibrios, &c. which M. Pouchet calls the proligerous membrane or stroma, and in which he affirms that the spontaneous production of the ciliated infusoria takes place. Now, it is evident that the life and death of myriads of minute organisms cannot have gone on without working some change in the constitution of the fluid in which they existed; and it is surely less improbable that that change should be one which should fit it for the maintenance of creatures more advanced in the scale of creation than that it should be such as actually to give rise to the formation of these creatures themselves. It is therefore most reasonable to believe that in the experiment just referred to the ova of the highly-organized animalcules do, of course, fall in greater numbers into the wide and shallow vessel than into the deep and narrow one, but finding a fluid in the former not suited to their development, remain inert and unproductive, and thus escape unnoticed.

Another step has since this time been made by M. Pasteur in investigating the natural history of these minute organisms, which, if it should be corroborated by the evidence of other observers, will certainly go far to explain their appearance, without compelling us to have recourse to M. Pouchet's theory of a proligerous membrane formed by the bodies of bacteriums and vibrios, in which the more complex organisms are synthetically constructed. M. Pasteur takes a mixture, consisting of tartrate of lime and small quantities of phosphate of ammonia, and other alkaline and earthy phosphates, in distilled water. From this mixture, by a process which I need not detail, he draws off and excludes all free oxygen or air. He sows in it a few infusoria taken from some spontaneously fermenting tartrate of lime, and he finds that the creatures multiply in the mixture till the whole of the tartrate of lime has disappeared. Thus he is led to the conclusion that these are capable of living, and growing, and multiplying, without any contact with oxygen, at least in its uncombined state, and further investigations have convinced him that the natural order of the phenomena which take place in an organic infusion exposed to the air is somewhat as follows: The first organisms to be developed in it are certain low infusoria, such as bacteria. These pervade the whole fluid, consume whatever free oxygen it contains, then die for lack of this very gas, and give place to other equally low or lower creatures which live without oxygen, and, indeed, perish if brought in contact with it. These occupy the lower strata of the fluid, while those near the surface are inhabited by organisms which consume the oxygen of the air above, and thus protect the others from its destructive contact. It may be mentioned by the way, that it is these newly-discovered creatures, thus capable of existing without oxygen, which M. Pasteur looks upon as forming the real agents in fermentation, thus reducing this process to one of the growth and nutrition of living beings, and entirely exploding the old views of it as a catalytic or contact action, whatever that may mean, or a process to which the presence of albuminous matter in a certain state of decomposition is absolutely necessary.

Since this very important announcement of M. Pasteur, the contributions made by the various disputants in France to the literature of the subject before us have not been calculated either to throw much new light on the matters in dispute, or to enhance very greatly the dignity of the learned men engaged upon it. In a former part of this article I mentioned the fact that M. Pasteur had established to his own satisfaction that a vessel containing a highly alterable decoction of organic matters, sealed up *in vacuo*, may be opened, filled with air and resealed, in certain localities, such as the tops of high mountains, and yet the contents remain in many cases unaltered. Thus he found that among twenty vessels so treated on the Jura, organisms were developed in five only, and from this he concluded that the property of the air which gives rise to such phenomena is not always to be found in it; in other words, that they are produced not by air merely as such, but by something in the air which is not equally distributed through it, as might, for instance, be supposed to be the case with germs[c]. These experiments three of his opponents in conjunction—viz. MM. Pouchet, Joly, and Musset—repeated in the course of last summer, employing eight vessels only, and opening them at considerably greater elevation on the Pyrenees than that at which M. Pasteur's experiments were performed. These observers, as might be expected, obtained results completely contrary to those of M. Pasteur[d], finding organisms developed in every one of their vessels, although they professed to have observed all the precautions recommended by their opponent[e]. They communicated their success to the Academy, and from that time the dispute has almost taken the

[c] Op. cit., p. 77. [d] Comptes Rendus, Sept. 21, 1863.
[e] Comptes Rendus, Nov. 2, 1863.

form of a personal squabble. M. Pasteur replied by some very minute criticisms upon his adversaries' method of conducting their experiments (which, however, received the countenance of several eminent physiologists, among them M. Milne-Edwards), and by a reiteration of his previous assertions. The chief points of M. Pasteur's criticism were, that the experimenters had not made a sufficient number of trials, and that in opening the necks of their vessels on the mountain top they had employed a file instead of a pair of pincers with long handles. The latter objection is so very minute, that, but for M. Milne-Edwards' notice of it, it might almost have been disregarded; but even allowing it some weight in itself, we think it must lose it when taken in connexion with the numerical differences between the results. Thus it is conceivable that the use of the file might have spoilt one or two experiments, but more effect than this we cannot reasonably attribute to it. The other objection may at first sight seem to have more force, but in reality it has but little, for a simple calculation of chances is sufficient to show that if, in M. Pasteur's experiments, five vessels only out of twenty were found to contain germs, it is in the last degree improbable that eight in succession should follow the exceptional instances, and not the rule.

M. Pouchet has, however, not contented himself with attacks made desultorily from time to time, in reply to M. Pasteur's communications to the Academy, but has in the present year published an elaborate essay containing his more recent experiments on the whole subject of spontaneous generation. This essay has derived some additional interest from the fact that it was originally sent in competition for the prize offered to the Academy for researches on this subject, but was withdrawn before the day of decision (as was also that of MM. Joly and Musset), because, as the author states, the commissioners appointed to adjudge the prize were all previously committed to the doctrine of his opponent. I cannot pretend, in an essay which treats of the whole question generally, to review thoroughly either of the two systematic treatises of M. Pouchet[f], but I must in fairness say of them, that they deserve more attention by far than they seem at present to have received at the hands of English physiologists, and that no one can fairly judge of the state of the question at issue unless he has given them a thorough consideration. Speaking for the moment only of the two principal controversialists, I think that the experiments of M. Pasteur have hitherto been considered as more exact and convincing than those of his rival; indeed, the weak point of the 'Hétérogénie' throughout has

[f] Pouchet: Nouvelles Expériences, p. 14.

lain in an insufficient exclusion of disturbing causes. This defect in the defences of his opponent has not escaped the vigilance of M. Pasteur, and accordingly no weakness of the kind can be charged upon his own experiments. They, on the contrary, are as rigid and as exact as if the problem with which he had to deal was of the simplest kind. Nevertheless, I am by no means convinced that M. Pouchet's objections to them are not somewhat formidable. He says in effect, life and organization are too delicate, too subtle, to be handled so roughly as you propose; and if, after torturing your substances as you do, and submitting them to all imaginable unnatural conditions, you find no signs of life in your vessels, it is at least as probably because you have destroyed its necessary conditions as because you have excluded those *quasi*-metaphysical 'germs' which you maintain are diffused throughout the atmosphere.

