PASTEUR AND RABIES

T. M. DOLAN M.D.

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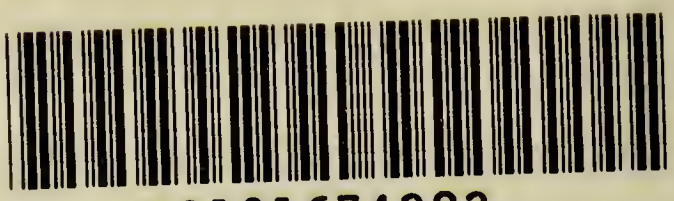

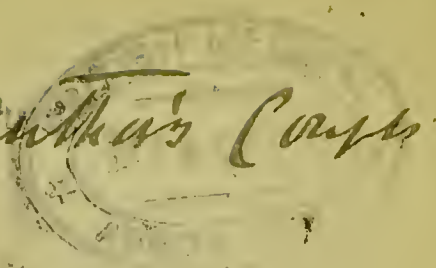

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BY

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AUTHOR OF

"THE NATURE AND TREATMENT OF HYDROPHOBIA" (1878); "PASTEUR AND HYDROPHOBIA" (1886); "DRINK AND PAUPERISM;" ETC., ETC.

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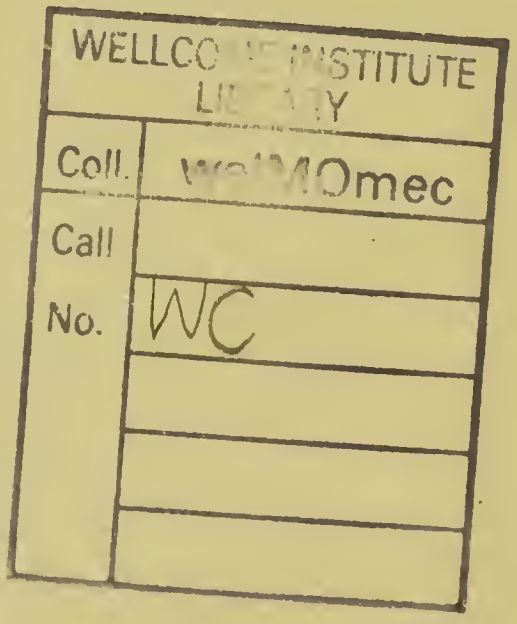
"Let not the authority of the author be in thy way, whether he be of little or great learning, but let love of simple truth lead thee to read."

"Inquire not who may have said a thing, but consider what is said."

THOMAS À KEMPIS.



PREFACE.

THE following letter from Professor Peter, Member of the Academy of Medicine, Paris, the great French clinician, and successor of Trousseau, has been received by the author of this work.

"DEAR DR. DOLAN,—I am entirely in agreement with you that M. Pasteur's so-called preservative against hydrophobia is at once a mistake and a danger. The same may be said of his anti-charbon inoculations. I will subsequently furnish proofs of this. This treatment, altogether empirical, devoid of scientific basis, wavers at the hazard of experiments sometimes *simple*, at another time *intensive*, then quickly reverting to the *simple* method, contends vainly against disastrous facts which condemn it. From this point of view nothing can be more pitiful than the answer of M. Pasteur with reference to the death of Lord Doneraile. "Death occurred because he was not inoculated until eleven days after the bite, and because Lady Doneraile objected to the intensive treatment." Whence it appears that all who have been inoculated eleven days after having been bitten should not, since the treatment is of no avail after the eleventh day, be counted among those protected by the treatment of M. Pasteur. On the other hand,

all who have not been inoculated by the intensive method ought not to be counted. This then is the only benefit. But consider that in presence of the lamentable facts of this homicidal treatment pointed out by myself, M. Pasteur has felt obliged to revert to the simple treatment (which he had declared inefficacious and inert). Was ever seen such confusion? Would it not be more natural to confess that medical treatment is of no avail—the simple no more than the intensive? But then the interests of M. Pasteur and his pupils would have suffered.

"For the same reasons, however unscientific, M. Pasteur has been obliged to propagate a belief in the frequency of hydrophobia. Now, hydrophobia in man is a rare, a very rare disease. I have seen only two cases in thirty-five years of hospital and civil practice; while my colleagues, both in town and country, count the cases they have seen by units, and not by dozens (still less by hundreds). To magnify the advantages of his treatment, and to conceal his failures, M. Pasteur has every interest in exaggerating the annual mortality from hydrophobia in France, but this is not in the interest of truth.

"For example, among my colleagues of the Academy of Medicine who have had an extensive practice, M. Worms has seen but one case in thirty-five years. Professer Ball has seen only one case in thirty-three years. Dr. Polaillon has seen two cases in twenty-eight years. In twenty-six years Dr. Léon Labbé has met with two cases. Professor Tillaux has seen three cases. I have before me thirty-four letters from perfectly

unbiassed medical men, who in the whole course of their lives, had not seen a single case of rabies in man. Dr. Paul Meilhac and his son have, at Argentat (Correze), seen four cases out of a population of 12,650 inhabitants. Dr. Seguy, at St. Flour, with a population of 51,000 has met with five cases, which makes one case in fifteen years for the former, and one in eleven years for the latter. We are far from the 1,500 *enragés* cared for in less than six months at the laboratory in the Rue d'Ulm.

"The scientific idea of M. Pasteur is to flood the human system twice a day, many days in succession, with a progressively stronger virus, with the view of neutralising the infinitesimally small quantities of virus of rabies already introduced into the organism by a bite, a purely chimerical idea. If the mortality from hydrophobia in France had diminished, this would be a proof of its efficacy, but the mortality has been augmented since Pasteur began his work, and not only is the mortality increased, but cases of paralytic rabies have been induced by later inoculations.

"It is, then, to expose the mischief of the intensive method that I addressed the Academy. This method, M. Pasteur says, has been *spontaneously* abandoned, which is at variance with the truth. But, allowing that it is true, it was because of its danger, and because it was quoted in the case of Lord Doneraile. I do not think it necessary further to dwell upon this point. M. Pasteur's treatment must be judged by the statistics of the annual mortality from hydrophobia in France. This has increased instead of

having decreased, as was pompously announced by Vulpian and Pasteur. Pasteur's treatment is equally condemned by the analysis of deaths: their clinical analysis showing that a certain number of fatal cases are due to the inoculations, which explains the increased mortality from hydrophobia in man.

"But M. Pasteur not only conveys rabies to man, but transmits charbon to animals (for details and statistics, see a brochure 'The Value of Pasteur's Treatment as a Preventive against Rabies.' Paris: Asselin and Houzeaux, 1887).

"Inoculation as preventive of charbon was practised upon 4,564 sheep at Kachowka, in Southern Russia, of which 3,696 died. M. Bardach, in August, 1888, inoculated 4,564, of which only nineteen per cent. survived. This is called protective inoculation! The promotor of this gigantic holocaust was M. Meczikow, a doctor of philosophy, director of the Bacteriological Institute of Odessa. This doctor of philosophy is, it thus appears, as ignorant of medical matters as M. Pasteur, doctor of chemistry. Hence is explained the temerity with which Pasteur approaches the solution of the most complex medical problems. He would do more than Jenner, but he does not understand that Jenner arrested the development of dangerous small-pox by inoculation of the milder and safer cow-pock. Without doubt M. Pasteur derived his practice from the inoculators of a former time, in inoculating artificially the disease they wished to prevent, with this difference, that they, acting in a rational manner, inoculated preventively under

the most favourable conditions ; whereas, on the contrary, he inoculates his morbid poisons when the disease has already invaded the system. He is in the position of a physician who, consulted by a subject of small-pox, should insert a second dose of the variolous poison in order to subdue the force of the first.

"To conclude, it is shown that M. Pasteur has given charbon to animals, and rabies to individuals who would not otherwise have had it. It is time to raise a cry of alarm.—Accept the assurance, etc., "PETER."

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M. PASTEUR AND RABIES.

I. INTRODUCTORY.

In 1877 I wrote for the proprietors of the "Medical Press and Circular" a series of articles on Hydrophobia, subsequently published and extended in book form under the title of "Rabies and Hydrophobia." My chief objects in writing these articles were :—

1st. To excite some interest in this subject, amongst my professional brethren, so as to lead to a better comprehension of and a more hopeful tone on the disease and its treatment.

2nd. To dispel a number of superstitions, which appeared to cling round the subject, and to oppose periodic panics.

3rd. To protect the dog.

For the last object, I suggested throwing additional responsibility on dog owners. As an instance of my pleading for the dog and my exposure of some fallacies, I quote one paragraph. ("Rabies and Hydrophobia," page 154, 2nd edition, 1879.) "*The majority of*

persons who are bitten escape, and hence the success of vaunted specifics which have been in vogue from the days of Mithridates down to the present days. *No bad result can follow from the bite of a healthy dog* except an unpleasant injury, though every wound should be attended to promptly, and all reasonable precautions adopted. The animal which inflicted the bite should be watched, in order to ascertain accurately the state of its health. When proved healthy, the patient will be relieved of suspense, and will know with certainty that hydrophobia cannot follow. Dogs will bite if they are teased, we are too often apt to forget this; they are loving, affectionate, and much enduring animals, but their endurance is oftentimes sorely taxed. We have many times felt astonished that dogs have so patiently allowed their ears or tails to be pulled, and other torments to be inflicted upon them, without retaliation, and we cannot too strongly impress the duty that dog owners owe to these animals, of treating them with kindness and consideration. Statistics show that a large number of persons are bitten annually without any bad results, and also that a large number are bitten by rabid animals with impunity, and that this impunity is secured by attention to the wound."

I gave a number of statistics collected from different sources, to which I shall have to allude,

and as they were collected without any reference to subsequent controversy, they are, I consider, all the more valuable.

Whilst writing these articles, I had letters from all parts of England offering me cures ; amongst the most extraordinary was the following :—

"1, Spring Terrace,
"Habergham,
"Nr. Burnley.

"DEAR SIR,

"I read special report on the nature of Rabies, with much interest. I send you a few cases out of many which have come to my notice, and some, as you will see, under my treatment.

"The lotion and antidote are discoveries of my own, which I do not think I ought to divulge under a peerage and a subsidy from Government, or at least £50,000. It is a true and perfect antidote, and there has been no failure in upwards of 2,000 cases.

"I remain,
"Yours truly,
"E. A. Verity,
"D.D., Fellow Royal His. Society."

This was a genuine letter from a clergyman at Burnley, who had a great reputation in Lancashire for the cure of hydrophobia ; he sent me a table of some of his cases, which were drawn up in the following form :—

"Case V. Lilly Bilbesboro', aged 5 years, bitten on right cheek by a lurcher, just under the eye. Wounds not cauterized, consultation eight weeks after bite. Treatment: lotion and 8oz. antidote—recovery.

"Case IX. James Hargreaves, aged 14, bitten on the wrist by a rabid dog, wounds not cauterized, full of pus, consultation six weeks after. Treatment: lotion and antidote 1 pint—recovery."

I need not give any more extracts, *ex uno disce omnes.* I inquired into these cases, and I found that the Rev. Dr. Verity had a large number of dog-bitten patients. A few of them had died from hydrophobia after his treatment, but the majority escaped; the reason being that they had been bitten by non-rabid dogs, or been bitten through clothing, etc.

I inquired into many other alleged hydrophobia cures, but always with the same result, hence I was able to formulate this proposition, "if any-one obtained a reputation for the prevention of hydrophobia, and if all the dog-bitten sought or took his remedy, the result would be statistically favourable." Here I had to deal with unscientific though earnest men, who really believed, through disregarding the fallacy which underlies such statistics, that they had antidotes.

When M. Pasteur startled the world by his first statistics, and his first report, I was

naturally interested, and disposed to believe, that at last, in the hands of a man of such well-known scientific fame a cure was found, and I was at one time so favourably impressed by some portions of his work as to say that it was deserving of the highest praise.

I could not however but see that the very same fallacy was running through his statistics, which ran through those of the Burnley Vicar. The dog-bitten were certainly rushing to Pasteur, but the results were explicable, for the probability of hydrophobia occurring in persons bitten by rabid dogs, depends on many factors, such as the severity of the bite, the protection afforded by clothing, and the bleeding of the wound; whilst from the evidence it was clear that, owing to panic and fashion, those bitten by non-rabid dogs were seeking protection, for the unparalleled number of dog-bitten patients cannot be otherwise explained, unless we choose to make a *tabula rasa* of all our past experience of the disease.

This first and important objection was supported by other and more powerful ones, for as in process of time the deaths began to fall in, I was able to compare the statistics given, with the statistics of ante-inoculation days—and I found that since Pasteur's prophylactic came in vogue, the supposed rabid dog-bitten of France had increased in extraordinary proportions, whilst

the average mortality of France remained almost *in statu quo*, showing moreover a singular absence of fluctuation.

But these are transparent popular objections. The scientific ones are more cogent still. The action of the supposed prophylactic when examined, resolved itself into what might be called pure Empiricism, in contradiction with scientific evidence. Thus, a certain number of injections of solutions from spinal cords, from 14 to 5 days old, were used, and the old *post hoc* argument was employed. Because the patients treated by these injections did not subsequently develop hydrophobia, *ergo* the prophylactic was the remedy.—*Q.E.D.* But here at once we are face to face with other aspects of the problem.

