COMMENTS

ON

PASTEUR'S METHOD

OF

TREATING HYDROPHOBIA

BY

CHARLES W. DULLES, M.D.

Fellow of the College of Physicians, and of the Academy of Surgery of Philadelphia; Surgeon to the Out-Patient Department of the University of Pennsylvania, and of the Presbyterian Hospital in Philadelphia, etc.

Reprinted from THE MEDICAL RECORD, *February* 13, 1886

NEW YORK
TROW'S PRINTING AND BOOKBINDING COMPANY
201-213 EAST TWELFTH STREET
1886

COMMENTS ON PASTEUR'S METHOD OF TREATING HYDROPHOBIA.[1]

I HAD intended, as the notice of this meeting states, to discuss this evening the whole subject of the treatment of hydrophobia. But I have found that I could not cover so much ground at one time, and so I have decided to confine myself to that phase of the question which has been forced upon the notice of the whole world by the recent announcement of the discovery of a preventive method by M. Pasteur. If this method were trustworthy, it would eclipse anything which has ever before been accomplished in dealing with this mysterious and dreaded disorder. If, on the other hand, Pasteur's method is not to be trusted, it can only prove an obstacle to any real advance in our knowledge of a subject already too much encumbered with error.

For this reason it seems to me to be important that such a body as this should critically examine the evidence adduced in support of this method; so that if it appears to rest on a sound basis of reason and experiment we may adopt and recommend it; while, if it appears to be founded on untrustworthy experiments and unsound reasoning, we may reject and condemn it, in the interest of humanity as well as of science.

M. Pasteur's first communication on the subject of hydrophobia was made in a discussion before the Académie de Médecine, of Paris, January 18, 1881, when he announced that on December 11, 1880, he had inoculated two rabbits with saliva (or buccal mucus) from the mouth of a child supposed to have died of hydrophobia. The rabbits died in thirty-six hours, and in their blood he found a microbe in the shape of a figure 8, which he declared to be a new one, and to be the cause of a new disease (*Bull. de l'Acad. de Méd.*, January 18, 1881).

M. Colin, of Alfort, said this microbe had been described and figured ten years before, and that Pasteur should have searched for it elsewhere than in the saliva of persons dying of hydrophobia. He also said that

[1] Read before the Philadelphia County Medical Society, January 13, 1886.

Pasteur should have made control experiments to show that the inoculation of other sorts of saliva would not produce the so-called "new disease." To this Pasteur promptly replied with an interruption: "These experiments have been made without giving any results" (*loc. cit.*, p. 78).

Pasteur made his second communication at the meeting held the following week, January 25th, in a "Note sur la maladie nouvelle provoquée par la salive d'un enfant mort de la rage" (*Bull. de l'Acad. de Méd.*, 1881, pp. 94–103). In this communication he says that he had cultivated his new microbe and repeated his inoculation experiments with the cultivation as well as with the blood, a great number of times, the result being *always the same.*

These positive and unqualified declarations of Pasteur soon proved to be utterly erroneous. All that Colin had affirmed, and all that Pasteur had denied, was soon shown to be true. The microbe was not new; it was not peculiar to rabies; and the inoculation of other kinds of saliva, even that of a healthy adult, produced the same results. Pasteur himself had to acknowledge this in a letter dated March 22d, only eight weeks after his vehement and bitter repudiation of Colin's objection (*Bull. de l'Acad. de Méd.*, 1881, pp. 380, 381), and, as we know, his acknowledgments have been confirmed in this city by the experiments of Drs. Sternberg and Formad.

Pasteur's third communication was read May 31, 1881. It was entitled "Sur la rage," and was founded on experiments made with MM. Chamberland, Roux, and Thuillier (*Bull. de l'Acad. de Méd.*, 1881, pp. 717–719). In this communication he declared that "up to the present time the disease has not been communicated by the inoculation of the blood of those having rabies" (*loc. cit.*, p. 717).

In saying this Pasteur (who has never seemed aware of anything done by other experimenters, except a very few of his own immediate time and country) was, of course, utterly ignorant of the admirable and accurate experiments of Hertwig (published in 1828, in a supplement to Hufeland's *Journal*) in which the blood of mad dogs, rubbed into cuts made in healthy dogs, caused their death, after a typical incubation, in two instances, one of the dogs dying "fully mad." But he ought to have remembered that at the very meeting at which he had made his first essay in regard to hydrophobia, MM. Raynaud and Lannelongue made a detailed report of experiments, in which they had got their inoculating materials from the same patient which had supplied Pasteur with his, and that inoculations of the blood of a rabbit, dying after inoculation with the medulla oblongata of the child that had died of hydrophobia, killed two other rabbits, while the blood of one of these latter caused the death of a third. This, according to Pasteur's own standard of proof, constitutes a demonstration of what he calls the "virulence"—that is, the specific rabic nature of the inoculation material.

