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ESSAYS

ON

PHYSIOLOGICAL SUBJECTS

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BY

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'The materialist and spiritualist controversy is a mere war of words, in which the disputants are equally absurd—each thinking he understands that which it is impossible for any man to understand.'—*Herbert Spencer.*

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PREFACE.

THE present edition of this book varies considerably from the former, and in so doing approximates much more nearly to its original design.

The first Essay of the former edition is omitted altogether from the present. The remaining three are reprinted almost unaltered.

The fourth is almost entirely new, only a small portion of it having appeared as a note at the end of the former edition. It discusses certain questions in general Philosophy, which are, however, closely connected with those treated of in the first three Essays, and also in the sixth. It was written in the autumn of last year, and I cannot forbear expressing the great gratification which I feel at finding that some of the opinions which I have here put forth coincide very closely with

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ESSAY I.

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

[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.

legitimate use, as a guide in experimentation, as any one pursuing an investigation in the science 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 I 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 I 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 paper 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

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

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 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 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 word, 'degenerate' either in body or 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

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

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 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 gauge, 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, I 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 develope 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 thorough-bred short-horns, 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 these 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 perpétuer 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 réciproques, 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 remote 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, I 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, I 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 perserved 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:—

Foljambe. Foljambe.

Bolingbroke. Phœnix (*a*)

Favourite (*b*) Phœnix (*a*)

Young Phœnix. Favourite (*b*)

Comet.

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 thorough-bred 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 advantage 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 connection 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 my 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 connection 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 my 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 connection 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 the many 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, in at least 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 walking through the Great Exhibition of 1862, many of my 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 develope 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 ESSAY I.

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 independently 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 a distinguished English physiologist, the late 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 II.

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 organisms, a subject which has been one of the vexed questions of biological science

[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.

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 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. They 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 the fourth chapter 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 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,

[b] Pouchet: Hétérogénie, p. 225.

Thus he took 10 grammes each of maize, pease, lentils, and beans; and having literally, as he says, carbonised 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., infusoria were produced in all of them. In similar infusions, in which the grain used was not carbonised, 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-by. 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 development; 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 I 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 I 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 I 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.*

I leave all criticism upon these experiments until I 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 I 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 an ordinary essay 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 experiments? 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 porligerous 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 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

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

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 connection 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

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

exact and convincing than those of his rival; indeed, the weak point of the 'Hétérogénie' throughout has 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 my 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 decoctiòn 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

g Nouvelles Expériences, p. 39 et seq.

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 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 spontanees*), 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 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

h Nouvelles Expériences, p. 111 et seq.

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 I 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 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

[i] Nouvelles Expériences, p. 116.
[k] Joly and Musset: Comptes Rendus, 1860, vol. I.
[l] Schaafhausen: Comptes Rendus, 1860, vol. liv.
[m] Cosmos, 1863, p. 630.
[n] Nouvelles Expériences, pp. 126, 127.

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 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 me to attempt a decision; nevertheless, as has been well said by a distinguished writer on a very 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 I am 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 the 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 exist 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 I have placed third in my 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 air-pump. 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 me 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 parmeciums 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.

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 question 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

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

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 organisms 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 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 organisms, 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 III.

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 those so-called organisms are found, would render it doubtful whether Bacteriums, Vibrios, &c. ought to be considered as independent organisms in any higher senses 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 n the instances in which the substance was boiled, hey 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.

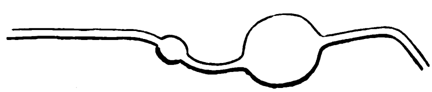
Exp. XII. July 25.—A similar experiment: boiled 10 minutes.

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.

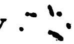
The bulb used in this last experiment was of a different form, which I have found much more convenient, and have always employed in my subsequent experiments, which are presently to be described (as represented in the figure).



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 $3\frac{1}{2}$ 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 $3\frac{1}{2}$ 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 $6\frac{1}{2}$ hours, not filtered: boiled 10 minutes.

Exp. VIII. Oct. 5.—Similar.

Exp. IX. Oct. 7.—Flour infused $3\frac{1}{2}$ hours, not filtered: boiled 10 minutes in a stream of oxygen, and sealed as before.

Exp. X. Oct. 7.—Similar: boiled $10\frac{1}{2}$ minutes.

Exp. XI. Oct. 7.—Flour infused $4\frac{1}{2}$ 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:—

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

IV.

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

II.

$\frac{1}{25}$

$\frac{1}{50}$

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

VI.

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 (more 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 $\frac{1}{8}$ 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.

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 inclosed 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.

ESSAY IV.

Some Aspects of the Theory of Evolution.

The object of the following Essay is not to give an exposition of the theory of evolution, but rather to show what are its relations to various scientific questions, and especially some of those discussed in the preceding Essays; to trace certain consequences which seem to follow from its admission or its rejection; and, finally, to point out certain misconceptions as to its scope and consequences which seem to be entertained even in quarters where one could scarcely expect to meet with them.

In order to do this, however, it is necessary to state as shortly and clearly as possible what, as I apprehend it, the theory of evolution is. It is, then, a belief that all the phaenomena of the universe, as we observe them, physical, biological, mental, social, political, proceed or are evolved in accordance with one universal law or set of laws, their whole evolution being a process of what is called by Mr. Herbert Spencer 'differentiation and integration,' whereby they progress continually from a state of comparatively indefinite and incoherent homogeneity to one of comparatively definite coherent heterogeneity; that all phaenomena whatsoever go through this process just in the same way, to use a familiar illustration, as we find that in a small country village, the keeper of 'the shop' is a grocer, a haberdasher, and perhaps half a dozen other trades in one, but that when it grows into a town it has its separate shops and separate tradesmen for each of those various callings; or just as we see, to take a more scientific example, that whereas among the lowest organisms the general surface of the body serves for all the purposes of absorption, secretion, respiration, &c., as we ascend in the scale we find in different portions of the body different structures separated and set apart—told off, as it were, for the performance of each of these several duties. Further, the evolution theory involves the notion that the process thus universally taking place is brought about in all cases by the same agency, or at least that so far as our faculties will carry us, this would appear to be the case. Mr. Herbert Spencer has worked out his theory both inductively and deductively, and has laboured to show, with great industry and with no small success, both on the one hand that the changes cognizable in every class of phaenomena may be reduced to stages in a process of evolution; and further, that certain fundamental ideas (themselves inductions from experience) which are the ultimate notions at which our minds can arrive, lead deductively to the conclusion that such an order must obtain. Thus, if we start with the idea of Force presented to us under the two modes of Matter and Motion, and the two inevitable conditions of Space and Time, and if we accept certain well-established laws of Force, these ideas and these laws followed out to their legitimate results will land us in the conclusion that all phaenomena submitted to the action of Force will of necessity go through the process of evolution as above defined.

A similar idea forms the subject of Mr. Grove's famous address to the British Association at Nottingham in 1866, under the name of 'Continuity.'

The evolution theory had its beginning, as Mr.

Spencer himself observes, in the phaenomena of the organic world, which have given rise to the various ideas of development or transmutation of species, finally taking the form lately presented to the world by Mr. Darwin. In this form those ideas have become, in the words of Dr. Hooker, 'an accepted doctrine with almost every philosophical naturalist.' They have been worked upon by Mr. Herbert Spencer as the foundation for the much wider generalizations which I have above attempted to present to my readers. Any such presentation of it will, as I am well aware, be little less than unintelligible to any who are not already acquainted with the theory, but it may serve to recall it pretty distinctly to the minds of those who are so. Other persons who desire to understand it are recommended to study Mr. Spencer's work on First Principles, in which they will find it stated and explained with equal clearness, boldness, and caution.

I proceed now to the direct object of the present paper, viz. to consider the application of the evolution theory to some of the questions mooted at the present day, and to some recent discoveries or supposed discoveries.

In the first place, the bearing of the question of the spontaneous production of living beings upon the theory of evolution is most important, and has been greatly misunderstood. It is somewhat remarkable that many of the writers who hold most fully and clearly the evolution hypothesis, as, for instance, Mr. Darwin, Mr. Grove, and even Mr. Herbert Spencer himself, seem to have failed to perceive its relation to the question of heterogeny, and even to entertain confused and inadequate notions as to what heterogeny really means and necessarily involves.

The experimental evidence on the subject has been sufficiently discussed in preceding Essays. It is admitted by all candid writers to be conflicting and inconclusive; indeed, it is idle to speak of the doctrine of the heterogenists as receiving its *coup-de-grace* from M. Pasteur's experiments, so long as his results are disputed by competent observers in all parts of the world, who, like himself, have devoted special attention to the question. Among such I may enumerate, in addition to M. Pasteur's persevering opponents in his own country, Professors Mantegazza of Pavia, Schaffhausen of Bonn, Wyman of Cambridge, U. S. To their evidence I may add the results of my own experiments, which were undertaken certainly with no prepossessions in favour of the results to which they led; and finally, that of Professor Hughes Bennett of Edinburgh, whose researches on this subjcet are to be found in the Edinburgh Medical Journal for March, 1868, and whose conclusions are entirely adverse to those of M. Pasteur. I believe that although, in the end, our judgment on this question may be determined by experimental evidence, yet that at the present time and with our present appliances it does not admit of being so determined. The disputants on both sides alike fail to bring the ultimate terms of their series of phaenomena within the reach of our senses, the 'pan-spermist' cannot show us his germs passing into his sealed vessels; and the heterogenist, on the other hand—if we except S. Mantegazza in this instance—does not attempt to demonstrate the process by which his so-called bacteriums are constructed.

In point of fact, what is demanded of each party is to prove a negative. The heterogenist is required to show that germs—which are invisible—are *not* contained in or admitted into his experimental vessels; the pan-spermist that *no* organisms, however minute or few in number, are to be found in his. It is not easy to see how experimentation under such circumstances is to lead to any trustworthy result, and in practice it ends in this, that each party in the controversy considers that where experimental evidence fails to support his theory, it is more probable that some of the conditions of the experiment should have been faulty than that the theory which he deprecates should be true. In default, therefore, of direct evidence, we must have recourse to certain general considerations in order to judge of the comparative probability of the two theories; and these considerations will be found to have been greatly modified when looked at in the light thrown on them by the knowledge recently gained in various independent lines of investigation. When the whole evidence is fairly considered, and the misconceptions above referred to are removed, it will be seen that heterogeny is part and parcel of the process of evolution; and that to believe in the latter and deny the former is not less illogical than to admit that a chalk rock contains the shells of foramenifera, and deny that the foramenifera lived during the formation of the deposit. M. Pasteur's most trenchant argument against heterogeny is thus stated by Mr. Grove:—

'In proportion as our means of scrutiny become more searching, heterogeny, or the development of organisms without generation from parents of similar organism, has been gradually driven from higher to lower forms of life, so that if some apparent exceptions still exist, they are of the lowest and simplest forms; and these exceptions may probably be removed, as M. Pasteur considers that he has removed them, by a more searching investigation.' This argument Mr. Grove considers 'well worthy of remark.' It is so certainly in one respect, viz. that, when rightly understood, it affords a strong support to the very hypothesis which it is intended to overthrow; but in the hands of M. Pasteur it is no more than the trick of a clever advocate who seizes upon any convenient weapon that may damage the cause of his opponent. Heterogeny, like other advanced doctrines, has suffered much at the hands of injudicious or ignorant supporters. The maggots in Redi's days, and the Acarus Crossii in our own, have been as fatal to a fair consideration of its claims as the various crude views of transmutation which preceded Mr. Darwin's great work were for the time to those of the now generally accepted doctrine of the origin of species. Mr. Grove continues: 'If it be otherwise, if heterogeny obtains at all, few will not now admit that at present the result of the most careful experiments shows it to be confined to the more simple organic structures, and that all the progressive and more highly-developed forms are, as far as the most enlarged experience shows, generated by reproduction.' This passage affords a good instance of the misconception with which I have to deal, and, taken as it is from the work of so thorough a philosopher and so advanced a thinker as Mr. Grove, it will be held amply to justify a careful examination.

If heterogeny is to be considered at all in an intelligent way, not looked upon as a species of magic, but as a part of the order of nature, where is it, I would ask, but in 'the more simple organic structures,' or rather in the very lowest and very simplest of all such structures, that we should expect it to obtain?