In one series cords were used, based on one formula—and "cures" resulted. Deaths, however, occurred; then the formula was altered and made more intensive, and still more deaths occurred. A return was made to the first formula with a modification, but still deaths occurred.

In explanation of the deaths, a general affirmation was made that the cases that died came "too late," but in looking through the list of patients I found that other case of the same duration were treated, and in case of recovery there was not a word said about their being "too late."

Reasoning logically, if, as in the case of Lord Doneraile, 11 days was too late for treatment, then all cases that came over that period must be expunged from the list of "cures," and again, if Lord Doneraile's death was due to the application of the weak or first method, then the cases of the others treated by the same formula, fall to the ground, and more especially is this true in regard to those bitten more severely than Lord Doneraile, who were treated in the same manner.

The other objection from the physiological side has never been answered. If we inject a number of spinal-cord solutions, on the theory supported by Pasteur, what is the rate of absorption of each injection? Do we know anything of the physiological processes in connection with each injection?

All these objections I shall endeavour to substantiate by chapter and verse, quoting Pasteur's own words, and giving statistics and names, so that the unprejudiced reader may be able to judge for himself as to whether there is any force in my arguments.

As the appeal is now from the profession to the reading public, I think the subject should be presented from both aspects. I have not hesitated to accept the full responsibility for my attitude towards the new method of inoculation. Though I have had hundreds of dog-bitten

patients since 1885, I have treated them at home, as I had done since 1869, and I have not sent a single patient to M. Pasteur. I have not had a death from hydrophobia as the result of my scepticism, though I have had some of the same class of patients as those described in the list furnished by the English Hydrophobia Commission, and treated by inoculation.

II. FALLACIOUS STATISTICS.

WE have just cause to complain of the unscientific nature and arrangement of the statistics which from the first have been given to the public. The following cases, taken from M. Pasteur's lists, are two only out of many which would equally conclusively show the imperfect evidence on which the rabidity of the dogs rests.

"Case 9. Albert Michard, 15, Rue Nazarine, bitten by dog. The boy was carrying a large loaf and fell over dog. Dog not seen again. No evidence that dog was mad.

"Case 13. Charles Aubertin. Bitten by a strange dog. Entered his door and bit him. Killed immediately by police. Nothing else known."[1]

One remarkable thing, amongst many other remarkable things in connection with this so-

[1] Report of British Hydrophobia Commission.

called discovery, is the tabular form adopted in M. Pasteur's statistics, viz. :—

"Class A. Cases in which the dog was proved to be rabid by the experimental test.

"Class B. Cases in which the dog was recognized to be rabid by a veterinary surgeon.

"Class C. Cases in which the dog was only suspected of being rabid."

This classification pre-supposes that all the patients have been exposed to danger: it makes no allowance for non-rabid dogs; it accepts as a test a veterinary surgeon's opinion (*post mortem*), though this evidence is regarded as "worthless" by the most eminent of veterinarians; and according to these statistics there have suddenly sprung up in France thousands of rabid dogs.

I may here be met with the objection that the most eminent medical men in England support Pasteur, and it may be said, "Surely you will not put yourself in opposition to them."

My answer to this is very simple. If I take the words of one of the most eminent of these names, and prove that his observations on the inoculation system are not supported by statistical, physiological, or pathological evidence, I need not offer any further apology or explanation for differing even with such eminent men.

At the meeting at the Mansion House, Monday, July 1st, 1889, Sir James Paget is reported

in the "British Medical Journal," 6th July, 1889, to have said :—"Those who knew him (Pasteur) would rely on his word without any question." These words show that our eminent surgeon does not speak from personal knowledge, as is further shown by the following contradictory statements :—

Statement at the Mansion House, July 1st, 1889.
15 *per cent. mortality.*

"The fairest estimate that could be made, and it was not in the least too high, but if anything too low, showed that, taking the average of every person bitten by rabid dogs, 15 would suffer from the disease and 15 per cent. would die.

"Of the 7,000 bitten, if 15 of each 100 had died, there would have been as nearly as possible 1,000 deaths, but only 100 died, the other 900 lives were as absolutely saved as if they had been snatched from drowning.

"Last year (1888) 1,673

Statement on the Hydrophobia Commission, June, 1887.
5 *per cent. mortality.*

"Making fair allowances for uncertainties and for questions which cannot now be settled, we believe it sure that, including the deaths after bites by rabid wolves, the proportion of deaths in the 2,634 persons bitten by other animals was 1 and 1·2 per cent.—showing even on the lowest estimate the saving of not less than 100 lives. . . . . If, as in the estimates used in judging of the utility of that method of treatment, these numbers are taken as representing *only* 5 *per cent. of the persons bitten*, this preventive treatment will be required for 860 persons

persons were treated, all certified to have been bitten by rabid dogs. Of these 13 died. . . . Thirteen deaths instead of the number that would have been before the treatment of M. Pasteur—about 250."[1]

in all England, for 170 in London alone. . . ."

Now here we have, in the first place, two estimates of 5 and 15 per cent. as the mortality from rabies or hydrophobia, and according as we accept these percentages does the value of M. Pasteur's work increase. Leblanc fixed the mortality at 16, Vulpian at 16, Aiken at 60 per cent. With each percentage we obtain a supposed increased saving of life. We come to the heart of the question here, which I shall put into another form.

III. THE AVERAGE MORTALITY IN FRANCE.

WHAT was the mortality in France for thirty years before M. Pasteur took up his work? For if Pasteur has saved in France during the five years 900 lives, then there ought to have been an equal or proportionate mortality in France in antecedent years.

The illustrious Tardieu in a report presented

[1] "British Medical Journal," vol. ii., 1889, pp. 38, 39.

to the Minister of Hygiene in 1863, maintained that twenty-five cases of rabies per year approximately represented the mean mortality of France.

"It is not the absolute expression of truth," he said, "but it is not far from it; for, thanks to the incessant stimulation of the administration, and to the support of the local authorities and the Councils of Hygiene of the arrondissements, replies have been made to the inquiry from all the departments." Twenty-five deaths a year; but times have changed perhaps. Let us see, and we shall put the figures to the very worst advantage, for we have other figures, and as it is alleged that they are imperfect, we may even double them, and even then we shall fall far short of the estimate made by the eminent surgeon simply on Pasteurian authority.

1850	27	1862	26
1851	12	1863	49
1852	46	1864	66
1853	37	1865	48
1854	21	1866	64
1855	21	1867	57
1856	20	1868	56
1857	13	1869	36
1858	17	1870	36
1859	19	1871	14
1860	14	1872	15
1861	21		

Allowing for errors, and for the non-registration of cases, what becomes of Sir James Paget's 250 lives saved in one year, when we see that even doubling the greatest mortality ever reached, we can only reach a figure of 132 in 1864? and if we deduct from Sir James Paget's calculation 50 as representing the deaths that might have occurred in other countries, we have still 200 as the mortality in France in one year —for five years a mortality of 1,000. Our table shows a sliding scale of 12, 14, 15 in certain years, and also a fluctuating scale in almost every year.

As it is permitted to the Pasteurians to question the mean average, it is also permissible for me to say that this average of Tardieu may be even too high, from the fact that cases may have been returned as hydrophobia which really were delirium tremens, meningitis, mania, etc., a by no means improbable thesis. I may well question, then, the unsupported assertion of even so distinguished a surgeon as to the saving of 900 lives, finding my justification in verification by figures, which were prepared without any regard to controversy, and which rest upon the national statistics of France, and published *sans parti pris.*

But these statistics are disputed. It is said they are imperfect, that they should be doubled, nay quadrupled, to bring them up to the figures

desirable, *viz.*, to harmonize with the Pasteurian theories. Fortunately we have some figures which must be accepted; we allude to those of Dr. Dujardin Beaumetz, published in his official capacity as Officer of Health. If we take the statistics of hydrophobia in the department of the Seine before Pasteur's prophylactic came in vogue and the statistics subsequently, we shall be able to demonstrate with mathematical precision the complete failure of the Pasteur system in a given area.

In this department M. Pasteur has been operating under the most favourable conditions, that is to say, he has had the assistance of the police laws to keep down the wandering street dog and we have to chronicle the strange circumstance that M. Pasteur, the preventer of hydrophobia, has had to encourage the Prefect of Police in the seizure and destruction of stray dogs—a satire if it were not so tragical.

M. Dujardin Beaumetz gives us the means of comparison. The deaths in the department of the Seine from 1880 to 1889 have been as follows:—

1880	4	1885	22
1881	21	1886	3
1882	9	1887	9
1883	4	1888	19
1884	3	1889	6
	41		59

I may here quote the opinions of Professor Michel Peter [1] on these statistics :—

"I have said, repeated, and professed, that the inoculations—pretended to be anti-rabic—of M. Pasteur are in principle nonsense, and in practice would be a deception. Here is the proof :

"My learned colleague, M. Dujardin Beaumetz, has given the figures relative to deaths from rabies in the Department of the Seine for the last ten years. These are the official figures, and if we count the deaths for the four years which have preceded the use of the method we have—

1882 . .	9 cases.	1884 . .	3 cases.
1883 . .	4 „	1885 . .	22 „

Thirty-eight cases.

Now, if we take the figures for the four years in which we have enjoyed the advantages of "the method," we have—

1886 . .	3 cases.	1888 . .	19 cases.
1887 . .	9 „	1889 . .	6 „

Thirty-seven cases.

But the better demonstration is that the sum of the deaths during four years 1880-3 is, also, thirty-eight cases. Thus we have for this Department—

[1] "Journal de Médecine de Paris," July 5th, 1890.

For the four years before Pasteur 38 cases.
For the four years after Pasteur 37 „

Is it possible to give a clearer proof of the inutility of the method ?

"But here is another point of higher importance still and, again, quoted from the text of M. Dujardin Beaumetz :

"'The observations of cases of rabies in Paris in 1889 include three children and three adults.

"'Of the SIX sufferers who perished, three were under the anti-rabic treatment, and three were not taken to L'Institut Pasteur at all.'

"Now what need we say after this, the demonstration is brutal in its simplicity. M. Dujardin Beaumetz ends his report: 'As Pasteur's treatment does not prevent the persistence of hydrophobia, we must still depend upon the execution of the law of July 21st, 1881, which orders the killing of every dog which has been bitten by a rabid animal.'"

IV. THE MORTALITY IN THE PARIS HOSPITALS.

WE know with an almost absolute certainty the mortality from hydrophobia in the Paris hospitals, both antecedent to the work of Pasteur

and subsequent to it. We have the names and the hospitals wherein the deaths occurred. I have most carefully collated these statistics. According to the inoculation school, there has been an annual mortality in the Paris hospitals of 12 per year during the five years, 1881, 1882, 1883, 1884, 1885. "We know," said M. Pasteur, (in a note communicated to the Academy of Medicine, 2nd Nov., 1887,) "that 60 persons have died of rabies in the Paris hospitals during the last five years. A mean of 12 per year." We give the actual death rate, names and particulars of the hospitals.

1881.

Hôpital Trousseau. Renaut (Henri), died 30th March. *Hôpital Beaujon:* Masse (Alfred), died 30th July. Holu (Alexis), died 3rd November; Martin (Etienne), died 29th December. *Hôpital Lariboisière:* Potier (Edward), died 10th August. *Hôpital de la Pitié:* Becker (Michel), died 22nd June; Cluet, died 16th September. *Hôtel Dieu:* Chicanot (Celestin), died 1st December. *Hôpital des Enfants Malades:* Phlé (Emile), died 23rd July; Fauvet (Charlotte), died 25th July; Rull (Georges), died 27th July.

Total—11.

1882.

Hôpital Beaujon: Pedzer (Emile), died 9th August; Aizieres (Emile), died 19th August; Millot (Victor), died 7th December.

Total—3.

1883.

Hôpital Trousseau: Grucy (Alphonse), died 12th May. *Hôpital Necker:* Lambert (Leon), died 15th August; Huette (Carmille), died 14th August. *Hôpital des Enfants Malades:* Fauque (Severin), died 5th November.

Total—4.

1884.

Hôpital Trousseau: Mathon (Albert), died 13th June. *Hôpital St. Louis:* Paulice (femme Monnet), died 1st March; Matho (Alphonse), died 8th July.

Total—3.

1885.

Hôpital Lariboisière: Bouillet (Eugène), died 19th August; Bonnenfant (Jacques), inoculated by M. Pasteur and not mentioned in the death statistics, died 8th September; Bibiant (François), died 8th September. *Hôpital St. Louis:* Schneider (Pierre), died 14th August. *Hôtel Dieu:* Raffin (Rene), died 18th December.

Total—5.

—

General total—26.

This gives an average of 5·2 as a mean per

year in place of 12 as estimated by M. Pasteur. We are in a position to compare these statistics with other and older statistics as those furnished by M. Bourrell,[1] Veterinary Surgeon. The rise and fall of the mortality in the Paris hospitals is marked in all the statistics—ancient and modern.

