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6

HYDROPHOBIA.

M. PASTEUR AND HIS METHODS:

A CRITICAL ANALYSIS.

BY

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AUTHOR OF "THE NATURE AND TREATMENT OF RABIES OR HYDROPHOBIA," ETC.



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HYDROPHOBIA

AT PASTURE AND THE MOUNTAINS

A NOVEL

CONTENTS.



PART I.

	PAGE
The study of Hydrophobia entered on a new phase in 1880.— First experiments of M. Pasteur.—A special microbe and a new disease.—Not substantiated.—The same disease described in 1844 by Samuel Wright, M.D.—Experiments of Professor Rey, of Lyons.—Communication of M. Pasteur to the French Academy of Science, 1881.—Previous experi- ments by Eckel, Röhl, Lafosse, Haubner, Rossi, Hertwig.— Theory of M. Duboué.—Communication of M. Pasteur to the French Academy, December 12th, 1882.—Summary of M. Pasteur's views by M. Vignal.—Conclusions of M. Duboué.—Original nature of M. Pasteur's experiments .	5

PART II.

Address at International Congress, Copenhagen, August 17th, 1884.—Virulence of virus weakened by passage through monkeys.—Increased through rabbits and guinea-pigs. —Initial inoculation on dogs.—Report of Commission. —Further experiments and methods of affecting the virus. —Communication to the Academy of Sciences, October 26th, 1885.—The boy Meister.—Details of experiment.— Communication 2nd March, 1886.—350 persons bitten under treatment.—Communication 12th April.—Number reached 688 bitten by dogs, and 38 by wolves.—Case of Dr. J. Hughes.—Certificate from R. Hughes, veterinary surgeon. —Death of Louise Pelletier.—Explanation of M. Pasteur.— Is it true that deaths are registered as meningitis which are really due to rabies?—Statistics.—Establishment of an <i>Institut Pasteur</i> .—Wolf-bites.— Since discovery of M. Pasteur enormous number of people bitten by rabid dogs .	18
-------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------	----

PART III.

	PAGE
The nature of the poison of Hydrophobia yet unknown.— The great point to decide.—Opinion of Jesse Foote.— Statistics on the proportion of Rabies in the dog, and Hydrophobia in man.—Experiments of Hertwig.—The Newark cases.—Work of the Commission.—The difficulties in the way.—A reduction of the time in which patients should come under treatment.—The deaths of the Russians. —An argument against centralisation.—The great value of M. Pasteur's work.—Something more required.—Mortality from Hydrophobia in England.—Evidence from the Hague. —A new Dog Act.—Evidence of value of police measures by Drs. Frisch and Bollinger	35

THE PREVENTIVE MEASURES OF M. PASTEUR.

I.

SINCE 1880 hydrophobia as a study has entered on a new phase. It then attracted the attention of one of the greatest chemists of the age, and we are told, that M. Pasteur's attention was drawn to it by the mysterious nature of its incubation.

As we unfold the history of M. Pasteur's researches on rabies, and give his experiences, we shall be compelled, I think, to admit that however mysterious the incubation of rabies has seemed to be in the past, it is rendered more mysterious still by the newer light thrown on it, and the problem is removed even a step further, as to the nature of the virus. As we shall presently see, the poison or virus admits of the greatest change under conditions of heat or cold, according to the media in which it is placed; and still more, that it is capable of various alterations as it is passed through certain animals, as monkeys, rabbits, guinea-pigs, etc. Eluding all grasp, the poison only makes itself known by its manifestations. Its true nature is unknown, and its *modus operandi* is

also unknown. Verily hydrophobia is a complex study, after the lapse of centuries furnishing for us fresh problems. By a grand generalisation M. Pasteur has made a *coup* by which he has astonished the world. After a few years' study, we are assured by him that hydrophobia has been brought under subjection. Knowing the history of previous failures, it is necessary, in the interests of science, to examine this claim, and to submit it to rigid scrutiny, and to apply the touch-stone of analysis and examination, just as is always done with all new discoveries. If M. Pasteur's conclusions be correct, they will stand the test, and should they be true, they will shine all the brighter after having passed through the furnace of criticism.

The story of Pasteur's first studies in hydrophobia is told in a very interesting volume called "Louis Pasteur: his Life and Labours." This work is written by M. Pasteur's son-in-law, and as the proof sheets were no doubt revised or submitted to M. Pasteur, we may accept this account, so far as it goes, as being substantially accurate. We learn, then, that on December 10th, 1880, a child of five years old, who had been bitten in the face about a month previously, was dying in the Trousseau Hospital. Dr. Lannelongue introduced Pasteur to his first case of hydrophobia. Four hours after his death the mucus of the palate of the child was collected, and two rabbits were

inoculated under the skin of the abdomen. The rabbits perished in less than thirty-six hours. The saliva of these dead rabbits also transmitted the disease to fresh rabbits. Did it not seem as if one had got hold of an inoculation of hydrophobia? Such was, in fact, the conclusion of Dr. Maurice Raynaud, who having been informed, at the same time as Pasteur, of the illness of the child, had made on his own account some experiments on rabbits. His rabbits were dead. Already a year previously M. Maurice Raynaud had announced the transmission, by the saliva, of rabies* from man to rabbits. "We are, then, in presence of a new fact of this kind," he said, "and we really believe, until a proof to the contrary is given us, that these latter rabbits died of hydrophobia."

With his usual prudence, and trusting more to the results of experiments than to medical observation alone, Pasteur was not in a hurry to form such positive conclusions. He began by doing what Dr. Maurice Raynaud had neglected to do. He examined with the microscope the tissues and the blood of the rabbits inoculated in the laboratory; he discovered, both in those that were dead, and in those which were on the point of death, the presence of a *special microbe*, easily cultivable in a pure state, and of which

* The term *rabies* is applied to the disease as it exists in the dog and other animals, *hydrophobia* to the disease in man. The latter name is a misnomer, but custom has now sanctioned it.

the successive cultures caused the death of other rabbits. Invariably the same microbe appeared in the blood. As one or two days sufficed to cause death, hydrophobia could not have had time to make its appearance. Pasteur, moreover, found this same microbe in the saliva of children who had died of common maladies, and even in the normal saliva of healthy adults. It was a new microbe, causing a disease unknown up to that time. To Pasteur it seemed, in the case of the experiments made with the mucus from the child's palate, to be simply an accompaniment of the rabid virus.

This *microbe* of the saliva is very easily cultivated on sterile infusions,—that of veal, for example,—and successive cultures can be made in the usual way.

1. This may be considered M. Pasteur's first stage in his hydrophobic studies, and he communicated a note thereon to the Academy of Sciences, entitled "On a New Disease produced by the Saliva of a Child dead from Hydrophobia." This initial communication of M. Pasteur was hardly a happy one, as I shall attempt to show, apart altogether from the criticism to which it was subjected by MM. Colin, Galtier, and others.

M. Pasteur did not describe a *new* disease, nor was he correct in assuming that he had discovered a new microbe. M. Pasteur has himself since admitted that this microbe is found in the buccal

cavity of healthy human beings, and M. Vulpian also established, that the saliva of healthy persons can kill rabbits, the same organism being found. This fact was established in 1844 in England by Samuel Wright, Esq., M.D., Physician to the Birmingham General Dispensary.* Dr. Wright's papers "On the Physiology and Pathology of the Saliva" are well worth reading by all interested in hydrophobia. Dr. Wright says: "Healthy saliva, when injected into the veins, produces symptoms closely resembling, if not identical with, those of hydrophobia; and if the quantity injected be sufficient, death, apparently hydrophobic, is a consequent result." He also observes (*loc. cit., ante*, p. 123): "Two specimens of my own saliva, which had been reduced to dryness and kept respectively for four and six months, produced, when injected into the carotids, *precisely the same effects* as the recent secretion." In a foot-note Dr. Wright remarks (p. 124, *loc. cit., ante*) "that *animalculæ* are frequently found in saliva, both healthy and morbid. And their casual presence in the saliva of hydrophobic persons is no proof whatever that they have the least chance in contributing to, or transmitting the disease." We assert, then, that M. Pasteur did not describe *une maladie nouvelle*. It is but just to M. Pasteur to point out that, in a letter to the Academy of Medicine, he stated his recognition of the influence of healthy buccal mucus in

* *Lancet*, vol. i., 1844, p. 122.

producing a new (to him) disease. It is also just to Dr. Wright to give him the credit of having discovered this fact long antecedent to M. Pasteur.