If people will look for it in the 'more highly-developed forms[p],' if they expect to see an elephant

[p] I have been favoured with a letter from Mr. Spencer touching

or an acarus spontaneously produced, it is no wonder that the whole theory is laughed out of court. Such an expectation is very much like a crude idea of the development of species which should look to see a camel transmuted in the third generation into a whale, if only placed under external circumstances favourable to the change. It is, I repeat, only the lowest and simplest organisms which can reasonably be supposed to be developed by heterogeny; it is only such which recent experimenters claim to have found, and it is further only in accordance with the analogy of all our knowledge of nature, to believe that even these are

this reference to his views, which appeared first in a note attached to the concluding Essay in the first edition of this work, in which he uses the following language: 'At the close of your appended note you refer to certain views of mine, and to my apparent disbelief in spontaneous generation. This reference and the apparent incongruity that seems to be indicated, made me feel that it might have been well to express the limit to that disbelief. Were it to be shown that these arise in some other way than by ordinary genesis, minute aggregates of protoplasm altogether indefinite and variable, the fact would not impress me as intrinsically anomalous; but that which I regard with scepticism, is the alleged spontaneous production of organisms *of quite specific characters.*'

It will be seen that the view here expressed by Mr. Spencer, with his usual remarkable lucidity, accords most accurately with that which I have here endeavoured to work out, and which was indicated in my paper read before the Royal Society, and elsewhere in my writings.

not yet known to us in the very earliest stages of their development. But that such should be so developed seems to me to be a necessary result of the law of evolution, if such law obtains at all. 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 continuous differentiation and integration, passing through every stage of complexity till it has reached 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 of but a few of the elements of the inorganic world, and contains 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.

Let us now look at an entirely different class of facts, viz. the phaenomena of what are called contagious diseases, and the views which have recently been held concerning them. It is to be observed that these views have been held by chemists, physiologists, and practical physicians, who have arrived at their various conclusions from the facts which they have observed, and quite independently of any general theoretical notions as to an universal Law of Evolution. They will be found to be sufficiently inconsistent both with each other and with themselves, but so far as they are not mutually destructive, they will also be found, if I mistake not, to lend a strong support to the theory of evolution, if therein be included the notion of heterogeny, but to be perfectly inexplicable except by the assistance of the latter.

It is now many years since the analogy was pointed out, first, if I mistake not, by Baron Liebig, between these diseases and the process of fermentation; and so strongly did this analogy impress itself upon the minds of nosologists, that it has given rise to the title of 'zymotic,' now

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universally given to the class of diseases of which I am speaking. It is indeed sufficiently forcible. Compare, for example, the action of a particle of yeast upon a quantity of wort, and that of a particle of the virus of small-pox upon a human being. In each case a small portion of foreign matter of a specific kind is introduced into a large mass of a decomposable organic fluid, in the one instance wort, in the other blood. In each case a change is produced in the whole mass which cannot be produced again, and in each case the matter which effects this change, itself increases enormously in quantity in the process of effecting it, and though it cannot again produce the same change in the same organic fluid, yet every particle of it is capable of starting a similar change in a fresh portion of the same decomposable fluid. The phaenomena in the one case are as closely analogous to those in the other as can possibly be conceived. Now, when this hypothesis of fermentation or quasi-fermentation was once admitted in the case of small-pox, it was readily adopted as affording an explanation of other more or less similar disorders — scarlatina, measles, typhus, cholera, &c., with more or less of probability in different cases. But, after all, it afforded so far but little in the way of true explanation. Fevers were like fermentation; but what of fermentation itself? Berzelius had suggested that it was the mere contact of the ferment with the fermentible fluid which caused the latter to be decomposed, that is to say, that it decomposed it because it did. Liebig went, indeed, a step farther than this, and suggested that the particles of the decomposable fluid being in a state of unstable equilibrium, and those of the ferment itself in actual decomposition, and therefore motion, the latter communicated their motion mechanically to the former, and thus induced decomposition likewise in them.

This, unsatisfactory as it was, remained the only theory of fermentation for a considerable time, until M. Pasteur undertook his laborious researches on the subject, and in due time announced that the real agents in the process were innumerable minute organisms, the process itself being simply one of the growth and multiplication of these organisms, which affected the constitution of the fermenting fluid by appropriating certain of its elements to their own nutrition; the kind of organism and the elements so appropriated being different in various cases, and thus serving to explain the different kinds of fermentation known as alcoholic, acetous, butyric, &c.

These views admitted, of course, of being extended so as to include the phaenomena of zymotic disease and contagion. It is obvious that if what is done in the process of inoculation for small-pox is literally a sowing of the seed of small-pox, and the eruption of the papulae is the development of the fresh crop of plants, very little assumption is required to account for other epidemic fevers by the hypothesis of the presence in them of similar though not the same organisms. The previously unaccountable phaenomena of contagion also are at once rendered intelligible by this most convenient hypothesis; for if the organisms supposed are propagated by germs of extreme minuteness, these germs may be easily carried about in the air or water, or in clothes or furniture, and the whole phaenomena as we observe them would result. Accordingly, many observers have been at work to discover and isolate the offending organisms in each specific disease; and several, notably Professor Hallier, of Jena, believe that they have succeeded in detecting some of them. Nothing certainly can be more plausible than this doctrine; but if it be held to the exclusion of that of heterogeny, as it is, for instance, by Dr. W. Budd of Bristol, it can be shown to lead inevitably to some rather incongruous results. Dr. Budd's view, so far as it may be gathered from his Essay on Variola Ovina, which formed his address in medicine, delivered before the British Medical Association in 1863, appears to be as follows. He looks upon all the zymotic diseases as caused by germs or minute organisms which find their way into the current of the blood. He holds that these diseases are never propagated otherwise than by direct contagion, i. e. by the introduction of one or more of such germs bodily into the circulation; and he holds that these germs are specific, that is, that they are always of one kind, so that germs coming from a case of typhus will always produce typhus, and never measles or typhoid or small-pox; and further, that each disease or rather each organism can only grow in the particular medium suited to its peculiarities. From these data it would seem to follow that every specific disease must have had a specific origin coeval with the rest of creation, and therefore apparently that our first parents must have between them shared every single zymotic disease which is extant in the nineteenth century, and any other which may have existed and become extinct in the intervening ages! Nothing less than this monstrous proposition is the logical conclusion from Dr. Budd's premises. On the other hand, the hypothesis of heterogeny, logically, as I have suggested, an integral part of the evolution theory, accounts in an intelligible way both for the facts which we already know and for our ignorance of those which we do not know.

An intelligent view of Continuity will, as it appears to me, include the idea of a fresh evolution of life in its earliest stage, as well as in further stages, as continually going on.

If we suppose the mass of the earth to have gone on developing by a constant process of differentiation from its early stage as a homogeneous mass of molten matter, till its superficial portions reach its present highly complex structure, we must also conceive that the same process is still going on, and moreover that different portions of its surface will be at any given time in diverse stages of advancement in accordance with the constantly increasing diversities of the forces acting upon it. As the condition of things on the earth's surface, and on any given space upon it, becomes progressively more and more differentiated, it will arrive, after a long lapse of time, at an extreme degree of complexity; and then it is easy enough to see that the relation existing between different classes of phaenomena at its surface will be so greatly involved and interwoven that it will be — as in many cases we are sure that it is — quite impossible for us so far to disentangle them as to be able to say which are the earlier and which the later stages, which the more and which the less advanced among some of these phaenomena. Hence, if we once admit that the production of organisms is but a stage in the process of evolution, the following propositions will have to be admitted:—

(1.) That organisms are not likely to be produced until a late stage in the process of evolution. (2.) That they will not be produced once for all, but will be continually being produced as the circumstances are locally occurring which give rise to them. (3.) That they will not be absolutely alike, but more or less diverse, even in their earliest stages. (4.) That each set of them which come into existence may either commence a course of gradual development, such as that which we now believe has given rise to all the higher organisms with which we are more particularly acquainted, or they may at any stage of that process become utterly extinguished. (5.) That it is unlikely we should be acquainted with them while in their very earliest stages.

If we are to maintain an evolution theory at all —and it is impossible to deny that such a theory is gaining ground daily among scientific men—I do not see how any hypothesis less thorough-going than that of heterogeny will serve to bridge over the gulf which intervenes between the advanced process of differentiation which has evolved the solar system out of a nebula, and the earth ready for habitation out of a mass of molten matter, and the other commencing process of differentiation, which was afterwards to develope the monad into the animal and vegetable kingdom as we now see them. If, on the other hand, the hypothesis be granted, then the five propositions which I have just laid down would seem to follow. Those also admitted, we can account for much that is inexplicable in the history of diseases, as well as in the history of the organic world. And it is certainly remarkable, as I have noticed, that the theory of fungi, as the active agents in epidemic diseases and in fermentation, should have sprung into notice at the present time.

There is every reason to suppose, in accordance with the results arrived at by Dr. Murchison, that two at least of the epidemic fevers only too well known among us, are distinctively *produced* anew under certain conditions—to wit, typhus, from overcrowding of human beings, and typhoid, from the decomposition of certain organic matters. Now if these two fever-poisons are essentially low organisms, here is clearly a case of the actual production of organisms. Of course the case is one which does not admit of *proof*, but when a number of facts independently observed are found to be explicable by one and the same hypothesis, it is not to be denied that a strong presumption is afforded in favour of the truth of that hypothesis.

Among the difficulties which the present hypothesis helps us to explain, is that most difficult of questions, the *origin* of epidemic disease. If we suppose it to depend upon the existence of organisms, and if we further suppose that such organisms are evolved under peculiar conditions and collocations of matter, then at once we can perceive both how specific diseases may arise, and how they may also from time to time disappear, while at the same time their propagation by contagion is equally well accounted for. Now, on the other hand, without this supposition, we are driven to a merely miraculous or magical origin for each disease, or else to the ridiculous ideas just noticed. Similarly we may understand at least in some degree those peculiar and anomalous forms of disease which sometimes occur, and which partake more or less of the characteristic symptoms of two distinct fevers. Such cases, however rare, are well attested, although the thorough-going partisans of specific distinctions are apt to escape from the difficulties which they present to their theories by denying their existence.

The conclusions then at which I cannot but arrive from a survey of the whole facts of the case are shortly these. (1.) That the theories respectively of the Development of species and of Evolution, which would include such development, cannot be said to be proved, though both of them are daily advancing in credit, and obtaining adherents among the most philosophical section of scientific men. (2.) That if they are admitted, heterogeny cannot be denied. (3.) That the theory of evolution together with that of heterogeny serves to explain a large number of hitherto inexplicable phaenomena. (4.) That if the fungus-theory of disease should be established, it will afford almost a demonstration of the truth of heterogeny.

It will be well in the last place to say a few words upon the relation of such theories as that of evolution to religious belief. Upon no subject, as it appears to me, is more misconception prevalent, and no subject appears to require to be discussed boldly, fairly, and unreservedly more than does this; yet, on the other hand, there is no subject upon which one meets with so much inconsequent and insincere discussion. Almost every inquirer either starts *θέσιν διαφυλλάττων*, or else comes to his subject with a sort of shivering dread that he may find himself unwillingly and even unconsciously involved in some concessions that must lead to atheism. Even writers by no means deficient in either boldness or honesty seem to be sometimes carried away by the force of popular opinion upon this particular matter, it being pre-eminently matter which requires a man to throw aside popular prejudice and follow his own conscience and reason, if he is to reach a result of any value.

I find Professor Goldwin Smith, in an avowedly controversial Essay, speaking of Mr. Herbert Spencer's work as 'a great system of philosophy,' which his opponent and himself would 'agree in calling atheistical[q].' I venture to demur to this language. To me it appears that Mr. Spencer's theory, or at least the essential portions of it, affords the only tenable conception of the kosmos as it exists, the only escape from the merely tinkering theory of creation; yet I am by no means prepared to give up my own belief in a God 'who has made all things in heaven and earth, visible and invisible.' Nay, more—so impossible does it appear to me that an intelligent and unprejudiced student of science can really hold the ordinary view, that I think it is only by adopting the evolution theory in some form or another that such an one can escape from an atheistic conclusion without at the same time becoming involved in a hopeless confusion of thought. Hence it follows that I attach no little importance to such a misconception as that just quoted, and that it appears to me worth some trouble and some risk to attempt its removal.

[q] Rational Religion and the Rationalistic Objections, &c., by Goldwin Smith, p. x. Whittaker, 1861.

I am not concerned to defend the whole of Mr. Spencer's chapter upon the 'Reconciliation' of scientific knowledge with the religious instinct, inasmuch as no author would probably be less thankful for any such defence, and none certainly is more thoroughly capable of defending himself. There are many statements contained in that chapter which I should entirely decline to endorse, but such appear to me to be for the most part either not of the essence of Mr. Spencer's argument, or even to be inconsistent with his premises. They may or they may not be indications of his own views upon theological questions, but unless they are inseparable from his theory as a whole, which I cannot think they are, they would seem to concern his readers but little or not at all.