Year.	Deaths.	Year.	Deaths.
1865	4	1869	12
1866	8	1870	16
1867	8	1871	9
1868	3	1872	6

I must here emphasize the fact that in certain years in pre-Pasteurian days the mortality has been lower than it now is.

Now we must see what the general mortality in France has been since M. Pasteur's method was introduced; with the number added of those who did not avail themselves of the aid of the Institute.

[1] M. Bourrell. Unpublished MSS. in possession of the Royal College of Physicians, London.

V. PATIENTS WHO HAVE DIED AFTER TREATMENT.

We show in the following tables the full particulars relating to some French patients who have died *from Nov.*, 1885, *to Nov.*, 1889, giving all possible details as to the mode of treatment, the period, and the cause of death, etc.

It has required an enormous expenditure of labour to complete this table, and we are indebted for it to the energy and ability of Dr. Lutaud.

Do these statistics represent all the deaths? I cannot answer in the affirmative, because many patients have left Paris for the provinces, and it has been impossible to trace their subsequent fate. Journals have had to be searched to collect the data here given, and all precautions have been taken to verify their accuracy. The average mortality in France ought to have lowered, during these years, from the same causes operating in England and Germany.

TABLE OF DEATHS AFTER TREATMENT BY PASTEUR, IN FRANCE, FROM 1ST NOVEMBER, 1885, TO 30TH NOVEMBER, 1889.

	Name: Age: Address.	Date of Bite.	Nature of Animal.	Date of Treatment.	Date of Death.	Duration of Incubation.	Notes, etc.
1	Bonenfant, Jacques: Hôp. Lariboisière, Paris.	30th Aug., 1885.	Dog.	Sept., 1885.	1st Nov., 1885.	62 days.	
2	Lepelltier, A. G.: Hôp. Lariboisière, Paris.	3rd Oct., 1885.	„	9th Nov. to 30th Nov., 1885.	4th Dec., 1885.	33 „	
3	Lagut: girl: 11 years: Dole, Jura.	18th May, 1886.	Cat.	24th May to 2nd June, 1886.	17th June, 1886.	31 „	
4	B——ag: 13 years: Paris.	18th June, 1886.	„	25th June to 7th July, 1886.	4th Aug., 1886.	48 „	
5	Christin: child: 12 years: Evian.	29th June, 1886.	Dog.	1st to 12th July, 1886.	16th July, 1886.	18 „	Paralytic hydrophobia.
6	Peytel: 6 years: Poleyneux.	28th June, 1886.	„	30th June to 9th July, 1886.	16th July, 1886.	19 „	
7	Bouvier: 40 years: Grenoble.	30th April, 1886.	Cat.	4th to 15th May, 1886.	21st July, 1886.	84 „	
8	Clediere: 21 months: Bordeaux.	17th June, 1886.	Dog.	21st to 30th June, 1886.	17th Aug., 1886.	62 „	

	Name: Age: Address.	Date of Bite.	Nature of Animal.	Date of Treatment.	Date of Death.	Duration of Incubation.	Notes, etc.
9	Moermann: 40 years: Sarthe.	28th June, 1886.	Dog.	11th to 21st Aug., 1886.	7th Sept., 1886.	72 days.	
10	Moulis: child: 6 years: Paris.	31st July, 1886.	„	5th to 12th Aug., 1886.	8th Sept., 1886.	40 „	
11	Leduc: woman: 70 years: Paris.	14th July, 1886.	„	18th to 28th July, 1886.	10th Sept., 1886.	59 „	
12	Astier: child: 2 years: Paris.	4th Aug., 1886.	„	5th to 21st Aug., 1886.	16th Sept., 1886.	44 „	
13	Videau: child: 3 years: Villeneuve.	24th Feb., 1886.	„	27th Feb. to 7th March, 1886.	24th Sept., 1886.	213 „	Remarkably long incubation.
14	Duresset: 20 years: Paris.	28th Aug., 1886.	„	1st to 17th Sept., 1886.	30th Sept., 1886.	34 „	Paralytic hydrophobia.
15	Grand: 41 years: Paris.	5th Sept., 1886.	„	14th to 28th Sept., 1886.	10th Oct., 1886.	36 „	
16	Clerjeot: 27 years: Paris.	7th Aug., 1886.	„	11th to 27th Aug., 1886.	24th Oct., 1886.	79 „	
17	Edwards: coachman: 34 years: Versailles.	30th Aug., 1886.	„	1st to 12th Sept., 1886.	30th Oct., 1886.	62 „	
18	Sodini: 46 years: Constantine.	12th Oct., 1886.	„	21st to 31st Oct., 1886.	24th Nov., 1886.	44 „	Paralytic hydrophobia.

19	Rouyer : child : 12 years : Paris.	20th Oct., 1886.	Dog.	25th Oct. to 5th Nov., 1886.	26th Nov., 1886.	38 „	Paralytic hydrophobia.
20	Letang · 59 years : Gourgeon.	30th Nov., 1886.	„	8th to 20th Nov., 1886	8th Dec., 1886.	36 „	Paralytic hydrophobia.
21	Nee, Leopold: 42 years: Arras.	1st Dec., 1886.	„	3rd to 13th Dec., 1886.	17th Dec., 1886.	17 „	Bittten outside clothing. Par. hydrophobia.
22	Reveilluc : 25 years : Paris.	4th Nov., 1886.	„	6th to 30th Nov., 1886.	21st Dec., 1886.	48 „	Paralytic hydrophobia.
23	Jansen, Louis: 47 years: Dunkerque.	18th Aug., 1886.	„	21st Aug. to 3rd Sept., 1886.	31st Dec., 1886.	136 „	Paralytic hydrophobia.
24	Gerard : 28 years : Boran, Oise.	1st Dec., 1886.	„	3rd to 21st Dec., 1886.	3rd Jan., 1887.	34 „	Paralytic hydrophobia.
25	Albert : woman : 51 years : Vallouse.	19th Dec., 1886.	„	21st Dec., 1886, to 3rd Jan., 1887.	5th Jan., 1887.	18 „	Paralytic hydrophobia.
26	Goriot, Paul : 12 years : Paris.	30th Nov., 1886.	Cat.	21st Dec., 1886, to 10th Jan., 1887.	14th Jan., 1887.	46 „	
27	Foulup : 30 years : Tour-du-Pin.	1st Dec., 1886.	Dog.	12th Dec., 1886, to 5th Jan., 1887.	24th Jan., 1887.	55 „	
28	Alphaud : woman : 42 years : Paris.	13th Dec., 1886.	„	15th Dec., 1886, to 2nd Jan., 1887.	30th Jan., 1887.	49 „	Paralytic hydrophobia.

	Name : Age : Address.	Date of Bite.	Nature of Animal.	Date of Treatment.	Date of Death.	Duration of Incubation.	Notes, etc.
29	Bergé : 40 years : Bordeaux.	22nd July, 1886.	Dog.	25th July to 15th Aug., 1886.	30th Jan., 1887.	193 days.	
30	Saintis : child : 11 years.	15th Aug., 1886.	„	20th Aug. to 10th Sept., 1886.	24th Feb., 1887.	194 „	
31	Chavagnac, Louise : 13 years : Arlie.	12th Oct., 1886.	„	17th Oct. to 4th Nov., 1886.	10th Mar., 1887.	150 „	
32	Hydjean : 56 years : Salleie.	5th Oct., 1886.	„	9th to 28th Oct., 1886.	17th April, 1887.	195 „	
33	Gachet : 23 years : Vierzon.	4th April, 1887.	„	10th to 29th April, 1887.	2nd May, 1887.	29 „	†
34	Hurot : 42 years : Paris.	29th May, 1887.	„	30th May to 16th June, 1887.	4th July, 1887.	37 „	
35	Bourgeot : 27 years : Audigny.	24th April, 1887.	„	28th April to 15th May, 1887.	11th July, 1887.	79 „	
36	Jeuie : woman : 57 years : Arcezac.	27th Mar., 1887.	„	29th March to 15th April, 1887.	21st July, 1887.	117 „	
37	Desclide : 42 years : La Rochefoucault.	22nd May, 1887.	„	23rd May to 10th June, 1887.	21st July, 1887.	60 „	
38	Valentin : 16 years : Reberrey.	24th Feb., 1887.	„	28th Feb. to 15th April, 1887.	27th July, 1887.	156 „	

39	Saulat : girl : 5 years : Paris.	9th July, 1887.	Dog.	9th to 30th July, 1887.	4th Aug., 1887.	27 „	†
40	Marchois : child : 8 years : Seulis.	3rd July, 1887.	„	5th to 25th July, 1887.	16th Aug., 1887.	45 „	†
41	Penichaud : 18 years : Fontaines.	12th June, 1887.	„	14th to 30th June, 1887.	19th Aug., 1887.	69 „	
42	Jammot : woman : 38 years : Paris.	3rd Aug., 1887.	Cat.	4th to 20th Aug., 1887.	24th Sept., 1887.	53 „	
43	Viai : woman : 70 years : Arles.	29th Aug., 1887.	Dog.	5th Aug. to 20th Sept., 1887.	3rd Oct., 1887.	64 „	†
44	Palu : girl : 8 years ; Codalet.	1st Sept., 1887.	„	6th to 30th Sept., 1887.	10th Oct., 1887.	40 „	†
45	Sintes, Edward.	10th Jan., 1887.	„	25th Jan. to 12th Feb., 1887.	15th Oct., 1887.	240 „	
46	Thierry : child : 4 years : Melleville.	1st Sept., 1887.	„	7th to 24th Sept., 1887.	17th Oct., 1887.	47 „	†
47	Cauvy, Doctor : Beggiers.	30th April, 1887.	„	2nd to 19th Oct., 1887.	3rd Nov., 1887.	35 „	†
48	Valla, Salette : Isere.	8th Nov., 1887.	„	13th Nov. to 15th Dec., 1887.	20th Dec., 1887.	43 „	
49	A. : child : 4 years : Paris.	6th Dec., 1887.	„	12th to 30th Dec., 1887.	22nd Jan., 1888.	48 „	"Medical Press," 21st March, 1888.
50	B. : woman : 52 years : Paris.	23rd Jan., 1888.	„	24th Jan. to 10th Feb., 1888.	17th Feb., 1888.	26	

	Name: Age: Address.	Date of Bite.	Nature of Animal.	Date of Treatment.	Date of Death.	Duration of Incubation.	Notes, etc.
51	Sid i ben Israel.	10th Jan., 1888.	Dog.	20th Jan. to 10th Feb., 1888.	18th Mar., 1888.	69 days.	
52	Marinot: soldier infantry: Paris.	15th Feb., 1888.	„	15th Feb., at 1 o'clock.	1st April, 1888.	47 „	Treated one hour after bite.
53	B.: 54 years: Paris.	9th Nov., 1887.	„	10th Nov. to 1st Dec., 1887.	3rd April, 1888.	„	
54	C.: child: 6 years: Paris.	2nd April, 1888.	„	3rd April.	4th April, 1888.	3 „	Case reported by M. Dujardin Beaumetz, "Journal le Temps," 4th May, 1889.
55	Cotte: 28 years: Thiers.	3rd Mar., 1888.	„	6th to 21st Mar., 1888.	2nd April, 1888.	41 „	
56	Auvray: child: 11 years: Rouen.	12th April, 1888.	„	17th April to 2nd May, 1888.	29th May, 1888.	48 „	
57	Olin: 19 years: Paris.	23rd April, 1888.	„	26th April, 1888.	17th June, 1888.	56 „	
58	Bertin: child: 18 months: Paris.	15th May, 1888.	„	17th May, 1888.	20th June, 1888.	37 „	
59	Villemain: child: 31 months: Marseilles.	9th May, 1888.	„	14th to 30th May, 1888.	23rd June, 1888.	46 „	

60	B. : 28 years : Paris.	4th Dec., 1887.	Dog.	6th to 22nd Dec., 1887.	1st July, 1888	211 „	"Medical Press," 11th July, 1888.
61	Labeaume : 37 years : Chatmay.	29th May, 1888.	Cat.	30th May to 14th June, 1888.	6th July, 1888.	39 „	
62	Poulet : 20 years : Le Pecy Pains.	6th Dec., 1887.	Dog.	26th April, 1888.	15th July, 1888.	223 „	"La Paix," 16th July, 1888.
63	Ducos : 28 years : St. Jean de Bonnefond, Loire.	16th June, 1888.	Cat.	20th June, 1888.	18th July, 1888.	„	
64	Sinardet : 26 years : Porthai, Ain.	26th April, 1888.	Dog.	3rd to 20th May, 1888.	28th July, 1888.	93 „	
65	Mesnil : 44 years : Chatenay.	25th Mar., 1888.	Cat.	26th March to 10th April, 1888.	30th July, 1888.	128 „	
66	Sarazin : woman : 44 years : St. Maurice.	1st July, 1888.	Dog.	4th to 20th July, 1888.	4th Aug., 1888.	35 „	†
67	Guers : 27 years : Chelles.	13th July, 1888.	„	16th to 30th July, 1888.	8th Aug., 1888.	27 „	†
68	B. : 20 years : Paris.	15th July, 1888.	„	16th to 30th July, 1888.	20th Aug., 1888.	37 „	Paralytic hydrophobia. Obser. Cons. Gen. de la Seine.
69	Conzimer : 65 years : Courbevoie pres Paris.	12th Sept., 1888.	„	12th to 30th Sept., 1888.	14th Oct., 1888.	33 „	† Treatment two hours after bite.

