NEW THEORY

AND

OLD PRACTICE

IN RELATION TO

MEDICINE AND CERTAIN INDUSTRIES:

BEING AN ANALYSIS OF

CURRENT LITERATURE OF THESE SUBJECTS.

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PREFACE.

NEARLY fifty years ago an emiment Physician wrote, that:— "In the sciences which deal with the powers of living bodies there is a great temptation to grasp at premature inductions and when such have been brought forward with confidence there is often difficulty in exposing their fallacy, for in such a case it may happen that as long a course of observation is required for expressing the false conclusion, as for ascertaining the truth. (*Dr. Abercrombie on the Intellectual Faculties*, 1837, p. 18).

And what was then true has obtained further confirmation by means of investigations that have been pursued in the interval which has meantime elapsed. Modern science is indeed said to take nothing for granted; its conclusions to be the results of experiment and analysis; its teachings to be based upon facts, and facts only; its methods alike, whether the subject of processes be in organic matter or living organisms. Adopting a similar method of procedure, I have, in the following pages collected, and placed in comparison with each other, various records of investigations pursued with regard to the subjects dealt with. And with what result? Undoubtedly that the statements adduced are in almost every instance mutually contradictory, and that they neutralise the significance of each other; but it has happened with regard to every point examined, that, as expressed half a century ago by Dr. Abercrombie, "as long a course of observation (and of comparison) has been required for exposing the false conclusions (arrived at by a particular class of investigators) as for ascertaining the true."

An important and suggestive fact brought into prominence in the course of the comparative analysis which constitutes the bulk of this brochure, is, that not more than a small minority of the medical profession appear to advocate the method of research therein discussed; a still smaller minority support the "conclusions" arrived at by means of that method.

With regard to matters purely medical, it is shown that, for the most part, results obtained experimentally in animals are inapplicable in the case of man, whether in health or labouring under disease or accident; in certain instances conclusions attributed to that method could equally well have been obtained by clinical and pathological observation. In fact, as also expressed by Dr. Abercrombie, at the date already mentioned:—"By means of observations respecting the phenomena of disease, and the morbid appearances in cases that are fatal we shall be able to trace the relation between symptoms and the nature and seat of disease. A full collection of uniform and essential facts on these subjects is the only true foundation of medical science. Whatever leads the mind from the importance and the difficulty of this investigation is injurious to medical science." (p. 419).

The claims in favor of zymotic inoculation based upon statements with regard to animals "protected" against disease by this means, and of money value represented thereby are shown to be unsupported by the statements and reports on those points adduced.

Practical men engaged in the several industries mentioned in the text are directly opposed to the measures employed in reference to those industries in accordance with certain theories which are noticed in detail. They declare that heavy pecuniary losses, and in several instances actual ruin to the persons most concerned have resulted from the adoption of those measures; that they are therefore being abandoned in favor of methods, the suitability of which for their purpose was indicated by ordinary experience.

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MODERN PHYSIOLOGY.

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I. Introductory Remarks.

The subject of "Modern Physiology," otherwise "Experiments" on living animals, has taken its place among the public questions of the day. It is discussed on public platforms, in journals non-professional as well as professional, and even in private society. It is considered in reference to its relation, actual or assumed, to utility, to "science," or knowledge, to public ethics, and to religion; more recently still, it has advanced its claims to be included among the tenets in regard to which political candidates are called upon by constituents to express their individual opinions.

It is obvious that a question which has attained such extensive development and ramifications, demands careful and serious consideration from correspondingly numerous and wide points of view; and further, that much time and labour must be given to the inquiry by whoever is prepared to undertake it on the lines thus indicated. It is no less obvious that this time and this labour cannot be spared by men engaged in the daily work and struggle of life, and that therefore a full and free inquiry in reference thereto comes best within the province of those who have sufficient leisure carefully to compare and balance against each other the very definite statements which occur on either side of the general controversy. Inasmuch, however, as the very extent of the subject is such that the ramifications already mentioned require to be followed one by one, the circumstance immediately presents itself that only by this method of treating it can its general bearings be satisfactorily exhibited.

In this pamphlet I take up and consider one such point, and it, one to which public as well as professional attention has recently been drawn in a very marked way. Without personally having anything whatever to gain or to lose by this controversy, and fortified by the knowledge obtained from authorities whom I quote in the succeeding pages that in it, men of eminence are arrayed on either side, I proceed to compare the different views taken by them respectively, according to the method usually adopted in the analysis of contending evidence in regard to any other question, whether in its nature, abstract, or matter of fact.

2. "EXPERIMENTS" IN RELATION TO THE HEALING ART.

The obvious purport of the communication to the *Times* (September, 29, 1885) in reference to which the following remarks are made, was to indicate generally, that by means, and as a result of certain "experiments" on living animals definite gain has accrued to practical medicine and surgery, and thus to the best interests of humanity. Equally obvious is the corollary though unexpressed, that therefore, a continuance of that method of research will result in still greater benefits to the healing art, and through it to mankind.

The points thus submitted deserve very careful consideration; and it is hoped and intended now thus to deal with them. Accordingly, it is proposed to take, item by item, such statements in that communication as admit of being so discussed, and balance them with and against other statements on the same points which occur in the writings of acknowledged authorities on the subject referred to. The statements to be thus dealt with are respectively numbered, and included within inverted commas; those having reference to them follow under each head respectively, namely:—

(*a*) *Medicine.*

1. "Several important researches (in practical physiology, otherwise vivisection) now being carried out by British investigators in Continental laboratories bid fair to exercise a notable influence upon medicine and surgery generally."

The terms of the above statements are vague and leave unspecified the particular "researches," and the kind of influence alluded to. By the light of the past, however, we are justified in forming an estimate of the future. With reference to the degree and nature of the influence exerted on Medicine by "Practical Physiology" the following statements made by authorities who are entitled to confidence are here quoted. namely:—In 1817 Mr. Lawrence wrote to this effect:—In reference to biology or the science of life—that descriptions of particular animals, and surveys—are not so much wanted at present as the assemblage and assortment of facts already accumulated, and the employment of them. It is employment, and not mere possession, that gives a value to intellectual as well as material wealth. We have had workmen enough to toil in the mine and the quarry;—we now only wait for the architect who shall be able to employ them in constructing a temple suitable in majesty and simplicity to the Divinity, whose shrine it is destined to contain. (Lectures on Man. p. 47). Since that date experiments have extended and multiplied, and still the temple here indicated remains unbuilt, materials, such as they are, have accumulated, but no architect has as yet succeeded in giving them definite form. Living dissections are either prolific in disseminating error and doubt, or are entirely barren of useful results. (Two Prize Essays, p. 79). They have been the cause of introduction into medical practice of many serious errors, for the reason that there is no real analogy between man and animals in a physiological point of view. (Three Essays, pp. 67, 280). The conclusions (from experiments) are doubted, disputed, and contradicted by experimenters themselves. (*Nineteenth Century*, Feb. 1882.) They have contributed to healing diseases nothing but false paths and wrong roads. (Dr. Garth Wilkinson.)

The most relentless practical physiologists are forced to acknowledge that they are helpless against "innumerable" diseases; citizens who have the advantage of the best medical skill enjoy no better health than do residents in the country. (M. Scholl.) The suggestions of practical physiologists contradict each other, and a physician in practice does not act upon them. (Dr. Berdoe). Not only is it (namely, "practical physiology") devoid of good results, but it has exercised a terribly sinister influence in diverting men's minds from legitimate paths of study, namely the bedside of the patient, and the pathological theatre. (Dr. Bell Taylor). It has added nothing whatever to the physician's power of healing. (Dr. Pritchard). The attempt to transfer to medicine, results obtained by physiology is essentially disastrous to medical science. (Claude Bernard, Int. to Study of Exp. Med., p. 549). The whole of physiological science is so encumbered with a mass of contradictions that people are apt to say there is nothing found out in physiology. (Royal Commission, Q, 4238). A great deal of what is termed physiology has turned out to be a mistake; it has got mixed up with our notions of disease to a deplorable extent, and has hindered the progress of practical medicine. (Vivisection, Gimson, p. 199). It lies outside the path of men who are engaged in the routine of daily practice. (Ryde Address, 1881).

Theoretical instruction is all very well, but in this country practical medical teaching is in a great measure ignored, and time wasted in cramming the heads of students with learning which they quickly forget, and which for the want of practical knowledge they cannot apply. (*British Medical Journal*, Sept. 30, 1882). As expressed by Abernethy, it is spending our nights in study, and our days in the observation of human diseases which can alone enable us to understand, alleviate, or remove them. (Literary Characters, ed. iv. p. 196). Our security is in the recognition of indications at the bedside. (Harveian Oration, 1880 p. 176). Medicine is not a science, and never can be; it is the practical application of experience, and the best physician is he who to the largest experience adds the most extended knowledge of the experience of others, with the highest faculty of deducing therefrom rational inferences, and the skill to put them into practice. (*British Medical Journal*, Oct. 25, 1884.) In our eagerness to reach an ideal standard of perfection, we are too apt to forget that the medical art is much older than what we call the science, and of independent origin. Medicines and cures, says Celsus, were first found out, and then, after, the means and causes were discovered; and not the causes first found out, and by light from them the medicines and cures discovered. This distinction still holds good. In ultimate result it is upon intuition, rather than upon syllogism that the solution of many medical problems depends. The great variety of subjects, and the immense number of empirical facts, which the present system of medical education demands, is to be regretted, because this state of things tends, with increasing force, to the study of isolated phenomena, and small and narrow domains. It causes the knowledge of Nature, as a great and comprehensive whole to be sadly neglected. Nearly all the subjects that medical examinations embrace may be discarded immediately the ordeal is over, without detriment to any future professional prospects. (*British Medical Journal*, August 8th, 1885, p. 282).

The separation between (experimental) scientific physiology and the practical work of medicine is illustrated by the advice reported to have been given by a professor of physiology to medical students namely, that they should " reside for a time in hospital." With reference to that remark the observation occurs that " it is most unlikely that any considerable number of men will deprive the practice of the profession of brilliant guides by espousing the cause of pure science. (Medical Press and Circular, October 21, 1885). It is thus made evident that "brilliant guides" of the practical parts of the profession need not "scientific" men, otherwise experimenters. Dr. Chapman, in alluding to the numerous and opposing theories which exist in reference to every point connected with the etiology of cholera (notwithstanding the numerous experiments performed on animals with the *bacillus*, &c., writes:—Considering the divergent views of the delegates (to the International Commission) an impartial observer would probably conclude that the several

Governments who appointed them had assisted to excite the contempt of Europe for the so-called "medical science" of the last quarter of the nineteenth century. (Cholera Curable, pp. 69 and 86).

While engaged in preparing these notes I carefully examined the contents of a recently published text-book on physiology. According to my view, the greater part of that work is occupied with details of experiments on animals which have no relation whatever to the healing art; the references to those subjects which do bear such a relation include particulars which were long ago ascertained, and could have been so perfectly well in the absence of "experiments" on living animals. "Modern Physiology" being thus separated from practical medicine, the question presents itself for consideration by the responsible authorities, how far is it expedient that time and attention of medical students should be taken up with studies and investigations which give no help in regard to the actual purposes of their intended profession, to the necessary omission and neglect of such as are essential to that end?

(*b*) *Surgery.*

With reference to the claims in favour of "practical physiology" in relation to surgery the following statements, out of many similar in purport are here given, namely :—The knowledge of surgery possessed by Sir W. Fergusson was derived according to his own evidence, from clinical and pathological observation. He was not aware that "experiments" had led to the mitigation of pain or improvement as regards surgical details; neither did he see of what value to surgery such experiments could be,—the abnormal conditions produced by such experiments must render their results "no great value." In surgery, vivisection has been more used to confirm results than to make discoveries; few great surgeons have been vivisectors. (Qns. 1024 to 1034). The most striking experiments on animals referring to surgery were previously performed on the human subject, and were therefore unnecessary. John Hunter's most brilliant performance was done on the human subject by him, and years afterwards was copied on animals; no great vivisector has made himself a great surgeon by vivisection. As his own practical knowledge of surgery increased, his opinion of experiments became less than it was before he had attained a 'grasp' of his subject. (Evidence of Sir W. Fergusson before the Royal Commission, 1876). Modern surgery has not been uniform in its progress;—its advance materially affected by the prevailing bias of the day. Anatomical at one time, physiological at another, influenced in one direction by the mechanical spirit of the age, in another by the advanced pathology. (Mr. Erichsen, Med. Congress, 1881). Experiments on the lower animals have led to conclusions seriously at variance with well established facts of clinical and pathological observation. The decisive settlement of such points must depend mainly on careful clinical and pathologial observation. Experiments have led to different views in different hands. (Ferrier, Functions of the Brain, Preface). The claims adduced in favour of certain advances of medicine and surgery being attributed to vivisection have been so because some experiments were mixed up in the history of each instance, but no attempt is made (in the instances being replied to) to show that the advances were due to that practice. The whole progress of abdominal surgery dates from the first successful case of ovariotomy performed by Houston, in 1701. In 1867 Baker Brown, disregarding all the conclusions of experiment, showed us how to bring our mortality of ovariotomy down to 10 per cent. In 1876 Keith showed that it might be still further reduced. The methods of this reduction were such as only experience on human patients could indicate, experiments on animals could and did teach nothing, for operations have been performed on animals every year for centuries, and nothing whatever has been learnt from this wholesale vivisection. (Tait, Uselessness of Vivisection). But now science jostles Practice, as Quackery did previously. (Harveian Oration, 1880, p. 80).

The fact is acknowledged that by a class of persons, influential by reason of the position of some at least among them, an endeavour is being made to demonstrate a connection between the advance of medicine and surgery, and the performance of "experiments" on living animals. But according to the particulars thus given the inevitable conclusion to be arrived at is that no such connection has been established; and also, that whatever influence has heretofore been exerted by "practical physiology" upon medicine and surgery has been pernicious rather than beneficial. Judging therefore of the future by the experience of the past there are no grounds for the hope above expressed, that benefit to the healing art generally will in the future accrue from further prosecution of researches of the kind, and in the manner indicated. But another fact is made apparent by the analysis now given, namely, that the strongest and most definite evidence adduced against the claim alluded to is that which occurs in the evidence and writings of practical physiologists, practical physicians, and practical surgeons. Strange as the circumstance is, it is none the less true that the chief advocates of "experiments" are non-medical men, and the chief organs which support that practice are non-medical journals.

(*c*) *Therapeutics.*

2. "There never was a time when the practical utility of physiological experiments upon animals was more demonstrable (than the present). Therapeutics and surgery are destined to gain enormously from the same source."

A careful examination of the evidence and arguments by capable and practical men quoted in the preceding paragraphs disposes of the advocacy of those experiments on the plea of utility above adduced. It has been clearly demonstrated that in regard to medicine and surgery, the plea there stated is opposed alike by physicians, by surgeons, and even by advanced "practical physiologists." Science has nothing to do with utility; some animal suffering is nothing as compared with one "scientific discovery." (*Revue des Deux Mondes*, Feb. 15, 1883, p. 835). Nor is utility always the object for which the experimenter performs his experiment, what he seeks is knowledge. (*British Medical Journal*, Dec. 17, 1881). The public must not imagine that every new physiological "truth" necessarily adds to the resources of medicine and surgery. (id., June, 7, 1883, p. 1100).

Adverting to the reference made above to the prospective gains of therapeutics from "physiological experiments" the following statements on the subject of such experiments are here given, namely:—It is not easy to see the connection as an antecedent and result between physiological experiment and treatment of disease which may be founded on it. (Two Prize Essays, p. 98). The action of remedies can only be definitely ascertained by observation; and experiments on animals are more likely to mislead than assist in gaining this definite knowledge. The only certain knowledge of the influence of substances on the human subject must be obtained by observation (of cases in private or hospital practice), and experiments on animals are more likely to mislead than to assist in gaining this definite knowledge. (Three Prize Essays, p. 48). Experiments on animals have been extensively conducted with drugs, the properties of which are perfectly well known; but in no city in Christendom does a single medical man treat disease (simply) from a knowledge of remedies derived from "experiments" on animals. (Dr. Bowie's Lecture). The experiment (with drugs), must be tried on man before a conclusion can be drawn (from experiments on dogs). (R. Com. q. 2996). This method of ascertaining the effects of medicines, and applying the results so obtained, by analogy, to the human frame is rendered fallacious in many instances, in consequence, not only of differences of the digestive organs, but of the organisation of the nervous system in man and animals. Six ounces of tartar-emetic have been given to horses without producing any remarkable or permanent derangement of the principal functions; a few grains of the same drug will cause almost immediate vomiting in dogs; they will take large quantities of arsenic with simply the effect of rendering them sleeker and fatter; and they have been known to eat eight pounds of belladonna leaves without any ill consequences. The construction of the stomach or stomachs of Ruminantiæ renders any deductions drawn from medicines exhibited by mouth to them little to be relied upon; besides which some animals will eat with perfect impunity substances which prove poisonous to man. (Pract. Therapeutics, by Waring, Introduction). The inhabitants of the Tarantino keep black sheep alone because the *Hypericum crispum* abounds there, and this plant does not injure the black sheep, but kills the white ones in about a fortnight's time. (*Edinburgh Review*, October, 1884, p. 472). Guinea pigs get fat on belladonna. (*British Medical Journal*, Feb. 16, 1884). In Virginia, black pigs eat the paint-root (Lachanthes), but white pigs die if fed upon it. (Darwin, Origin of Species, p. 9). In South Africa the bite of the tzetze fly is fatal to horses and oxen, but is harmless to man, the ass, mule, goat, and to wild animals generally. (Africa, Past and Present, p. 88). Aloes, a convenient purge for horses is irregular and uncertain in its action on cattle; dogs require a dose eight times larger than is required in man. Table salt, a neccessary condiment to man acts as an emetic on dogs. (Vivisection, is it necessary, p. 56). And numerous examples equally illustrative could be recorded did space permit.

The knowledge of the healthy functions of animal life does not contain in itself any knowledge that can lead us to the therapeutic agents for the treatment of any single morbid state. (R. Com. Q. 1721). Variety of medicines is the child of ignorance. Sydenham when he was young possessed at least twenty remedies for every disease, but when advanced in age he found twenty diseases without a single remedy. Sir Benjamin Brodie said, "Make yourselves masters of the old remedies. Learn how to handle them. If you always begin with new remedies you throw away the valuable results, not only of your own experience but of the experience of those who have gone before you. You have to begin as it were *de novo*." (Harveian Oration, 1880, p. 184). Every experienced physician knows well that in the treatment of disease but little medicine is really necessary. Whenever experience has seemed to show success from treatment by a variety of remedies the efficient cause lies in the disease itself. (*British Medical Journal*, Sep. 30, 1882, pp. 611—618). Scientific prescribers too often fail when the " all round " prescriber succeeds. (*Medical Press and Circular*, Feb. 11, 1885, p. 126). As we learned the natural course of most acute diseases, so have we come to trust less in drugs, and to leave more to nature, or to careful nursing, rest, cleanliness, and suitable nourishment. (Address in Med., Cambridge, 1880). In further reference to this subject, the views expressed by Sir Thomas Watson are valuable. He considered that drugs and poisons tried on animals are unreliable, the effects being often very different upon the human subject and upon the animal; he accordingly places no faith in such experiments. (His evidence to Royal Commission). He questioned the safety of arguing from what happens in the lower animals to what we suppose would happen in man. (Prin. and Pract. of Med., vol. i., p. 576).

There are the further questions to be considered of hereditary idiosyncrasy, and personal conditions, in regard to each of which it is from the nature of things impossible from any " experiment," other than upon the individual immediately and directly concerned, to acquire a correct knowledge of the best method of acting in his particular case. " A patient is not merely an object of interest as the victim of some morbid process, nor as furnishing an opportunity for individual advancement merely. He is an elaborate and interesting organism possessing certain definite qualities, in fact he is a man." (Fothergill, Handbook of Therapeutics).

Another question, and it of great importance presents itself at this point, namely—How far have changes in regard to Therapeutics that have taken place in recent years been in their nature advances and improvements? Dr. Hare, while reviewing the process of medicine and therapeutics during the time that had elapsed since 1847, acknowledges that enormous advances have taken place in regard to definite and well based knowledge. But he points out the circumstance that with all that advance men have been led to attribute almost all disease to syphilis and gout. The term " arterial tension" has been employed with a convenient looseness; bacilli threaten to be enthusiastically credited with being the one thing responsible for, and explaining all diseases. At the same time that new drugs come into fashion, others go out, among the latter many " good remedies." In that period the wild rage for giving alcohol in almost every case of disease have subsided in favour of milk. In bronchitis and in diphtheria the use of emetics, which formerly was so beneficial, has of late ceased; so also in delirium tremens opium and opiates are less frequently used than formerly (being superseded by hypodermic injections). Although "experiments" are said to prove that mercurials are not cholagagues, the administratian of a good dose of calomel or blue pill, followed by some Epsom salts, will remove the black bile, the "melan-choly." If at present more aperients and fewer tonics were given, more and speedier cures would be effected. Such fluctuations and changes take place in opinion and in practice that it seems almost as if the wave of fashion sweeps over and obliterates the result of the experience of ages. Bleeding in all its forms (alas for the welfare of the patients) has been almost banished from practice, a change which must be attributed to oscillation from one extreme to another in things medical as well as in other things. (Some Remedies out of Fashion, 1883).

At the same time that the opinions of an eminent practising physician are expressed as above, the objects of "experimental" pharmacology are declared to be to "discover specific antidotes for each of the infectious diseases. (*Medical Press and Circular*, Nov. 23, 1881, pp. 443, 444.) According to a distinguished physiologist, "the pharmacologist will supply the physician with the means of affecting, in any desired sense, the functions of any physiological element of the body; it will, in short, become possible to introduce into the economy a molecular mechanism, which, like a very ingeniously-contrived torpedo, shall find its way to some particular group of living elements, and cause an explosion among them, leaving the rest untouched. (*Transactions of Medical Congress*, 1881, vol. I., p. 100). A second eminent physician remarks to the effect that although the treatment of every disease and injury is now conducted on well-ascertained principles, this rule does not apply in medicine so strictly as in surgery, "owing to the uncertainty of drug treatment." (*British Medical Journal*, September 30, 1882, p. 109. A third and no less eminent physician was "not a great believer in drugs." (Royal Commission, Q. 2,5545). A fourth eminent practitioner discussing the subject of pneumonia, rheumatism, and catarrh, "will not say that severe cases of these diseases recover as well under one plan of treatment as another; but in each and all examples of this type of inflammatory attack there is *a natural tendency* to recovery at a certain stage. (*Medical Press and Circular*, August 30, 1882, p. 167). And so, as a boast of modern research, we return to the doctrine of the *vis medicatrix naturæ*, as expressed by Hippocrates, B.C. 450 to 361 (Meryon, vol. i. p. 59), that is, two-and-twenty centuries ago!

We have it on the authority of advocates of "practical physiologists" themselves that it is not utility they seek in the pursuit of their experiments; and moreover the fact is rendered sufficiently clear in the preceding remarks that it is not by a majority of medical men, nor in publications of a professional nature that benefit to man is looked for from the performance of experiments with drugs on animals. With regard to therapeutics, a comparison of the statements adduced renders clear the circumstance that in this respect, as in that of medicine and surgery, results of experiments on animals when applied to man are misleading, fallacious, and untrustworthy. Reasons are given why they must of necessity be so. In fact the whole teaching of the evidence quoted is adverse to "experiments" in respect to drugs. The more closely the statements quoted under this section are examined and their source observed the more evident does the fact appear that advocacy of "experiments" does not come mainly from practising medical men nor from medical publications. This fact is of great importance in itself, and I desire to commend it in an especial manner to non-professional persons, into whose hands this pamphlet may find access. It is true that the Resolution on this subject by the Medical Congress of 1881 appears to point to a different conclusion. That Resolution, however, together with another considered at the same time, was not made subject of discussion, it was simply adopted from the physiological section, and put from the chair, an intimation at the same time made that the names of dissentients would be recorded (Transactions, Vol. I. p. 102), whereas the views now recorded are for the most part those which have been published since that date. The latter are entirely opposed to the anticipation expressed in the communication which forms the text of the present remarks.

3. Brain Physiology.

3. "Brain physiology has continued to advance—thanks largely to experiments—a full detail of which has lately been published by the Royal Society, with the conclusions drawn from them, and a great gain of exact knowledge is evident. The experiments were made upon monkeys in all cases narcotised with chloroform."

The record of the "experiments" above alluded to, and from which it is stated that brain physiology is largely indebted is published in the Philosophical Transactions of the Royal Society for 1885, Vol. 175, Part II, pages 479 to 562. The following is a summary of the experiments alluded to, arranged according to Sections as in the original version; the conclusions recorded by the performers of the several experiments are briefly stated, and it will be for the reader of the epitome thus presented, or if he prefer it, after a careful study of the full details as published in the Philosophical Transactions, to judge for himself how far the statement is justified that "a great gain of exact knowledge" in regard to Brain Physiology has been obtained from those experiments.

(*a*) *Lesion of the angular gyri and occipital lobes*:—12 experiments performed. *Conclusions* arrived at:—No physiological defect, either as regards motion or sensation. A psychological alteration very evident, but very difficult to define. The effects of the experiment on one angular gyrus were unexpected, and unlike the usual results following injury to one gyrus when the other gyrus is intact. *Total result*:—Vision is possible with both eyes when only portions of the visual centres remain intact on both sides.

Remarks. But, "a connection between a definite part of the retina and a definite part of the "visual area" (of the brain) could not be proven. (*Dublin Journal of Medicine*, Aug. 1885, p. 153). According to another statement contained in the published record—it is easily understood from anatomical considerations why both sides of the brain should be completely able to maintain sight. A consideration of the whole leads to the result that the only point definitely ascertained could readily have been learnt by means of anatomy; in other respects no "exact knowledge" is stated in regard to any of the points enumerated. Works on Military Surgery (Hennen, ed., 1825) contain reports of cases of wounds, the teachings from which furnish actual data—not comparative, as above, of the effects of injuries of different parts of the human brain. Neither do the "experiments" on animals above related furnish any index in regard to the proper treatment of injuries of the brain in man.

(*b*) *Lesions of the Temporo-Sphenoidal Convolutions*:—2 experiments. *Conclusions* arrived at: —Hearing was abolished by destructive lesion of this convolution in both hemispheres; but no impairment of hearing when the medullary fibres of the other convolutions of the temporo-sphenoidal lobe were broken up. The auditory centre is situated in the superior temporo-sphenoidal convolution.

Remarks.—Psychical deafness follows extirpation of both temporal lobes, but absolute deafness is not permanent. (*Dublin Journal, ut cit.* p. 153). Notwithstanding all the "experimental" researches that have of late years been undertaken by practical physiologists, we know very little definitely about the position in the brain of, and still less about the nature of the auditory sensorium or central end-organ of the auditory nerve. (Elementary Physiology, by Huxley, p. 238). By means of anatomical investigation (on the dead brain) the ultimate origin of the auditory nerve has been traced, as far as this is possible. (See Quain's Anatomy, vol. ii., p. 367). So that no result in regard to function or anatomy in relation to the sense of hearing has accrued from the "experiments" detailed, beyond what had previously been much more effectively and correctly gained by more ordinary means of investigation, namely, those of clinical and post mortem investigation.

Lesions of the Convolutions bounding the Fissure of Rolando. 4 Experiments. *Conclusions* arrived at :—Destructive lesion of the cortical areas cause local or general hemiplegia (without loss of sensation) on the opposite side; the degree of paralysis varies with the completeness of destruction of grey matter.

Remarks.—Hennen writes thus (Military Surgery, 1829, p. 304).—"Although we can with much probability say that paralysis or convulsion will take place on the side of the body opposite to the wound (of the brain), yet that occurrence (which is uncertain in its period of attack) will frequently take place either in the upper or the lower extremity, or in the entire of the opposite side, and be either partial or general, from causes which are altogether beyond our reach."

M. Brown-Sequard, as the result of experiments performed by him, arrived at the conclusion that facts thus observed are at variance with the commonly received views that :—each half of the brain is alone the seat of centres for the voluntary movements of the limbs on the opposite side; that the conductors used for voluntary movements coming from the brain descend along the crura cerebri and decussate, either in the pons varolii (this is the view of most physiologists), or at the lower part of the medulla oblongata (this is the view of most physicians); that paralysis is the direct effect of a lesion in the parts of the brain supposed to belong to the voluntary motor apparatus. He writes that :—In rabbits, adult or young, it is almost always on the corresponding side that—paralysis—appears, and it is the same in very young cats and dogs. In adult guinea pigs, cats, and dogs, the result not rarely is just the reverse; *i.e.*, paralysis appears on the opposite side. But in either case it is hemiplegia that takes place, and not an incomplete paralysis of the four limbs. The place in the base of the brain above which a transverse section will give rise to cross hemiplegia, and below which it will produce direct hemiplegia varies extremely, not only according to the species of animals, but also to their age. Great differences also exist in animals of the same species, of the same age, and in every respect as similar as possible one to another. M. Brown-Sequard repeats his statement that all the above conclusions arrived at by him from experiments, are in absolute opposition to the admitted views regarding the physiology of the base of the brain. (*Lancet,* 6th August, 1881).

In 1805 Dr. Bateman wrote :—1. One hemisphere of the brain being affected (by injury) morbid symptoms generally appear on the other side of the body. 2. When both are affected, the whole body suffers; 3. If only one is violently affected the whole body suffers; 4. Though the cerebrum alone is hurt, it produces morbid symptoms in all the muscles of voluntary motion, from whatever point their nerves may arise; 5. In cases of external accident, the prognosis is most favourable where one side only is affected. The cases from which these conclusions were drawn were by Mr. Anderson, and are given in the transactions of the Royal Society of Edinburgh, vol. ii. p. 17. (Hennen's Military Surgery, p. 304). Morgagni gives a case of cerebral lesion (separation of the corpus striatum from the cortex) which had been carefully observed, and in which the paralysis was on the same side as the injury. It was clear to him that *sometimes* the paralysis occurs on the same side as the lesion. (Dr. Gimson, p. 110).

Thus the conclusions become manifest that from this class of "experiments" no result whatever was obtained beyond what had already been otherwise ascertained. It is further shown that results of "experiments" performed by different physiologists have differed widely among themselves and in different animals operated on. But for all practical purposes the indications presented by injuries of different parts of the human brain occurring by wounds in battle and otherwise were described early in the present century and still hold good, whatever be the phenomena presented by animals of various kinds under "experiment."

(*d*) *Lesions of the Frontal Lobes.* 5 experiments. *Conclusions* arrived at :—In one of the experiments performed, the case was unsuccessful; in a second "also unsuccessful;" in a third "the animal died suddenly;" in a fourth, though as regards discoverable symptoms, the case was negative, the microscopical examination of the brain revealed facts of great importance, both as regards anatomical relations, and probable physiological significance of the pre-frontal cerebral regions. The general results show "a remarkable absence of any discoverable symptoms in connection with the almost entire destruction of the pre-frontal regions, or anterior two-thirds of the frontal convolutions."

Remarks.—It accordingly follows from the above conclusions, recorded by the experimenters themselves, that while the "results" of the experiments were "unsuccessful," and "negative," important facts were discovered by the microscope—namely, by the ordinary means employed in post mortem examination. Thus the results as reported indicate that no good purpose was served by the experiments; and, further, that the only thing learnt was ascertained by other methods of research. But the records of military surgery contain full reports of injuries to the frontal lobes and other portions of the brain, which, if duly studied are sufficient to enlighten the surgeon in regard to all points of actual use—namely the management and treatment of those injuries in man. With reference to injuries to the brain Dr. Hennen wrote thus :—"Excessive refinement in distinguishing these injuries and their varieties, I consider to be very unnecessary to the practical surgeon; they often, nay most frequently, are co-existent." (Military Surgery, 1829, p. 282). He records cases of wounds of the frontal lobes and of other portions of the brain in which no remarkable phenomena, either in regard to intelligence, motion, or sensation occurred; he also observes that "sometimes" a bullet drives forward into the brain itself, eluding the search of the surgeon, and *subverting the theories of the physiologist.* (pp. 285, 288 *et seq*). Legouest cites a case of a wound of the anterior lobe of the brain, in which no disturbance of the intelligence occurred. (Chirurgie d'Armée, p. 233). We further learn that cases of injury of one or other frontal lobe, without sensory or motor paralysis, are very numerous. In all of them there was an entire absence of sensory or motor paralysis, and in many there was nothing calling for special attention as regards the mental condition. An instance is recorded, of arrested development of the frontal lobes without any objective symptoms as regards mobility or sensibility, although the girl in whose case this happened was idiotic from birth. (Dr. Gimson, p. 102).

With further reference to the same series of experiments, the conclusion is drawn by the operators that—"The frontal regions of the brain seem to have considerable relation to mental processes." On the point here raised Fleurens has stated that large tracts of brain cortex can be destroyed without causing any very evident mental disturbance; that any one part of the brain may be destroyed with a like result. Disease shows that extensive lesions may exist on one side of the brain without any mental symptoms, but that if both sides be attacked the mind becomes disorganised. (Three Essays, p. 271). Professor Ferrier, in quoting the above remarks, notices the many fallacies physiologists are led into; that if it is difficult to test the mental condition in a human being, how much more so must it be in the case of the lower animals? (Id). Pain inflicted on the body, causes on the mental side intense emotion as the accompaniment of the physical condition. (Three Essays, p. 226).

As a general admission, the anterior lobes of the brain are accepted as being particularly interested in the functions of intelligence and will. But even here we encounter a thousand difficulties and contradictions. Any division of the lobes of the brain is a purely artificial matter according to the highest scientists. The grey matter of the anterior of the brain, is in intimate relationship with the grey substance of the middle and the posterior regions of that organ, so that while our psychic nature is composed of a grand network, in which are comprised the sensations, the perceptions, the judgments, and volition, so the physical organism corresponding therewith is also a vast and complicated network of cellules and nerve-fibres, reciprocally united, and in functional relationships, one with the others. It can thus be readily seen how difficult it is to analyse the least phenomenon, where exists at the same time, the conception of intelligence and will—a series of harmonious movements. (Paris letter, *Madras Mail*, Feb. 6th, 1882).

Apropos to this subject the statement occurs that "the heaviest brain ever weighed in the United States was that of a man (Madden) who died in 1882. The doctor who attended him during his last illness had observed the immense frontal and lateral development, and determined to weigh the brain, which brought down the scales at 64½ ounces. The brains of Napoleon, Agassiz, and Webster were much lighter than Madden's. Now Madden was not a soldier, naturalist, or statesman, but a gambler." (*Knowledge*, Aug. 25th, 1882).

It accordingly follows from the comparison of views thus presented, that the experiments above detailed presented among themselves no results which could be looked upon as definite or reliable in reference to their bearing on practical surgery. Nor was the teaching to be obtained from them of any value whatever in solving questions in regard to the "seat" of human intelligence.

(e) *Lesions of the Hippocampal region*—by means of a wire cautery passed through the extremity of the occipital lobe, downwards and forwards in the direction of the hippocampal region. 10 experiments. *Conclusions* arrived at:—Many of the experiments were unsuccessful. In others varying success was met with. The mortality was such as to interfere with the solution of some points of importance. The destruction of the hippocampal region in the manner carried out involves many risks, and is purely a matter of calculation founded on anatomical measurements of the probable depth of the parts; a certain amount of injury of other regions than those intended to be operated on was unavoidable. In one case the permanency of the symptoms could not be determined owing to the death of the animal very shortly after the establishment of the lesion. In another the animal died suddenly. In another, "the case is altogether unique in the symptoms which were induced."

Remarks. It accordingly appears that no reliable and constant result was obtained from the above series of ten "experiments." "Experiment" was unnecessary to indicate that the hippocampal region could not be mechanically injured without inflicting injury upon other parts; anatomy sufficiently demonstrated that fact. Nor is it apparent in what manner the destruction af the hippocampal region of animals could lead to the benefit of man. The published record of the experiments in question affords no light upon this point.

Adverting to the series of "experiments" here described, the *Dublin Medical Journal* observed that all experimenters except Ferrier, now agree that the motor and tactile areas (of the brain) coincide to a great extent. The position held by Ferrier that tactile sensations are perceived in the cornu ammonis has not been confirmed, either by experiment or by pathological observation and may be abandoned. (August, 1885, p. 154). Says the *British Medical Journal*—"We are too much in the habit of projecting our own inner consciousness into the lower animals; we imagine that they feel and think as we do." (March 21, 1885, p. 587). In animals with great muscularity is associated a large sigmoid gyrus, animals in whose daily life sensations of smell play a large part present long brains, with considerable development of the gyrus hippocampi, and of a part of the temporo-sphenoid lobes, the prolongation forwards of the latter over the fossa Sylvii being very characteristic. The development of the inner part of the occipital lobes varies with the sense of sight. Any attempt at an exact determination of the different areas on anatomical grounds is impossible until a method has been invented by which the percentage superficies of different regions can be obtained in a large number of brains. (*id*). But the point of immediate importance is the acknowledgement that "exact determination of different areas is impossible."

(*f*) "Some of the experiments furnished most interesting examples of how an animal can learn to compensate its loss of one sense by the education of the other."

But the results of "experiments" having reference to this point are unsatisfactory and contradictory. Hitzig regards the notion that a nerve-centre can be entirely newly-formed as untenable. Nothnagel has pointed out the *non sequitur* in Hitzig's argument. Carville, Duret, and Vulpian that the " nerve-centres which carry on the restored functions have their seat in the portion of the brain which has not been injured." Gotts " that to pursue the hypothesis is superfluous " (" Physiological Fallacies, pp. 125-131). In fact, the results of experiments on the subject are—chaos.

Dr. Hughlings Jackson is of opinion that when a part of a centre is destroyed, the rest of the centre—the part not destroyed—serves next as well as the part lost; and that in some cases compensation is practically perfect. (*Medical Press and Circular*, Nov. 22nd, 1882, p. 434). In fact this law of compensation in regard to function is daily recognised as existing in cases occurring in practice; it is elucidated by pathological examination in fatal cases of disease;—not by " experiment."

(*g*) " a more extended series of experiments was directed to proving the effect of destruction of those portions of the brain which had been previously shown to be the centres of voluntary motions for distinct portions of the body."

But, says M. Legouest:—The attempts at localisation (of cerebral functions) which have been made are unfortunately so often set at naught by clinical observation that they cannot inspire much confidence. (Chirurgie d'Armée, p. 234).