With regard to the evolution-theory in all its essential features, it appears to me that those theologians who denounce it fail to see and understand the real interests of religion, at the same time that they run a tilt against the whole series of the facts of nature, which can only end in their own utter discomfiture. A fair consideration of the facts of human nature and the range of human knowledge as presented in Mr. Spencer's work on First

Principles, avowedly brings us to the following conclusion. 'We are obliged to regard every phaenomenon as a manifestation of some Power by which we are acted upon; phaenomena being, so far as we can ascertain, unlimited in their diffusion, we are obliged to regard this Power as omnipresent; and criticism teaches us that this Power is wholly incomprehensible[1].' Now when it is considered of what process this conclusion is the result, it can hardly be reasonably looked upon as inimical to religion. The process is spoken of as atheistical, and we are told that it reduces God to the Unknowable, but in reality it is no more and no less atheistical than is any ordinary treatise on arithmetic or geometry; that is to say, it is atheistical only in an etymological sense, in so far as it does not treat of theology. All that it professes to be is a survey of human knowledge and of human faculties, from the strictly human point of view afforded by those faculties themselves, and subject, therefore, to their limitations. Yet we see that this may bring us to the conclusion that our faculties, together with their objects, compel us to believe that there exists some Power, which is

[1] First Principles, p. 99.

the origin and efficient cause of all phaenomena whatsoever, and that this Power is and must for ever remain incomprehensible to our faculties. This conclusion appears to be so far from atheistical that it is merely the translation into scientific language of that truth which has ever been a commonplace with orthodox theologians, 'that man cannot by searching find out God.' It is, in fact, the modern and scientific form of natural religion, and it is useful as showing that modern science followed out with the most unquestioning loyalty to its very latest conclusions, unembarrassed by any bias on the part of its explorer in favour of any system of theology whatsoever, so far from tending to the destruction of religion, thoroughly establishes its existence and its necessity. It is true indeed that it takes us no farther than natural religion, but this is all that it could possibly do by the conditions of the case.

But there is a further remark which here suggests itself, viz. this—that since the conclusion which the evolution theory reaches is that the unknown Power *is unknown* and even *unknowable*, it follows that with it must be tenable any conceivable theory of revelation which is not destructive of the very data upon which the evolution theory itself is built, since if the Power in question is unknowable, it is monstrous to dogmatise concerning the modes in which such Power may act, and whether or how it may reveal itself to us. Hence we are driven, —upon Mr. Spencer's principles, no less than upon Dr. Mansel's,—to the conclusion of the latter[s], that it is by the evidences of a revelation and not by its contents that mankind is to judge of it. There are, however, important qualifications, without which this statement cannot be received. For if it were admitted without qualification, it would be open to the charge which Professor Goldwin Smith[t] has made against it, that its only evidence would be that of miracles, or rather of human testimony to the occurrence of miracles in its support, and that such could be alleged in favour of some of the lowest and vilest religions that have ever disgraced the world.

There must then be two qualifications introduced, which are, however, in truth, only two forms of the same qualification. The supposed revelation cannot be accepted if it contradicts proved facts of nature, whether physical or moral. Thus it is impossible to

[s] Bampton Lectures, 1858, p. 234. [t] Op. cit. p. 14.

accept the verbal and literal account of a six days' creation, as a revelation, not on account of its miraculous character, (for it is just as illogical to assert that the Unknowable cannot interfere with the established order of nature, as that the Unknowable cannot reveal itself to us,) but because we have visible and tangible existing facts to show, not that it *cannot have been*, but simply that as a matter of fact it *was not* the case. The theory of evolution treated logically does not interpose any obstacle to the acceptance of a miracle upon sufficient evidence, but only to the acceptance of any evidence as sufficient to establish historically a miracle, the occurrence of which is disproved by demonstrable physical facts.

Thus, for instance, suppose I am told that a particular mass of stone fell down from heaven. Nothing sounds, in the first place, more improbable; but if the evidence is good, and if, upon examination, the mass turns out to possess the character of a meteorite, I have nothing to say against it. But suppose, on the other hand, that I find that the mass in question has not the character of a meteorite, and that it is in fact a mere block of the same material as the rock which forms the upper stratum of the ground on which it is placed, and with which it bears evident marks of former continuity, all the evidence in the world will not persuade me that it fell from heaven, and I say not that it could not have so fallen, but that, as a matter of fact, it did not so fall. The evidence against the alleged fact outweighs any that can be brought forward in its favour.

The answer to the question as to the moral difficulty may not be so obvious, but I believe it will be found to be conclusive. If it be asked, if God be incomprehensible to us, be the Unknown and the Unknowable, how are we to judge of any revelation from Him by its contents? whence are we to get any standard whereby to pronounce that an immoral or licentious religion can be no revelation from Him? The answer must be that morals like everything else are subject to the law of evolution, that their development has always been mainly in one direction, viz. towards that which we all agree to call the highest standard[u]. There may have been, as in fact there have been, partial and apparent movements in other directions, but there can be

[u] See on this point Mr. Leckey's History of European Morals, ch. i, which has been published since this Essay was in the printer's hands.

no doubt that on the whole there is an universal coincidence of opinion among civilized, i. e. the highest races, as to the difference between good and evil, as to the higher and the lower standard in morals.

It must therefore follow that nothing can be admitted as a part of a genuine revelation which is repugnant to the highest standard of human morality in the sense of being below it. And thus we are able to judge negatively at least of the contents of a revelation. If we extend the idea of evolution, as we must, into the region of morals as well as of biology, psychology, &c, and if we are able in any way to judge which of two given stages in any evolution-process is the more advanced, then we have so far at least a negative test of the truth of a revelation. For if any part of an alleged revelation be opposed to the observable process of evolution, (tend to reverse the process,) then either the pretensions of the whole revelation must be false, or the defaulting portion can be no genuine and integral part of the revelation itself.

Taken in itself, the evolution theory is neither theistic nor atheistic. It is demonstrably impossible for the human mind to get beyond a mere knowledge of phaenomena; every attempt to do so lands us in absurdity and contradiction. This was shown with admirable clearness in Dr. Mansel's work already referred to; but the mind cannot rest in this mere negation, and the great advance, of which the credit belongs to Mr. Herbert Spencer, consists in the pointing out that what is thereby proved is not that phaenomena have no cause, but that they have a cause, and that that cause is inscrutable to us. This I maintain, so far from being an atheistical conclusion, is all that Religion can claim at the hands of Science, and all that she needs to claim in order to establish a firm alliance between them. Science asserts a right to the whole universe of phaenomena, either *in esse* or *in posse*. And this right she is gradually establishing. It will be wise and right for Religion to respect it. But where phaenomena end the realm of Science ends too, and she is able to say to Religion[x], 'All else I leave to you. I have searched out and learned much. I know the order and succession of phaenomena in sidereal systems, in the solar system, in the crust

[x] It is hardly necessary to warn the reader that this passage must be understood to refer to the possible future condition of science, not to its present actual acquisitions.

of this planet, perhaps also in that of others. I have learnt the succession of life upon the earth, in the sea, and in the air. I have learnt even the order of development of the intellect and thoughts of man, and of his moral nature too. I have discovered the order of the social and political progress of mankind, and I have learnt that all their multitudinous and innumerable phaenomena have one invariable order and system, one regular law of Evolution, in accordance with which they one and all proceed—and all this together compels me to acknowledge some one universal omnipresent Power which is the origin, cause, and substance of the whole. What that Power is I am wholly unable to conceive or to imagine; I am driven to absurdities and contradictions when I attempt to represent it to my consciousness; nay, more, I am able to perceive that I do not possess the faculties requisite to apprehend it; and here where my empire ends yours begins.' It is well that theologians should look the facts straight in the face. Every day that we live here in England the tendency becomes more marked to the establishment of a state of things here, which has often been charged upon foreign countries, viz. a state of positive open war between

Science and Religion. Such a state of things, should it arise, would be the greatest of possible calamities, and for it the theologians will have chiefly to thank themselves. Many of them insist upon the acceptance of the literal interpretation of the Mosaic account of the Creation, and many more who do not go so far as this, maintain a reserve on the subject, and seem to be afraid of the consequences of a candid recognition of facts as they are. Now it is not too much to say—and it is well that it should be said without reserve—that this literal interpretation theory simply cannot be held except by a person either grossly ignorant of the facts of natural history, or affected with an entire incapacity for consequent thought. On the other hand, this point once conceded, there is no further difficulty in the way of the Evolution theory from a theological point of view. Certainly it cannot be objected that it gives a low, or mean, or mechanical view of Creative Power. For which is the nobler ideal? —that which represents to us an almighty and inscrutable Power which has produced the whole universe of cognizable phaenomena gradually evolved as far as we can trace them in an ever-changing series of which we cannot perceive or even imagine either a beginning or an end; or that ultra anthropomorphic representation which exhibits to us the Supreme Being in the guise of a human artisan turning off, as I may call it, the various species of plants and animals one after another as a potter turns off cups and saucers from his wheel? The latter surely is a conception suited to a barbarous and uncultivated age, and to men with limited powers of thought, and scarcely any capacity of abstraction, and but little acquaintance with the facts of nature; but the former commends itself at once to the naturalist and the philosopher, to whom all his habits of thought render the other an impossibility. If therefore the theologians of our day cannot be brought to see and acknowledge that Science must and will be 'within her dominions supreme,' but will engage in a war of extermination, there can be no possible doubt with which side the victory will remain. Science must conquer; but it is not easy to reckon the mischiefs and calamities which must be entailed upon mankind by the war. And the war is as needless as it must be calamitous.

ESSAY V.

PHYSIOLOGICAL EXPERIMENTS[a].

THE correspondence which many of my readers will remember to have taken place in the *Times* during the year 1863 on the subject of physiological experiments upon living animals, or vivisection, as it is commonly called, was remarkable in many particulars. The subject was one the discussion of which was especially well calculated to bring out into a strong light both the progress which has been made of late years in the general enlightenment of the newspaper-reading classes, and also the very considerable amount of ignorant bigotry which still finds place among them.

The opinions which have been and still are held upon it vary to the very utmost; and it is a matter

[a] 1. 'The Beneficent Distribution of the Sense of Pain.' By G. A. Rowell. Second Edition. London: Williams and Norgate. 1862.
2. 'Report of the British Association for 1863.'

in which a person's feelings are apt to be very closely bound up with his opinions. The man of refinement and sensibility finds his whole being stirred with indignation when he hears of what seems to him only the cruel and coldblooded torture of innocent and gentle creatures. The scientific man, on the other hand, is wont to feel something very like contemptuous impatience when he is told that his best-directed and most laborious efforts to improve the condition of his fellow-men by extending the sphere of their knowledge, are to be checked and thwarted by what is, in his eyes, either mere morbid sentiment, or, at best, an utterly disproportionate valuation of animal life as such. But in this, as in other matters, it is not by insisting on extreme views that any progress can be made towards a real uniformity of opinion. The subject is one in which very complex relations are involved, and which cannot be settled off-hand by sentimental or contemptuous declamation.

In order to impose some limits upon the extent of this investigation, I must begin by the assumption that the destruction of animal life by man for his own food and for other purposes, is necessary, and therefore justifiable—a proposition which may possibly be disputed in the abstract, but which is, at least, irrefragable in so far as regards our own country and our present stage of civilisation.

It may be said, indeed, that death and pain are not inseparable; and that a right or a necessity of inflicting one will not imply, even if it be proved to exist, a justification of the infliction of the other; but a very slight consideration of the facts of our social life will suffice to show that no distinction of the kind is observed in practice.

The sense of pain, as it exists among the lower animals, is a subject upon which it is impossible to doubt but that a vast amount of misconception exists, not among the ignorant only, but also among the refined and highly educated. It is one the investigation of which is very difficult, and leads to results which are matters of inference only, and do not admit of rigid demonstration, and which may seem at first sight to tend towards the encouragement of cruelty rather than its repression. But on this point, as on others, it is well to remember that truth can never be immoral, or, in the long run, even inexpedient. It is certain, however, that in any attempt to estimate the degree to which animals of various grades in creation are sensitive to pain, we must go beyond mere appearances, or we shall be grossly misled. What are commonly spoken of as 'the ordinary indications of pain' are all of them more or less fallacious. Every surgeon has seen men writhe and heard them groan under operations when he has known well that chloroform had rendered them perfectly insensible before they were begun. Some of my readers, too, have no doubt seen persons afflicted with epilepsy writhe and wriggle apparently in the extremest agony; but yet there is evidence in plenty, that however distressing a disease epilepsy may be, from its effects upon both body and mind, almost the first step in the train of phenomena which constitute a fit or convulsion, is the complete loss of consciousness, and all the subsequent contortions are performed without pain, or even sensation, on the part of the sufferer.