We need not say very much on M. Colin's observation, or on his criticism, that the microbe of Pasteur had been described and figured ten years previously, or on the experiments of M. Lannelongue and Raynaud concerning the transmission of hydrophobia to rabbits.

In the history of hydrophobia it is found to be very true, that it is well to know what has been done in connection with this disease by previous observers. We have already given an instance of M. Pasteur's premature conclusions, and we shall have, again and again, to furnish other instances of his difference of opinion, or his disregard of what has been done by other experimenters. For instance, it is not generally known that experiments, long since made, have proved that the virus becomes less active after passing through several organisms. This was established by several experiments made by M. Rey, of Lyons. This veterinarian, by successive inoculations in several different animals with rabid virus obtained from a single primitive source, found that it appeared to act with much less promptitude on the last, than on the first inoculated, and that with sheep it had no effect after the fifth removal, or generation.

The account given in "Louis Pasteur: his Life and Labours," must, we think, be largely discounted.

2. On May 31st, 1881, M. Pasteur made another

communication to the Academy, and this is a very important note ; and before considering it, it is well to bear in mind the very important statements made by previous observers and experimenters.

(a) The blood of animals has been experimented on by Eckel, Lafosse, Röhl, Haubner, etc. Eckel inoculated a dog with the blood of a locksmith affected with hydrophobia. The dog was seized with unmistakable rabies, and died. Lafosse produced the disease called *Rage Tranquille* with blood.

Eckel, Röhl, Haubner, and other distinguished veterinarians are decidedly of opinion that the virus of the disease is present in the blood.*

(b) Rossi produced madness in a dog by inoculating it with a morsel of nerve, and Hertwig has done the same.

(c) M. Duboué started a theory in 1879 (which is really an old one), that the nervous system, and especially the medulla oblongata, were important factors in the development of rabies. In 1877 I restated a similar theory, and laid down the following provisional conclusion : † “ That the phenomena of rabies, as evinced by morbid anatomy, depend on structural alterations in the medulla oblongata and spinal cord influenced by a specific virus, the *modus operandi* of which is still involved in obscurity.”

* Fleming, “ Rabies and Hydrophobia,” p. 125.

† Dolan, “ Rabies or Hydrophobia,” p. 114.

(d) MM. Raynaud and Lannelongue established the virulence of the salivary glands in the communication of a disease which, if not hydrophobia, is very like it.

(e) Hertwig published some accurate experiments (in 1828) in which the blood of mad dogs rubbed into the cuts of healthy dogs caused rabies.

That the phenomena of rabies, as evinced by morbid anatomy, depend on structural alterations in the medulla oblongata and spinal cord, M. Pasteur has since more fully established, though it is hardly correct to draw the conclusion, that he was the first to consider the central nervous system as playing the most important part in the development of the disease.

Bearing the above facts in mind, our readers can themselves form a critical estimate of the following statements concerning the transmission of hydrophobia, as made by M. Pasteur, May 30th, 1881. A brief account of this communication is to be found in Pasteur's life, previously alluded to.

3. On this date M. Pasteur stated at the Academy of Sciences, "that as the blood of rabid animals failed to communicate hydrophobia, he and his assistants MM. Chamberland, Roux, and Thullier, had determined to test the truth of the view expressed by M. Duboué,* in 1879, a view not confirmed by M. Gautier,† of Lyons. M. Pasteur, however,

* *Bulletin de l'Académie de Médecine*, 1879, p. 865.

† *Ibid.*, 1881, 25 Janvier.

obtained more successful results. Pasteur, with due care as to purity, introduced under the skin of some rabbits and some dogs divers parts of the brain of a dog which had died in a rabid state. Rabies declared itself in both dogs and rabbits, with a duration of incubation about equal to that which followed the ordinary bite of a dog. One great result was obtained: rabies *could* be inoculated with other matter than saliva."* M. Pasteur also found that by inoculating rabid matter on the cerebral surface, he secured the appearance of rabies sometimes in the form of dumb madness, sometimes in that of furious, not one inoculation failing.

4. On December 12th, 1882, M. Pasteur contributed an additional note: "Nouveaux Faits pour servir à la Connaissance de la Rage."

In his previous experiments he had injected a virus subcutaneously, but here he makes another departure. Subcutaneous injection produced rabies, but after a comparatively long period. M. Pasteur was anxious to introduce the disease more rapidly, and therefore hit upon the plan of inoculating the virus direct on the brain. This method, according to this note, demonstrated the virulence of the nervous system which he used for inoculation, and furnished an accurate method of inoculation for rabies, shortening the time for the experimenter.

In the *British Medical Journal*, April 10th, 1886,

* "Louis Pasteur: his Life and Labours," p. 295.

M. Vignal has summarised some of M. Pasteur's fresh facts up to that date, as follows :—

1. "*All forms proceed from the same virus—dumb madness and furious madness.*" This is not a new fact, and is known to all familiar with rabies. The simultaneous occurrence of dumb and furious madness is frequently observed in packs of foxhounds.*

2. "*Symptoms are excessively varied.* The symptoms of hydrophobia vary. Each special case may be said to have its special symptoms."

This also is not new, and, as all veterinarians know, it is rare to meet all the signs present in any case. Professor Saint-Cyr has an excellent description of the varied symptoms in the *Journal de Med. Vet. de Lyons*, vol. xxi., p. 74 ; and I may here say he describes how he was bitten on the right hand by a rabid dog without any bad result. The theory put forward by M. Pasteur on the variation of symptoms does not explain anything. The variation of the symptoms, he says, "depends on the regions of the central nervous system in which the virus localises and develops." Here we have the difficulty restated, in another form, on the localisation of the virus.

3. "*The saliva in rabies may provoke death from three different causes—from the figure of 8 microbe, from the formation of a considerable quantity of pus, and from rabies.*" This, we need hardly say, is not new.

4. "*The medulla oblongata, spinal cord, and ence-*

* "Rabies and Hydrophobia," p. 233.

phalon of rabid subjects are virulent. The medulla oblongata is always virulent. The spinal cord is also virulent in parts, and frequently throughout its entire length; the encephalon is virulent, sometimes only in certain parts, sometimes in its entirety." M. Pasteur deserves the credit for herein stating some new facts and working on new lines, though this statement has to be very much modified by another, viz., "that the cord may have virulent properties whilst the medulla is unaffected, the difference being explained by the mode of inoculation, intra-venous injections causing medullary hydrophobia owing to the localisation of the poison in that part." In other words, there is fixation of the poison in the spinal cord.

Here we are placed in the presence of a seeming contradiction, on the fixation of the poison in the spinal cord in one case, when intra-venous injection is used, producing medullary hydrophobia, whilst in a subsequent communication M. Pasteur assures us that the form of hydrophobia, and consequent fixation in cord, or brain, depends more upon the quantity of the virus introduced. As we are here speaking of the fixation of the virus in the nervous system, it seems appropriate to give in full the conclusions of M. Duboué, so as to compare them with Pasteur's theories. M. Duboué* arrives at one primary con-

* "De la Physiologie Pathologique et du Traitement Rationnel de la Rage," par le Docteur Duboué (de Pau). Paris : Adrien Delahaye.

clusion. The agent productive of rabies is not absorbed. It is insensibly propagated along the nervous fibres attacked by the virulent liquid. M. Duboué lays down the following secondary conclusions :—

(1) “The propagation of the rabic virus takes place through the substance of the axile filaments, and corresponding nerve cells.”