Dr. Elliotson wrote:—Attempts to mutilate the brain are not calculated to afford much information. Brutes can generally give no opportunity of minutely observing what mental change has been produced by the removal. When various parts of the brain are removed, how can any inference be drawn during the short existence of the animal, as to its various faculties and inclinations? It is difficult, if not impossible, to remove one cerebral organ entirely and alone,—other parts of the encephalon, &c., are almost certain to be injured; and, if not injured, they may be influenced by the extension of irritation from the injury, and by sympathy with the injured parts—just, for example, as we see epilepsy from exciting causes in every part of the encephalon, and from exciting causes in distant organs; amaurosis from wounds of the supra-orbital nerve, infra orbital nerve, and portio dura. Injury of different portions of the same organs may have the same effect; blindness from wounding the optic nerves, the tractus optici, or the corpora quadigemina. (Vivisection Forty Years Ago, p. 27).

Dr. Carpenter was of opinion that in regard to such subjects as the functions of different parts of the encephalon, experiments can give no trustworthy results, since violence to one part cannot be put in practice without causing functional disturbance of the rest. He considered that a careful anatomical examination of the complicated forms of the encephalon, from fishes up to man, is more likely than any number of experiments to elucidate the problem. (Fleming's Essay, p. 79).

"The code of experimentation," said M. Colin, "has need to be revised. Certain experiments are complex in their nature, when they are applied to important functions, the perturbations of which re-act on nearly the whole economy. Apply your instrument to the brain, or to the heart, and quickly you will have general and serious troubles, which it is necessary to disengage from those which belong to the direct and immediate result of the experiment. Often the same experiment, repeated twenty times, gives twenty different results, even when the animals are placed apparently in the same conditions. It may even happen that the same experiment gives contradictory results. Which of the contradictory results is the true one? It is necessary to recommence in order to learn, and when we have done so, the other set remains to be accounted for." (Mr. Flemings Essay, p. 27). "Well ascertained facts" have no sooner been accepted than a new vivisector has arisen, and, by experiments, has demonstrated the unsoundness of the "well ascertained facts." Error has propagated error, and those beautiful but mysterious operations of nature, when sought to be rudely exposed, have only been seen or manifested in an abnormal and disordered condition, from whence proceeds false reasoning and conflicting conclusions. Take the brain. After an amount of torture which the mind shrinks from contemplating, what do we obtain? Speculation! Gratiolet says—"Let us confess that in our ignorance of the true construction of the medulla, the peduncles and the optic thalami the question is abandoned to the *speculations* of physiologists. A host of vivisectors disagree as to the functions of the brain, each disagreement calling forth a fresh series of tortures still more contradictory in their results—because depending upon so many different influences; but all partaking of the horrible. The results of experiments by Fleurens, Rolando, Hertwig, and others, have shown that on slicing away the brain the animal becomes more dull and stupid in proportion to the proportion of cortical substance removed. But clinical observation points out the fact that in cases in which disease has been found to commence at the circumference of the brain, and proceed towards the centre, the mental faculties are affected first; whereas in those diseases which commence at the central parts of the organ, and extend outwards, they are affected last. How impotent then has been experiment, and how much more valuable the observations of pathologists. The effects of disease are by far the most important means of research open to us. If the cerebrum has rendered such unsatisfactory evidence under the heart-sickening cross questionings of the investigator's knife, the cerebellum has afforded them even less. After thousands of experiments made by Dugès, Rolando, Magendie, Flourens, Hertwig, and other vivisectors on the functions of the cerebellum, Schiff concludes from numberless other experiments he has made that the functions of that organ are altogether unknown! The common sense view has been gradually gaining ground with all, save those who delight in living dissections, and it is acknowledged by the best physiologists, that a careful register of the phenomena of disease, followed by post mortem examination, is generally more to be depended upon than those experiments so revolting to humanity, which have disfigured the emblem of science; but from which, nevertheless, different investigators drew different theories, each to suit his own special view of the case. (*op cit. sup.* pp. 26—35).

Brown-Sequard asked, Are there parts of the brain and spinal cord which, being diseased, give rise to symptoms which no other parts can produce? He endeavoured to show that although there is no symptom which alone possesses an absolute and pathognomionic nature concerning the seat of the disease, there are marked manifestations which establish almost certainly, and sometimes quite certainly, the special parts that are diseased. He subsequently communicated some facts to the Paris Biological Society, the object of which was to show that the movements induced by irritating different parts of the encephalon are unlike those which ought to occur in accordance with the accepted theories of the cerebro-spinal motor and sensitive apparatus. (*Lancet*, August 13, 1881, *British Medical Journal*, Aug. 5, 1882). According to *Brain*, October, 1884, a physician "guided by his former (clinical) experience was able to make a happy guess at the nature, and accurately to predict, as the corpse lay upon the table, what would be found when the head was opened"—this also without the performance of any "experiment." Brown-Sequard was further of opinion that experiments on the nervous system were very difficult, for they are under the dependence of irritation, and often fail. According to evidence given before the Royal Commission of 1876; so many disturbing elements are brought into the case that it is extremely difficult to interpret the experiment. Bernard states that a single operation performed on a single point of the nervous system gives rise to a general hyperesthesia of the whole apparatus. (Fleming's Essay). He advised his class to "throw overboard all that they had learned from the experimental method on nervous functions as being utterly false." (Dr. Bowie's lecture, p. 13). The teaching of experiments on the functions of the brain and nerves are a tissue of mistakes created by those experiments, but rectified at last by clinical observation during life, and examination of the diseased structure after death. (Summary in *Eddows' Shrewsbury Journal*, Aug. 15th, 1877).

Professor Bowman was of opinion that;—Experiments on so complex an organ as the brain are ill calculated to lead to useful or satisfactory results. (Gimson, p. 67). The study of the functions of the brain and nervous system is beset with the greatest difficulties and perplexities. The dissimilarities existing betwcen animals of the same species, and between animals and man, render the results obtained by experiments unreliable when used to explain disease in man. (id. p. 100). Mrs. Kingsford points out one important cause for the discrepancies noticed by Professor Ferrier between the results of his own experiments, and the observations by physicians on human patients, namely that in man lesions or derangements of the brain are in nine cases out of ten set up from within, and are the result of slow pathological processes, gradually advancing from centre to cortex. But the vivisectional injury is the reverse of this process, and therefore in no way compatible with it. (*Nineteenth Century*, Februaıy 1882).

The *Medical Press* (October 31st, 1883) observed that:—Our knowledge of the functions of the various parts of the nervous system cannot, we believe, be even yet considered approaching to certainty. The translator of Professor Charcot's lectures on the localisation of cerebral and spinal diseases, states that the author's views, regarding the function of vision, are in accordance with many clinical facts. Nevertheless, he goes on to say, some researches of Professors Yeo and Ferrier must be taken into careful consideration. The translator then gives the results of experiments by the latter two gentlemen, which do not accord with those of Charcot, after which follows the remark that it is not possible to read this work without its leaving upon our minds a degree of uncertainty which prevents us from accepting its teachings as fully as we otherwise would. In the same journal (May 14th, 1884) it is stated that;—the relations and functions of the several parts of the roots of the optic nerve are, to the writer of the article, very obscure. He adds:—The experiments detailed as being performed on dogs—can they be accepted as absolute truth? We answer in the negative.

Dr. Ferrier is quoted thus :—We are every day confronted with the fact that the most widely abnormal deviations from healthy functional activity of the nerve centres may be manifested, which leave no trace discoverable by ordinary dissection, or by any of our most advanced methods of investigation. Nor do the facts of experimental physiology seem so consistent with themselves, or with undoubted facts of clinical research as to inspire us with unhesitating confidence as to their applicability to human pathology. (Dr. Gimson, p. 82). Further, that one great fallacy has been the assumption that the results of experiments on frogs, pigeons, and other animals are capable of application to man without qualification ; an assumption which vitiates the conclusions of numerous physiologists of the present day. The very fact that there exist such patent differences between the effects of the déstruction of the cerebral hemispheres in different orders of animals ought to inspire caution in the application to man of results obtained only by experiments on the brains of animals low down in the scale. (Three Essays, p. 271).

Schiff animadverted on the difference of opinion in reference to cerebral localisation held by physiologists and physicians—that the former were (at the time he wrote) opposed to the doctrine, the latter were its only supporters. The idea of localisation of function being once more revived, physiologists proceeded to ascertain its truth in a far different manner from Gall. He collected his facts from simple observation of nature. They interrogate nature by experiments on living animals, ignoring the plain teachings afforded by disease. If the phenomena of disease were more accurately attended to and interpreted, physiology would not be in the lame and halt condition it now is. (Three Essays, p. 270).

Fleurens stated that large tracts of the brain cortex may be destroyed without causing any very evident mental disturbance, and that any one part of the brain may be destroyed, with a like result. Hence he believed that there was no localisation of function in the brain, but that each part of it is a micrencephalon, capable of itself performing all the functions pertaining to the whole. There must always be a certain amount of doubt attached to the conclusions of even the most cautious experimenter, and, to quote Professor Ferrier, the slightest doubt is absolute failure; therefore, it is added, "the whole series of the experiments alluded to are absolute failures." Every day old theories are being upset by new, and they, in giving way to others, and so on, until a profound reasoner shows them all to be fallacious by applying the results of patient clinical and pathological research. (*Op. cit.* pp. 270—272).

When in 1881 a monkey and a dog that had been "experimented" on, as already detailed, were examined it was found that the phenomena which were present differed in character in the respective subjects. In reference to this fact the Medical Press (10th August, 1881, p. 130) observed that:—The exhibition of these two animals was a remarkable proof of the fallacious arguments from considerations of comparative results; and it will probably tend to convince all who witnessed it that while it is impossible to regard the matter in dispute as any nearer settlement than before, it assuredly indicates how little reliance can be placed on deductions drawn from the study of widely separated groups of animals rather than from those of the same species. Professor Esner arrives at the determination of certain areas or centres the lesion of which always causes the same symptoms. He defines centres for the upper and lower limbs more especially; these he calls "absolute" centres. He also defines a number of "relative" centres, for other movements, and for different forms of sensibility, these he calls "relative" centres being those in which lesion does not always cause affection of the function with which they are supposed to be in relation. Ferrier alludes to the impotence of the fortuitous experiments of disease as regards the determination—of the exact position and limits of any centre whatever. These so-called centres are seen to be capable of destruction without discoverable symptoms, and the same region—he is writing of Esner's "centres"—seems to play many parts, being indifferently a centre for the leg, arm, face, speech, sight, and so forth. He goes on to state that—had Esner's localisation of relative centres any foundation in fact, it would be nothing short of a *reductio ad absurdum* of the whole doctrine of localisation. (*Nature,* Jan. 5th, 1882). According to the *British Medical Journal* also:—If these researches have not furnished irrefutable arguments to the doctrine of functional cerebral localisation, they have shown that in man as in the mammalia the nerve fibres start from certain determinate points in the grey matter of the brain. (26th November, 1881, p. 865). But the fact is self-evident that this result is more completely and effectively obtained by dissections of the dead brain than of the living.

Dr. Munck holds opinions the opposite to Professor Ferrier. He considers that the experiments of the latter were performed in an inefficient manner; that in consequence of the gravity of the mutilations inflicted, the conclusions obtained were erroneous; that the edifice raised thereon was "altogether arbitrary;" that the rest of the data adduced were equally valueless; that his experiments were arranged and prepared for the purpose of demonstrating preconceived opinions; that not one of the principles laid down by him is tenable in reality, and in no way to be distinguished from absolutely capricious interpretations;—his pictures of brain centres as "fancy pictures." (Ayez Pitié, pp. 96, 97; *Zoophilist*, May 1st, 1882, pp. 3, 5). Professor Christian could not accept the doctrine advocated by Munck, Ferrier, and Hitzig regarding the localisation of activity of the cerebrum. (*Nature*, July 10th, 1884).

The general question discussed in the preceding paragraphs has been summarised after this manner:—Originally one with phrenology, Gall's theory was that twenty-seven special cerebral organs existed for an equal number of special faculties. Fleurens demonstrated that in the outside grey matter of the brain there are no special centres. The experiments of Fleurens, however, were performed on pigeons, and the data thus obtained were applied to man. Majendie and others performed a series of experiments, all tending to prove that the substance of the convolutions was not sensitive, and that it contained no special centres for the localisation of function. In 1870 Hitzig and Fritsch arrived at the conclusion from experiments, that the conclusions arrived at by their predecessors were wrong; that some parts of the surface of the brain were sensitive to electricity, and that when certain points were excited, movements followed as a matter of course. Subsequently, Ferrier was led to establish localised centres in the convolutions, for certain defined muscular movements. Goltz followed in the same path; but he noticed that the conclusions arrived at by Ferrier and Munk differed essentially between themselves, and that there are no sufficient grounds for many of the conclusions of Ferrier regarding several of his localisations. Ferrier replies, that however it may be with dogs, it is different in the monkey and in man. According to Charcot:—The result of experimentation on animals—regarding cerebral localisations—in reference to man, can only furnish presumptions more or less well founded, but no absolute demonstration. It is on man himself that truth must be sought. The conditions of an experiment, produced indeed spontaneously, are realised daily in man under pathological circumstances. (*Zoophilist*, May 1st, 1882, p. 23).

The results obtained by Galvanic or Faradaic, stimulation of the surface of the hemispheres have led to considerable differences of opinion among experimenters as to their real significance. Schiff regards the movements resulting from stimulation of the hemispheres as of a reflex nature, but denies the existence of centres which exert any direct influence on the muscles of animal life. (*id.* p. 30). Dr. Calderwood writes:—While distinct areas or circles of the brain have thus been marked, warranting localising of certain functions, the facts connected with these experiments do not favour the view that each area is to be taken as so rigidly distinct that it may be supposed to operate separately in a quite isolated manner. On the contrary, a conjoint action of several centres seems more commonly implied when the natural activity of the brain is contemplated in the line of these results. Additional weight must be given to this consideration, when it is noticed that the centres are nominally motor centres; nevertheless many of the movements occasioned by electric stimulation are those induced naturally as the result of sensation. (*Science and Religion*, 1881, p. 233).

Hermann says:—Physiological experiments conducted on the encephalon are most indefinite. Lesions induced in the most delicate and complicated organs of the body by means so rough, that they may be compared to injuries to a watch by means of a pistol shot. Apart from the fact that it is impossible to localise the lesion itself, results so obtained may be due to irritation of centres, or paralysis of centres, stimulation of conducting centres, or paralysis of conducting centres, without being able to say which. Hence the interpretation of even these phenomena, which are constant in their occurrence, is always uncertain. The best method of investigation which is possible, is the observation of cases of disease in which the exact nature of the lesion is accurately observed after death. (Gamgee's Translations of Hermann's Physiology, 1878, 2nd edition, p. 494.—*Zoophilist*, March, 1884, p. 268).

Dr. Althaus wrote that:—Experimental pathology, of which such brilliant results were at one time expected, seems to have been singularly unproductive in this special department of localisation. Vulpian accounts for this by the utter inability of the experimental pathologist to produce in animals maladies such as we see in men. (*British Medical Journal*, June 4th, 1881). In June, 1884, a curious controversy arose between two practical physiologists as to the result of the removal of the cerebral hemispheres; in that controversy one experimenter contradicted the other, and finally declared himself an opponent of the teaching of localisation, which, he said, "at best was only an hypothesis. (Ditto, May 22, 1882, p. 771). The *Lancet* (June 16th, 1883) observed that pathological observation is doing more to advance our knowledge of cerebral functions than physiological experiment; for whereas the physiologists agree to differ upon the interpretation of their experimental results in this matter, the clinical and pathological evidence in support of the doctrine is rapidly accumulating. According to the same journal

(November 10th, 1883) :—If Dr. Ferrier's suggestions meet with much practical response, it is to be feared that cerebral localisation will soon have more deaths to answer for than lives to boast of.

In the case of the bird or mammal, when the cerebral hemispheres are bodily removed, the portions of the brain left behind are so profoundly affected by the " shock " of the operation and so obviously in an abnormal condition that no just deductions can be made as to what are their normal functions. (A Text Book of Physiology, by M. Foster, F.R.S., &c., 1883, p. 611.) The more we study the phenomena exhibited by animals possessing a part only of their brain, the closer are we pushed to the conclusion that no sharp line can be drawn between volition and the lack of volition, or between the possession and absence of intelligence. We cannot fix on any lineal barrier in the brain, or in the general nervous system, and say beyond this there is volition and intelligence, but up to this there is none. (p. 613). For a while the teachings of pathology and experiment were contradictory (in regard to localization of cerebral function), but continued experimental inquiry showed that the former was in the right. (p. 625). What, then, can we conclude as to the nature of the events which take place (under experiment) in the several cortical areas? To this question, unfortunately, no clear answer can yet be given, for the results of different inquirers are so far irreconcileably opposed. (p. 630). The nervous system is not to be considered as a collection of isolated organs, each fulfilling its functions independently of its fellow, but as a large machine, integrated into a whole, the constituent parts of which are in almost all its actions acting and reacting on each other in various ways. (p. 635).

No light is thrown on the unquestionable connection between all the functions of life and the mechanism of the body by cutting out from an organism certain parts of the machinery which are known to be those of consciousness and will and then finding that the animal is still capable of certain movements, which are usually indicative of reason and of purpose. Surely the reasoning is bad which argues that because a given movement goes on after an animal has been mutilated, this movement must therefore continue to possess all the same elements of character which accompanied it when the animal was complete. And not only is the reasoning bad, but as a matter of fact, the conclusion has been proved to be erroneous. (Unity of Nature, by the Duke of Argyll, p. 110).

4. Conclusions arrived at.

A careful perusal of the evidence brought forward in the above remarks is therefore surely sufficient to convince the reader that throughout, the very numerous experiments, the "results" of which are enumerated, present nothing better than a succession of what is unsatisfactory, contradictory, or "negative," at the same time that by the experimenters themselves the admission recurs that the conclusions thus obtained are in repeated instances at variance with the phenomena of disease. The only point in reference to all the continually contradictory statements thus presented which strikes one is surprise that having such a knowledge of those contradictions, as they must of necessity possess, "experimenters" should persist in advocating a method of research which in their own hands has been productive only of such "results" as are above recorded. The view on this subject above expressed from one of the leading medical journals deserves to be reproduced and emphasised, namely, "that if the theory of 'localisation' of cerebral function meet with much practical response, it will soon have more deaths to answer for than lives to boast of."

4. The following commentaries are quoted from the article in the *Times*, now under reply, namely :—"As to mental operations, results (of experiments on animals) must be imperfect where the animal concerned cannot be subjected to questioning." It was almost impossible to get at these parts (namely, the basal portion of the brain), without injuring some other portion. Whenever a portion of the brain is injured, it is, to say the least, doubtful how far the loss of that part influences the function of another region." "It is still open to question how far we can argue from results obtained on monkeys to the higher organisation of man."

Remarks.—In the short paragraphs above extracted, arguments are advanced which in their nature are destructive of the reasons adduced in that article in favour of a practice already demonstrated to be valueless in its assumed relations to the benefit of man. That these arguments have the support of practical physiologists, of physicians, and of surgeons, is apparent from their several writings quoted in the preceding pages of this pamphlet.

In a physiological point of view, so far are the results of the "experiments" detailed from having led to correct knowledge regarding the "seat" of cerebral functions, that they consist only of divergent views and different hypotheses among the various experimenters themselves.

The circumstances deserves to be particularly noticed that in the quotations from journals, periodicals, and other works, which constitute the bulk of this brochure, the fact is prominently brought out that the preponderating amount of medical evidence is adverse to the claims in favour of the experiments detailed, and set forth in the communicated article in the *Times*, to which it is intended to be a reply—and an answer.

Finally, therefore, may I not confidently appeal to members of the medical profession who have done me the honour of following me so far, and also to experimenters, whether anything useful to medical science, properly so-called, is likely to be gained by a continuance of a method of research, which has thus failed in its ostensible purpose in the past? For my own part, and from a comparison of the evidence adduced in this analysis, I unhesitatingly express my opinion that nothing useful can possibly be gained by such means.

C. A. G.

THE GERM-THEORY OF DISEASE.

The whole practice of "Zymotic Inoculation" being based upon the theory that a large class of diseases are due to the presence in the living body of micro-organisms of various kinds, included under the general expression "germs," it seems desirable, as an introduction to what is to follow on the subject of "Inoculation," that a brief consideration should be given to the "infinitely little" organic bodies thus indicated.

The idea that "germs" of disease have an actual existence is of very ancient date among the Chinese. According to their standard work on the natural sciences, the 'Pêntsao' (date A.D. 1370-1650, but referring to much earlier periods) the "germs" of small-pox were capable of being conveyed, by means of articles of clothing, from place to place. According to the Chinese also, consumption is infectious; this they account for by the hypothesis that, "at the moment of death of the phthisical patient, a *worm* (*Bacillus tuberculi ?*) is expelled, which enters the body through the breath of those in attendance." (Epitome of Report's Chinese Customs Department, pp. 70-159.)

In countries more to the westward, from the earliest recorded periods of history diseases that are in their nature communicable have been ascribed to causes foreign to the normal life of the organism. Those causes have in succession been indicated under expressions such as "the arrows of Apollo," fabulous animals, "winged worms," "little dragons," "spiders," and so on—thus pre-shadowing present views with regard to the organised nature of elements deemed capable of producing disease. The power by which they are resisted as the *vis medicatrix naturæ*. (See *British Medical Journal*, August 13, 1881, p. 279.)

In the remoter districts of modern Greece also, there survive women who are commonly resorted to for the cure of all diseases which depend, they say, upon the operations of various sorts of parasitic *worm-like* organisms which invade and prey upon the human body,—which surely is but the "germ-theory" writ large. (*Medical Times*, December 26, 1885).

Johann Lucas Schönbein was the first investigator in modern times to give definite form to the hypotheses above expressed. Bassi and Adouin had indeed previously investigated the nature of the muscardine disease of grapes, and to that circumstance was due the impulse which led to the first discovery made by Schönbein, namely of the *Achorion Schonbeini.* (*British Medical Journal,* August 13, 1881.)

It is observed that :—" The state of disease is not a radically different condition from that of health. The pathological condition is, to the physiological, simply a prolongation of the limits of variation, higher or lower, proper to each phenomenon of the normal organism, and it can never produce any new phenomenon." (Comte, " Positive Philosophy," book v., chap i. Quoted in *Westminster Review*, October 1881, p. 406.) —Thus is expressed the theory of disease looked at from the standpoint of philosophy. But, from the days of Hippocrates, medicine has been separated from philosophy; and here are some of the results of this division : " Hypothesis heaped on hypothesis, unsupported by observation, based on no truths." (Knox, "On the Races of Man," p. 84).— How far the last quoted remarks apply to the theory of *disease germs,* which at present commands a large share of professional attention, remains to be seen in the remarks which follow.

At a meeting of the Medical Congress of 1881, Dr. Wilks spoke of the germ-theory in conditional terms after this manner :—" *If* the specific diseases be due to organisms, and the *hypothetical* contagium vivum be a reality, and *if* the doctrine of evolution," &c., &c. (*Lancet,* August, 1881, p. 223).

The statements occur that :—" The disastrous disturbances by which wounds and life are threatened, are ultimately due to the intrusion of lower organisms. (*Lancet,* August 13th, 1881, p. 281).—Drs. Fokker and Naegelli consider that there is only one form of bacterium, subject to varieties and modifications. (*Ibid.*, p. 290).—In anthrax the only agent necessary, or able to produce the disease, is the bacillus anthracis. (*Ibid.*, p. 299).—Everybody who did not believe in the specific nature of germs was wrong." This in the estimation of the author quoted. (Pasteur—*Lancet,* October, 1881, p. 548).

In accordance with the theory of germs, means were taken to diminish the chances of infection after great operations; morbid germs could be attenuated, and by inoculation with the product, virulent disease prevented. (Raynaud—*British Medical Journal* August 13th, 1881, p. 272). The method of preparation of, and inoculation with the virus of chicken-cholera and of splenic fever, was fully described by the greatest living advocate of the system: (Pasteur—*British Medical Journal*, August 13th 1881, p. 283). Also Greenfield—*Journal of Agricultural Society of England*, vol. xvii., part i., p. 30).

And numerous further arguments and statements of similar tendency to the above are adduced by different advocates of that hypothesis—a particular method of medical treatment being based thereon.

Against the theory of disease germs are the following out of many arguments:—The figures given as results of treatment of wounds in accordance with the antiseptic theory have been *slighted*. (Volkman—*Lancet*, August 13th, 1881, p. 281).—Equally favourable results would have followed the observance of perfect cleanliness. (Spiegelberg—*British Medical Journal*, September 3rd, 1881, p. 401).—It was said (Lister—*Lancet*, October 1st, 1881, p. 290) that "the relation of micro-organisms to diseased processes was being exaggerated. Whether bacteria exist only as one form, or in that of several different forms, is a point on which opinions differ. (See *Lancet*, *op. cit.*)—Portions of the brain, in cases of injury of that organ, contained substances that not unlikely were mistaken for organisms. (*Ibid.*, p. 299).—The hay bacillus acquires virulent properties when cultivated in an albuminous fluid. So does aspergillus. (Buchner—*Lancet*, October 1st, 1881, p. 547). —The bacillus anthracis could be converted into hay bacillus, and *vice versâ*. (Greenfield, Virchow, *ibid*). It was stated that this could be explained by an error in manipulation. (Pasteur, *ibid.*)—The transformation of a micrococcus had never been seen, therefore it was not believed in. (Pasteur, *ibid.*)

M. Béchamp refused to accept the principle on which M. Pasteur had founded most of his theories (namely, that of disease germs). It is not outside the body that we must look for "germs, or elements of destruction"; they are to be found within our body in the form of microzymes.

With regard to the *spirillum obermeyri*, cases of relapsing fever occur in which competent observers fail to detect spirilla in the blood. Kelbs is of opinion that the classification of this organism involves difficulties.

(*British Medical Journal*, August 13, 1881).—Persons were equally affected when inoculated with the blood of patients suffering from relapsing fever, no matter whether it contained spirilla or not. Observations on septicemic infection go to discredit the ministry of organisms. (*Indian Medical Gazette*, September 1st, 1881, p. 260).—The doctrine of specific organisms, as the cause of specific diseases, is alluded to as being only an *hypothesis*; its adoption as the *assumption* of such a *theory*. Kelbs observed that the task of microscopical pathological anatomy had been to demonstrate in many diseases the presence of organic foreign bodies in pathologically altered organs. But whether these little organisms are to be regarded as the cause of such morbid process, or are introduced from without, or that such normal inhabitants of the human body may, by alteration of the medium in which they occur, acquire virulent properties, and so become causes of disease, can only be regarded as *possibilities*. But even supposing this possibity were a fact, it would still not be possible for every disease to be produced in this way; thus, in times of epidemics of cholera, that disease may be induced by sudden fright or chill (*British Medical Journal*, August 13, 188I).—Dr. Grawitz believes that even some of our most innocent microphytes can be changed by artificial culture into disease germs of deadly infectiveness.—Dr. Roberts suggests that disease germs may be " sports " from harmless saprophytes; Dr. Smith, that putrefaction in confined places might give rise to them; Mr. Faraday, that innoxious germs inhaled into the lungs; may there undergo sucessive cultures. (*Medical Times and Gazette*, October 7, 1882). And much more of equally divergent views might readily be quoted.—Drs. West and Jacobi, speaking in the section of diseases of children, said that the presence of bacteria, or other organisms in the membrane of diphtheria, does not prove that they are the cause of that disease. It is hardly possible to think of a neutral living organism. (*Monas tuberculosum* of *Klebs*) being charged with the power of conveying so complex details from one body to another. (Creighton—*Nature*, October 17, 1881).

The Press, professional and non-professional, in 1881, discussed the subject thus prominently brought forward; and, from articles which then appeared, the following summary is made:—" The presence of minute organisms in diseased tissues being admitted, it is observed that, since the healthy tissues have a resistant power which preludes the development of these organisms in or upon them.—How can an artificial experiment be transferred to the natural processes of the body? To that extent the question is *sub judice*." (*Westminster Review*, October, 1881).

Observations on what for the time being are called "disease germs" indicate a relationship between their development and the varying circumstances of season and climate. Certain of them multiply under the influence of warmth and moisture, and their relative abundance in dust varies inversely with the degree of humidity of the air—such as are of the nature of algæ and moulds prevailing during rain, bacteria during drought. (*Medical Times and Gazette*, October 8, 1881).—But our friends, the germ theorists, will forgive us if we take this opportunity of pointing out that the public is being immensely "fetched" by the simple and attractive notion of numerous common diseases being caused by aërial swarms of organisms, which have, as it were, declared war against mankind. The desire on the part of the public, acting on the specific-germ theory, to isolate the very causes of disease, and to stamp them out, is based upon an intellectual conception of disease, which is curiously inadequate to the complexities of the case. (*id.* October 15, 1881).

Dr. Richardson protests against the attempt to connect our knowledge of the minute organisms with the phenomena of communicable diseases, or to assert, as some do, that the diseases in question arise from germs; that the person affected has been fertilised like a piece of ploughed land or virgin soil with a crop of germs. (*Medical Press and Circular*, November 23, 1881).

The opponents of the germ theory hold that such organisms are the result (*not the cause*) of the morbid conditions of their habitat:—(1) Because, whether bacteria, parasites, or fungi, they are only found on the parts of animals or vegetables of lowered vitality, or in their dead tissues; (2) That by strengthening the vitality in living, or arresting decay in the dead tissues, they disappear; (3) That when present in infectious matter it loses its power to infect, as observed in small-pox and vaccine virus. (*id.*, December 14, 1881).

In reviewing (experiments having reference to the germ theory disease) it is clear that we are either on the eve of a great discovery that will revolutionise pathology and therapeutics, or are being entangled in the meshes of a marvellous delusion. (*Indian Medical Gazette*, February, 1882).

Hitherto the advance of bacterial pathology has not resulted in that increase in our power over developed disease, which might have been anticipated from so considerable an addition to our knowledge of the nature of morbid products. (*Lancet*, September, 1882, p. 456).

Up to the present time the precise relation which exists between micro-organisms and particular forms of morbid action is a matter of doubt and uncertainty; that they are themselves the effects, and not the cause of those morbid actions, is admitted by inquirers, whose conceptions extend beyond "the infinitely little"; but that knowledge on this point has gained nothing from "experiments" is apparent from the following extract from a leading medical journal, namely :—"The relation of the micro-organisms found in pus, with the process of suppuration itself, has been carefully investigated, and several important papers have been published, especially in Germany. The experiments consisted generally in subcutaneous injections of sterilized irritating fluids, or in the introduction under the skin of aseptic solid bodies. The results, however, *have been contradictory*." (*British Medical Journal*, February 7, 1884, p. 281).

Micro-organisms could live in the body; but the question is, were the changes called disease produced by them. In spite of spray and carbolic-acid, micro-organisms existed in the serum and pus of "sweet" wounds. Lister's success depended upon something else than the exclusion of germs (namely, cleanliness.) Many experiments had been performed by cultivating micro-organisms, and injecting the culture-fluid into animals. The evidence was conflicting; the balance of evidence appearing to favor their *harmless* nature. The fact of old putrid fluid being less virulent than fluid recently putrid, while both swarmed with micro-organisms, indicated that the organisms were not the cause of that virulence. There were animal alkaloids which could be obtained from putrifying corpses, and these were of different degrees of poisonous activity according to the time the process of decomposition had been going on. Bacteria were present alike in poisonous and nonpoisonous blood, and they could be injected into living tissues without harm. There seemed to be no grounds for the assertion that anthrax was a parasitic disease, or for supposing that bacilli were the cause of small-pox, scarlet fever, measles, &c.; no special, constant micro-organisms had been found in these diseases. Micro-organisms were always present, but epidemics were not. Epidemics might, under certain conditions, arise spontaneously. The fact that bacilli could be cultivated outside the body was an argument against their being the *cause* of disease; if they could thrive outside, it was difficult to understand why there were not epidemics all over the country. If, as stated by Cohn, the bacteria (in typhus fever) had so great a power of quick multiplication, why should the incubative period be so slow and irregular? The parasitic theory of disease did not solve the problems with which physicians and surgeons had to deal; nor could Dr. Simpson see in anything that had been advanced, any reason why he should embrace the germ theory, and throw over the deductions long made from clinical experience. Bacteria were only carriers. There was (in disease) some occult influence at work more than bacteria could explain. (*British Medical Journal*, April 28, 1883, pp. 815-817).

If our correspondent will consult the index of any of the medical journals of the last ten or twelve years, he will have but little difficulty in recognising a few of the defunct "germs" to which we have referred. History repeats itself, and we sometimes wonder whether a few of even the most popular of the germs of to-day may not ten years hence be distinguished by an epitaph only. (*British Medical Journal*, December, 1883). We, as clinical investigators, will not follow pathologists into finding micro-organisms in almost every disease under the sun, and, in assuming that because a micro-organism is *post hoc*, it is of necessity *propter hoc*. (*id.*, July 5, 1884).

The German theory of germicides afforded no help whatever in the cure of disease, for experience had shown that germs could not be killed in a body without the patient being killed also. When once germs had entered the body, they could neither be forced out, nor sweated out, nor driven out by diuretics, nor diverted to some special part by the use of derivatives, rubifacients, blisters, &c., nor be killed within the body by germicides. (Dr. Hayward, Homœopathic Congress, 1884).

Professor Pèter considers that thus extended (as the germ theory has recently been), the doctrine of microbes, as followed up by M. Pasteur, constitutes "a social danger." (*Les Maladies de la Vigne par Chaveé-Leroy*, p. 60).

Remark.—It is upon this theory of "germs" in relation to disease as above summarized, that the modern practice of "Zymotic Inoculation" has been based. That theory is shown by the authorities above quoted to be no more than an hypothesis, inapplicable, and therefore useless as regards practical medicine. And yet such is the "base" upon which the "fabric" of that form of "inoculation" is reared and propped up by a certain class of eminent "scientists." As is the foundation, so is the stability of the fabric reared thereupon.

ZYMOTIC INOCULATION.

The fact that diseases of various kinds are readily communicated by means of "inoculation" was well understood many centuries ago, very often from practical experience, by people of all classes and of every country—notably those affections which belong to the enthetic class. Medical officers in the army in former days knew very well that among soldiers purulent opthalmia was thus transmitted. The negro slaves in the West Indies and in Africa knew how to inoculate themselves with the disease (Framboesia) called Yaws, as did also certain native tribes in Ceylon, particularly in the Vanni district, as a prophylactic against the affection there known as parangi. According to an account given by Dr. Wright, the method followed in Ceylon is to make children, when they are about one year old, partake of rice off a leaf or plate from which a person suffering from parangi has eaten. In a short time pustules like itch appear on the child's body, and then medicine containing minute quantities of mercury are administered, which was said to cause the pustules to dry up in seven days, the scales to fall off, leaving deep dark marks, which in course of time disappear. It is stated that this is an almost certain prophylactic, and that though the disease may attack one who has been so guarded, the effects are never serious. (*Medical Press and Circular*, February 15, 1882).

The theory, according to which the system of inoculation followed by M. Pasteur, dates from a time antecedent to his experiments. For example:—In 1864 M. P. Schützenberger wrote to this effect:—"All contagious diseases are evidently produced by the introduction of foreign poisonous substances into the living organism, producing true poisoning. The hypothesis is that these poisons are living germs of ferments, or living ferments already formed. During ten years (subsequent to that date) skilful experimentalists, guided by the same ideas, among them Pasteur, have studied this subject with great care, and yet, it must be admitted, there has been no result from these inquiries. The question of the etiology of infectious diseases has made no important advance." (Fermentation International: Scientific Series, p. 250). Another ten years have passed, and the same remarks are applicable, namely:—"There has been no result from these inquires, and the question of etiology of infectious diseases has made no important advance." Indeed, none whatever, as a comparison of writings by experimenters and their advocates, sufficiently proves.

But "experiments" have demonstrated the fact that the results of inoculation is not always or necessarily the reproduction of the self same disease, the morbid product of which was inoculated; nor is the resulting affection necessarily specific in its nature. For example:—Professor

Heubner declares that the disease which arises after inoculation of true diphtheria matter is not true diphtheria, as inoculations of rabbits with the oral secretions of healthy individuals produced sometimes septicemia, sometimes malignant ædema, and sometimes necrosis. (*Medical Press and Circular*, April 23, 1884, p. 376). That various diseases cannot be communicated to man or animals has been demonstrated by observation and by "experiments" performed more recently. According to Clot Bey, true plague is one of these. (*Medical Press and Circular*, Jan. 10, 1883). No man knows whether rabbits are susceptible to true glanders, although such inoculation produces in them a new disease, not unlike septicemia, similar to that which can be produced in those animals by inoculation with almost any decomposing material. (*Medical Press and Circular,* January 17, 1883). It is not satisfactorily proved that the disease produced by the inoculation of the *Bacillus tuberculi* in animals is the same thing as the disease supposed to be produced by that organism in man. (*Medical Times and Gazette*, November 3, 1883).

Micro-organisms from human patients affected with typhoid fever, leprosy, cholera, &c., do not produce those diseases in animals subjected to inoculation with them. But medical men are not at one as to the so-called bacilli of typhoid fever and leprosy being the veritable causes of those diseases. (*Indian Medical Gazette*, May, 1884, pp. 142-145). And, besides the "anomalies" above related, there are diseases which cannot be transferred from animals to man; among these rinderpest, and pneumonia of cattle. (*British Medical Journal*, Sept. 1884, p. 455).

Adverting to the subject of proposed vaccination against the various diseases, to which animals that yield food to man are subject, it has been asked:—"Are we and our cattle to be porcinated against measles, equinated against glanders, caninated against rabies, and inoculated with sheep rot?" But modern experimenters are on the way to taint the whole community, and all our domestic animals with disease, for the sake of the public health. If we are to add, charbonising our cattle, and porcinating, equinating, and caninising ourselves, have we not reached something like a *reductio ad absurdum* of the method of inoculation. (*Zoophilist*, October 1881, p. 103).

To inoculate, and re-inoculate sheep, for rot or other formidable disease should only be considered as a possibly useful experiment. The effects which might be produced on the quality of thoroughly inoculated mutton as human food are unknown. Th instincts of the people generally would certainly lead them to prefer the healthy mutton to that which has been inoculated with various formidable diseases. (*Modern Review, October*, 1881, p. 758.)

Perhaps the modern desire to perform inoculation as a prophylactic against, and as a remedy for, diseases in general can find no better illustration than in the claims put forward by a French chemist, that he has discovered a method of averting *Phylloxera* in vines, by "inoculating" the plants with phenol. He asserts that the vines are in no way affected by the process. (*Knowledge*, *January* 19, 1883, p. 31.)

"How it will be?" it has been asked, "when we and our cattle are to try twenty different kinds of vaccination for twenty other diseases? Are we, and our oxen, our sheep, our pigs, our fowls (that is to say, our own bodies and the food which nourishes them) all to be vaccinated, "with various diseases," twenty times in our lives, or in a year? Are we to be converted into so many living nests for the cultivation of disease germs? Is our meat to be saturated with "virus," our milk drawn from inoculated cows, our eggs laid by diseased hens—in short, are we to breakfast, dine, and sup upon disease by way of securing the perfection of health. (*Contemporary Review*, *April*, 1882.)

