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# REMARKS ON HYDROPHOBIA.

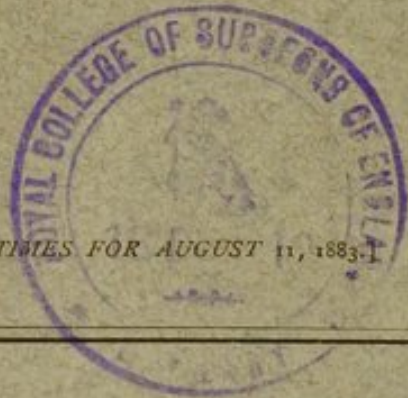
*Read before the Philadelphia County Medical Society, May 23, 1883,*

BY CHARLES W. DULLES, M.D.,

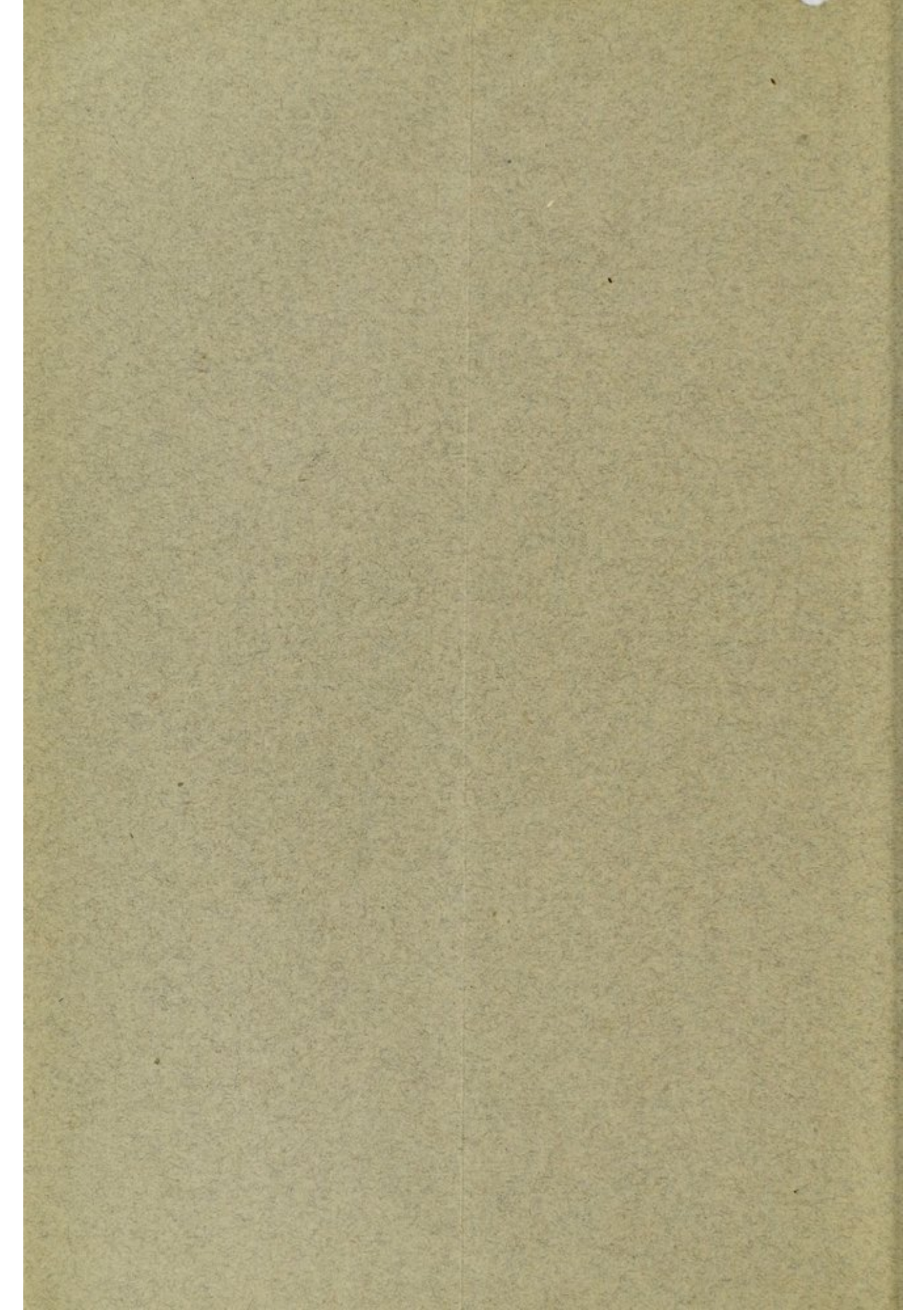
Surgical Registrar to the Hospital of the University of Pennsylvania, Surgeon to the Out-door Department of the Presbyterian Hospital.