	Name : Age : Address.	Date of Bite.	Nature of Animal.	Date of Treatment.	Date of Death.	Duration of Incubation.	Notes, etc.
70	Bourrely : 14 years : Marseilles.	27th May, 1888.	Dog.	7th to 30th June, 1888.	2nd Nov., 1888.	160 days.	
71	Mayland, Rose : 3 years : Bolbec, Seine Inf.	13th Dec., 1888.	„	15th Dec. 1888 to 1st Jan., 1889.	9th Jan., 1889.	28 „	†
72	A. : 19 years : Peyrestates, Pyr. Or.	8th Dec., 1888.	„	9th Dec. 1888 to 4th Jan., 1889.	9th Jan., 1889.	33 „	†
73	Allegre : 14 years : Cambes, Gironde.	25th Dec., 1888.	„	28th Dec. 1888 to 13th Jan., 1889.	14th Jan., 1889.	21 „	†
74	Druant, Melie Blanche : 16 years : Paris.	25th Dec., 1888.	„	26th Dec. 1888 to 15th Jan., 1889.	8th Feb., 1889.	46 „	
75	Druaux : 15 years : Auberville, Paris.	7th Jan., 1889.	„	9th Jan. to 5th Feb., 1889.	11th Feb., 1889.	36 „	†
76	Dufour : 72 years : Veyras.	23rd Dec., 1888.	„	25th Dec. 1888 to 16th Jan., 1889.	13th Feb., 1889.	53 „	
77	Arenes : 50 years : Pyrenées Orientales.	26th Jan., 1889.	„	29th Jan. to 15th Feb., 1889.	22nd Feb., 1889.	28 „	

78	Rey : 9 years : Oissel, Rouen.	Dec., 13th 1888.	Dog.	17th Dec., 1888 to 10th Jan., 1889.	4th Mar., 1889.	82 days.	
79	Mahout : 8 years : Levallois, Paris.	31st Jan., 1889.	"	3rd to 25th Feb., 1889.	9th Mar., 1889.	One year and 38 days.	
80	Rascol : 32 years : Murat, Jarn.	28th Feb., 1889.	„	9th Mar. to 5th April, 1889.	14th April, 1889.	46 days.	Paralytic hydrophobia.
81	Moons : 24 years.	2nd Mar., 1889.	„	7th Mar. to 2nd April, 1889.	17th May, 1889.	77 „	
82	Maillot : woman : 86 years : Coatascorn.	27th April, 1889.	„	3rd to 17th May 1889.	2nd June, 1889.	36 „	
83	B. : 10 years.	3rd April, 1889.	„	5th April, 1889.	17th May, 1889.	45 „	"Etoile Belge."
84	Marais : boy : 10 years : La Neuville, Nancy.	2nd April, 1889.	„	April, 1889.	21st June, 1889.	81 „	"Republicain" de Juin 28, 1889.
85	Coudurier : child : 7 years : Grenoble.	9th June, 1889.	„	14th to 24th June, 1889.	24th June, 1889.	16 „	Died before the end of treatment.
86	Trottet : child : 13 years : Paris.	23rd May, 1889.	„	25th May to 10th June, 1889.	14th July, 1889.	53 „	Obser. published by Professor Lancereaur.
87	Gilbert : child : 11 years : Paris.	18th June, 1889.	„	24th June to 10th July, 1889.	1st Aug., 1889.	45 „	

	Name : Age : Address.	Date of Bite.	Nature of Animal.	Date of Treatment.	Date of Death.	Duration of Incubation.	Notes, etc.
88	Claudel : Nancy, Meurthe et Moselle.	8th July, 1889.	Dog.	19th July to 5th Aug., 1889.	6th Aug., 1889.	30 days.	Paralytic hydrophobia.
89	Sorthaux : 54 years : Bougival, Paris.	5th July, 1889.	„	5th to 30th July, 1889.	13th Aug., 1889.	40 „	Paralytic hydrophobia. Treatment commenced three hours after bite.
90	Auroux, Gilbert : Neris, Allier.	7th July, 1889.	„	9th July to 4th Aug., 1889.	19th Aug., 1889.	44 „	†

NOTE.—Those names marked with a † are reported to have died of paralytic hydrophobia—a form rarely met with in the human subject. I have not sufficient evidence, at present, to confirm this in all the cases, but I have positive evidence of the accuracy of the diagnosis in many of the cases. That they died of Hydrophobia is certain ; the only question is as to the form.

Recapitulation.

1886.—19 Deaths.
1887.—27 Deaths.
1888.—23 Deaths.
1889.—21 Deaths.

Total 90, equal to a mean of 22 per year.

I may deduct 10 to allow for possible mistakes and Algerians, which reduces the number to 80, or a mean of 20 per year.

These cases only represent the deaths of individuals who have been inoculated by M. Pasteur.

To establish the annual mortality of rabies in France, we must add to the above the deaths of those persons who have not been treated at the Institute. According to statistics established by Pasteur himself for 1886, the deaths among the non-inoculated amounted to 17. We must add therefore to the 19 who died after treatment 17 who died without treatment, which gives an annual mortality of 37. We have seen that, according to Tardieu and others, 25 to 30 deaths a year was accepted as the annual mortality of France.

In view of this table well might Professor Peter ask, "Has the average mortality of France lowered? No. Does the mortality even tend to increase? Yes. Where then is the benefit?"

If we look at the above statistics, and examine

the speeches made at the Mansion House, we are forced to the conclusion that the words uttered by some very eminent speakers were prompted by their generous impulses, or feelings of respect and friendship for Pasteur, more than by a strict regard to statistical data. The saving of life has no existence in fact, and absolutely depends on percentages, which may be altered at will or the freak of the speaker.

Sir James Paget fixed the general mortality at 15 per cent. Pasteur, in his article in the "New Review," accepts this, but he thinks it too low; for dog-bites on face and exposed situations he thinks the figures should be "93 and 65 per cent."[1]

If we add up the number who have been bitten on exposed places, and accept these percentages, then Pasteur's saving of life has been much greater, and his "cures" amount to hundreds a year for France alone. In view of the ascertained mortality in France, and the rarity of hydrophobia there, this reduces the system to an absurdity.

VI. THE ANTIRABIC INOCULATIONS.

THE injections made by M. Pasteur or his assistants have varied in their degree of intensity. When we come to consider the theory upon which the efficacy of the injections rests, we find that it

[1] See "New Review," Dec., 1889.

is based upon the belief that rabies depends upon a specific microbe. The saliva of the dog is supposed to contain this, and when he bites he places in the wound some saliva containing the virus—the virus of the dog having a considerable start in its course through the body over the injections. In point of fact, we are absolutely ignorant of the true nature of the virus of rabies. Microbes have been discovered by different observers, but all are as fabulous as the dragon of the fairy tales. The microbe of Föll, the microbe of Pasteur, the microbe of M. Gibier, etc., have all resolved themselves into "airy nothings." The attempt to treat disease by M. Pasteur's method, and to superimpose it as an article of faith on the medical profession by the weight of authority, I have resisted, just as I have opposed the new bacteriological school, which is trying to replace clinical medicine by experimental therapeutics. To kill germs in the peritoneum after grave surgical operations, carbolic acid has been used, and the patients have been killed as well as the germs; to kill the germs in puerperal fever corrosive sublimate was used, with similar results. This is now old medical history. Bacteriology is useful, but only as an adjunct to the science of medicine. The practitioner who has to treat disease, and who is trusted with the lives of the people, must satisfy himself that the methods he adopts or sanctions have some

rational basis. He must not be content with guesses, otherwise called hypotheses. When we come to apply these principles to the actual practice of the injections, we find ample cause to doubt.

M. Pasteur employs a graded series of injections which rest their efficacy on experiments on human beings, some of which have proved fatal, as in the cases of those who have died from the intensive treatment of paralytic rabies.

The formulæ used by M. Pasteur have varied. The boy Meister, who was one of the earliest cases, was treated by the following formula. He was bitten so severely that it is said that M. Grancher and Vulpian diagnosed that he was doomed to die.

The inoculations began on July 7th, 1885, and were 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, „	7
„ 11.	11 „	„ 5, „	6
„ 12.	11 „	„ 7, „	5
„ 13.	11 „	„ 9, „	4
„ 14.	11 „	„ 11, „	3
„ 15.	11 „	„ 13, „	2
„ 16.	11 „	„ 15, „	1

"The medullas or cords which were removed from rabbits and inoculated on the child July 11th, 12th, 13th, 14th, 15th, and 16th respectively —that is, which had been desiccated six, 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."

This formula was changed, as we learn from the Hydrophobia Commission (Appendix, p. 22), to the following :—

Days of inoculation.	1st.	2nd.	3rd.	4th.	5th.	6th.	7th.	8th.	9th.	10th.
Days cord had been dried.	14	13	12	11	10	9	8	7	6	5

M. Pasteur now suppressed in the treatment the most virulent cords, 4, 3, 2, and 1, still, however, professing to cure the thousands who came by a method which produced, as he said, "*pas un absces, pas une phlegmon ; un peu de rougeur oedamateuse seulement a la suite des derniers inoculations.*"

By the experimental method on human beings he established the tolerance of a five-day cord, but as deaths occurred, in September and Octo-

ber, 1886, he adopted another formula. It is to be noted particularly that by the first formula the Pasteur School claims the cure of cases of severe and dangerous bites in patients who have been bitten a long time before the treatment.

The following are a few cases to support this assertion :—

1. The widow Faure, bitten 1st September, is recorded in the series treated by M. Pasteur from the 1st November till 15th December.

2. Lorda, bitten 25th October, was treated 21st November.

3. Martha Wright, bitten 24th January, was treated 14th March, 1886.

4. Richard Smith, bitten 24th January, treated 14th March, 1886.

5. Thomas Gibson, bitten 24th January, treated 14th March.

6. Asa Moore, bitten 24th January, treated 14th March.

7. Turner, bitten 24th January, treated 14th March.

8. James Hostey, bitten 14th January, treated 14th March.

9. Stephen Barker, bitten 25th March, treated 21st to 30th April.

10. Ann Sharp, bitten 6th December, treated 8th January.

In these cases the remedy, according to Pasteur, was of course effectual, even though the

patients came after more than the eleven days! M. Pasteur himself, however, recognized that deaths were occurring, and he altered the formula, making it more intensive.

Days of inoculation.	1st.	2nd.	3rd.	4th.	5th.	6th.	7th.	8th.	9th.	10th.	11th.
Days drying of cords.	14.13.12	11.10.9	8.7	6.5	4·3	2	1	6 5	4·3	2	1

Under this method several patients died in France in 1886 of paralytic rabies, whilst Goffi, of the Brown Institution, bitten by a cat, also died of the same affection. An attempt was made by the British Hydrophobia Commission to attribute the death of Goffi to the virus of the cat. The "British Medical Journal," July 2nd, 1887, at once exposed this fallacy. "This statement," it said, "however, appears to go near to begging the whole question, for the matter in dispute is the behaviour of the intensive virus in man, and a most important element is the duration of incubation. The unusual nature of the symptoms also requires explanation, for the suggestion that the cases hitherto described under the term 'acute ascending paralysis' are in many instances examples of the dumb or paralytic form of rabies in man, rests upon this single observation of Mr. Horsley's, and ignores the fact that a large proportion of such cases recover." That M. Pasteur himself shares to

some extent the apprehensions which have been expressed, is shown by the fact that he soon modified his intensive method. The tolerance of this system by the other patients may be compared to the tolerance by the human subject of the natural virus of the rabid dog, just too as it was found possible for patients to stand injections of corrosive sublimate and carbolic acid without causing death.

Days of inoculation.	1st.	2nd.	3rd.	4th.	5th.	6th.	7th.	8th.	9th.	10th.	11th.	12th.	13th.	14th.	15th.
Days drying of cords.	14.13	12.11	11.10	10.10	9.9	9	8	8	8	7	7	7	6	5	5

Paralytic rabies in the human subject being an almost unknown disease, Professor Peter, at the Academy of Medicine, Paris, raised his voice against the system, and, as the Hydrophobia Commission says, "when it appeared possible that it *might be* dangerous (p. 22, *loc. cit. ante*)," M. Pasteur changed it for that which he now uses, and which is printed at the side :—

The three formulæ above mentioned indicate very clearly the empirical[1] nature of the prophy-

[1] "Empirical" is the word used by Dr. B. Ward Richardson in Eng-

lactic, and I would ask my readers to bear them in mind.