In Germany the reaction against the inoculation fever has set in. In addition to "canination" for hydrophobia, and "equination" for glanders, "ovination" has been carried out against sheep-pox in Germany. Against this practice, Virchow in supporting a bill introduced with a view to prohibit the further practice of inoculation of sheep against sheep-pox, asserted that, whatever benefit might accrue to the individual sheep, it kept the disease alive and active among the ovine population. (*The Times*, *Nov.* 14, 1885. See *Zoophilist*, *Dec.* 1885, p. 131.)

Dr. Koch has written a pamphlet in which he combats the theory on which M. Pasteur advocates his system of "protective inoculation"; Dr. Koch says it is not proved that all infectious diseases are parasitic in their nature; also that the parasitic nature of each disease must be proved separately. (*Medical Press and Circular*, *Jan.* 17, 1883.)

In France professional opinion is becoming pronounced against the theory of "zymotic inoculation." According to the *Gazette Medicale* de Paris, November 28, 1885:—"The researches with regard to attenuated virus and vaccine pursued by savants as competent as they are upright may give rise to dangerous "preservatives" and "vaccines," should less scrupulous men than they take advantage in a pecuniary sense of the confidence in those preservatives, and vaccines entertained by the public. If the pretended liquid inoculated be not the veritable vaccine, it may prove more dangerous than nonefficacious. According also to a well-known writer in that country, "the public begins to recognise the fact that the inoculators laugh at it; that they speculate on its ignorance of scientific questions. (*Les Maladies de la Vigne par Chevré, Leroy*, p. 65.)

The remark has been made by Dr. Carpenter that, with the diminution of small-pox mortality, which has occurred in recent years, and which has been considered to be due to, and a result of, compulsory vaccination, there has been an increase in that by measles and scarlatina, exceeding that which increase of population would account for. (*Nineteenth Century, October* 1881, p. 538.)

Dr. Longstaff observes that medical science, like most other things, is more or less under the dominion of fashion; hence the same disease is at different times given different names. He points out that, while certain affections, among which are zymotic diseases, developmental diseases, phthisis, diseases of the stomach and intestines, and "sudden death, cause unknown," decreased during the five years ending 1879, certain other diseases increased in fatality during the same period, among them diseases of the lungs, heart, brain, kidneys and liver; cancer, diphtheria and croup, tabes mesenterica, whooping cough, rheumatism, and "other causes," the total rise in the deaths from non-zymotic diseases being 212 per million. Urban deaths are much higher than rural deaths. Whereas also forty years ago, for every 100 female children born there were 104·8 male children; there are now only 103·9 male children born. (*Epidem*: *Society*, *March* 18, 1882)

Remark.—It is to be observed with reference to these particulars that a variation in the rates of mortality is noted in regard to those diseases of the "zymotic" class, against which no vaccination or inoculation is practised; that whereas we have, in the first instance, an increase of that class, we have in the second a decrease, that decrease concurrent with that in the prevalence of disease, against which vaccination is employed as a protective measure. Other circumstances also, notably the difference in the proportion of sexes of new-born children, indicate the effects of a *law*, the investigation of which is beyond the capacity of "scientific research."

INOCULATION AND VACCINATION FOR SMALL POX.

Practices more or less similar to, or closely identical with that to which in very recent times the term, "Zymotic Inoculation" has been applied, have been in use by various races of people, civilised and uncivilised, in different countries, and for periods coeval with history. That particular practice regarding which the most reliable particulars have come down to our day, have reference to the prevention, modification, or continuance of small pox, and included the processes of inoculation and vaccination. As already noticed in these pages, inoculation with and for "Heaven's Flowers," has been practiced in China for many centuries, and primarily with a religious object. In India, "inoculation for cow pox" was known of old times to the Hindoo medical writers. In the *Sactéya Grantham*, after describing nine different species of small pox, of which three are declared incurable, the author lays down rules for the practice of inoculation, thus: "Take the fluid of the pock of the udder of a cow, or the arm of a human subject, on the point of a lancet, and lance with it the arm until blood appears; then, mixing the fluid with the the blood, the fever of the small pox will be produced." The Ooriah Brahmins (in Orissa) took "a certain quantity of cotton, wetted with the matter of a favourable small pox, and a cut being made in their arms, it was thus inoculated."—*Madras Courier*, January 1819; see also *Med. Press and Cir.*, April 16, 1884, p. 365. In Rome, A.D. 82-92, some kind of inoculation for small pox is believed to have been practised. In certain parts of Britain, more particularly Wales, inoculation for small pox, under the name of "buying the small pox back, existed as distant as tradition can be traced (Rees's Encyclopedia); and various methods of performing the operation, brought down from ancient times are detailed. The Highlanders of Scotland have also, for ages, performed a kind of inoculation, and the practice has also been followed in Germany, Denmark, Sweden, France, and Italy; in Barbary and the Levant. In the year 1722, experiments with inoculation for small pox were first performed in London on six condemned criminals; next on five pauper children. (Dr. Guy on Public Health, p. 202).

In England, the popularisation of empirical knowledge in regard to vaccination, properly so called, occured in the following manner:—Dr. Jenner observed that many of the people in the dairy district of Gloucestershire enjoyed an immunity from small pox; that cows had occasionally a pustular eruption on the udder; that the persons who milked them had a similar eruption on their hands, and that these enjoyed the immunity from small pox. This was the long known, in fact, from time immemorial experience of the country people. He observed that the cows affected had been milked by persons who had had handled horses suffering from "Grease" in the foot. Dr. Jenner inoculated a boy with this animal virus instead of with small pox matter, but in the year 1796, on his being inoculated some month afterwards he was proved to be secure against small pox. A number of children were inoculated (vaccinated) one from another, and after several months they were then inoculated with, or otherwise exposed to the infection of small pox, but all resisted it. The discovery was made and the demonstration completed before a single experiment had been made upon a living animal. A few experiments were afterwards made, but not by Jenner. (Three Prize Essays on Vivisection, p. 29.)

At the present day, and notwithstanding the long continued experimentation that has taken place in respect to the relationship between small pox and vaccination the results obtained "have not been in satisfactory agreement with each other." In England the majority of physicians are "probably" inclined to accept the views of Messrs. CEELY and BADCOCK that cow pox and small pox are the result of one and the same "poison." In France, the prevailing opinion seems to be in favour of the duality of the "poisons" of vaccinia and variola. (*Indian Med. Gazette*, January 1884, p. 27). And thus the question stands at the present time—unresolved.

INOCULATION FOR SHEEP POX.

Although inoculation with the object of producing variola in a mild form in sheep has been largely practiced on the continent; yet, from time to time, writers have alluded to the disadvantages attending it as a general measure. It is stated that:—

1.—It gives disease to animals which would not, perhaps have had it at all.

2.—It is capable of producing as deadly and malignant a form of the malady as that due to natural infection.

3.—It causes loss when performed at certain times; retards the growth of lambs, by diminishing the secretion of milk in the ewes, and often interferes with the selling of the animals at a favourable opportunity.

4.—It is often possible to preserve the sheep from infection, by isolation, quarantine of foreign flocks, &c. ; and to introduce (by inoculation) the disease into these places, and infect all the sheep therein, is hurtful and unnecessary.

The practice of "ovination" for sheep pox, followed in Germany, has been protested against by Virchow. As already observed, he vigorously supported a bill to prohibit that practice, on the ground that whatever might accrue to individual sheep, it kept the disease alive and active among the ovine population. (*Med. Times*, Nov. 14, 1885).

Fürstenberg declares that in Eastern Prussia, thousands of cases of infection could be traced from inoculated flocks, which were so many centres of contagion from which the malady might radiate, and in many countries, that the malady was only maintained by this practice. So serious have the results of "preservative inoculation" been found that special establishments have (Mr. FLEMING believes) been abolished in those countries which instituted them. It will generally be found that the danger of diffusion of the contagion offers a formidable obstacle to inoculation. (*Veterinary Science and Police by Fleming*, Vol. II, p. 59).

Thus, both arguments and facts are adverse to the practice above alluded to.

INOCULATION FOR ANTHRAX.

Upwards of forty years ago, Dr. Davaine expressed an opinion that anthrax or splenic fever in cattle and sheep is identical with the plague sent upon the flocks and herds of ancient Egypt. He subsequently entertained the opinion that a microscopic organism found in the blood of animals so affected, was the *cause* of the disease. The observation was made, however, that in other diseases other microscopic beings were discovered in the blood, also that the most deadly among them was undistinguishable from the most harmless. (*British Medical Journal, Oct.* 8, 1881). In 1884 the work by Koch, on the Etiology of anthrax, appeared; in the year following that by Pasteur, on the same subject, was published.

On the subject of M. Pasteur's inoculations against anthrax, the statements occur. (1.) That he has demonstrated that animals of the ovine and bovine species may be prevented from contracting the disease charbon, by inoculating them with attenuated germs, ascertained to be the efficient *cause* of the disease. (2.) That the bacterium of charbon is capable of retaining its vitality for several years in the earth, and that, when brought to the surface by worms, it is capable of infecting the animals which eat the grass polluted by its contact. (3.) That in respect to anthrax the average mortality of sheep, oxen, and horses, has been reduced by these inoculations in the proportion of 10 to 1 for sheep, and 15 to 1 for oxen, cows, and horses. According to statistics at the same time given, regarding the ravages made by this disease, in France and foreign countries, the losses by it during the three years, 1881 to 1883, both inclusive, are stated to have been 353,330 sheep, 32,230 oxen, and 1,346 horses. *(British Medical Journal, Dec.* 6, 1884.*)*

The statistical particulars above given are reported to be derived from official records. In the absence of a distinct reference to the records in which those particulars are contained, and of which personal examination could thus be made, it is only practicable in the observations that follow, to utilise such observations on the general subject as occur in reports and other published documents quoted in connection with the abstracts taken from them respectively. By this process, somewhat indirect indeed, we shall be able to appraise the value of figures that have been officially given. But taking the figures above extracted, as they stand, the questions present themselves :—When, and where, did the reductions in mortality, as indicated, take place? What was the precise rate of mortality of anthrax, and of the particular diseases which are known to have been induced by "vaccination" prior to, and subsequent to, the introduction of that practice? What the comparative mortality from these combined causes, in districts where that practice was followed, and in others in which it was not? A simple examination of the figures as they stand is sufficient to indicate to a person accustomed to deal with statistical information, that in the form in which they are presented they give no definite information whatever.

In several of the daily journals, a paragraph to this effect appeared towards the end of 1885, namely :—

Since June last, M. Pasteur has vaccinated 90,000 head of stock, among which were 10,000 oxen, cows, and horses. In every instance his process was successful: the animals vaccinated escaped the charbon malady, while those non-vaccinated fell victims to that plague. M. Pasteur (and others have corroborated his view) lays down, that the effects of his preservative vaccine do not last longer than eight months, so that vaccination must be repeated annually, and that April is the best month for executing the operation."

In the paragraph above quoted there is no information given in regard to the state of health of the animals vaccinated, or of the mortality among them by diseases other than anthrax; neither do we learn what estimate was their flesh held in as food. We do learn however, on the authority of the paragraph itself, written as ostensibly it was, to advocate the "experiments" it alludes to, that the "protective" influence of the vaccination extended no longer than the duration of the particular epidemic in which it was performed.

M. Pasteur himself expressed a guarded and qualified opinion with regard to the advantages claimed for this kind of "vaccination"; he even expressed an opinion that the money grant proposed for creating a laboratory, in which to prosecute his experiments, was premature, "The pest," anthrax, he remarked, "does not prevail to any considerable extent during winter. By the month of March or April vaccination may be usefully commenced. During the first year it will be necessary to watch the state of the preserved vaccine, and *the permanence* of its protective power." (*British Medical Journal, October* 1881, p. 569.) [It is therefore evident that doubts with regard to the points mentioned existed in the mind of M. Pasteur.]

Dr. Spinzig observed that M. Pasteur advised the farmers of France to vaccinate their sheep to prevent them from taking anthrax, the so-called splenic fever, but, that as this splenic fever only prevails where there is low ground, the sheep in such localities become affected with that disease, while those on high ground escape it. M. Pasteur stated that if they are properly vaccinated it does not signify where they are. In Germany it was found that this was not the case. M. Pasteur said that when a flock of sheep died from splenic fever and were buried, and other sheep were grazing on the same ground they would die, even when grazing, some years afterwards. Now, this is a gross delusion. (*St. Louis Medical and Surgical Journal, December*, 1881, p. 611.)

According to M. Chavée-Leroy, M. Pasteur inoculated the sheep, and by a happy coincidence, the outbreak of charbon suddenly ceased, as often happens with epidemics; advocates of the theory of microbes, however attributed the result to the inoculations, without reflecting that the animals that had been inoculated did not constitute a thousandth part of those that had not been so. (*Les Maladies de la Vigne*, p. 52.) The vaccine of M. Pasteur against anthrax has been the means of destroying horses in the department of Aisne, and cattle and sheep nearly everywhere, without producing any certainty of preservative action. (Id. p. 55.)

At a Meeting of the Academy of Sciences, October 17, 1881, the subject of "Charbon" in cattle was discussed. The circumstance was noticed that in localities where the disease prevails, only calves of one or two years of age are attacked by it; that after the age of four or five years they are exempt from the affection. These experiences appear to have been obtained from observing the herds of cattle in Bassigny. In Algeria, cattle brought from the plateaux, where charbon is absent, to the plains where it prevails, became attacked at all ages. According to M. Boulay, the lambs of sheep in localities where charbon was endemic, or that had been inoculated for the disease, were exempt from it.

On a subsequent occasion the subject was thus alluded to by Dr. Friedrich of Munich :—Starting from the idea that a connection exists between the disease Anthrax and meteorological conditions, he concluded that anthrax is also a soil disease, and associated with some localities. A close connection exists between it and the rainfall, the disease becoming more frequent according to the dampness of the soil. A second factor in the outbreak of the epizootic is a certain temperature. The bodies of dead animals themselves, with their sanguineous discharges constitute one of the principal causes why the disease does not die out in a locality. (*Medical Press and Circ.*, Jan. 20, 1886.) Mr. Fleming also points out that the disease is enzootic, or even panzootic in many districts, as for example, such as are marshy, low, or that have tenacious soil. (*Nineteenth Century* March 1882).

M. M. Arloing, Cornevin, and Thoms have investigated the well-known immunity of adult cattle from bacterian anthrax in the infected districts. They ascribe that immunity to a gradual and infinitesimal vaccination which they have undergone, since aged cows and oxen from districts where anthrax is not even known, if brought into an infected locality succumb as readily as do the calves. (*Medical Press Opinion*, Jan. 4, 1882, p. 11).

The hopes which were excited by these investigations seem to have been much damped by experience; for it is found that, with the exception of sheep, and perhaps of cattle, other animals are not at all, or only with great difficulty, rendered immune; *while a larger proportion seem to die from the vaccinations than succumb to the disease naturally.* The tendency seems to be now to cease inoculating for this disease till some safer and more certain means has been devised. (*British Medical Journal*, December 30th, 1882).

Dr. Koch was of opinion that in anthrax, the law of immuuity, in Pasteur's sense cannot be maintained, for Loeffler, Gotti, Guilebeau, and Klein, have shown that no such immunity is obtained in the case of guinea-pigs, rats, mice or rabbits; and man himself can be attacked more than once. It is only among sheep and cattle that any immunity is conferred by preventive inoculation; and with these it remains to be proved that the results out-weigh the immediate risks, and how long the immunity persists. It is equally remarkable and important, "that deaths from other diseases, such as catarrh, pneumonia, pericarditis, etc., occurred exclusively amongst the inoculated. It follows from this that the fatal issue of other severe but latent diseases is acclerated by a protective inoculation." (*British Medical Journal*, Nov. 3, 1883).

Dr. Koch asserts as the result of his own observations, as well as those by Loeffler, Gotti, Guilebeau, and Klein, above alluded to, that, from the uncertainty and short duration of such immunity as these inoculations afford, as well as the danger to which not only the animals themselves, but those who have not been inoculated, and human beings in contact with them, are exposed, Pasteur's protective inoculation cannot be looked on as of any practical value. (*Lancet*, March 17, and *British Medical Journal*, May 5th, 1883).

At Beauchery, in April, 1884, 296 animals were inoculated with attenuated anthrax virus. Of these animals, four died from spontaneously acquired anthrax, between June 22 and 24; while a flock of 80 non-inoculated, and therefore "non-protected" sheep, were for purposes of test, under exactly the same circumstances, did not lose a single animal. At Montpelier, on 18th April, 1884, 220 sheep were inoculated with the preliminary vaccine, and nine of them died from the effects of the inoculation. A week afterwards the survivors were again inoculated with the preliminary vaccine, and nine of them died from the effects of the inoculation. A week afterwards, the survivors were again inoculated, and seven animals succumbed. A fortnight later the remainder were inoculated with secondary vaccine, when one sheep died. From June 11th to 13th six sheep died of anthrax spontaneously acquired. A fourth inoculation was followed by the death of the last half-dozen animals. Accordingly, M. Koch whosubmitted the report now quoted, concludes that M. Pasteur's system of prophylactic "vaccination" is devoid of practical value. He says: "Preventive inoculation according to the method of M. Pasteur cannot be considered utilisable in practice. 1.—on account of the insufficient immunity it offers against natural infection; 2.—on account of the short duration of even this preventive action; 3.—on account of the dangers which it developes for mankind, and other non-inoculated animals." (See *Zoophilist*, Nov. 1, 1884).

Adverting to M. Pasteur's inoculations, performed in Hungary, Dr. Alader Roszahegyi wrote to this effect :—In Budapest and in Kapuvar, after the control infection, out of 69 inoculated sheep, a single one (1·45 per cent.) died, while out of 75 unvaccinated animals 70 (93·3 per cent.) died of anthrax. But there is much more to be learned from our experiments if we consider the whole course, and do not fix our attention solely upon the final results. First of all, some of the animals died, even after the first protective inoculation, with symptoms of anthrax. Fifteen animals at Kapuvar died of anthrax, which could only be attributed to the second inoculation—presumably because the inoculative material used was too virulent for the animals to withstand which had only been inoculated with the weakest material. These experiments were conducted with the greatest care, and, to a certain extent, for a theoretical demonstration. It is therefore feared that in the practice of inoculation, injection with anthrax and septic injections, may frequently occur. We cannot overlook the fact that, after protective inoculation, the deaths in which post-mortem examination indicated other diseases, namely catarrh, pneumonia, distoma, strongulus, and pericarditis, but not anthrax, occurred exclusively amongst the inoculated animals. From a practical point of view it is pretty nearly the same whether the loss is caused by anthrax or by other diseases; and therefore, deaths from them should be added to those from anthrax. If we do this and add together all the deaths which occurred after the protective inoculation, and the "control infection," we shall get as the chief result that of the uninoculated animals 94 per cent., and of the inoculated, 14·5 per cent. died. Thus, very considerable difference remains, but the fear continues that in practice a higher rate of mortality would be reached. Against this experimental mortality of 94 per cent. of the uninoculated animals we must put only the rate of mortality among cattle in districts affected with anthrax, and which is very considerably less. But, inasmuch as three of the sheep inoculated at Kapuvar sickened and died after being subjected to the "control infection," the protection afforded by the inoculation is thereby shown not to be complete, it follows that these cases do not give support to the expediency of general adoption of the method in question.

At Kapuvar the epizootic disease anthrax has everywhere almost entirely ceased with the approach of winter, to return with the advent of warm weather. Till then it cannot be proved that these inoculations protect as perfectly from the natural anthrax contagion, as they do from the material used in the "control experiments." There are also doubts about the method from a public health point of view; thus the question is important whether the meat, milk, &c., of inoculated animals can convey anthrax to man; also, what period of time must elapse after inoculation before flesh and milk of the animals would with safety be used as food.

As a conclusion arrived at on the subject by Dr. Roszahegyi, he entirely concurs in the opinion of the (Hungarian) committee that the general application of Pasteur's method in the form demonstrated would be precipitate; that the performance of protective inoculation by private individuals should be completely forbidden, and only performed by a Government official. The committee is very far indeed from finally rejecting Pasteur's method. It is of opinion that the results already obtained justify the highest hopes of the possibility of its reaching perfection. Accordingly, it recommended to the Ministry that further experiments should take place. (*Practitioner, March*, 1882.) [But the fact remains that results obtained in practice are against the efficacy of anthrax inoculation. The foundation on which rests the suggestion for its continuance is—hopes!]

Dr. Klein arrived at conclusions adverse to the general adoption as regards England, of M. Pasteur's proposal to inoculate cattle with anthrax as a prophylactic measure against that disease. The results of a series of experiments performed by him led to the following conclusions, namely:—(*a*) Animals inoculated with this "vaccine" (premier and deuxième) are not made immune against fatal anthrax; and (*b*) both the first and second vaccine may produce fatal anthrax.

This country is comparatively free from anthrax, and therefore the introduction and use of this so-called "vaccine charbonneux" seems to me most dangerous, and capable of producing incalculable mischief. (*Lancet, September* 23, 1882).

When we consider that the inoculative material contains anthrax microzymes in colossal quantities, although of diminished virulence, and that the microzymes multiply to a gigantic extent in the organism of the inoculated animals, we see that the general employment of protective inoculations would spread these germs [the germ theory was evidently accepted as true by the noble writer] in inconceivable quantities through the whole country. (Sent to *Daily Telegraph*, by Lord Shaftesbury, *March* 17, 1882, as a warning to the farmers in England).

Dr. Klein criticised somewhat severely the views expressed by M. Pasteur, in 1881, on the subject of anthrax and inoculation for that disease. He pointed out that the results of experiments performed by him with the *bacillus anthracis* differed from those reported by M. Pasteur; that after a time those bacilli undergo degeneration in liquid cultivations, and although capable of producing the disease in certain animals (guinea pigs and rabbits) they were incapable of infecting others (white mice). It was found that two separate and consecutive specimens of M. Pasteur's *vaccin charbonneuse*, obtained from his accredited agent, entirely failed to produce the results claimed for them. Sheep inoculated with the fresh blood of a mouse which had died of anthrax suffered only in that its temperature rose slightly (*i.e.* it became feverish); they were not however in the least affected when subsequently inoculated with a virulent cultivation. Dr. Klein confirmed the statements of Gerlach and Bollinger that putrefaction destroyed the infective power of the blood, and organs of an animal dead of anthrax. He therefore concludes that M. Pasteur was mistaken in supposing that in animals dead of anthrax, and buried, the bacillus retained its infective power for years; and believes, on the contrary, that this power is quickly lost, owing to the complete degeneration of the bacillus. (See *British Medical Journal*, May 31, 1884, p. 1052).

M. Pasteur found on examining earth worms, in places where carcases of animals, dead by anthrax had been buried, that ten or twelve years previous an "extract" of the contents of these alimentary canals, when inoculated into rabbits and guinea pigs, gave rise to the severest form of charbon in them, "due to the multiplication in their circulating current of the deadly anthrax bacillus." (*Nineteenth Century*, October, 1881).

Dr. Koch demonstrated by experiments (as shewn above) that Pasteur's theory as to earth worms being the agents in infection of anthrax, was pure romance from beginning to end. (*Zoophilist*, February, 1883).

On the same subject Professor Greenfield writes:—
"The blood and tissues of animals dying of anthrax usually swarm with the *bacillus anthracis*. But it is not in every case of that affection that bacilli present this appearance of rods, or are found in such numbers. In some cases examined after death, the rods may be much longer than

four times the diameter of a red blood corpuscle. He remarked that it is scarcely safe—at the time he wrote—to draw any very general conclusion from the experiments which had been up till then performed. The blood obtained from the spleen swarmed with bacteria of ordinary decomposition. In one case there was evidence that decomposition had begun during life.

On the general subject of "protective" inoculation, against the disease under discussion, a periodical devoted to the interests of farming and farm stock, expresses itself to the effect that:—

"Against the theory of inoculation of cattle with virus of spleen fever, it is stated that the results of experiments differ in French and in English cattle: also, according to breed, and that results obtained on the former do not necessarily hold with regard to the latter." (Greenfield. *Journal of the Agricultural Society of England*, vol. xvii., part i., p. 30).

"The theory of 'anthrax poison' being brought to the surface by worms was shown to be inapplicable." (Wortley Axe, *ibid*).

In an outbreak of anthrax at Cambridge, in February, 1880, Professor Greenfield was unable to detect any bacteria whatever; no traces of bacilli or their remains were discovered. Cases of anthrax also occur in which the process does not affect the spleen. (*Journal of Agricultural Society of England*, vol. xvi., 1880, p. 278).

The same author endeavoured to ascertain whether any new modification of the poison of the disease could be artificially produced without the employment of animals for that purpose. With this view he cultivated the virus artificially in organic fluids, and found that it may thus be modified, so that a certain degree of poisonous activity may be attained at will.

He describes an outbreak of splenic fever on a sewage farm at Harden. To this farm three of the animals were sent, with the result that for several months they resisted the contagion, a circumstance which encouraged further investigation. These experiments, however, in their effects, are largely modified by the breed of cattle on which they are made; in fact, the relative susceptibility of various breeds is very different, so that in the practical application of any such method of preventive inoculation, what is good for French cattle does not necessarily hold good for English breeds. (Op. cit., vol. xvii., part 1, p. 41).

M. Pasteur, adverting to the results obtained by Dr. Klein, observed that the question of species and race should be taken into consideration.

A vaccine which vaccinates rabbits, vaccinates sheep very badly, or to a very small extent. Some breeds of sheep did not tolerate it at all, or tolerate it very badly. In one of the experiments undertaken in Italy, the virulent virus employed was septicemic as well as anthracic, the vaccinated animals died as well as the non-vaccinated.

As an illustration,—a guinea-pig inoculated with the blood of a dog suffering from distemper becomes affected with anthrax, a guinea-pig and a rabbit inoculated with attenuated anthrax virus becomes attacked with tuberculosis. (*British Medical Journal*, *Oct.* 11, 1884, page 717).

Observations by Buchner seemed to show that the bacillus of anthrax might be developed from a non-specific fungus found in hay. His experiments however were not conclusive. (*Lancet*, *Feb.* 18, 1882, page 279).

Dr. G. C. Henderson was doubtful as to the permanence of the protection alleged by Pasteur, Toussaint, and Greenfield to have obtained from their respective procedures. (*Medical Times and Gazette*, *Nov.* 19, 1881, p. 604).

As a result of some experiments on the inoculation of rabbits with the blood of animals dying of splenic fever, Professor Semner, of Dorpat, found that the disease produced was not anthrax nor ordinary septicemia, but a form of pyemia which he believed to be new. An inoculated dog died, on the third day, with purulent conjunctivitis and a low form of pneumonia. (*Lancet*, *Oct.* 29, 1881, p. 767).

Remarks:—Here, then, in the analysis thus presented, the important and suggestive fact appears that medical opinion is altogether against the claims put forward in favour of this form of inoculation; also the no less suggestive fact that the chief, if, indeed, not the only, advocates of that process are non-medical men.

SUMMARY OF REMARKS.

From what has transpired in this and the preceding sections of the present pamphlet, it becomes appropriate to offer the following observations by way of summary on the three main points adduced in support of the preventive inoculation for anthrax, and given at the commencement of this article, namely:—

1. *That animals of the ovine and bovine species may be prevented from contracting the disease charbon by inoculating them with attenuated germs, ascertained to be the efficient cause of the disease.*

Discredit is cast upon "germs" as such in relation to their causative, and their protective power against disease. They may be "carriers" of contagion, but nothing more. Any "protective" power inoculation may have is only against the artificial, not against the natural disease; that "protection" extends only to the duration of the epidemic prevailing at the time, and it is uncertain in degree. Mortality by other diseases than anthrax have destroyed many of the "protected" animals. It is questioned how far the flesh of those that have been operated upon in this manner can safely be made use of as food by man. In other countries than France, notably in Hungary, Germany, and England, this kind of "inoculation" has been unequivocally condemned.

2. *That the bacterium of charbon is capable of retaining its vitality for several years, and, if brought to the surface by worms, is capable of infecting animals that feed on grass polluted by it.*

The theory here stated is distinctly refuted by German and English observers, whose evidence is quoted. The circumstance is noted that the prevalence of anthrax varies according to season, atmospheric conditions, age of animals, and nature of position, including subsoil. Position, subsoil, season, and weather favourable to its appearance, are precisely those which bring earthworms to the surface of the ground. But that is all.

3. *In respect to anthrax, the average mortality of sheep, oxen, and horses has been reduced by these inoculations in the proportion of* 10 *to* 1 *for sheep,* 15 *to* 1 *for oxen, cows, and horses.*

See what has been said above (1) on this head. Statistics of mortality among animals inoculated, not only in Hungary but in France, are directly opposed to the averages here referred to as obtained from official statements. Moreover, the definite remark occurs that in the department of Aisne "the vaccine for anthrax has been the means of destroying horses, cattle, and sheep," and also the two latter species of animals "nearly everywhere."

Finally, therefore, inoculation for anthrax, for the reasons now given, wherever practiced is not only not beneficial, but, as described with especial reference to England, is "most dangerous, and capable of producing incalculable mischief."

INOCULATION FOR CHOLERA.

During the prevalence of cholera in Europe, 1853-56 Professor Thiersch inoculated 56 mice with the dejections of persons affected with that disease; of the animals so inoculated 24 sickened with symptoms of a choleraic sort, and 14 died. (*Transact*: *International Medical Congress*, 1881, vol. iv, p. 414).

Both the German and French Commissions in Calcutta, in 1884, made repeated attempts to communicate cholera to the lower animals. Mice, rats, monkeys, pigs, rabbits, guinea-pigs, dogs, cats, fowls, chickens, puppies, and other animals were experimented on. They were given cholera evacuations and tissues, fresh and dried, in their food; fluids containing these substances were injected into their bowels; cholera-blood was transfused into their veins—in short, every imaginable device was resorted to, but without success, except in one case in which a fowl, submitted to a diet contaminated with rice-coloured stools, died on the third day of the experiment. The contents of its intestines contained the particular body which the French Commission considered to be the microbe of the disease. But, although the intestines of this fowl were administered to others, and its blood inoculated into them, no result followed. (*Dr. Cameron's Belfast Address*, 1884, p. 8.) [From which it follows that the results of the two sets of "experiments" here recorded were mutually at variance with each other; the total result of the whole "negative" in character.]

"Could any species of brute," Dr. Koch added, "have contracted cholera, this must have occurred in Bengal, where choleraic infectional matter is spread throughout the whole year and whole country, and it must have been noticed in a reliable manner." But he could learn of no such observation. With reference to the inquiry and "results now recorded, the *Indian Medical Gazette*, January 1884, remarks that "The sole statements in this matter which are deserving of attention have been made by Thiersch, who saw a number of mice get diarrhœa and die after having been fed upon the contents of a cholera-infected intestine. This experiment has been confirmed by Burdon Sanderson, though certainly disputed by others. Monkeys, too, which are the only species of animals susceptible to some human diseases, such as small-pox, were used in these experiments, also dogs and poultry. But in spite of all efforts these experiments were without result."

Such are some of the most recent "results" obtained in regard to the question of animals in relation to cholera. The investigations now related are by no means new in India. A reference to the history of cholera in that country would have shown that experiments in all respects similar to the above had already been performed, and with results precisely similar, namely, negative; also that, although there is important evidence in support of the opinion that cholera cannot be artificially communicated to animals, the statement has been definitely made that, during an epidemic of cholera in man, the same disease has prevailed in certain animals. For example, reports having reference to the year 1872 state that the horses of the 19th Bengal Native Cavalry, then stationed at Lucknow, suffered from "a disease in all respects similar to cholera."

In 1884, while cholera was prevailing in Spain, Dr. Ferran adopted a plan of "protective inoculation" of human patients against that disease, with results which led to speedy condemnation and compulsory abandonment of his system. No fewer than 6,500 persons voluntarily submitted themselves to his "inoculation"—many of them with results, not from cholera but from blood-poisoning—disastrous to themselves. The Supreme Board of Health, therefore, by a majority, endorsed the action of the Government in prohibiting the further adoption of the practice. (*Daily News*, July 7, 1885.) In November of the year in which this species of "inoculation" was most extensively practiced, it had so completely fallen into dis-favour that, according to a medical journal of the day, "the method of inoculation, by Ferran, was deemed quite unworthy of consideration." (*Medical Press and Circular*, November 11, 1885).

And such was the transient career of this particular "protective" inoculation.

INOCULATION FOR "FOWL CHOLERA."

The coincidence of the affection of poultry so named, with outbreaks of cholera in man, has served to procure for it its particular designation. It affects not only all kinds of poultry, but also rabbits. Of poultry, the young birds are those which chiefly suffer.

M. Pasteur (His Life and Labours) adopted the "suspicion" entertained by M. Moretz, in 1878, that Fowl cholera is produced by a microscopic organism of the germs called *micrococci*. He prepared an infusion of the muscles of the (diseased?) fowl, neutralised by potash, and sterilised by a temperature of 110° to 115°, the infusion being made in the water of yeast. This medium, when neutralised, furnishes nourishment to the most diverse organisms; but it is unsuited to the life of the "microbe" of fowl cholera. This decoction, when inoculated in a very small quantity into a fowl, produces the disease (fowl cholera) and causes death. But in guinea pigs so treated, the only effect is usually an abscess at the spot where the inoculation took place; fowls inoculated with the contents of the abscess die rapidly, while the guinea pig from which the *virus* was taken recovers. Drops of the same liquid being placed on the food of fowls it becomes easy thus to account for the propagation of the disease; inoculation with the excrements of the infected fowls produces death. Hence the only means of arresting the disease is to isolate the fowls and chickens, to remove the dung heaps, and to wash the yard with water acidulated with sulphuric acid, or carbolised water, and thus destroy the "microbe," or at least suspend its development. (See p. 212—219).

By successive "cultures" the "microbe" of fowl cholera gradually loses its virulence. Inoculation with "culture" thus modified, produces in fowls illness, more or less, for several days. If, after their recovery, the same fowls are inoculated with a very virulent virus, they would perhaps become rather ill, but they would not die. The disease can thus "protect from itself." This is not unknown in pathology. Formerly it was the custom to inoculate with small pox to preserve from small pox. Sheep are still inoculated to preserve them from *rot*; horned cattle from peripneumonia. (p. 223.) Pasteur did not flatter himself that he has unravelled the difficulty; he has nevertheless amassed facts. (p. 228).

Dr. Spinzig, speaking before the Medical Society of St. Louis, observed that the views expressed by M. Pasteur before the Medical Congress on the subject of inoculations, and more especially inoculations for fowl cholera, were erroneous. M. Pasteur took it for granted that the disease chicken cholera was an entity What were its causes he did not say, only that it was an ontological *something*, requiring no other inquiry as to its origin or nature. M. Pasteur brings nothing to support his theory of bacteria or the product of retrograde metamorphosis. (*St. Louis Medical and Surgical Journal* Dec. 1881, p. 611.) Koch does not admit the high value claimed for preventive inoculation in this disease of the poultry-yard. (*Medical Press and Circular*, Jan. 17, 1883.) Elsewhere he expressed his opinion that the hopes raised by M. Pasteur respecting fowl cholera have not *apparently been realized;* for we have not as yet learnt of aviculturists having practised inoculations with the attenuated virus of fowl cholera.—(*Lancet*, March 17, 1883).

As previously observed in these pages, "we now hear nothing regarding inoculation for fowl cholera." It has run its brief career, and only its epitaph remains.

INOCULATION WITH SYPHILIS.

About five and twenty years ago a proposal came from Norway to employ "vaccination" with the virus of syphilis as a means of diagnosis, and of cure in that disease. The method was abhorrent to professional opinion of that day in England, and except by a very small number of men, it was never adopted; even by that small number it was quickly adandoned.

Professor Neumann inoculated various animals with syphilitic virus taken direct from the diseased person. In *no* case did any result obtain other than such as would naturally be expected to follow from the introduction of an irritant material into the tissues. Nothing bearing any resemblance to a chancre was witnessed. The subjects of the experiments were apes, rabbits, a horse, white rat, marten, and cat. Fifty-four inoculations were performed in all; and as a grand result of his repeated experiments Neumann forms the conclusion that syphilis is a peculiarly human disease, and is not exhibited or contracted by the lower animals. (*Medical Press and Circular*, May 30, 1883).

What, then, are the results of the "experiments" above detailed? Simply that the malady above named is a disease peculiar to man. [This circumstance in no way interferes with the fact that animals have their disease which bears the same name.]

INOCULATION WITH LEPROSY.

Unlike syphilis, leprosy has no power of self augmentation in the system, or of perpetuating itself by transference to the healthy. We should be rash to conclude that a bacterium, described by Hansen, Eklund, and Neisser, had anything to do with the causation of leprosy. Though one expects to find maggots in a dead body, yet we do not attribute the existence of the body, or its death, to the maggots; neither, when we find bacteria in the tubercles of a leper, should we, without other evidence than mere concomitance, attribute the tubercle, and the leprosy, to the bacterium.

The *bacillus lepræ* may, and probably will, turn out to be a Mare's nest, like so many of its predecessors. (*Epitomé of Medical Reports, Chinese Imperial Customs*, pp. 150-152).

Mr. J. D. Hills, in his work on Leprosy in British Guiana (p. 145) alludes to experiments described by Neisser as having been made by inoculating animals with the *bacillus* of leprosy. According to those experiments inoculation with leprous matter produced no result. In dogs the same experiment was straightway followed by the leprous new formations, "namely, a nodule under the scar of the incision, which increased in size." Mr. Hills adds, "Neisser observes that whilst in man leprosy becomes a constitutional disease, in animals only a local leprosy was produced."

The *British Medical Journal* of January 17, 1882, contains (p. 917) an extract from Virchow's Archiv. (Vol. XXXVIII., 1882.) According to that extract Professor Köbner, of Berlin, gives an account of an attempt to inoculate leprosy on animals. The results of those experiments was negative. A (leprous?) tubercle was excised from the thigh of a patient of healthy family, who had contracted the disease in Pernambuco. Inoculation with the tubercle and with its "juice" were made upon various animals, including a monkey. The monkey died 126 days afterwards of *tuberculosis*, but no leprosy tissue was found, either in it or in any other of the animals inoculated.

From the details above given, what are the conclusions to be drawn? Simply that leprosy is a disease of humanity. This fact was acknowledged by early physicians and in the absence of "experiments." [See St. Luke's Gospel, chap. xvii.]

INOCULATION FOR YELLOW FEVER.