(2) “The sensitive nerve fibres are very probably alone affected, to the exclusion of the motor fibres.”

(3) “The morbid agent progresses slowly in a CENTRIPETAL direction from the seat of the bite to the rachidian bulb, and very rapidly in a CENTRIFUGAL direction.”

(4) “The phenomena of rabies commence when the virus arrives at the bulb, and often announce themselves by pain radiating from the nerves on the bitten side.”

(5) “The period of incubation is in proportion to the distance of the bite from the bulb ; thus it is shorter in children than adults, and in bites of the face.” We may leave out Nos. 6, 7, and 8.

(9) “The lesions of rabies are of two kinds: some primary, only visible with the microscope, and consisting of a greater opacity of the nerve cells and of a granular state of these cells, and of a certain number of the afferent or efferent nerve cells; others later, visible to the naked eye, consisting of congestion, more or less marked, of various organs.”

(10) “Once in contact with the nerve cells of the

bulb, the rabid virus is carried rapidly in all directions, following the track of the fibres emanating from the nerve centres."

In Nos. 11, 12, 13, and 14 Dr. Duboué speaks of the buccal fluid, and of the unilateral characteristics of rabies, and of bites from wolves. We again take him up at No. 15.

"The virulence of the buccal fluid persists for twenty-four hours after death."

Dr. Duboué favours the nervous theory, and his indications for treatment consist in destroying the rabic virus *in situ*, diminishing the sensibility of the bulb, etc. M. Duboué ranges himself against

"la theorie sanguine qui a toujours dominé, et domine encore dans l'esprit des mediciens ;"

M. Pasteur being included among the number of those who accept the very old blood-theory, and who, in fact, has gone to the trouble of carrying on experiments to show that the virus does not travel along the peripheral nerves to the central nervous system.

M. Pasteur would seem to agree with M. Duboué in certain particulars.

(1) In the location of the virus in medulla, spinal cord, and nervous system.

(2) In the rapidity of action of the virus according to the seat of the injury. He differs from him on the transmission of the virus by the nerves, favouring the view that it is absorbed by the system.

Finding, then, that the brain substance contained rabic properties, M. Pasteur continued his experiments, which possess an undoubted interest from their originality. He inoculated dogs with diseased brains, and found that the dogs became rabid in an average period of fourteen days. This fact established, he looked out for a method weakening the material, and perhaps if we take M. Pasteur's own description, given at the International Congress, Copenhagen, of the results he obtained, we shall form a better idea of his method.

II.

IN his address at the Congress he first called attention to the fact, that the characteristic changes of the tissues in animals which had died from hydrophobia are often limited to the medulla oblongata, in which organ the poison producing the disease is found most concentrated and pure; secondly, to the fact that inoculation of the poison obtained from the medulla oblongata is not always followed by positive results, unless the poison is introduced into the subarachnoid cavity by means of a trocar, after trepanning the skull has been performed. These two facts have been of vast importance in solving the great difficulty in obtaining the poison sufficiently concentrated for experimental purposes, experience having shown that two dogs, both bitten by the same rabid dog, may take the disease in quite different degrees, and after longer or shorter periods of incubation. Liability to

error may, however, be obviated by pursuing the following method:—Take the medulla oblongata from a dog which has died of rabies, crush and put it into sterilised bouillon, taking all necessary precautions. By introducing two drops of this preparation into the subarachnoid cavity of rabbits by means of a Pravaz syringe, the same results will always be produced; the rabbits will all develop hydrophobia within twelve to fifteen days, neither more nor less. It is, however, necessary to take the poison from dogs whilst the disease is at its maximum, otherwise the period of incubation will be very different. By transferring the poison, originally taken from a dog, from one rabbit to another, from a second to a third, and so on, Pasteur has come to the result that after the tenth inoculation the incubation period is reduced to from eight to nine days. After this point the incubation period is of exactly the same length until the fifteenth inoculation, which during the last few days he has succeeded in reaching. Guinea-pigs reached the incubation period proper to them after a shorter series of inoculations, but neither in these animals nor in rabbits or dogs is it possible to weaken the poison by inoculation. Pasteur has, however, succeeded in finding animals in which it is possible. The latter part of the address was as follows:—“Jenner first propounded the idea that the poison which he called ‘grease,’ but which we now more accurately call horse-pox, must, before it can be given to man without danger, be weakened

in its poisonous effect, if I may use the term, by being transferred to the cow. After this the idea of weakening the hydrophobia poison by introducing it into certain animals seemed worthy of a trial. Many experiments were made, but in the greater number of animals experimented upon, the poison only increased in virulence, as, for instance, in rabbits and guinea-pigs; happily monkeys proved an exception. On the 16th December, 1883, a monkey was inoculated by trepanning from the medulla oblongata of a dog, whose rabid state had been confirmed by the fact that it had bitten a child who afterwards died of hydrophobia. The monkey became mad eleven days after. From this one, the poison was given to a second monkey, which also became mad in the course of eleven days. A third monkey was only attacked after twenty-three days. From the medulla oblongata of each monkey two rabbits were inoculated. The rabbits inoculated from the first monkey were seized after thirteen and sixteen days, those from the second monkey after fourteen and twenty days, those from the third after twenty-six and thirty, those from the fourth after twenty-eight, from the fifth after twenty-seven, and from the sixth after thirty days. It cannot be doubted after this that by transferring the poison from monkey to monkey and from several monkeys to rabbits, it at last became weakened. In the same way it is weakened in dogs. A dog which was inoculated from the fifth monkey had an incubation period of

fifty-eight days, although the inoculation was done by trepanning. Other experiments of the same nature led to like results. Thus, we are now in possession of a method by which the contagious power of hydrophobia is weakened. Inoculations from monkey to monkey produce a poison which, when given to rabbits, has an incubation period which gradually increases in length. If, on the other hand, the poison is given to a succession of rabbits, it becomes subject to the law already mentioned, by which it increases in violence when transferred from one rabbit to another. From the application of these facts we obtain a means of inoculating dogs against hydrophobia. As a starting-point is taken a rabbit poisoned from a monkey, which has been inoculated by poison that has gone through so many individuals that it is not able to cause death through hypodermic or intravenous injection. Having now applied this method for protecting dogs against hydrophobia, and having collected a large number of dogs which had been made impervious to infection, I determined, as I had a higher aim in view, and remembering the opposition which Jenner had encountered, to place the results, which, in my opinion, are the ground-plan of the protection of dogs against hydrophobia, before a competent commission. The Minister of Education, M. Tallières, to whom I spoke on the subject, was willing to support me, and he appointed MM. Béclard, P. Best, Bouley, Tisserand, Villemin,

and Vulpian to examine the facts which I had already laid before the Academy of Sciences at its meeting of the 19th of May last. The Commission, having chosen M. Bouley as president and M. Villemin as secretary, at once set to work, and I have the pleasure to announce that it has within the last few days sent its first report to the Minister. The Hydrophobia Commission has up to the present experimented on thirty-eight dogs, of which nineteen were from me, and declared to be impervious to contagion, whilst nineteen were unvaccinated. Those dogs which have not died under treatment are still under observation, and will be for some time to come. As far as the condition of those dogs which have been experimented upon is concerned, of the nineteen unvaccinated, three out of six of those bitten have become mad, five out of seven of those inoculated in the popliteal vein have become mad, and all of the five which were inoculated by trepanning have become mad, whilst not one case of hydrophobia has declared itself amongst the nineteen vaccinated dogs. One vaccinated dog died on July 13th from hæmorrhagic diarrhœa, which had shown itself in the first days of July. In order to see whether hydrophobia had had any part in its death, three rabbits and one guinea-pig were at once inoculated by trepanning from its medulla. These four animals are still in perfect health, which is a certain proof that the dog did not die of hydrophobia, but of an ordinary disease.