My readers will find this subject followed out with equal thoughtfulness and ingenuity and at greater length than I have space to pursue it here, in the essay the title of which is given above. In this essay Mr. Rowell argues, with a force which must carry conviction to the mind of any unprejudiced person, that the sense of pain, even in the most highly-organised animals, is very much lower in intensity than it is in man; and that in many of the lower creatures it can scarcely exist at all. He shows further, that the acknowledgment of this truth, so far from affording any encouragement to cruelty, only enables us to form a more correct notion of what cruelty really is, and would, if it became general, lead to a great improvement in the relations which subsist between man and the lower animals. Thus, for instance, Mr. Rowell shows that there is good reason for believing that hunger, even when not felt in an extreme degree, is a source of more uneasiness to an animal so highly organised even as the horse than is a severe bodily injury. He quotes several cases—pp. 22, 23—in which horses have met with accidents on the road of such severity that their leg bones have been found protruding through the skin, and actually in contact with the ground, as they walked along; yet in all these cases the animals began to graze, standing on their wounded extremities, almost as soon as they were left to themselves. Now, it is not pretended that under such circumstances as these the creatures feel *no* pain at all; but it may very reasonably be argued that the degree of pain which they feel must be almost immeasurably short of what a human being endures in a similar case. We do not generally see a man sit quietly down to dinner within half an hour after his leg has been crushed by a waggon — even although he may not have to stand upon the wounded limb while he eats it. It will be doubtless a new idea to many a man who fancies himself humane, that if he keeps a horse upon a short allowance of food, there is at least some reason to fear that he inflicts more misery upon the animal than does the cab-driver whom he has himself perhaps threatened with prosecution for cruelly flogging his horse. Nevertheless, it is clear that the consideration of this probability will tend rather to induce the humane man to see that his horse gets enough to eat than to encourage the cab-driver in the habit of brutal flogging. This is but one among innumerable instances which might be given in order to demonstrate the truth of the proposition which we are now discussing. That the proposition itself is an unpopular one I am fully aware; but if it happens to be true, its admission will be followed by no ill result. The distinction between that which is true and that which is edifying, is one which the English public has not yet learned to appreciate. I will mention but two more facts in support of this view. It occurs not unfrequently to fly-fishers to take a salmon or other fish, which has already one or more hooks fastened in its jaw, but which does not seem to have suffered in health or condition in consequence; yet compare with this the effect of any injury of similar magnitude upon a human being, and the contrast will be striking enough. Another instance, even more to our present purpose, is to be found in the different effects of similar surgical operations, when performed on the lower animals or on the human subject. Thus it is a well-known fact that any operation upon the human subject which entails the opening of the cavity of the peritoneum is attended with the most serious danger. Inflammation of that membrane is very commonly the result, and it is a most dangerous affection. In the dog, on the contrary, and in other animals, the same membrane may be cut into with comparatively little fear of any such result; yet it would trouble any physiologist to assign an intelligible reason for this difference, unless it is to be found in the immeasurably greater susceptibility of the nervous system in man than in the lower animals.

Many thinkers have, as is well known, found great difficulty in reconciling the pain and misery which they saw, or believed they saw, existing in the animal creation around them, with the idea of an infinitely beneficent Creator. They have argued that as the lower animals could do no moral evil, it was inexplicable that they should suffer, as it were, as a consequence of the moral evil done by man. Mr. Rowell, in the essay to which I have before referred, anticipates this whole difficulty by some very cogent reasoning upon the proposition that the animal creation in reality suffers a mere minimum of pain, and that even of that minimum a considerable portion is produced by the direct interfering agency of man himself. For the working out of this theory I must refer my readers to the essay itself; but I may mention here that Mr. Rowell goes far to show (1) that the lower animals, as noticed above, are endowed with vastly less susceptibility to pain than we are disposed to believe; (2) that their ordinary and natural mode of death, viz. by falling a prey to other animals, entails but very little suffering; and (3) that but for that they would suffer vastly more than they now do in the way of old age, infirmity, and consequent want and starvation.

The proposition, then, that the animal creation, not excepting even its higher classes, is immensely less sensitive to pain than is mankind, is one which I believe will not admit of serious dispute; but it is worthy of notice in this connection that the degree of sensibility to pain even in mankind varies directly with the increase of civilisation, and that to a degree which superficial observers will not be very ready to admit. It would be easy to bring forward a number of facts in proof of this, were it needful; but one will suffice for the present. Mr. Palgrave, in his recent work on Arabia, gives the following instance of extreme insensibility to pain in the case of a young Arab:—

'What is really remarkable among them (the Arabs) is a great obtuseness in the general nervous sensibility. On more than one occasion I had to employ the knife or caustic, and was surprised at the patient's cool endurance. While at Riad, a young fellow presented himself with a bullet lodged deep in the forearm; it gave him some annoyance, and he insisted on having it cut out. The operation was, for my inexpertness, a difficult one; the muscular fascia had to be divided down to the bone. Meanwhile the Nejdean held out the limb steady and inflexible, as though it belonged to a third party, and never changed colour, except it were a flush of excited pleasure on his face when I finally drew out the ball through the incision and placed it in his hand. After a short interval of bandaging and repose he got up and walked home, carrying his leaden trophy along with him. Much similar I saw and heard; the Arabs are not a nervous or excitable race.'

I pass on, in the next place, to consider shortly the comparative value of human and of animal life. The true relation which subsists between them is to be found expressed with inimitable accuracy in two detached sentences in the New Testament. In one place our Lord tells us, that 'not a sparrow is forgotten before God:' in another He says, 'ye are of more value than many sparrows.' But even apart from any such authority, our own reflections would lead us to the same conclusion. The more we consider the question, the more certainly shall we conclude that there can be no possibility of a comparison between man and the brute creation, whatever some few theorists may say to the contrary. A distinguished physician of the early part of this century (Dr. Baillie) used to express his belief, that as a general rule men go out of this world as unconsciously as they enter it, and no one, I think, who has often witnessed death can doubt that this is true. Whence, then, can come that intense dread of death which most men feel, whether they confess it or not? It is not fear of pain, for death in most forms is painless; and, moreover, men would and often do voluntarily submit to the severest pain in order to put off the death they fear, even for a short time. I do not mean to say that there are not exceptions to this rule—cases in which men are so worn out with pain that they welcome death itself as a relief, though I do not believe such exceptions are nearly so frequent as they are supposed to be; but the rule is the other way. The enemy of mankind showed plainly enough his accurate appreciation of their character when he said, '*all* that a man hath will he give for his life.' 'Propter vitam vivendi perdere causas' is a maxim which has ever been condemned by heroes and philosophers, but always acted on by the mass of mankind; and men will but too generally choose to live in almost any amount of physical misery or moral degradation rather than to die. It is, in fact, the simultaneous consciousness of moral guilt, and of immortality, which accounts for all this—that 'dread of something after death,' as Hamlet says, which all but the very highest and the very lowest of mankind are conscious of. Shakespeare tells us in a passage which is constantly misquoted in connection with the very subject now before us, that 'the sense of death is most in apprehension;' and here is, in fact, the great gulf which is fixed between man and the lower animals. Any man who has reflected on as well as experienced the many miseries of life, if he could only believe that death is really 'an eternal sleep,' would, on the first serious trial, betake himself to it; but, as a fact, such a belief never, in ordinary times, makes much progress among mankind; and where, under some very special circumstances, as during the first French Revolution, it does for a time obtain a considerable body of converts, an utter recklessness of human life is the natural and necessary consequence. The gambling and dissipation of the French prisons, and the fashion of men laying their heads upon the block with a jest upon their lips, during the Reign of Terror, were but the logical result of the worship of the Goddess of Reason. All one can see and learn of the psychical condition of the lower animals would lead us to believe that they, on the contrary, are no more afraid of death than they are able to reflect and generalize concerning it, and that they avoid dangers, when they do avoid them, by a kind of blind and mechanical, but nevertheless unerring instinct, which is given to each of them by Providence to preserve the race and to prevent unnecessary suffering and misery.

Since these questions are but subordinate portions of the subject of this Essay, I am unwilling to discuss them at greater length, but I will remark before leaving them, that while the two principles which I have laid down do not even necessarily exclude the opinion which certain persons somewhat fancifully maintain, that a future life may be in store for animals as well as for man, it is, on the other hand, by the adoption of some such principles only, that we are enabled with our present knowledge to reconcile the actually existing condition of the animal creation with the idea of a benevolent and at the same time omnipotent

Creator. Coleridge has somewhere said, that 'in the whole vast harmony of creation, man is the only jarring string.' This is a sentiment which naturally approves itself to our ideas of what should be, for, without indulging in speculations as to the origin of evil, it would seem probable, *à priori*, that, as man enjoys the terrible privilege of alone being able to act wilfully in opposition to the will of the Creator, so also should he possess a monopoly of that suffering which is the appropriate punishment of so doing. But, unless upon the principle here advanced, I do not see how it is possible to believe that such is the case. If the value of mere animal life can in any way be put in competition with that of human life, or if the animal creation is capable of suffering in any degree at all approximate to ours, then indeed is man no longer the only jarring string, but the whole lyre of the universe is jangled and out of tune throughout.

It is well observed by Mr. Rowell, that the most obvious objection to the view which would reduce the sufferings of the animal creation to a minimum, viz. that they, like man, suffer from disease, is an objection which is mainly of man's own making, since disease is mostly to be found in domesticated animals, and lingering suffering from it probably belongs to them alone, inasmuch as in its natural state of existence in the primeval forests a creature disabled in any manner is certain to fall a speedy prey to its fellows.

The questions hitherto considered and the principles laid down do but clear the way for a right view of the two propositions which it is my proper business to discuss. They do not pretend to do more than this, for since there can be no doubt that animal life is of high value, as we may learn from the many contrivances existing in nature for its preservation, and since, too, in depriving any animal of life we are taking away that which we can never restore, it follows clearly enough that we can have no right to do so wantonly. Since, too, no one in his senses can doubt that all the higher classes of animals possess some degree of feeling, however much it may be below the standard attained by man, we can find no justification for torturing an animal which possesses any sensation on the grounds that it feels but little, any more than a pickpocket can defend his robbery of a five-pound note from a millionaire on the score that the latter has so many such that he will never feel the loss. But the real force of these preliminary discussions is simply this: that whereas it is as much a crime to sit idly by and permit an evil which it is within a man's power to prevent, as to become oneself the agent in the mischief; so it becomes necessary in certain cases for a man to consider whether by the artificial production of suffering and death in animals he may ward off similar suffering and death from men: and inasmuch as men are 'of more value than many sparrows,' it seems to follow that surgeons and physiologists are not only justified, but positively bound to inflict suffering upon the lower animals, IF it can be shown that it is necessary or greatly advantageous so to do for the purpose of saving human life or mitigating human suffering. If a man conscientiously believes that he can by a certain course of vivisection obtain the means of curing a disease hitherto intractable, or of materially improving upon the treatment of one as at present practised, I do not scruple to say that he neglects his duty if he neglects to perform such vivisections. For what such a man does is really to set a higher value on a few dogs or rabbits than on an indefinite number of his fellow-men. Does any reasonable man, for instance, doubt that he may lawfully ride a horse to death to save a human life, whether his own or another's? But those who deny that vivisection can in any case be justified should hold, in order to be consistent, that an aid-de-camp is to consider the life and suffering of his horse when the safety of a whole army depends upon the rapidity with which he delivers his despatches.

From the considerations thus far adduced, it would appear that in attempting to form a judgment as to the justifiable character of vivisection, we must be guided by the following general principles: viz. that while wanton cruelty can in no case be excusable, on the other hand that suffering, and even very severe suffering, not only may be, but must be, inflicted upon the lower animals, where any adequate benefit is thereby to be secured for mankind, or any considerable evil to be averted from them. To these two principles a third must also be added: viz. that in judging of all matters of the kind regard must be had to the existing grade of civilisation; that is to say, we must take into consideration the habits of society *as it exists around us*, in order that we may not attempt to begin reforms, necessary or desirable in themselves, at the wrong end of our social system.

It may be well, perhaps, before proceeding to the consideration of the necessity or otherwise of vivisection for the general progress of physiological science, to say a few words as to the other purpose for which its utility has been alleged, that, viz. of giving skill to the operator. This portion of the subject is, however, one which need not detain us long. My readers may remember that it was in connection with this question that the whole newspaper discussion upon vivisection arose. It was in consequence of the habitual use of vivisection in the French veterinary schools that the agitation of the question was begun, and their sole and avowed purpose in so employing it was that of rendering their students skilful operators, and, by accustoming them to retain their coolness in the presence of animals struggling under torture, to enable them to avoid accidents in the course of their subsequent practice. Now, without following the French professors through the whole course of an ingenious but not very convincing defence of their practices made before the Imperial Academy of Medicine[b],

[b] See Bulletin de l'Acad. Imp. de Médecine for 1863.