This evidence of ignorance on the part of Pasteur was followed up with an implied claim, which has grown stronger since then, that he and his assistants first considered the central nervous system as peculiarly involved and active in the development of the disease. Without discussing the value of this claim, or of the idea, it is interesting to observe that the evidence of its correctness rests upon Pasteur's statement that on inoculating the brain-substance of a mad dog upon the surface of the brain of another dog, by trephining, the symptoms of rabies appear after "a week or two," and that the dog *dies* in less than three weeks (*loc. cit.*, p. 718). Now, what Pasteur means by the "first symptoms" of rabies may perhaps be surmised from his statement in another place: "Nothing is more varied than the symptoms of rabies. Each case of rabies has, so to speak, its own peculiar ones" (*Bull. de l'Acad. de Méd.*, 1882, p. 1442); and again (speaking of rabbits), he determines the onset of the disease "by a change of temperature" (*Bull. de l'Acad. de Méd.*, 1884, p. 343). It is true that he says, in the communication of which I was speaking, that he got "sometimes dumb madness and sometimes furious madness," and that "none of the inoculations made in this way have failed;" but he omits to mention how many experiments he had made, or what proportion of them produced either form of rabies, or what he means by these terms.

The fourth communication of Pasteur in regard to hydrophobia was a paper entitled "Nouveaux faits pour servir à la connaissance de la rage," in which the names of the same assistants appear, and which was read December 12, 1882, nineteen months after the last (*Bull. de l'Acad. de Méd.*, 1882, pp. 1440–1445).

In this he says that heretofore "the saliva was the only material in which the presence of the virus of rabies had been established," and that the inoculation of this was uncertain and tedious in its results. By experiment he had found that the central nervous system was the principal seat of the virus; that here the latter can be gathered in large quantity and "in a state of perfect purity," and that its inoculation on the surface of the brain causes rabies "promptly and surely." At the same time he declared that he and his assistants had "found the same advantages, with forms of rabies slightly different, in another method still more easily applicable, intravenous injection of the virus" (*loc. cit.*, p. 1441). To his assertion that the virus is found in the brain, he adds the statement that it is also found in the spinal cord, and often (note: it is only often, not always) in all parts of the cord. By inoculating either brain or cord on the surface of the brain, or by intravenous injection, rabies, he says, can be developed promptly and surely (p. 1443). By the latter method (intravenous injection) he got a rabies in which "early paralysis often occurs; fury is often absent; rabic howling is rare;" but, as a set-off to the lack of these characteristic signs of real rabies, he gravely asserts, "there is sometimes frightful itching" (!). He had also seen his animals with experimental rabies spontaneously recover; and, in some cases, seem to get well, and then have a recurrence of the disease two months afterward and die. (In passing, I may remark that such a course is diametrically opposed to all the history of this disease.)

In this communication we have also the only statement which I can recall as to the sources from which Pasteur has obtained his virus, viz., two dogs in 1880, since which time, he says, rabies had been kept up (*entretenue*) "without interruption" in his laboratory. At "different times" they had utilized dogs which had died of rabies in the Veterinary School at Alfort; and very recently they had received the head of a cow which had died on a farm "in consequence of bites received from a mad dog." It is interesting (as Pasteur says) to know that all the animals inoculated from various parts of the brain of this cow had already died of *rabies*. The sum of his experiments at this date was over two hundred, including dogs, rabbits, and sheep (*loc. cit.*, p. 1445).

Pasteur's fifth communication in regard to hydrophobia was made on February 26th, 1884 (fourteen months later), in his own name and in that of MM. Chamberland and Roux. It was entitled "Nouvelle communication sur lar age" (*Bull. de l'Acad. de Méd.*, 1884, 337–344). In it he announced the theory that the difference between the form of rabies produced by intravenous inoculation and that produced by inoculation upon the surface of the brain, was due to the fact that in the former case the virus first becomes fixed and multiplies in the spinal cord. When he killed dogs at the moment of the "first symptoms of paralysis" (a most vague limit, especially when determined by such a man as Pasteur) he found that the cord, especially at the lumbar enlargement, might be "rabic" when the medulla oblongata was not yet affected (*loc. cit.*, p. 333). He claims that he had already demonstrated (we might say asserted) that the virus had its seat in the brain and cord, and that later he had found it in the nerves and in the salivary glands. The whole nervous system, he now declares, is capable of "cultivating" the rabic virus. But all his efforts to "cultivate" the virus of rabies artificially had failed.