Arguments of this kind M. Pouchet uses constantly. In themselves, indeed, they do not go far, inasmuch as his conditions of life are as much his own speciality as are his opponent's germs, and he never fails, moreover, to enforce them in an effective if somewhat Hibernian manner by relating experiments of his own, in which, under conditions almost exactly similar to those employed by M. Pasteur, he has obtained precisely opposite results. And it is to the experiments, after all, and not to the argumentation, that we must look for the final decision of the question, if, indeed, it be capable of decision at all. In experimental evidence, accordingly, this last work of M. Pouchet is remarkably rich. One or two of the experiments I cite, but they will serve for examples only, and I must refer my readers to the Appendix to M. Pouchet's work for the rest.

In the first place, there is a modification of Schultze's well-known experiment, to which we have already referred. This is performed by M. Pouchet in the following very simple and apparently satisfactory manner:—He places his infusion in a flask, boils it for a considerable time, then immediately adjusts to its neck a funnel with a syphon tube attached, in the curve of which are several bulbs containing concentrated sulphuric acid. The infusion is then boiled again for five minutes, so that the steam bubbles through the sulphuric acid; and the whole, after being cooled slowly, is set aside. By this process it will be seen that the air within the flask is only renewed to the extent necessitated by any changes of temperature to which it may be exposed, and that whatever renewal actually takes place does so only by the passage of the air through the acid; yet, nevertheless, M. Pouchet assures us that, though he has tried it with a variety of substances and very frequently, he has constantly found organisms developed in the infusions. This experiment is the more worthy of notice inasmuch as Schultze's investigations have attracted great attention, and were believed by many to have settled the question against heterogeny altogether. Schwann's early experiments with heated air have also been repeated by M. Pouchet with quite opposite results to those which have followed them in the hands of M. Pasteur and others; but in this case he has introduced a modification which, in our opinion, completely vitiates the experiment. Instead of simply boiling his substance in the water contained in his apparatus, M. Pouchet has submitted it first to a dry heat of 200° (cent.), and then contrived, by placing it in the neck of the flask, lying horizontally, to expose it to the steam of the water while boiling, and immerse it therein only after the boiling has ceased. The object of this arrangement appears to be to prevent the decoction being submitted to the action of the distilled water dropping from the sides of the vessel during boiling—an action which M. Pouchet believes to hinder, in some not very intelligible manner, the development of organisms in it. The arrangement, however, as I have said, vitiates the experiment, inasmuch as the degree of *dry* heat which organisms in some forms can endure without destruction is a matter still in doubt; nay, it is one intimately connected with the question of spontaneous generation, and almost as hotly disputed; one, therefore, which the experimenter should be especially careful not to import into his investigations. M. Pouchet, no doubt[g], has settled this question by his own experiments to his own satisfaction; but so long as it is not looked upon as settled by the scientific world in general, to assume it for the purpose of deciding another disputed point is merely to expose oneself to an unnecessary defeat.

I have room for but one more of M. Pouchet's new experiments; and I select one which from its simplicity is easily repeated, and yet which, if exact, is of no small importance. M. Pouchet takes a quantity of flax, soaks it in water for six hours, then filters it, and divides the clear fluid coming from the filter into two parts. Of these one is placed in a flask and hermetically sealed; the other is put into a narrowish upright vessel, and enclosed under a bell-glass of the same capacity as the flask used for the other portion. The bell-glass dips into some mercury previously heated to 160° (cent.). Thus the two portions of the fluid are placed without any precautions in equal quantities of air, and all change of atmosphere in both cases equally precluded. After eight days the two fluids were examined, and the organisms found to be quite

[g] Nouvelles Expériences, p. 39 et seq.

unlike in the two. In the flask were only the lowest kinds of infusoria—monads, bacteriums, and vibrios. In the portion under the bell-glass were some 'spontaneous eggs' and myriads of ciliated animalcules, such as colpodas, &c. If this experiment is accurate, it is certainly difficult to see how the germs of the one kind of creatures should have entered or become developed in the one vessel and entirely different kinds in the other.

The phrase, somewhat new to the English reader, 'spontaneous eggs' (*œufs spontanées*), which I have just quoted from M. Pouchet, may serve to suggest that I have left quite unnoticed hitherto what may be called the positive side of the heterogenist's argument. I have noticed the objections to the reasoning of their adversaries, and have quoted some of the experiments which have led them to such opposite results. It remains to say a very few words upon the observations and experiments by which they propose to establish, not the probability, but the actual existence of spontaneous generation, as a phenomenon which is capable of being observed. M. Pouchet gives a distinct account of the various steps in the process of the spontaneous production of animalcules of the genus *Paramecium*, as observed by himself in an infusion of darnel (*Lolium temulentum*)[h]. The grass was steeped in water for one

[h] Nouvelles Expériences, p. 111 et seq.

hour, and the water then filtered off and put aside. On the next day a number of monads appeared on the surface of the filtered fluid. These were nearly all dead on the following day, and their bodies formed a thin granular scum on the surface. On the third day there began to appear some of the spontaneous eggs above mentioned in various stages of development. They appeared at first as little greenish-yellow masses, formed of some of the granules of the scum. The central granules were larger and closer together than the rest, and the outside ones more delicate and less closely packed, forming, as the mass gradually took a spheroidal form, a kind of *Zona pellucida.* This was more distinct in other specimens, and then the vitellus was seen in gyration. On the fourth day almost all the eggs were perfectly formed, and on the fifth perfect parameciums appeared. The changes seen were in fact exactly the same as those observed by M. Pouchet himself as taking place in the development of mollusca, and by other naturalists in various low organisms. In the lowest infusoria, such as the Bacteriums, all these changes cannot be followed; but they are seen, upon close observation of an infusion, to appear *en masse* in a way quite inconsistent with the notion of their being produced from eggs dropping accidentally from the surrounding air. The surface of a fluid in fermentation is seen covered with an almost imperceptible mucous film. In this film there appear all at once a number of pale motionless lines, nearly parallel to one another, and of the form and size of bacteriums, and these after some hours become living and active infusoria [i]. These results, though brought out by the original researches of M. Pouchet, do not rest upon his authority alone. MM. Joly and Musset, of Toulouse [k], and Professor Schaafhausen [l], of Bonn, have arrived at very similar conclusions quite lately; and Professor Mantegazza, of Pavia, [m] as long ago as 1852, gave a most interesting and striking account of an observation made by him upon the development of bacteriums, in almost the same terms as those which we have just borrowed from M. Pouchet. Mantegazza spent sixteen consecutive hours in observing these phenomena with the microscope.