I may be permitted to remark that the superiority of their veterinarians as operators to those of other countries, where such a course of education happily neither is nor is likely to be tolerated, is a matter which it requires more than the mere *ipse dixit* of the said professors to establish. It is, moreover, not very obvious that the good to be effected by extra skill in the performance of veterinary operations is at all commensurate with the evil of the demoralization which cannot but be produced in ignorant or ill-educated persons by the mere fact of habitual vivisection. It can be but a money advantage, and that, considering the small number of horses which require to undergo severe operations, but a very slight one. With regard to the question of accidents, it may be sufficient to remark, that where they occur in veterinary practice it is quite as likely that the victim should be one of the grooms, stable-boys, or idlers who serve as temporary assistants to the veterinarian, as to that functionary himself; and hence, unless all these persons are also to be practically instructed in the art of 'maintaining their composure throughout the struggles of tortured animals,' the art itself will be found to go very little way in the prevention of accidents. On the whole, I cannot think that any case has been made out in defence of habitual vivisection for the purpose of imparting operative skill. That the very highest degree of skill can be attained without its aid the present position of English surgery will suffice to show; and the course of instruction which is enough to produce that skill in human surgery might surely suffice for the veterinary art as well; and a trifling amount of additional dexterity is attained—if indeed it be at last attained—at too great a sacrifice, when it gives rise to the amount of animal suffering which was formerly the case in the victims at Alfort and elsewhere, and to the degree of brutalisation among the students which cannot but be inseparable from its habitual and therefore familiar and thoughtless infliction.

If we come now to the more interesting and far more important question of the relation of vivisection to the general progress of physiology, we shall find that the opinions of men of science are much divided. One school of physiologists set up vivisection as the one great means for the investigation of biological facts, and have accordingly employed it extensively and remorselessly; another school bases its hopes of the progress of the science almost exclusively upon anatomical and chemical investigation, and holds vivisection accordingly to be almost as useless as it is repulsive. A very few instances, taken from the history of science, will probably do more than a volume of argumentation to place the matter in its proper light. In entering upon this portion of the subject, however, I must guard against a misconception into which the use of the word 'vivisection' is not unlikely to betray the reader. The word, if strictly used, means, of course, the dissection of animals during life; but it is hardly necessary to say that, for the purposes of our present argument, as well as indeed of every discussion upon the subject, its meaning must be extended so far as to include all experiments upon living animals which are of a kind calculated to inflict pain upon them. It would be absurd to raise an objection to the infliction of a wound, however slight, and to justify the administration of an irritant poison, which is capable of inflicting the most frightful torments. This very subject, then, of the administration of poisons and of medicines (for no line of demarcation can be drawn between them) to animals, for the purpose of learning their effects upon mankind, is the first to which I will call attention in relation to the matter now in hand.

The objects with which drugs are administered to animals are two: viz. either to ascertain whether any particular drug is capable of destroying animal life, and if so, in what manner; or, secondly, to learn whether a reputed remedial agent has any definite physiological action, and how that action is modified by its administration in different quantities or combinations. Of the second of these objects it is needless here to speak; but the first, as is well known, is pursued not only with the view of advancing Toxicology as a branch of science, but, on certain very important occasions, for purposes of legal inquiry also; and my present business is to determine whether, in these cases, experiment upon animals is capable of affording indispensable or valuable information.

Professor Taylor, who is by no means disposed to over-value evidence derived from physiological experiments, and who agrees with other modern authorities in the opinion that its importance has been exaggerated, uses nevertheless the following language concerning it:—'There is, however, one instance[c] where evidence from experiments upon animals cautiously performed may be of some importance on a criminal trial. I allude to the case in which a poisonous substance is not of a nature readily to admit of a chemical analysis, as, for example, in substances belonging to the neurotic class of poisons. . . . In the case of Reg. *v.* Dove (York Autumn Assizes, 1856,) the proof of the presence of strychnia in the stomach of the deceased was partly based on the effect produced on animals by a prepared extract of the contents. A sufficient quantity was procured to kill several animals under the usual tetanic symptoms produced by the poison. This evidence was conclusive, and more satisfactory than the application of chemical tests to extracts of organic matter containing the poison.'

And again, on the very next page:—'A woman named Sherrington was tried at the Liverpool Spring Assizes, in 1838, for the attempt to administer poison to one Mary Byres. The evidence showed that the prisoner had sent to the prosecutrix a pudding

[c] 'Treatise on Poisons,' p. 211. Second Edition. Churchill: 1859. It is evident from the context that the learned professor means here one 'class of instances.'

by two young children. On the way the children tasted it, and finding that it had an unpleasant taste, the prosecutrix was put on her guard. The pudding was sent to a surgeon to be analysed, but he could detect no poison in it. He suspected, however, that it contained a vegetable narcotic poison. He gave a piece about the size of an egg to a dog. In twenty minutes the dog became sick, in forty minutes it lost the use of its limbs, and died in three hours. The prisoner was convicted. Cases in which evidence of this kind accidentally obtained, has been made available on charges of criminal poisoning, are now very numerous.'

Now, in the former of these cases, it is clear that had physiological experiments been deemed unlawful, one of the most atrocious criminals that was ever brought to trial would have escaped scot-free, and might have continued his course of crime to this day. Yet, here, *several* animals were sacrificed, and that under circumstances of torment which can scarcely be over-estimated. It is obvious, also, that in the case of the discovery, or the first use for criminal purposes, of any new poison, we could never become thoroughly acquainted with its nature, so as to be able either to detect crime or to remedy accidental poisoning, unless a course of experiments upon animals formed a part of our investigation into its properties.

It may, perhaps, be maintained that means may justifiably be used for the purpose of judicial investigation, involving the issues of life and death, which no less important matters would excuse; and, therefore, it is necessary to go further than thus showing that, for judicial purposes, physiological experiments are indispensable, and to prove that they are also necessary for the progress of science, and through its means for the relief of human suffering. For this purpose, I will proceed to examine only a single point in the history of our knowledge of the physiology of the nervous system. The progress of knowledge in this branch of physiology has been so great since the days of Unzer and Prochaska, as to leave no doubt in the mind of any one acquainted with its history that it is from the side of physiology, much more than that of empiricism, that physicians both have attained to their present improved practice in many of the most fearful forms of disease that afflict humanity, and that they may expect hereafter to learn to treat them with still greater success.

It can hardly be necessary to insist, in the year 1869, upon the importance of the study of physiology as a branch of science. It is acknowledged on all hands, and has obtained such a hold upon the minds of the educated classes, that within the last few years the force of public opinion alone has compelled the Universities to admit this science to a place in their ordinary course of studies; and in the science of physiology the study of the nervous system must always hold the very foremost place. Now, beyond all controversy, the physiologist to whom we owe the most decided advance that has ever been made in this study is the late Sir Charles Bell. He it was who first demonstrated the distinct motor and sensory roots of the spinal nerves, and by a brilliant series of discoveries led the way to the present position of our knowledge of the nervous system. Accordingly, in a letter published in the *Times* of August 13, 1863, the testimony of Sir Charles Bell is quoted as decisive of the fact that nothing is ever learned by vivisection. The words as cited are—'In a foreign review of my former papers the results have been considered in favour of experiments (on living animals). They are, on the contrary, deductions from anatomy;

and *I have had recourse to experiments, not to form my opinions, but to impress them on others. It must be my apology that my utmost powers of persuasion were lost while I urged my statements on the ground of observation alone.*'

And again:—'Anatomy is already looked on with prejudice; let not its professors unnecessarily incur the censures of the humane. *Experiments* (vivisections) *have never been the means of discovery;* and the survey of what has been attempted of late years will prove that the opening of living animals has done more to perpetuate error than to enforce the just views taken from anatomy and the natural motions.'

The italics are my own. Most unfortunately, the reference to the portion of Sir C. Bell's works from which this extract is taken is not given in the letter, and I have not been fortunate enough to meet with it; consequently, I do not know the date or the occasion upon which these remarks were written. Probably, however, they had reference to the attempts which were made by some Continental physiologists to subvert Bell's conclusions by means of a series of experimental vivisections. However this may be, there is evidence enough elsewhere in Bell's works both of his humane reluctance to employ vivisection and also of the fact that he felt himself compelled to resort to it, and did resort to it frequently. Thus, for example, in a general view of the nervous system prefixed to the third edition of his collected memoirs[d], he states, in explaining his great discovery of the separate functions of the roots of the spinal nerves—'It was necessary to know whether the phenomena exhibited on injuring the separate roots of the spinal nerves corresponded with what was suggested by their anatomy. After refraining long, on account of the unpleasant nature of the operation, I at last opened the spinal canal of a rabbit, and cut the posterior roots of the nerves of the lower extremities,' &c. He goes on to state how the protracted cruelty of the dissection deterred him from repeating the experiment, and how he reflected that an animal recently stunned would serve his purpose. Similarly Sir C. Bell speaks afterwards of cutting across the fifth nerve and the seventh nerve on the face of an ass (p. 26). Again, at pp. 52-3, may be found an elaborate account of some experiments on the facial nerves of the ass; indeed, the whole of this paper, presented to the Royal Society in 1821, is

[d] 'Nervous System of the Human Body,' pp. 24-5. Renshaw: 1844.

full of vivisectional experiments—asses, dogs, monkeys, being all pressed into the unwelcome service. Again, in the paper on the motions of the eye, there is contained a whole section entitled 'Experimental Inquiry into the Action of these Muscles[e],' i. e. muscles of the eye, which is, as its name implies, occupied mainly with a detail of the results produced by experiments on living animals.

It is thus sufficiently evident that Sir C. Bell had recourse not unfrequently to experiments upon animals, and these, too, of a very painful nature; and I am persuaded, moreover, that an unprejudiced consideration even of the passage above cited, as proving his low opinion of experiment as a means of discovery, will show that it bears a somewhat different interpretation. In the first place, the passage, taken as it stands, has somewhat the air of being written by a philosopher standing in some sort on his defence against a charge which, if admitted, might endanger the popularity of his doctrines; he speaks, for instance, of 'anatomy' as being 'looked on with prejudice,' just as any man might do who, in a generation less enlightened than our own, stood in awe of the babble of

[e] Op. cit. p. 156, et seq.

a half-educated society, which was ready enough to twit the medical profession with ignorance and slowness of progress, while it would, in deference to an ignorant sentimentalism, deny to its members the only practicable means of acquiring extended and accurate knowledge. Moreover, it is to be observed, that in this apology, as he calls it, for his experiments, Sir C. Bell gives a reason for their performance which, in point of fact, amounts to the assertion that they were absolutely necessary in the case of his own discoveries, and will be so in those of other physiologists. He states that he found himself unable, except by their aid, to prove his deductions from anatomy to the satisfaction of other minds; and what, I may ask, is the value of a discovery unless it can be made plain to others besides the discoverer? And yet, in the case before us, it is clear to the most limited intelligence, that by experiment alone could Bell's deductions from anatomy be proved. He had examined the spinal cord, and he found two distinct roots to each nerve, and these roots entering the spinal cord in two distinct places; the nerves themselves, too, having two distinct endowments, those of sensation and motion. He felt no doubt, that of the two roots each subserved one of these faculties. But such a conclusion as this could never have been raised above the region of mere hypothesis otherwise than by experiment. It was open to any other anatomist to maintain any other imaginable view as to the functions of the two roots; as, for instance, that they were intended by nature merely as a measure of precaution, that in case of the destruction of one, the powers of the nerve trunk might still be exercised by means of the other; or that one root was intended to exercise an influence upon the nutrition of the nerve itself, while its special powers resided in the other, or any other hypothesis of the kind. But when the experiment had been tried, when it had been shown that upon dividing the anterior roots of the nerves of a limb in a living animal, its power of motion was lost while the capacity of sensation remained, and *vice versâ;* when this had been repeated upon several different animals, always with the same result, then, and not till then, the discovery became a real discovery: thus, and thus alone was made the greatest advance in our knowledge of physiology which has been achieved since the days of Harvey. But the history of physiology will carry us yet one step further in this matter. Bell, as we have seen, was sparing of experiment, and delighted in deductions from that minute and careful anatomical investigation of which he was so great a master. He found that he could trace all the anterior or motor roots of the spinal nerves into a distinct double tract of nerve-matter forming the front portion of the spinal cord, and well known to anatomists as its 'anterior columns,' and the posterior or sensory roots, similarly into the middle or lateral columns of the spinal cord. These columns he traced up into the brain, and concluded that the anterior ones were the channels of motor power, or of the orders of the will, so to speak, and the lateral the channels by which sensory impressions are conveyed to the brain itself. Now, more legitimate deductions from anatomy than these two it is not easy to conceive. It was admitted on all hands that in the brain is the seat of sensation and of volition; it was proved to demonstration that the anterior nerve-roots subserve the purposes of voluntary motion, and the posterior those of sensation, and the anterior and middle columns of the cord seemed to supply the exact link wanting to complete the chain of evidence, and to enable us approximately to comprehend the marvellous apparatus by which our minds are brought into relation with the external world around us.