When asked by M. Bouley if there were no *microbe* of rabies, Pasteur answered that he could only say that if shown two brains, one healthy and the other of a rabid animal, he could say from a microscopic examination: "This one is rabic; that one is not;" adding, that "both (of these brains would) exhibit immense numbers of molecular granules, but those of the rabic medulla are smaller and more numerous, and one is tempted to believe in a microbe of infinite smallness, having neither the shape of a bacillus nor that of a *microcoque étranglé*; they are like simple points." This very vague description of the supposed germ of hydrophobia was not even original; for M. Béchamp, of Lille, soon showed that he had already studied, and described under the name of "microzyme," what Pasteur spoke of as if *he* had first recognized it.

The next thing which attracts attention in this communication of Pasteur is the reassertion that inoculation by intravenous or hypodermic or intracellular injection is followed by paralytic rabies; coupled with the paradoxical statement that furious rabies can be produced "on the sole condition of using *very small* quantities of virus." "The less virus," he says, "employed for hypodermic or intravenous inoculations, the more readily is furious rabies obtained."

Now, in addition to the difficulty of comprehending how intravenous inoculation of small quantities of virus can produce the furious form of rabies, and larger quantities the mild form, what becomes of Pasteur's explanation of the production of the milder form by "fixation" as he calls it, of the virus in the spinal cord? If the ordinary quantities become "fixed," why do not small quantities become fixed too? Again, he says that the employment of small quantities of the virus may prolong considerably the length of the incubation, and that by pushing the dilution beyond a certain limit, which is not very high, inoculation of the virus is without effect (*loc. cit.*, p. 340). Now, in this same communication Pasteur declares that he judges the virulence of a virus by the relative shortness of its incubation. So he stands as asserting that a virus used in very small quantity is relatively weaker (as manifested by a lengthened period of incubation), while in the same breath he demonstrates that it is relatively stronger, since it produces more easily the most furious forms of rabies! In this connection we must not overlook the fact that these contradictions have been followed in his last communication on the subject by the astounding assertion of Pasteur that in his preventive inoculations in human beings the attenuation consists not in a reduction of the virulence (the key-stone of the former theories), but in a reduction in the *quantity* of the virus employed.

Again Pasteur here mentions an experiment in which a rabbit after inoculation by trephining had paralysis (*rabic*, he calls it) thirteen days later, and then recovered entirely; but forty-three days afterward the paralysis returned and the animal died in three days. The same, he says, he has also seen in dogs.

Here we note three discrepancies: 1, That inoculation by trephining did not produce the characteristic form of rabies *promptly and surely*, as Pasteur has so often said it will; 2, that it did not produce *furious* rabies at all! and, 3, that the rabies "recovered" and recurred—a thing which never happens in natural rabies.

It was at this time that Pasteur advanced the ingenious theory that inoculation of the hydrophobic virus in different animals (in series) produced a fixed degree of virulence, differing with the species of animals used, and declared that he had found "the practical way to obtain dogs refractory to rabies, in numbers as large as one could wish." He couples this statement with the reasonable assertion that to do this solves the question of the prevention of rabies in dogs and in man. But if this was true in February, 1884, how can we account for the fact that in the two years which have since elapsed not a single dog has been rendered refractory to rabies outside of Pasteur's laboratory?