In recent publications of the *Anglo-Brazilian Times*, the Government Commissioner, Dr. Domingos Freire, gives an account of experiments establishing (so he states) the facile transmissibility of yellow fever by inoculation, &c., and proving also that the disease is due to a most rapid development and increase of a cryptococcus, to which he has given the name of c. xanthogenicus. In experiments previously narrated, inoculation with a drop of blood from a person dead of yellow fever caused the death of a rabbit in a short space of time; a drop of the rabbit's blood had brought about the death of an inoculated guinea-pig, and a second guinea-pig inoculated from the first, died with fever and grave symptoms exactly like those of yellow fever in man, while the autopsy immediately after death revealed lesions completely in agreement with those observed in the preceding cases.

Following the method of investigation pursued by M. Pasteur in regard to the (supposed) presence of the *bacillus anthracis* in the soil of particular localities [which theory has been refuted by competent authorities, as described more in detail in the section of this pamphlet devoted to that disease], a mixture of earth from the Jurujuba cemetery was made with pure water. Ten drops of the mixture were diluted with a gramme of water, and some drops of this dynamisation were vaccinated under the skin of a guinea-pig, which died next day. With its blood another was inoculated, and died in three days. The advanced putrefaction of the latter prevented use of its blood for a third inoculation. However, it is clear that the earth in question (taken from a cemetery of yellow fever dead) is a vehicle for the perpetuation of the disease. [But it is not contended by the writer quoted that this "perpetuation" is constant. On the contrary, epidemics of yellow fever, as of cholera, and other diseases belonging to the same category recur at long intervals; whereas the soil of cemetries is constant, and its disturbance for fresh interments frequent; yet confessedly, the earth thus disturbed is not perpetually a vehicle as here described.]

The learned doctor then goes on to say that the discovery of a preventive means is not impossible. [But surely it needed no "experiments" to arrive at this conclusion: a conclusion which has been over and over expressed by hygienists and sanitarians, but which still remains to be realised] and that some encouraging results in this direction have already been obtained by him, he having succeeded in attenuating *microbes* of yellow fever [Yellow fever has not been definitely connected with microbes, any more than the diseases already noticed. In fact the germ theory in relation to the causation of disease has already been refuted] by a method of his own, having nothing in common with those of Toussaint and Pasteur. His method is founded on a principle he calls *anti-microbism*, that is, of the culture of organisms antagonistic to the microbes of yellow fever, and injuring and impeding the development of the *cryptococcus xanthogenicus* [That is to say, a microbe which has nothing to do with causation is to be destroyed by another microbe which also has nothing to do with it. Generally, in logic, two negatives make a positive; here the total result is a *third* negative, *i.e.* :—no fever.]

Needless to say, the experiments above referred to were performed on animals; their conclusions applied to man, *not* "experimented" on.

In accordance with the theory of microbes thus "confirmed" by experiments—by means of injecting phenic (carbolic) acid into the veins, and otherwise, of patients affected with yellow fever, some remarkable instances of recovery were recorded by M. de Lacaille, of Rio, the "cure," so rapid, that, notwithstanding his long experience, he asked himself [A very natural question under the circumstances] if they really could have been cases of yellow fever. (*British Medical Journal*, Oct. 8, 1881. *Medical Press and Circular*, Oct. 12, 1881).

In September, 1881, it was reported in the public journals that M. Pasteur proceeded to Pauillac, there, in a Lazaretto, to search in the dejections of persons labouring under yellow fever "brought from Senegal" for the microbe, that "may perhaps" be the cause of this affection, in the *hope* of being able, by his culture of it, to find the "vaccinator" for the black vomit. Information with regard to the success or insuccess of that "search" was not available at the moment when these notes are being prepared. But in other instances of inoculation given, success has been proclaimed even before it was attained.

"A Commission was subsequently appointed to examine the discovery by M. Friere. It has replied to him by a formal contradiction, and, moreover, declined to recognise that his vaccinations are harmless. M. Rochard, who presented M. Friere's supposed discoveries to the Academie des Sciences, confesses that he is not convinced. He adds: "It is the tendency of those theories of microbes, violently to invade the domain of therapeutics. It is a temerity to apply to man the practices of a laboratory." (*The Zoophilist*, June 2, 1884, p. 22).

And so another "epitaph" of another "discovery."

INOCULATION WITH DIPHTHERIA.

Under the direction of the American National Board of Health Drs. Wood and Formand have investigated the nature of the poison which is active in diphtheritic epidemics. Their inquiries are *full of promise* that *before long* complete protection will be found against diphtheria. Shreds of *diphtheritic* membrane from human patients inoculated into rabbits produced *tubercular disease*; also that the false membrane, supposed to be characteristic of diphtheria, appears as a result of severe inflammation of the trachea, however produced. They showed that the disease can be communicated artificially from animal to animal. M. Talaman that it can be conveyed from domestic animals back to man. (*British Medical Journal*, October 8, 1881; *Medical Press and Circular*, October 12, 1881).

At the Medical Congress, 1881, a discussion took place on the nature and mode of propagation of the contagion of diphtheria. Dr. Jacobi said that the nature of the contagion was *probably* chemical, the presence of *bacteria* in diphtheria did not favour its parasitic character; the entrance of diphtheritic poison was not the same in all cases. Dr. Thorne contended that diphtheria was not a distinct entity, but a form of nonspecific angina. Dr. Ashby remarked that, in relation to the geographical distribution of diphtheria the disease was rarer in Manchester than in Liverpool. Dr. West observed that the presence of bacteria or other organisms in the (affected) membrane did not prove that they were the cause of the disease. (*British Medical Journal*, September 24, 1881.)

The "results" of experiments related above arrange themselves after this manner: they are *full of promise* [As many others have been, that *promise* unfulfilled]. Dipththeritic membrane inserted into an animal gives rise to quite another disease than diphtheria. But membrane-like that of diphtheria occurring in non-specific disease in man may induce diphtheria in animals. Whether the "poison" of the (specific) disease is in its nature chemical or bacterial is still in question. It is even denied that the disease is itself specific or that it is a distinct entity. That is to say, "experiments" have rendered previously doubtful points still more doubtful, but have thrown light and definite information upon none.

PLEURO-PNEUMONIA.

At a meeting of the Paris Academie de Medicine, September 8, 1881, M. Bouilly read a report on the inoculation of contagious pleuro-pneumonia. According to him, inoculation of the virus was a sure guarantee against that disease. He also expressed the hope that one day he would be as successful in attenuating pleuro-pneumonia as M. Pasteur had been in the cholera of fowls.

M. Leblanc opposed the statement of M. Bouilly. He showed that in pleuro-pneumonia inoculation was of no benefit whatever, and that "its consideration was only loss of time." (*Medical Press and Circular*, October 12, 1881).

Here, then, as in every other instance in which reports by "experimenters" have been subjected to the process of comparative analysis, the conclusions arrived at by means of the experiments detailed are mutually contradictory; the absolute "result" negative—in other words, nothing whatever.

RABIES AND HYDROPHOBIA.

1. THE NON-MEDICAL PRESS ON THE SUBJECT.

The question of "inoculation" with the "virus" from an animal affected with rabies as a prophylatic against, and a remedy in actual cases of hydrophobia has recently been revived in the columns of the *Times*, and other journals. The circumstance to which this revival is more immediately attributable, is the fact that M. Pasteur lately read to the Academy of Sciences in Paris an account of certain "experiments" on individuals who had been bitten by rabid dogs. From those experiments that *savant* drew the conclusions.—(1) That by means of "virus" prepared by means of a particular process to which living animals were to be subjected, protective inoculation against rabies in dogs could be performed on those animals. (2) That by means of similar inoculation, a person actually suffering from hydrophobia may be "cured" of that disease. With a view to indicate in as clear a manner as possible the arguments in support of these two positions adduced by M. Pasteur as expressed in the *Times*, and to present other arguments having an opposite tendency, I propose in the following summary, in the first place to quote the more prominent paragraphs from communications on the subject published in that journal; in the second place to interpolate in reference to each extract of that kind, a summary of arguments given more in detail in a subsequent part of my remarks; and in the third place to draw from the whole such conclusions as appear to be justified by the premisses thus presented. According, therefore, to this plan the following are extracts from the *Times* of October 28, 1885, and the other journals named, together with the brief comments on them respectively:—

"A boy, 12 years of age, named Meister, had been bitten 14 times. The autopsy of the dog which had bitten him left no doubt that it had suffered from hydrophobia. The treatment (namely by "inoculation" of virus from the spinal cord of a rabbit) thoroughly succeeded, and Meister is now in perfect health. When the treatment began he had been bitten 60 hours. [Veterinary authorities assert that in certain cases the presence of rabies in a dog can only be correctly diagnosed by a veterinary surgeon. A large proportion of persons bitten by mad dogs do not suffer from hydrophobia. The period of incubation is much longer than 60 hours. Results similar to those of Pasteur were in 1879 said to be obtained from inoculation of sheep with saliva of rabid dogs.] Mad dogs do not worry; they snap and pass on. Various prophylactics, more or less successful, have from time to time been described."

"A shepherd boy of 15, named Judith, bitten a fortnight ago has been a week under treatment, and M. Pasteur is confident of curing him." [A sufficient time has not elapsed to justify the belief expressed. It cannot be asserted of any person bitten that hydrophobia will certainly occur.]

"The gallery applauded enthusiastically (the reading of the paper by M. Pasteur), but the Academicians could not be expected to do the same." [Why not expected, if the conclusions presented were such as commended themselves to them as being in accordance with their experience, and with analogy? It is not asserted that the "gallery" consisted of persons equally capable as the "Academicians" of estimating the bearings of the subject discussed.]

"If by a general and compulsory inoculation *for several* generations, dogs were made incapable of hydrophobia, the malady would have disappeared." [The period during which protective vaccination affords protection is limited, and certain direct evil results arise from such vaccination. Hence it follows that this operation would have to be repeatedly performed on each animal, and its risks of the disease resulting therefrom increased proportionally in each successive operation. By analogy the conclusion is that the measure above indicated would thus become impracticable. Nor does it present any compensating advantage over the simple measures of a preventive nature, which admit, of being employed in the absence of "experiments" of any kind. In other diseases "vaccination" protects the individual to some extent, but not the masses. Rabies may arise spontaneously, thus *the natural* disease may similarly defy the "artificial" protection.]

"M. Pasteur remarked that his theory will require study by the medical profession, in order to be made practical, but he emphatically stated that a cure for hydrophobia had been found." [Caution is necessary to guard against the desire which is natural to seek for "specific" remedies against diseases. Inoculations for cholera have failed in Spain and elsewhere. "Experiments" having reference to the same subject have had different results in the hands of different experimenters. The potency of the virus differs in respect to different animals, and in different outbreaks of rabies. M. Pasteur here refers to the practical experience and study of the medical profession. The medical journals inculcate caution in regard to the acceptance of M. Pasteur's conclusions. In the comments regarding these cases, the circumstance transpires that whereas non-medical journals for the most part upbraid that profession for its alleged partiality to "experiments," fault is now found with it because its representatives hesitate to accept conclusions laid before it by a professed "experimenter." The theory of combating the action of one animal-poison by inoculation of another animal-poison is of a date antecedent to that of M. Pasteur's "experiments."]

"What has to be done is to cultivate a hydrophobic virus, which has prophylactic qualities, and this virus is formed by means of the (spinal) marrow of rabbits which have been made hydrophobic." [Conclusions differ in regards to the presence, or otherwise, of the "poison" of rabies in particular tissues and secretions, and in the nervous system. But opinion is all but unanimous that in the bite of a rabid animal the conveying medium of the virus is the saliva and mucous secretion of the mouth. The question remains--Are rabbits subject to true rabies? So far, opinions indicate a reply in the negative.]

"This is a scientific triumph worth all the conquests in the world." [But is it a triumph? Medical opinion adduced, and analogy derived from actual occurrences strictly point to the necessity of caution and delay before accepting it as a "triumph" of the nature indicated.]

M. Jules Guérin rose to call in question the conclusions of the great man of science." [He is among the oldest and most experienced surgeons in Paris; his reputation for calm judgment acknowledged by all who know him. Two of the leading medical journals of London also counsel the exercise of caution against the conclusions presented by M. Pasteur. It will be noticed by a succeeding paragraph that M. Pasteur himself defers the present subject to the medical profession, of which M. Jules Guérin is a distinguished member; also that so far, medical opinion is against the conclusions of M. Pasteur.]

"Not, of course, that all who have been bitten by mad dogs have died; on the contrary, at least half, and probably two-thirds of such cases have escaped, even where no treatment has been resorted to, and the proportion in other cases has been much larger." [This admission neutralises much of the (supposed) advantages claimed for the "inoculation" of hydrophobic virus as a prophylactic against hydrophobia. That the disease is more prevalent on the Continent than it is in England has an importance with reference to the epidemiology of rabies. The fact that only a portion of those bitten become so affected has been made use of by proposers of various so-called "specifics" against the disease. In 1827 few of the persons bitten by rabid foxes in Switzerland became affected, while many animals so bitten suffered from rabies.]

"The usual modes of prevention, and of treatment, are to bind a light ligature above the wound, if in the arm or leg; to suck the scar, drawing out as much blood as possible—if the mouth is well rinsed with vinegar and water; the other is cauterisation. But when the disease has declared itself, no cure till now has been known." [If by such means the occurrence of hydrophobia can be averted, in some cases at least--and there is every ground for believing that it may be so, then the argument adduced for "vaccination"—that is, the introduction of diseased matter into the system, is by so much diminished in force.]

"By the invention of a method analagous to (Jenner's) vaccination, M. Pasteur has already saved hundreds of thousands of sheep (from charbon)in France." [In other hands than those of M. Pasteur, inoculation for charbon has failed, and has been declared to be productive of diseases other than charbon in the animals inoculated. In Hungary the practice was officially prohibited; in England declared to be useless and dangerous.]

"On the 6th July two patients arrived from Alsace; viz., M. Wohl, and the boy Meister. M. Wohl was found to be in no danger of hydrophobia." [There are persons who seem to be insusceptible to the "poison" of rabies, as Wohl appears to have been. As already observed, only a proportion of persons bitten by rabid dogs are attacked by hydrophobia. Besides this it does not appear that in the case of M. Wohl the wound penetrated the skin, so that reference to it does not affect either way the question now under review.]

"It is, then, probable that we are in possession of a cure for a frightful disease, which till now defied all the attacks of science. If, as we have every ground for hoping, M. Pasteur is shown to be right, the importance of his discovery, not only to man, but to the animals themselves cannot be over-rated." [The probability here expressed, derives no support from the opinions and statements of fact epitomised in the preceding and following paragraphs. History records definite occurrences and prevalence of rabies and hydrophobia in different countries; thus indicating in regard to that disease obedience to laws of epidemics, such as are inferred from phenomena, presented by outbreaks of that nature. In certain other diseases, notably measles and plague, inoculation has been a direct means of extending their attacks. There is no true analogy between the "vaccination" of Pasteur, and that of Jenner; the "virus" of the former is in no sense a "vaccine." The question is asked, will the dog into which the virus of rabies has been inoculated be a safe companion for man, and for children? And there are other circumstances which indicate that the proposal of M. Pasteur is unutilisable. That this is the case is apparent "to any one accustomed to weigh evidence in a court of law." But hydrophobia is only one out of several zymotic and enthetic diseases to which man and animals are liable. Are we to have every animal and every person vaccinated several times for each disease of this nature, of which there are some twenty in number? The thing is of course impossible and impracticable. Some very extreme points in this direction are quoted from writings on this subject.]

"The first result of M. Pasteur's discovery, assuming it to be established beyond question, will be to cause dogs to be very generally inoculated, and thus rendered proof against the disease." [This assumption is denied to have any real foundation by the statements of authorities already quoted. The existence of rabies in dogs has been "suppressed" by means of measures, which required no "experiments" to be performed on those animals. Instructions are indicated by a recognised veterinary authority, whereby similar results may further be looked for in the same direction. Nor is it in more than a per-centage of persons bitten that hydrophobia occurs.]

"There would be no need to dwell upon the value of a discovery, which removes this scourge (hydrophobia) from the human race—were it not for the strange perversity of those who will only see in the whole story fresh ground for attacking physiological experiment." [So far the value alluded to remains to be made out; indeed the balance of actual evidence, and of analogy is adverse to it. The circumstance is important and suggestive that the "perversity" noticed by the correspondent of the *Times* rests, with medical men, and others, who are well qualified by their training and by their practical experience, to form sound opinions on the subject. According to the evidence regarding this "discovery" as it at present stands, the inoculation of morbid matter, which in an animal would give rise to rabies, would in a human being prevent or "cure" the corresponding disease. But the specific character of conditions in the rabbit which follow inoculation of "virus" from rabies is not made out, except that they simulate those of septicemia.]

With every deference to M. Pasteur, the idea of inoculating against hydrophobia dates from a time antecedent to his "experiments." That idea resembles, if it be not identical, with that expressed by Hahnemann in his well-known aphorism *similia similibus curantur*. There exists generally a popular belief that the gall of a poisonous animal is a certain remedy against its bite when applied to the part bitten. In 1858 Dr. Aitkin wrote to this effect;—"The property which some animal poisons have of controlling and of interrupting the actions of other morbid poisons on the constitution has caused this class of agents to be tried in the cure of this disease. The rapid and powerfully acting poison of the viper, led to the hope that the bite of that reptile might prove an antidote to the hydrophobic virus; but the experiment tried in France, Germany and Italy upon animals has been entirely unsuccessful. [Thus somewhat modifying the treatment adopted by the Hottentots in cases of snake bite after the symptoms due to the bite have appeared—namely, the application of the virus of "some other reptile," with the result that the bitten person escapes without ill effect? (See *Pall Mall Gazette*, Nov. 16th, 1885.)] M. Grindard conceived that the vaccine virus might influence hydrophobia; he vaccinated a hydrophobic child in three places and afterwards injected five charges of vaccine lymph into the veins, but the child died without any marked remission, and in the usual time." (Science and Prat. of Med.; Art. Hydrophobia). And a still earlier date is assigned to the same idea. Attention is called in the German journals to the fact that in 1849, the question of the usefulness of inoculation with rabies-poison as an antidote and preventive against the effects of bites by mad dogs was discussed in Jahr's "Klinischen Anweisengen," in the articles on "Poisoning," and "Dog Rabies." Constantine Hering, a physician, then living in Philadelphia, is there mentioned as having actually made use of this remedy. (*St. James' Gazette*, Nov. 21, 1885). Yet another case is on record: The *Evening Standard* of May 29, 1875, contains an account of an experiment performed at the Leblanc Hospital, Paris. M. Lebeau, who professed to have discovered a cure for canine madness, inoculated sixteen dogs with rabic virus. [A question transpires in a subsequent part of this paper as to whether the presence of a specific "rabic virus" is necessary for the causation of rabies in a person bitten. Another question, whether such "specific virus" exists in the sense in which that expression is applied, for example, to the venom of a poisonous snake, or in certain diseases, syphilis, small pox, &c.] The result of the experiment, however, does not appear; but in reference thereto the remark occurs that as these inoculations "were performed a little over ten years ago, had the result been favourable we should certainly have heard more of them." (*Zoophilist*, January 1886, p. 162.) With regard to these or similar inoculations with saliva of rabid dogs performed in 1875, in Paris, M. Bourrel pointed out that the inocubatory stage of the disease was so uncertain that no dependence could be put on the results claimed for them. (*Le Rappel*, November 3, 1885).

The pecularity of M. Pasteur's "cure" for hydrophobia is that he does not wait till the disease has appeared before commencing his treatment, and he pronounces his patients "cured" of the disease before they have manifested a single symptom of it. [It is accordingly inappropriate to make use of the word "cure." Nor is it possible to say that in the cases attended to, hydrophobia would have occurred had the patients been let alone.] In animals he applied the vaccine to their brains by opening the skull; he cannot do this in man. How then is he to make a proper comparison? (*Zoophilist*, December 1, 1885).

But M. Pasteur says that inoculation must be effected within a month of the bite; that although he does not refuse to inoculate persons who have been bitten at a more remote interval, the fact of their falling victims to hydrophobia would not invalidate his *theory*. Inoculation can no more be a remedy against hydrophobia when it has declared itself, than vaccination is against small pox, when once a patient is down with it. (*World*, Jan. 15, 1886). [From which it follows that the method advocated by that distinguished savant "cures" a malady which is not manifest, but fails to "cure" the specific disease when actually present].

2. SPECIAL CASES OF INOCULATION.

With immediate reference to the two cases of inoculation which are the subject of the present analysis, a daily paper wrote to this effect, namely :—"If M. Pasteur has discovered a cure for hydrophobia, he is entitled to the gratitude of the human race. But the natural desire which prompts mankind to accept without question, what are pronounced to be specific remedies for what are regarded as incurable maladies, must inculcate a certain amount of caution in accepting conclusions which that distinguished physiologist regards as absolutely convincing. Only a few months have elapsed since a Spanish physician satisfied himself that he could confer upon those who submitted to the operation (of inoculation) absolute immunity from cholera. But the terrible mortality which attended the epidemic, more especially in the province where inoculation was most practised, demonstrated only too conclusively that his inference was erroneous." Adverting more particularly to the two cases above related it is remarked that :—" Logically speaking, the only result of these experiments is that two human beings who have been inoculated with the virus of hydrophobia within the past three months are still alive, though rabbits submitted to the same treatment have perished. [Evidence hereafter adduced throws doubt upon, if it does not completely disprove the belief now expressed that the matter inoculated contained any specific "virus" at all]. To this extent, but no farther, this novel mode of treatment may be regarded as successful. But, notwithstanding our desire to accept the conclusions of the great French physiologist, we are bound to say that the length of time which has elapsed is not sufficient to justify the assumption that either of the two patients is out of danger. The virus (?) of hydrophobia lies dormant in the human system for months, and may do so even for two years, and this being so, we confess we are just as unable to admit the conclusiveness of the cures that have been effected as if the patients had simply had their wounds cauterised and were still living. We sincerely trust that it will be ultimately shown that M. Pasteur has achieved as signal a victory over hydrophobia as he has done over the disease which has decimated so many sheepfolds. [It is elsewhere shown that exception is taken to this statement on the grounds that it is unsupported by events]; but we are compelled in candour to say that a sufficient period of time has not elapsed to entitle us to congratulate him on the assured efficacy of his supposed discovery." (*Morning Post,* October 29, 1885).

On the occasion of M. Pasteur's address at the Academy, one gentleman thought that perhaps M. Pasteur had too hastily concluded that the dog which bit Meister was mad because its stomach contained pieces of wood, straw, and grain. M. Pasteur considered it was mad because it had shown all the outward signs of rabies, such as shunning its masters, and attempting to bite everybody near it. In animals killed while mad, the rabid virus is found only in the spinal marrow, while in dogs that have succumbed to rabies the virus is found throughout the whole nervous system. [The method by which the circumstance here stated has demonstrated is left unrecorded; the circumstance itself is therefore held to be "conjectural."] The savant said that he always took the virus from the brain [why from the brain, when, accepting the theory that specific *virus* exists, and is necessary for producing the disease, a sufficient quantity was surely enough obtainable from the saliva?] of animals, and even from the saliva or foam issuing from their mouths." *Telegraph*, October 30, 1885).

Half the so called symptoms of rabies in a dog are no symptoms at all. He has been slain for mere morbid excitement, when there was nothing more the matter with him than a tender melancholy engendered by the loss of his master, and the want of a home. His frothing and foaming at the mouth has been treated as a capital offence, and fits have often led to his destruction on suspicion of this terrible disease. (*Daily News*, Jan. 15, 1886).

According to "*The Guide*," November, 1885:—In the case of the boy Meister, the fourteen bites sustained by him had been cauterised before he came under Pasteur's care. But nothing is more uncertain than the result of a bite from a dog supposed to be mad. What is this pretended specific? It is this—if you are bitten by a dog supposed to be suffering from rabies, you must allow yourself to be innoculated with the marrow of a rabbit which has undoubtedly [Query, Has it?] perished of that disease. If you really are infected with the virus of latent hydrophobia, then (it is alleged), the artifically-induced disease will neutralise the poison which has been introduced by the dog's bite; but if it should happen that you are not destined to have hydrophobia, but only feared you were, what then? On this point there is little room for speculation. The marrow that Pasteur employed in the case of the boy Meister was tried on healthy rabbits, and these died of hydrophobia. [Note on this point what occurs hereafter with reference to experiments performed on rabbits, and their non-susceptibility to rabies.]

The *Medical Times*, October 31, 1885, says:—"With respect to the first patient it must be observed (1) That sixty [or rather ninety to ninety-five] per cent. of people bitten by mad dogs do not contract hydrophobia; (2) That the incubation of the disease is sometimes extremely long, extending to two years. The experiment, therefore, is not absolutely conclusive. However inclined one may be to jubilation, it is not wise to indulge in extravagant laudation yet. The extravagant hopes raised by reports of cholera vaccination were not fulfilled. The infecting medium of rabies is probably the saliva. In 1879 Galther injected the saliva of a mad dog into the veins of eighty sheep ; a form of rabies resulted, which, though not fatal, was [said to be] protective against further infection. There is undoubtedly some difference in the action of the salivary secretion. The blood of men suffering from rabies has been injected into rabbits by Raynaud with negative results, the saliva of the same patient being, however, fully virulent. We shall temper our admiration of the brilliancy of the researches with the scientific soberness which regards no research or result as perfect which is not confirmed by experience and time." According to the same journal, November 14, 1885 :—" As practical men, we would ask to be allowed to pause awhile before submitting to an experiment which, if it did nothing else, would certainly increase the cost of the poor man's rabbit pie. [With reference to the large number of rabbits " made use of " in the (supposed) cultivation of "hydrophobic" virus.] We should like first to give a fair trial (before introducing Pasteur's system into England), to those old time-honoured expedients by which determined Governments and sensible subjects have before now succeeded in staying the plague of rabies." [And having been thus successful, they leave no necessity for the introduction of other means, the very essence of which is to " prevent " the occurrence of disease by means of propagation of the same disease.]

The *Lancet*, October 31, 1885, writes :—" Of Pasteur's announcement we must speak with reservation until the complete documents are before us. We cannot but think, however, that M. Pasteur's inferences are sanguine and premature. Speaking of the boy Meister, we cannot assert of any individual, however frequently bitten, that hydrophobia will assuredly develop. A large proportion of individuals bitten by dogs escape that terrible disease." In the same journal Mr. Walley writes from the Royal Veterinary College, Edinburgh, to this effect :—" Dogs in the advanced stages of distemper frequently show symptoms almost identical with those of the acute form of rabies, and on several occasions he has seen dogs that have been supposed to be rabid in which the conditions presented were due entirely to bad usage. [A circumstance commended to the notice of persons who are disposed to maltreat dogs.] The conditions mentioned can only be differentiated by experienced (veterinary) surgeons." He then mentions that a few years ago in that city the magistrates grappled with that evil; rabies was got rid of, and with it many useless and dangerous dogs. The *Lancet*, November 14, 1885, observes:—"It may be true that the cases on which M. Pasteur bases his claim to have proved the efficacy of inoculation as a cure for rabies are somewhat too recent and too uncertain in their history to satisfy all critics. It may also be true that fear has some influence in depressing the nervous system." Fear, or nervous apprehension, can induce a fatal disease having nearly, if not all, the characters of hydrophobia. [A very important circumstance, and one to be borne in mind. Cases in illustration are given farther on]. But hydrophobia is not always brought on by the mental anxiety that a dog bite not unfrequently occasions. Hydrophobia is a nervous disease; it has a material cause—a poison—which is most likely a "germ," or micro-organism. [Altogether hypothetical, no such "germ" or microbe having been ever detected.] Birds, some other animals, and some persons, appear to have an immunity against hydrophobia. Whether it is those persons who are not given to fear or nervous apprehension who always escape hydrophobia, even though bitten by a rabid dog, we are not in a position to state." (*id.*, 21st November, 1885).

According to the *British Medical Journal*:—M. Pasteur's method of preventing the disease (hydrophobia) by inoculation is at present *sub judice*, and the decision cannot, owing to the uncertain duration of the period of incubation of rabies, be given for many years. Meanwhile the experience of Holland (under regulations put in force in 1875) shows that we have, in stringent police regulations, a simple and effectual method of preventing this disease, so terrible to man and his faithful friend. (Dec. 5, 1884, p. 1077.) [In other words, Pasteur's inoculation is put on the shelf. Nor is it required in practice even were it proved to be advisable and practicable—which it is not—as the end arrived at can be, and is, attained by more simple and ordinary measures.]

Mr. Bryan wrote:—"M. Pasteur's much-vaunted cure for, or preventive of anthrax was long since proved to be illusory. A commission, appointed by the Hungarian Government reports that "the fatal issue of other severe but latent diseases (in sheep) is accelerated by a protective innoculation." Dr. Klein (in England, see *ante*, p. 48) found that the protective vaccine proved fatal to several of the sheep which he inoculated therewith. His verdict was that this country is comparatively free from anthrax, and therefore the introduction and use of the so-called *vaccin-charbonneuse* seems most dangerous, and capable of producing incalculable mischief." (*Times*, November 3, 1885).

M. Cheveé-Leroy enumerates previous experiments performed by M. Pasteur, and the unsatisfactory nature of the results which attended them. Among these experiments and results are the following, namely:—on the silk-worm, *failure*; on vines, *ruin* to a large number of viticulturists who practiced them on a large scale; Beer, *impracticable* in manufacture on the large scale; Wine, that which is prepared from grapes grown on healthy and appropriate soil remains good, while that produced from grapes grown under opposite circumstances, deteriorates, both under similar exposure; and besides this, the method of heating, as proposed by M. Pasteur, has proved in practice, so far from having the advantages claimed by that savant it has resulted in failure and disappointment. Charbon, completely *inopportune*; Cholera, inoculations *dangerous*, and capable of *propagating the disease.* (*Etude Sur le Vins and Les Maladies de la Vigne*).

Dr. Dudgeon expressed himself to this effect:—"Medical men would eagerly welcome the discovery of a remedy or preventive of hydrophobia, but the facts adduced by M. Pasteur hardly prove that he has discovered either. In the first case (Meister), the boy had been bitten only sixty hours before being seen by Pasteur; in the second (Judith) the bite was nearly a fortnight old. Our best authorities agree that only a portion of those bitten get hydrophobia, that is, 5 per cent. according to Hunter, 50 per cent. according to Tardieu and others. The proportion is larger in France than in Britain. [A circumstance quite in accord with the relative nervous impressionability of the inhabitants of these two countries.] The period of incubation, *i.e.*, from the bite till the disease shows itself, varies from twelve days and upwards to one year (and even longer than that), the average being from 44 to 75 days. Now, as neither Meister nor Judith, as far as appears, showed unmistakable symptoms of hydrophobia, it is possible that the dogs they were bitten by were not really rabid, or that if rabid, these boys might have been insusceptible, as Wohl, who was bitten by the same dog as Meister, undoubtedly was. Again, there is no record of a genuine case of hydrophobia having been developed sixty hours after the bite, as in Meister's case, and even a fortnight only of incubation, as in Judith's case, is extremely rare; so that we must wait for further evidence before deciding that M. Pasteur has discovered a remedy for hydrophobia. That his graduated vaccinations, as he calls them, will render the subject of them proof against the strongest virus is interesting, but it does not prove that these vaccinations will cure or prevent hydrophobia from a bite." (*Times*, November 3, 1885).

Ouida writes : No dispassionate person can fail to be struck with the absolute want of proof that the dogs who are said to have bitten the persons who go to M. Pasteur, were really rabid. Even the first case of the boy who was so eulogised at the French Academy, told the most preposterous tales A dog of great stature, raving mad, descended on this shepherd boy and some small children ; yet this gigantic mad dog was so peaceable that he allowed the boy to tie up his jaws with a piece of string, and beat him to death with a wooden shoe. Who, but scientists in blind search for a "case" would have believed such a Munchausen-like tale. There is no proof that the dog was mad at all. (*Morning Post*, January 12, 1886).

Hitherto the only chance, when a human being has been bitten by a mad dog, has been to remove or isolate, and destroy the virus by suction, or ligature, or cautery. And it has been doubted whether these methods are really successful, even when the disease does not manifest itself afterwards, for there is reasonable doubt as to the nature of the disease in the biter. All vicious dogs are not "mad," and all mad dogs are not truly rabid. Moreover, when bitten by a rabid dog, the sufferer may yet escape. When once the virus is introduced into the living tissues, there is every reason to believe that the terrible effects are constant and uniform ; but so far as we know, there is no power in the most healthy organism by which the subtle venom, once absorbed, can be thrown out. [But as elsewhere shown there is every reason to believe that certain persons, or individuals under certain (undefined) conditions do actually resist the "poison" of rabies.] The length of incubation is far longer than it is in the case of small pox, of cow pox, of syphilis, and other contagious diseases. There is an interval of days or months in which the latent plague, established in the patient's body, but not yet ripe for mischief, may be attacked. The only promising path of investigation is to seek for some method of forestalling the action of the virus by rendering the organism unfit for its action. By this method Jenner robbed small pox of most of its terror, and almost all its danger. Pasteur's method of dealing with hydrophobia is avowedly based on that method of vaccination. Nor is it exactly analogous to Jenner's vaccination. [A very important admission this.] For in vaccination an allied disease, (or possibly small-pox itself greatly modified), is inoculated. [Quite true. It is inoculated however—or rather, intended to be so—in a person into whose system the specific poison of small-pox has not already been received—a very important distinction.] But in the case of hydrophobia, as in that of "chicken cholera," and anthrax, the same disease is transmitted through a succession of "bearers," until it is so modified that it may be safely inoculated. Fifty dogs were inoculated with virus obtained from the spinal cord of rabbits which had themselves been affected with rabies by inoculation. [That these animals are susceptible of being so affected is questioned elsewhere.] None of the vaccinated dogs showed signs of the dreaded disease. [The rabbits from which they were inoculated being insusceptible of rabies, it naturally follows that these dogs were not inoculated with the *virus* of hydrophobia at all, even if we accept the theory of that *virus* being an existing entity. In the course of these remarks various arguments transpire against the theory that a definite specific poison peculiar to the disease really does exist.] Then came cases of human beings bitten by mad dogs, namely, Wohl and Meister. M. Pasteur believes —[His belief is distinctly opposed by the results of previous inoculations performed with *virus* obtained from animals affected with specific diseases]—that Meister is safe from hydrophobia for the rest of his life. "If" [note the *if*] similar cases should be followed by similar results, medical science has for the first time a method of combating a frightful and incurable disease. But beyond this, by inoculating dogs, as infants should be vaccinated, they will be rendered insusceptible to rabies. Any mad dog will be destroyed, and the dog he has bitten will escape. Thus the disease may, it is hoped, be extirpated altogether. [Analogy is opposed to such a hope; no other epidemic or epizootic disease having ever been "extirpated." They have severally run their course, and one form has in some instances given place to another form or type, but that is all.] These, however, are but hopes; the whole question is *sub judice*." (*Nature*, Nov. 5th, 1885).

"We feel rather sceptical about the value of M. Pasteur's alleged discovery. Can he certainly prove that his patients would have gone mad if nothing had been done for them; or that sufficient time has not elapsed to prove their safety. [It is obviously impossible to prove either of these positions]. Mr. Scoborio, the manager of the Dogs' Home, was bitten hundreds of times in the course of his duties, without ill effects. Nor can we credit the story of M. Pasteur's patient being bitten fourteen times by a mad dog. Mad dogs don't worry their victims, they snap suddenly and then go on their weary way." (*Graphic*, Nov. 7, 1885).

It is rather singular that French physicians, who deny the inoculation experiments for cholera of Dr. Ferran should be so ready to jump to a conclusion in favour of Pasteur's similar method in connection with rabies. But if the truth is told, there is at present no proof that hydrophobia is curable. The evidence is of a negative character that one of Pasteur's patients would have developed the disease if his inoculation had not prevented it, and there is reason to fear that his admirers have been too hasty and too positive. Those who remember how many alleged cures for snake-bite have been discovered in India will await further development before implicitly believing in the "cure for hydrophobia." (*Echo*, Nov. 10, 1885).

Dr. Jules Guérin, a great scientific authority, gives a non-proven verdict.—*Daily News*, Nov. 10, 1885. The circumstance is noteworthy that, on the occasion of reading the paper by M. Pasteur, neither he nor M. Colin was permitted to address the Academy of Sciences; consequently, M. Guérin had to formulate his opinions on the subject to this effect:—He based his objections against the proposed method on Pasteur's own experiments and results. He argued that the rabbits "used" to supply the "vaccine" were not really rabid, but were simply suffering from the effects of an artificial malady, produced by M. Pasteur's method of operation, and of developing the morbid material introduced into their tissues; and he pointed out the circumstance that cauterisation having, in the first instance, been used in the case of Meister, any argument that would have been adduced in support of inoculation alone is thereby set aside. (*Le Rappel*, 3rd November, 1885.) Four years ago a similar malady, similarly produced was notified to the Academy by M. M. Raynaud and Lannelongue, and it was then agreed, M. Pasteur himself concurring that the relations of this artificial malady with true rabies were very problematical. Indeed, judging from other experiments duly reported to the Academy, rabbits appear not to be susceptible of true rabies at all. The symptoms set up in them by M. Pasteur's inoculations rather resemble septicemia, that is "blood-poisoning," than rabies. (*Spectator*, Nov. 21, 1885; the *Bulletin de l'Académie de Medicine*, containing M. Guerin's speech). There are many in Paris besides M. Guérin who do not in the least believe in the inoculations of M. Pasteur. (*Zoophilist*, August 1885, p. 72).

Some years ago M.M. Girard and Vatel inoculated with the saliva of a rabid sheep two other sheep, a young dog, and a horse, but none of these animals ever evinced any symptoms of the disease. Professor Dupuy failed, as well, to communicate the malady to ewes and sheep by rubbing their wounds with a sponge saturated with saliva of animals of the same species; he stated, however, that he produced rabies when he used the saliva of a rabid dog in the same way. (Mr. W. Douglas, *Chronicle*, Jan. 2, 1886).

M. Boulay wished to know whether young dogs, having been inoculated, would not, while under the influence of the virus, be dangerous to those with whom it might come in contact. This question had not presented itself to M. Pasteur. In order to supply the necessary quantity of virus, it will be essential to maintain "a manufactory of mad rabbits." [The remedy in this case would be worse than the disease. As it is, a larger number of dogs are believed to have already suffered from rabies artificially produced than would have been affected in many epidemics of that disease. It is important further to observe that no dog has been *cured* of rabies by this inoculation; no dog has been certainly proved to have been rendered insusceptible, notwithstanding the statement on this point by M. Boulay in 1884. Stress must also be put on the circumstance above stated that rabbits and certain other animals were found to be insusceptible of rabies; and also upon the question raised above as to whether there really exists a "specific" virus of the disease.]