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## REMARKS ON HYDROPHOBIA.

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IN that part of his "Medical Inquiries and Observations" in which Dr. Benjamin Rush speaks of hydrophobia, he says, "In entering upon the consideration of this formidable disease I feel myself under an involuntary impression somewhat like that which was produced by the order the King of Syria gave to his captains when he was conducting them to battle: 'Fight not with small or great, save only with the King of Israel.'"

It is with a similar feeling that I come before you this evening. For the more I have studied the subject of hydrophobia the more have I felt the intricacy of the problems it presents for solution, and the difficulty of formulating an opinion in regard to them which shall avoid the mistake, on the one hand, of too great credulity, and, on the other, of too stubborn a scepticism.

This difficulty is so great that I have no idea at this time of attempting to discuss the subject in general, but simply to consider the state of knowledge in regard to it, and to inquire whether this cannot in some way be improved.

More than a year ago I was called one morning to attend a boy who, about a month after being bitten by a dog, had fallen into a state presenting an almost typical picture of what is known as hydrophobia. The course of this case (which I described before the West Philadelphia Medical Society: see *Medical News*, May 27, 1882) was brief and striking. Not-

withstanding every effort that could be suggested from my own stock of knowledge, and from the larger one of Dr. C. K. Mills, whom I asked to see the case with me, the young patient passed through an extremely interesting series of phenomena of sensori-motor and ideo-motor excitation, and, within twelve hours after I first saw him, died.

The scenes of this day put to a sharp test my previous convictions in regard to hydrophobia, and left me in a state of uncertainty as to their correctness, which has prompted me to give a large part of my leisure during the last fifteen months to the work of studying what others have thought and written about it.

For those who would study the subject of hydrophobia exhaustively there is a mass of literature whose great extent may be estimated from the fact that in the "*Literatura Medica Digesta*" of De Ploucquet, published in 1808, there are sixty-four quarto columns of titles on Hydrophobia and Rabies. Besides these, there have been many writings which are not included in this list, and which have appeared since it was collated. A great part of the literature of hydrophobia, though curious and interesting, is but unprofitable reading. Yet happily he who will be satisfied to learn the most salient features of the disorder in a sort of epitome may find excellent accounts of its history and phenomena, and full discussions as to its nature, in certain articles contained in general treatises. Of



these I would name the article on "Hydrophobia," by J. L. Bardsley, in Forbes's *Cyclopædia of Practical Medicine*; that on "Rabies," in Copland's *Dictionary of Practical Medicine*; that on "Hydrophobia," by Ernest Hart, in Cooper's *Dictionary of Practical Surgery*; that on "Hundswuth," by Reder, in Pitha and Billroth's *Handbuch der Chirurgie*; that on "Wuthkrankheit und Wasserscheu," by Virchow, in his *Handbuch der Speciellen Pathologie und Therapie*; that on "Hydrophobia," by Bollinger, in the translation of Ziemssen's *Cyclopædia*; and, finally, those on "Rage Animale," by Signol, and on "Rage Humaine," by Doléris, in the recently-issued *Nouveau Dictionnaire de Médecine et de Chirurgie*. So far as my observation goes, there is not a single monograph on hydrophobia which can be compared, in interest or in instructiveness, with these articles.