Pasteur made his sixth communication in his own name and in those of MM. Chamberland and Roux, to the Academy of Medicine, May 20, 1884. It was entitled "Sur la rage" (*Bull. de l'Acad. de Méd.*, 1884, pp. 661–664). In this he announced that he believed he could "surely bring about a refractory state in (human ?) subjects before the mortal disease breaks out, in consequence of a bite (*loc. cit.*, p. 663)." To a reporter of the *Figaro* he said at this time, "anybody bitten by a mad dog has only to present himself at the laboratory of the École Normale, and by inoculation I will make him completely insusceptible to the effects of hydrophobia, even if bitten subsequently by any number of mad dogs." And, again, "Whoever gets bitten by a mad dog has only to submit to my three little inoculations, and he need not have the slightest fear of hydrophobia." If we pass by the boastfulness of these statements and confine ourselves to a study of the ground upon which they rest, we find that it can be resolved to this: Pasteur claims that when he "extracts" his virus from a mad dog, and inoculates it by trephining upon a monkey, and then from monkey to monkey, it diminishes in intensity. Virus obtained by similar successive inoculations in rabbits increases in intensity. He obtains his attenuated virus, then, by inoculating a series of monkeys (and it is a lucky coincidence, since monkeys are scarce, that it requires only a few monkeys to get what he wants); after which he can grade it up to any point he chooses by transferring it to the rabbit, in series. Thus, by working first backward and then forward, he comes by an attenuated virus, with the weakest power of which he inoculates at the same time a dog and a rabbit; the second inoculation of the dog and a new one in a third rabbit is made from rabbit No. 2; a third inoculation of the dog is made from rabbit No. 3.

"By this time" (*ensuite*), Pasteur claims, "the dog is entirely refractory to rabies, either by intravenous inoculation or by trephining with the virus of dogs with ordinary hydrophobia" (*loc. cit.*, p. 662). In trying to understand this, we ask in vain how he extracts his virus? from what part of the animal he gets it? how he judges that his virus is getting stronger? since he had before asserted (p. 343) that it required a *series* of transmissions to fix the virulence of inoculated virus, and that before reaching this "fixed" point it "*varies ceaselessly.*" This certainly would imply more than three experiments before he could assume any trustworthy diminution or increase in intensity. I have figured it out that if he started with a rabbit dying after an incubation of ten days (the very shortest limit supposable), one experiment would require thirty-two days, four rabbits, and a dog. If his monkeys, be supposed to enter into his practice, as well as into his theory, this limit would rise to not less (for three, we will say) than about three months in all, assuming that the weakening of the virus is judged in monkeys also by lengthening of the period of incubation. How many such experiments can we imagine that Pasteur has ever practised. I say "imagine," for he has nowhere given any hint in regard to it.

Once more, this communication contains the following sentence: "By inoculations of the *blood* of rabid animals, in determined conditions, I have succeeded in simplifying considerably the operations of vaccination, and in securing for dogs the most decided refractory state" (*loc. cit.*, p. 663). Here I call your attention to the appearance of blood upon the scene, and the fact that it simplifies matters so much; whereas, we have seen that Pasteur at first denied the inoculability of the blood, and we shall see that, without giving any reason whatever, he soon switches off to the spinal cord, and we hear of this "simplifying" blood no more.

Pasteur's seventh and last communication on this subject is boldly entitled "Méthode pour prévenir la rage après morsure" (*Bull. de l'Acad. de Méd.*, October 27, 1885, pp. 1431–39). In this, after a complacent announcement of the value of his earlier discoveries, he confesses that his previous method would only render refractory to rabies fifteen or sixteen out of twenty dogs. To ascertain the fact of refractoriness requires not less than three or four months, he says, which restricts very much the application of the method—and, I may add, indicates how few experiments he could have carried out to their conclusions.

He had, therefore, attempted to discover a method which he could "dare to call *perfect.*" "After experiments, so to speak, innumerable, I have," he says, "discovered a preventive method, which is practical and prompt, the successful applications of which to dogs are already numerous enough and sure enough for me to have confidence in its general applicability to all animals and to man himself" (*loc. cit.*, p. 1432).

This method may be summarized as follows: An attenuated virus is obtained by inoculating a rabbit, by trephining, with "rabic spinal cord" of a dog dying of ordinary rabies (*rage des rues*), and then a second rabbit with the spinal cord of the first, and so on in series. After a *very long* series it is found that a "virus" is obtained which kills rabbits in seven days. When this point is reached pieces of the spinal cord of one of the victims are removed "with precautions of purity as great as it is possible to secure," and suspended in small flasks in which the air is kept dry by a piece of caustic potash. With each day that it is kept such a piece of spinal cord becomes less virulent.