Deputy Surgeon-General Downes writes to this effect:—"The alleged discoveries and methods of prevention of hydrophobia proposed by M. Pasteur consist in the introduction into the human system by inoculation of the virulent and deadly virus of the disease. This proposition is only a repetition of that brought forward many years ago with reference to inoculation for small pox; but the prejudice against it was so great that it soon fell into disuse, and was finally prohibited by state legislation, being succeeded by the more harmless operation of vaccination." (*Morning Post*, Nov. 5, 1885.) [So have been Pasteur's inoculation for anthrax, and Ferran's for cholera. Note also the essential difference elsewhere described as existing between the method of Pasteur and that of Jenner].

A lady, whose son was recently bitten by a dog proved to be mad wrote to tell M. Pasteur that his discoveries were worthless—an assertion she supports by the fact that both she and her son were bitten by rabid dogs, washed and cauterised their wounds, and have experienced no bad result from the accident." *Evening Standard*, 11th-12th November, 1885.) She challenges M. Pasteur's treatment with its necessarily complicated arrangements against her own more simple and effectual plan. (*Zoophilist*, Dec. 1885, p. 130).

But it is reported that :—A little girl who had been bitten by a mad dog, and who was afterwards subjected to M. Pasteur's "inoculation" died of hydrophobia. This child was not taken to M. Pasteur's laboratory until thirty-six days after she had been bitten. (*Echo*, Dec. 8, 1885.) The following details are given regarding that fatal case :—Louise Pelletier, ten years old ; bitten on October 3rd in two places, the wounds "cauterised" with carbolic acid within three hours of their infliction. From that date the child continued in perfect health for thirty-six days, at the end of which time M. Pasteur began his inoculations with rabid virus, and continued them during ten consecutive days. Fifteen days after the termination of this treatment the child was attacked with hydrophobia, and died three days afterwards, namely, on December 4. While she was suffering from the disease the inoculations were repeated, but no other treatment was applied. [And yet, the definite statement occurs that "inoculation can no more be a remedy against hydrophobia when it has declared itself, than vaccination can be against small pox, when once a patient is down with it." With what object then, was it adopted in this particular case to the exclusion of all other means ? Information on this point is absent.] Who then shall say whether she died of the dog's bite or of repeated inoculations with rabid matter—artificially performed. (See *Spectator*, Dec. 26, 1885).

On the 17th December, 1885, "it was alleged that whereas there have been two deaths from hydrophobia after inoculation, sufficient time has not elapsed to enable any appreciative result to be given. M. Pasteur avows that at least 12 months must elapse before he himself can be practically satisfied of the efficacy of his new system." According also to information received, five deaths had occurred among the patients vaccinated up to the end of January, 1886.

That inoculation was employed in at least one case in which the dog that bit the patient concerned was not truly rabid appears from the following narrative of the case :—" M. Ringeval, living at Rueil, was bitten by a dog. He was inoculated every day by M. Pasteur, who is said to have expressed a confident hope that he might yet be saved. The dog was shot; the veterinary surgeon who performed a post mortem examination declared that the animal was not in the least mad, but had been suffering from a gastric attack, and must have been irritable on that account. (*Daily News*, Jan. 14, 1886).

3. THE GENERAL SUBJECT OF RABIES, OR HYDROPHOBIA.

Referring to the general subject of rabies, or hydrophobia, it is considered desirable in this place to lay before the reader a summary of views expressed by authorities on that disease as it affects animals and human beings. By this method, it is hoped, a standard will be afforded wherewith to compare the various statements already alluded to, and facility given to arrive at conclusions with regard to the asserted nature of the " discovery " which forms the theme of these comments.

The most disquieting suggestion that has been made regarding hydrophobia is one originally advanced in 1847 by Dr. Wright (*British and Foreign Medical Review*, vol. xxiii., resuscitated by Dr. Muscroft, *Lancet*, vol. ii., of 1874, pp. 512-864), and by various American physicians, namely, that under certain circumstances, or at certain times, the saliva of a dog apparently healthy, and subsequently presenting no symptoms of disease may, when applied to a wound, [by means of a bite?] produce rabies (hydrophobia) in man. A case is stated in which hydrophobia occurred from the bite of a dog immediately after it had for a quarter of an hour been subjected to cruel treatment. (*Standard*, Dec. 26, 1885).

In certain diseases, also, the generation of specific poisons is recognised as being an attendant concomitant of the morbid conditions which constitute these diseases, respectively. This applies, for example, to cholera. Living tissues could secrete a poison when simply irritated by some germicides, such as iodine; and peritonitis thus induced has been communicated by inoculation to other animals. (*British Medical Journal*, April 28, 1883, p. 815-815.) It is a well known fact that the flesh of game animals that have been killed by the infliction of cruel suffering, has in some instances become poisonous. An instance of a roe deer which was killed after a most agonising struggle, is given in illustration by Baron Leibig. (*Letters on Chemistry*, *Appendix*). On eating its flesh, symptoms resembling those caused by the bites of rabid animals, manifested themselves in some of the persons who made use of it. (*Three Prize Essays on Vivisection*, p. 227.) [A sudden gust of passion may impart poisonous properties to the milk of a wet nurse.]

According to Professor Selmi and Dr. Gautier, the flesh of over driven cattle, of those that have undergone violent exertion, as well as those that have been tortured before death, on occasions become poisonous, by the self-development of *ptomaines*, the action of which in several respects resembles muscardine or fungus poison. Not only the saliva of enraged animals, but that of man, under similar circumstances, acquires poisonous properties. (*Journal of Science*, Dec. 1881, p. 736.) It is even stated that M. Pasteur has recently "cultivated" the poison of the human saliva to such a point as to develop the toxic symptoms of the serpent-poisons in small birds.

Dr. Shinkwin relates cases of hydrophobia as having occurred in persons bitten by dogs in perfect health; he observes that such cases *oblige* him to come to the conclusion that the bites of non-rabid dogs can in some rare cases, and under circumstances which tend to excite their anger, produce true hydrophobia in man. (*British Medical Journal*, August 19, 1882, p. 341.) Similarly, hydrophobia has followed upon bites by non-rabid cats.

It is even asserted that under the influence of "nervous irritability" induced by other causes than cruelty or unkindness, a dog may inflict a bite, the result of which may be hydrophobia. Such a case occurring in Paris was reported in the *Daily News*, of January, 1886.

In extreme northern latitudes the result of the prolonged night of many months, is frequent mania (rabies?) among the numerous dogs employed in harness. When a dog is excited by a strong passion, such as anger or fear, the alteration of the secretions may be fatal to the person bitten. In the case of the Duke of Richmond, in 1819, the pet *Vixen* that licked an accidental wound on his chin, did so in fondness, but had been worried just before by some bull pups. (*W. A. F.*, *Morning Post*, January 15, 1886.) [It is elsewhere stated that true rabies is infrequent in extreme northern regions.]

Dr. Wright established, as a result of fourteen experiments, that whatever may be the case as between dogs and man, the saliva of a healthy dog is capable (in some instances) of producing rabies when injected into the veins of another dog, and Dr. Muscroft details two cases which establish the proposition with which he starts. [On the other hand, many dogs remain unaffected by hydrophobic virus when introduced into their blood by inoculation. (*Zoophilist*, December 1885, p 145.]

The question has also arisen :—Is it absolutely necessary for the occurrence of hydrophobia in man that he should be bitten by a rabid dog or otherwise inoculated with "the specific poison" of rabies? In the *Indian Medical Gazette*, November 1885, there occurs a report of a case by Mr. Branfoot, which tends to support the belief that it is not; and the further belief that under exposure to certain meteorological conditions cerebro-spinal meningitis may be set up, the attending symptoms, and the manner of termination being undistinguishable from those which happen in hydrophobia traceable to the bite of a rabid dog. Analogy and experience teach us that symptoms and lesions, identical in their characters may be and are set up by exciting causes, differing in their nature from each other. (See *Medical Press*, Dec. 26, 1885).

A somewhat similar case to the above is recorded in the *Manchester Examiner*, of December 11, 1885. According to that journal M. Dujardin Beaumetz has found a case of *spontaneous hydrophobia* in a human being. He read before the Hygienic Society of Paris, a paper on the case. All the symptoms of the disease were present, and inoculations from him caused the malady in animals. Yet the man declared, when admitted to the Hotel Dieu, that he had not been bitten nor scratched by a dog or any other animal, nor in any sort of contact with one. No signs of a wound were discovered on his skin after death. (*Zoophilist*, January 1886).

A third case is thus recorded :—M. Reeve Raffin (Department of the Loire) exhibited the first symptoms of hydrophobia. During his journey to Paris to be treated by M. Pasteur he had an attack, and halted. The next morning he continued his journey, arrived in Paris, and was admitted at the Hotel Dieu. Immediately after his arrival he had another attack, and died six hours afterwards. The parents do not remember that the sufferer had been bitten by a dog. A bull dog of which M. Raffin was very fond had suddenly disappeared about five months previously. (*British Medical Journal*, Jan. 9, 1886, p. 80).

The following remarks are taken from the article "Hydrophobia," in "Reynold's System of Medicine":— "The disease rabies is far more common on the continent than in England. [A circumstance believed to be due partly to the temperament of the people, as much as to the more general ill treatment dogs receive in continental countries than in Britain, bad as that often is.] It is singular that even in relation to so active and certain a virus as that of rabies, we find instances of remarkable constitutional resistance in the dog to its effect. (Of such cases, some are quoted, p. 717.) There are no cadaveric lesions which can be said to characterise hydrophobia. (p. 723.) The large majority of all those who are bitten by mad dogs escape hydrophobia, even when no treatment is adopted. [It is therefore obvious that the subsequent "inoculation" of such persons from truly rabid animals would subject them to a source of danger over and above that which their own constitutional powers were in the first instance sufficient to protect them against.] The greater number of cases in man occur between the thirtieth and fortieth day after being bitten. At the end of the second month the large majority of the patients may be considered safe. It has long been known that the evil effects of the bite of the mad dog are probably often prevented by the adoption of an active local treatment of the bitten part—excision, cauterisation, the application of the various caustics, &c. Mr. Youatt placed the greatest confidence in cauterisation with nitrate of silver. From the fact that so few of those bitten by rabid animals actually contract hydrophobia, we should expect that a large number of specifics should have been proposed for its treatment. A credulous physician, who happened to have administered some remedy to a few persons bitten by a mad dog, finding that no evil consequences followed, and forgetful that had nothing been administered his patients would, in all probability, have enjoyed equal immunity, was only too ready to believe that he had at last discovered a specific for this terrible disease." (p. 724.) [How far the similar remarks apply to the subject of "inoculation" now before us individual readers will judge, each for himself.]

Mr. W. Douglas also dissents from the prevalent theory that rabies only follows inoculation by a bite, or otherwise. He recognises in all the symptoms of the disease only a form of *tetanus*. Quoting from Hooper's Medical Dictionary (p. 283) he observes that tetanic affections are occasioned either by exposure to cold, or some irritation of the nerves in consequence of injury. The wound caused by the bite of a dog would constitute such an injury, and would probably bring on tetanus. The chief distinction between the two diseases is that in tetanus the muscles of the lower jaw are affected; in hydrophobia, the muscles of the throat. The symptoms in hydrophobia, moreover, follow the course of the nerves rather than that of the absorbents; and the freedom of the lymphatic glands from the disease has been adduced as an argument that the disease does not depend upon the absorption of any "virus." Persons of a nervous predisposition are more susceptible to the disease than are those who are differently constituted; and in support of this statement Mr. Douglas alludes to a fatal case of hydrophobia in St. Mary's Hospital after a bite by a dog, which was not even alleged at the inquest to have been rabid. He further quotes from the *Italian Journal of Physic* for January and February, 1817, a case in which a young man, five days after having been bitten by a dog evinced symptoms of hydrophobia, of which he was nearly dying, when the dog that bit him was shown to him perfectly well; this tranquilised his mind so effectually that he recovered from that moment. John Hunter, in his lectures, is said to have described a similar case in which he believed the patient would have died if the dog which inflicted the wound had not been found and shown to him perfectly well. And a fatal case (of Albert Evans) is recorded, from the bite of a dog, the madness of which there appeared to be no proof. (*Chronicle*, January 2, 1886).

A very striking case in further confirmation of this view is thus recorded. (*Telegraph*, Jan. 23, 1886.) In October last some boys had driven a mad dog (it is not definitely asserted whether the animal was *rabid*) into the shop of John Stock, then living at Kingston, and while he was stooping to throw the animal something to eat it flew at him and bit him on the lip. He was very nervous about it, and in the night had a fit of fainting and shivering. A week ago he had another of the shivering and fainting fits, but otherwise he did not seem affected by the bite. The house surgeon, said there were no pronounced symptoms of hydrophobia except a twitching of the lower jaw, when the deceased was admitted. He was very depressed, and said he should soon die. As the day went on he got very excited, and then the spasms came on. Heavy doses of morphia and chloroform had scarcely any effect on him. He was quite conscious up to 3.30 a.m. on Wednesday. After this he became delirious and violent, and died about eight a.m. In reply to the coroner, the witness said there was no recorded cure when the symptoms had set in. Verdict, death from hydrophobia. According to the history of the case as here presented, it tallies in several important respects with those above recorded; nor is there any ground shown in the record as it stands that the disease which proved fatal was in its nature such as is usually recognised as "specific."

Baron Larrey observed the frequency of tetanus among the wounded French soldiers of Napoleon's expedition to Egypt, and in reference thereto he wrote that "this affection was more intense, and bore a greater resemblance to *hydrophobia*, than in the colder climate of Germany." Dr. Parry also published some cases of tetanus, and of *rabies contagiosa*, and the circumstance is at least noteworthy, that in the cases of tetanus which occurred among the wounded at the battle of Waterloo the disease was referred pathologically to the spinal marrow. [as rabies now is by M. Pasteur.] See *Ballingall's Milit. Surgery, Art.* "*Tetanus.*"

According to Mr. Fleming, "There appears to be a mass of evidence to prove that rabies may be generated spontaneously in the canine, feline vulpine, and some other species of the carnivora. [Special attention is solicited to this statement from so high an authority as Mr. Fleming, confirmed, as it is, by evidence adduced above.] Confirmation of this view comes from France. According to the *Journal de l'Aisne*, December, 1884, a veritable epidemic of rabies has broken out at Rouhaux. In some of the dogs attacked the disease is the result of a bite; but in the greater number it is *spontaneous*. The greater number of the dogs attacked by the disease are watch-dogs, kept constantly chained up. Still more recently Mr. C. Rotheram adds further confirmation of the same view. According to him—and there is no greater authority on canine diseases—"it is frequently assumed that rabies can only be communicated by the bite of a dog." But he has a different theory based upon *experience*, that rabies may be transmitted by an infected sire to his offspring. (*Standard*, Feb. 1, 1886).

In rabies the morbid changes, though in some cases numerous and marked are yet in others so trivial and variable that it may be said the disease has no fixed or pathognomonic lesions. By the diversity in the post-mortem appearances, it must always be a most difficult matter to decide whether an animal was really rabid. Only is there certainty when the history of the animal is known, and when the symptoms during life have been noted. (*Reynold's System of Medicine*, p. 252.) In practice, however, if indigestible or foreign substances are found in the stomach, and the lining membrane of that organ is highly congested and exhibits erosions, that animal was rabid. (p. 253.) The contagium of the disease exists in its most potent condition in the saliva or mucous of the mouth. Eckel and Delafosse have been successful in producing the disease by inoculating the blood of diseased goats, hogs, and dogs. The experiments of Breschet, Majendie, Dupuytren and Renault. were not attended with the same results, though they transfused the blood of rabid to healthy dogs. [Whence it follows that the results obtained from the two sets of experiments were contradictory of each other.] The evidence of the existence of the virus in the flesh of diseased animals is rather conflicting. (258.) The same amount of conflicting evidence has been offered with regard to milk. There is no sufficient reliable evidence that the contagium exists in the nerves, perspiration, or breath. [Compare this statement with what has been already quoted on the same subject, and note the divergence of opinion thus presented. It is moreover directly opposed to that by M. Pasteur, to the effect that hydrophobia could be communicated to a dog by inoculation with fragments of "marrow," or of nerve, taken from a mad dog. (*British Medical Journal*, March 1, 1884, p. 424.) On a subsequent occasion, also, Mr. Fleming observed that the virus was found in other fluids than the saliva; also in the brain and spinal cord. (*Nineteenth Century*, March, 1882).] The evidence is contradictory as to whether the deleterious principle is present as an infecting agent during the inocubatory period of the disease. The potency of this contagium would appear to vary not only in certain animals but in different attacks of rabies. [And in different kinds of carnivorous animals in those outbreaks]. It would also seem that this potency is impaired by passing through several bodies. The experiments of Rey would go to prove that repeated transmissions attenuate its virulency. (260) Like other of the contagious diseases, certain influences of an unknown kind—[Such as determine and modify the prevalence and type of epiemics generally, and to which the expression "epidemic influence" is applied, for want of a better?]—favour the development and extension of rabies. Some remarkable instances are given in which it appeared as a general epidemic among dogs at one time, foxes at another, and wolves more rarely. (268.) In different outbreaks it has been observed that certain rabid dogs have infected the majority of the creatures they have wounded, while others affected with the disease, and equally guilty of biting have only exceptionally transmitted it to those they attacked. It is necessary to remember this when considering the probabilities of the occurrence or extension of rabies. Every creature inoculated with the virus of rabies does not contract the disease. There appears to be different degrees of susceptibility to its action not only among animals of the same, but also among those of different species. [These remarks would be equally applicable to the argument, elsewhere stated :—" that hydrophobia is tetanic in its nature, and not necessarily dependent upon a " specific poison." Personally I offer no opinion on this matter.]

It may happen that of different persons bitten by a rabid dog, those first bitten may escape hydrophobia, while those subsequently bitten may be attacked by, and die of that disease. A case of this nature occurred to Dr. Mackenzie, C.B., C.S.I., in which a child got bitten in the bare cheek, and an officer who came to the rescue got bitten in the arm, through the sleeve of his uniform, yet the child suffered no harm beyond the temporary fright, whereas the officer died within two months after the receipt of his injury. [The circumstance here recorded added to similar cases which have been noticed by other observers is of importance when considered in connection with what is adduced above in regard to hydrophobia and tetanus. Had the disease been dependent upon a specific poison alone, the subject of the first bite would naturally have suffered the most severely—as happens in cases of cobra-bite.]

4. REPRESSIVE MEASURES.

Mr. Fleming does not think it would be prudent to conclude that the facts are sufficient to warrant the conclusion that the means of preventing the appearance of hydrophobia in persons who have been bitten by mad dogs have yet been discovered. [Elsewhere he writes: No result has followed the " experiments " on dogs for rabies, performed by M. Pasteur; no cure for the disease has been found; and the preventive measures are purely those of police. (*Nineteenth Century*, March, 1882.) These measures are equally applicable whether the disease which, in some instances, follows upon dog bite, be *tetanus* or specific hydrophobia.] He mentions the fact that he himself, at the time that he had an unhealed wound on the finger performed a tedious post mortem examination of a dog dead of rabies, and yet escaped the disease. [A circumstance which militates against the theory of "specific poison." Frank Buckland relates how, after dissecting the body of a rat killed by the bite of a cobra he having had a small wound on his throat; he became affected in a minor degree with the symptoms of cobra poisoning. (See his Life.) Syphilis is readily contracted through abrasions or slight wounds on the hands; so also is septecemia while examining bodies of persons dead by peritonitis.] He thinks that experiments on rabbits or even on dogs cannot always be successfully realised when dealing with mankind [Very important evidence against the utility of such experiments.] Much has to be done and learnt before this method of protecting beings from the action of a most deadly poison can be accepted as certain; and then will arise the question as to its duration, the dangers, if any, attending or following it, and the practicability of its application to animals as well as to mankind. Mr. Fleming is of opinion that by the enforcement of due precautions the disease rabies would be eradicated; as indeed it has been in countries where these measures have been efficiently applied, and he urges that effect be given to similar measures in Great Britain. (*Times*, Nov. 26, 1885.) [Note what has already been stated with reference to the "eradication" of epizootic and epidemic diseases in general.]

The natural result of what is stated above would be that the plea of "necessity" for experiments could no longer stand. It is well to observe in this place that the fact of a dog suddenly snapping at and biting a person is no more a necessary indication that the animal is rabid than a similar action on the part of a horse is proof of "madness" on its part.

With regard to *Suppressive Measures*, Mr. Fleming writes:—"The most important point, perhaps, with the prevention and suppression of rabies is (as with all other contagious maladies) the vulgarisation of a knowledge of the disease and its symptoms. Provide dog owners when they receive their tax-papers, with printed, easily understood instructions as to the proper method of keeping their dogs healthy, and how to detect the early symptoms of rabies, as well as information respecting the preservative and sanitary police measures which they should comply with, in order to prevent the disease. All this might be printed on the back of the tax-papers, which could, in addition, be made a valuable means of arriving at certain important information such as the sex, age, breed, &c., of the licensed dogs. The most serious consequences result from the ignorance of the public on simple matter like this of rabies."

(*Animal Plagues*, p. 275.) Reference then occurs to the regulations contained in the "Dog Act," chap. 56, 24th, July 1871, p. 283.) [To maintain dogs in health they should be properly (and kindly) cared for, in regard to their food, water, exercise, and general hygienic management. The practice of muzzling and of tying up dogs is condemned as cruel and unkind. The slightest sign of illness should be at once attended to.]

Published reports during the past few years have recorded the introduction and subsequent abandonment of various kinds of "inoculation" as prophylactics against, or curative measures in actual attacks of particular diseases "specific" in their nature. Among such may be again mentioned syphilis and cholera. For the former disease, the practice, introduced from Norway, and almost immediately abandoned and condemned; for the latter, recently tried in Spain with disastrous results. That for charbon or anthrax, advocated in France, was in Hungary suppressed by Government as being directly injurious to the animals inoculated; in England declared to be valueless for its assigned purpose. In China the system of "inoculating" for small-pox was originally introduced, and has been subsequently employed—not as a preventative of—but in order to *keep up* the disease small-pox. In that empire, where the same practice is still followed, and is employed as a method of cure in actual cases of small-pox, that disease is very far from being extinct—on the contrary, it is very prevalent. It has been found that inoculation for measles has been the means of propagating that disease. In the case of plague the same operation has been performed with fatal results, and has in consequence been acknowledged to be unjustifiable; in practice the form of disease thus artificially introduced was not found to be milder in type than that which was naturally acquired. Inoculations practised on animals in Calcutta and in France, with tissues and fluids from subjects of cholera have yielded contradictory results." In March, 1882 certain inoculations were performed with the "microbe" of yellow fever, and thus, it was stated, 400 persons of Rio Janeiro rendered (yellow) fever proof. The commission which was appointed to examine the "discovery" alluded to, replied to M. Freire the "discoverer" by a formal contradiction, and declined to recognise that his vaccinations were harmless. (See *Zoophilist*, June 1884, p. 22).

It has been pointed out that the liability of different persons to contract hydrophobia is apparently very different. [So it is the liability of persons to tramuatic tetanus very different. The frequency of that affection also differs according to geographical region, climate, and season; in these respects, it corresponds with hydrophobia.] "But just as there is in some persons a liability to second attacks of small-pox or of scarlet fever, a liability which the bulk of mankind does not share, so there may be liabilities in man which the lower animals do not share, and the observations made upon the latter may require [Another important admission] some modification in their application to the former. (*Globe*, May 21st, 1884.) Considering that it occasionally takes (several) years for the disease rabies to show itself after a person has been bitten, and in dogs generally fifteen months, it is difficult to understand M. Pasteur's confidence in his own conclusions.

By the light of what has already been adduced on the relation existing between tetanus and hydrophobia, it may be fairly questioned how far such cases as are here alluded to are connected with a bite received at so distant a time.] He can never tell whether his animals, if they survive, may not at any time become affected with madness. (*Zoophilst*, June 2, 1884.) This is the third discovery of the same kind. There was first the inoculation for "chicken cholera," of which we now hear nothing; there was then the supposed preventive against anthrax, which was disposed of by the report of the Hungarian Commission; there is now the inoculation by means of which there is to be no more hydrophobia. But poultry-yard cholera, ovine carbuncle, murrain, rinderpest, small-pox and scarlatina are as severe as ever. (Ditto, Sept. 1st, 1884).

"Notwithstanding the entire confidence of M. Pasteur the first inoculation in the case of man must necessarily be as experimental as any of those with the lower animals. Cases of hydrophobia [or of an affection so-called] are increasing in most of the countries in Europe. The disease appears to be restricted to places where the canine race meets with care and kind treatment. [Evidence occurs elsewhere that ill-usage is by itself a sufficient cause of rabies in a dog.] The wild dogs of Eastern towns, and the Esquimaux dog do not suffer from rabies, and the disease is confined to the old world." (*Morning Post*, June 3rd, 1884.) [Certain exceptions to this statement with regard to the Esquimaux occur elsewhere in this summary.]

In Peking rabies is extremely rare, although every householder owns one or two dogs, and these animals, which prowl about the streets and devour every kind of garbage, are never muzzled. (Epitome of Medical

Reports, p. 166.) In Constantinople ownerless dogs prowl in thousands in streets and alleys, they live on garbage, and thus perform the functions of scavengers, yet among them a case of rabies is never met with. In Peking, a similar state of things is recorded. In those cities, the remark has been made that these ownerless animals meet with no direct unkind treatment. The disease appears to be absent from Australia, Madeira, South Brazil, and the Hudson's Bay territory; also from large tracts of Africa. In the chronological record given hereafter, however, several occurrences of the affection in America are noted. In Texas the circumstance is remarkable that although dogs, wolves, racoons, and other wild animals are often affected with rabies, very few persons bitten by rabid animals in that country suffer any inconvenience; the chances are ten to one that the bite will not hurt them. (*Globe*, October 2, 1882.) In Russia and in India, on the contrary, the liability of persons so bitten to become attacked with hydrophobia appears to be considerable. (Dr. Henderson.) [According to the theory of "specific poison" it becomes difficult to explain these differences.]

Pasteur's system differs radically from that of Jenner. The latter represents an attempt to guarantee the economy against a certain malady by the introduction of the virus of an analagous but far milder malady. This vaccine being a special and distinct substance, having a special and distinct nature, is procurable directly, and without further preparation from the tissues of any child or adult recently inoculated with it. But the " attenuated virus of M. Pasteur's school is quite another thing. It is in no sense a 'vaccine;' it does not represent a special and natural disease, but a manufactured disease, a substance artificially prepared, and of which the efficacy is so rapidly destructible that it is necessary to produce it afresh from week to week. There is no such thing known as 'lymph' bearing to hydrophobia the relation that vaccine bears to small-pox. If the proposition to compulsorily inoculate all dogs be ever entertained, it will be necessary to determine the duration of the period for which the efficacy of the "vaccination" may be expected to last, for it is almost certain that the canine organisation, like that of the human being under similar circumstances will entirely free itself from the preservative effects of the prophylactic virus, and will again become susceptible to rabid disease. Attenuated virus is still virus, it is not vaccine. (Mrs. Kingsford, M.D.).

But the question obtains increasing importance as our inquiry proceeds —Is there, in relation to rabies in certain animals, a specific virus at all? So far, indications point somewhat definitely to a reply in the negative, if such a virus actually does exist. Will inoculated dogs be perfectly safe domestic associates for our children and ourselves? May not the poison inoculated as "protective" of the dog be capable of producing madness in a person accidentally bitten? When regard is had to the vast number of canine inoculations that would be enforced, it is impossible to avoid the apprehension that in some cases, danger of the kind indicated might ensue.

The circumstance already noticed (*ante* p. 48) has also an important relation to the present subject, namely :—that the commission appointed in 1881 by the Hungarian Government for the purpose of inquiring into the value of M. Pasteur's inoculation as a prophylactic against cattle disease, found that that system "tends to accelerate the outbreak of certain morbid conditions which bring about fatal results from other serious diseases." Farmers have been heard to say that a beast once inoculated is never himself again. At Beaucherry and Montpelier a series of "protective" inoculations for anthrax were performed, with the result that several deaths by that disease occurred among them, M. Koch being led by that circumstance to the conclusion that "M. Pasteur's system of prophylactic 'vaccination' is devoid of practical value. (Science Monthly, July, August, 1885).

Certain it is that the vast series of experiments in splenic fever performed by M. Pasteur have yielded results which are worse than valueless, so insufficient and so evanescent is the immunity against natural infection conferred by his preventive inoculation, and so grave are the dangers which it develops for man, and other non-inoculated animals. And there is not the slightest reason for believing that the new vaccine of rabies, prepared in the laboratory of the same *savant* by similar processes, will be one whit more efficacious. Here is a question purely of matter of fact. And any mind trained to weigh and appreciate evidence according to the rules followed in our courts of law is in a better position to judge of it than a mind destitute of that discipline, warped by professional prejudices and fettered by medical etiquette. (W. S. Lilley, Esq., *Fortnightly Review*, August, 1885.) [The subjects of anthrax and of zymotic inoculation here touched upon are discussed at greater length under these respective heads in a previous part of this pamphlet.] (See pp. 35 and 42.]

With reference to the question as to whether or not rabbits are capable of being affected with rabies the following extract is made from the

Gazette Medicale de Paris, January 22, 1881, namely ;—" M. Pasteur inoculated two rabbits with saliva from a child just dead from hydrophobia, with the result that these animals died in less than thirty-six hours afterwards. Their blood, after, death, when examined microscopically presented minute organisms having the appearance of the figure 8; these occurring in masses. These organisms, when introduced into a *bouillon* of veal propagated in it rapidly, and when inoculated in other rabbits, induced death similarly as in those from which they had just been taken. But, it is asked, was the disease in either case *la rage?* M. Raynard said yes. M. M. Colin and Dujardin Beaumetz say no. M. Pasteur says neither yes or no.

On the subject of micro-organisms, Koch was of opinion that M. Pasteur did not find that of rabies *if there be such*, but merely a bacterium which he quite gratuitously assumed to be that of a new disease, whereas the phenomena to which it led (Dr. Koch evidently considers the bacterium to be the *cause*, not the effect, of the disease) were none other than septicemia in the rabbit. (*British Medical Journal*, May 5, 1883, p. 870).

That the phenomena presented by animals after "inoculation" for rabies differ according to genus and species has been noticed by various writers. M. Pasteur admits the fact that they differ in inexplicable ways, and that experiments performed on animals are utterly contradictory of each other in their results. (*Zoophilist*, July 1884, p. 53.) According to Dr. Gimson, M. Pasteur inoculated some rabbits with saliva taken from a child affected with hydrophobia; shortly afterwards he preformed similar experiments with the saliva of children dying of broncho-pneumonia, and with the result that similar symptoms occurred, and similar microbes were found as in the previous case. Professor Vulpian injected under the skin of rabbits saliva from persons in good health, with the result that the rabbits died within forty-eight hours, their blood filled with the special microbe Pasteur believed he had found in hydrophobia. Other rabbits placed under identical conditions and inoculated with the same saliva did not experience the slightest inconvenience. (*Science Monthly*, July 1884, p. 2.) [It accordingly follows that there is nothing specific in character in the diseased condition in rabbits which follows inoculation with the so-called virus of rabies.]

Hydrophobia is not the same as rabies. Rabies is canine madness; hydrophobia is that disease which is produced in man by the bite of a mad dog. [According to evidence already adduced, it does not always follow that the bite of a mad dog; it sometimes occurs after the bite of a dog that is not mad, and symptoms in all respects similar to those of hydrophobia in some instances have been recorded in persons who have not been bitten by, or been in contact with any dog.] The two diseases resemble each other; they are not identical; nor does it follow that a *specific* for rabies (did one exist) should likewise prove a specific against hydrophobia. Even were it granted that Pasteur's method of inoculation conferred the protection claimed for it, there is no evidence to show that inoculation after the bite of a rabid animal will prevent the development of rabies in a dog so bitten. From certain cases already recorded, there is every reason to question the belief that inoculation of a human being after he has been bitten by a rabid dog will prevent the occurrence of hydrophobia in him. [At least one definite case in which the disease occurred after the patient had undergone "inoculation" has been already related.] According to Professor Youatt, and other high authorities already named, rabies may and does originate spontaneously in dogs. If, as is claimed, Pasteur's vaccination is only available against inoculated rabies, what measures are to be adopted to prevent idiopathic or spontaneous rabies? There exist already so many better, and unobjectionable plans of dealing with that disease that Pasteur's "inoculation" may be allowed to fall into disuse. That system necessitates a continuation of the very evil which it is designed to counteract. (*Zoophilist,* January 1, 1885, p. 489).

Mr. Tait observes that:—There are some twenty zymotics among our domestic animals. He asks: "Are we to have each of them inoculated some ten or twelve times, each time for a different disease?" He adds: "To take the analagous case, of vaccination and small-pox, vaccination protects the individual to a large extent, but does not protect the community." (Uselessness of Vivisection, par. 55).

An admirer of M. Pasteur's method thus describes his process, and the condition of the animals thus operated on: "They are tied down to a board, put under the influence of chloroform. They do not recover consciousness until the top of the head has been sewn up again, and the suffering does not commence until hydrophobia" declares itself. In the Rue Vauquelin dogs that have been thus operated upon "may be seen in every stage of hydrophobia, some in a dazed and somnolent state, others foaming at the mouth and dashing themselves furiously against the bars of their cages." (*The World*, January 15, 1886).

On the other hand M. Chavée-Leroy points out that in the case of all the patients taken to the laboratory of M. Pasteur the application of the cautery had already taken place; that as to enraged rabbits they exist only in imagination; that of the animals retained in that laboratory—or rather laboratories, including 80 rabbits, 37 dogs, 12 guinea-pigs, 6 monkeys, and 15 pigs, not one was really suffering from the disease in question. (*Les Maladies de la Vigne*, 1885, p. 58.) [Here, then, in the circumstance recorded sufficient explanation appears of the small number of cases of hydrophobia which occur in patients who have undergone inoculation under M. Pasteur. But the question still remains, whether the true disease, rabies, is transmissible by artificial inoculation according to the method there followed. According to the theory elsewhere in this paper noticed, that the disease in man is a form of tetanus, and the evidence given that spontaneously arising cases occur in dogs and in men, the inference becomes natural that for its production in the first instance the introduction of the "poisonous virus" is not absolutely necessary. It is also to be observed that all the symptoms described in the preceding paragraph are very natural results of operations on the skull, including injury to the brain and its membranes. To the doubts already cast upon the dependance of hydrophobia in man on the presence of a specific poison, or, indeed, any poison at all, we are thus led to question whethor the animals here alluded to were themselves the subjects of specific disease. In fact, the closer these points are examined, the less secure do the data on which the prevailing theories with regard to them appear to be.]

The extremes to which the doctrine of "zymotic vaccination" has led is described after this manner in the *Medical Press and Circular*, December, 1882: "It need hardly be said that certain new 'remedies' alluded to are of American origin, the names of which are sufficiently indicative of their composition; namely, syphilin, bubonin, gonorohin, glandrin, anthracin, &c." The several preparations here alluded to are said to find a place in the Homœopatihic Pharmacopœia of the Great Republic; they are all obtained from morbid products of the animal kingdom, and are dignified with the titles "isopathics" or "nosodes." What is all this but the theory of "specific poisons" and "inoculations" run completely wild?

"Science," it has been observed, "in the main most useful, but sometimes proud, wild, and erratic, is lately proposing a desperate device founded on a hypothesis clever and specious, but not yet gilded by wisdom or proof. Science proposes to inoculate new diseases into our old stock, in the anticipation that thereby new diseases will put out the old." But—Dr. Richardson is reported to have advised his audience when, as President, he addressed the Health Congress at Brighton :—" Be not led away by this new conceit." (Macaulay's Claims of Vivisection Partridge and Co.). The propagation of disease on the pretext of, thereby arresting disease is bad in logic, wicked in morals, and foolish in practice. (*New York Medical Tribune,* 1881).

5. RABIES AS A ZYMOTIC DISEASE.

The degree to which rabies prevails among animals and hydrophobia in man is not uniform ; on the contrary, the affections so called have their periods of activity and non-activity—in other words, they occur and recur as epizootics and epidemics. The following record will illustrate this point, the particulars being gathered from Reynold's "System of Medicine," from Fleming's "Animal Plagues," and from other sources, namely :—

A.D.

1271. Wolves in Franconia affected with rabies. Those so affected spared the herds and sheep, and attacked human beings.

1500. Canine madness prevailed in Spain.

1586. Among dogs in Flanders, Hungary, Austria, and Turkey.

1590. Among wolves in the province of Monthelliard.

1604. Among dogs in Paris.

1691. The heat of summer 'intolerable,' the crops withered from want of rain, animals died in great numbers, and dogs went mad.

1708. Rabies among dogs in Suabia.

1719 to 1724. Several outbreaks of rabies occurred in France and Germany, the disease continuing to prevail during the whole period.

1722 ,, 1723. Attacks of rabies occurred in Hungary.

1725 ,, 1726. Several outbreaks of the disease occurred in various parts of continental Europe; dogs, wolves, and "wild animals" in Saxony being affected.

1734 ,, 1735. Rabies in England.

1741. Rabies in Barbadoes.

1760. Rabies prevalent in London.

1765. A hydrophobia panic in London, and throughout England.

1770 to 1771. Rabies in America.

1776. In the Antilles. Also in London and in England generally, the nation actually "groaning under the malignity" of the epidemic influence.

1779. In Philadelphia, and other places in America.

1783. In the West Indian Islands.

1785 to 1789. In various parts of Europe. At this time rabid wolves terrified the people more than even rabid dogs. In the latter year rabies prevailed in Germany and in America.

1803. Among foxes in the Pays de Vaud, and in several districts of the foot of the Jura. Also in Peru.

1804. On the northern shore of Lake Constance, and extended throughout Germany.

1806. In the vicinity of London, and elsewhere in England.

1807. In Ireland.

1808 to 1809. In the Kingdom of Würtemberg and in the Grand Duchy of Baden rabies prevailed, foxes being extensively affected.

1809 to 1812. Rabies in foxes near Zurich.

1810. Rabies spread throughout Russia. It appeared also in America in Ohio, where it destroyed wolves, foxes, and various domestic animals, besides human beings.

1813. It appeared in Mauritius.

1815. It raged in Denmark, Norway, and Austria.

1820. It was again on the increase in England, and for three or four years continued alarmingly common, but afterwards moderated for a few seasons.