It is to be noted, however, before proceeding further, that the view of the connection of the nerve-roots with the cord, and through it with the brain, which I have just described as being held by Sir Charles Bell, was only that at which he finally arrived. He had previously believed that the posterior roots of the nerves were connected, not with the middle columns of the spinal cord, but with another distinct tract of nerve-matter called the posterior columns. This opinion was afterwards maintained by the distinguished French physiologist M. Longet, and, what is somewhat remarkable, continued to be popularly represented as the theory of Sir Charles Bell long after he had himself given it up in favour of that above described.

The fact, then, that different physiologists were thus at variance upon a point of anatomical fact, viz. whether the posterior roots of the spinal nerves are connected with the posterior or with the lateral columns of the spinal cord, is of itself sufficient to show that anatomy is quite incompetent of itself to lead us to satisfactory conclusions in regard to the physiology of the nervous system; and this becomes all the more obvious when we discover, as we pursue the history of this branch of science, that later and more minute observers, such as Stilling, Lockhart-Clarke, and Schroeder Van der Kolk, have ascertained that the posterior nerve-roots are not directly connected with either of these parts of the spinal cord, but with another portion, viz. the grey-coloured nerve-matter which exists in the centre of the whole organ. The whole question of their connection is, in truth, one of extreme difficulty and intricacy, and is yet far from being satisfactorily solved; and did we depend upon anatomy alone for our knowledge of nervous physiology, it is hardly an exaggeration to say that there would have been till very recently almost as much to be said for any one of the three views now enumerated as for any other. But exactly at the point at which anatomy becomes helpless vivisection steps in, and in a very great degree clears up the difficulty. There is certainly no living physiologist, probably none since the days of Bell himself, to whom this branch of science is so deeply indebted as to Dr. Brown-Séquard. Yet it is as an experimenter — a vivisector, if you will—that he is chiefly known to fame. He has shown conclusively, amongst other facts most important to the advancement of physiological science, that the views of Bell and Longet upon the subject of the course of sensory impressions in the spinal cord, are alike untenable, and that they in reality pass from the posterior roots to the brain itself, mainly by the instrumentality of the grey matter of the spinal cord. This he has done chiefly by means of an extensive series of experiments upon living animals[f].

It is quite true, indeed, that Dr. Brown-Séquard's conclusions are not based upon experiment alone; they derive important support from a careful record of cases of disease, of which the symptoms were accurately observed during life, and the morbid appearances noted after death; but it is by means of the experiments alone that the diseases could be interpreted. Cases of disease have been occurring and being recorded for generations and centuries, but they could not, and as a matter of fact did not, lead us to the point at which we now stand in our knowledge of physiology. Those who are

[f] See his Lect. on Phys. &c. of Nervous System, delivered at the Royal College of Surgeons, 1858.

familiar with the symptoms of disease and with the traces which it leaves upon the body after death, know only too well the disproportion which exists between them, and feel only too keenly the necessity which exists for a knowledge of physiology—a knowledge, that is, of the natural functions as well as natural structures of the organs in health, as the only possible foundation for a rational knowledge of disease, and for that scientific treatment of it to which it is the object of all but mere empirics ultimately to attain. I have been compelled, in the discussion of this portion of my subject, to enter into somewhat more technical details than I could have wished; but this was rendered necessary, in order to demonstrate, not from any merely theoretical instance, but from actual facts in the recent history of science, a proposition which I believe is admitted by almost every one who is practically acquainted with physiology and in any way engaged in advancing its boundaries, viz. this: that there are three different methods by which physiological knowledge can be pursued, viz. by anatomical and chemical investigation, by observation of diseases, and by physiological experiment; and that, in so complicated and difficult a science, we cannot afford to discard any one of these methods; on the contrary, we can only hope for any considerable advance by persistently working at all the three; and it is only in those cases in which discoveries arrived at by one method are corroborated by evidence afforded by the others that real and substantial advances have been made.

We have thus seen that there are certain instances, at least, in which science has attained to its present position partly by the help of vivisection, and in which it is not easy to see how it could have done so without that help. It is obvious that legal investigations in cases of poisoning could not be properly conducted without experiments upon animals; and in regard to Sir C. Bell's discoveries in the physiology of the nervous system, it has been shown, (1) that they could not have been proved without experiment; (2) that certain errors into which their discoverer fell were due to his very sparing use of vivisection; and (3) that later physiologists have been enabled to correct these very errors, because they have not been equally sparing in its use. It is needless to multiply examples, or I might cite further the cases referred to by Professor

Rolleston in his address to the Physiological Subsection of the British Association, delivered at the meeting of 1863; of the researches of Drs. Brown-Séquard, Pavy, and M'Donald, upon the subject of epilepsy and diabetes, as showing how in certain instances the knowledge gained by vivisection can be applied directly to the benefit of the human race. These diseases are two of the most terrible evils which afflict humanity, and anything which enables us to combat them to advantage is no mere addition to theoretical knowledge, but a relief of the direst and most hopeless forms of human misery, and so far surely worth the infliction of some amount of pain upon the lower creatures.

'Vivisection,' says Professor Rolleston, 'produces a certain amount of pain; but is this pain, voluntarily and of deliberate purpose produced in a few laboratories, greater in amount, in intensity, in duration, than the mental pain, moral distress, and bodily agony endured in many a cottage and many a palace by the victims of the very two diseases which in these last years vivisection has most assisted medicine to combat[g]?'

[g] 'Newcastle Daily Chronicle,' Friday, Aug. 28, 1863.

I am thus compelled to arrive, however unwillingly, at the conclusion that vivisection *is* necessary for the general purposes of science; and if, as I entirely believe, the advancement of science, and more especially of physiology, is of very great importance to the interests of mankind, it follows that vivisection must be permitted to go on. It is doubtless a sorry necessity, but not on that account to be ignored.

I come, in the next place, to speak of the limitations under which vivisection should be carried on, and the means which are to be used to mitigate its severities; and in so doing, I hope to be able both to meet some of the objections commonly made to its practice, and to clear up some of the misconceptions which are current upon the subject. And here I cannot probably better convey what I believe to be the true and the really humane doctrine upon this matter, than by adopting the sentiments expressed by Professor Rolleston in his address above referred to. The Professor there[h] reminds his hearers, firstly, that in a large number of vivisections, the very first step of the experiment

[h] See 'Report of British Association for 1863,' p. 110.

destroys the life of the animal, and that more quickly and less painfully than is the case where animals are killed by the butcher or the sportsman; and, secondly, that where such is not the case, in a very large number of instances chloroform, or some other anaesthetic, can be and is employed, and the pain of the operation thus avoided. The cases, therefore, in which really painful vivisection is necessary for the advancement of science are thus reduced to a minimum, and consist almost exclusively of those in which the subject-matter of the investigation is the physiology of the nervous system. In these instances, such experiments are, as I have shown, sometimes indispensable; but I will proceed further to show that such cases arise but comparatively seldom, and ought to be restricted within very narrow limits. There is a sense in which the words above quoted from Sir Charles Bell, that 'experiment or vivisection has never been the means of discovery,' are quite indisputable, and it is, I think, this sense which he must have intended to convey, viz. that the only fitting use of vivisection is for the purpose of *proof* and *confirmation* of a discovery otherwise arrived at; and that it is no more scientifically reasonable than it is morally justifiable to perform a number of experiments upon animals without a sufficient guide derived from previous investigation, and, in fact, without a clear and definite end in view. For instance, if Sir C. Bell himself had *begun* his investigations with vivisection, he would never, in all probability, have made the discoveries which raised him to the very first rank amongst physiologists. In so complicated and delicate a piece of mechanism as the animal body, one, too, in which the separate parts are so dependent upon each other and upon the harmonious action of the whole, for the power of duly performing their own offices, it is in the highest degree unreasonable to attempt to remove one organ or set of organs in order to learn, by the effect of such removal, what the functions of those organs are, until, by a long and careful course of observation during the action of the machine (i. e. during life), and of examination of the parts when inactive (i. e. after death), the experimenter has learned, as far as possible, what are the relations of the parts which he proposes to remove to the other parts, and to the organism as a whole.

Again, it may be further safely asserted, and in this I am but repeating the opinion of distinguished living physiologists, that the work of vivisection in the matter, at any rate of the nervous system, is in great part already completed. We know now, thanks to the labours of a few great discoverers, the cardinal points in nervous physiology, and beyond such points it may well be doubted whether vivisection can ever carry us. Even the most skilful operator can never be sure of the exact distance to which his knife penetrates, and the extent and violence of the operation required before the brain or the spinal column of a living animal can be brought within reach of the knife at all, is such as utterly to do away with anything like minute accuracy of experiment. Even Dr. Brown-Séquard, himself the very prince of experimenters, says on this subject:—'I must say that it is impossible to know *while* we make a section of parts of the spinal cord what is the precise depth of the injury; it is mere guess-work. But if we study well the phenomena, and then, after having killed the animal, if we put the spinal cord in alcohol we render it hard, and we can ascertain what is the extent of the incision[1].'

[1] Op. cit. p. 42, note.

By minute I mean therefore microscopical accuracy, and I repeat that it is impossible by vivisectional experiment to know which microscopical elements of the nervous tissues of the animal we destroy. But any one who is acquainted with the present position of physiology is aware that it is upon questions of the minute structure and arrangement of the elements of the nervous tissues that the points most in dispute amongst physiologists now rest, — questions therefore which it is quite beyond the province of vivisection to decide. Hence experiments of a really painful character can be required only in exceptional cases, and need be performed, indeed can be properly performed, only by experienced physiologists engaged in original researches designed to extend the boundaries of their science. It is true, indeed, that a question may be and has been raised, as to how far it is necessary or expedient to demonstrate to students the main facts in the physiology of the nervous system by means of vivisection. To this question I have no hesitation in answering that such a proceeding is in no case necessary, and therefore in no case justifiable. Those facts could, no doubt, as I have shown above, be originally proved to the satisfaction of physiologists only by means of vivisection, but when they have become admitted and received, there is no need to prove them over and over again to every successive class of students. It is true that the sight of some of the effects—for instance, of a section of some part of the spinal cord in a living animal—is likely to impress the physiology of that organ upon a student's mind much more forcibly than is the mere dissection of it after death, and the reading or being lectured upon the functions of its different parts; but there does not seem to be any reason why it should be necessary to bring this particular portion of physiology directly under his eyes any more than many others, which, as a fact, nobody ever attempts to demonstrate to students in a similar way—such, for instance, as the functions of the stomach or of the salivary glands. It is indeed very much less necessary, inasmuch as but few students can carefully attend on the practice of a large hospital for a single year without seeing cases of disease or injury which will serve the purpose of demonstration perfectly well. Such cases might be, as we have seen, quite insufficient to establish any physiological doctrine not otherwise demonstrated, and yet suffice perfectly to illustrate it when once proved and admitted. If experiments could be performed without pain to their subjects, it might be the teacher's duty to perform them before his class; but since such is not the case, the mere purpose of proving to students doctrines which it is their business as students not to judge of, but rather to learn and understand, and which can be perfectly well understood without any such experiments, cannot be considered an adequate reason for their performance. But there is a further reason also why experiments should not be performed for the instruction of classes of medical students. As a rule, they require not only great skill on the part of their performer, but also a large expenditure of patience and of time; though there is about them a certain degree of morbid excitement, they are, as a rule, both repulsive and tedious. A course of physiological lectures, if illustrated by means of experiments, would certainly occupy much more time than one not so illustrated, and the requirements now made of medical students during their three or four years' course of professional study are such that any additional calls upon their time, especially for the purposes of those subjects which are but indirectly useful to them in their future practice, is very much to be deprecated. It is quite impossible, and happily quite unnecessary, to make the great body of medical practitioners accomplished physiologists, and no one who does not aspire to become an accomplished physiologist has any need whatever either to perform or to witness vivisection. And it is hardly necessary to say, that when either performed or witnessed without necessity, they are entirely without advantage, and do but tend to demoralize those who are engaged in them. In speaking thus, I do not intend to confine the remark to experiments on the nervous system. For though it is true that these only are necessarily painful, yet the fact of witnessing such vivisections as only apparently give pain can have nothing but an ill effect upon the mind of the spectators, unless they be performed for a good and sufficient purpose, and one, moreover, in prosecution of which all the persons engaged are thoroughly in earnest. We may find here a reason why there is a wide distinction between the performance of vivisection by a physiologist in his laboratory, with merely the one or two assistants who may be necessary, and who will be in most cases persons themselves engaged in similar investigations, and their performance by a professor for the instruction of his class; in the former case, all the persons concerned are thoroughly in earnest, they are engaged in their business, and, in fact, their duty; in the latter, however intent on labour and duty the professor himself may be, a large proportion of his class will be the merest dilettanti, and the effect upon their minds will be about as elevating as is that of witnessing an execution upon those who habitually attend such spectacles.