The treatment consists in taking a small piece of one of these cords and "dissolving" (*délayer*) it in sterilized veal-broth, and injecting a Pravaz syringeful under the skin of the dog. The age of the piece of cord used must be such that it does not endanger the life of the subject of the experiment. How to ascertain the proper age Pasteur says he knows from experience; but unfortunately he forgets to say how any one else may decide the matter (*loc. cit.*, p. 1433). The effect of this treatment Pasteur, when he made his report, had tried on one human being, the now famous Joseph Meister, nine years old, bitten July 4th by a dog supposed to be mad. His bites were numerous. The principal ones had been cauterized the same day with carbolic acid. At an autopsy the dog's stomach was found to contain hay, straw, and bits of wood; and on this fact alone the diagnosis of rabies in the dog rests to this day. Pasteur called Dr. Vulpian and Dr. Grancher to see the boy, and they said he was almost inevitably exposed to contract hydrophobia, "*on account of the severity and the number of his bites.*"

"The death of this child appearing inevitable," Pasteur then decided, "not without keen and cruel solicitude," to try his new method on him. He inoculated the boy with a half syringeful of spinal cord (he says, meaning no doubt diluted or *délayé*) fifteen days old. He made in all twelve hypodermic injections in ten days, each day using a fresher cord, and then sent the boy home cured, having "escaped not only the hydrophobia which his bites might have developed, but that with which I had inoculated him, to test the immunity due to the treatment, a hydrophobia more virulent than that of street dogs" (*loc. cit.*, p. 1436).

In commenting upon this communication there are two sets of objections to be raised. One relates to the case of the boy Joseph Meister, which has attracted such attention the whole world over. The other relates to the general statements made, regarded from a scientific standpoint.

In regard to the case of the boy, it may be briefly stated: 1st, Because there is no proof that the dog that bit him was mad (everybody ought to know that the contents of a dog's stomach are of no value as evidence of rabies), and because the boy's wounds had been cauterized, there is no reason to assume that he was in danger of having hydrophobia; and, 2d, if he was, it is by far too soon to say he is free from the danger.

When we come to compare the assertions in this communication with the evidence in support of them, we do not wonder that the prudent Jules Guérin begged the unwilling President to let him utter a word of protest before this announcement should go out to the world with the sanction of the Academie de Médecine. As usual, Pasteur, in this communication, speaks of his last experiments as "always" successful (*loc. cit.*, p. 1432). Again, his experiments have been "innumerable, so to speak." Unfortunately, here as elsewhere (with one exception), the actual number is not named, and his vague statement will bear analyzing. The conclusions at which he has arrived could only properly rest on a number of *uninterrupted series* of experiments, each complete and successful from beginning to end. Now, I have taken the trouble to figure out what a single series would involve, and I find it means no interruption and no failure in experiments requiring one hundred and thirty or one hundred and forty rabbits, and a period of from about two and a half to nearly three years! An interruption anywhere would break up a whole series. Now—supposing no interruption had occurred—how many such series could Pasteur have been carrying on during the three years since, he says (*loc. cit.*, p. 1432), he began them? Again, what seems to me the most fatal objection to the idea that Pasteur's experiments could possibly have been trustworthy throughout these immense series is the fact that, by his own admission, *a full half* of the spinal cords, used in the crucial experiment on Joseph Meister, which gave him such "cruel inquietudes"—

as well it might—*proved to have no virus* when tested on rabbits! Out of eleven, he says, five were without virus, five were virulent; and of one, singularly enough, he says nothing (*loc. cit.*, p. 1436). If this could happen in the only detailed experiment which Pasteur has ever recorded, and when everything seemed to depend upon the infallibility which Pasteur had so often claimed, what are we to think of the experiments done in the secrecy of his laboratory, of which no record has ever been given, and of which not a single witness has ever spoken except Pasteur?

Another matter to be remarked just here is that until now Pasteur had given no hint that the virus of hydrophobia could be attenuated so simply as by desiccating the spinal cord. And yet, if his own statements are true, he must have been far advanced in his experience with this method at the very time when he was startling the world with his backward and forward modifications of the virus in monkeys and rabbits, and presenting this as the way to obtain the virus for what he called his "three little inoculations." If we take the trouble to place side by side Pasteur's statements at different times, we see that they are so inconsistent that the cordial acceptance of almost any one of them seems to demand that the preceding ones should be banished from our memory. At first it was in the brain that the virus was to be obtained in perfect purity; then trephining and intradural inoculation was the sovereign method; then intravenous inoculations were said to simplify the matter; then blood was a good virus; then smaller quantities produced fiercer rabies; then inoculations in series modified the virus after many variations; then a few monkeys and rabbits did the work; then rabbits alone sufficed, while the virus was weakened by drying the cord. And, to crown all, forgetting the traditions of his own work in regard to charbon and chicken cholera, Pasteur says that the protective character of his virus depends upon a reduction in quantity and not in the virulence of the virus (*loc. cit.*, p. 1437).