1819 to 1829. Rabies was prevalent in Italy, wolves, as well as dogs, being affected with the disease. From 1819 to 1826 it affected foxes in Switzerland and Germany. the foxes communicating it to dogs, cats, horned cattle, horses, pigs, goats, and sheep. In 1822 in Holland. In 1823 in Norway, Russia, and England. In 1824 among foxes, wolves, cats, and reindeer in Sweden, Norway, and Russia; among dogs in England and Ireland. It prevailed also in India. In 1827 in Switzerland, and in Würtemberg; "many men were bitten by mad foxes, but owing to the prompt measures usually adopted, the individuals escaped; dogs and cats that were bitten, however, became rabid."

1830. Fox hounds in England were affected with rabies to an extent never before witnessed, nor since. The same disease also prevailed in Vienna.

1830. In Barbadoes.

1834. In Saxony.

1835. In Chili.

1836. In Paris.

1837. In Germany and Austria.

1838 to 1841. Rabies continued to prevail in Vienna.

1839 ,, 1842. In Würtemberg, affecting foxes and dogs.

1840 ,, 1842. In Lyons, and other parts of France.

1841. In Vienna, Germany, and France.

1851. In Mauritius.

1866. In England.

1868. An epizootic of rabies broke out in Lancashire, and continued during several succeeding years. The same disease prevailed also in Vienna.

1870 to 1872. Rabies, which prevailed for several years among the sledge dogs in South Greenland, extended to Smith's Sound, where it threatened the destruction of the species, and consequent extinction of the Esqumiaux, who are dependent on their dogs. [From what is stated elsewhere, rabies appears to be a very rare disease indeed in Greenland.] In the latter year the disease existed at Lucknow, India.

1871. It prevailed severely in England.

1873. In New York, and in other parts of the United States.

1873. to 1874. In and around Vienna.

1874. Rabies, which appeared in Lancashire in 1868, continued to prevail in various parts of England, with a progressive increase in the number of animals that fell victims to it. The same disease also appeared in various parts of America, the skunk (*Mephitis Americana*), being among the animals affected.

1883. A hydrophobia panic existed in Geneva. Seventeen cases of rabies having been reported in that city, a raid was made upon dogs in general, stringent orders issued regarding muzzling, and in fact alarm "exploited" among the public much as panic in London has recently (1885) been encouraged, and added to by certain organs of the press. Then, no distinction was drawn between spontaneously originating cases of the disease, and those which were due to the bite of a rabid animal; the circumstance was also noticed as singular that outside the city of Geneva little, if anything, was heard of the disease. (*La Tribune de Genêve*, June 9, 1883).

1884. There occurred 19 cases of rabies in the vicinity of the Brown Institution, London; the outbreak then ceased. (*Zoophilist*, July 1885, p. 50).

1885. It is said that a more severe outbreak of rabies exists in England than has occurred for many years past. There is every reason to believe that the scare in London is traceable to M. Pasteur's "experiments" in Paris—as there always is a scare among impressionable people about something or other—trichinosis at one time, cattle plague at another. In London the disease was reported to have increased subsequent to the general muzzling of dogs as enforced by orders of the police. In America, to judge by the papers, the disease had suddenly become as common as measles; mad dogs and frightened patients were daily reported. But some people dispute the reality of the epidemic there and declare it to be a senseless alarm. In France there was also a hydrophobia scare; the general alarm took almost the form of a new mental disease. As a fact, hydrophobia is extremely rare in dogs; a man who has spent a life-time associated continually with forty or fifty of these creatures may perhaps have seen one or two cases of that disease. But it becomes a matter of public inconvenience and annoyance—to say nothing of the suffering to harmless sensitive, and sensible creatures as dogs, when a temporary "fad" like the present is artificially kept up and banned.

[It is impossible to carefully study the record now given and not be struck with the frequency with which an "epidemic" of rabies occurs in London.]

From the above particulars, imperfect as they confessedly are, the circumstance becomes evident that, like other epidemics, rabies in animals, and hydrophobia in man, have their periods of incidence and their periods of absence. These diseases affect animals and man in different proportions in different outbreaks, and in different localities. Similar differences also take place with apparent reference to season, and general atmospheric conditions, in which respect both affections appear to obey the law which governs the occurrence of diseases generally which implicate the nervous system.

6. METHODS OF TREATMENT.

The occurrence in man of hydrophobia as a result of bites by rabid animals, namely dog, jackal, fox, wolf and tiger was recognised, and both diseases fully described by the physicians of ancient India. Among the means recommended by them, alike for the prevention and for the cure of the disease in man several were enumerated which at the present day are adopted; they include suction, excision, and cauterisation of the part bitten—[It is even asserted that "he who has been cauterised may go about his business with equanimity, though the dog that bit him be ever so mad"]—the application of oil, of the flesh of the animal inflicting the wound, and the administration of datura. (Hindoo Medicine, Dr. Wise, p. 404.) In China similar measures are employed, with the addition that the dried body of the Spanish fly is said, when swallowed, to be a *specific*; as a matter of fact, however, it has no more right to be considered as such than have the various other remedies which have from time to time been so designated. (Epitome ot Medical Reports of Chinese Customs).

Unfortunately the good effects of cauterisation above indicated was not realised in London during the "epidemic" of 1885. It was particularly observed that in a number of the fatal cases in that "epidemic" the wound had been cauterised when freshly inflicted. [A circumstance which supports the theory already expressed that hydrophobia is a form of tetanus, as against that that of specific poison.] There are persons who make very light of dog bites ; a poultice is enough in their opinion to prevent all evil consequences. It sooths the torn surface, it settles the startled imagination, [an additional point in favour of the nervous versus the specific-poison origin of the disease] and the rest may be left to nature. (*British Medical Journal*, January 30, 1886).

In the *Salut Public* of Lyons, June 1865, a case is detailed in which Dr. Buisson reported that he "cured" himself of hydrophobia by means of a vapour bath of the temperature of 52 deg. centigrade. [In the *Rappel* of November, 1885, a detailed account is given of the symptoms under which the gentleman here named suffered ; aud taking that account in connection with what has already been adduced with reference to similar cases, there is every ground for relegating those so detailed, to the same category, namely, what may be called, "fictitious hydrophobia."] It is stated further, that he had attended more than eighty persons bitten by mad animals, and had not lost a single case. [Observe, it is not stated that those persons were suffering from hydrophobia.] As a preventive measure he recommends that when a person is bitten by a mad dog he must for several days take a bath *à la Russe*. When the disease is declared, it only requires a vapour bath rapidly increased to 57 deg. centigrade, then slowly to 63 deg. centigrade. Dr. Buisson administers these baths daily for seven days, the patient under certain circumstances, being subjected to the bath during twelve consecutive hours. He considers this treatment to be certain if adopted on the first day of the disease, uncertain the second, and hopeless on the third. This method, however, has not been generally adopted, as no doubt it would have been, had the medical profession been satisfied with regard to the success claimed for it. Other authors in this country, as well as on the Continent, have similarly recommended the employment of hot baths, both as a preventive and as a means of treatment in cases of hydrophobia. (Atkin's Science and Pract. of Med).

In 1878 Dr. "G. R." wrote that he had "discovered an efficient and permanent cure for hydrophobia," but nothing came of it. (*Medical Press and Circular*, Nov. 18, 1885, p. 482). [Neither are particulars given regarding its nature.] In 1882 pilocarpine was tried in the treatment of this disease, but without success. (*Globe*, Aug. 16, 1885.) [This drug, for a time, vaunted as being equal in efficiency to the Russian bath, was soon afterwards declared to have no curative effect in the disease.] Among other remedies to which a trial, though unsuccessful, has been given, are venesection, opium, belladonna, mercurials, iron, arsenic, nitrate of silver, camphor, musk, cantharides, turpentine, tobacco, nitrate of lead, cuprum ammoniatum, hydrocyanic acid, galvanism, electro-magnetism, strychnine, oxygen, nitrous oxide, chlorine, guaiac, and nearly a hundred and fifty others. (Aitkin.) Injection of warm water into the veins has been unsuccessfully employed. The root of the *Alisma plantago* (water plantain) has been stated to be successfully administered, both in Russia and in Italy. The root is grated, and being spread on bread and butter is thus taken, with the result that if used before distinctive symtoms appear it prevents the occurrence, if after they have developed it stops them and cures the patient. (See *Evening News*, Dec. 23, 1885.) [The use of *Alisma plantago* in the treatment of hydrophobia has been known in England for the past 150 years. It occurs in an MS., written by Thomas Drake, of Feltwell, and the recipe has never been kept secret.] (*Morning Post*, January 5, 1886.) Had the remedy been remarkable for success, doubtless its writers would have been acknowledged. Cannabis Indica and Calabar bean have had their advocates. In France the employment of garlic has been revived as a "specific" in hydrophobia, the reputation of the drug in this respect dating from the year 1560.

Instances are on record in which to all appearance the development of hydrophobia was checked by the excision of the part bitten, several days after the receipt of the injury, that is, before the eighth or ninth, as mentioned by Blaine "on the dog." In that work the statement occurs that local irritation occurring in the seat of the wound was a sure prelude of the virus being about to become active, and that excision might then avert the threatened attack. Dr. Mackenzie, C.B. met with a case in which (as already mentioned) this actually happened.

But there are on record some cases of hydrophobia in which recovery took place, and was due, not to any particular method of treatment, but to simple resolution on the part of the patient to conquer the disease, after its symptoms had become manifest. Such a case was that of Dr. Barthelemy, described by himself. (Dr. Tuke, *Influence of Treatment on the Body*, 1884, vol. ii., p. 236.) Another remarkable case of the the same kind was that of the celebrated Andrew Crosse. (Memoirs, p. 125.) He compelled himself to walk, after the attack had set in, and finally recovered. (*Zoophilist*, January 1886, p. 155.) Spontaneous recoveries from hydrophobia are acknowledged to occur by a committee, as reported to the Paris Academy of Medicine by Decroix. According to the same committee no treatment has been found to be anti-hydrophobic; all means used since 1874, including injections of azotate of pilocarpine, appears to have hastened rather than retarded death. Rabid (hydrophobic) patients when left in the dark and kept quiet are not subject to fits; and when cases of recovery take place, the "cure" is due to the efforts of nature. (*Medical Press and Circular*, August 2, 1882, p. 92).

How far similar cases could be multiplied is uncertain; but the few particulars here given are important in their relation to the recognised influence exerted on the bodily condition in other diseases than hydrophobia, through the exercise of powerful nervous resolution. They are also of importance with reference to the alleged prevalence of that disease in persons of weak nervous power.

7. SUMMARY OF THE PRECEDING REMARKS.

1. A record of published statements in support of the advantages claimed for "inoculation" for the prevention and cure of hydrophobia is given. To each statement thus presented is appended an abstract of opposing or counter-balancing evidence. It is shown that the present "idea" and practice of such inoculations are not new; on the contrary, they are resuscitations of an old "idea," and of a practice which had already fallen into disuse and neglect. Examples are given of inoculations with animal virus of different kinds, including viper and rabies poison against hydrophobia, but unsuccessfully. The manner in which "inoculation" of animals is said to be performed by M. Pasteur, namely by trepanning and direct application of the "virus" to the brain cannot, and dare not be practised on man. Hence, in his case conclusions drawn therefrom are inapplicable.

2. From the date on which the first record appeared of "inoculation" by M. Pasteur of human beings, for hydrophobia, the great majority of the organs of the press, medical, and non-medical, inculcated caution against a too ready acceptance of conclusions proclaimed by the chief (and sole) advocate of that practice. The grounds upon which this was done are detailed in paragraphs quoted from those organs. It having been found practicable by more ordinary measures to check or moderate the prevalence of rabies, no necessity is shown to exist for the complicated and questionable

method of "inoculation" proposed by M. Pasteur. Whether dogs that have been "inoculated" can with safety to non-inoculated dogs and human beings be permitted to be at large has not yet been answered in the affirmative. No dog has been cured of rabies by means of inoculation. Pasteur's method has been directly challenged, against the ordinary measure of cauterising and otherwise treating the parts and patient bitten by a rabid dog. Five deaths of persons under treatment by "inoculation" are recorded; full details given in regard to one such case. The circumstance is especially noteworthy that the advocacy of "inoculation" in the instances above related comes almost entirely from non-medical men; also, that medical men, as a body, hesitate to accept, or absolutely disbelieve in that method. Past experience and analogy are against the practice. In fact, M. Pasteur himself, as already shown by the evidence given in the preceding paragraphs, is not satisfied as to the efficacy of his own system.

3. Various questions present themselves:—Is the saliva of a healthy dog capable of causing hydrophobia in a person bitten, or rabies in another dog? Does hydrophobia ever occur in the absence of a dog-bite? Does rabies ever occur spontaneously in dogs or other carnivora? Answers in the affirmative are given to all these questions. Instances of resistance to the "poison" of rabies occur in man and in dogs. Questions also arise, and are discussed, as to whether hydrophobia is specific in its nature, except that it is a form of tetanus; and whether a disease presenting its characteristic symptoms has or has not arisen idiopathically. Great variety is observed in regard to the degree of severity of the disease rabies in different epidemics; as to the kinds of carnivorous animals chiefly affected; and the proportion of persons bitten in whom hydrophobia occurs.

4. The means of preventing the occurrence of rabies after bites have not yet been devised. Nor can results of experiments on this point performed on animals be realised in respect to mankind. The belief is expressed that by the application of measures, the nature of which is indicated, the disease could be eradicated. [Judging from analogy with other diseases, this belief may be questioned.] Inoculations for those diseases which are enumerated has been successively tried and abandoned. Differences in the liability of men and of animals to become affected by means of inoculation are noticed, and certain geographical regions named in which the disease does not prevail. There is a radical difference between the natures of "vaccination" for

rabies, and that by Jenner against small-pox. According to "experiments" on the subject rabbits are not affected by rabies after inoculation, but with pyemia. Questions have arisen as to whether the animals inoculated by M. Pasteur's method, namely, by trephining the skull, suffer—not from specific rabies, but from symptoms which are the natural results of the injuries to the skull and brain thus inflicted.

5. A record of the recurrence of rabies as an epizootic disease is given. From that record the belief is authorised that the prevalence and distribution of that affection are regulated by similar laws to those which determine the phenomena of the great class included under that term. And varieties in the incidence of rabies take place as there do in other epizootic diseases.

6. Various methods of treatment are described. The circumstance is noticed that recoveries—infinitely few in number, have occurred in the absence of remedial treatment of any kind,—but simply due to innate resolution of the affected persons.

The general result of the foregoing inquiries therefore is: that by the practice of "inoculation" alluded to there is no proof whatever that hydrophobia can be prevented, but that the practice itself carries with it a possible danger over and above the risk pertaining to a bite by a rabid dog. The occurrence of rabies in carnivorous animals may be looked for in the future, as it has been observed in the past to occur from time to time as an epizootic disease. What is by some persons held to constitute the "stamping out" of that disease signifies no more than that certain measures being employed, and the epizootic meantime running its natural course towards cessation, the two circumstances are looked upon in the light of cause and effect, whereas they are verily contemporaneous with each other.

Note.—The final correction of the proofs of the above article was made in March, 1886. At that date no distinct evidence was forthcoming that the "inoculations" described had resulted in the prevention or "cure" of hydrophobia in any single case. On the 11th of that month the statement was officially made in the House of Commons, that—"the information then in possession of the Local Government Board with reference to the discovery, by M. Pasteur, of a *cure* for hydrophobia were *too vague* to afford material for an examination of the results obtained." On the 18th the statement was published, that—"Herr Gossler, Minister for Public Instruction, did not consider that the moment had arrived for

Prussia and the Empire to apply those results." Persons accustomed to official phraseology will readily interpret the actual significance of these expressions. In fact, the journals had ceased—from that point of view to discuss the question—which had thus, in its turn, and like so many others in relation to "experimental research" been relegated to—oblivion.

On 20th March, according to the *Pall Mall Gazette*, "What M. Pasteur injects into children who are under his treatment is not stronger than pure rain-water; the four children sent from America were so to qualify them to "draw" at a show in New York, where Pasteur's method of operating is exemplified on them with his five syringes, filled, however, with distilled water, instead of with cultivated virus. As to the "enthusiasm of the banking houses which have subscribed handsome sums to the Pasteur Institute, of the ladies of the Faubourg St. Germain, the Russian Princesses, *et cetera*, that should not be allowed to weigh a feather in the appreciation of any reasonable being." According to the *Standard* of 23rd, "There are plenty of people, even among M. Pasteur's own countrymen, who prefer to exercise a wholesome scepticism with regard to his assumed discovery." According to the *Standard* of 24th, "One of the nineteen Russian peasants undergoing professor Pasteur's treatment, died on 22nd at the Hôtel Dieu of Hydrophobia." And so ends the present section!

TUBERCULOSIS.

As a matter of convenience, the remarks on the subject of tuberculosis which are to follow will be arranged in chronological order, it being considered that in this way the several points to be brought forward will best present themselves in their process of development. Commencing, therefore, with the year 1881, it is recorded that M. Toussaint vaccinated a cow in the advanced stage of tuberculosis with lymph absolutely pure. The vesicles progressed normally, and with the lymph obtained from them he vaccinated different animals, all of whom subsequently became tuberculous. (*Medical Times and Gazette*, Sep. 3.) Dr. Creighton wrote:—"It is possible to conceive of the pieces and particles of the primarily diseased body acquiring a kind of spermatic virtue which gave them the power of communicating the specific disease (tuberculosis) as a whole, and in its several manifestations to another body in which they should happen to lodge. But it is hardly possible to think of a neutral living organism, such as that named by Klebs, *Monas tuberculosum*, being charged with the power of conveying so complex details of form and structure from one body to another." (*Nature*, October 27, 1881).

In 1882, Dr. Drysdale wrote to this effect, with respect to certain experiments detailed by Dr. Koch:—"Dr. Koch's experiments are a further extension of the discovery made, some twelve years back, by Dr. Villemin, by inoculating a number of rabbits with the fluids of consumptive patients. We all know the history of these inoculations, and how the profession is still sceptical as to the contagiousness of pulmonary consumption. Although he has seen a number of cases in which the disease had been previously present in the husband or wife of the patient, the overwhelming mass of cases have no history of a like kind. Among the richer classes, consumption supervenes in families where the father, mother, or near relations have died of it, and this is so constant that he feels quite unable to accept the view of consumption being in any way contagious." (*Medical Press and Circular*, May 31).

Dr. Yeo stated, as a proposition, "that tubercle is an infectious malady, originating in a specific virus, propagated by the conveyance of that virus from body to body, and originating in no other way." He then describes the experiments performed by Villemin, in which animals inoculated with tubercular matter, presented within fifteen days afterwards tubercular matter in the viscera; also, that cats and guinea pigs were readily inoculated, but sheep, goats, and birds escaped infection. The same results were obtained from injecting hypodermically the sputa of phthisical patients. Blood from phthisical animals gave no results; but, that taken from phthisical men, after death, produced general tuberculosis in rabbits. Villemin's results were confirmed by those of Chauveau, by means of experiments on oxen, animals disposed to tuberculosis. To them, human tubercular matter was administered by the *stomach*, with the result that typical tubercle granulations were found in the lungs.

Drs. Burdon Saunderson and Wilson Fox said, that rabbits and guinea pigs were rendered tuberculous by inoculation other than with tubercular matter; namely, with pus, or with caseous matter of inflammatory origin, or with sarcoma, as well as with tubercle; that in a guinea pig it could be produced by the insertion of a simple seton, and in the rabbit by a simple deep incision without inoculation of any sort. Wilson Fox introduced beneath the skin of guinea pigs portions of putrefied muscle, fatty liver, and vaccine virus, with the same result. It was maintained by others that aniline blue, cinnabar, cautchouc, cotton, etc., caused similar effects; also that carnivorous animals could be fed long on tuberculous living without the production of tuberculosis. Then it was said that the lesions produced by Villemin were not tubercle, but simply inflammatory lesions, or embolic infracts. On the other hand, Chauveau, Klebs, and Bollinger supported the views of Villemin.

Two physicians inoculated themselves with the serum of a blister applied to a phthisical patient, and without effect. In 1874, three medical men of Syra, in Greece, inoculated a man of fifty-eight years of age with tubercle. Three weeks afterwards there were signs of commencing induration in the apex of the right lung. On the thirty-eighth day he died of gangrene with which he was affected at the time of inoculation, and the autopsy disclosed small tubercles in the apex of the right and left lungs, and of the liver.

Tappeiner and others have shown that animals could be rendered tuberculous if tuberculous matter were diffused in the air they respire. Gerlach, that tubercle may be conveyed to cattle by the stomach, either by means of tuberculous matter, or by milk. Dr. Hyppolyte Martin makes the distinction between true and false tubercle. He points out that the structure, microscopically, of true and false tubercle is identical; but that true infective tubercle is reproduced indefinitely, while false, or pseudo-tubercle is innocuous. Dr. Martin injected into the peritoneum of guinea-pigs, lycopodium, pepper, and cantharides. These experiments show that foreign bodies, non-specific, may set up inflammation, the products of which have a complete anatomical resemblance to tubercle, yet have no specific virulence. On the other hand, pus from a scrofulous gland, as well as scrofulous products, produced general tuberculosis. Dr. Martin observes that, "we have two inflammations, one specific, infective, and truly tuberculous; the other non-specific, non-infective, and not true tubercle;" that the only difference between them is that the former differs from the latter by the presence of the property of a morbid agent at the present time unknown. It is this unknown morbid agent which Koch believes he has shown to us—an agent which the microscope failed to discover until the employment of the special methods of preparation discovered by him. His experiments appear to have proved that the "virus" of "tubercle" is the property of a micro-organism peculiar to tubercle, and which may be called the tubercle bacillus.

But Dr. Martin has also obtained a series of cases of generalised tuberculosis by successive inoculations in guinea pigs, the original inoculation being, in one case, from a small collection of pus, found after death, encased in the sub-maxillary gland of a child who had died of measles and broncho-pneumonia without any trace of tubercular disease, but with characters of scrofula; in another from non-degenerated scrofulous products removed by operation. On the other hand, followers in this country of Niemeyer's theory adopt the origin of pulmonary consumption in ordinary inflammatory processes. Then, again, it is stated by Dr. Yeo that such cases are not cases of phthisis, but of pneumonia. He writes :—"The idea that consumption is a contagious disease is not a new one. This doctrine has always been maintained in the south of Europe." He quotes Sir Thomas Watson, as saying, "Is phthisis contagious? No: I verily believe not. A diathesis is not communicable from person to person. Neither can the disease be readily (if at all) generated in a sound constitution." Nor is it ever imparted, in his opinion, even by one scrofulous individual to another. On the other hand, Dr. William Budd promulgated the view that tuberculous disease was eminently contagious. Dr. Yeo, however, considers that consumption is not contagious in the sense usually attached to that word,—that communication of the disease from wife to husband is very rare,—from husband to wife less so. In married women, the fact of the onset of the disease following or occurring in connection with impregnation and utero-gestation, naturally provoked the suggestion that infection took place through impregnation, and from the fœtus, just as constitutional syphilis is conveyed from husband to wife. Another hypothesis is that during the puerperal state the female constitution is very prone to the reception of *germs* of infective disease. (*British Medical Journal*, June 17, 1882).

Dr. Allinson writes with reference to cases of phthisis to which Koch's theory cannot be applied :—"There are a few well known kinds of consumption,--knife or scissors-grinders' consumption; the miners', the stone-masons', and the cotton workers'. There is also the fibroid consumption due to excessive drinking of alcoholic liquids. Many of the present generation are carrying the germ theory to excess. (*Knowledge*, July 7, 1882).

Mr. Vacher, "that bovine and human tuberculosis are essentially the same, and that bacilli are the existing cause of tubercle. But there must be a predisposing cause essential to the production of the disease— a tubercular condition." (*British Medical Journal*, Sep. 16, 1882).

Dr. Williams, in alluding to the discovery of the tubercle bacillus, by Koch, writes:—"How far consumption is infectious, is a question which has been under discussion for centuries, and on which great differences of opinion has prevailed, and still prevails; the north of Europe holding, as a rule, its non-contagiousness, and the south its contagiousness. Heredity, which is the source of a large amount of phthisis, cannot be reconciled in its action with the bacillus theory. How are we to account for the cases where the parents, having died of consumption, the children are successively attacked on arriving at a certain age, with a severe type of the disease?" Having given statistical particulars in regard to the Hospital for Consumption, Brompton, (*British Medical Journal*, Sep. 30, 1882, p. 618.) Dr. Williams says:—"A consideration of the above statistics, would lead us, as Dr. Cotton says, to hesitate before classing phthisis among the infectious diseases in the ordinary sense." Dr. Thompson (p. 620) "concludes that phthisis is not a zymotic disease, capable of sowing itself, but rather an ulcerative process capable of giving rise to pyemia." Dr. Thompson, however found that "excluding all cases of hereditary taint among 15,000 consumptives, fifteen instances of wives becoming infected through nursing consumptive husbands." Dr. Vacher's cases (*Clinical Transactions*, vol. vii., p. 144) prove the possibility of phthisis being communicated from husband to wife." In reply to comments made on his paper, Dr. Williams (p. 321) said, "that he did not question the existence of the bacillus, but only the part it played in the pathology of phthisis, which he *thought* consisted more in spreading the secondary inflammation than in the causation of the disease."

Dr. Robertson gives details of one hundred cases of phthisis, then draws the following *conclusions*, namely:—

1. *Probably* in every case of phthisis, the inception and presence of a specific bacterium is essential.
2. *Probably* there is "a certain risk" of communicating the disease to unaffected persons.
3. Continued association with a consumptive person is *probably* not of itself sufficient to originate the disease.
4. The preparation of the lung tissue by a chill, debility, etc., is *probably* as essential to the destructiveness as the presence of the specific bacterium itself. (*British Medical Journal*, Sept. 30, 1882).

According to Dr. Flint, "Pulmonary phthisis, in a certain proportion of cases, has a self-limited duration, the disease ceasing to exist after more or less progress of the local affection, all symptoms referable to the lungs disappearing, and recovery as regards the general health being complete. The disease advances not by a continuous progress, but by a series of successive invasions, separated by variable intervals." He will not discuss "the question whether—the explanation is—by the statement that each eruption of tubercle, for a time, exhausts the tuberculous cachexia, or whether the fact be owing to the production of the *bacilli tuberculæ*. It suffices to state the clinical fact." (*British Medical Journal*, Sept. 30, 1882).

Koch's conclusions, with regard to a bacillus, are by no means generally accepted, and numerous objections have been raised against his interpretation of facts which all agree in observing. According to Dr. Formad (of America), tuberculosis is a disease peculiar to individuals in whom a special anatomical condition of connective tissues exists; certain animals, of what he terms the scrofulous class, are naturally subjects of such anatomical structure, and as it was in such animals that Koch's experiments were performed his theory is thus far supported. He claims that the tuberculous infective material of Koch may be developed in such animals by any foreign body, *e. g.:* clean glass, in powder, introduced into the peritoneum or elsewhere; therefore, bacilli are a result, not a cause of tuberculous inflammation. From experiments performed by Professor Burdon Sanderson, in 1868, conclusions not unlike those formulated by Dr. Formad may be deduced. At present, the question of Dr. Koch's explanation of tuberculosis remains *sub judice.* Not a little harm has been done by the "scare" which pervaded the world on publication of the supposed infectiousness of consumption. (*Medical Press and Circular*, Dec. 27, 1882).

Dr. Watson Cheyne experimented on twenty-five rodents. In six, setons of various kinds were introduced subcutaneously and *into the eye;* in ten vaccine lymph was employed; in three pyemic pus; in six various materials, as cork, tubercle hardened in alcohol, and worsted thread were introduced into the abdominal cavity. None of these twenty-five animals became tuberculous. He injected "cultivations," obtained from Dr. Koch, into twelve animals, "chiefly into the anterior chamber of the eye," with the result that all of them became tuberculous. He writes regarding them, that "all that has as yet been absolutely proved is that a variety of materials in man which we class together as tuberculous, produce acute tuberculosis when inoculated into rabbits, guinea pigs, and other animals, and that result is only due to the tubercle bacilli in the material inoculated." It therefore remains for inquiry, what relation these bacilli bear to the morbid processes in man in which they are found." (*Medical Press and Circular*, March 21, 1883.) [No explanation occurs in reference to the precise object with which the eye was selected as the part of the body to be experimented on. It is not asserted that the *virus* of consumption, even did it exist, is received naturally into the system through the organ of vision. The reader will also observe that no other organ is more sensitive to pain than the eye.]

According to Messrs. Greenish and Holt, " no certain means have yet been described of detecting the *Bacillus Tuberculosis* from any other forms of bacteria. The test by aniline dye is not a satisfactory one. Balogh and Lichtheim have found bacilli with the same reactions, the former in the marshes of Budapest, the latter when examining fœces. So that bacillus cannot be looked upon as characteristic of phthisis, still less as the cause." (*Medical News*, Feb. 16, 1883, p. 200).

Spina denies the inoculability of tubercle; he denies the very existence of tubercle bacillus distinguished from the bacteria of decomposition; he denies that the bacteria of Koch are impermeable to acids. Koch maintains that if investigators have failed to obtain results similar to his own, the fault has, in every case, been theirs. (*Medical Press and Circular*, April 11, 1883).

Spina did not deny that tuberculosis was an infectious disease, but considered that the *proofs* that it was so did not stand. He maintained that Koch had adduced no proofs that the " bacillus tuberculosis " was a distinct species. It had been found in lupus, in the tongue coatings in healthy, and in unhealthy persons; in alvine evacuations in a case of typhus; in sputa of bronchial asthma; in the lochia of a healthy woman; in crupous pneumonia. Tuberculosis had been produced by the injection of non-tuberculous matter, as powdered glass and cinnabar into the peritoneal cavity of guinea pigs. The subject of tuberculosis at the stage at which Koch had left it, presents, at certain points, even more difficulties than it did before his investigations. (*Medical Press and Circular*, June 27, 1883).

According to Dr. Heron the " tubercle bacillus " was found in a case presenting the symptoms of acute pneumonia. The organism has been observed in cases of lupus, in freshly opened and scrofulous glands, in synovial degeneration of joints, in the meninges of the brain, in an ulcer of the tongue, and in a previously unopened suppurating knee-joint. But he considers it to be a fact that practically, in every case of consumption this organism is to be found in the system. As to the infectious nature of phthisis, there are not wanting records of observations tending to answer the question in the affirmative, and no lack of evidence which is regarded by many as proving that phthisis is not infectious. (*British Medical Journal*, April 28th, 1883).

Klebs maintains that Koch has not succeeded in proving his case. He has not proved that his bacilli are organic structures. His cultivation experiments are imperfect. Other bodies besides these bacilli are acted upon by aniline dye, and in an exactly similar manner. (*Medical Press and Circular,* May 9, 1883).

The *Lancet* (Sept. 15, 1883, p. 465) describes the "discovery" by Koch of the parasitic nature of phthisis as *exaggerations.* The statement also occurs that the contagiousness of that disease is maintained by an "overwhelmed minority." Bouchardat considers that in particular conditions lymphatic corpuscles are metamorphosed into pathogenic bacilli.

Let us now, in brief, present a summary of the several conclusions which, according to the preceding paragraphs have resulted so far from the "experiments" narrated. Those conclusions are to the effect as follows, namely:—Certain cows, inoculated with vaccine lymph, became themselves tuberculous. It is not comprehended how a neutral organism such as the *monas tuberculosus* can convey tubercle from one body to another. Notwithstanding certain experiments on rabbits the medical profession remains sceptical with regard to the contagiousness of pulmonary consumption. Next, that tubercle originates in a *virus*, and is infectious. While certain kinds of animals are inoculable with tubercular matter, certain other kinds are not so inoculable. Certain animals were rendered tubercular by the injection of non-tuberculous matters. A distinction is drawn between "true" and "false" tubercle, at the same time that the structure of both is admitted to be identical. Products of non-specific inflammation may resemble tubercle anatomically. Pus from a scrofulous gland may produce tuberculosis. The virus of tubercle is the "property" of a micro-organism. The doctrine that phthisis is contagious has always been maintained in the south of Europe. Phthisis is a diathesis; and a diathesis is not contagious. Tuberculous disease is eminently contagious. It is not contagious.

Whether it is or is not contagious remains an unsettled question. Heredity cannot be reconciled with the bacillus theory. The bacillus spreads the secondary inflammation in phthisis rather than is a cause of that disease. Various " probabilities " with reference to the etiology of phthisis are enumerated. Bacilli are a result, not a cause of tuberculosis. The part they play is *sub judice;* it remains for inquiry. The bacillus is undistinguishable from other forms of bacteria. Its very existence is denied. The question of tuberculosis now presents more difficulties than it did, even before the performance of " experiments " alluded to.

The total results of the " experiments " detailed being among themselves mutually contradictory as now shown, hospital and other clinical experience has to be appealed to. From the *experience* thus obtained on the subject of communicability of pulmonary consumption or tuberculosis, Dr. Pollock, of Brompton Consumption Hospital, showed that in that institution, so far was that disease from being contagious that the attendants on patients were actually more free from the affection than the generality of the same class who were not exposed to the supposed contagion. (*Lancet*, April 28, 1883.) Dr. Andrews, of St. Bartholomew's also was of opinion that there is not sufficient evidence to prove that its prevalence is materially affected by direct contagion. (*id.*, May 10, 1884).

Whence it follows that in so far as the benefit of man is concerned, the " experiments " above detailed were needlessly and uselessly performed. The circumstance also is noteworthy, that not a syllable occurs in the details connected with them calculated to have a bearing upon medical treatment, which is the true end and object of medical knowledge.

SILKWORM DISEASE.

M. Pasteur having assured himself that the corpuscles or bacteria, seen for the first time in the bodies of silk worms by M. Guérin Menneville were only met with in diseased insects, demonstrated that these bacteria multiplied during the metamorphosis of the worm, and finally destroyed it. He attributed the spread of the disease to contagion; and to the circumstance that the droppings from diseased worms polluted the mulberry leaves on which the other insects fed, and that thus a single infected silkworm was capable of destroying a whole "school" of others. His preventive measures were the selection of healthy insects. With this view he crushed the bodies of such as had laid eggs, and by means of the microscope examined the pulp so made for corpuscles. If found, the eggs just laid were immediately destroyed. By this application of scientific research to the silkworm industry, the silkworm disease has been almost wholly put an end to, and thus, a great industry which was threatened with extinction has been saved from the fate which threatened it. (*British Medical Journal*, Dec. 6, 1884).

We shall see as we proceed how far the benefits here assigned to the "experiments" on silk worms by M. Pasteur are or not established by evidence on authority to be adduced.

With a view to indicate how far the teachings of the past could, if referred to, have furnished rules with regard to the management of silkworms in health and when diseased, it is deemed appropriate in this place to revert to the writings of old Chinese authorities on these subjects. Accordingly, the following particulars are selected from translations of works of this nature contained in the *Chinese Miscellany*, 1845-1849, only those being taken which have a bearing upon the "experiments" of M. Pasteur, to be subsequently alluded to. In the first place, repeated instructions occur in regard to *cleanliness* and the frequent removal of *excrements* of the insects. (p. 68.) It is observed that the most important thing in rearing silk-worms is the getting of proper seed. The best, most healthy, and strongest of the worms are to be *selected* and set *apart*; only such insects as are perfect and good are to be retained. This is the way to secure good seed. Moths that have stunted wings, bald eye-brows, scorched tails as if yellow with smoke, or that have red bellies without hair; also those that come out either before or after the others must all be rejected. (p. 71.) [These characters of objectionable insects are sufficiently definite, nor do they need the use of a microscope for their detection.] Minute instructions then occur with reference to manure suitable to the mulberry-tree, irrigation, and plants that may be raised with them. (p. 77).

In 1849 a translation of a work on the manufacture of silk, written by Tseu-kwang-k'he, was published at Shanghai. According to that translation the earliest Chinese work on mulberry silk was published within the period B.C. 2356 to B.C. 722. The usual tradition is that silk culture was discovered and encouraged by Shi-ling-Shi (Yuen-fe), wife of Hwang-ti, "the Yellow Emperor," about B.C. 2640—2600 B.C. (p. 1.) Eight kinds of silk-worms are described (p. 6); so is the advantage of retaining the "seed" in low temperature prior to placing them for hatching. (p. 7.) In selecting cocoons for seed those that mount highest on the bush will probably produce the thinnest silk, and those that select a lower position will not lay eggs; hence the middle ones should be selected. (p. 8.) [Instructions of easy application, and requiring no artificial aid.] In choosing the cocoons for seed—hold them up to the light, and separate the bright and clear from the thick and solid ones. (p. 11.) [Simple and effectual.] In selecting the eggs which the female moths have laid on the cards, those which are laid in a circle or in a heap may be rejected. (*id.*) [No less simple and precise, and requiring no other aid for discovery than that of the naked eye.] When fresh-cut mulberry leaves are placed for the worms to feed upon they will descend to them of their own accord; those worms which will not leave the card, or crawl about on the back of it, can be rejected. (*id.*) [Naturally, because the circumstance recorded shows that they are sickly.] Again, stress is laid upon the fact that "the most essential thing in rearing silk-worms is the *selection* of seed. (p. 19.) When moths are allowed indiscriminately to deposit their eggs, when they are huddled together so that they steam on account of the heat thus generated, they hatch too soon and never come to maturity. "The case of the sickness of a parent descending to the offspring is thus exemplified." Worms that mount upwards towards the light, or get into the thatched covering of their rearing-shed, will produce the strongest and healthiest cocoons. Reserve only the sound, perfect, plump, and good moths, reject such as have the character already described; set them all aside as useless. (*id.*) Take means (the nature of which are described in detail) to escape the inconvenience of cold; follow the instructions given with regard to the feeding of the worms; thus the worms will be preserved from sickness, their sicknesses will be few, their cocoons properly formed, and your whole establishment will succeed. (pp. 28, 29).

When worms grow much they must be separated; when their excrements abound they must be removed. Some persons leave them for a long time huddled or confusedly mixed together, or even throw or toss them about. Sickness and injury of worms spring in a great measure from these causes; hence the necessity of gentle usage and roomy accommodation for them. When crowded together or allowed to remain damp they will be indisposed. (p. 32.) "The Silk-worm Classic" says:—"When the silk-worms fall into and awake from their torpor at irregular intervals the silk will be deficient in quantity, which is very true." (p. 33.) Want of care in feeding the old silk-worms as well as the young will be injurious; refuse leaves will generate in them the affection called "bush dampness." (p. 39.) The insects fed upon damp leaves will pass a white liquid and die. (p. 40.) Minute instructions appear with regard to substances which are not to be brought near silk-worms, and what may be called the personal hygiene of their attendants. (p. 42.) Late silk-worms are not only subject to "many sicknesses," but they produce less silk than the early insects. (p. 43.) In warm weather the worms are subject to disease from the heat; in very rainy weather to "the white sickness." (p. 55.) The pallets of old rice straw provided should be renewed once a day, the neglect of which precaution will produce much sickness (p. 56); after which follow minute directions with regard to the different kinds of mulberry plant, the method of propagation, manuring, and general management.