In concluding this portion of the subject, I will mention but two more experiments. They appear to be well established; and if they should turn out correct, it is at least not easy to see how they can be reconciled with the Pan-spermist theory. The

[i] Nouvelles Expériences, p. 116.

[k] Joly and Musset: Comptes Rendus, 1860, vol. I.

[l] Schaafhausen: Comptes Rendus, 1862, vol. liv.

[m] Cosmos, 1863, p. 630.

first is an experiment of M. Pouchet's own.[n] He takes a flask, plunges it into a vat of wort, which has been boiling for five hours; and after having kept it there for ten minutes, seals it while under the surface, and brings it out again. It is admitted on all hands that all organisms are killed by a moist heat of 100° (cent.); yet in this case, after eight days, a considerable quantity of yeast plant was developed in the flask. The other is one which he quotes from Treviranus and others, and has often verified himself. Three beakers are taken: in one is placed cyder, in a second urine, in the third a mixture of these two fluids. The vegetation in the first differed from that in the second, and that in the third was quite distinct from both.

I have now finished, not indeed a summary, but a very rough and imperfect sketch of the evidence which has been adduced on both sides of this vast, obscure, and almost hopeless but still interesting and highly important question. It remains in the last place to endeavour to estimate the value of the evidence, and to show, if possible, what is the actual position in which the controversy stands. Where some of the greatest names known to science are to be found ranged on opposite sides it is not for us to attempt a decision; nevertheless, as has been well said by a distinguished writer on a very

[n] Nouvelles Expériences, pp. 126, 127.

different subject, 'the attempt to decide questions in philosophy (or science) by polling authorities on either side would be interminable and hopeless,' accordingly I do not attempt either to poll authorities or to decide the point in dispute between them, but content myself with an attempt to lay before my readers as clearly, as impartially, and, above all, as shortly as possible, the present position of the question. To do justice to the subject would require a volume rather than an essay.

On the one hand, we have the conclusions drawn by M. Pasteur from his own experiments, forming a complete chain of facts with no link missing, and, if they fairly represent the actual state of our knowledge, justifying M. Flourens, Mr. Huxley, and others in their assertions that the whole is definitively decided by them, M. Pasteur finds: (1) That he can actually show certain bodies collected from the air which have all the appearance of being the eggs of minute creatures. (2) That when proper means are taken to admit into a putrescible decoction such air only as has been passed through a heated tube, such decoctions may be kept free from all signs of organic life for an indefinite time, and that they may even be left freely exposed to the ordinary air, provided that it can reach them only through a long, narrow, and crooked tube, in the bends of which all the ova contained in it are deposited. (3) That when the supposed ova collected from the air are sown in decoctions previously kept as above described without change, organisms are produced within a given number of hours. (4) That under certain circumstances, as, e.g., at the tops of mountains and in deep cellars, air may be obtained in considerable quantities which is as incapable of producing change in the most highly alterable fluids as is that which has been passed through a heated tube. Besides these main propositions there are a number of other conclusions to which M. Pasteur has been led by his researches, which are of the highest interest, and which are related inseparably, though only indirectly, to the subject immediately before us—viz. (5) That all putrefaction, as also all fermentation, properly so-called, has as its efficient cause the life, growth, and reproduction of some kind of infusorial organism, without which no such action can proceed; and, (6) That so far is oxygen from being the cause or even a necessary condition of such actions, that many of the infusoria on whose existence they really depend pass their lives without oxygen, and are even killed by its presence. The order of phenomena in the decomposition of many fluids, according to M. Pasteur, is therefore as follows:—The infusion being exposed to the air, and having air dissolved in it, derives first some ova from the surrounding air; then certain kinds of infusoria are developed in it. These consume the oxygen contained in the fluid, and all die except those at the surface, and are succeeded by another kind which live without oxygen, being protected from its approach by the active oxygen-breathing infusoria at the top and the film formed by the bodies of their predecessors. Such are M. Pasteur's chief generalizations from his experiments, and we are bound to admit that, if the experiments are accurate, they are not wider than the grounds upon which he goes will bear. But it is difficult to exaggerate their importance. Not only do they claim to settle for ever the vexed question of heterogeny, but also to revolutionize completely the whole theory of fermentation, and, in some measure, also all our ideas of the phenomena of life. That they should be subjected therefore to the most rigid scrutiny is no more than is to be expected, and no more than is right.

Accordingly, on the other hand, we find that M. Pouchet disputes almost every single proposition of M. Pasteur, and sets to work to establish positively the very contrary doctrine. Even if M. Pouchet stood alone, his views would deserve more consideration than they have met with at the hands of some distinguished men, both in his own country and also here. A physiologist whose work is spoken of by Professor Owen in the highest possible terms is at least not a person to be ignored. But, as a matter of fact, M. Pouchet does not stand alone. When such men as MM. Joly and Musset at Toulouse, Professors Mantegazza at Pavia, Wyman at Cambridge, U. S., and Schaafhausen at Bonn, all working independently of one another, all in a greater or less degree lend their support to his views, and all controvert those of M. Pasteur, it is surely idle to speak of the question in dispute as finally settled by the experiments of the latter. On the first point—namely, that of the discovery of ova in the air—while other observers find them but very few and far between, M. Pasteur himself, as far as appears from his plate and his description, does not discover a sufficiently large number to play the very important part which he assigns to them, except on the supposition that the reproduction of these creatures is much more rapid than we have any reason to believe it to be. Then as to the cardinal proposition, that no infusoria are produced in putrescible infusions supplied only with air previously heated to 100°, it must be remembered that M. Pasteur is the only observer who, having tried this fairly and extensively, is able to state that he has been always successful, and that, on the other hand, no less than five thoroughly competent observers have arrived at contrary conclusions; and it is a question which is open to every one to answer as he pleases, whether it is more probable that M. Pasteur should be mistaken, or that the whole of those other five physiologists should be incapable of carrying on an investigation requiring care and accuracy. There are two considerations connected with this part of the subject which ought not to be disregarded. In the first place, it will be admitted by all that, whatever be the efficient cause of the life of these low organisms now under investigation, it is produced in close glass vessels under considerable disadvantages, more especially when the fluid in which it is to live has been boiled. Hence we should naturally expect that it would be scanty in quantity, as well as low in the scale of existence, and therefore very easily overlooked in a microscopic examination. Again, it should be remembered that the organisms produced under such adverse circumstances are also very short-lived, and are not succeeded by others, as is the case under natural conditions; and I speak from my own experience when I say that if an observer in such cases judges by the fluid in his vessels becoming turbid, he will often reckon a specimen as altogether sterile in which a careful microscopic examination would detect a few organisms. They would, moreover, more easily escape notice if the decoction in which they exist be left, as was the case with some of M. Pasteur's, until they had long since died and fallen to the bottom of the vessel as a granular precipitate.