The next report from the Commissioners will treat of the history of twenty dogs, the inoculation of which they will superintend themselves."

Events and experiments now moved forward rapidly. M. Pasteur was astonished at his experiences with the monkey, but monkeys being scarce, he was fortunately able to select another animal, the rabbit, on which to make almost innumerable experiments, which he found equally favourable, and we shall here again summarise some of the facts.

(a) After experimenting for some two years, and using some hundred rabbits, he satisfied himself that if he inoculated a primary rabbit, in the brain, with virus from a rabid dog, the first rabbit would become rabid in just the same time a dog would if treated in a similar manner.

(b) He next carried on a series of experiments in the same manner he had done with the monkeys, and, after inoculating a series of rabbits, found that the brain virus degenerated in virulence, so that it killed rabbits in a certain period. He used the same material in dogs with a similar result.

(c) Establishing the virulence of the brain, he next tried the spinal cord and medulla, which were found to act exactly as the cerebral substance had done.

(d) The next experiments were very important; heat and cold were brought into play, medullas were removed with the utmost care from rabid rabbits,

and hung by a thread in jars in which a lump of desiccated caustic potash had been placed.

(*e*) When kept in a cool place the medullas retained their virulence a long time, but this could be increased by filling the glass jars with carbonic acid gas. If, on the contrary, they were kept in a room with a constant temperature of 20° C., it was found that they lost in virulence with each day's action of the dry atmosphere and the surrounding heat, until after, or on the expiration of fourteen days, a medulla was found which produced no perceptible action upon rabbits or dogs when introduced into the brain.

(*f*) The next step was to discover whether these medullæ possessed any constancy of action according to the time they had been thus treated. M. Pasteur found that any number of medullæ of approximately the same thickness had the same action upon animals, if introduced into the brain, after having had the same amount of exposure. For instance, a canine medulla would cause rabies in a rabbit in fourteen days; a supervirulent rabbit medulla would cause rabies in a rabbit in seven days. A slight action followed the one of twelve days, more of one of ten days, becoming more virulent as one of one, two, three, four, or five days was used.

We must think with astonishment of the number of experiments that have been made before M. Pasteur was able to make the next great move, and with which he almost startled the world at the

Academy of Science, Paris, October 26th, 1885. M. Pasteur announced that he was anxious to apply his test to human beings, but before venturing to operate on man he had so tested his method upon dogs, that he had rendered fifty of every age and variety insusceptible. He had very soon an opportunity of gratifying his desires.

In July, 1885, three persons travelled from Alsace and presented themselves in the Rue d'Ulm—a man of ripe age, M. Wohl, who had been bitten, but not seriously, by a mad dog; a boy, aged nine years, who had been bitten so seriously, in no less than fourteen places, that MM Vulpian and Grancher declared that his death from hydrophobia was absolutely certain; and finally, the mother of this unfortunate child. The man was declared to be in no danger, and returned home; the mother and child remained with M. Pasteur. The same evening the child was inoculated with the marrow of a rabbit which had died of rabies, and for two successive days, morning and evening, and for the eight following days every morning, he was again inoculated with virus of constantly increasing intensity, until he was finally treated with virus stronger than that received in his wounds from the dog which originally assailed him. "Witness" rabbits were also inoculated, and they developed rabies—and died. In the month of August M. Pasteur informed the mother that her child, Joseph Meister, was not cured, for the graduated inoculations had prevented the

development of the disease, but out of danger ; and he remains in good health to this day.

As regards the boy Joseph Meister. The inoculation began on July 7th, as follows :—

Date.	Time of inoculation.	Date upon which rabbit was inoculated from which the spinal cord was taken.	No. of days the cord was dried after removal.
July 7	9 a.m.	June 23, 1885	14
" 7	6 p.m.	" 25, "	12
" 8	9 a.m.	" 27, "	11
" 8	6 p.m.	" 29, "	9
" 9	11 a.m.	July 1, "	8
" 10	11 "	" 3, "	6
" 11	11 "	" 5, "	7
" 12	11 "	" 7, "	5
" 13	11 "	" 9, "	4
" 14	11 "	" 11, "	3
" 15	11 "	" 13, "	2
" 16	11 "	" 15, "	1

At the same time the child was inoculated, another rabbit was immediately afterwards inoculated with exactly the same material used on the child. This, we believe, is done with the virus used on every person inoculated by M. Pasteur in order to keep the experiments under the most exact control.

In reference to the inoculations on this boy, we have it on authority that out of eleven spinal cords used for this the boy Meister, five were without virus, and five were virulent when tested on rabbits.*

The medullas or cords which were removed from

* *Philadelphia Medical News*, January 23rd, 1886.

rabbits and inoculated on the child July 11th, 12th, 14th, 15th, and 16th respectively—that is, which had been desiccated seven, five, four, three, two, and one day—all gave positive results when reinoculated upon rabbits in a degree corresponding to the time they had been dried—*i.e.*, the freshest cords caused the appearance of rabies in the rabbits in seven and eight days, while the others gave results later, and the driest gave none.

The fresh cords having therefore caused the outbreak of rabies in the regular time, which the most virulent cords from rabbits had previously done, and the same not having caused any disturbance whatever to appear in the boy Joseph Meister, Pasteur concluded, “that the boy has not only escaped a future outbreak of canine rabies, but also that the systematic inoculation has so prepared the elements of his body that they were enabled to resist the action of still more virulent rabbit-rabies.”

In *Le Progrès Médicis*, December 19th, 1885, I find the following :—

“LA QUESTION DE LA RAGE.—Nous publions à titre de renseignement la note ci-dessus tirée du *Deutsche Medizinal Zeitung*, No. 100, p. 1128 :—‘ On écrit d’Alsace à l’*Arztl. Intellig.-Blatt* que le même chien qui avait mordu l’enfant J. Meister, avait mordu également le jour précédent un autre garçon, âgé de douze ans, nommé Max Vonné. Celui-ci ne put, faute de ressources suffisantes, être envoyé à Paris ; il resta à Meisengolt et continue à jouir d’une excellente santé.’ Le journal allemand émet des doutes sur le caractère rabique de l’affection dont le chien était atteint.”

The results of this case were watched with great interest, and it was not until the last week of October that M. Pasteur operated upon his second case, a shepherd boy of fifteen, named Jean Baptiste Judith, who was bitten in the hand, on the fourteenth of that month, by a mad dog against which he was defending a smaller companion.

Naturally there was some hesitation to undergo an operation of this nature, though there must have been, if we are to judge by M. Pasteur's more recent statistics, a large number of people bitten by dogs between June and October. It is very important to note this point. Are there any means of ascertaining the number that were bitten in France between those dates and the number of cases of hydrophobia reported?

M. Pasteur again addressed the Academy of Medicine* on March 2nd, 1886, when he reported that up to February 25th he had, with the assistance of Dr. Grancher, inoculated 350 bitten persons; and when he addressed the Academy on April 12th, the number had reached 688 bitten by dogs, and 38 by wolves.

In his address at the Academy, March, he stated that he was surprised at the large number of persons bitten by *des chiens enragés*, as well he might be. The explanation he gives is to the effect, that when rabies was considered incurable, friends

* "Résultats de l'Application de la Méthode pour prévenir la Rage après Morsure."

tried to conceal that the dog was rabid, so as to console the bitten person, and everything was done to drive away all thoughts of hydrophobia. The conclusion to draw from this is, that now rabies is brought under control, all cases are known. In the same communication M. Pasteur stated that, in order to keep his statistics above suspicion, he required a certificate from a veterinary surgeon or a medical man on the state of the dog inflicting the bite, and that he refused to vaccinate persons bitten through the clothes, and on whom there was no abrasion. In certain very grave cases he felt obliged to treat persons bitten by dogs which had disappeared, and which were suspected of being rabid.