What! And when we catch our breath, we cannot but recall what has gone before, and say: If the hypodermic injection of reduced quantities of virus was the means Pasteur found would most readily produce the most furious forms of rabies, in February, 1884, when he must have been half-way through the series of experiments upon which the present communication rests, how could the remaining half have sufficed to show that the same way of proceeding would exert a kind and protective influence on the same animals and on men?

But I must close my comments on this communication with the latest of Pasteur's theories. He actually intimates that the virus of hydrophobia "may be formed of two distinct substances, and that by the side of one which is animate and capable of germinating in the nervous system, there may be another, inanimate, having the power, when in proper proportions, to arrest the development of the former" (*loc. cit.*, 1438).

Since M. Pasteur made his last communication to the Académie de Médecine and to the Académie des Sciences, the enterprising *New York Herald* has given much space to discussing his method and its application. We have all heard of the four children sent from Newark to Paris after being bitten by a dog of which there is not the remotest evidence that it was mad, and of their return to America in apparently as good a state of health as is still enjoyed by the remaining two, who were bitten at the same time by the same dog, and who have never stirred from home. Three other Americans have gone over, viz., a man named Kaufmann, another by the name of Sattler, and a boy named Edward Bucklin. In regard to these patients there is equally little evidence that they have been exposed to the bite of a rabid dog. Indeed, in the case of one of them, it is denied that he was bitten at all. Pasteur is said to have refused to practise on him, and again he is said to have done so. All these seem to have maintained their health in spite of their bites, their fears, their long journey, and their inoculations with Pasteur's weak and strong viruses. One of M. Pasteur's patients, a girl, aged six, died of rabies while under treatment by him. This mishap Pasteur explained by saying she had come to him thirty-six days after being bitten, and that the virus in her system had made too great progress to be stopped. Its rapid action he explained on account of her youth—an assertion which is not borne out by facts—and because she had been bitten on the head, *i.e.*, near to the brain. This latter explanation is in accord with one of Pasteur's theories borrowed from Davaine; but it is also opposed to facts with which he should have been familiar; for, as lately as April 8, 1884, Dr. Dujardin-Beaumetz added, to what was clear enough before to any one who had carefully studied the subject, the confirmation of a report to the Academy of Medicine, of which M. Pasteur is a member, on the cases occurring in the Department of the Seine, in the three years 1881, 1882, and 1883 (thirty-four cases in all), which showed that there was no relation between the point of inoculation and the period of incubation.

On December 8th Pasteur said: "I am confident my treatment will be successful, if commenced *at any time before actual hydrophobia sets in*, even if a year or more elapses between the bite and the commencement of treatment." This was on the very day, I believe, on which the little girl died, and suggested to him that thirty-six days was a long time to wait, while on January 1st of this year he says that his treatment is efficacious *even* fifteen days after a bite!

The very last utterance of Pasteur on this subject is contained in the statement prepared for the *New York Herald*, under his direction, by his assistant, M. E. Wasserzug, on January 1st of this year. This embodies a good account of his last method, in which it is noticeable that while he specifies that he uses half a Pravaz syringeful of his mixture of rabic medulla and sterilized veal-broth for a child (as compared with three-quarters of a syringeful used for adults), he omits entirely—as he has *invariably* omitted—to say how much of the veal-broth is used to mix with a given quantity of so-called rabic spinal cord. This quite material omission is accompanied by two opposite statements: one, that his method is "very simple and very practicable;" and the other, that "outside of M. Pasteur, and of his laboratory, there does not exist a single person in the world capable of undertaking the treatment with certainty of success (*sûrement*)."

This must prove a disappointing *dictum* to those well-meaning men in New York who so hopefully incorporated a *Pasteur Institute* on January 2d; although it cannot be considered too harsh a judgment of the men in St. Louis, who, on December 31, 1885, "perfected arrangements for the treatment of hydrophobia after the method followed by M. Pasteur," announcing that "in three weeks, at the outside, patients may be treated."

It is very true, as Pasteur declared, on January 4th, to an American in Paris, that in this matter our countrymen "go too fast"—although it seems a pity that he should have applied this expression to the men of New York who so trustfully relied upon his help; while he cannot have known of those in St. Louis who proposed to imitate him by accomplishing in about two weeks what he says requires two years. For the credit of our country let us hope that Pasteur will never hear of some of the New York men, whose names appear in the *Herald*, who proposed to cultivate the "bacteria" of rabies in "glycerine and agar-agar jelly." If he does, he may wonder that our scientific men do not know that he has never found a bacterium in this disease, or that he has never said a word about cultivating his virus in glycerine or agar-agar jelly.