But no one can claim to have gotten at the best that has been said and written about this subject unless he has gone over the papers and discussions upon it read and spoken before the French Académie de Médecine during the last twenty years, where may be found the views of Tardieu, Trollet, Trousseau, Villermé, Vernois, Raynaud, Lannelongue, Leblanc, Boudin, Decroix, Colin, Pasteur, Galtier, and Bouley,—all names inseparable from the history of hydrophobia. Nor can one be said to have completed even an elementary course of reading unless he has acquainted himself with the writings of Lauder Lindsay on "Madness in Animals," and kindred subjects, in the *Journal of Mental Science* for 1871, 1877, and 1878; as well as with those of Daniel H. Tuke on "The Influence of the Mind upon the Body," in the same magazine for 1870, 1871, and 1872. I would also strongly recommend to every investigator a very valuable series of articles—not referred to by a single writer on hydrophobia—on the Physiology and Pathology of the Saliva, by Dr. Samuel Wright, which were published in the London *Lancet* during 1842 and 1844.

Even this curtailed list may seem a sufficiently long one; but it is, as has been intimated, small compared with the mass from which it has been selected.

A cursory glance over the history of hydrophobia discloses the fact that the first authentic record of its occurrence was made about three hundred and fifty years B.C., by Aristotle, who, singularly enough,

says that dogs are subject to *lyssa*, or madness, and that all animals bitten by mad dogs become rabid, except man. After Aristotle there is no authentic allusion to it to be found until the time of Cælius Aurelianus, who gave a good account of its symptoms. About the time of the Christian era it begins to be mentioned pretty regularly by medical writers, Dioscorides, Celsus, Galen, and others speaking of it specifically. After the time of Galen a wider and wider departure from the simple dictum of Aristotle is to be observed. So far from holding man exempt from the communication of rabies even from mad dogs, he came to be considered not only liable to it, but liable from bites of a great variety of rabid animals, such as dogs, cats, wolves, foxes, bears, and even men. At the same time records began to accumulate of hydrophobia acquired in other and most fantastic ways. Thus, the contagion was attributed to eating the flesh or drinking the milk of rabid animals, to breathing the exhalations of a person already affected. It was said to have been conveyed by fomites, by kissing, by coitus; and it was a question whether it might not descend by heredity. It has been credited to the bite of a boy by an angry comrade, and even to a bite inflicted upon himself by a man in a passion; while there are single cases where it has been seriously believed that hydrophobia was caused by the bite of a duck and by the peck of an angry cock. It will not be surprising, after this, to hear that hydrophobia has been attributed to the bites of dogs not rabid, or that it has been believed to have arisen spontaneously in man as well as in the lower animals.

This process of development extended also to the symptomatology of hydrophobia. From a condition fitly expressed by the word "mad," it came to be regarded as one in which there was a simulation of the canine nature. Patients no longer simply fell sick and dreaded attempts to drink water; now they began to act like dogs. They howled and barked and tried to bite. They were said sometimes to turn round and round before lying down, as dogs are often seen to do. Their eyes were said to glow in the dark like those of cats.

The reputed length of the period of incubation of hydrophobia bears the marks of a similar process. Originally we find nothing said about such a period. Aris-



tote says all animals bitten by a mad dog become mad. The inference is that they do this in a reasonably short time, as they die after being bitten by venomous serpents. But after a while the idea of an incubation comes in, and grows and changes until its length is variously estimated from half a day to twenty—even to forty—years.

Finally, as was natural, while the notions in regard to the causation and the character and order of the manifestations of hydrophobia underwent such change, the methods of treatment multiplied and became more remarkable. Pliny recommended eating the raw liver of the dog that did the biting, or the saliva from under his tongue. Celsus advised throwing the victim of hydrophobia unexpectedly into a pond, when if he could not swim he was to be allowed to sink, and if he could he was to be held under until he was filled with water. Charms, mummeries, and nostrums—some of the most repulsive character—were constantly employed both to avert the outbreak of hydrophobia and to cure the fully-developed disorder. Bleeding, saturation with mercury, and such medicines as belladonna, opium, prussic acid, and the venom of the viper, were used, not to speak of the heroic method of suffocating the subject of hydrophobia between two feather beds for his own good and for the safety of others.