It is the point which we have placed third in our summary which has perhaps gained more proselytes for M. Pasteur than any or all the others—the statement, namely, that he has been able to sow particles of dust collected from the air in decoctions preserved for a long time unchanged, and in a few days organisms have been developed in them. This statement M. Pouchet meets very summarily, but not very convincingly, by affirming that M. Pasteur really sowed nothing, and that what he saw spring up was simply that which is ordinarily produced by spontaneous generation in such fluids as he used for his experiments. This assertion is, I think, hardly a fair one; for the productions which M. Pasteur enumerates, and some of which he figures as produced by this means, are somewhat too various to be accounted for in this way. There is, however, another objection to which these experiments are open—viz. this M. Pasteur, in the somewhat elaborate apparatus which he uses for these experiments, employs a T-shaped tube with three stop-cocks, and in order to introduce his bits of cotton with the ova attached into the previously sealed vessels, he is obliged to exhaust this T-shaped tube several times in succession by means of an airpump. M. Pouchet is quite justified in his remark, that had the heterogenists ventured on so inexact a proceeding they would inevitably have been charged with inaccuracy in the experiment. It certainly seems to us that such a proceeding as that of opening the glass globes in which the decoctions had been preserved unchanged, and thus renewing the air contained within them, even although the newly-admitted air has also been heated before admission, at least introduces an element of uncertainty into the experiment, and may easily be supposed to alter its condition in other ways than the one intended by the experimenter. With regard to the unfruitfulness of the air as found upon high mountains and in caverns, it is sufficient to remark that, as has been already shown, the accuracy of M. Pasteur's facts is disputed by his three principal opponents.

It is thus seen that the experimental evidence on the whole subject of the production of infusoria is unsatisfactory and conflicting. It is also incomplete, and is especially incomplete on the side of the 'Pan-spermists,' for M. Pasteur has entirely failed to show anything like a sufficient number of ova in the air to produce the results which he attributes to them, and has not sufficiently accounted for the succession of different kinds of organisms which take place in the same fluid. It is difficult to see why, the ova of both being supplied by the surrounding atmosphere, a quantity of bacteriums should be developed and die first, and a generation of parameciums follow them; whereas there is some show at least of analogy to the other phenomena of nature in the gradual advance in the type of living beings produced, on the hypothesis of their being spontaneously generated. The experiments of the chief supporters of both views are, we believe, shortly to be repeated before a commission of the Academy of Sciences, and it is to be presumed that so much of the question as can be decided by experiment will by this means be cleared up. It is unfortunate, however, that several of the members of this commission should be persons who are already committed to one of the doctrines which are to be brought before them for judgment. I do not for a moment doubt the perfect fairness and impartiality of these distinguished individuals, but at the same time their selection as judges on this occasion is unfortunate, inasmuch as it is always an invidious thing for any man to have to sit in judgment on the conduct of another who has expressly come forward as the opponent of a doctrine of which the judge is an avowed supporter.

I repeat that the experimental evidence is at present unsatisfactory, and if the balance inclines to either side it is rather in favour of the heterogenists, inasmuch as their direct observations of the production and development of 'spontaneous eggs' have not been disproved, or successfully controverted. If any one therefore desires to form a judgment upon the question in its present stage, he will be driven to do so upon *à priori* grounds, or on merely analogical reasoning. And even here there is more to be said in favour of heterogeny than many are aware of. Even Professor Huxley, who considers M. Pasteur's experiments to have settled the questions against it, as a fact, does not look upon it as impossible *à priori* [o]. It has been said—and it is a view of the subject which is sure to be attractive to many minds—that the belief in spontaneous generation varies directly with our ignorance of the real physiology of reproduction and development. Thus the ancients believed in the spontaneous generation of rats and mice. This belief was speedily dissipated by the advance of knowledge; but still, till the time of Redi, the maggots in putrid meat were universally supposed to be immediate products of decomposition, and so from that time downwards the belief has attached always to any class of organism of the real history of which we are ignorant, until at last it has become confined exclusively to the lowest, most obscure, and least known of all classes of living beings, and is probably as false in this last case as in those which have gone before. Now the value of such reasoning as this is really

[o] See his Lectures to Working Men (1862), p. 71 et seq.

very small. It would apply with just as much force to a number of facts which are now universally admitted. Take, for instance, the phenomena of reproduction by fission or by budding. If we could suppose some naturalist now for the first time to announce this as taking place in some one particular class of organism, he would most likely be at once told that, as all analogy was in favour of sexual reproduction, his observations must be erroneous, and his conclusion a mistake. But as we now know that nature has a line—not well defined and sharp and abrupt perhaps, but, like all the lines of demarcation in nature, indistinct and sometimes hardly traceable, below which reproduction does take place in this to us abnormal manner—why should there not be another line fixed far lower in the scale of creation, below which creatures are formed piece by piece, as M. Pouchet says, out of particles of dead matter, in the way which he and Schaafhausen and Mantegazza tell us that they have themselves witnessed?

ESSAY IV.

On the Production of Organisms in Closed Vessels.

The experiments which form the first series described in this paper are twenty in number, and were performed during the summer of 1863. The substances used were in ten experiments milk, and in ten, fragments of meat and water. These were in all cases placed in a bulb of glass about 2½ inches in diameter, and having two narrow and long necks. The experiments are divided into five series of four experiments each. In one series the bulbs were filled with air previously passed through a porcelain tube containing fragments of pumice-stone and heated to vivid redness in a furnace. In the others they were filled respectively with carbonic acid, hydrogen, oxygen, and nitrogen gases. In each series two experiments were made with milk, and two with meat; and each substance was boiled in one case, and not in the other. The joints of the apparatus were formed either by means of non-vulcanized caoutchouc tubing, or india-rubber corks previously boiled in a solution of potash; and in every case, at the end of the experiment, the necks of the bulb were sealed by the lamp. The time of boiling such of the substances as were boiled varied from five to twenty minutes, and the boiling took place in the bulbs, and with the stream of gas or air still passing through. The substances were always allowed to cool in the same stream of gas before the bulbs were sealed. The microscopic examination of the contents of the bulbs took place at various times, from three to four months after their enclosure.