And now when we look back over the whole of M. Pasteur's work in connection with the subject of hydrophobia, how shall we judge it? The final impression is —to say the least—disappointing. In spite of the distinctness of his assertion and belief that he has solved the problem of the cure of hydrophobia, or of its prevention at any time before its outbreak, when we try to follow the steps by which he has reached this conclusion we find ourselves in bewilderment. We seem to be in a maze, travelling blindfolded, cheered here and there by his assurance that all will be well. At length he seems to say, "Look about you, we are safe!" But when we look about, the safety does not appear. The proofs of it are still nothing but his bare assertions. When we examine these we find they are in part contradictory of each other, while in part they contradict facts of which he does not seem to be aware. When we seek for evidence that he is familiar with the clinical manifestations of what is called hydrophobia, or with its history in past ages, we cannot find it. It does not appear anywhere that he has ever seen a case of this sort or ever studied the descriptions which others have given of it. The same is true in regard to the work of others who have studied hydrophobia experimentally. He seems to be ignorant of, or he wilfully ignores, the labors of his own countrymen, never mentioning one as if he had contributed anything of value to our knowledge of the subject, and being doubtless utterly unaware that any one outside of France has brought patience, perseverance, and skill to bear on the intricate problem.

The very outset of his work in the beginning of 1881 is marked by a positive announcement of a new microbe and a new disease, and an angry repudiation of the suggestion of Colin that control investigations and experiments would show that neither microbe nor disease was new.

This announcement, however, was soon shown to be erroneous in every part of it. He next adopts the ingenious (but mistaken) notion of Davaine, that the nervous system is the channel, and its centre the goal, of the virus of hydrophobia; for which appropriation of his ideas Davaine soon appeared in the lists against Pasteur and joined the number of those who have questioned his honesty as well as his ability.

A year and a half later, December 12, 1882, Pasteur claims to have discovered that the virus of hydrophobia has its principal seat in the brain, and that it there can be gathered in perfect purity and inoculated with absolute certainty, its effects being "prompt and sure." We have not time to discuss the theoretical objections to this assertion, which are very many; it is enough to point out that although Pasteur has repeated it many times, it has never been confirmed outside of his laboratory, and it stands opposed to the experiments of a much more candid experimenter, Galtier, of Lyons, and that it is not true of what happens even in Pasteur's own laboratory, for some of his animals recovered spontaneously (*Bull. de l'Acad. de Méd.*, 1883, p. 92).

He next announces the finding of the virus in the spinal cord, often in all parts of it. The evidence of this, as of the preceding assertion, rests upon Pasteur's opinion of what may be called rabies. This we have already seen to be elastic enough to cover almost any result of his experiments, and entirely too elastic to be trustworthy.

Fourteen months after this, Pasteur announced a new doctrine, viz., that when what he calls hydrophobic virus is introduced into the circulation it becomes "fixed" and multiplies, at first in the spinal cord, and that one part of the cord might contain the virus when the rest did not. This is, on its face, simply absurd; but it illustrates the boldness of Pasteur in filling up with an assertion a gap in his demonstrations. This very boldness has misled some who have followed with incautious and uncritical haste the rapid succession of brilliant discoveries announced by Pasteur. For example, when he passes smoothly over an admission that he has never discovered a microbe in his virus, that he is only "tempted to believe," in one of infinite smallness from the detection of certain *minute* granules, by which he claims to be able to distinguish a rabid brain from a healthy one, who pauses to reflect that, on the one hand, he has never demonstrated his skill in this mode of diagnosis; and, o the other hand, that such granules as he speaks of were described before he had thought of them, and are not peculiar to the brains of rabid animals at all?

Again, when Pasteur casually admits that he has never been able to cultivate or to isolate what he calls the virus of hydrophobia outside of the body, who calls attention to the relation of this admission to the fundamental principles of his own work in regard to anthrax and fowl-cholera? Again, who has lifted up a voice against the contradiction of these fundamental principles in Pasteur's assertion that he could produce the more grave and furious forms of hydrophobia on the sole condition of using a weaker virus, and less of it? And is it not new to-day, to ask how, if this be true, it can also be true that his so-called protective virus owes its protective character to a reduction in the *quantity* and not in the *quality* of the virulent material? or how this last statement can be believed at all?