Thus, under a constantly-accumulating supply of testimony and with an ever-increasing pretension to knowledge, the ignorance of the medical faculty in regard to hydrophobia grew more and more profound. Here and there a writer rejected some egregious absurdity; but the current of misapprehension and error grew stronger and broader until it swept before it even the best intelligences so late as the beginning and middle of the last century. Towards its end protests began to be made and efforts to extricate the subject from the mass of falsehood with which it was encumbered. But how mistaken some of these efforts were may be gathered from the fact that so able a man as the late Dr. Rush regarded hydrophobia as a malignant fever, —though this ought not to be stated without adding that he, with the modesty of true greatness, acknowledged that he might have been misled by the principles of fever he had adopted, and hoped the reader might not be discouraged by his errors from using his reason in medicine.

The present century has supplied almost everything that can be considered of value in forming a correct opinion of the nature of hydrophobia. This is largely due to the advances that have been made in experimental pathology and veterinary science, and more than all to the incomparable researches of both veterinarians and medical men connected with the French Academy of Medicine during the last thirty years. The outcome of these researches has led Doléris to the roseate opinion that our pathological knowledge concerning rabies "is but little behind that which we possess about the majority of diseases of the same order thus far studied."

But it is not necessary to go so far as this to do justice to our own age, for it is not without its own faults of pretension and credulity, and if we examine the state of knowledge in regard to hydrophobia as it is represented by the most recent writers, we find that it is still dark, confusing, and unsatisfactory.

As to its causation, for example, we find that Virchow admits the possibility of contagion by the handling of sabres used to kill rabid dogs. Decroix, so recently as 1863, felt called upon to eat the flesh of a rabid dog, both raw and cooked, to disprove that this could communicate hydrophobia. Vernois, about the same time, said it could be conveyed from man to man; and Bollinger admits the possibility of its communication by the migrations of small animal parasites.

As to its incubation, while Dolan says this varies from one to three months, and that there is a scientific certainty of escape if an outbreak does not occur within a year, Trollet credits a case where the period was less than a day; Doléris says fifteen months is as long an incubation as is indubitable; Bollinger is credulous up to two years; while Drs. Harrison Allen and H. C. Wood, of this city, had in 1881 a case they considered one of hydrophobia where the inoculating bite occurred three and a half years before; and Sir William Gull (*Lancet*, November 17, 1877, pp. 745, 746) records a case that came under his own observation in which he says the period of incubation was thirteen years.

A consideration of the symptoms attributed in most recent times to hydrophobia discloses no less confusion than exists in regard to its means of communication and period of incubation. There is a certain



agreement as to its general character, but there is the greatest possible disagreement as to its details. According to many authors, at its outbreak the site of the inoculation wound manifests changes of an irritative nature. It is said to become painful, to be the starting-point of peculiar sensations or of wandering pains. At times it changes color, reopens, and discharges an ichor. Others, and the great majority of observers, have not seen any such phenomena.

The manifestations of general constitutional disturbance, also, have been different and even contradictory. The striking symptom of dread of water, which gives the disorder its name and which the accounts prove has been relied on as pathognomonic in a multitude of cases, is by the best authors set down as of little diagnostic value, being absent in many cases of real hydrophobia and present in all or almost all spurious cases, while rabies in the lower animals is marked by a great greed for water. We may also recognize even among modern writers and reporters of cases a singular disposition to attribute to hydrophobia a resemblance to canine characteristics. Barking, biting, and howling like dogs are set down over and over again as symptoms, when their occurrence is in fact rather a strong ground of suspicion that the case is one of spurious hydrophobia. Such a case was reported last year by Decroix, and said to have been cured by pilocarpine, but it was disproved by Bouley. Some writers attach importance to the presence of albumen in the urine. The latest example of this which I have found is in a clinical lecture by Dr. Bristowe, senior physician of St. Thomas's Hospital in London, published in the *British Medical Journal* for April 21 and April 28 of this year (1883). But, in fact, the presence of albumen is inconstant, and its value as an evidence of hydrophobia is impaired by the frequency of its appearance in other convulsive disorders.

Again, some of the most eminent of recent writers speak of satyriasis and nymphomania as phenomena of hydrophobia. These manifestations are not only remarkable, but also of a suspicious significance, when it is observed that they present themselves only within certain geographical limits. Thus, they have been observed in France and Germany, but not in England. Grisolle records a case called hydrophobia

where a man practised coitus thirty times in a night, and Reder another where the wife was "stormed," and where there was continual erection of the penis and ejaculations, often with some blood following; while Bardsley says that there occurs no satyriasis or nymphomania in Britain, and I have found no record of its occurrence in America.