He gives a sample of his daily work at the laboratory, furnishing the names of twenty-five persons bitten by dogs in a period of ten days.

We must admire the wonderful patience and skill which must have been displayed in treating these patients. In this first list published we find the name of Dr. John Hughes, of Oswestry, England, bitten November 13th, 1885, on the lower lip. As the report runs: "*Chien reconnu enragé par le docteur lui-même.*" This case presents us with one of the difficulties in connection with such a large number of people supposed to have been bitten by rabid dogs. M. Pasteur took Dr. J. Hughes' own word in default of the certificate of the veterinary surgeon.

We have received the following letter from the veterinary surgeon who saw this dog :—

“VETERINARY ESTABLISHMENT, WILLOW STREET, OSWESTRY,

“*March 27th, 1886.*

“DR. DOLAN,

“Dear Sir,—Please excuse my very long delay in answering your letter.

“Respecting your query, I have no hesitation in stating that the dog that bit Dr. John Hughes *was not affected with rabies*, but was suffering from ‘Acute Dyspepsia’ and its attendant depravity of the appetite, which was the only symptom that raised the alarm about rabies.

“I am, yours faithfully,

“R. HUGHES.”

Who was the more competent to judge—the doctor or the veterinary surgeon?

At the meeting on March 22nd, M. Pasteur detailed the death of Louise Pelletier, who had undergone this treatment after bites which were of a very severe nature. When M. Pasteur undertook the treatment he did so with grave apprehensions. In the interest of science, he ought not to have treated this case; yet, through the anguish of the parents and the interests of humanity, he submitted her to the process. The girl died on the 3rd of December, eleven days after the completion of his treatment. A grave question then presented itself. What virus had led to the death, the bite of the dog or the preventive inoculation?

This, we are assured was easy to determine.

We quote M. Pasteur’s own words as to how this was proved :—

“Vingt-quatre heures après la mort de Louise Pelletier, avec l'autorisation de ses parents et du préfet de police, le crâne fut trépané dans la région de la blessure, et une petite quantité de la matière cérébrale fut aspirée, puis inoculée, par la méthode de la trépanation, à deux lapins. Ces deux lapins furent pris de rage paralytique dix-huit jours après, et tous les deux au même moment. Après la mort de ces lapins, leur moelle allongée fut inoculée à de nouveaux lapins, qui prirent la rage après une durée d'incubation de quinze jours. Ces résultats expérimentaux suffisent pour démontrer que le virus qui a fait mourir la jeune Pelletier était le virus du chien par lequel elle avait été mordu. Si la mort avait été due aux effets du virus des inoculations préventives, la durée de l'inoculation de la rage, à la suite de cette seconde inoculation à des lapins, aurait été de sept jours au plus. Cela résulte des explications de ma précédente note à l'Académie.”

M. Pasteur was not discouraged by this death. He gave a number of statistics furnished by M. Leblanc, a veterinary surgeon, and member of the Academy, to show the number of persons bitten in the Department of the Seine in six years. M. Pasteur even hinted that the number of deaths from hydrophobia might be swelled, but that, through the connivance of medical men, deaths were registered as meningitis which were really due to rabies, a very gratuitous assertion and a very erroneous one. This remark occurs in the following passage in which he alludes to the silence :—

“garde très-souvent par les familles et par les médecins sur l'existence des morsures par chiens enragés, et même sur la nature de la mort, désignée, parfois sciemment, sous le nom de *méningite*, quand on sait bien qu'elle est due à la rage.”

In 1878, in the Department	}	103 persons were bitten, 24 deaths from rabies.			
of the Seine					
In 1879		76	12	12	
In 1880		68	5	5	
In 1881		156	23	23	
In 1882		67	11	11	
In 1883		45	6	6	

Statistics, as we well know, are double-edged weapons. The statistics of M. Leblanc cannot represent the actual number bitten in any of these years, if what M. Pasteur said be true, that bites of rabid animals were concealed.

The result of M. Pasteur's communication led to a very important resolution passed by the Academy on the 8th of March: "That an establishment for the treatment of rabies should be established at Paris under the name of the *Institut Pasteur*, which should be open to receive patients from all countries." A very excellent proposal, but I must confess I favour the view of Professor Bouchard, of Bordeaux. "Bien que je ne sois nullement partisan d'une centralisation, contraire à toutes mes idées, et que je préférerais l'installation de services vaccinaux rabiques dans toutes nos grandes villes, je ne crois pas moins devoir solliciter une participation à la souscription nationale pour la fondation de l'Institut Pasteur."* It is a step in the right direction, and must lead other countries to imitate it. Such centralization cannot exist long.

On the 1st of March M. Pasteur had treated 350 persons. On the 12th of April M. Pasteur announced to the Academy that he had had up to

* *Journal de Med. de Bordeaux*, March 21st, 1886.

that date 688 persons bitten by dogs (*enragés*), and 38 bitten by wolves (*enragés*). The nationalities are as follows :—

France	505	North America	9
Algeria	40	Finland	6
Russia	75	Germany	5
England	25	Portugal	5
Italy	24	Spain	4
Austro-Hungary	13	Greece	3
Switzerland	1	Brazil	1
Belgium	10		

Out of the 688 persons treated after the bites of dogs all have done well, with the exception of Pelletier; out of the 38 Russians bitten by wolves, 3 died rabic. M. Pasteur directs attention to the severity of wolf bites and to their fatal nature. When we look at this long list, gathered together in such a short time, does not the following critique appear appropriate? *

“ A propos de rage, il est impossible que nous ne fassions pas une réflexion qui, certainement, se sera déjà présentée à beaucoup d'esprits; c'est que depuis la découverte de M. Pasteur, le nombre des enragés ou plutôt de ceux qui craignent de le devenir, s'est accru en France et partout d'une manière extraordinaire. Il est réellement phénoménal de voir quantité considérable de gens et des chiens qui le croient, ou que l'on croit atteint de la rage. *Avant cette découverte c'est à peine si, de loin en loin, on signalait un cas de rage pendant la période caniculaire.* A quoi cela tient-il? l'influence morale ne joue-t-elle pas ici un rôle prépondérant? On pourrait le croire facilement en présence du bruit incessant que l'on fait, dans la presse et dans les sociétés scientifiques, à propos de cette terrible maladie. Cette remarque ne peut en rien atteindre la découverte de

* *La Presse Medicale Belge*, March 14th, 1886.

M. Pasteur, que nous sommes disposés à accepter quand tous les doutes qui planent encore sur l'efficacité de cette méthode auront été dissipés."

I accept M. Pasteur's statement on the fatal nature of bites of wolves, which he has been at such pains to establish by very old statistics, as I thus wrote in 1877.* "Statistics afford ample evidence that bites inflicted by rabid animals upon uncovered parts of the body, such as the face and hands, are much more disastrous than when the teeth have had to penetrate the clothing. Wounds upon the face seem most dangerous; hence the mortality from the wounds of rabid wolves, which generally fly at the face of those persons whom they attack. French statistics on this point are almost conclusive."

We have no doubt Pasteur will soon reach his thousandth case, but he does not really require any additional number to swell his lists; 500 cases are as good as 1,000. If he has treated 500 persons bitten by rabid dogs without a single case of hydrophobia following, then indeed we shall be forced to admit that his system has been effective, and we must bow down before the weight of evidence, and give all the honour that must be due to one who has brought into subjection that which has been for over two thousand years the terror of mankind. But even if M. Pasteur did not substantiate his discovery, yet we must still do honour to the man who in such an original way has grappled with this great problem,

* "Rabies or Hydrophobia," p. 262.

bringing to bear upon it all the resources of his great mind; performing experiments which are hazardous in their nature; and, above all, remembering, that pecuniary considerations are absolutely disregarded.

III.