I conclude, then, upon a review of the whole question, that vivisections are not justifiable for the mere instruction of ordinary students; that they should be performed only by accomplished physiologists; that when performed by them they need be of a painful character only when the nervous system is the subject of investigation; and finally, that, even in this last case, in the present position of physiological science, vivisections are but very seldom necessary. It thus becomes clear that the limits within which vivisection ought to be permitted are the narrowest possible; and it remains only to consider what means should be used in order to restrain its practice within those limits. Now, in this country and at the present time, there are two means by which the actions of individuals are restrained within the boundaries necessary for the preservation of public morality: one is law as expressed in Acts of Parliament; the other is that unwritten law, which is of greater force than any Act of Parliament, and which has power in regions which no Act of Parliament can reach, viz. enlightened public opinion. I do not hesitate to say that it is to the latter that we must look for the enforcement of a due moderation in the matter now before us. This might probably be shown without even taking into consideration the intense antipathy entertained by most Englishmen towards over-legislation in all forms. Yet this is not a consideration to be overlooked. Let any one consider the number and magnitude of the evils which this very antipathy persuades Englishmen to endure, and he will be able to judge in some measure how formidable an obstacle it would place in the way of any legislation which should propose to hamper in any way the actions of scientific men in the pursuit of knowledge. If Englishmen are unwilling, as they undoubtedly are, to interfere by legislation with the liberty of the subject to perform actions which can admit of no defence of any kind, much more are they likely to decline to interfere with the liberty of a small class of men, whose actions, whether good or not, at least admit of apology, and are not thrust obtrusively before the public eye. But quite apart from the general dislike of meddling legislation, there is among all classes in this country, and more particularly among the better educated of the middle classes, a strong special feeling in favour of the prosecution of physical science, and against any interference with its professors. If there is any one thing in which the mass of half-educated Englishmen undoubtingly believe, it is the connection of their material prosperity with the progress of science, and though many of them have scarcely shaken off the old mediæval horror of anatomy and anatomists, yet they would, I believe, be very slow to put any new obstacle in the way even of these unpopular branches of knowledge. The lessons, moreover, of history are not entirely thrown away; and one of these lessons most undoubtedly is the utter futility, as well as the impolicy, of putting legislative restraints upon the progress of knowledge. If we except, perhaps, the last half century, it may well be doubted whether the study of anatomy was ever so enthusiastically pursued as in times when its cultivation was attended with positive personal danger; and in days, which some now alive may remember, the necessity of 'body-snatching,' as it was vulgarly called, stimulated, much more than it hindered, the practice of *post-mortem* examinations. How greatly the feeling of the masses on such subjects has changed of late years is well known to medical men, to most of whom it has happened, that after the fatal termination of an uncommon case of disease, even among working people, the first suggestion that the body should be examined has come from the friends of the deceased.

In the earlier part of this paper, I have laid it down as a principle to be observed in judging of the questions before us, that we must take into our consideration *the existing grade of civilization*, that is to say, the general habits of society, as they respect the relation subsisting between man and the lower animals; and with a few remarks upon this subject, as it affects the desirability of legislation on the subject of vivisection, I will conclude the present essay.

The suffering inflicted in physiological experiments, I must once more remind my readers, is at least not wantonly inflicted. It is not inflicted causelessly or carelessly, or as a mere matter of convenience or amusement, but deliberately, for a set purpose, as part of a toilsome and generally ill-rewarded course of study, and by a small class of men who, as a rule, are certainly not deficient either in humanity or enlightenment. The physiologist, in this country at least, is generally also a physician or a surgeon, who spends a considerable portion both of his time and his labour in the unrequited service of the poor and suffering. Let us see whether as much can be said of the sufferings endured by the lower animals at the hands of other classes of mankind: I omit the stock instances of cruelty—the crimping of fish, the skinning of eels, and many similar customs; merely remarking that, though I believe nobody defends them, yet nobody thinks it worth his while to go out of his way to attempt to abolish them. But I am confident that any man who will use his eyes and his understanding as he goes about the world, will find that, *as a rule*, the relation subsisting between man and the lower animals is simply that of cruel oppression on the one hand, and helpless suffering on the other; or, more correctly speaking, that the welfare of the animal creation is simply not taken into account by the major part of mankind, when brought into relation with it. Of course, exceptional instances in plenty will occur to the mind of any educated man, and on the first view, as he calls to mind chiefly his own friends and their conduct towards their domestic animals, he may be disposed to look upon this charge as too sweeping; but I believe, a little further consideration will convince him that it is not so. Sights, to which we have been accustomed all our lives, do not awaken any special associations in our minds until we come to look at them from a special point of view. Let any man go into a market or a fair, in almost any town in England, and he will soon see that the one matter considered is the convenience of the buyers and sellers, and that the comfort and even the freedom from torment of the animals exposed for sale, do not seem to enter into consideration in the slightest degree. In one corner he may see a drove of Welsh or Irish ponies driven together in a mass, while one fellow cracks a long whip about the drove in all directions, and crams the frightened and half-starved creatures into a space so small that it is matter of wonder that any of them come out with whole limbs, and another seizes on an unfortunate brute that is to be shown to a customer, drags about its mouth, and kicks and drives it hither and thither, precisely as if it were a machine quite incapable of feeling. In another direction he may see a herd of oxen goaded into pens, in which all movement is impossible, and standing sweltering in the heat of an August sun, with bloodshot eyes and sore feet, and lips clogged with mucus, and suffering all the agonies of unslaked thirst. He may leave the fair in disgust,—or more probably because he has finished his business and wants to get home to dinner,—and as he drives along he will pass cart after cart containing two or three, or half a dozen, wretched calves, placed on their backs, with their feet tied tightly together with cords, and their heads hanging over the tail-board of the cart; and he may, if it so please him, reflect upon the almost unspeakable agony which is implied in the forcible retention of one position, and that an unnatural and painful one, for many hours in succession. Such examples crowd upon the memory upon the very slightest consideration of the subject, and I have even now said nothing of the fate of costermongers' donkeys, or of the methods employed by butchers for destroying life—though on the latter subject there is plenty to be said—or of a hundred other matters of the kind. But to take an apparently milder instance; let any of my readers reflect, the next time his convenience compels him to hurry to a railway station, as he hears the swish, swish, thwack, thwack, of the cabman's whip, in relation to how much bodily pain that must stand, when it hardly serves to force along the aching limbs and half-filled stomach of the unhappy horse at the rate of some six or seven miles per hour, and he will perceive how very slight account we mostly take in the commonest transactions of life of mere animal suffering. I do not say that these things are as they should be, nor do I even assert that some of them, at least, might not well be checked by legislative enactment; I merely bring them forward in this place, in order to show how continually the ordinary machinery of our lives produces animal suffering, and how much of this is brought about from mere carelessness, or as a mere matter of convenience, and how, for the most part, in our present stage of civilization, we are habitually indifferent to it.

There is, however, one other custom, or class of customs, which is looked upon as peculiarly English, to which I must refer in illustration of this matter, viz. sporting and game preserving. Fox-hunting, the first of English field sports, is certainly also the least cruel, and may probably with justice be acquitted of the charge of cruelty altogether. For while no one who has had any experience of it can doubt the correctness of the old huntsman's opinion, that 'the horses and the dogs like the sport as well as the gentlemen,' there is some reason to hope, if the principles laid down by Mr. Rowell be correct, that the fox, if he does not 'like it too,' has at least no very valid reason to object to it, inasmuch as it is almost the only way in which, in the present day, he can meet his inevitable death, in what Mr. Rowell has shown to be the most natural and least painful manner. The only cruelty therefore remaining is the constructive cruelty of preserving foxes for the purpose of the chase. Horse-racing occupies a somewhat more exceptionable position, since it is hard to justify the severe and cruel punishment so often inflicted on horses towards the end of a race. But whether this can be defended or not is a matter of little importance; for most undoubtedly the typical form in which the modern English sporting instinct developes itself, at least among the wealthier classes, is that of shooting; and the cruelty of shooting cannot well be questioned. The birds that are shot and bagged are killed out of hand, and it makes but little difference, for our present purpose, if an animal is to be killed for the table, whether it is shot as it flies over a stubble-field or has its neck wrung in a farmyard; but not all the birds that are shot, it must be remembered, are bagged. There are birds with broken legs and broken wing-bones, which flutter down into the cover, and which the retriever fails to find. And what is the condition of these? Why, they may be picked up, as we are told by sportsmen, days and days after they have been shot, and you may find the broken limbs in every stage of inflammation, swollen and red, and tender and suppurating, and the animal dragging itself about with what limbs it has remaining, and suffering all the time the additional agony of starvation—an agony probably in its case much more severe than that arising from its wounds. It is to be observed, too, that by keeping down, as we commonly do, all beasts of prey in our woods, except, perhaps, an occasional fox, we deprive the wounded creatures, in most cases, of their rightful refuge in a natural and merciful death. The case of a winged bird is, in fact, precisely that of an animal submitted to vivisection without any provision being made either for the extinction of sensibility during the process or of life at its termination; except, indeed, that in the one case the experiment is made carefully and for a definite and useful purpose, in the other carelessly and for sport; the one is performed perhaps once, where the other occurs a thousand times.

Again, it is a common habit in the present day for gamekeepers to set snares for rabbits in all directions in a gentleman's park, and the habit proceeds apparently unchecked, yet any one has only to ride by and see the rabbits caught in order to perceive that it is hardly possible to inflict more suffering on an animal of the kind, whether in the shape of terror or of bodily pain, than is done by this means.

Now, I do not intend from all this to proceed to the conclusion that it is expedient, or even desirable, that we should immediately make an attempt to put down the cruelty of shooting and game-keeping by legislation; but I do consider that even this hasty and imperfect survey of facts is sufficient to show that, whether rightly or wrongly, it is not the habit of our present age to put the sufferings of the lower animals into serious competition with the convenience, or even with the pleasures, of mankind; and that while for these purposes suffering is inflicted habitually and as it were wholesale, and without care or check, by all classes of men, gentle and simple alike, it would be the very height of absurdity to endeavour to check by legislative enactment its infliction by a small class of enlightened and laborious men by whom it is practised but rarely, and even then not wantonly or for amusement, but with care and pains, and for no less a purpose than to extend the empire of human knowledge, and to diminish the amount of human suffering. Surely, to make any such attempt were indeed to begin our reforms at the wrong end of our social system, to strain at a gnat after swallowing innumerable camels.

ESSAY VI.

PHYSIOLOGICAL PSYCHOLOGY.

'A MAN'S body and his mind, with the utmost reverence to both I speak it, are exactly like a jerkin and a jerkin's lining;—rumple the one, you rumple the other.' Such was the philosophy of a shrewd observer of men a century ago, a contemporary, therefore, of Berkeley, of Hume, and of Reid, and it may fairly be doubted whether he had not approached, after his own fashion, at least as nearly to the truth as any of the professed metaphysicians. To say that psychology has been and is unprogressive, that amongst all the progress and advance in other departments of human knowledge, this, the most interesting of all, the most important of all in its scope and its consequences, remains to this day where Plato left it, has become a mere commonplace. Or if, indeed, we flatter ourselves that this science too has in our day at last begun to move forward, like so many other branches of knowledge stationary hitherto, we are told that it owes its advance not to the metaphysicians or psychologists *ex professo*, but to the physiologists, not to introspection or the interrogation of consciousness, but simply to the scalpel and the microscope, the reagent and the balance; in other words, that if we have learned anything at last of the human mind it has been learned from the side of the body, by giving up the attempt to study mind as such, and working at the anatomy and physiology of the nervous system. Such is, in the main, the doctrine which Dr. Maudsley puts forth in his now famous work[a]. My object in what follows will be to put forward, in the first place, a short statement of what appears to me to be the present position of our knowledge of mental physiology, and of the method of studying it which promises the best result at the present day. In my pursuit of this object it will appear how far my conclusions agree with Dr. Maudsley's, and in what points I differ from him.