In every case but one in which the substance had not been boiled low organisms were found, apparently irrespective of the kind of gas in which they had to exist. The case in which they were not seen was that of the meat enclosed in a bulb filled with nitrogen. This bulb burst apparently spontaneously, and its doing so may be looked upon as a proof that in it also some change had taken place most likely connected with the development of organic life. Where the substances had been boiled, the results were as follows:—

1. In the carbonic acid experiments, no sign of life.

2. In the hydrogen experiments, no sign of life.

3. In the heated air experiments, organisms found in both cases.

4. In the oxygen experiments, organisms found

in the experiments with milk. The bulb containing the oxygen and meat burst spontaneously, therefore probably contained organisms.

5. In the nitrogen experiments, organisms were found where meat was used. None where milk was used.

No definite conclusion can be drawn from so limited a range of experiments; but it is worthy of remark that organisms were found here under the precise circumstances in which M. Pasteur states that they cannot and do not exist. The very abnormal conditions under which some of these so-called organisms are found, would render it doubtful whether Bacteriums, Vibrios, &c. ought to be considered as independent organisms in any higher sense than are white blood-corpuscles, pollen-grains, mucus-corpuscles, or spermatozoa.

The further researches, an account of which is contained in the remaining part of this paper, are in continuation of those which, through the kindness of Professor Phillips, I had the honour of communicating to the Royal Society in May last, and of which an abstract appeared in the 'Proceedings' for June 16, 1864. The former series of experiments did not pretend to be, in any respect, complete. Those which I am now about to describe will, I hope, be considered to be more so in regard to one main subject to the inquiry; but they also suggest further researches upon some collateral branches of it, which I hope to find time and opportunity to prosecute.

In the former series I experimented with animal substances mixed with water and enclosed in glass bulbs in atmospheres either of common air passed through red-hot tubes or of various gases, and the result at which I arrived was that where oxygen was present organisms of a low type were produced, but not so where that gas was not present. Thus, whatever the gas employed, where the substance was not boiled, the organisms appeared; but in the instances in which the substance was boiled, they appeared where oxygen or common air was used, but not where nitrogen, hydrogen, or carbonic acid was employed. One experiment only appeared to have produced a result which could not be reconciled with the rest, viz. in which some meat and water had been boiled and sealed up in an atmosphere of nitrogen. In this, some organisms were found; but so completely was this result unlike that found in the whole of the rest of the series, that I felt convinced that some error must have been made in the experiment itself.

The experiments now to be described have a narrower range than the others. With the exception of a few, which were mere repetitions of the experiments with nitrogen just referred to, and which were undertaken solely with the view of seeing whether the experiment just mentioned were correct or not, they are confined to the single object of observing whether or not organisms are found in close vessels containing vegetable matter and water sealed up in an atmosphere of common air previously passed through an efficient heating apparatus.

In these experiments I have adopted some slight modifications of the apparatus used in the former ones. That now employed consists of a porcelain tube, the central part of which is filled with roughly pounded porcelain; one end is connected with a gas-holder, and to the other the bulb is joined which contains the substance to be experimented upon. The bulb has two narrow necks or tubes, each of which is drawn out before the experiment begins, so as to be easily sealed by the lamp; one neck is connected with the porcelain tube, as already stated, by means of an india-rubber cork, and the other is bent down and inserted into a vessel containing sulphuric acid. The central part of the porcelain tube is heated by means of a furnace, and when it has attained a vivid red heat the bulb is joined on, the end of the porcelain tube which projects from the furnace being made thoroughly hot immediately before the cork is inserted, the cork itself being taken out of boiling water, and the neck of the bulb being also heated with a spirit-lamp immediately before it is inserted into the cork. A stream of air is now passed through the apparatus by means of the gas-holder, and bubbles through the sulphuric acid at the other end. The substance in the bulb is then boiled for ten or fifteen minutes, the lamp withdrawn, and the bulb allowed to cool while the stream of air is still passing through the porcelain tube, maintained during the whole time at a vivid red heat. When the bulb is quite cool, the necks are sealed by means of a lamp. The advantage gained by means of this apparatus is that there is only one joint the perfection of which in any degree affects the success of the experiment, and of that joint it is easy to make sure. The porcelain tube also being, for a considerable part of its length, filled with small fragments of porcelain, all heated up to redness, easily ensures that every particle of air admitted to the bulb shall be thoroughly heated. A precisely similar arrangement was used for the nitrogen experiments, substituting a glass combustion-tube filled with copper-turnings for the porcelain tube, and a piece of india-rubber tubing for the india-rubber cork. The copper oxide was reduced by means of a stream of hydrogen when necessary between one experiment and the next.

A single experiment was tried on May 18, 1864, using apparatus similar to that employed in the experiments of the previous year.

Some pea-meal infused in water was boiled in a stream of heated air, allowed to cool, and then sealed and put by. I was then prevented from resuming my experiments for several weeks.

Then several experiments were made with nitrogen, for the purpose of confirming or correcting the nitrogen experiment of the previous year. Into the particulars of these I need not now enter, further than to say that seven experiments were tried with various infusions. Five of them were afterwards examined, and in no case were any organisms found, thus confirming me in the opinion already expressed upon that experiment. The series with which I am now concerned began on July 18.

Exp. VII. July 18.—Hay infused in water three hours, then filtered and boiled 12 minutes in a stream of heated air, and sealed up as above described.

Exp. VIII. July 18.—A similar experiment: boiled 10½ minutes.

Exp. IX. July 22.—Toppings, i. e. coarse flour infused in cold water 3 hours, filtered and boiled 10 minutes in a similar stream of air.

Exp. X. July 22.—A similar experiment: boiled also 10 minutes.

Exp. XI. July 25.—A similar experiment: boiled 12 minutes.

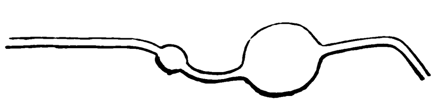
Exp. XII. July 25.—A similar experiment: boiled 10 minutes.

Exp. XIII. July 28.—Some sage-leaves bruised and infused in lukewarm water previously boiled. Allowed to stand 15 hours, filtered, and the clear fluid boiled 10 minutes in a stream of heated air, as in the other cases, and sealed up.

Exp. XIV. July 28.—A similar experiment: boiled 7 minutes.

Exp. XV. July 29.—A similar infusion of celery, allowed to stand 12½ hours, and treated as the last: boiled 12 minutes.