IT is our duty to criticise M. Pasteur's statements, even in view of M. Pasteur's great name and past achievements, and it is, therefore, with the fullest appreciation of M. Pasteur's work that we attempt a critical analysis of the preceding histories. We have said the true nature of the poison of hydrophobia is unknown. It has eluded the skill of the chemist and the microscopist; its *modus operandi* is known only by its manifestations. M. Pasteur does not now lay claim to having discovered its microbe, though he takes a good working hypothesis, that there does exist a microbe. He has shown us that he can juggle with the virus; that he can transmit it to animals, that he can weaken it, and almost play with it at his will. He brings out new properties, revealing to us laws, if we may so call them, on its action, which place it, as a virus, on a basis of its own. If we interpret its mode of action, we cannot place it in the same category as the vaccine for small-pox, or the vaccines made by M. Pasteur himself, for the prevention of anthrax, fowl cholera, or typhus in pigs.

After all, it is a matter of comparatively little im-

portance as to how M. Pasteur prepares his virus. The great questions to decide are not as to the nature of the disease these rabbits suffered from, or whether M. Pasteur has varied his methods of preparing his virus, but whether the 688 persons who went to him were bitten by rabid dogs, and were exposed to the danger of contracting hydrophobia; whether we have any other instances on record to show that such a large number of people ever came under treatment by other observers, also escaping hydrophobia; and whether cauterisation, previously performed, plays any part in the preventive measures. In reference to cauterisation, Dr. G. M. Steinberg observes (*Medical News*, Philadelphia, April 24th, 1881, p. 453),

“that Pasteur does not attach as much importance to the prophylactic value of early and thorough cauterisation, as this measure seems entitled to. The considerable number of cases in which cauterisation was practised, may have had a greater influence upon the favourable result, in the extended series of cases reported, than Pasteur is willing to admit.”

Whether M. Pasteur introduces simply veal broth or a virus does not affect the result. His method, he states, has been proved innocuous, as he himself says:—

“Pas un phlegmon, pas un abcès, un peu de rougeur œdémateuse seulement à la suite des dernières inoculations.”

We may accept such a simple remedy without questioning its source, if the proofs are sufficient.

This is not an easy problem to solve. If we

approach it on the statistical side, we are confronted with evidence of a very contradictory nature; the proportion between transmission of rabies from dog to man being very varied. Owing to this variation, we have to consider an objection urged by Jesse Foote, in an essay published in 1793. Foote believed that excision of the bitten part, by the knife, was the only sure and certain method of prevention. He insisted:—

“1st.—That every medicine that ever was given for the prevention of hydrophobia had failed.

“2nd.—That although many who have taken medicine have escaped from infection, it ought not to be attributed to the power of medicine, because many have died notwithstanding the use of boasted specifics.

“3rd.—That of many who are bitten few are actually infected, owing to the bite being made through clothing, and that they have escaped by the infecting fluid of the dog being wiped away by the teeth passing through the clothing, or by some other fortunate circumstance.

“4th.—That as some medicine or other is indiscriminately given to every one who is bitten, so every one who is not actually infected by the bite does well, as every one who is actually infected by the bite dies; and as there are few instances of the latter in comparison with the former, thus it is that the reputation of a cure, by prophylactic or preventive medicines, has been supported, when in fact the safety or danger of the patient has been dependent upon another circumstance, which is whether he was, in consequence of the bite, actually infected or not.

“5th.—That if the patient were not infected he would do well, whether he took any medicine or not; but if he were infected it would be fatal, whether he took medicine or not.”

Let us see, however, what statistics have to say on the proportion between rabies in the dog and hydrophobia in man. In 1851 there was an outbreak of rabies in Hamburg. Though no fewer than 267 mad dogs were destroyed during the intensity of the epizooty, yet according to Boudin there was no case of hydrophobia recorded. The objection may be raised that medical men concealed the deaths.

For England we can answer, No, under similar circumstances.

“The ‘Alfort Register,’” says Fleming, “gives the list of mad dogs admitted annually for ten years :—

1853	.	11		1857	.	17		1860	.	20
1854	.	3		1858	.	19		1861	.	37
1855	.	16		1859	.	17		1862	.	32
1856	.	20								

For the last four years of the above decade, M. Bourrel admitted into his canine laboratory in Paris 85 mad dogs. If we add these to the above we shall find an annual mean of nearly 48 mad dogs in these two establishments alone; while in the whole of the department in which Paris is situated, the average deaths from hydrophobia are only 2.35 a year.”*

With this number of rabid dogs, we presume, a large number must have been bitten. Unfortunately there are no statistics on this point available. We have only the death-rate from hydrophobia. We

* Fleming, “Rabies and Hydrophobia,” p. 161.

have some evidence* which may fill up some lacunæ in regard to the foregoing. From Zurich we have returns (*Med. Chir. Rev.*, vol. vi., p. 265, 1826) of 223 persons who were bitten by rabid animals in a period of 42 years; of this number only 4 died. Wendt, of Breslau, treated 106 persons bitten by mad animals between the years 1810 and 1823. He also attended 78 others who were bitten by animals not rabid; out of this number 2 died. Ekstrom reports (*London Medical Gazette*, vol. vi., p. 689) that in 1824, rabies was prevalent in Stockholm, and 106 persons presented themselves at the Royal Hospital with wounds from the bites of animals rabid, or supposed to be so; out of the entire number only *one* (who did not attend to his wound) suffered from hydrophobia. Youatt, one of our best authorities on canine madness, was bitten himself five times; he cauterised himself the wounds of 400 persons bitten by rabid animals, all of whom escaped. Earle, surgeon to St. George's Hospital, told Youatt that nearly ten times that number (4,000) had undergone the operation of excision at that institution, after being bitten by rabid, or suspected dogs, and it was not known that any of these subsequently developed the disease.

Here we have some statistics which are difficult to explain away. Are they too old? Surely not, when M. Pasteur himself for his wolf-bites goes

* Dolan, "Rabies or Hydrophobia," pp. 156, 157 (1879).

back to the years 1706, 1806, 1811, 1812, 1822 (*loc. cit.*, *ante*, *Le Traitment de la Rage*, April 12th, 1886).

We may certainly conclude from these figures that the proportion of bites and deaths to the number of rabid dogs is very variable. As regards M. Pasteur's patients, there must have been an extraordinary number of mad dogs about in France to furnish 505 patients for him in such a very brief period; and they must have shown a great propensity for biting. That M. Pasteur should have 505 persons bitten by dogs not rabid within this period would not be extraordinary, but would certainly be something to be wondered at. As regards those who have come from foreign countries, we have some evidence to throw doubt on the rabid state of the dogs inflicting the bites, as in the Newark cases, United States. The dog which bit the four children sent to M. Pasteur bit several other dogs, and if we are to believe Dr. Austin Flint,* the dogs bitten by it have been unaffected. But here again we may be met with the objection, based on experiment, that there is a certain immunity in some dogs against bites, and this objection has a certain amount of weight even against M. Pasteur's own experiments on dogs.

“At least two-thirds of the animals bitten accidentally by dogs,” says Fleming, “supposed to be, or which are really mad, escape, without being submitted to any treatment, while of those experi-

* *Philadelphia Medical News*, April 3rd, p. 387.

mentally exposed to receive the contagion under the most favourable conditions for its transmission, about one-third do not contract the malady, even when abandoned to their fate. Hertwig reports that from 1823 to 1837, out of 137 dogs bitten in the streets of Berlin, and brought to the hospital of the veterinary school to be kept under observation, only 16 became rabid. The same talented veterinarian inoculated 59 dogs, of which number 14 contracted the disease. In some cases he found that several inoculations—from two to four—were necessary. A young mastiff for three years resisted all Hertwig's attempts at inoculation, although seven others inoculated at the same time, and with the same saliva, succumbed to the malady."