About the character of the convulsions of hydrophobia there is substantial agreement among modern observers. They are rarely tetanic, but of an intermittent and clonic kind. Yet there can be little doubt, in reading many of the accounts, that they were caused, or at least aggravated, by the handling patients have received from their attendants. To understand this, we have only to picture to ourselves the mental condition of the victims of this disorder who were treated according to the method of Celsus, or whose neighbors thought it an act of humanity to smother them between feather beds. To a lesser degree such excitements are probably produced in almost every case in consequence of the ignorance and fear of the by-standers. In the case which I had, and which was as near typical as could be imagined, the violences of my patient ceased immediately upon my removing the restraints against which, on my reaching him, I found him furiously struggling.

Let us now turn our thoughts for a moment to the issue of hydrophobia. Most modern authors regard it as inevitably fatal. From time to time cures have been reported. They were commonest in the heyday of those violent methods of treatment which the present generation condemns as cruel, or of those fanciful remedies which it regards as absurd. The use of charms and nostrums claims its cures, and so do ducking, and bleeding, and mercurial saturation, and such violent poisons as belladonna in large doses, and prussic acid, and, last of all,—in our own days,—pilocarpine, oxygen, and curare. How this latter has sometimes been administered may be seen in the lecture of Dr. Bristowe, published only a month ago (*British Medical Journal*, April 21 and 28, 1883), where the lecturer says he gave hypodermically, within twelve hours, doses of curare which, when added together, I find amounted in all to more than *seven and two-thirds grains*! This patient did not recover, however, and the lecturer admits



that after its use he learned that the curare had been kept in stock for some time and probably was not of full strength.

But perhaps the most confusing aspect of the present condition of knowledge about hydrophobia is seen when we direct our attention to the teachings in regard to its pathological lesions. In addition to the physical peculiarities said by some observers to be discoverable at the seat of the inoculation wound,—namely, breaking out anew and discharging an ichorous secretion,—a specific form of vesiculation was described by Urban, about the beginning of this century, as occurring just before or at the time of the outbreak of the disorder. And this statement of Urban's has secured a certain number of believers.

Others, even very recently, have looked for and found the *lyssi* under the tongue, described first by the ancients, and again, in the beginning of this century, by Marochetti, an Italian adventurer, who said he had learned from the Cossacks of the Ukraine the secret of their significance, and that their removal eliminated the virus from the system. Others still have found lesions of the fauces, of the salivary glands, of the respiratory and digestive tracts, or of the brain, spinal cord, and nerves, which they hold to be pathognomonic of hydrophobia. But all these have been shown by other observers to be inconstant and unreliable. They are declared to be effects, not causes, and in some cases clear evidence that the disease was something other than hydrophobia. Even such a writer as Bollinger, who contributes the article on hydrophobia in Ziemssen's *Cyclopædia*, and whose credulity in regard to the communication of hydrophobia has already been alluded to, denies that post-mortem examination of dogs or men has contributed anything of value to our knowledge of the pathogenetic process of rabies or hydrophobia.

It might be expected, of course, that a microscopic germ, lying at the bottom of all the trouble, would be pointed out; and, indeed, Hallier has described one, which he named the *lyssophyton*, the like of which Klebs says he too saw. But the Milan Commission appointed to investigate the subject of hydrophobia soon exploded this error. Not very long after this, Pasteur made some experimental inoculations with the saliva of a patient sup-

posed to have died of hydrophobia in a Paris hospital, and rushed before the public with the announcement of a germ he had discovered peculiar to the disorder induced by inoculation with rabietic saliva. But that this was a premature assertion he himself soon found by experiment, and frankly acknowledged that similar effects to those of the inoculation of his supposed rabietic saliva were produced with the saliva of patients sick of other diseases, and even with that of perfectly healthy individuals,—experiments which have been repeated and confirmed by Sternberg and Formad in this country.

And now let us turn our attention to some of the views in regard to the nature of hydrophobia entertained in modern times.