Before, however, I enter upon this task, there are

[a] The Physiology and Pathology of the Mind, by Henry Maudsley, M. D. London. Macmillan. 1867.

two remarks which I feel called upon to make—one in justice to Dr. Maudsley, the other for the purpose of defining my own position. In the first place, then, it is only right to say that Dr. Maudsley's book is written with a double aim, and has two quite distinct characters. The first part is a treatise on the physiology of mind, and it is to this exclusively that I intend to confine my attention; but the second part may, in the author's words, 'stand on its own account as a treatise on the causes, varieties, pathology, and treatment of mental diseases, apart from all question of the proper method to be pursued in the investigation of mental phenomena.' It is to be understood, therefore, that it is not because I undervalue this portion of the work that I leave it unnoticed on this occasion, but simply because I have taken quite enough in hand in the above programme without it, and to enter fairly upon the questions of the pathology and treatment of insanity in addition would lead me too far afield.

The other purpose for which I desire to detain my readers for a moment further is to enter a protest in plain terms against any deductions in theological matter which may be made from propositions which I lay down in physiological. I decline in the most positive terms to look at these questions from a theological point of view, or to admit that any facts which I may point out involve any theological conclusions whatsoever. 'Da fidei quæ fidei sunt.' Neither religion nor science has ever profited, or ever will profit, by a half-hearted and dishonest habit of estimating facts not exclusively and fairly according to their own value, but always with a collateral view to their effect upon some dogma of the schools much more warmly cherished than clearly understood. Science has not profited, for to this habit we owe all those demi-scientific attempts to 'reconcile geology with Genesis,' &c., which have done so much to foster a thoroughly unscientific tone of mind among our countrymen; and still less have the interests of religion been advanced by it, since the successive collapse of such attempts has given rise to the notion of a perpetual antagonism between religion and science, in which the former is being gradually driven from each successive line of defence. No proposition which can be advanced about the relation of mind and matter can ever be more subversive of the popular theological notions of the nineteenth century than were the astronomical propositions of Galileo of those of the sixteenth; yet doubtless there have been as good Christians since Galileo's time as ever there were before; and I would further remind my readers, in the words of Sir W. Hamilton, that 'religious disbelief and philosophical scepticism are not merely not the same, but have no natural connexion[b].'

In discussing the right method to be followed in the study of mind, Dr. Maudsley makes himself merry with the divergent results at which philosophers have arrived by the methods of interrogating consciousness, whether introspectively or psychologically. Whether he is justified in so doing must be determined by the success or failure of the positive side of his argument. If he can show that by the use of a new method more consistent results may be obtained, he is so far justified in asserting that the older one is worn out and discredited; but to bring forward the differences of professors as a general reason for discrediting the branch of knowledge which they profess, is to employ a weapon which turns every way, and is most undoubtedly a very efficient bar against all real approach to the

[b] Appendix to Lecture, i. 394; see also Mill upon Hamilton, p. 139.

tree of knowledge. It is but the most elementary facts in any science which meet with immediate acceptance, or at least in any science which has not reached the deductive stage; and the physiologists are not less obnoxious to such reproach, if reproach it be, than the psychologists themselves. Of the truth of the charge, however, as applied to the latter, there can be no doubt. While all alike agree that the witness of consciousness must be received as final, there is a never-ending dispute as to the facts to which it bears witness. One school of philosophers hold with Hamilton, that consciousness testifies directly to the existence both of the *ego* and the *non ego* — the mind itself and the external world; others affirm with Mill, that we are conscious only of the modifications of the mind itself; and others again, like the late Professor Ferrier, look upon both these opinions as untenable, and indeed, as self-destructive, reject all analysis of perception, and hold a view almost indistinguishable from that of Berkeley — viz. that matter, and the perception of matter, cannot be divided in thought.

If results so divergent as these are all that psychology can give us, we may at least reasonably look round to see if physiology cannot do more for us; and if any further justification were needed for so doing, it might be found in the phenomena which the most superficial observation of facts around us brings under our notice, almost whether we will or no.

The very slightest consideration of such facts is sufficient to show us, not merely the intimate relations which subsist between bodily conditions and what are commonly spoken of as states of mind, but, further than this, how precisely analogous mental results may be produced by conditions which we hear called in one case purely mental, in another purely physical, and might therefore serve to suggest that something might be learned from the side of the body as to the conditions at least of the operation of the mind. Thus we perceive that the imbibition of a given amount of alcohol often produces exactly the same effect on a man's mental state as does the reception of a piece of good news; or again, that the sudden announcement of a terrible calamity will affect some persons much in the same way as will a heavy blow on the head, or an overdose of opium; or again, that a nauseous smell or a disgusting sight will bring about the same condition as a rapid loss of blood. Coming to instances slightly less obvious than these, I may remind the reader of the existence of well-established cases in which raving madness has resulted from the presence of a splinter of glass in the foot, or the absorption of a poison by the blood. Of this class of instances some of the most remarkable and suggestive may be found in those cases which have been known to physicians ever since the days of Sydenham as occasionally occurring in districts affected with marsh miasm, in which, instead of the ordinary symptoms of intermittent fever, persons have been attacked with perfectly well-marked mania which has intermitted and recurred with the same regularity as the ordinary ague, and has yielded to the common remedy with as much readiness as the fever itself in the other cases in the neighbourhood. Sometimes, too, the attack of insanity begins after the ague has lasted some time; the ordinary symptoms of ague suddenly disappear, and the maniacal attacks come on at the precise intervals at which the paroxysms of fever should have appeared. In such occurrences as these we see two effects, a physical and a psychical one, brought about by the same material agent, and the one taking the place of the other with a regularity and completeness which remind one of nothing so much as of that substitution of one elementary substance by its equivalent of another which occurs in a chemical decomposition. I will mention one other instance of affection of the mind by the condition of the body, because, while even more familiar to most persons than some which we have already noticed, it is pre-eminently one in which no other than a purely bodily cause can be assigned for the production of a mental effect. It is a matter of common experience, that while persons affected with certain classes of disease suffer the most terrible depression of spirits and dejection of mind, those subject to other complaints are almost invariably cheerful and hopeful. Thus while a consumptive patient is almost always hopeful to the last, and generally in good spirits throughout his illness, another suffering from diseased liver and jaundice adds much to his own sufferings by perpetual depression and gloomy forebodings. Now, in such cases, and they are so common as to be almost proverbial, there is simply no difference in psychical conditions to which we can refer the obvious difference in psychical results. The pain and uneasiness to be endured in the one case may be by no means less than in the other, the prospects of recovery may be far worse in the pulmonary than in the hepatic disorder, yet the result remains the same; the man ill of the former will be, as a rule, cheerful; the one suffering from the latter will be, as a rule, wretched and despondent. Now, however much of the pathology of such diseases may yet remain to be discovered, of this we can have no doubt—viz. that the constitution of the blood is altered in both cases, and altered differently in each; and from this alteration of the bodily conditions it results, that while what make up ordinarily the psychical circumstances of the two patients, i. e. prospects of recovery, social and pecuniary condition, family affection, domestic comfort, freedom from anxiety, &c., remain the same, the psychical phenomena presented by the two will differ, and differ withal in accordance with a fixed law which admits sometimes of being predicted when we only know the name of the disease to which they are victims.

If, then, such facts as the above are open to ordinary observation,—and they might be indefinitely multiplied,—it is only surprising that philosophers should have been detained so long from undertaking the study of mind from the side of physiology, and that they should admit the conclusions arrived at by physiologists so tardily and grudgingly as they have hitherto done. Into the causes of this reluctance on the part of philosophers to make common cause with the physiologists I cannot now inquire at length, but I may enumerate three which appear in different ways to have conduced greatly to this result. 1st, The natural conservatism of mankind by which all but men of real power, on the one hand, or mere coxcombs on the other, are constrained to follow in the road which has been already well worn before them; 2nd, The ease and comfort with which a man can sit in his easy-chair, and read books, and spin theories, and write annotations, as compared with the labour and discomfort, and the many disgustful incidents with which he must be prepared to meet, if he will ever become practically acquainted with the researches of physiologists, and far more if he will devote his personal efforts to extend the boundaries of the science. 3rd, The morbid dread of theological error to which I have before adverted. Probably the most potent of all.

Having now indicated very shortly a few of the phenomena within the range of every observer, which serve to demonstrate the close interdependence which subsists between the condition of the body and the action of the mind, and to suggest, therefore, that something may be learned concerning the latter by a careful investigation of the former, I proceed to place before my readers the results, in the shape of observed facts and legitimate inferences from those facts, which the careful study of the anatomy and physiology of the nervous system has actually added to our knowledge.

Anatomy, then, has shown in the first place that in all animals whatsoever, which possess a nervous system at all, whether in the simplest or the most complex form, that system consists of two elements, both in structure and function diverse from one another. These are (1) white matter consisting of fibres of a peculiar structure, and (2) other matter of a grey colour[c], consisting of a mass of granules

[c] I have used throughout the term 'grey matter' in speaking of the vesicular nerve matter. It is the term in common use among anatomists, and is sufficiently correct when vertebrate animals are spoken of; but it is necessary to note that the vesicular matter is not by any means distinctively grey in invertebrate animals, a fact which adds considerably to the difficulty of observing the nervous system in the lower creatures.

and cells of various shapes and sizes, the latter having numerous branches thrown out in all directions, and now known to become, in many instances, continuous either with similar branches of other cells, or with some of the strands of the white fibres already noticed. It has further shown that the white fibres are to be found in all parts of the nervous system, that their office is simply internuntiant, and that no fresh force is ever generated by them. The grey matter, on the other hand, is placed in masses of various size and form in definite portions of the nervous system. It is highly probable that wherever such matter exists, there is a true centre of nervous force, and quite certain that where it is not there is no such centre; no fresh nerve-force is ever produced without the agency of the grey matter. To employ the well-worn illustration of the electric telegraph, than which none better can be used, the grey matter resembles the battery at the station, and produces force of a particular kind and degree, the white fibres are precisely analogous to the telegraph wires which propagate the force generated by the battery to a distance, but produce no force themselves.

In order to render this portion of the subject intelligible to those of my readers who are not anatomists, it is necessary to attempt a slight sketch of the principal forms of the nervous system, as it exists in various classes of animals. It is, of course, impossible to render this complete, and it may be well here to state that I omit all reference to the whole of those very various and dissimilar forms of life which make up Cuvier's sub-kingdom radiata, as well as some of the lower forms of mollusca. This I do, not because these creatures are destitute of a nervous system in all cases, but because the existence of a nervous system in many of these is still doubtful, observation of them is extremely difficult, and it can hardly be said that enough is certainly known about them at present to render it obligatory upon us to take them into consideration in forming any general conclusions in regard to the physiology of the subject. It is, however, only right to remark that indications are not absolutely wanting of possible discoveries in this direction, which might necessitate considerable modifications in the views generally entertained as to both the development and the physiology of the nervous system. In many creatures of the classes of which I am now speaking (e. g. in planaria) there are eye spots ; these we can hardly suppose to be other than more or less rudimentary organs of special sense, and it is hard to conceive the discharge of the functions of an organ of special sense without the existence of a nervous system. It has even been suggested by Professor Rolleston, that the nervous system is developed gradually, as it were, to meet the occasion for its use, and that the order of its development is from without inwards. Thus, for instance, in some infusoria, where there is no distinct differentiation of tissues, there are to be found certain granules of pigment which must manifestly be affected by light in a different manner from the remaining mass of the animal's body. In other instances in the same class, we find a small transparent highly-refracting body—in fact, a lens—in the midst of the pigment granule, and the next step, as it is suggested, would be the differentiation of a portion of tissue in immediate connection with such bodies, in order to take cognizance, as it were, of their affection by light, and communicate it to the organism at large; the tissue thus differentiated would be, in fact, a rudimentary peripheral nervous system, and thus the whole creature would become more sensitive to the stimulus of light, and be raised in the scale of organic life. Now, if we suppose further that the particles of matter upon which such creatures live, are more numerous in light than in dark portions of water, or that the light, as it is reflected from them, will, if it can be perceived by the animalcules, be a guide to the portion of water in which they abound, we have at once a reason, upon Darwinian principles, why such an advance in organization should gradually take place.

Returning from this digression, I will begin our review with the simple case of the nervous system as it exists in one of the lower mollusca, the ascidian, or common sea-squirt. This consists of a small mass of vesicular matter, or ganglion, as it is called, with two simple cords of white fibre. The mode of action of this simple arrangement is as follows. When any neighbouring body touches the tissues in which these cords are distributed, one of them, called the afferent cord, instantly propagates the irritation upwards to the ganglion of grey matter; thence it is reflected back along the other or efferent cord to the muscles to which that cord is distributed, and by these the movements required for the benefit of the organism are forthwith per-