The bulb used in this last experiment was of a different form, which I have found much more convenient, and have always employed in my subsequent experiments, which are presently to be described (as represented in the figure).



The examination of the above series of experiments took place partly on Sept. 19, when Dr. Beale kindly visited me at Oxford, in order to give me his valuable assistance, and partly at Dr. Beale's house in London, on Nov. 16, 1864.

Exp. of May 18.—Viz. pea-meal and water. In this were found small organisms moving, as given by Dr. Beale in the accompanying drawing marked Z. Their size was extremely minute, as they are here drawn as they appeared under a power of 1700.

Z.

Exp. VII.—Hay + water + heated air. Some large dumbbell-shaped crystals and a few bacteriums, very minute, but not so small as in the former case. These also are drawn by Dr. Beale.

VII.

Exp. VIII.—The pair experiment to VII. Similar crystals, and organisms also similar, but larger. Drawn to $\frac{1}{12}$ Ross, i. e. 750 diameters nearly.

VIII.

Exp. IX.—Coarse flour + water + heated air. The result of this experiment was unsatisfactory, and serves well to show the difficulty of the decision upon these questions.

Even with the high powers above named, we were unable to be certain of our result in this and several following cases. There were no organisms distinctly recognizable as such, but many minute round spore-like bodies moving about the field.

Exp. X.—The fellow experiment to the last, and similarly unsatisfactory.

Exp. XIII.—Sage + water + heated air. A few crystals were seen, but no organisms.

Exp. XV.—Celery + water + heated air. Some prismatic crystals; no organisms.

It was resolved to leave the rest of these experiments till a longer time should have elapsed since the vessels were closed. The examination was accordingly resumed Nov. 16.

Exp. XII.—Coarse flour + water + heated air, contained some indeterminate granular matter and some few bodies which might be dead bacteriums, but nothing that could safely be considered as such.

Exp. XI.—The fellow experiment to XII., and equally without result.

Exp. XIV.—Sage + water + heated air, gave also no definite result.

Now, omitting altogether the nitrogen experiments, seven in number, we have here a series of ten experiments instituted with a view of showing whether organisms can be produced in vegetable infusions within closed vessels supplied with heated air. In my desire to try a variety of substances I took almost anything which my garden afforded, and in this way probably my selection of sage and celery may have been a bad one, as the aromatic ingredients of these plants may be supposed to influence the result of the experiment, especially as in a close vessel any volatile oil would be retained. If, therefore, the three experiments with these substances be eliminated, there remain seven experiments, one with pea-meal, two with hay, and four with coarse flour. Of these, five were examined on Sept. 19, and in three (viz. the pea-meal and the two hay experiments) the vessels were found to contain moving organisms. In two (those where coarse flour was used) none were found, and in the remaining two, examined on Nov. 16, also none were found.

In the meantime, when, from several of the above experiments having produced negative results, I looked upon the series as inconclusive, I instituted a fresh series of twelve experiments in the end of September, as follows.

The apparatus employed was the same as that used in the last series, except that I had some large double bulbs made for the present series. In other respects the process was the same as before.

Exp. I. Sept. 30.—Hay infused 3½ hours in water, filtered, and boiled 10 minutes in a stream of heated air—sealed up when cool.

Exp. II. Sept. 30.—Similar in all respects.

Exp. III. Oct. 1.—Similar.

Exp. IV. Oct. 1.—Similar.

Exp. V. Oct. 5.—Flour infused in warm water 3½ hours and filtered: boiled 11 minutes, as before, and sealed.

Exp. VI. Oct. 5.—Similar: boiled 10 minutes.

Exp. VII. Oct. 5.—A similar infusion infused 6½ hours, not filtered: boiled 10 minutes.

Exp. VIII. Oct. 5.—Similar.

Exp. IX. Oct. 7.—Flour infused 3½ hours, not filtered: boiled 10 minutes in a stream of oxygen, and sealed as before.

Exp. X. Oct. 7.—Similar: boiled 10½ minutes.

Exp. XI. Oct. 7.—Flour infused 4½ hours and filtered: boiled 10 minutes in oxygen.

Exp. XII.—Similar.

On Oct. 8 this series of experiments was divided into two sets: [B], Nos. II., IV., VI., VIII., X., XII., were placed on a high shelf in my dining-room; the rest [A] in a hot closet, by the side of the cooking-stove, in the kitchen.

The object of the latter arrangement was to ensure the vessels being kept warm enough during the winter months; but the heat was, I have no doubt, too great. I saw the thermometer on more than one occasion over 140° Fahr., and have reason to believe that I did not see it at its highest. Moreover, the bulbs here were almost wholly deprived of light. Thus, before opening the vessels, I had made up my mind that the results of the other half of the series were most to be depended upon. The temperature of the room in which they were probably never fell below 40° Fahr., and was generally between 50° and 60°.

The examination of the B division of this series took place at Dr. Beale's house, Feb. 7, 1865. The results were as follows :—

IV.

Exp. IV.—Hay + water + heated air. A few bacteriums were found in active motion (see drawing by Dr. Beale).

II.

1/25

1/50

Exp. II.—Hay + water + heated air. Very large numbers of similar organisms were found.

VI.

Exp. VI.—Flour + water + heated air. Few were found as compared with the last, but still several in active motion.

Exp. XII.—Flour + water + oxygen. No organisms found.

Exp. VIII.—Flour + water + heated air (unfiltered). A good many bacteriums, similar to the others.

Exp. X. — Flour + water + oxygen (unfiltered). Some bacteriums, but not moving.

The other set of experiments was examined by me at Oxford on various evenings between Feb. 16 and March 8; but during some part of that time I possessed no object-glass of sufficient magnifying power to avoid all uncertainty in the results.

In two of them, viz. Nos. V. and XI., I could find nothing like bacteriums. In the three others, viz. III., VII. and IX., there were what appeared to me dead ones (but a dead bacterium is an object of which few persons who have seen many would think it very safe to be very positive), and in one only, viz. No. I., an infusion of hay, were they numerous and moving. This I mention particularly, because the objects were very well seen, and moving actively in the first slide which I examined, and could be the better seen on account of the clearness of the fluid and the absence of granular matter; but upon examining several portions after the vessel had been open for a few minutes, though they continued to be seen in equally large numbers, all movement had ceased. They were examined with a $\frac{1}{25}$ object-glass of Messrs. Powell and Lealand. Now, if we omit from these two series of experiments those which I have already shown reason to distrust, we have, in all, seven in the first, and six in the second series, which seem fairly to test the question; and these having been examined by Dr. Beale, as well as myself, bacteriums were found and seen by both of us in three out of the first seven, and five out of the remaining six—in all, in eight.