VII. THE NUMBER OF ENGLISH PATIENTS.

In 1885 M. Pasteur treated two English patients only. What became of the other dog-bitten patients who ought to have gone to him during that year? Was the annual mortality in England swelled? The answer is clear, and given by the death-rate. In 1886 the number who flocked to Pasteur ran up owing to the influence of fashion and to the impulse given to the movement by the Press and some members of my profession, and I take the figures of 1886 from M. Pasteur's letter to the Lord Mayor :—

"We have had under our care," he says, "88 of your countrymen in 1886."

In 1887, owing to the deaths and to opposition, the number ran down to 23. Let me ask, What becomes of the calculation of the English Commission?

In 1888 the number ran up again to 63, with 35 in the first five months of 1889, "making a total of 210, or an average of 50 for each year."

Taking the percentage of the Hydrophobia

land. "The empirical method of M. Pasteur," he said, "is wanting in scientific control."

Commission at 5 per cent., and taking the English mortality estimated by the same Commission at 40, what has become of the 860 persons in all England who ought to have gone to M. Pasteur each year, calculated to require the treatment?

During these years, according to the Hydrophobia Commission, M. Pasteur ought to have had 3,440 English patients. The number from London alone should have been 680; but we have seen that M. Pasteur has only had altogether 210 English patients, so that for their own credit's sake the English Hydrophobia Commission are almost pledged to beat up patients in order that their calculations may be sustained. They never would have made this calculation had they read the able lectures on medical statistics published by Dr. John S. Billings, of the Surgeon-General's Office of the United States Army; they fell into the error so ably exposed by the Editor, "New York Medical Journal":—"It is utterly wrong to reason from facts obtained which apply to masses, when the individuals composing the masses are to be considered. Suppose that in a given disease 10 per cent. die, as shown by large masses of figures, say 100,000 cases. Does this mean that the chances in any one case can be represented as one in ten that death will result? Not at all. The chances in that case may be enormously in favour of the

patient's recovery or of his death. It would be as foolish to assume that the average represented the actual chances, as to think that it indicated the death of one-tenth of each person, instead of one-tenth of the mass."

Out of the series of cases published by the British Hydrophobia Commission there died,—Henry Colling, Smith, Goffi, Arthur Wild, Lord Doneraile (not returned by English Commissioners as dead). Martin Cahill, Albert Kirkham, Fred. Lindley, and others died subsequently.

The statistics of the English Hydrophobia Commission are imperfect and misleading, even though they are divided into three classes, A, B, C. For instance, the child Tattersall is tabulated in the English report in column B—*Dog certified to be rabid by veterinary surgeon.* The fact is, the dog was never seen by a veterinary surgeon, as it bit the child and ran away, and was never traced. I attended the child on her return from Paris, as she was in a high state of excitement and nervousness.

If we take the Hydrophobia Commissioners' statistics, we find a list of those bitten by dogs in class A. Here there is another error, for Arthur Wild was not bitten at all, but was hurt by a man named Oates, and ought not to have been returned as having been bitten by a rabid dog. In the whole history of hydrophobia there

is no case on record of the communication of hydrophobia from one human being to another, though there are numerous instances recorded of man biting man, so that there was not the slightest necessity to send Wild for treatment. The symptoms that Wild suffered from on his return were anomalous, but I do not very strongly insist upon classing him as amongst the victims who owed their death to the new treatment.

VIII. EXAMINATION OF THE SYSTEM.

"THE most remarkable point in the whole discovery against rabies," said M. Roux in his Croonian Lecture, "is that it has been carried out, the virus itself being still unknown; not only do we not know how to cultivate it outside the body, but in allowing it to be really a microbe we can but do so by analogy, for as yet no one has been able to isolate it. . . . . There is no stronger example of the power of the experimental method applied to medical matters than this one of the prevention of a malady the absolute virus of which is still obscure." *Primâ facie* it would seem illogical and unscientific to inject a virus, about which you know but little, in order to counteract a poison about which you know less; and fortunately for the credit of

medicine in modern times, this form of medication is but rare. In my book on hydrophobia I disputed the soundness of the view supported by some eminent men in regard to hydrophobia, "that in the presence of a malady which constantly ended in death, the physician was justified in trying anything." We are not the arbitrators of life and death, we are not justified in reckless experimentation on man or the lower animals, and we cannot use the deadly poisons which pharmaceutical science places at our disposal in the reckless manner advocated even by some of the most eminent men, so that I have no hesitation in stating that M. Roux' assertion is not in harmony with the temper of medicine in modern times. M. Pasteur in his experiments on human beings with the artificial rabies of the laboratory has found out that human beings can stand injections up to five days with almost absolute immunity, and that the spinal cord injections produce no symptoms of disease if confined to that length of time; but he has also established the fact, that by using the cord of one day, the patients are exposed to danger, though some even have a tolerance for it, just as some patients are safe after the bite of a rabid dog.

The dangerous nature of the one day injection, as we have seen, compelled M. Pasteur to greater prudence, and in the last formula given he does not use stronger injections than those of five days.

As the air has been full of hypotheses, I hazarded one of my own to account for the want of effect of M. Pasteur's first method, viz., that most of the injections were "sterile" when injected subcutaneously, as into the abdomen of a human being; that is to say, through various causes unknown, the injections did not have any effect, and no disease was produced, a theory supported by the failure of some of the cords in the case of the boy Meister. Thus only could I account for the singular facts that :—

1st. Patients not bitten by dogs underwent the process for the sake of experiment without any result.

2nd. Patients bitten by rabid dogs were also treated in the same way, as well as those licked by dogs.

3rd. Patients bitten by non-rabid dogs submitted to the treatment without injury. The microbe, if microbe there be, perished in the subcutaneous tissue by digestion or resolution—stillborn as it were.

Here I must allude to another strange statement of the English Hydrophobia Commission. Pasteur's treatment has been compared by them to vaccination, but it bears no resemblance to the method of Jenner. In vaccination, after the introduction of the virus, we have definite symptoms, culminating on the eighth day in the pock on the arm, which after that date resolves itself

into a scab, then fades away, leaving in its place the well-known cicatrix of vaccination, so that it is a misuse of terms to apply the term vaccination to the new method of inoculation. In Pasteur's process the virus is introduced after another virus has been a varying time in the system, and after it has had a start; and it is introduced rapidly, without our knowing anything of the regular laws of absorption with such viruses. Thus, for instance, a preparation of a 14 days cord is used, and again the same day another 14 days old cord. Time is required in the human economy for any physiological process to take place. Even admitting M. Pasteur's theory, we know not whether the virus and the excretory substance, given in daily emulsions of dried cords, have time to permeate the system.

XI. THE DEATHS.

THE death of Lord Doneraile, the particulars of which were kindly furnished me by a member of the family, and the explanations given for it by M. Pasteur, open out the whole question as to the causes of failure. "Lord Doneraile allowed an interval of eleven whole days to elapse," said M. Pasteur, "from the time the bites were inflicted till the beginning of the treatment.

In addition to this, Lady Doneraile and the medical man who sent her husband to us, insisted that only the simple treatment should be applied, and not the modified method which I had been led to adopt, especially in the case of severe bites."

"Professor Grancher and M. Roux yielded to the desire which was so warmly expressed; several inoculations were practised, without using medullæ of more than *five days' drying. Carried out under such conditions, the treatment* could only, alas! allay the *development of the rabic virus for four or five months.*" (Letter of M. Pasteur, published in "British Medical Journal," September 17th, 1887.)

Now what shall we say of this letter? Shall we say that it shows an absolute want of good faith, or that it supposes that medical men cannot reason? M. Pasteur had been using the five days injection on patients who were bitten as severely as Lord Doneraile, and who had come to him a longer time after the bite; and these persons figure in his returns amongst the cured. Lord Doneraile's servant was bitten eight hours only sooner than his master, but he has been "cured." What a cruel deception for Lady Doneraile, who built her hopes on the published cases, and on Pasteur's "successes." What shall we say of Pasteur's pretensions, "that he *delayed* the sad event for four or five months"? Lord

Doneraile was bitten very severely on both hands by a pet fox. The coachman was bitten eight hours sooner than Lord Doneraile by the same fox. Was the coachman treated in the same way? The delay in the treatment! when M. Pasteur treated the Russians by the same process, who came after a longer period. Let us see the results in some of the cases that came within the prescribed time.

1. Jamot, bitten on the arm by a cat, went next day, and was under treatment one month; she died.

2. Penichaud went next day, and for fifteen days underwent the intensive process; he died.

3. Marinot, a soldier of the French army, went within an hour after having been bitten; he died.

4. Deché, bitten through a *gros pantalon*, on the 20th May, treated on 21st May, 1887; he died after fifteen injections.

5. Eugene Palu, bitten on Sept. 1, 1887, treated from the 6th to the 4th of Oct.; died on the 10th Oct.

6. Dr. Cauvy, bitten on the hand by a little dog, was treated at once; he also died.

7. Revaillac, coming under treatment two days after bite, was treated by the intensive process; he died.

In the above seven cases of death, and in others already given in my tables, all the conditions of speedy recourse were fulfilled, and yet

we have to chronicle fatal results, so that, *in the face of this necrology*, Lady Doneraile and the medical attendant may feel no compunction or regret at their having insisted on the use of cords no older *than five days.*

This letter of M. Pasteur is one of the most remarkable in the whole history of the controversy. The British Hydrophobia Commission assured the English public that M. Pasteur was only using cords five days old, in accordance with the formula we have already published, and on the faith of their authority some of the British public went to the Institute. What confidence can we repose even in Pasteur's good faith to them! But we have not done with this letter. I have quoted M. Pasteur's own words as given in a letter published in the "British Medical Journal," as to the cause of failure of the antirabic inoculations in the case of Lord Doneraile, viz., that Lord Doneraile came eleven days after his bite,—too long a period for effective treatment.

In view of this positive declaration, repeated by M. Pasteur's admirers in England, what shall we say of the following words of M. Pasteur in the "New Review," p. 626, December, 1889?

"It is never too late to begin the treatment, as, if not treated, the odds are all against the patient. On the other hand, it is quite evident that the chances of the treatment being successful increase largely if the bites be of very recent date;

the danger, if a long time elapses between the date of the bite and the time in which the treatment is begun, consists in the *fact* that rabies may suddenly appear in the process of treatment."

This "fact" is a new explanation for failure, but one not verified by experience of M. Pasteur's patients. Rabies did not appear in Lord Doneraile's case while he was undergoing the treatment, nor in the others who came over that time. We have tried to reconcile the conflicting statements of M. Pasteur, but this last one is past reconciliation.

We continue the quotation :—

"Madame Luisa Caressa came all the way from Spain to our Antirabic Institute. She had been bitten nearly one year before, on Sept. 15th, 1888, by a dog. . . . . A few weeks have now passed since the last inoculations were made on her. *The latter will, no doubt, prove just as efficacious as if she had undergone the process immediately after being bitten in* 1888."

Is this scientific evidence or logical evidence? Is it even consistency? Madame Caressa was frightened, as the dog bit a man, who died from hydrophobia ten weeks after the bite. M. Pasteur can promise her safety, though, in the case of Lord Doneraile, he could only "delay the fatal event four or five months."

I cannot better expose some of M. Pasteur's hypotheses, or theories—more correctly called

guesses—on the deaths, than by putting them in the form of propositions.

Proposition I.—"There will be time for patients to come to Paris from all parts of Russia." (Letter of M. Pasteur.)

Proposition I. negatived by the subsequent deaths, and by the following statement in M. Pasteur's recent article in "New Review," Dec. 1889, p. 623.

Proposition II.—"How could it be possible, after fourteen days had elapsed, to find a remedy, when the whole body had already been permeated by the virus?"

Proposition II. negatived in the very same article, by a statement which I put in form.

Proposition III.—"It is never too late to begin the treatment, as, if not treated, the odds are against the patient."

Proposition III. negatived by Proposition II. and Proposition IV. What is the use of treatment if Proposition II. be correct, or if Proposition IV. be maintainable?

The odds are calculated on the "heads I win, tails you lose" principle.

If you come over fourteen days or eleven days, and if you do not contract hydrophobia, you are "cured." If patient dies, the *deus ex machinâ* "too late" is invoked.

Proposition IV.—"Lord Doneraile died because he came too late—eleven days after

bites." (M. Pasteur's Letter to "British Medical Journal.")

Proposition IV. negatived by Proposition III. Still more by his "cures" of persons who came a longer time after being bitten.

Madame Luisa Caressa treated a year after bite with perfect success.

Proposition V.—"Lord Doneraile died because weak cords were used, none older than five days."

Proposition V. rejected by the report of English Hydrophobia Commission, which assured us that M. Pasteur had now altered his formula "to the one he now uses" (see p. 33), viz., five days cords. Still more contradicted by the "cures" reported as resulting from the use of the very same formula used on Lord Doneraile.

Proposition VI.—"The danger of delay consists in the *fact* that rabies may *suddenly* appear during the process of treatment." (See "New Review," Dec. 1889.)

Proposition VI. negatived by the fact that hydrophobia appeared in nearly every case after the treatment, in one case 231 days after.

But why multiply the contradictory propositions which have, from time to time, emanated from the Pasteurian School? It is useless to argue with men who throw aside all the rules of logic.