Dr. Rush says, "The disease produced in the human species by the bite of a rabid animal is a malignant fever."

Dr. Physick thought that in hydrophobia death was due to spasmodic constriction of the glottis, and suggested opening the trachea for its relief.

Copland asks, "Is the secretion merely the vehicle of a nervous aura or emanation, which is actually the infecting agent, and which is retained by its vehicle only for a time?" Again, he says, "The supposition, lately published, that there is no such specific disease as rabies, and that it is merely the result of mental anxiety, etc., is only one of the absurdities thrown up on the surface of medical doctrine, and hardly deserves mention, and much less refutation."

Tardieu rejects the idea that it is a disease of imagination and fear, saying that the commission of which he was a member reported thirty cases under five years of age. He believes, also, in the occasional spontaneous origin of rabies, and gives two cases, one a cat which became rabid from a burn, and the other, also a cat, which became rabid from being robbed of her young.

Jolly, speaking before the Académie de Médecine, turns the tables on the doubters by asking who knows but that hydrophobia acquired by a forgotten bite may have been mistaken for acute meningitis, tetanus, pernicious cerebral fever, acute mania, or other nervous affections? He believes that it may be caused by the bite of a non-rabid dog, and credits a case where a young woman acquired hydrophobia by being



bitten by a dog she was suckling,—the dog not being rabid, but only irritated by a correction.

Pasteur, whose acquaintance with rabies is not very great, asserts most positively that he has produced it by inoculation of fragments of the medulla and frontal lobe of the cerebrum and the cephalo-rachidian fluid, and that the incubation is shortened and the communication made surer by inoculating on the surface of the brain, and by using the unadulterated cerebral substance of a mad dog.

Bouley, by far the most careful and thorough student of this subject in France, agrees with Boudin that there does not exist a *scientific* proof of a single case of spontaneous rabies, and illustrates the difficulty of getting at the actual truth about so-called cases. He quotes Renault as being a most scrupulous observer, and as telling him and Reynal that after thirty years of research there were only three cases of rabies that he dared to consider certain, and that for these he trusted to the fact that he had got the details from persons he considered entirely trustworthy. Bouley, uncertain in regard to the possibility of a spontaneous origin of rabies, illustrates the difficulty of getting at the truth by a capital story, and immediately afterwards furnishes in himself an example of the error to which this difficulty may lead, by saying he would be positive of such an origin if he had some more facts like those given by Leblanc *filis*,—the latter being a history of a case which was subsequently *detected* by Decroix and shown to be the very opposite of what Leblanc *filis* believed, and, in fact, a clear case of inoculation.

Virchow believes that certain individuals have a predisposition to hydrophobia. He believes in a specific virus, and that it is transmissible by retro-inoculation. He admits the probability of ancient accounts of its transmission by means of weapons used to kill mad dogs with, and by scarifying and blood-letting instruments.

Roucher calls "very probable" a case of communication, by suckling, from a negro woman to her child.

Dolérís fully accepts the specific theory of hydrophobia, believing that it is caused by the inoculation of a distinct virus which elects the nerve-centres. He believes the virus resides in a germ, and quotes Nocard as having obtained from saliva, *by dialysis*,

a solid substance which, being inoculated, produced "positive results,"—which the liquid parts did not.

Bollinger denies the spontaneous origin of rabies, but intimates that fleas and lice may transfer the poison and thus produce inoculation. He also admits that meat and milk may communicate it, though this is "extremely rare." He says further, "The poison is only in very rare cases communicated in any other way than by the bite of a rabid animal: such cases, for example, are those in which infection takes place by means of *coitus* [!], or through intermediate vehicles, or by the consumption of milk." He considers hydrophobia to be inevitably fatal.

Many other authors might be quoted who, though differing very materially in regard to the details of hydrophobia, have all been believers in its specific nature and inoculability.

But there have not been wanting in modern times those who have been led to other views by observing the discrepancies in the facts and theories of these very believers. Bosquillon, a physician to the Hôtel-Dieu in Paris, in the beginning of this century made a vigorous, though at that time futile, stand against the commonly-received opinions about hydrophobia. He pointed out the unreliability of the evidence upon which the belief in its specific nature rested, and the influence of the imagination in producing morbid physical and psychical conditions. He claimed that the ravages of so-called hydrophobia were increased by the popular education on the subject,—in other words, by the very means taken to warn and defend the community against it.