Now, it may be asked, why the same or similar organisms were not found in the other cases, if the experiments were fairly tried? The answer is this, viz. that we do not know all the conditions under which they exist. It is pretty clear that they appear more easily in some substances than in others. Thus, in the first series above described, it will be noticed that the four instances in which none were found were all those in which coarse flour was the substance used. In the remaining three, where pea-meal or hay were employed, there the bacteriums were seen. So also in the other series, the one case in which nothing was found was a case in which flour was used, and in the remaining five the most numerous and distinct bacteriums were seen in the hay infusion. This may arise possibly from the fact that the infusion of flour is not so clear as the others, and always contains more granular matter; thus bacteriums are less easily distinguished in it: and, where doubtful, it is my practice to decide in the negative; that is to say, unless the bacteriums are clearly seen, I enumerate the experiment amongst those in which they are not found. Further, it is possible that in some infusions they may live and die sooner than in others, and in most of these experiments with flour there was a mass of indeterminate granular matter which might have contained the bodies of whole populations of bacteriums. Finally, it is quite possible that they might, if existing in small numbers, escape observation. Their minuteness is extreme, and observation of them far from easy. At any rate, positive evidence in a matter of this kind is of more value than negative; and the fact that in eight cases out of thirteen they have been seen not by myself only, but also by so accurate and practised a microscopist as Dr. Beale, is of more weight than our having been unable to discover them in the remaining five cases.

The question which now remains to be discussed is, how it is that the results above given so entirely disagree with those arrived at by M. Pasteur, and now, to a certain extent, vouched for by the Commission of the Academy of Sciences. I have observed all the precautions which M. Pasteur himself speaks of as 'exaggerated,' yet I have shown bacteriums to be produced exactly under the circumstances in which he asserts that they do not exist. I believe this discrepancy is very easily accounted for. M. Pasteur, in his memoir, speaks of examining his substances with a power of 350 diameters. Now my experience throughout has been that it is impossible to recognise these minute objects, with any degree of certainty, even with double that magnifying power. When once their existence on a slide is shown with a power of 1500 to 1700 diameters, it is quite possible afterwards to recognise the same object with a power of 750, but I have repeatedly failed to satisfy myself in the first instance with the latter power; and on the one occasion on which I enjoyed the use of an object-glass giving a power of 3000 diameters, I found the recognition of these very minute objects rendered very much more easy. On one occasion I tried the effect of a power of 450 (not possessing one of 350), and found that all satisfactory investigation of such objects with such a power was impossible. Any person has only to examine the drawings which accompany this communication (in one particularly, that marked Z) in order to satisfy himself that to come to any conclusion as to the presence or absence of such objects as are there represented, with a magnifying power of little more than $\frac{1}{5}$ linear measurement of that from which they are drawn, would be quite impossible. The Commission of the Academy of Sciences, which has not yet concluded its labours, has not, so far as its present report goes, concerned itself with the microscopy of the question; it has, in fact, confined itself to the dispute (which has almost become a personal one), between MM. Pasteur and Pouchet. It is worth noticing, that the fact so often referred to by writers on this subject, of the fluid in the closed vessels becoming cloudy or not as a test of the presence or absence of bacteriums, is not satisfactory; I have constantly predicted, from the cloudiness or clearness of an infusion, the presence or absence of bacteriums, and very frequently been mistaken—quite as often too in the former case as in the latter.

As to the conclusions which can be drawn from these experiments, I need say very few words. I can now have no doubt of the fact that 'bacteriums' can be produced in hermetically-sealed vessels containing an infusion of organic matter, whether animal or vegetable, though supplied only with air passed through a red-hot tube with all necessary precautions for ensuring the thorough heating of every portion of it, and though the infusion itself be thoroughly boiled. But how far this fact affects the question of what is called 'spontaneous generation' is quite another matter.

It seems clear that either (1) the germs of bacterium are capable of resisting the boiling temperature in a fluid, or (2) they are spontaneously generated, or (3) they are not 'organisms' at all. I was myself somewhat inclined to the latter belief concerning them at one time; but further researches have gone far to convince me that they are really minute vegetable forms.

The choice therefore seems to remain between the other two conclusions. Upon these I will not venture a positive opinion, but remark only, that if it be true that 'germs' can resist the boiling temperature in fluid, then both parties in the controversy are working upon a false principle, and neither M. Pouchet nor M. Pasteur is likely at present to solve the question of spontaneous generation. In truth, if M. Pasteur's facts are incorrect, the whole question is relegated to the domain of what the French Academy Commission calls 'pure discussion;' and the one point which I claim to have established by these researches is precisely that M. Pasteur's facts are inexact—not because his experiments were not most admirably performed, but simply because the magnifying power of his microscope was insufficient for the work to which he applied it.

I desire to append two remarks to this paper. The first is, that the common *à priori* objection, which M. Pasteur so well expressed in his memoir, to heterogeny in all forms, viz. that it is a doctrine which has been gradually driven from all the higher forms of life precisely in proportion as our observation of them has become more exact, until at last it has been compelled to take refuge in those lowest forms which we are almost or altogether unable to observe, is really of little or no force. Its cogency depends on analogy, and the analogy has no existence. It is quite equally to be expected *à priori* that if any forms of life are generated spontaneously, they will be the very lowest and simplest forms, and since these happen to be also the most minute, the objection loses its whole force. And it is also a thing to be expected that we should find only the lowest forms, the earliest, *i. e.* in the scale of existence, produced under the disadvantageous circumstances in which they must be placed in such experiments as those above detailed.

The other remark is this, that, so far as my present researches have led me, I cannot but look upon improvement in the construction of microscopes, and increase of their power, as the only way in which our means of investigation of such questions as the production of Bacterium is likely to be largely increased. The $\frac{1}{50}$ object-glass recently constructed by Messrs. Powell and Lealand for Dr. Beale, of which a notice has appeared in the Proceedings of the Royal Society, has already shown something like an appearance of structure in these minute objects, and leaves, I think, no doubt about their organic character.

NOTE ON ESSAYS III AND IV.

Those who are interested in the subject of these Essays, and have followed the questions treated in them through the numerous memoirs and discussions which have taken place in the French Academy, must perceive that they seem to be almost interminable and hopeless. At least the chance of settling them by means of experimentation directed expressly to that end seems so, and this for two reasons, viz.