The next theory announced by Pasteur was that inoculation of animals in series produced a fixed degree of virulence for each species; and for want of opportunity to experiment on man that monkeys could be considered a suitable substitute. This latter idea shows how Pasteur has been misled by the supposition that similarity of physical structure indicates a similarity in physiological nature—an error which has been admirably exposed (*apropos* of another matter) by M. Béchamp (*Bull. de l'Acad. de Méd.*, May 8, 1883).

In May, 1874, came Pasteur's announcement that by weakening his virus by transmission through monkeys, and by strengthening it by transmission through rabbits, he had obtained a virus which would prove protective in dogs, and the application of which would eradicate hydrophobia from the world. In regard to this claim I content myself with a single remark, viz., that in the two years which have elapsed since Pasteur made it, not a single dog has been made refractory to rabies outside of his laboratory; and that in his laboratory he has only succeeded in rendering "refractory," as he calls it, fifteen or sixteen out of twenty dogs!

The next and last announcement of Pasteur, made last October, was that he had devised a method to prevent the outbreak of hydrophobia after the bite of a mad dog. In this method we find him, without any explanation, dropping entirely the recent theory about monkeys as a part of the machinery for manipulating his virus, and passing by what he had declared to be a simpler mode of operating, viz., by intravenous inoculation, to take up hypodermic injection as the method of inoculating, pieces of spinal cord, rubbed up in an unstated quantity of veal-broth as a virus, desiccation as a means of attenuation, with the astounding explanation that the protective character of his virus was due to a reduction in quantity and not in quality! It seems strange that Pasteur should not have noticed the mutual contradictions of different parts of this announcement; but it is still stranger that he should have failed to see the destructive inference to be drawn from the facts to which I have already alluded—that in his crucial test (in the case of the boy Meister) one-half of the inoculation material afterward proved to contain no virus whatever!

Finally, we have Pasteur's positive announcement that his method would prove protective at any time before the outbreak of hydrophobia, even if one or two years had elapsed since the bite, which he stuck to until a case dying under his hands led him to reduce this long period at one sweep to thirty-five days, which was soon after cut down to about fifteen. How soon the short space of fifteen days may be reduced to fifteen minutes we may tremble to contemplate, in view of the rapid rate of reduction which has prevailed thus far.

One other point must not be overlooked in judging Pasteur's theories from a scientific standpoint, and that is, that he has utterly abandoned the only natural virus of hydrophobia, the only one which can be asserted to communicate the disorder from one animal to another—the saliva—and has used an artificial irritant, which probably has never produced rabies at all, but only a peculiar form of septic or simple inflammatory disease, of the significance of which his lack of medical education and experience make him unfit to judge, and which he has quite arbitrarily labelled hydrophobia!

The results of the application of Pasteur's method may be summed up as follows: One death under his hands, with a lame explanation; over a hundred persons to testify that his inoculations probably do no immediate harm; an almost equal number to illustrate the well-known advantage of having one's fears allayed—in all, no more than is credited to a host of nostrums. Besides which, the excitement it has aroused has brought about a senseless alarm in regard to dogs, and the killing of innumerable innocent and unfortunate animals to bear witness to the sharpening of men's fears and the dulling of their judgment.

In conclusion, then, I venture to express my opinion that Pasteur's so-called method of treating hydrophobia before the outbreak *does* appear to be founded upon untrustworthy experiments and unsound reasoning, and ought to be rejected and condemned, in the interests of humanity as well as of science.

But while I thus characterize this last work of M. Pasteur, let me not be misunderstood. Here, as in other theories and practices which he has presented to the world, there is much to criticise. Pasteur's arrogance, impatience of correction, ignorance or disparagement of what others have done ; his secret methods, his hasty assumptions, his illogical conclusions—have made him many scientific and personal enemies. But, after all, Pasteur is the man who placed on a new and apparently secure basis our knowledge of the nature of ferments ; who disproved the theory of spontaneous generation ; who laid the foundation upon which rests the whole system of antiseptic surgery, for the establishment of which the name of Lister will be justly immortal. Such a man is Pasteur ; and, whatever of error may seem to mar some of his best endeavors, and however the future may adjudicate their claims, he seems to me to deserve all the honor he has received for his unwearied labors, and his magnificent achievements in the cause of science.

His very achievements in the right direction, however, make any error on his part all the more dangerous to the cause of truth, and make it all the more the duty of thinking men to sift the evidence upon which he rests his extravagant claim of having discovered a means of preventing the outbreak of hydrophobia.