This objection we mention, supporting it by statistics, so as to give it its full force. The same dog, however, bit *two* children who were not sent to France, yet they are in as good health as the four children who were treated by M. Pasteur.

We have previously mentioned the case of Dr. Hughes, and given a certificate of a veterinary surgeon on the state of the dog which bit this medical man. We would not say *ex uno disce omnes*, but we think we may fairly, in view of such evidence, come to the conclusion, that a very large number who have been under M. Pasteur's treatment were bitten by dogs not rabid, and that they flew to him driven by their fears, just as they fly in England to those who profess to cure hydrophobia.

We have a letter from one who pretends to cure hydrophobia in Lancashire, stating that he has treated upwards of 2,000 cases by a lotion and antidote. This is a genuine letter, and the writer of it has applications for his remedy from all over England.

It is also on record that a well-known physician, Dr. Mead, claims to have also cured 1,000 persons by means of his antidote. The *post hoc* is too often taken as the *propter hoc*.

I may illustrate this by my own experience. Since 1870 I have treated some hundred cases of dog-bites, by simple incision and nitric acid. In not a single case has hydrophobia subsequently developed, though I have had patients bitten by dogs reported to be rabid. For instance, in October 1884, a dog bit seven persons at Sowerby Bridge, near Halifax.* The dog was killed at once. All those persons are in good health. The bites were chiefly on the legs, though one boy was bitten on the face in three places. There has not been a case of hydrophobia in Halifax since 1869, when there were five almost in succession, four under care of one surgeon. Now, I could not say, as a scientific fact, that my patients have been saved from hydrophobia by the nitric acid treatment. We can only draw inferences from other statistics. I only state a difficulty in connection with all our statistics on hydrophobia. We must ever remember *that the bite*

* Reference, Superintendent Copland, West Riding Constabulary Office, Halifax.

of a healthy dog cannot produce hydrophobia. We cauterise in every case, not knowing the state of health of the dog, and this uncertainty vitiates the scientific accuracy of all our statistics.

We consider we have good grounds then to question M. Pasteur's data, and to ask for further evidence, and a rigid scrutiny by an independent commission. It will indeed be a difficult task for any commission to follow up all the cases, and to collect evidence from different countries on the state of the dogs inflicting the bites, but the difficulties are not insuperable. If the commission is to be really valuable, it must do its work thoroughly. The commission will have to perform, control experiments, and examine M. Pasteur's data, which are hardly given with that exactness usually seen in scientific accounts. They will have to reconcile many discrepancies. For instance, M. Pasteur states that the girl Pelletier came under his care at too late a date, thirty-seven days after the bites, but we find that he treated a patient bitten at a longer date than this—viz., the widow Faure, bitten on the 1st September, 1885, recorded by M. Pasteur in the series treated by him from the 1st November to 15th December.

Lorda, bitten 25th October (in what place M. Pasteur does not state), was treated by him 21st November, twenty-seven days after the bite. M. Pasteur has limited the time to *fifteen days* since the death of Pelletier: more recently he says:—

“ Dans tous les cas, pour le loup en particulier, il est bon de se soumettre le plus tôt possible au traitement préventive. Les Russes de Smolensk ont employé six jours pour le voyage, et ne sont arrivés au laboratoire que *quatorze* et *quinze* jours après les accidents. On aurait donc pu, à la rigueur, commencer leur traitement huit jours plus tôt ; et l'on ne saurait dire quelle aurait été l'influence de cette modification pour les trois qui ont succombé.”

This shows that M. Pasteur is willing to profit by experience, for when the Russian Minister of Public Instruction wrote to him asking him to receive some Russian students, in order to establish his system in Russia, M. Pasteur replied :—“ Je suis prêt à recevoir des jeunes savants Russes, mais je persiste à croire, qu'en ce qui concerne la rage on aura le temps de venir de tous les points de la Russie en temps utile.” The deaths of some of the poor Russians have proved that M. Pasteur was not correct in supposing that there would be ample time for those bitten to reach Paris.

We learn from *Le Progrès Medical*, May 1st, 1886, that a Russian has died from a dog-bite.

“ Une *Russe* venue il y a un mois à Paris, à la suite d'une morsure de *chien enragé*, est morte à la Salpêtrière. Cette femme avait été amenée du laboratoire de la rue d'Ulm dans le service de chirurgie du docteur Terrillon. Depuis quinze jours seulement, elle avait pu être soumise aux inoculations de M. Pasteur ; *et le retard apporté au traitement curatif paraît être cause dans ce cas de l'insuccès*. Cette femme avait été mordue très-profondément au bras et au visage.”

If this woman came *too late*, what shall we say

of the value of this treatment in those cases where treatment was employed a much longer time after the bites? By all accepted rules of reasoning we must conclude that all the Russians came too late. We cannot be content with a theory which admits a cure for a certain number of the Russians and which failed also in a certain number; the reason for the failure being assigned, that treatment had not been commenced early enough.

The deaths of these Russians furnish an unanswerable argument against the centralisation of an *Institut Pasteur* at Paris, to which patients from all the world must flock. If there be a scientific basis for this form of inoculation, its methods must be capable of being accurately followed in other countries outside of M. Pasteur's laboratory. Patients cannot reach Paris in time if even fifteen days are too short. There is a pecuniary aspect to it. It would be unjust to put patients to the expense of going to Paris, and to all the inconveniences which arise from such an enforced journey.

The commission must satisfy itself as to how the virus is made, as to the strength of each injection, and all other details of the process. There should be no secret on a matter affecting humanity. We remember the outcry raised against Ferran because he would not reveal to the French Commission his method. We have a certainty that M. Pasteur will allow the commissioners the fullest information, and that he will explain in detail all the steps of his process.

The explanation of the phenomena is of interest. M. Pasteur says on this point:—

“ Il se pourrait que ce qui constitue le virus rabique soit formé de deux substances distinctes, et que à coté de celle qui est vivante, capable de pulluler dans le système nerveux, il y ait une autre non vivante, ayant la faculté, quand elle est en proportion convenable, d'arrêter le développement de la première.” Had I not reason to say that M. Pasteur has made the subject of hydrophobia more mysterious still?

Microbes have been looked for. M. P. Gibier has discovered one, and figured it in his “*Doctoral Thesis*.” M. Pasteur does not believe in this microbe, and has not been able to confirm M. Gibier's experiments. M. Vignal thinks M. Gibier has mistaken granules of myelin for microbes. Another microbe has more recently been described by Professor Föl, discovered by him in preparations made by hardening the spinal cord or brain, by immersion directly after death in a solution of 2.5 grms. bichromate of potash and one gramme of cupric sulphate in 100 parts of water. Cut in sections of about 1.200^{ths} of a millimetre, and then steeped in Weigert's solution of hæmatoxylin, and afterwards placed in absolute alcohol, decolourised groups of minute micrococci are seen. A cultivation made from the brain and inoculated into healthy animals produced, we are told, all the symptoms of rabies. All cultivations become inert in six

days. The microbic theory is the natural outcome of modern thought, but it only complicates the problem as to the action of the virus. Wolf-bites are more dangerous than dog-bites, not because the quality of the virus in the wolf is different, but because it implants a larger quantity, owing to the larger size of the wound it inflicts. Such a fact does not harmonise with the microbic hypothesis.

The great value of Pasteur's work.—Whether Pasteur has succeeded or not in finding the preventive against rabies, his work deserves the highest praise, and must have a beneficial result. It will encourage research. Hydrophobia was given up to despair; the hydrophobic sufferer was abandoned. Even Sir Thomas Watson proclaimed that all we could do was to promote his euthanasia in the pleasantest and easiest manner. Rabies could not be controlled; that was the old formula. Pasteur set us on a new field, and has given us fresh hope. With such encouragement, persons bitten by rabid dogs will not lose heart; hope is again infused into society. I feel a certain measure of satisfaction that in 1879 I took a view opposed to such a master of my profession as Sir Thomas Watson, when I penned the following lines:—

“We have shown how much we know, how much we have yet to learn. We are willing to acknowledge our ignorance on many points, but we cannot admit the impossibility of discovering a remedy, and we are not disposed to write over the door of the

patient's room the despairing and well-known words of Dante :—

‘Lasciate ogni speranza.’