I. The evidence as to actual results of experiment is contradictory. Upon this point, however, I cannot doubt that the balance of evidence is, on the whole, against M. Pasteur; for to the observations enumerated in Essay III, there are now to be added the results of my own experiments detailed in Essay IV, and also some further very careful experiments by Professor Wyman of Harvard College, described in the American Journal of Science and Arts, vol. xliv. Sept. 1867.

I can therefore feel no doubt but that organisms have been found under the precise circumstances in which M. Pasteur states that they never exist. But whether this be admitted or not, the solution of the question remains as far off as before, for

II. In order to the establishment of either doctrine beyond the possibility of cavil, it is required to prove a negative. A believer in spontaneous generation in order to demonstrate his view must be able to shew that *no germs* are contained in his vessels; or, at least, that all germs are killed by the conditions to which he subjects his experiments, and further, that no germ contained in his experimental vessels could possibly have escaped the action of these conditions. On the other hand a supporter of the opposite view is called upon to shew that *no organisms* exist in his infusions, and has to meet the somewhat difficult objections that since he knows not all the conditions requisite for the maintenance of life in the external world, he may have failed to reproduce them within his flasks, and that therefore the sterility of his infusions proves nothing; and further, that even this sterility cannot be certainly shown, since organisms may actually be present and yet from their small number and minute size, or the brief duration of their life, the experimenter may easily fail to discover them.

It is curious that the results of some of the most recent experiments seem to afford equal support to the objections on each side. Thus Dr. Wyman states that though in his experiments organisms certainly appeared under the same circumstances as they did in my own, and as they never did in M. Pasteur's, yet if the infusions were boiled for six hours no organisms ever appeared. This would seem to suggest that the organisms were the product of germs which were destroyed when subjected to a high temperature for a sufficient time, though it does not of necessity involve such a conclusion. Another recent experimenter, M. Lemaire, has shown (see Comptes Rendus, vol. lix. p. 696.) that the mere fact of an infusion being enclosed within a hermetically sealed vessel, even without any application of heat, is in itself sufficient to check the production of organisms; for that in such circumstances fermentation begins but cannot continue. This certainly tends to show that other conditions besides the mere presence of germs are required for the development of organisms, and that such conditions are interfered with where infusions are hermetically sealed up.

It would seem therefore that it is rather by careful and systematic observations of the whole phænomena of the lowest organisms, such as those for instance recently published by Professor Hallier, that we are likely to learn the conditions of their production rather than by any further attempts to decide by direct experiment whether they are or are not spontaneously produced in organic infusions.

Our final belief or disbelief in spontaneous generation must no doubt depend, like other scientific questions, upon experimental evidence and not upon theoretical considerations; yet it is remarkable that the present extreme unwillingness on the part of physiologists to admit its probability should coincide in point of time with the production of Mr. Darwin's views of the origin of species, and of Mr. Herbert Spencer's philosophy.

Mr. Darwin's theory, whatever objections may lie against some special portions of it[a], is daily gaining ground among men of science, yet it seems completely to work into the far wider generalization of Mr. Herbert Spencer, and serves almost of itself to suggest the probable truth of the latter. Mr. Spencer is himself apparently no believer in spontaneous generation[b], yet he avowedly extends those laws of evolution, by which he proposes to account for the whole phænomena of the inorganic world, not only to organic life but even to all the facts

[a] See, for example, Essays I and II of this volume.

[b] See his 'Principles of Biology,' vol. i. p. 210, note.

of mental and social development[c]. He says, 'To the classes who alone are likely to read these pages, the hypothesis of a fundamental unity, extending from the simplest inorganic actions up to the most complex associations of thought and the most involved social processes, will have an *à priori* probability. All things being recognized as having one source will be expected to exhibit one method. Even in the absence of a clue to uniformities, co-extensive with all modes of Force, as the mathematical uniformities are co-extensive with Space and Time, it will be inferred that such uniformities exist. And thus a certain presumption will result in favour of any formula, of a generality great enough to include concrete phænomena of every order. In the chapters on the "Law of Evolution" there was set forth a principle, which, so far as accessible evidence enables us to judge, possesses this universality.' &c. And again[d], in speaking of the ultimate laws of Matter, Motion, and Force, under the head of correlation and equivalence of forces, the same author says, 'The genesis of sensible motion by insensible motion, and of insensible motion by sensible motion, as well as the like reciprocal production of those forms of insensible motion which constitute Light,

c 'First Principles,' p. 488.
d Ibid. pp. 491, 492.

Heat, Electricity, Magnetism, and Chemical-action, was shown to be a now accepted doctrine, that involves certain corollaries respecting the processes everywhere going on around us. Setting out with the probability that the insensible motion radiated by the Sun, is the transformed product of the sensible motion lost during the progressive concentration of the solar mass; we saw that by this insensible motion, are in turn produced the various kinds of sensible motion on the earth's surface. Besides the inorganic terrestrial changes, we found that the changes constituting organic life are thus originated. We were obliged to conclude that within this category come the vital phænomena classed as mental, as well as those classed as physical. And it appeared inevitably to follow that of social changes, too, the like must be said.' Now if, on the one hand, it be held that the whole inorganic world has been produced from a homogeneous mass of nebulous matter, by a process of gradual evolution, passing through every stage of complexity till it reaches the infinite variety of structure and arrangement which at the present moment excites our wonder and confounds our attempts at exhaustive knowledge; and, on the other hand, that the whole organic world has been, in all its endless variety, developed by a like process from the simplest forms; if it be remembered, too, that organisms are compounded out of but a few of the elements of the inorganic world, and contain no new element exclusively their own; and further, that chemical synthesis has in some instances, at least recently, bridged over the gulf which formerly seemed to divide organic from inorganic substances; it seems an almost irresistible conclusion that there must have been a stage in the development of the universe when the earliest forms of organic life were evolved from some special collocation of inorganic elements by the continued operation of the laws already in action: and thus spontaneous generation would appear, to my apprehension at least, to be merely a necessary term in the evolution series.

It will be objected that the doctrine of Development of species, and still more that of Evolution, are mere hypotheses, and not proved facts, and this no doubt is true; but the latter is at least listened to with respect, and the former is heartily accepted, by physiologists who look upon spontaneous generation as the exploded crotchet of a past age, whose advocates hardly deserve a serious hearing.

The reader who has followed me thus far in my examination of these questions, will have seen that I am as far from considering spontaneous generation to be proved, as I am from believing it to be disproved. But I would suggest for the consideration of physiologists, in conclusion, that the doctrine of spontaneous generation can hardly 'receive its *coup de grace*' while theories which necessarily involve it are constantly advancing in probability and in credit. It is not a legitimate proceeding to admit a given proposition, and to repudiate its necessary corollary.

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