You cannot pin them to one statement or fact; for when pressed they shift their ground—one fallacy is exposed, and another springs up in its place.

The above propositions are fairly put; the intelligence of the public is sufficient to judge of their contradictory nature.

X.—APOLOGIES FOR FAILURE.

When M. Pasteur's method was first introduced at the Academy, it was received with such enthusiasm and faith as are only accorded to miracle workers. M. Pasteur's infallibility was assumed by his disciples. "Rabies, that terrible malady, has at last found its remedy," said one admirer; "we have a method which *a coup sûr* prevents the disease," said another. Now we have M. Pasteur exclaiming, in his letter to the "British Medical Journal," "One cannot be expected to perform miracles." We did not expect anything of the kind, but we did, at least, expect consistency in a man of science.

The fatal cases awoke the Pasteurians from their dream. The crowd of "obscure blasphemers," composed of such men as Jules Guerin, Peter, Bouchard, Collin, Lutaud, etc., who questioned the efficacy of the method in France, had a difficult task at first; but the deaths at last enabled

them to obtain a hearing. The failures carried out under every condition demanded by the master of the system, told their own sad tale. M. Pasteur did not at first tell us that he was experimenting on human beings; that was not his attitude or the attitude of his school. It was one of dominant assertion and affirmation, in which opponents were stifled down by the breath of authority. Before even time was allowed for the proper incubation of the malady and for the proper testing of the effects, the pæan was sung by MM. Charcot, Vulpian, and others; and whatever modification has been brought about in this treatment has been due to those who had the courage to challenge such premature conclusions. M. Pasteur has run down the changes in the method which "he dared to call perfect," and which could "be adopted any time before the appearance of hydrophobia." He ran down first to thirty-five days, and then again he had to reduce the time, so that even eleven days became, in his opinion, too long, though he contradicts himself in his latest utterance on the subject, intended for popular readers.

"There is no necessity," he said, at one time, "to establish a Pasteur Institute in Russia, there is time for patients to come from all parts of this empire;" and Russians came to test "the perfect method," but they died. Then, when challenged and forced to explain, he had the

aplomb to say, "wait, time will reveal many facts connected with this question. It is only by continued experiments and careful observations carried on for a considerable time, in hundreds of cases, that we shall be able to arrive at positive and definite results"! The late Vincent Richards, who for twenty years was engaged in the study of snake poison, very justly observes: "Well may we rub our eyes and exclaim, Are we awake, or do we dream? Does he tell the scared creatures who flock to him that they are being experimented on, to enable the world of unbelievers to arrive at positive and definite results? The unbelievers asked him for proof, and he has advanced these assumptions and assertions. . . . But the unbelievers know by this time quite enough of the elasticity of M. Pasteur's logic to convince them that he sniffs the enemy in the field."

XI. THE AWFUL EXAMPLES.

At meetings in favour of the extension of the system of Pasteur to England, awful examples are held up of the consequences of neglecting to resort to the inoculations. A child or a man is bitten. He does not go to Pasteur, and he dies of hydrophobia. Another, bitten by the same dog, undergoes the treatment and is cured.

This is very telling, and might help to bring up patients. But there is a reverse side to the picture. Two or more persons are bitten by the same animal, the patient who resorts to Pasteur dies, whilst the other subjects, untreated by his method, do not suffer. What shall we say of this evidence? Let me give instances.

A postman was bitten on the right leg through the pantaloons; another man was bitten on the right leg by the same dog on the 20th February, 1889. The postman was sent to the Institute, the other man remained at home. The postman was treated for fourteen days, but returned to the country to die of paralytic rabies; the man who remained at home is perfectly well. This case is well known, and reported by the medical man who attended the patient, Dr. Victor Rascol, Murat, Jarne.

Another remarkable case was reported by the "Progrès Médical," edited by Br. Bourneville, December, 29th, 1886: "A game-keeper in the department of Sarthe was bitten by a dog; the result of the *post mortem* was to establish that the dog was not rabid. However, the man was sent to Pasteur and inoculated. This man died subsequently of hydrophobia." "Le Progrès Médical" observes: "the question was asked, was the dog rabid, or did the inoculations produce hydrophobia?"

The two sons of a peasant from Dordrecht,

Holland, bitten by a cat, were both treated by M. Pasteur. One died subsequently from hydrophobia, the other was cured. What was the value of the treatment—what was the percentage in this case?

Need I go on with the reverse side of the picture not presented at pro-Pasteurian meetings?

Another device is to emphasize the cases wherein children have been reported to have been bitten by rabid dogs, and wherein a large number have died, and the effect of all this exaggeration is to produce a panic and a fear of dog bites which has been unknown before the Pasteurian era. Let me also give here the reverse of the picture in the case of seven persons bitten on the same day by a rabid dog.

On June 14th, 1884, Edward Dobson, William Ashworth, Mary Hoyle, Thomas Broadhurst, John Crossly, J. Chadwick, and Asquith, were bitten by a rabid dog. Not one of these persons have suffered any after effects. Another striking case is the following, reported by me at the time in "Medical Press and Circular":—

"In April, 1881, a rabid retriever was seen rushing up Hunslet and New Wortley, Leeds, biting all with whom he came in contact, and before he was killed by a policeman nearly twenty persons are known to have been bitten by the rabid brute. One unfortunate fellow, named Thomas Mann, was walking quietly along Huns-

let Lane, when he was attacked by the animal, which approached him from behind. It jumped up to the back of his head, but failing to get hold there, it rushed at him a second time, and seized him by the thigh. The dog then rushed on, and bit another person before reaching Thwaite Gate, when it turned back, and again proceeded along Hunslet Lane, snapping and biting at every one it approached. The dog proceeded up Church Street, where it bit one or two other persons severely. It seized one man by the hand, and inflicted a serious wound between the finger and thumb, and afterwards bit him on the leg, tearing his trousers to pieces. Another young man was suddenly seized by the left hand, which was bitten through, and his leg also lacerated considerably. Indeed, so serious were the poor fellow's wounds, that after having them attended to by a doctor, he was deemed unfit to go to his work. Three other men, named Joseph Hutchinson, Samuel Haigh, and William Green were also savagely attacked by the brute, all being bitten in the leg and thigh. Another workman named Wm. Shaw, who was walking down the street, was placed in a terrible plight. The animal unexpectedly rushed at his back so violently that he was thrown to the ground face downwards, and whilst he was in that position the unfortunate fellow was severely bitten on both legs before he could effect his

escape. Several other persons were bitten more or less severely in Hunslet, the dog in its mad course snatching at every one it approached. No one appeared able to stop it or put it out of harm's way, and it rushed wildly on to New Wortley, where ultimately it was killed by Police-constable Spencer, who courageously attacked it with his staff. He also got possession of another dog which had been bitten by the mad one, and had it poisoned without delay. Altogether it is feared that nearly twenty persons have been bitten by it more or less seriously."

I have made all possible inquiries, and no ill results have ever been reported, as following from the bites of this animal. Had Pasteurism been in vogue, these persons would have been sent to Paris and "cured."

I could give other instances, but it appears to me like crushing a fly on a wheel to pile up any more evidence.

XII. THE EXPERIMENTS ON ANIMALS.

IF the experiments on human beings were the same as the experiments on animals, such as dogs, they would be comparable. We do not know what might have been the result if M. Pasteur had treated his human patients in the

same manner as he treated the monkeys, dogs, and rabbits, but there is no analogy either in the method of experimenting or in the results. The dogs were not inoculated subcutaneously in accordance with formulæ based on the same lines as the formulæ for human beings. The virus was introduced into the dog, as we know from Pasteur's own words, directly by trepannation. How then can we compare what is not comparable? Experiments on animals are not always safe guides as to what will be best for human beings—moreover, as the behaviour of the virus in the monkey, dog, and rabbit are different, we have another potent reason for objection.

The experiments on animals in connection with rabies are really marvellous—that is the only word.

Peyraud of Leybourne performed a series of experiments on animals which eclipse all that Pasteur has done. Peyraud's experiments were tested by the Academy of Medicine and verified.

He treated four rabbits with the oil of *Tanacetum vulgare*, and then inoculated them with the virus of rabies, but no symptoms of rabies manifested themselves even nine months after, though two test rabbits were killed with the same virus used for the experiments, and used in the same way. Experiments on animals have, without doubt, produced the most surprising

results, proving to us that matter in the wrong place, or in altered conditions, with a new environment, will yield new effects. But the conclusions to be drawn from these experiments when applied to human beings, under other and altered conditions, surely cannot be pressed into the service.

The experiments on animals by M. Pasteur have yielded different results in the hands of Von Frisch, De Renzi, Amoroso, from those of the English Hydrophobia Commissioners.

Von Frisch applied M. Pasteur's more perfect method of experimentation.

Is it not enough to make all reasoning minds in the profession pause, before accepting the Pasteurian theories, to hear the opinion of Dr. Klein, one of the Inspectors of the Local Government Board:—

"In the first place," says Dr. Klein, "the method of inoculation as practised by M. Pasteur on the human subject, *i.e.*, subcutaneous inoculation—intracranial injection not having been, and for obvious reasons not being likely to be employed—is not sure of success, if as a basis for such a proceeding the knowledge gained by animal experiments is to be relied upon.

"For in the series of experiments made by Von Frisch in Vienna, and by Dowdeswell at the Brown Institution, it is clear that the results in the rabbit or the dog are altogether dis-

similar, for whereas in the latter species this method did lead to protective inoculation in some animals, in the first species, *i.e.*, the rabbit, it produced true rabies. M. Pasteur's protective inoculations in man also led, in a certain percentage of cases, to fatal results.

"Secondly, the question of the degree of attenuation which the virus of the cord undergoes by drying, although established for the rabbit's cord, is not established as far as its application to the human subject is concerned.

"Pasteur's and other methods of using the cord of rabid rabbits in successive degrees of attenuation, starting with the use of a higher and proceeding to a lower degree of attenuation, either slowly or rapidly (intensive treatment), is tentative; and for this method no firm and clear basis is as yet available, since no one knows what is the minimum and what is the maximum degree of attenuation of rabbits' cords *quoad hominem*.

"Thirdly, another uncertain factor in the application to man of the protective inoculation is the fact that the incubation period of rabies in man exhibits such a conspicuous difference in the different cases, as is well-known. Persons bitten by rabid dogs and seized with hydrophobia show a wide range of incubation period.

"If, as Pasteur assumes from his animal experiments, a difference in incubation period indi-

cates a different degree of virluence of the rabic poison, then no assumed degree of attenuated virus could rationally be recommended for protective inoculation, since the duration of the incubation period in a given human case is obviously a matter of conjecture.

"Fourthly, but above all, the intimate nature of the rabic virus being as yet unknown, no definite and conclusive insight into its *modus operandi* is available."

Grave words these, and full of meaning! Need I say another word on the animal experiments?

XIII. WHY THE BITTEN ESCAPE.

THOSE who are bitten escape because bites are made through clothing; because the virus cannot find a proper environment in which to live; because the dog may have exhausted the virus by previous bites, and the teeth be clean; because it may be, as M. Pasteur says, "that the saliva contains together with the microbe of hydrophobia other microbes of various kinds which may give rise to morbid complications and thus prevent the occurrence of rabies." ("New Review," 1889.)

M. Pasteur confirms the very old observation or fact, "that if dogs are bitten by rabid animals the disease does not appear in all of them. A

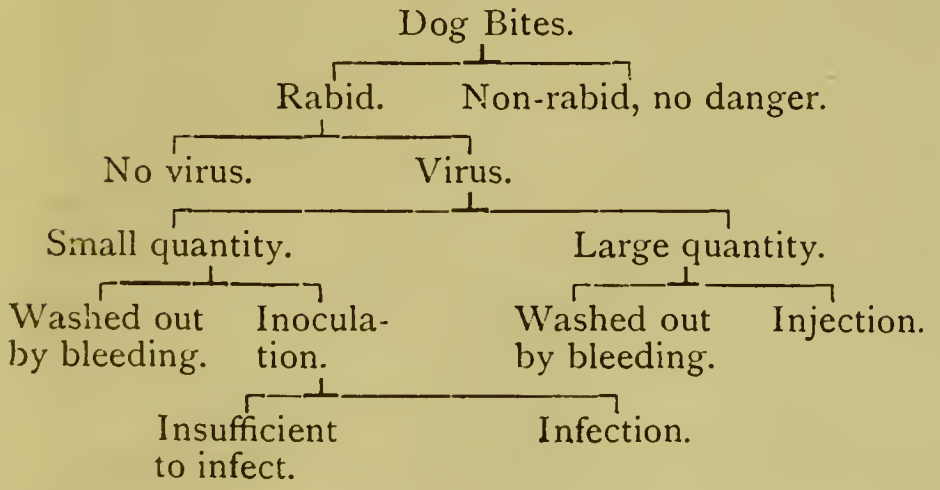
direct subcutaneous inoculation of the saliva of a rabid dog is hardly ever successful." ("New Review," p. 909, Nov. 1889.)

Hertwig established this fact long before Pasteur. He inoculated fifty-nine dogs, of which number only fourteen contracted the disease. A young mastiff for three years resisted all his attempts to induce the disease.