It is therefore to be observed, with reference to the general code of instructions above summarised, that the Chinese, while paying every attention to the silk-worm, recognised the fact that the health of that insect was itself dependent upon the conditions by which it was surrounded, including food, cleanliness, climate, season, and conditions affecting the plant on which it fed. It is also evident that while measures were indicated in regard to sickly and unhealthy insects, the prevention of epidemic diseases formed an important portion of the general rules in regard to sericulture.

In a native Chinese treatise on the cultivation of silk, translated by M. Julien, rules are laid down with reference to the provision of pure air and wholesome food for the silk-worms, and suggestions for improvement on the part of unprejudiced European silk-growers. From which expression the circumstance transpires that, want of success following upon innovations in regard to methods which experience had proved the utility of in the rearing of silk-worms, had many years ago become subject of notoriety, that notoriety extending from Europe—even to China. In 1860 it was deemed probable that "for a long time to come we shall have to resort to China for fresh supplies of silk-worms' eggs, when from *epidemic disease* those hatched in France from acclimated worms cannot be depended upon." "If there have been diseases among the worms the previous year the walls, boards, and tools for cutting off branches and spreading out leaves must be most carefully scraped, washed, and purified, or the building and works destroyed or disused." Writers have enumerated as diseases of silk-worms "calcinès" and "lusettes," "gras," "tripes," and "muscardine;" the two former produced by electrical and atmospherical causes, the next two by improper state or quality of food, the last as "an ineradicable plague or leprosy; the cause unknown." Hatching of silk-worms, so we learn from the article now quoted from, "has been saved when supposed to be beyond restoration to vigour, and thrown out upon a straw-yard by the *clear cold air* of night and *almost every worm* has formed its sound cocoon, the crop being four days after gathered from the straw;" and further and fuller information on the same subject appears in the article quoted from. (Encyclop. Britan., Art. Silk). Here, then, in the extracts given are instructions, simple and ample, with regard to the management of silk-worms and the diseases to which they are subject.

"The Manufacture of Silk" forms the subject of the "Special Series" of Reports, No. 3, issued by the Imperial Customs' Department of China, 1881. In the course of individual reports, included in that series, there occur certain references to the management and diseases of silk-worms, to which it becomes interesting to refer in connection with the subject immediately in hand, the observation being premised, that the measures to be alluded to have recommended themselves to the Chinese as a result of practical experience extending over many centuries. [Besides the dates already given, the silk industry was extensively conducted in the provinces of Chensi and Shansi twelve centuries B.C.]

With regard to Newchwang, the statement occurs that a disease among the silk-worms, which is common in Japan, is there rarely met with. The disease in question is occasioned by the "uji" fly (believed to belong to the genus Ichneumon), which penetrates with its ovipositor the bodies of the caterpillars, and deposits in them their eggs, which latter produce parasitic larvæ—and these destroy the further development of the silk-worm. Other diseases among them are unknown in Manchuria. (p. 16).

At Chinkiang, the following instructions are attended to in the management of silk-worms:—Each bundle of cocoons is carefully examined, as it is taken off the straw on which the larvæ had been previously put, in order to "spin their silk;" any black (Flacherie?) or putrid worms are removed in order to avoid the cocoons being soiled.

Maggot-bitten cocoons, the product of worms stung by flies, or upon the bodies of which flies have laid their eggs—are to be kept separate.

So also are *sick* or *filthy* cocoons made by worms that, on having finished weaving, die, instead of changing into chrysalides, and discharge filthy water. (p. 53).

At Wenchow, in removing layers of leaves, carefully, with the worms upon them, *the residue* and *excrementitious matter* must be all *thrown away*. After the second torpor, every time the worms are removed, they must be put wider apart. (p. 128.) The most important thing in rearing of silk-worms is the getting of proper seed. The worms that are intended to be reserved for seed should be nourished with double care. From these select the best sort of worms and give them extra feeding; when come to maturity, *set apart* those that *appear* most healthy and strong. Five days after the cocoons are primed, *pick out them that appear thickest and fullest*. (p. 129.) [From these particulars further evidence appears of the care with which the instructions alluded to were drawn up, and also of the easy applicability of these instructions. A simple reference to them might have saved much time, and many "experiments" in France and elsewhere.]

At Canton silk-worms are subject to frequent outbreaks of disease—thus not only being the cause of loss of hoped-for profit, but of money invested in their culture. Failures take place, from time to time in the quality of the fibre of the cocoon, or in that of the mulberry twigs on which the insects feed. Thus the actual yield of silk becomes extremely variable from year to year. (p. 154).

Mr. Fortune states, with reference to the cultivation of silk in China, that the produce and management of silk-worms take place, not by large farmers or extensive manufacturers, but by millions of cottagers, each of whom owns and cultivates, for that purpose, a few roods or acres of land only (*Residence among the Chinese* p. 342), except in certain monasteries where the process takes place on a much larger scale. (p. 367.) Everywhere, the greatest possible care is taken in the management of the "worms" in the several stages of their existence. (p. 373).

From the particulars here given we accordingly learn that the Chinese are well acquainted with the necessity of selecting healthy silk-worms for the production of healthy "seed;" that they practice the separation of diseased worms from those that are healthy; and that they know how to distinguish, by unaided sight, the healthy from the unhealthy and the diseased silk-worms. We, moreover, learn that notwithstanding all this practical knowledge, epidemics prevail from time to time; that there are intervals of freedom between such outbreaks; and that as a result the produce of fibre varies very greatly from one year to another.

It is convenient also, to state, in this place, certain particulars with reference to the cultivation of silk in China, the better to follow some remarks on the subject which occur in the notes hereafter given regarding measures taken to introduce into France new "seed," during the epidemic of *pebrine* in that country, about to be narrated. These remarks are made on the authority of M. Natalis Rondot; *L'Art de la Soie*, Paris, 1885:—In China the plantations of mulberry trees are often attacked by an insect. (*Cerosterna punctator*, p. 217.) [Here is an obvious cause of failure of silk-worms by reason of *famine*.] It has been remarked that a pale colour on the part of the silk-worm (Bombyx mori,) and of the silk produced by it indicates a weakening of the individual or of the species. (p. 219.) Cocoons of a sulphur or orange colour are considered to be such as are degenerated. (p. 221.) China, as far as "seed" is concerned, was of no assistance to France during the epidemic. Seed raised in China by Italians and French cultivators gave no better produce than that which was indigenous ; the worms that issued from them did not resist the disease. A portion of that " seed " presented *corpuscles;* it was not to that cause, however, that the want of success was attributed, but to the *weakening of the race*,—that weakening, the result itself of artificial and forced rearing, which has been introduced into various localities in China. According to M. Brunat, however, the worms of different races, having white cocoons, are very robust, and have such a degree of resistance, that even, when full of corpuscles, they form excellent cocoons, and their moths lay eggs of excellent appearance. (p. 231.) According to Duseigneur, " in the rearing of Chinese silk worms in France, nearly all, and at times all the worms are diseased," —proof of falling off in the race, in his opinion, of non-acclimatization, in that of other observers. It is remarked, however, that the ordinary mortality of worms in China (at How-chow-foo, 1859,) was forty per cent. (p. 232).

M. M. Pila et Cie have observed, in their *Statistique*, 1883-4, that in China, the diseases *pebrine* and *muscardine* have prevailed for a very long time (très longtemps), and are ever increasing. They assign as reasons of those maladies the inclemency of seasons ; and with the return of more favourable seasons they looked for a more productive silk-harvest. Besides the two diseases already named, *la flacherie* committed sad ravages, more especially in Tché kiang and Kiang-su. The statement also occurs that in addition to inclement seasons, the causes of these several diseases were to be found in the circumstances that of late years, in order to force the production of silk, " old traditions—with regard to the method of rearing worms—which had their *raison d' étre*—had been broken ; "—that in certain localities, together with this disordered method of training there were also inevitable neglects. (p. 237.) Then follows a brief summary of rules for the rearing of worms, including a return to usages established by past experience (" Le salut de cette grand industrie dépend de la science des *Barbares*") after which, is the important remark that, " in general, in China and Japan the worms of the indigenous races support a much greater proportion of *corpuscles* than do those of European races." (p. 238.) In the provinces of Kiang-su and Che-Kiang various races of silk-worms are reared ; but sericultors are of opinion that the quality of the silk depends less upon the race of the worm than upon the nature of the leaf on which it is fed, upon the mode in which the worms are reared, upon the period, and upon the manner in which the cocoons are chosen. (p. 244.) Except, however, in the two provinces named, the quality of cocoons in China is inferior to that of the cocoons in France and Italy. (p. 248.) In fact, for eighty years Asiatic silk had not been used by the manufacturers of of Lyons. (p. 252).

In Japan, according to M. Rondot, the indigenous races of silk-worms were formerly remarkably strong, and when imported into Europe resisted the epidemic ; it was known, however, that the same form of disease had affected the worms being reared in Japan. That power of resistance was attributed to the nature of their food, and to the method of "crossing" species. When in 1866 twenty-five *cartons* of "seed" were selected with care and were sent to France, eighteen of these *cartons* presented eggs containing *corpuscles*. From this circumstance it is seen that *pebrine* may be distributed in a country and affect successive rearings even when the greatest care is bestowed upon them, yet without inevitably bringing about failure of sericulture. A parasitical insect, (*udschymia sericaria*) the eggs of which became hatched in the larva of the silk-worm and developed in the chrysalis, is thus the cause of great destruction among these insects. (Op. cit. p. 289.) From 1868 to 1870, under the combined effects of ready sales and high bounties, the training of silk-worms in Japan was pressed on by artificial means and without prudence; thus the quality of the silk produced became so depreciated that whereas the exportation in 1864-5 had been 800,000 kilogrammes it was no more than 400,000 kilogrammes in 1870-1. The Japanese therefore reverted forthwith to their ancient methods of work. (p 285.) Thus was preserved to that country the industry which constitutes its chief richness. (p. 291).

[In these remarks is presented in vivid contrast the opposite results of "experiment" as applied to artificial rearing of the silk-worm, and of "experience" in restoring prosperity which had in that manner come to be threatened with extinction.]

Adverting to the disease of silk-worms in Italy, M. Duseigneur expresses his opinion that fineness of the silk is a result of so-called improved cultivation which itself threatens to prove fatal to that industry ; also that several "races" of worms that are considered distinct are mere varieties, the result of crossings, and modified by conditions of climate, &c., under which they were reared; the *Bombyx mori* being the original stock from which those varieties or "races" have sprung. (*L'Art de Soie par M. Rondot*, p. 61.) During the prevalence of the epidemic in that country it was found that the produce from Japanese "seed" reproduced in Italy was more successful than that direct from newly imported eggs; hence supplies from Japan were speedily abandoned. (p. 64.) Crossings between the indigenous worms with the Japanese proved in some places successful while in others they failed. (p. 66.) Even in 1877 the disease (in Italy) had not ceased, and the success with which different crossings of worms were reared variable; hence the increase in the yield proceeded slowly. (p. 84).

In Spain the disease of the silk-worms appeared in 1853, among those of indigenous growth. From that date the produce of silk has yearly diminished in that country, notwithstanding that "seed" of the best quality was obtained from Japan. The rate of decrease, however, has not been equal in different parts of the country. In the mass it was progressive from 1850 to 1878; from the latter date till 1882 there was an increase, but in 1883 again a material falling off. (pp. 157, 158.) That decrease is accounted for in part by inclemency of seasons, but chiefly by the fact that mulberry trees have been uprooted and their places taken by vines, orange and other fruit trees, rice, and by pot herbs, all of which are more profitable than silk culture. (p. 159).

According to instructions in common use for the management of silk-worms (in European countries), when chang'ng their abode from the small room in which they are first hatched, to the proper apartment where they are to be brought to maturity, and set to spin their balls, they must be well cleansed from the litter. The litter, *as well as sickly worms* may be readily removed without handling a single healthy one. The exposure of chloride of lime, spread thin upon plates, to the air of the nursery (*magnanière*), has been found useful in counteracting the tendency which sometimes appears *of an epidemic disease* among the silk-worms, and the fœtid exhalations of the dead and dying. (Dr. Ure's Dict. of Arts, 1875).

In 1857 the Rev. M. J. Berkeley wrote that the *oidium fructigenum* "affects the tissues of plants. A species not distinctly allied is no less destructive to silk-worms, more especially in the larvæ state, though the pupa is sometimes affected in the cocoons. Where the disease has once made its appearance, nothing will arrest it except the most complete sanitary measures; every part must be well washed with chloride of lime, or some other disinfecting substance, and a new stock must be procured from an uninfected place. (*Cryptogamic Botany*, p. 309).

The silk-worm is liable to various diseases, particularly to one by which great numbers are often destroyed, and which "is either caused or characterised by the growth of a small fungus known as silk-worm rot or muscardine. (Chamber's Encyclopœdia).

Thus we obtain further evidence that in Europe, the fact that silk-worms are liable to epidemic diseases (like all other living things) has long been well-known, and also the nature of precautions that are best calculated to be successful against those diseases. There is the additional fact that the presence of a microscopic fungus in the bodies of worms affected with a particular form of disease has been well understood for the last thirty years at least.

The following account of the silk-worm disease in France, and of the "experiments" with reference to that disease, is taken from the recently published "Life of Pasteur." The comments within brackets are such as are suggested and supported by extracts of books on the subject already quoted or which follow :—

In 1849 a disease which gradually assumed the proportions of an epidemic occurred among the silk-worms in certain departments of the south of France. In 1850 and 1851 there were renewed failures. Some cultivators attributed their losses to "bad eggs" [Strange that their views did not extend to ulterior conditions to which the "badness" of the eggs was itself due, and of which it was a result], and accordingly got their supplies from abroad. At first all went well, and in 1853 the produce was plentiful. In 1854 "a singular degeneracy" occurred in the eggs of the imported moths; they became of no more value than the French eggs. [A circumstance readily accounted for by the fact elsewhere recorded, that the mulberry-tree had for several years been undergoing deterioration, consequently the produce of imported "seed" suffered even more than the indigenous from having to feed upon its leaves, for the reason that the indigenous was in a measure accustomed to the gradual deterioration.] The plague spread to Spain and Italy. [Because in those countries the causes to which in France the disease was ultimately due were also in operation. In all these countries the outbreak was contemporaneous.] Fresh eggs were imported from the islands of the Archipelago; but these in their turn became infected, and the disease spread to those islands. Eggs were then procured from Syria and the Caucasus, but the disease followed the trade in the eggs. [That is to say, the eggs brought from the places named being themselves in some instances the produce of affected insects, the silkworms hatched from them, naturally enough, showed also the disease. In this respect disease *followed* the trade; but it was *to* France, not *away* from that country, as the text evidently implies.] In the far east Japan alone healthy in the year 1864. [That is to say, the mortality by pebrine was not particularly great. As already seen, however, the silk-worms, and even their eggs contained *corpuscles;* but the insects being themselves robust were thus able to *resist* the parasite.]

M. de Quatrefages had discovered in the bodies of the worms and moths microscopic corpuscles; Lebert affirmed that they might be detected in diseased silk-worms, and Dr. Vittadini had proposed to examine the eggs with the microscope in order to insure having good ones. Pasteur was attracted to the microscopic study of these bodies, after the method adopted by him regarding ferments. [And further evidence has been already given that the presence of *corpuscles* had been detected by observers antecedent to M. Pasteur. But those observers looked upon the organisms as accompaniments of the diseased condition of the insects, not as the *cause* of that condition.] When, in 1865, M. Pasteur began his investigations he came to the conclusion that the only infallible method of procuring healthy eggs must be by having recourse to moths free from corpuscles. [The presence of corpuscles in the silk-worms in China does not within certain limits affect the production of silk. Worms that die of *pebrine* in France present fewer corpuscles than do some in China that live and propagate healthy eggs.] He succeeded by microscopic examination in finding some moths free from corpuscles. He preserved some of those eggs, as also other eggs which had proceeded from very corpusculous couples; thus he had samples of originally healthy and of originally unhealthy cultivations. His idea was to observe the production of pure eggs by means of moths free from corpuscles. The point to be ascertained was whether there existed the relation of cause and effect between the corpuscles and the disease. [Analogy would have indicated that "corpuscles," "organisms," &c. in other instances occur as the *effect* of a non-physical cause. In this instance that ultimate *cause* were the conditions upon which depended the deteriorated condition of the mulberry plant on which the silk was fed.] For five years his investigations proceeded. [Information already given shows that Chinese sericulturists have had for many centuries a very simple and effective method of detecting diseased or deteriorated moths and "seed," without any such complicated method as that involved in the use of the microscope.]

Opinions differed as to whether the disease was contagious or in its nature purely epidemic. ["Epidemic" in the sense in which famine-fever, scurvy, and other diseases in man are which are directly due to insufficient or inappropriate food.] But every one believed in the existence of a poisonous medium, rendered epidemic by some occult influence. [Why advert to complicated and "occult" influences when the deteriorated food already mentioned was an apparent and sufficient cause? But the circumstance is somewhat remarkable that neither that *cause* nor the conditions to which it was due were alluded to by the distinguished savant named. His views were solely "microscopic."] Pasteur proved that the evil was contagious. [The more readily "contagious" that all the insects were under the same conditions.] He took some sound worms and fed them on corpuscular matter. After a time corpuscular matter appeared in the insects so treated, and from the appearance presented by the skin the disease was called *pebrine*. [Naturally enough, creatures fed on diseased matter became themselves diseased.] While the epidemic was raging there existed examples of successful cultivations conducted in nurseries which had failed from *pebrine* the year before. [It often happens that after a period of great mortality there follows a time when the death-rate becomes relatively small; this in man as well as in animals.] This Pasteur explained by saying "that the dust can only act as a contagion when it is fresh." [How, then, taking this statement as correct, is the extension of the disease to distant countries as already stated, to be explained, and when in transit the "dust" from diseased silk-worms must have long ceased to be "fresh?" But, as also observed, *pebrine* affected silk-worms in distant countries, even before its presence in France was recorded.] The occurrence of the disease in the eggs he explained by the consideration that it was "hereditary."

In 1867 Pasteur observed that out of sixteen broods derived from non-corpuscular parents fifteen succeeded, while the sixteenth perished. [Naturally, healthy parents produce healthy off spring. The Chinese have always been careful to select healthy moths for breeding purposes, nor had they to search for "corpuscles" to distinguish the healthy from the unhealthy.] The worms that so died were free from *pebrine* and from corpuscles—their disease was characteristic, namely, that known as *morts flats* or *flacherie*. It thus appeared that the two diseases *pebrine* and *flacherie* co-existed; it was also observed that the last-named disease came from eggs produced by parents free from corpuscles. [So that the microscope, after all was incapable of indicating the presence of other disease than that *accompanied* by "corpuscles." The Chinese are able by the unaided eye to detect diseased silk-worms.] He found that every *ver flat* is one which digests badly, and every worm which digests badly is doomed to perish. [Naturally, a living creature that is very sick "digests badly."] Like *pebrine, flacherie* is hereditary, [It was with a view to avoid this hereditary descent of disease that instructions by the Chinese are so explicit with regard to the selection of healthy moths for breeding purposes], and also contagious. By means of the microscope it is possible to obtain information as to the health of the worms, the chrysalides, and the moths destined to produce the eggs. But if there is not time to make microscopic examination for the parasitic ferments in the intestinal canal a simple inspection of the worms in their last stage will suffice. [Such "simple inspection" being acknowledged to be sufficient for the purpose, as indeed it has "from all time" been found to be in the great empire whence sericulture sprang, what becomes of the necessity for, and advantages claimed for all the complications of microscopes, converting moths into pulp for examination, and so on? The fact is demonstrated that in reality none whatever either for the one or the other ever existed.]

The practical result of his investigation was, that after a female moth had laid her eggs her body was mashed and examin ed microscopically; if corpuscles are found all her eggs are destroyed, if not found they are preserved. This method of procuring healthy eggs is now universally adopted. (M. Pasteur, Histoire d'un Savant, 1884.) [That is to say, a process which by M. Pasteur's method is shown even by himself not to be absolutely necessary is by him laid down to be adopted.]

It is important to observe with reference to the abstract account of the "experiments" by M. Pasteur now given, that no claim is made by the author of the original record of any definite advantage to the silk industry in France as having accrued from those experiments. All that is claimed s that by employing the microscope it was practicable to detect the presence of one form of disease (*pebrine*) but not another (*flacherie*), and that whereas diseased moths propagated diseased "worms" healthy moths propagated healthy worms. But in China the two latter facts have been recognised for many centuries, and the characters of diseased silk-worms definitely detailed; these characters distinguishable without the aid of a microscope; and M. Pasteur acknowledges the fact that they are so. Consequently his researches in this respect have led, actually to nothing whatever beyond what was known and practised many centuries prior to the date of those researches. It is also important to notice the absence from the record as given above of particulars which bear upon the causation of the diseases named, such as are referred to in the writings of other authors whose writings are quoted.

M. M. Natalis Rondot in his valuable work entitled " L'Art de la Soie," writes to the effect as follows: " When, upwards of thirty years ago sericulture in France was menaced by the appearance of disease in the silk-worms, one of the measures undertaken to meet it was to discover in Asia healthy and strong worms, and elsewhere " seed " either of old indigenous, or altogether new races. " Seed " of this description was brought from distant regions, including Chili, California, The Argentine Republic, Manchuria and Turkestan; but these became the vehicles of the epidemic, which spread from west to east, even including China and Japan. [Compare this remark with that already quoted (p. 118), namely, that " seed " brought from China presented " corpuscles," also that in China *pebrine* had previously prevailed for a very long time.]

In both France and Italy, however, the silk-worms in certain localities escaped; the insects in those instances being of the races originally introduced into them. (p. 33).

In 1857 it was reported that the " germ " of the disease was in the seed, and in 1859 the microscope was employed in order that eggs containing " ovoid corpuscles " should be rejected. Between 1849 and 1859 investigations on the subject of silk-worm disease were made by M. M. Guérin-Meneville, Filippi, Lebert et Frey, Ozima, Carlo Vittadini, and de Quatrefages; in 1856 Emilio Cornalia, of Milan, for the first time expressed a belief that a connection existed between the disease, and the presence of the corpuscles alluded to. In 1865 M. Pasteur, in his turn demonstrated that the corpuscles were the *cause* of the disease called *pebrine*, and that in order to prevent the disease healthy worms and moths should be selected. (p. 34.) That " invention " resulted in the re-constitution of the ancient races introduced into France, especially of those the cocoons of which were of yellow colour. But besides *pebrine*, silk-worms often perished by other diseases, of which fifteen are enumerated, that malady, flacherie, (due it is said to the action of a ferment or a vibrio) muscardine (produced by a fungus) and " grasserie " being the most general. Pasteur's method had reference only to the *pebrine*; the other diseases named, to a great extent defying all such measures, and having within a period of ten years, in which the seasons were unusually severe, destroyed on more than one occasion a great part of the silk crop. (p. 35).

M. Tisserand has pointed out, that in agriculture, besides the labour of man other forces have to be taken into account. This is equally true in regard to sericulture. For example, although man plants the mulberry tree, it is through the operation of natural forces that its leaves develop, and acquire elements which admit of being transformed by the insect into silk; there are also the atmospheric conditions under the operation of which this process is completed. Thus, in the production of silk the work of man is limited. (p. 39.) It is also observed that strong and "rustic" worms do not generally yield silk of fine and brilliant quality. (p. 40.) [From which circumstances we gather (1) that the descendants of silk-worms originally introduced and thus naturalised as regards climate and quality of their food have been relatively free from disease; but that they yield silk of comparatively coarse (and consequently less marketable) quality. (2.) The imported "seed" yields insects which are not naturalised in these respects, and consequently are more liable to disease than those that are; also, that being in their nature delicate their produce of silk is also delicate, and consequently "fine and brilliant" in texture.]

Taking 100 as representing the nominal production of cocoons, that produce in France had decreased in 1871-2 to 66; in the triennial period 1878-1880 to 28. It is observed, however, that the cocoons spun by worms of ancient races on the elevations and hills of Ardèche have a superior quality. The soil in those places is for the most part granitic and schistose. (pp. 89, 93.) In 1865 opinion was favourable to the "seed" imported from Japan; but the remark occurs that the Italians were then in advance of the French in selecting silk-worm eggs by means of the microscope. (p. 94.) [It is important to notice the date here given.] From 1848 to 1850 the production of cocoons was abundant and regular, but most abundant in the former year. In 1853 it decreased from twenty-five millions of kilogrammes to twenty millions. The disease in the insects, first observed in 1820, had extended considerably by 1849; it had indeed existed from old time in the silk yielding districts. [That is, silk-worms, like all other living creatures, were subject to disease and death], and had been the cause of great fluctuations in the periodic yield. In 1856 the produce had decreased to ten million kilogrammes. From 1858 the produce was very unequal, descending in 1865 to five or six millions. After 1871 hopes were entertained that permanent improvement was to take place, and in 1875 the produce had increased to nearly eleven millions of kilogrammes; in 1876, however, the entire "harvest" was nearly lost by reason of inclemency of the weather, the whole quantity obtained being only a little over two millions of kilogrammes. [Here then, is a cause of failure, more calamitous than the *pébrine* had ever been.] From 1877 to 1882 the produce oscillated between four and eleven kilogrammes. (p. 95).

M. Rondot gives in tabular form the purport of the above figures, and represents, as shown below, the average produce of cocoons in a series of periods beginning with 1865, namely, the year in which M. Pasteur began his investigations, [but when, as already observed, the use of the microscope was common in Italy.] It will be sufficient for purposes of comparison to give the whole number of millions of kilogrammes, omitting minor quantities, thus :—

From 1865 to 1867	...	15.	From 1874 to 1876	...	7.
„ 1868 „ 1870	...	9.	„ 1877 „ 1879	...	8.
„ 1871 „ 1873	...	9.	„ 1880 „ 1882	...	8.

From these statistics the fact is brought out that in no period has the produce equalled that of 1850. (p. 96.) [A very important fact considering what is elsewhere asserted in regard to the results of M. Pasteur's "discoveries in having saved a great industry which was threatened with extinction."]

Besides the method recommended by M. Pasteur of selecting healthy eggs, and the use of the microscope, the measure has been taken of reintroducing the race of cocoons from the department of Ardèche, these being of the ancient Chinese race [and consequently naturalised to the conditions of climate, soil, and mulberry plant in France.] But, at present, cultivators have been *unable* [Note this] to preserve silk-worms from diseases which are often the consequence of unfavourable atmospheric conditions; it is also to be noticed that of late years the demand for silk has been less than formerly (p. 98), and consequently the number of sericultors has diminished from 297,130 in 1868 to 165,617 in 1883. (p. 99.) More recently, notwithstanding that there have been fewer cultivators of silk-worms and fewer eggs placed for hatching, the yield of cocoons has increased, but in the varying proportions of 91 from 1875 to 1877, 70 from 1878 to 1880, and 104 in 1881–1882. (p. 100).

At the same time that disease prevailed among silk-worms in France it prevailed also in the mulberry-trees, on the leaves of which the insects fed. [The food being vitiated, the health of creatures that had to live upon it naturally became vitiated also.] Within the past thirty years it has become the practice to plant the mulberry-bushes in too crowded lines, to place under other cultivation the ground so planted, and to supply to the saplings unsuitable manure. Soil which formerly supported these trees has of late become exhausted; indeed there occurs a question whether the soil has not become altogether unsuited for that culture. (p. 100.) At all events, under the action of decay of the mulberry parasites (animal and vegetable) have multiplied, so that those trees have had to be uprooted to an extent which has only left a quantity equivalent to the production of twelve millions of kilogrammes of cocoons—that is one-half of what was the former yield. (p. 101.) [Surely, therefore, the circumstances in regard to soil, manure, climate, and method of cultivating the mulberry plant referred to in the preceding paragraphs, which have resulted in the diminution of food for silk-worms, furnish sufficient explanation for the decay of the insect, which is dependent upon that one and only kind of nutriment; and as microscopic organisms occur in unhealthy plants, so do they chiefly in unhealthy animals—not however as the cause, but as the result of that unhealthiness.]

The practice of renewing the "seed" of silk-worms and of "crossing" different breeds of these insects has been adopted in France, Italy, and Spain from the sixteenth century, and in 1599 a work on this subject was published by Olivier de Serres. It was to this method of "crossing" that Duseigneur attributed the vigour of the Japanese silk-worms, and their resistance to *pébrine* and *flacherie.* (p. 127.) It is a question whether the old description of silk-worms can now be re-obtained, namely, those of a rose or yellowish-rose color, plump, round, pointed, and heavy. (p. 127.) [These being the characteristics of the best quality of insects, according to ancient Chinese rules dating from several centuries B.C.] As in China, so in France, for the production of good quality of silk it is necessary that the seed should have the advantage of a moderate heat prolonged winter temperature, and accommodation as quoted by Du Halde from an ancient Chinese author already alluded to; accordingly in 1830 two mountain stations were established with this view in France, namely one in Drôme, the other in Ardèche. (p. 129.) It is also found that the more the worms have of space, and the more leaves they consume, the richer are their cocoons in silk. (p. 130). [Also in accordance with Chinese teaching.]

Little attention is at present paid in the rearing of "worms" hatched from foreign "seed," or to the varieties of the mulberry on which the insects feed in their own native country, notwithstanding the remarks made on this subject by Réaumur in 1734 (p. 130), and more recently by Hutton at Missoure, in the Himalayas. [Further instructions with regard to the rearing and management of silk-worms occur in the succeeding pages of the same work, the purport of which is to indicate the very secondary position, if any, held by the system of "selection," solely trusted to by the followers of M. Pasteur. Among these, it is recommended to return to the practice (from old time adopted in China) of distributing the raising of seed in small quantities among a large number of families.] Much has also been done that is injurious to this industry in France by speculators for the rise and fall of prices. (p. 133).

With reference to the state of the silk industry in France in July, 1883, the statement occurs that "our rearers of silk are astonished, our spinners still more discouraged. We are unable (in France) to compete, as regards quality, against the Italians. (p. 145.) According to the *Bulletin des Soirs et des Soieries* of 1885 the production of silk in France during each of the five years from 1880 to 1885 was, in kilogrammes, as follows, namely:—in 1880, 525,700; 1881, 750,000; 1882, 772,000; 1883, 611,000; and in 1884, 483,000. In the latter year, although the checks due to *muscardine* and *flacherie* remained exceptional, yet the produce had nearly become a "quantité negligeable." But not alone in France had the decrease taken place; statistics given indicate that it has been general throughout silk yielding countries, China included. (Feb. 14, July 4, Sept. 19).

M. Chavée-Leroy writes to this effect:—Having discovered by the aid of a powerful microscope that the intestines of the diseased silk-worms were not in the same state as those of healthy insects, M. Pasteur declared that, in order to effect the complete disappearance of the malady it was only necessary to make a careful selection of healthy moths for the purpose of reproduction. But the expectations thus raised were not long before they met with disappointment; silk-worms produced by the selected insects contracted the disease, as did the others, when fed upon deteriorated leaves of the mulberry-tree. The celebrated theorist had not considered that the abnormal condition of the intestines in the diseased insects was the *effect* of their disease, not its *cause*. Thus it is that, having prematurely been regarded as the saviour of sericulture in France, he has come to assist at its death. It is true that, as he has observed, treaties of commerce have also contributed to the present precarious condition of that industry. (Les Maladies de la Vigne, 1885, p. 50).

The total result of the analysis now given therefore is that, so far from the silk industry in France having been "saved" by the investiga-tions described, the condition of that industry in the year 1885—that is the latest date to which information can be here brought down—is authoritatively described as being "most precarious," all but a "quantité negligeable." This according to official documents, to each of which it is easy to refer.

THE VINE DISEASE.

In 1865 the vines over thousands of acres in the south of France became suddenly attacked with disease, and for the most part perished. For nearly twenty years that disease has continued, notwithstanding the measures that have been proposed against it, and at the present time is the cause of serious injury to the vine industry in that country.

Various opinions with regard to the nature of that disease have been from time to time expressed. From the circumstance that the presence of the *phylloxera* on diseased plants was always found, the theory was readily and generally accepted that the disease itself was due to the presence of that insect. But objections, up to the present unsolved, have been raised against that opinion. For example, explanation is not forth-coming of the circumstance that all of a sudden an insect should appear and multiply over a great extent of country such as was affected by the phylloxera. On this ground, therefore, a certain number of observers, among them M. Naudin, refused to accept the theory in question. According to them the phylloxera itself became developed as a result of weakening of the plant, that weakening caused by a combination of con-ditions. They further asserted that in the first stage of that constitutional debility the *oidium* made its appearance, the phylloxera in the second and fatal stage. At first the opinion expressed by M. Naudin was contemned; it was adverse to that of MM. Pasteur, Dumas, and Bouley, all whom, notwithstanding the decision of practical men to the contrary gave their authority to the view that the phylloxera itself was the *cause* of the disease in the vine on which that insect occurred. [As a result of those opinions expressed by the savants named, the remedy for "the phylloxera" was sought for in the application to the vine affected of insecticides such as sulpho-carbonates, a method which failed to check in any degree the vine disease.] In 1885, however, M. Luiz de Andra de Corvo showed that the appearance of the phylloxera is always preceded by a diseased condition affecting the condition of the root of the plant, and characterized by definite anatomical appearances (described by him in the *Journal d'Agriculture Pratique*, April 16, 1885); consequently that the phylloxera is itself a sequence of that diseased condition.

M. Hamoir, a practical agriculturist, observed that in the north of France the field-bean had ceased to yield its flower and fruit, that it had "run to leaf," that the leaves rapidly became attacked by vermin, and fell off decayed. He concluded from that circumstance that the fault existed primarily in the nature of the manure used. It is well known that nitrogenous substances tend especially to the production of leaves, calcareous matters to that of fruit; hence due equilibrium between those elements must be maintained. The free use of phosphate of lime, which M. Hamoir recommended in the case of the diseased beans, is no less suitable in that of the diseased vine and all other plants which yield saccharine productions. It is pointed out that if a healthy young apple-tree be transplanted into unsuitable soil the bark will speedily become covered with lichens and mosses. By and bye theorists will see in these parasitical plants the cause of the deteriorated condition of the plant, not in the fact of the soil being unsuitable to its requirements.

MM. Joulie and Ville have pointed out that the vine requires for its regular and normal development, and consequently for its healthy condition, a soil containing in proper proportions phosphoric acid, potass, lime, and magnesia, while nitrogenous elements must be in moderate quantity. Such is the condition of the soil in the Marne district, and there the vine does not suffer from the phylloxera. It equally resists the microbes of oidium, pernospora, and of a crowd of other diseases which according to the modern school float continually over and around them. Here, then, is an explanation of the vine disease; it does not prevail in localities where the soil is calcareous; it does prevail wherever the soil is azotised, such as the valley of the Rhône, formed by alluvium of the Alps.

Fifty years ago M. J. B. Dumas taught that the value of a manure was in proportion to the nitrogen contained in it. His theory was at once accepted, and thenceforward artificial azotised manure was extensively made use of, not only for the vine but also for beet-root cultivation. The consequence was that crops in abundance were obtained, but a gradual diminution took place in the mineral elements necessary for vegetation, the formation of which in the sub-soil takes place very slowly; that exhaustion taking place the more rapidly as year by year, the more the system of artificial manuring was practised, the less was the attention bestowed upon the employment of calcareous materials to the soil. In this way the due proportion which should exist between the different substances required for maintaining the vine in health and developing in good quality its natural products, was disarranged. Little by little the plants, deprived of their necessary elements, began to languish, and then, after a time, became attacked with various diseases, cryptogamic and parasitical, as results of the abnormal conditions thus brought about. Accordingly, under the influence of the false principles above indicated as a result of "scientific experiment," as against practical knowledge of agriculturists, various diseases affecting the vine have extended their ravages, so that at the present time the evil thus occasioned is immense, the losses irreparable.

More recently cultivators in the north, as in the south of France, have experienced a general change of opinion adverse to the fatal theory of M. Dumas, a change in opinion which is rapidly gaining strength and extending. Intelligent practical men recognise the necessity of prudence in the employment of highly nitrogenous manure, not only to the vine but also to the root and cereal crops ; and that more attention is required than has heretofore been given to the use of calcareous matter. They have come to believe that it is by such measures, rather than to the use of insecticides, germicides, &c., that diseases will again become as rare and partial among those plants, as they were prior to the employment of a description of manure in accordance with theoretical, not practical considerations. But it is not anticipated that this result will rapidly follow. On the contrary, the formation of elements necessary for the plant takes place very slowly in the sub-soil, and the deeper the roots extend the longer time is necessary for the re-establishment of those elements. This end cannot be rapidly attained by the application of sulpho-carbonates as recommended by MM. Dumas and Thenard, for the reason that it is impracticable to obtain for whole vineyards a sufficient quantity of water to give effect to a theoretically perfect measure as that is admitted to be. In fact, during the ten years that the latter method already has been in use the disease of the vine has steadily increased year by year. (See also *L'Anneé Scientifique*, 1885.) [The remarks here made on the subject of artificial manures, prepared and applied in accordance with theoretical, rather than practical principles, have an important relation to the rearing of crops other than the vine, and to other countries than France.] Nor is it to be forgotten that the substances required to maintain the vine in healthy and otherwise good condition are numerous among themselves, that the degree to which they are required differs according to species of vine, and that their assimilation depends upon surrounding conditions of climate, season, moisture, soil, &c. By such means parasitic diseases may be averted rather than by the employment of insecticides and the various other means taken against them by the partisans of the "théories Pasturiennes."

According to the author now quoted, the French Government has placed at the disposal of the commission appointed to investigate the subject of vine disease the sum annually of 1,250,000 francs. (La Maladie de la Vigne, par M. Chavée-Leroy.) But up to the end of 1885 the results of all the "scientific" methods devised with a view to check, the prevailing disease have simply amounted to *failure* as very vividly shown in the following extract from the report for that year, namely :—

"It is a remarkable fact in connection with the French vintage, the returns of which have just been published, that, although the quantity of wine made (642,063,375 gallons) last year was 307,642,500 gallons less than the average of the last ten years, the quantity of wine imported into France was rather less than in 1884. At the same time there can be no gainsaying the fact that the situation is a very serious one for French vine-growers; for whereas ten years ago the exports of wine from France were 81,767,500 gallons, and the imports only 6,057,000 gallons, the exports have been gradually declining, being only 55,575,000 gallons; while the imports have risen with giant strides, and were last year 182,587,000 gallons. In other words, while ten years ago fourteen gallons were exported for every gallon imported, now more than three gallons are imported for every one exported. It has, of course, been known for many years that the phylloxera was doing a great deal of mischief; but few people not engaged in the cultivation of the vine, or dependent upon it for their livelihood, were aware of the full extent of the damage. At the present time the phylloxera has attacked the vines in no fewer than fifty-four departments, and upwards of a million acres have been destroyed, notably in the departments of the two Charentes famous for their brandy. The main reason, however, why the vintage of last year was so bad—the smallest, with the exception of 1879, for the last thirty years—was the unfavourable weather; the frosts of April and the great drought in July and August being followed by heavy rains in the early autumn." (*St. James's Gazette*. Jan. 12, 1886).