Girard and J. Simon supported the views of Bosquillon. Not long after, White demonstrated his conviction that hydrophobia was not caused by the inoculation of a virus peculiar to the saliva of a rabid dog, by inoculating a variety of animals, and, finally, himself, with such saliva,—all without injurious results.

Maschka, in Prague, and Lorinser, in Vienna, have denied the specific nature of hydrophobia.

Dr. Burder (in the *British Medical Journal*, October 26, 1872) bases a like opinion upon the incubation, which is without parallel in other diseases, upon the similarity of the phenomena of hydrophobia to those of other nervous disorders,



upon the sufficiency of the imagination to excite all the symptoms, upon the tendency to invent a connection between the phenomena and a bite, upon the frequent absence in cases of so-called hydrophobia of sufficient evidence of rabies in the dog which did the biting, and upon the frequent lack of good evidence of any bite at all.

Professor Dick, of Edinburgh, says hydrophobia is not the result of any poison introduced into the system, but merely the melancholy and often fatal result of panic fear and of the disordered state of the imagination.

Professor Macleod, of the University of Glasgow, said,—after a scare in consequence of which eleven hundred and fifty-five dogs had been killed by police regulation,—“There is a large lot of nonsense talked about hydrophobia, and people need not be in the least afraid of it.”

Dr. Tuke says, “Among the admitted difficulties attaching to the pathology of this disease, that is surely not the greatest which acknowledges the power of the imagination in combination with fear to excite not only a paroxysm in the course of the disorder, but to originate a group of symptoms by central excitation, which, in a susceptible state of the nervous, closely resemble those of genuine rabies.” As an illustration of this, he cites a case from Trollet, where a man, having been bitten by a dog, had no symptoms until three and a half months afterwards, when a dog one day attacked his horse. In a few days hydrophobia manifested itself, and he died on the third day after.

M. Girard de Cailleux, Inspector-General of the Service of the Insane for the Department of the Seine, has called attention to the similarity of the symptoms, course, duration, termination, and cadaveric lesions of acquired hydrophobia (*rage communiquée*) and of acute febrile delirium, saying that this “establishes an identity of nature worthy of attention.”

Dr. Lauder Lindsay says, “Hydrophobia in man is frequently, if not generally, the result of terror, ignorance, prejudice, or superstition, acting on a morbid *imagination* and a susceptible nervous temperament.” (*Journal of Mental Science*, July, 1871, p. 185.) Again, he says, “It furnishes one of the best examples that could be adduced of the wonderful influence of

the mind over the body, and of morbid mental conditions in the generation of fatal physical disease.” (*Journal of Mental Science*, January, 1878.)

Finally, I would quote the opinion of our fellow-member Dr. C. K. Mills, who, after seeing three cases of so-called hydrophobia and making a post-mortem examination of a fourth, has said that, while he would not assert positively that there was no such disease as hydrophobia in man, he was prepared to say that he believed all the cases so called could be explained on some other hypothesis, such as that of lysophobia, tetanus, chronic or acute nerve-lesions, extravasations, etc. (*Medical News*, May 27, 1882, p. 585.)

We see, then, that the study of hydrophobia, as the subject is presented to us in both ancient and modern writings, is in the highest degree unsatisfactory. Where so many contradictions are found there must be a great deal of error. If, now, we attempt to form an opinion for ourselves, we must, I think, find some way to escape from the influences which have introduced so much confusion in the minds of others. It has sometimes appeared to me that we would be no losers if the whole enormous mass of literature relating to hydrophobia were swept away and an opportunity afforded to begin the investigation of the subject without bias by the careful and critical observation of such cases as may arise from time to time, and with the stern determination to eliminate every particle of evidence which is not of a reliability commensurate with the delicacy and gravity of the case.