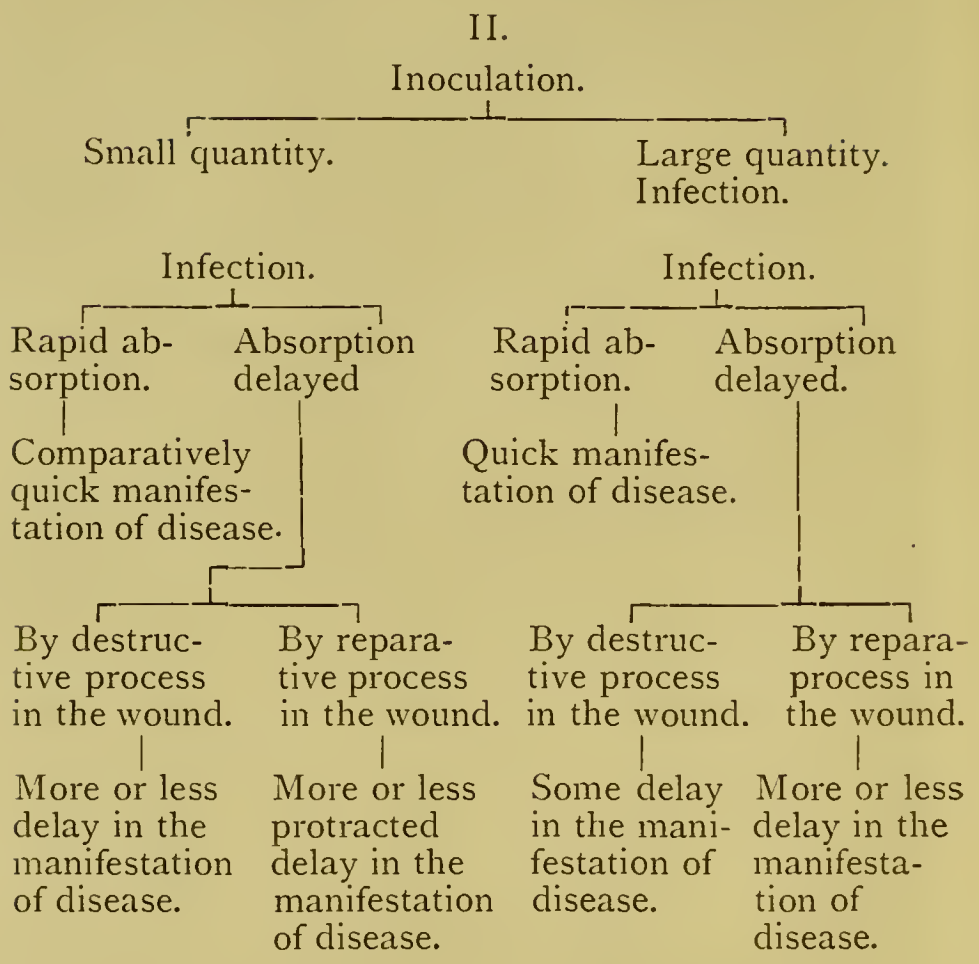
The saliva of the rabid dog is variable. It infects sometimes by the slightest scratch or abrasion, as when the dog licks the lips, so that the severity of the bite is not an indication that "the intensive treatment" is required.

The reason of the saliva failing to infect will be readily gathered by a glance at the following table.[1]

RABID OR NON-RABID, I.



[1] Prepared by the late Vincent Richards, F.R.C.S.



XIV. THE MORTALITY AFTER DOG- AND WOLF-BITES.

In order to bring into stronger relief the work of Pasteur, the mortality and danger after bites has been exaggerated; and as the consequence of this exaggeration dog-bitten patients have lately been more terrified than ever. Hydrophobia has been in the air for some years, and

the dog has been looked on with less favour. As an instance of how statistics are thrown into strong relief I may mention M. Pasteur's evidence on the mortality after wolf-bites, about which he has given statistics, dating back from the years 1706, 1806, 1811, 1822. This is old medical history. We give some more recent and trustworthy evidence. Dr. Kishensky[1] states cases, selected from the archives of the Katharine Hospital in Moscow. The whole number amounted to 693, and from this number, excluding cases bitten by other animals, 591 were bitten by mad dogs, and only eight died, or 1·35 per cent. But the author asserts that in this manner statistics cannot be compiled, because about many of the cases it was not known whether they had been bitten by rabid dogs, and therefore he omits all such cases and gives only those about which there was no doubt of their having been bitten by unquestionably rabid dogs. Some of them remained in the hospital under medical observation at least six weeks, but the greater part of them three months and longer.

By these statistics it is shown that out of 307 persons bitten by unquestionably rabid dogs, 18 were bitten in the head, 90 in the hands, 25 in the feet, 170 in places covered with clothes, and 4 in places not indicated. Of 18 bitten in

[1] See "Provin. Med. Journal," 1889.

the head 4 died, or 22·2 per cent.; of 90 bitten in the hands, 2 died, or 2·2 per cent.; in 25 bitten in the feet, there was no death, and of 170 bitten through the clothes, 1 only died, or 0·59 per cent. If we add to the deaths another, belonging to the 4 about whom it is not known where they were bitten, we have 8 deaths, or 2·6 per cent.

Out of 24 cases severely bitten by rabid wolves, 2 arrived at the hospital with the symptoms of hydrophobia; 5 were in the hospital during six weeks only, 4 of them were discharged in good health, and 1, very severely bitten, died of septicæmia. The remaining 17 cases were under observation during eight weeks. Of this number 11 were bitten in the head, 3 in the hands, and 3 in places not indicated; 5 of them died, showing a mortality of 30 per cent.: but, according to Pasteur, it is 62 per cent. All those who died had extensive wounds on the head. The author concludes that a greater number of persons died from the bites of wolves than from those of dogs, because the wounds of the former are larger and more numerous. Of 18 cases bitten by rabid cats, only 3 died. Of 17 cases bitten by rabid horses, 9 were in the hospital during 3 months, and none of them died of hydrophobia; but 1 died of erysipelas, and another of septicæmia. Of 4 bitten by a rabid dog, none fell ill. To this number we must add 4 cases bitten by rabid men, 1 by a

white bear, and 1 by a rabid squirrel. Thus, the whole number bitten by rabid animals—396—there died 18, or 4·52 per cent.

XV. THE POLICE AND HYDROPHOBIA.

IT is a very remarkable fact, and one which ought to reassure the timid, that the Metropolitan Police, who have been for years engaged in the seizure of dogs, have been absolutely free from hydrophobia. The evidence on this point is convincing. Mr. Sewell, Veterinary Surgeon, was examined by the Select Committee of the House of Lords, 6 July, 1887.

Question 489. *Earl of Miltown.*—"I suppose a large number of the police are badly bitten in taking up dogs?

A. "They have been bitten, but they have been very fortunate as a rule.

Q. 490. "Has there been any case of hydrophobia amongst the police?

A. "Never.

Q. 491. "Are any precautions taken when they are bitten?

A. "Two or three men were sent over to M. Pasteur last summer. (Why?)

Q. 492. "But before that, were there any precautions taken?

A. "They were merely *cauterised.*"

Sir Charles Warren, in reply to question 689 (*loc. cit. ante*), said :—

"I have had a great many men bitten by mad dogs, some of the men have had their hands covered with bites.

Q. 686. "Can you give us any returns of the number of constables who have been bitten in the discharge of this particular duty (seizing dogs) and the number of those who, in the discharge of this duty, have been bitten by mad dogs and have died ?

A. "None of our constables have died from bites. I sent 7 over to Paris to be treated by M. Pasteur. In August, 1885, there were 8 constables bitten; in September there were 2; in October there were 2; in November there were 20, and in December there were 38; in January, 1886, there were 25; in February 31; in March 24; in April 14; in May 20; in June 17; in July 19; in August 16; in September 10, in October 4: in November 3; in December 3. (Total 186.)

Q. 785. "The fact remains, that among the police who are engaged in the discharge of their duty in killing rabid dogs, there never has been a case of hydrophobia ?

A. "That is so."

The evidence of Mr. Colam, Secretary, etc., to the Dog's Home, Battersea, is remarkable,

and I regret that I have not space to insert it here. It is similar to evidence published by me in 1879. I do not for one instant want it to be believed that all the constables alluded to by Sir Charles Warren and others were bitten by rabid dogs, but there cannot be a doubt that among the thousands and thousands of dogs seized, some were rabid. The point I desire to emphasize is, that in a class most exposed to dog bites, hydrophobia has been markedly absent. Another point is that the police were bitten by the same class of dogs as many of the patients who went to Pasteur—wandering dogs. Had all the police gone to Pasteur the statistics would have been swelled, and M. Pasteur's apparent success would have been, perhaps, under his five days' formula, still more pronounced.

XVI. THE RARENESS OF RABIES, AND THE POST MORTEM SYMPTOMS.

I HAVE contended that rabies is a rare disease, though we have epizootics of the malady. Most veterinarians support this view.[1] This point is to be noted. We have statistics of rabies in France, and we have some evidence as to the number of persons bitten in some of the depart-

[1] See Minutes of Evidence, House of Lords Committee, *passim*.

ments, as, for instance, that of the Seine. It is contrary to previous experiences to find such a number of patients bitten by *rabid* dogs in such a short time as that embraced by Pasteur's work. As the authority for the state of the dog, included by Pasteur in Class B., rests on the opinions expressed by veterinary surgeons on *post mortem* examination, we must consider what is the value of this evidence. We give a few opinions.

Dr. George Fleming, Veterinary Inspector-General, was examined by the Select Committee of the House of Lords.

Q. 891. "No inspection by *post mortem* examination would tell whether the dog had been mad or not, would it?

A. "Not with certainty.

Q. "Is there nothing to distinguish between epilepsy and madness?

A. "Nothing."

Mr. Atkinson.—*Q.* 1154. "Do you think many dogs are killed and treated for hydrophobia when it has been another disease?

A. "Hundreds of cases.

Q. 1155. "What is the way of ascertaining?

A. "It is almost impossible to diagnose rabies by *post mortem*."

M. Pasteur's evidence on this point is, that the most eminent veterinarians may be mistaken, and that the only test is his own experimental one. I need hardly bring any more evidence on this subject.

In M. Grancher's returns, March 1887, we find it stated that, amongst French subjects, there were "1538 bitten by animals in which rabies was diagnosed experimentally or by veterinary observation—deaths of human beings, 16. Persons bitten by animals suspected, 321, deaths 2. A total of 1,929 French patients." It is not only incredible that the "suspected" should be in lower porportion to the other class, but in face of the opinions given by Pasteur, Fleming, and others, it is wanting in scientific control and consistency to have a class B, because, as we see, according to the best evidence, *post mortem* examination is a *worthless test* as to the existence of rabies.

XVII.—COMPARATIVE TABLES FOR THE COUNTIES AND GENERAL ENGLISH DISTRICTS.

We would ask our intelligent readers to look at the following Tables, and to weigh them. It will be clearly seen from them what each one's individual chance is of dying from hydrophobia. In certain parts of England, it will be observed that the chances amount to *nil;* but it will be further observed that the incidence of deaths falls on certain localities. Common sense points out that it is in those localities where the high

death-rate has occurred that we must give our particular attention and endeavour to find out what are the causes at work which produce in those localities such a death-rate. In my evidence before the House of Lords I pointed out how the mortality from hydrophobia was increased in 1869 by the diagnosis of medical men. I am strongly of opinion still that the suggestion I made in 1877 should be adopted, viz., that an official diagnosis of hydrophobia should be made by one of the officials of the Local Government Board. The majority of practitioners are unacquainted with the disease, as mania, delirium tremens, meningitis, and other affections have been mistaken for it. There is just ground for my suggestion. Though I have seen in consultation a number of cases, I shall probably not see a case again. The following Tables speak for themselves :—

Hydrophobia, Deaths and Death-Rates per Million in each County of England, and in North and South Wales, during the Twenty Years, 1864—83, *comprising* 707 *deaths.*

Lancaster . . .	214	3·6	Northumberland.	14	1·8
Chester	30	2·7	East York . . .	10	1·6
Buckingham . .	8	2·6	Derby	11	1·6
West York . .	96	2·5	Extra Metropo-		
Durham . . .	29	2·1	litan—Surrey .	12	1·5
Nottingham . .	15	2·1	Shropshire . . .	8	1·5
Metropolitan—			Stafford . . .	26	1·5
Surrey . .	29	2·1	Extra Metropo-		

litan — Middlesex	9	1·4
Metropolitan—Middlesex	6·2	1·3
Leicester and Rutland	8	1·3
Hereford	5	1·3
Extra Metropolitan—Kent	15	1·2
Sussex	10	1·2
Oxford	4	1·2
North York	7	1·2
Cambridge	4	1·0
Wiltshire	5	1·0
Essex	9	1·0
Bedford	3	1·0
Hants	9	0·9
Somerset	7	0·7
South Wales	0	0·6
Dorset	3	0·5
Devon	6	0·5
Cornwall	3	0·5
Cumberland	2	0·5
North Wales	4	0·5
Berks	2	0·4
Suffolk	3	0·4
Metropolitan—Kent	2	0·4
Warwick	5	0·4
Worcester	2	0·3
Gloucester	3	0·2
Norfolk	2	0·2
Monmouth	1	0·2
Lincoln	1	0·2
North Hants, Huntingdon, Hereford, Westmoreland	0·0	0·0
England and Wales	707	1·5

Figures Showing the Relative Prevalence of Rabies in the Counties of England and Wales. Average from 1870 *to* 1885.