WINE.

M. Pasteur demonstrated that to avoid the transformation of alcohol into acid it was necessary to destroy the *germs* remaining in wines which are poor in alcohol, by heating them up to 55·60 C. He also demonstrated that it is to the action of oxygen and light in wine that its particular flavour which it acquires by age is due; and further, that the ferment of wine exists on the surface of the grape when it has ripened. (*British Medical Journal*, Dec. 6, 1884).

From the time of Chaptal, who was followed by Liebig and Berzelius every body believed wine to be a liquid in which the various constituents react upon each other mutually and slowly. The wine was thought to be continually "working." Pasteur tried to show that wine does not "work" so much as it was supposed to do. The ageing of wine appeared to consist in the phenomenona of oxidation. In seeking for the actual causes of injurious alterations, Pasteur, always *obedient to a preconceived idea* asked himself whether the diseases of wine did not proceed from organised ferments. This hypothesis was verified. [What *hypothesis* by an experimenter is not "verified" by his own experiments?] The presence of the microscopic fungus, the *mycoderma aceti* in acid wine was known to Chaptal, who believed that it had no influence upon the quality of the liquid. Pasteur's hypothesis was that it is the necessary agent in the condensation of the oxygen of the air, and its fixation on the alcohol of the wine. Besides the *mycoderma aceti* there is also *mycoderma vini.* It is developed by preference in new wine, still immature, and is not hurtful to the old. [Beyond the hypothetical explanation given, nothing is stated in actual explanation of deterioration being due to the fungus in the first instance, any more than the good quality of the wine being due to the species found in it in the second.]

The diseases of wine in casks respectively called *turn*, *rise*, and *spurt*, are considered by M. Pasteur to be due to the presence of little filaments of extreme tenuity. This "ferment" generates carbonic acid; so that the disease named is nothing but fermentation. The remedy he found for this disease is equally applicable to other diseases of wines, such as that of bitterness or *greasings* (*maladie de la graisse*), the latter being also produced by a *special ferment.* [These different "ferments" must in that case be all present together in wine.] In fact, according to him, the deterioration of wines should not be attributed to a natural "working" of the constituents of wine, but to the development of microscopic organisms from pre-existing germs. [In other words, these organisms are the primary cause, not themselves the effect of a cause, as held by opponents of M. Pasteur's *hypothesis* on this point.]

He soon ascertained that to secure wine from all ulterior changes it sufficed to raise it, in some instances, for a few moments only to a temperature of from 55° to 60° (R.) equal to 140° F, ; accordingly, in 1858, 1862, and 1863, he prosecuted his experiments on this subject. But the idea of heating wines does not belong to Pasteur. [So stated by his own Biographer, p. 121.] That method is to be found described in the works of Latin agriculturists. Nearer our own time, Appert by this means, added to a long sea voyage, secured for the wines of St. Domingo their high reputation at the time ; and to his observations, the studies of M. Pasteur led him to refer. [It is honorable to M. Pasteur to acknowledge his obligations to M. Francois Appert. But the re-introduction of the process devised by him was due, rather to literary investigations than to "experimental" research.] A Burgundian wine grower, M. de Vergnette, has also devised the congealing, and the heating of wines as means of preservation ; and on this ground claimed for himself a part of the "invention" of Pasteur's process. According to M. Vergnette it was to the composition of the wine, its robust condition, and good constitution that it owed its power of supporting the heating process. [He evidently was an unbeliever in M. Pasteur's *hypothesis* in regard to the necessary action of fungi as "ferments."] (*M. Pasteur, Histoire d'un Savant*, 1884, p. 145, et seq.).

M. Chavée-Leroy expressed himself to this effect with reference to the "experiments" in question :—M. Pasteur devoted himself to the study of diseases of wine ; and, without concerning himself the least with a consideration of the elements required by the vine, so that it may produce good wine, he indicated the heating of that precious liquid as a certain means of preserving it. That proceeding, put in practice, made much noise ; but it had to be abandoned, having resulted in the ruin of a great many persons, who, too confidently had operated, according to that method, on a great scale. (*Le Journal Medicale, January* 14, 1886).

Thus, according to evidence adduced on the subject, a method of preserving wines by heating them, introduced prior to the time of M. Pasteur, and successful when practiced on a small scale, became, when applied as recommended by him on the large scale, unsuccesful itself, and ruinous to the vine manufacturers who placed confidence in it.

BEER.

M. Pasteur demonstrated that brewers employ generally a ferment containing injurious germs. He indicated a method for obtaining a pure ferment (by means of a very complicated apparatus). The æration (in tubes connected with that apparatus) is sufficient, for the carbonic oxide being heavier than air, the two tubes are so placed in such a way as to form a syphon ; moreover, the wort is kept in movement by the ebulition of the gas which escapes. By employing this procedure, secondary fermentations are no longer to be feared, and the spoiling of beer by secondary fermentation is also entirely put an end to. (*British Medical Journal*, 6th December, 1884).

Beer is more liable to contract diseases than wine. Old wine is often to be met with, but there is no such thing as old beer. According to M. Pasteur, all the diseases of beer are caused by microscopic fungi, or organised ferments, the germs of which are brought by the air in which they are constantly floating. But, inasmuch as beer is charged with carbonic acid, the employment of heat to considerable masses of the liquid, after the manner adopted with wine, would necessarily expel that gas. But this difficulty does not arise when the beer is bottled. Accordingly, Pasteur recommended that it should, after bottling, be exposed to a temperature of 50° to 60° C. ; and this method has been adopted largely in Europe and America. M. Pasteur, with a view to prevent the introduction of the " ferments " by the air with the beer, had an apparatus constructed so as to protect the wort while it was cooling from the organisms of the air, and to ferment this wort with a leaven as pure as possible. The happy effect of these studies is universally recognised. (Life of Pasteur).

M. Chavée-Leroy dissents from the theory of M. Pasteur that fermentation of beer is primarily due to the presence of "germs." According to him it is due to the action of oxygen, and not to the action of organic ferments. He disputes, one by one, the views expressed by M. Pasteur, and as a resumé of that criticism observes that :—" The whole, rational conclusion to draw from the experiments of that *savant* is, that the wort of beer, dis-oxygenated, exposed to dis-oxygenated air, will not ferment ; and, consequently, the fermentation element of the wort of beer is simply the oxygen." Consequently, that the theory of M. Pasteur being destroyed, it follows that he has only seen one side of the question. (*Etudes sur Le Vin,* 1881, p. 50.) M. Pasteur extolled his new method of "fabrication" of beer, which has only one fault—that it is impracticable. (*Le Journal Medicale,* 14th January, 1886).

What, then, are the total results of the "scientific experiments" above alluded to? In the first place, that a new *theory* of fermentation is enunciated by the scientist, but is opposed by the practical man. In the second place, that a procedure, based upon a contested theory, is declared to be impracticable in application.

APPENDIX.

Extracts from L'Indépendant du Midi, dated Nimes, 20th March, 1886 :—

The information given below reached me too late to be embodied in the text of my present pamphlet ; but, taking into account the importance of the protests embodied in those extracts, I have decided to add the chief points in the form subjoined, and as the original stands in the journal from which they are taken. In this way the reader will be best able to compare them with the remarks on the corresponding points in the body of this brôchure, and thus to judge of the relations they bear to each other.

The following is a protest against the "experiments" in connection with wine, silkworm disease, and "inoculations" generally, namely :—

> "Messieurs les députés,
> Si vous avez bien voulu lire les nombreux documents que j'ai eu l'honneur de vous adresser, contre les prétendues découvertes et remèdes de M. Pasteur, vous ne serez pas étonnés que je vienne encore protester contre le projet de lui accorder une subvention pour l'aider à créer un nouvel établissement destiné à exploiter de plus belle la crédulité publique.
>
> Comme tant d'autres, je crois avoir suffisamment prouvé que la science de M. Pasteur n'est pas même digne de figurer devant le tribunal du simple bon sens, et les résultats l'ont prouvé mieux que personne.
>
> *Le chauffage* des vins a ruiné tous ceux qui ont voulu l'entrependre sérieusement. (See *Ante*, p. 134).
>
> *La sériculture*, qu'il a prétendu avoir sauvée, est aujourd'hui une industrie à peu près perdue. (See *Ante*, p. 128).
>
> *Les inoculations* pour préserver contre l'inflammation de la rate (charbon) les beufs et moutons, et contre le rouget des porcs, ont fait périr un grand nombre d'animaux sans en sauver aucun, résultats constatés par les expériences et rapports des sociétés d'agricultures de Vaucluse, de la Haute-Garonne, de l'Ariège, du Gers, du Lot, et autres sociétés qui ont à leur tête des hommes indépendants. (See *Ante*, pp. 42—52).
>
> Les prescriptions de M. Pasteur contre le cholera, dont il aurait du continuer à s'occuper, n'ont eu d'autre résultat que la mort du malheureux Thuillier, qu'il avait envoyé en Egypte, alors que son devoir était d'y aller lui-même. On a dit : c'était trop loin. Copenhague aussi était loin,—ce qui ne l'a pas empêché d'y aller banqueter—Mais Marseille, Toulon et Nimes, n'étaient pas loin. (See *Ante*, pp. 53—54).

Quant à la rage, il existe partout en France et probablement à l'étranger, des rebouteurs qui possèdent chacun un remède secret, qui n'a jamais manqué son effet sur les gens mordus par des chiens plus ou moins enragés. J'en ai vu mille exemples, sans un seul insuccès. Remèdes infaillibles à petite distance et à bas prix. (See *Ante*, p. 65).

Par la savante note du docteur Lorinser, parue dans l'*Indépendant du Midi* du 27 février, que j'ai eu l'honneur d'adresser à tous les députés, vous avez pu voir que l'inoculation après morsure d'un virus plus ou moins rabique était une simple absurdité ; autant vaudrait se faire mordre une seconde fois, si c'était possible, le virus ne risquerait pas alors d'être éventé. Or si en grammaire deux negations valent une affirmation, en médecine on n'a pas encore admis, malgré les homœopates et leur nouveau chef, que deux causes de maladies valent une cause de santé.

Comme pour les vers à soie et tout ce dont il s'est occupé sans le connaitre, les prétendues guérisons de gens mordus ne sont que des tours de passe-passe, dans lesquels, comme pour la réclame, M. Pasteur est d'une habiletë que tient du génie. (See *Ante*, pp. 61—102).

Si ce *médecin sans diplôme* et sans étude qui fait faire ses semblants d'expériences par des aides et aux frais de l'Etat qui l'a accablé de subventions, de décorations, de pensions, et l'a laissé dépeupler toutes ses bergeries ; si M. Pasteur, dis-je, voulait être un bienfaiteur et non un exploiteur de l'humanité, au lieu de chercher a créer, toujours aux frais du public, un deuxiéme établissement privé pour y exploiter un nouveau remède secret, contrairement à toutes les lois, il aurait divulgué le secret de ses préparations et tout le monde aurait profité de ses découvertes (si découverte il y a) avec d'autant plus de droits que les contribuables en ont déjà payé chèrement les frais.

Je n'ai aucune animosité personelle contre "l'illustre académicien" et c'est par pur patriotisme que je le combat car je suis convaincu que rien n'est dangereux pour une nation comme le triomphe des charlatans de haute volée, qu'ils appartiennent à la science ou à l'économie politique, attendu que si ces derniers, comme je l'ai dit, conduisent à la ruine matérielle qui est difficilement réparable, les premiers conduisent à la ruine morale qui est irréparable.

L'ancienne Chambre dans sa dernière année, a voté 80,000 francs pour l'erection d'un chenil monumental ? ? ?......

Mieux inspirés donnez, l'argent aujourd'hui demandé, aux ouvriers sans travail, dont le nombre et la dètresse vont chaque jour croissant, et laissez le millionnaire Pasteur créer à ses frais sa nouvelle usine, il n'a déjà que trop coûté d'argent à la France, sans autre profit qu'une immense impulsion donnée a l'art de la réclame.

Et les mordus, me direz-vous ? Ils continueront à se faire guérir, sans frais de voyages, à plus bas prix et plus sûrement par les *rebouteurs du cru* qui, de temps immémorial, out fait leurs preuves.

(Signed) Eug. De Nasquard.

Vieux sériculteur hygiéniste, membre des sociétés d'agriculture du Gard, de Vaucluse, de la Haute-Garonne, de l'Ardèche, des agriculteurs de France, etc."

Following the above there comes an appeal which I also append precisely as originally published, namely :—

"AU CONSEIL MUNICIPAL DE PARIS.

On lit dans l' *Ami du peuple :*

I.

Nous appelons l'attention de toutes les sociétés savantes de la France sur l'attitude que vient de prendre le Conseil municipal de Paris, à propos de l'affolement dans lequel les actes de M. Pasteur ont jeté une partie de la presse politique et scientifique de la grande capitale. Nous constatons avec plaisir, en l'honneur de la science positive, que la dernière communication — prèvue depuis longtemps—du chimiste français sur la *Rage* a été accueillie par tous les savants étrangers d'Europe avec le plus *dédaigneux mépris.* Ce personnage a perdu tout crédit en dehors de la France. Il est temps que les Français eux-mêmes mettent un frein à cette comédie Pasteurienne, indigne de notre temps et d'un peuple civilisé.

II.

M. Strauss, un compère de Pasteur a eu la fantaisie de saisir le *Conseil municipal de Paris* (*séance du* 5 *Mars* 1886) d'une proposition d'intervention dans la création l'usine Pasteur pour la fabrication de la rage. La demande d'urgence a été repoussée, par suite d'une protestation de M. Paul Viguier.

M. Strauss, lui ayant objecté l'avis unanime de l'Académie des sciences, où les médecins et les biologists sont clair-semés dans le groupe de physiciens, de chimistes et d'astronomes qui en font la glorie, M. Paul Viguier lui a rèpondu avec un grand sens : " il y a ceux qui s'emballent et ceux qui étudient les choses : Je suis de ces derniers."

Cette judicieuse remarque a produit bon effet : la proposition-Strauss a èté renvoyée à la 8e section.

Parmi les signataires de cette malencontreuse proposition figurait le brave Robinet, excellent homme qui pourrait s'alimenter à une source plus pure que celle de la rue d'Ulm.

III.

M. Strauss et les compères de Pasteur qui l'appuient ayant fait diligence pour arriver à enlever un vote favorable, M. Paul Viguier a déposé la proposition sui-vante :

" Les soussignés,

" Considérant qu'en l'absence d'une résolution votée par l'Académie de mèdecine, concernant les demonstrations obtenues relativement au traitement prophylactique de la rage, il importe au Conseil d'apprécier en connaissance de cause les conditions dans lesquelles on lui propose d'intervenir dans la fondation de l'etablissement projeté par M. Pasteur.

"Demandent que le rapport de la 8e Commission soit imprimé et distribué au Conseil 24 heures au moins avant la discussion à intervenir.

"Signeé: Paul Viguier, Humbert, De Bouteiller, Chassaing, Mesureur, Patenne."

L'affaire en est là. On se demande si le Conseil municipal de Paris (nous ajouterons; et la Chambre des députés) suivra, *dans une question de medecine pure* le chauvinisme incompétent de l'Académie des sciences (astronomès, etc.) ou s'en rapportera aux éminents professeurs de la Faculté: M. Péter et consorts?

Les médecins ci-désignés: Dr. A. Oidtmann Maestrick (Hollande), Dr. A. Vogt, professeur d'Hygiéne à Berne (Suisse), Dr. Emery-Coderre, professeur d'Hygiène à Montréal (Canada), Dr. Wilder, professeur de physiologie à New York, Dr. S. Wonner, consul général de Colombie, à Montévideo, Dr. P. Dutrieux, à Paris, Dr. Pigeon, Fourchambauld, Dr. Ancellon, Nancy, H. Bonnewyn, membre de l'Académie de médecine, à Bruxelles, Dr. H. Oidtmann, à Linnich, Dr. Haughton, à Londres, et 98 autres praticiens et professeurs de médecine de la France et de l'Etranger, *adherent à la protestation de MM. P. Viguier, Humbert, etc.*

(Signed) HUBERT BOENS,
Dr. en sciences et médecine."

If is only necessary to observe, with reference to the preceding articles, that the fact is made definite and clear by them that, not only are the proceedings described altogether disbelieved in by an importaut section of completely capable men in France, but that a reaction and protest are rapidly gaining strength against those proceedings, among M. Pasteur's own fellow-countrymen.

SUBSEQUENT REFERENCES TO RABIES AND HYDROPHOBIA.

So profuse have been the references made by correspondents of daily newspapers, and others, to the subject of Inoculation of animals and human beings for rabies and hydrophobia, that a not inconsiderable literature has accumulated since the text of this brôchure was struck off by the printer. Naturally my desire is, having taken that subject in hand, to continue my investigation of it as fully as possible, and down to the latest date that I can; hence it is that I have selected from such journals as have come within my grasp, the particulars contained in the following paragraphs. How far the supplimental matter thus adduced will tend to inspire confidence or mistrust and doubt in the efficacy of that particular mode of "treatment" as applied to the cases of the diseases in question, must be left to be determined by individual readers —each for himself. I myself have no doubt as to my own interpretation.

LIABILITY TO HYDROPHOBIA.

(*See pp.* 71, 77, 81).

According to statistics given by M. Pasteur, in 1878, out of 103 persons bitten, 24 died by hydrophobia; in 1879, out of 76 bitten, 12 died; in 1880, out of 68 bitten, 5 died; in 1881, out of 156 bitten, 23 died; in 1882, out of 67 bitten, 11 died; and in 1883, out of 45 bitten, 6 died. That is, a general rate of mortality equal to one in six. *Telegraph*, 3rd March, 1886).

The risks of hydrophobia are, however, much greater in cases of bites inflicted by rabid wolves. *Daily News*, 15th March, 1886).

Children suffer more from hydrophobia than adults, and men nine times as much as women. (*Chronicle*, Feb. 20, 1886).

In India cases of hydrophobia are recorded as following the bites of rabid jackals; of the mongoose; the common bazaar dog; the wolf; bears, and pet monkeys. In America, the bite of the skunk is almost always followed by hydrophobia, although the animal be not rabid, but healthy. (*Medical Press and Circular*, 31st March).

In Russia there is a popular superstition that madness, with which an animal is afflicted, exhales itself through the pores of the skin, infects the air, and thus communicates the terrible malady to persons who have been neither bitten nor scratched by the infuriated animal. (*Evening Standard*, 17th March.) [The obvious significance of the "superstition" here alluded to, is, that in Russia, as elsewhere (see *ante* pp. 79-81) cases of *spontaneous* hydrophobia do from time to time occur.]

FICTITIOUS HYDROPHOBIA.

(*See pp.* 68, 70).

Many persons believe, without any good reason, that they have been bitten by rabid dogs. They are scared by the orders issued with regard to the muzzling of dogs. From various causes dogs snap without being rabid, No doubt many of the patients under M. Pasteur's care have never been bitten by truly rabid dogs. (*Evening Standard*, 25th March.)

Terror of rabies often broke down the nervous system, and induced symptoms mistaken sometimes for rabies—hydrophobia. (*Daily News*, 20th March, 1886).

In his proposed Institute it is the intention of M. Pasteur to devote laboratories to experiments as to the efficacy of the treatment (by inoculation) in *nervous maladies resembling hydrophobia.* (*Times*, March 8, 1886.) [The distinction is not clear as to the distinction implied between nervous maladies *resembling* hydrophobia, and hydrophobia itself.]

CONDITION OF ANIMALS INOCULATED.

(*See ante p.* 90).

The operation of trepanning the rabbits (in M. Pasteur's laboratory) and injecting them with virus is a painful one to witness. The rabbits, before trepanning, are put under chloroform. The first animal operated upon had its head clipped bare to the bone, and was then placed upon the trepanning board, its forepaws and legs being strapped to the table. A small bag-shaped piece of white blotting paper, soaked in chloroform, was placed over the animal's head and well against its nose. The skull was then incised, and the virus injected near the brain. The animal struggled slightly, and heaved, but the chloroform soon made it completely insensible and dazed. In the meantime its companion came near the sufferer and licked its sides pitifully, and as if filled with sympathy. The operation finished, the poor animal presented a hideous spectacle, with the ugly red gash in its skull, and its eyes heavy and dull from the effects of the chloroform. The other rabbit was then subjected to the same process. (*Telegraph*, 13th March).

Any visitor to the laboratory of M. Pasteur, in the Rue d' Ulm, would see in the cellars hundreds of rabbits that have been inoculated, which, nevertheless, seem to be as lively and merry as they ever were. (*Telegraph*, 20th March).

[Which of these descriptions bears on its face the greater look of probability ?.]

TREATMENT.

(*See pp.* 66, 76, 96).

Dr. Patrini, of Sicily, claims to have employed, and with success, the system of inoculation now known as Pasteur's. (*Morning Post*, 26th March, 1886).

The success claimed for the latter is extraordinary. If a person, bitten by a mad dog, be inoculated with attenuated virus within a month he is absolutely secure against hydrophobia. (*id.*, 24th March).

But it is for the (intended) Commission to inquire what is the evidence upon which it is claimed that persons who have been inoculated would have had hydrophobia had they not been so inoculated. (*id.*)

After the death of the Russian peasant, Yakulev, M. Pasteur was not sure that inoculation, sufficient against dog rabies was equally effective against wolf rabies. (*Daily News*, 24th March).

M. Pasteur has, himself, many times stated that his system is impotent against hydrophobia when once declared. (*Pall Mall Gazette* 31st March).

He admits that his treatment is still in the experimental and speculative stage, and that it is not therefore to be established or destroyed by comparative assertion. (*Chronicle*, March 10, 1886).

M. Pasteur asserts that cauterisation is totally inefficient as a preventative of rabies in animals, or hydrophobia in man. (*Standard*, March 31, 1886). [Compare this statement with evidence recorded, *ante* pp. 80, 96.]

An American doctor recommends the administration of Bromide of potassium, followed by a large dose of castor oil, the use of the vapour bath, and, at intervals, chloro-hydrate. (*Evening Standard*, 25th February, 1886).

According to Ruinat, a plant, native of Tong-King, known as Hoang-nan, has been used successfully in France in cases of Hydrophobia since 1882, and has been cultivated there since 1885. (*St. James' Gazette*, March 3).

Dr. Buisson's remedy was communicated by him to the Academy of Arts and Sciences of Paris many years ago, and in 1855 his treatise "Moyen de Prévenir et de Guiérir la Rage" was published. (*Pall Mall Gazette*, 31st March.) [See, on the subject *ante* p. 97.]

Up to 19th March, 1886, five hundred persons had been inoculated at the laboratory in the Rue d' Ulm ; four-fifths of whom were bitten by dogs whose rabid condition was ascertained by post mortem examination. (*Standard*, 19th March.) [*See ante* p. 80, where the quotation occurs that "there are no cadaveric lesions which can be said to characterize hydrophobia" (rabies.) See also remarks made above on *Fictitious Hydrophobia*, *ante*, p., 41.]

PARTICULAR CASES.

(*See pp.* 77, 84.)

Seven persons who were bitten by a dog, that was said to be rabid, were sent from Bradford to Paris to be treated by M. Pasteur. The dog having been killed a *post mortem* examination was made, after which the animal was declared not to have been affected with rabies. (*Chronicle*, March 15, 1886. See *ante*, p. 80.)

Yet it was stated that one of the persons so bitten died of hydrophobia. (*Globe*, 15th March.)

Also, that one of them was bitten in the calf and thigh, through trousers of thick tweed; one through the stockings; one in the eyelid. Now, the probabilities are that, those who were bitten through the trousers and the stocking would not have gone mad. They should not therefore be claimed as being rescued through M. Pasteur's method of treatment. (*Pall Mall Gazette*, 20th March).

M. Pasteur believes that a person who is bitten through a thick stocking is safe from hydrophobia. (*Daily News*, 20th March. *See ante p.* 84).

Adverting to the death by hydrophobia, recorded at page 102 of the text, as having occurred in one of the Russian peasants under treatment in accordance with M. Pasteur's system, the name of the deceased is stated to have been Yukalev. He was one of a party of nineteen persons (or fifteen, according to another account) who had been bitten by a wolf, and who had been sent to Paris, as already stated. But—a doubt was expressed as to whether the animal by which they were bitten was really rabid as no post mortem examination of its body was performed. (*Standard*, 17th March, 1886. See also *ante* pp. 80 and 141).

The doctors, who attended Yukalev, gave out that he was carried away by diptheria. All the attendants at the Hotel Dieu were charged to deny that any death had taken place. (*Daily News*, 24th March).

A dispute had sprung up as to whether he had died from hydrophobia or from his wounds. (*Globe*, 24th March).

The *post mortem examination* in his case showed death, as in cases of snake-bite, to proceed from paralysis of the whole breathing organs. The lungs, chest, and throat region were charged with clotted blood; the tongue and tonsils abnormally swollen; the covering of the spine showed *a tendency* to inflammation, but although the brain was slightly congested it was perfectly healthy, and the meninges normal. (*Daily News*, 25th March, 1886).

The *post mortem examination* shows him to have contracted the malady in as acute a form as it is received by the dogs to whom the virus is communicated by trepanning. (*Standard*, 27th March). [If this be so with regard to hydrophobia induced by a bite received in a fleshy part of the body, it naturally demonstrates the absence of all plea of "necessity" for the method pursued in M. Pasteur's laboratory of introducing the "virus" by means of trepanning the skulls of animals; some at least of those animals themselves insusceptible to the disease. But as stated above, doubt was expressed as to whether the wolf that inflicted the wounds was itself rabid. In fact there is nothing certain, but everything uncertain, with regard to every point adduced, save only that the death of Yukalev was a fact.]

In further reference to the same case the statement occurs, that:—The theory of M. Pasteur's is that until a patient has had the virus injected ten times it has no efficiency whatever, and is virtually useless in preventing hydrophobia. Also, that—the deceased Russian peasant was attacked after the seventh inoculation, that is, before the experiment was completed. But, it is added:—In spite of all, however, it is probable that the fact that he died in M. Pasteur's hands, will shake the belief in the value of his treatment. (*Evening Standard*, 26th March, 1886).

[Taking the evidence above adduced, precisely in effect as it stands in the original statements quoted and referred to, I submit whether it is of a nature to justify any support whatever being adduced from it in favour of a practice such as it is intended to maintain. According to my interpretation of its purport, it is all the other way.]

Nor is this all. A second death among the Russian peasants, undergoing the same method of "treatment" in Paris, was reported on 3rd April, 1884. (*St. James' Gazette*).

With regard to that method, generally:—There is a good deal of scepticism. (*Globe*, 15th March.) No resolution on the subject was adopted by the Academy of Medicine in Paris. (*Telegraph*, 15th March.) The Sanitary Council of Austria has advised Government not to send an official delegate, to Paris, to study M. Pasteur's system. (*Globe*, 2nd April, 1886.) See also *ante* p. 102.

Another of M. Pasteur's Russian patients, Ivanoff, died. It is to be hoped that this sad event will put an end to the campaign in favour of obligatory canine inoculation. (*Daily News*, 5th April, 1886).

While in the first fatal case of hydrophobia (that of Yukalev), that disease appeared before the patient had undergone all the prescribed inoculations; the entire treatment had been applied before the man who had just died was attacked. (*Standard*, 5th April, 1886.) [Whence it follows, that the theory of M. Pasteur as implied above, namely, that inoculation, ten times with the "virus" of rabies, confers immunity, was by the present case shown to be without foundation in fact.]

According to M. Rochefort, who entertains an unfavourable opinion of M. Pasteur and his theories, this fatal case, coupled with the death of the other Russian patient, goes to prove that the inoculations made, kill patients instead of preventing the appearance of hydrophobia. (*Evening Standard*, 5th April, 1886).

Another of the first batch of M. Pasteur's patients died on 7th April. This is the third death. He was named Golowvinski. He was taken with what resembled a violent fit of asthma. He died of paralysis of the lungs [?]. (*Daily News*, 8th April, 1886).

M. Pasteur explains that this death (of Golowvinski) arises from the bites having been so severe that the virus became infiltrated immediately into the "sublingual maxillary" glands. (*Chronicle*, 8th April, 1886). [The precise means by which he arrived at that conclusion are not stated.]

M. Pasteur declared that, in his opinion, the deaths which had occurred proved nothing against the efficacy of the prophylactic method. (*Telegraph*, 8th April 1886.) [Opinions expressed on the same point by others than M. Pasteur, as above recorded, were to an effect very different from his.]

Taking, therefore, the particulars above related, the natural and obvious conclusion to be drawn from them is, that "inoculation" neither averted nor mitigated fatal disease in the subjects of that operation whose names are recorded.

HYDROPHOBIA IN RELATION TO DIPHTHERIA, &c.

(*See ante p.* 59.)

In connection with the question of establishing an "Institute" in Paris for the treatment of hydrophobia, and "affections resembling" that disease, the remark occurs, that the data, M. Pasteur, has discovered in relation to hydrophobia must also have some bearing on other diseases. It is a question, now under discussion, whether diphtheria may not be successfully treated on principles based on those which M. Pasteur has already exposed. (*British Medical Journal*, 6th March, 1886).

[The question yet remains to be definitely and categorically answered:—What are the data, in relation to hydrophobia, which have been discovered by the method named after M. Pasteur? The further question—How can data which are in this position be applied to, or have any bearing upon, any other diseases? On the subject of Diphtheria see *ante* p. 59. On that of Zymotic Inoculation generally, pp. 35 to 60.]

CAN RABIES BE COMMUNICATED BY MAN TO ANIMALS?

Dr. Dolan writes to the effect as follows:—M. Pasteur has determined to try if rabies can be given by man to animals. This has long since been settled. There is conclusive evidence on the communicability of rabies from the human species to the lower animals. In 1823 Majendie and Breschet inoculated two healthy dogs with saliva from the mouth of a man who was dying of rabies. One of these animals became rabid 38 days afterwards. Earle, of St. George's, London, inoculated several rabbits with the saliva of a patient affected with rabies. Some of these became rabid. [Compare this with the evidence given at page 90, and note the divergence of results recorded.] Hertway and Renault also proved experimentally that the disease could be transmitted from mankind to the canine species (Rabies, or second edition—Hydrophobia, p. 120). Dr. Dolan confirmed this result with some saliva obtained from a case of hydrophobia which occurred at Keighley. Of two rabbits one died with unmistakeable symptoms of rabies. If the report be true M. Pasteur, in verifying the experiment of MM. Magendie and Breschet, will not establish a new point (*Medical Press and Circular*, April 7, 1886). [These remarks should also be read in connection with those at page 90, already referred to. They touch upon the questions raised as to the specific character of the "virus" of rabies and hydrophobia (pp. 66, 77, 92); as to the results from inoculation of saliva recorded at p. 67; as to whether, in order to cause hydrophobia in a person bitten, it is necessary that the animal that inflicted the bite should itself be rabid (pp. 74 to 82). If this be carefully done it seems to me that the general impression left upon the mind of the reader will be that the discrepancies with regard to these points are equally marked, as are those which are noticed in more detail in the text in relation to the general subject of rabies and hydrophobia.]

REMARKS ON THE REPORT OF M. PASTEUR'S RESEARCHES ON RABIES, BY M. WILLIAM VIGNAL, PUBLISHED IN THE "BRITISH MEDICAL JOURNAL," 10TH APRIL, 1886.

The writer of the report above named, adopts the most excellent method of giving, in conclusion, a " Summary " of observations, details regarding which he has in the body of the document more fully discussed. In the quotations from other observers, which form the bulk of the article on Rabies in this brôchure ; there are numerous references to the same researches as are alluded to by M. Vignal, so that the report and that article, if read together, will mutually elucidate each other. As however it is desirable in bringing this work to a conclusion, to refer to the "Summary" given by M. Vignal, I propose in the remarks which follow to quote the purport of the several heads into which he divides it, and in reference thereto to add such remarks as present themselves. The quotations from the " Summary " are given within inverted commas (" ") ; the remarks with reference to them within brackets [ ], namely :—

1. " Saliva and salivary glands of animals, as also the peripheral and central nervous systems are capable of transmitting rabies."

" M. Pasteur having inoculated rabbits with saliva from a child dead by hydrophobia, was not certain whether the condition called into existence by his inoculations was identical with hydrophobia. He seriously doubted that it was so. M. Galtier had not observed the lesions at the necropsies of the animals, that M. Pasteur had ascertained to exist in his. Dogs inoculated with the blood or saliva taken from M. Pasteur's rabbits did not exhibit either dumb or furious madness. M. Pasteur stated that the malady he produced in rabbits by inoculating them with saliva from a hydrophobic child was "a new disease." M. Vulpian had killed rabbits by injecting saliva from healthy people. The blood of rabid animals failed to communicate hydrophobia.

M. Galtier made more than ten inoculations with fragments of cerebellum and spinal cord removed from mad dogs, all of which were unsuccessful. M. Pasteur stated that by similar means as well as by inoculating with the cerebro-spinal fluid of animals dead from hydrophobia, he had succeeded in transmitting the disease. The symptoms of hydrophobia are extremely varied. Each separate case has its special symptoms. Hydrophobia communicated by venous injection of the virulent matter is frequently different from the form of hydrophobia resulting from the bite of a mad dog, or from cerebral inoculation. M. Pasteur hesitated to decide between two probabilities; one that the dogs (inoculated) were naturally refractory to the virus of hydrophobia, or that they had acquired immunity from it by the slight attack they had contracted (as a result of the operation.) There are not any dogs which were constitutionally refractory to hydrophobia. The saliva, and the salivary glands are as virulent in dogs seized with hydrophobia from venous or cerebral inoculation as in those attacked with *spontaneous* hydrophobia. M. Pasteur observed that the virus of hydrophobia is not always present in the cerebro-spinal fluid. M. Pasteur inoculated a rabbit in the auricular vein, then severed the ear with the thermo-cautery just below the inoculation point; nevertheless the animal contracted hydrophobia." (From *M. Vignal's* own *Report*).

[Thus according to the statements of M. Vignal himself, we gather that the effects of inoculation of certain animals with rabid or hydrophobic "virus"—in other words, with matters taken from those dead by that disease has been followed, not by hydrophobia, but by a "new disease"; that in certain instances death has followed inoculation with saliva of healthy persons; that in other hands than those of M. Pasteur inoculations with fragments of the spinal cord was not followed by rabies; that the disease produced by the injection of rabic matter is often different from that which follows a bite of a rabid dog; that the "virus" is not always present in the cerebro-spinal fluid; that the symptoms of hydrophobia vary to such a degree that each case has its own characteristics. The disease so named may arise *spontaneously*. See *ante* pp. 77, 79, 84.]

As therefore the results which follow inoculation with rabic matters are shown as above to be varied and non-specific in character, and the grounds are unrecorded upon which specific character is assigned to the matters of rabid animals so enumerated, it does not appear that the term "virus" is applicable to those matters in a definite and particular sense.

As so great a variety as that above recorded takes place in the characters of the disease which follows inoculation with rabic matters, neither is any ground shown for considering that disease to be specific in nature, in the sense in which small pox, for example, following specific inoculation is itself specific. The various other points touched upon by M. Vignal may be compared with what has already been advanced in the article on Rabies and Hydrophobia in the text. [pp. 61, 140.]

2. " This virulence is maintained during several days by observing a temperature varying from 0° to 12° Cent. (32° to 53.6° F), and avoiding decomposition."

[That is to say, the virulence, not of a definite and specific " virus " as that term is ordinarily understood, but of liquids and tissues of animals dead of a disease in which as shown in par. 1, there was no constancy or certainty in its symptoms, manifested in different animals. How then, to phenomena so various, the specific term *rabies* could with propriety and exactitude be applied does not appear by the report before us.]

3. " There is more certainty attached to inoculation for rabies with nervous substance than with saliva or salivary glands, because rabid virus can be obtained from the nervous system free from foreign micro-organisms."

[Let us refer to what is recorded on this point under paragraph 1 :— M. Galtier made certain inoculations with fragments of the cerebellum and spinal marrow, but as regards production of hydrophobia " unsuccessfully." Note also that inoculation with rabic saliva produced " a new disease " (therefore *not* hydrophobia). Nor is the " virus " of hydrophobia always present in the cerebro-spinal fluids. Therefore the obvious conclusion to be drawn is, that neither is inoculation with nervous matter constant in regard to the phenomena which succeed to that operation, nor in regard to its own " virulence." *Ante*, pp. 144, 145.]

[On the subject of micro-organisms see p. 90 of the text.]

4. " Inoculating the cerebral surface after trephining, also inoculating by venous injection is attended by more certain results than by introducing the virus in the subcutaneous areolar tissue."

[Hydrophobia was said to have invariably followed; not one inoculation failed. Sometimes dumb madness—sometimes furious—was developed. But in the record before us no statement occurs indicative of any difference that exists (if any does exist) between the symptoms which follow corresponding injuries of the skull and encephalon, but without any injection whatever taking place. See also p. 90, 92 of the text, wherein statements occur adverse to the existence of any such difference.]

5. " The form of rabies developed by inoculating the cerebral surface is generally that of furious madness (*rage furieuse*). Furious madness, paralytic madness, or dumb madness can be produced by venous injections of smaller or larger quantities of virus."

[These statements are confirmatory of the observations recorded under par. 1 in regard to the difference in the phenomena which follow such inoculation, a difference so great as to lead even M. Pasteur to doubt whether the disease produced was really hydrophobia ; the symptoms also so varied that "each separate case had its special symptoms. In fact, taking those statements as they stand, their obvious purport is to cast doubt upon there being anything at all of the nature of hydrophobia or in other respect truly "specific" in the character of the resulting disease.]

6. "An animal is not rendered exempt from hydrophobia after inoculation with too small a quantity of virus to produce the disease ; inoculation with a stronger dose will produce the malady."

[We have already seen that, according to the showing of M. Vignal in the report under notice, neither is there any property in the matters injected to which the term " specific virus" can with propriety be applied, nor is there anything constant and definitely specific in the symptoms which follow inoculation. This being the case, it does not appear in what manner the expression " the malady" becomes, in a definite sense, applicable in a series of cases which in character generally differ from each other, the " cause" of morbid phenomena in all being itself morbid in origin but in no other respect specific. In fact, the closer the several statements are examined and compared with each other the more indefinite becomes the significance to be attached to them.]

21 *April*, 1886.

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