The treatment in the past has been, in the majority of cases, irrational and unscientific. The majority of the deaths may be attributed as much to the bad treatment and the action of the medicines administered as to the virus introduced into the system. Encouraged by the recoveries that have taken place, we venture to prophesy further successes, with some remedy wrested by the skill of the scientist from the grand laboratory of nature. We believe there is not a poison in existence for which there is not an antidote. We have faith in our profession, a faith not resting on the changing quicksands of medical theories or opinions, but built on the sterling triumphs of medicine over diseases which were pronounced by our ancestors incurable. We cannot recognise, then, in rabies any special conditions which preclude us from hoping that the beneficence of the Creator, which enables us to grapple with other diseases, will grant us similar powers over this sad scourge of humanity.”

I could wish that these had been prophetic words, and that my dreams were realised. It is too soon to declare that the scientist has triumphed; but I believe we are on the threshold, and soon we may reach the promised remedy.

But something more is required than even a remedy for hydrophobia. Even though Pasteur gave us a

remedy against the bites of rabid dogs, yet we have something more to do—we have to extinguish the disease in the dog. *Combattre la rage, c'est prévenir l'hydrophobie.** Can rabies be extinguished in the dog? If all dogs were vaccinated, and rendered insusceptible, then, indeed, the consummation would be soon reached. Dog-owners, regarding the remote danger of rabies, will not resort to this method except under compulsion from Government. Has the time come for this compulsion? When we find such opposition to compulsory vaccination against small-pox, we hardly think that our legislators will take such a course. Failing then vaccination, can rabies be controlled by rigorous measures? Ought we to resort to rigorous measures? This last question may be answered by looking at the mortality from hydrophobia in England for a series of years.

The table on the following pages shows the mortality from hydrophobia in England and Wales from 1870 to 1883 inclusive.

A new Dog Act.—At the International Congress, London, 1881, Dr. Van Cappelle (the Hague) read a very practical paper on the methods adopted in Holland to check rabies. I supported a resolution proposed by him to the effect that that Congress should declare it desirable that the Governments of all civilised countries should agree as to the measures for the abolition of rabies. In Holland

* "Transactions of Congress," vol. iv., p. 476.

a stringent Dog Act is in force. Dr. Van Cappelle furnished evidence as to the efficiency of this Act in limiting rabies. I think a more stringent Dog Act is required in England; and I have drawn up an Act, which, if applied, would certainly go far to keep dogs under better control, and secure for them better protection. I throw responsibility on the owners of dogs.

The following is the text of the Dog Act I prepared in 1879.

An Act to amend the Dog Act of 1871, and to provide further protection against dogs:

Whereas it is expedient that further protection should be provided against dogs, Be it enacted by . . . and with the advice and consent of . . . in this present . . . assembled, and by the authority of the same, as follows:—

I. From and after the passing of this Act, any police officer or constable may take possession of any dog that he has reason to suppose to be savage or dangerous, straying on any highway and not under the control of any person, and may detain such dog until the owner has been found, or claimed the same and paid all expenses incurred by reason of such detention. The owner of such dog taken possession of by any constable shall be informed by letter, stating the fact of such dog having been taken possession of, at his usual or last known place of abode. When any dog taken in pursuance of this Act has been detained for five clear days, when the owner cannot be found as aforesaid, or for five clear days where he is so known without the owner claiming the same and paying all expenses incurred by its detention, the chief officer of police of the district in which the dog was found shall cause such dog to be destroyed. The owner shall be

summoned, and liable to a penalty not exceeding five pounds, and pay all expenses. All dogs detained under this section shall be properly fed and maintained at the expense of the local rate.

II. Any court of summary jurisdiction may take cognisance of a complaint that a dog is dangerous and not kept under proper control, and if it appear to the court having cognisance of such a complaint that the dog is dangerous, the court may make an order in a summary way directing the dog to be destroyed; and any person failing to comply with this order shall be liable to a penalty not exceeding twenty shillings for every day during which he failed to comply with such order.

III. In order to assist the police in establishing ownership, all dogs shall wear a collar with the name and address of the owner legibly stamped thereon, and such collar shall be a proof of ownership. Any dog found wandering without a collar shall be seized and detained by the police; if unowned, it shall be destroyed after three days' detention, and should the owner be found, the dog, if healthy, shall be restored on payment of expenses and a penalty not exceeding five pounds.

IV. The local authorities shall, if a mad dog, or a dog suspected of being mad, be found within their jurisdiction, issue, and when necessary revoke, an order placing restrictions on all dogs during such period as may be prescribed in such order, throughout the whole of their jurisdiction, as follows:—1. All dogs shall wear an approved muzzle, or be led by a string during such period as the order may direct. Any person failing to comply with such order shall be liable to a penalty not exceeding twenty shillings. 2. Any dog found wandering by the police without a muzzle shall be at once seized and detained, the owner of such animal to be liable to a penalty not exceeding twenty shillings. 3. Due notice of such order shall be published at the ex-

pense of the local rate. 4. The provisions in this Act contained as to the detention and destruction of dogs found straying on the highway shall apply to dogs found at large in contravention of any order made in pursuance of this section.

V. The local authorities shall assist and combine in any general action taken by other local authorities to restrict the spread of rabies, even though the malady may not be present and in the jurisdiction of such authority. Such general order shall be announced from the Privy Council, and published in the *Gazette*.

VI. All dog-owners shall report to the police the occurrence of rabies amongst any animals they may possess, either dog, cat, horse, or cow, under a penalty not exceeding twenty shillings, within twenty-four hours.

VII. Dog-owners shall be responsible and liable for any damage done by their dogs owing to their own imprudence or negligence.

VIII. Dog-owners shall report to the police the absence, straying, or loss of their dogs, under a penalty not exceeding twenty shillings.

IX. Any person wilfully exciting a dog, or otherwise ill-treating it, shall be liable to a penalty not exceeding five pounds, or in default, imprisonment not exceeding three months.

X. A register shall be kept at all police stations of all dogs destroyed, either for rabies, or by order of a chief constable of police, or of a court of summary jurisdiction; and a return of the number of dogs destroyed shall be published annually, specifying the reason for their destruction as aforesaid.

XI. On application from a veterinary school of medicine, or from a veterinary surgeon, or from any legally qualified medical practitioner, the police shall hand over any bodies of dead animals they may possess for dissection, but a guarantee must be given that the body is required for that

purpose, and that when done with the remains shall be burned.

XII. All dogs destroyed by the police shall be subsequently burned.

XIII. In England and Ireland any penalty under this Act may be recovered in manner provided by the Summary Jurisdiction Acts, and in Scotland all such penalties shall be prosecuted and recovered before a court of summary jurisdiction under the provisions of the Summary Jurisdiction Act, 1864. The other portions of the present Act to stand.

I said in 1879 that if an Act framed on the above lines were passed, both the human and canine species would be protected, for rabies in a few years would almost be stamped out. We should not then have periodic seasons of panic; nor should we hear of the destruction of so many dogs during the temporary excitement caused by the dread of hydrophobia.

If any further evidence on the value of prophylaxis is required, it is furnished by the later returns from Vienna of Dr. Frisch, who went to Paris to study Pasteur's method. He tells us the more severe the dog laws and regulations the greater is the guarantee against hydrophobia. Bollinger has recently given us statistics on the value of stringent police regulations in Bavaria (*Mün. Med. Wochenschrift*, March 1886), and all who are interested in the subject will find in Bollinger's paper interesting statistics confirmatory of the views of Dr. Van Cappelle, and of my own.

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