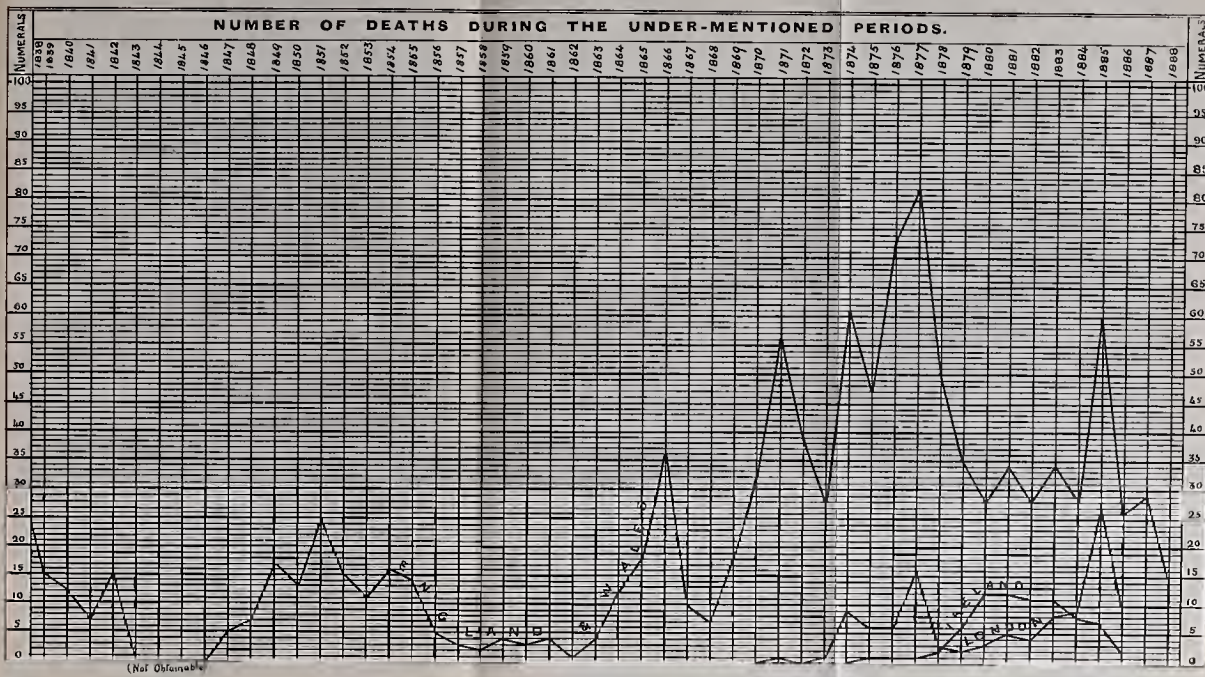
Lancashire	12
Yorkshire	6
London	6
Staffordshire, Cheshire, Durham	2
Derbyshire, Nottingham, Middlesex, Kent	1
Surrey, Yorkshire, East Riding, Yorkshire, North Riding, South Wales	1
Northampton, Shropshire, Leicestershire, Warwickshire	½
Oxfordshire, Buckingham	½

Essex . . .		Worcestershire .	
Somerset . .		Gloucestershire .	
North Wales	$\frac{1}{2}$	Berkshire . . .	
Hertfordshire		Norfolk . . .	Nominal
Devonshire		Suffolk	
Wiltshire .	$\frac{1}{3}$	Dorset	
		Cornwall . . .	

These tables may be seen in the appendix to the "Report of the House of Lords." They require no interpretation, but they may be compared with the following Table, which I drew up in 1880, from evidences furnished me by the Chief Constables of England in reply to a circular I issued. My Table is imperfect, but it affords us a means of judging how far the police, during the year 1871—1877, caused the destruction of rabid dogs—that is to say, suspicious dogs. It will be noted that the number is not very large, except in the centres where hydrophobia caused alarm.

The following diagram may be advantageously studied. It is prepared from figures courteously furnished by the late and present Registrar General. The fluctuations in the death wave is noticeable even in days long antecedent to the inoculation craze. The rapid drop from 1877 ? may be compared with the fall from 1855 ?.

Diagram, showing the Deaths from Hydrophobia in England & Wales, Ireland & London, from 1837 to 1888.



SUMMARY OF RABID DOGS THROUGHOUT ENGLAND, FROM 1871—1877, KILLED BY THE POLICE.

England: Division and Counties.	No. of supposed Rabid Dogs.								No. and Period of Manifestation.												Species of Dog.									
	1871.	1872.	1873.	1874.	1875.	1876.	1877.	Total.	January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.	Retrievers.	Greyhounds and Lurchers.	Setters, Springers, Pointers.	Spaniels.	Sheep Dogs.	Fox and other hounds.	Carriage Dogs.	Terriers.	Bull Terriers.	Crosses and other Breeds.
England.....	91	26	70	143	137	119	168	806	38	29	53	52	92	102	76	58	42	40	39	134	152	25	34	21	81	57	3	192	42	201
York Division :—																														
West Riding	84	20	61	126	121	68	28	510	35	29	42	47	76	67	58	36	30	30	33	27	94	10	25	8	30	10	1	143	30	148
East Riding	..	1	2	2	1	..	1	7	1	..	1	..	2	1	2	..	..	..	...	..	..	..	..	..	1	..	3	3	1	2
North Riding	..	2	1	..	..	..	1	4	..	..	2	..	..	1	..	..	...	..	...	..	...	Not	obtai	na	bl	e ..	..	..	..	..

The reasoning public will be wise to consider this evidence and oppose the formation of a Pasteur Institute in England, calling to mind the words of Dr. George Fleming, Veterinary Inspector-General :—" When we might suppress the disease altogether in this country, it would seem worse than foolish to keep it always with us, with its terrors, risks and inconveniences—and have to, at the same time, either send bitten persons (we could not well send animals) to Paris to be protectively inoculated, or to provide one or more expensive establishments on this side of the Channel for this purpose, in which rabbits must be dying from the disorder every day, all the year round, in order that their spinal cords might be prepared for inoculating some chance person who had been wounded by a mad or suspected dog. Such a procedure would not look very sensible, or even humane, so far as the rabbits are concerned at least." (" *Nineteenth Century*," March, 1890.)

It is to be noted that, wherever a Pasteur Institute has sprung up, there the number bitten by *rabid* dogs has increased; this is seen by reference to the figures published by M. Pasteur himself. We should look at the action of Germany and Belgium. The German Empire has been able to deal with the subject of hydrophobia, not by establishing Pasteur institutes, but by sanitary and prophylactic measures. The scientific atti-

tude of the profession in Germany is significant. Germany is in the first rank of Science. We have statistics of institutes established by young pupils of Pasteur at Odessa and other places on the outlines of civilization; but such examples should hardly excite us to emulation.

XVIII. PARALYTIC RABIES.

IF we carefully examine the tabulated statement of deaths, we are forced to the conclusion that, not only does M. Pasteur not protect from the disease, under the very conditions demanded by himself, but that he has added a new terror to it, by the introduction of paralytic rabies.

The paralytic form was almost unknown, now it is common.

CONCLUSION.

THE failure of the system is attested by the deaths.

Whole hecatombs of animals have been ruthlessly sacrificed in the quest after the virus, and well might Vincent Richards condemn the

slaughter which has taken place without, as we assert, benefit to the human race, nay, even to its injury. A new terror is now added to the bite of the dog.

The good old Dr. Berkenhout, writing about rabies in 1783, told us that he knew not of any human attempt which had more resemblance to the Knight of La Mancha's tilting at a wind-mill, than that of combating popular errors and reasoning against popularly received opinions. I have been at times inclined to accept this view, and have felt inclined to let popular fashion expend itself.

When I first criticised the method of Pasteur, what I said was received with incredulity and positive disfavour, but as time went on and many of my predictions were verified, the incredulity gave place to greater tolerance in regard to opinions expressed against the system. There has been a complete change of front, the infallibility of the method has been abandoned, its apologists adopting another tone.

"Pasteur's system was not perfect, no system of therapeutics was perfect. Pasteur would be an angel and not a man if he could at one coup bring rabies into subjection." "Give him time," says another. Then the law of averages is appealed to. "Pasteur had reduced the mortality from 15 per cent to 1 per cent," and so the apologists vary their tones. Very different in-

deed is the present attitude from what it was when Jules Guerin was howled down because he dared to question the method. The intolerance of M. Pasteur was never better manifested than when he had the audacity to question the competency of one of the first clinicians in France, to pronounce an opinion on the method because, forsooth, he was not an experimenter on animals. Had Professor Peter joined in the chorus, clinician though he was, he would have been perfectly competent. To Dr. Michel Peter the world owes the first exposure of the dangers of the intensive method. It required great courage in France on the part of Dr. Peter, Dr. Lutaud, and others to oppose the stream, and had they not been actuated by a pure love of science and of medicine, they would have been silenced.

The difficulty in obtaining an accurate return of the deaths has been very great, because many patients have left Paris to die in the Provinces. Dr. B. Ward Richardson here in England, with a few more, have alone dared to express their opinions on the treatment.

I have been met with the objection, that as I have not carried out experiments on animals similar to those of M. Pasteur, I was not competent to pronounce an opinion. Had I agreed with all that emanated from the Pasteur school, I should have been considered highly qualified. I am totally opposed to the dominancy of ex-

perimental therapeutics, and I refuse to accept the animal experimentalist, as my guide in the method of medicine I practise, and I have no hesitation in saying that progress in medicine cannot be made and has not been made purely by experiments on animals. There were brave men before Agamemnon, and there were great physicians, great clinical teachers, before the school of animal experimentalists ever originated. The intolerance and dominancy of the new school in France, exemplified in the case of Professor Peter, must bring about its own downfall.

During late years, the clinical observer has been pushed on one side by the men who are supposed to be engaged purely in "original research," the men who work simply and solely in laboratories, who look through microscopes at infinitesimal organism or who apply complicated instruments to curarized frogs to measure their heart beats, or who take up the more fashionable pursuit of bacteriology, as if the clinical observer were not equally entitled to claim that he was engaged in original research. As the consequence of fashion, we have a vast literature of the modern school, but unfortunately not valuable in proportion to its vastness. It is a mass of undigested, crude material, produced in accordance with the wants of the market. Theories have been started, based on imperfect

experiment, and these have been in turn superseded by newer theories. Bacteriological research being encouraged by endowments and by the fashion of the day, naturally has thriven; it was to be the key to unlock all the secrets of medicine.

I need hardly remind my medical readers of the *furore* that was created by the application of chemistry to medicine. Chemistry was to be the open Sesame; we had only to analyze the various parts of the body to find out the various constituents of blood, bile, etc., and then, knowing the component parts in health and disease by means of physiological chemistry, we should be able to build up new tissue.

Physiological chemistry had its uses, and has been of benefit to medicine, but that it did not fulfil the expectations raised by it, is proved by the establishment in our own days of the new system working on new lines. The mistake we now make is that in place of making bacteriology the handmaiden of clinical observation, we put it in the position of mistress. This may sound medical heresy. The chemist and the bacteriologist may do us some service, but they must be kept in the place of servants. Laboratory experiments on dead tissue or on living matter out of place cannot alone solve for us the problems of life or disease. We must accept the discovery of the comma and the bacillus tuberculosis, but

we must not deceive ourselves with the belief that because we have discovered these organisms we thereby know all about cholera or consumption.

Have we any treatment that is satisfactory, based on these discoveries? There can be only one answer to this question, No. The clinical observer has been very patient, just as he was patient when he was threatened with displacement by the chemist, and perhaps for this reason, that he could afford to wait, as he knew that his time would come again. If we read medical history we shall find that the greatest advances have been made by clinical research, and by the bedside observation of disease—the facts being interpreted by great minds.

Professor Peter in one of the great clinical observers of the day. A worthy successor of Trousseau, true to his education, to his experience and allegiance to the true experimental method, he has tried to deliver medicine from the reign of terror formed by the coterie, which, in the name of science, anathematized all who ventured to doubt. "You are unscientific," said the coterie, "you do not believe in our methods of modern research, and you cannot have a hearing."

This kind of language has silenced many, because when there is a fashion, men foolishly imagine that they may be looked on as progressive men if they go with the tide. Martyrdom

is not so eagerly sought after, and one is socially ostracized when one appears for a time to be in a minority as Prof. Peter was at the Academy.

Clinical observers may take heart, there are some signs of the lifting of the cloud, and of the emancipation of medicine from the trammels of what has been so well called "Vaccinomania."

Our motto must be:—*Pax et Scientia sed Veritas sine Timore.*

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