A careful study of a large number of recorded cases has convinced me that the source of most of the errors connected with this subject is to be found in a general absence of a proper attitude of criticism, together with a deficient acquaintance with the methods by which the nature of all cases of supposed hydrophobia should be tested. The former want ought for its correction to require no more than that attention should be called to it. The latter requires that medical men should be more familiar with all the conditions under which the phenomena usually attributed to hydrophobia may be produced. Here is a field especially inviting for the alienist, the student of mental and psychical phenomena. The ground has already been broken by Tuke and Lauder Lindsay.



Others, it is to be hoped, will carry on the work they have begun.

#### DISCUSSION.

Dr. H. C. Wood, opening the discussion by request of the Chair, said: I consider the paper an extremely able production, but cannot agree with the conclusions at which the author arrives. Though much nonsense has been spoken and written about this subject, and almost every ridiculous theory has been advocated, yet we cannot reject the inference from certain facts which are well known. A disease undoubtedly exists among the *carnivora*, and especially among canines, which is propagated by biting, and attended with decided nervous manifestations and disturbance of the salivary apparatus. This disease has been passed from one animal to another hundreds of times by biting and by injection of saliva. Such results cannot be ascribed to the influence of fright, as they occur in lower animals which are incapable of continuous fright and of disease-simulation. In physiological laboratories animals are subjected to an endless variety of injuries, but no one has ever seen hydrophobia arise from these causes; but when we take a small amount of saliva from a rabid animal and inject it into a healthy one we have the specific disease developed in about sixty-six per cent. of the cases. When the virulent saliva is introduced into human beings by the bite of a rabid animal, we have a series of symptoms of analogous character. It must surely be regarded as very remarkable that out of say one thousand persons dying with hydrophobic symptoms a much larger proportion than nine-tenths are found on investigation to have been previously bitten by dogs. Very few cases are known in which hydrophobic attacks have followed the bite of a dog not mad.

The pathology of the disease is involved in doubt. The lesions that have been detected in the brain are rather the effect than the cause of the phenomena. We would naturally expect that such violent action would give rise to the alterations which have been observed with the microscope.

The question as to the long incubation does not affect the main point as to the existence of the disease. Mistakes may have been made in some of the cases where very long incubation has been supposed and infection more recent may have been overlooked. We must remember that a lick or a very slight scratch may be the means of communicating the disease. In the case reported by me lately, no instance of a bite more recent than three and a half years could be found.

A striking case, almost positive proof of the correctness of the accepted views on hydrophobia, is detailed by Dr. Colin,—a case which was discussed before the French Academy and re-

ceived the credence of Bouillaud. In 1865 a French soldier in Algiers was bitten by a rabid dog, and a comrade who rushed to his assistance was also bitten. Both wounds were cauterized by the hot iron. The first soldier died in a few days; the second experienced at the time no serious effects, but after an interval of five years died of acute hydrophobia. The case was officially reported on to the French government, because if it could be established that the death was due to the bite received in the attempt to rescue a comrade, the widow would be entitled to a pension. The most careful search failed to show any possibility of infection other than the one alluded to; and the case therefore remains as one of long incubation, though it is true that objection might be made that some unnoticed or forgotten introduction of virus had occurred more recently. It is not possible to explain why the virus remained so long inactive.

One of the great difficulties in the way of a thorough knowledge of rabies is the danger of investigating it. Even Dr. S. Weir Mitchell, although accustomed to play with rattlesnakes, had declined to undertake experiments on hydrophobia. Youatt is supposed to have committed suicide because he felt the beginning of the symptom of the disease after having repeatedly escaped from the effects of bites from rabid dogs.

Dr. Mills: I think Dr. Dulles has very well set forth the arguments in favor of the view that in man at least hydrophobia as the result of a specific infecting virus does not exist. One of the best demonstrations of this is the fact that a distinguished investigator was able in thirty years to obtain only three cases which could be considered hydrophobia. Of course the results of experiments on inoculation might be regarded as favorable to the theory of specificity; but it has been shown that hydrophobic symptoms may be produced in animals by the inoculation of the saliva of non-rabid animals. It is, however, the existence of hydrophobia in the human species that is most doubtful. In nineteen out of every twenty cases the disease is probably not hydrophobia at all, and in many instances, certainly in every Philadelphia case which I have seen or of which I have knowledge, the bite supposed to have caused the disease was by a dog not rabid. Every symptom in so-called hydrophobia is capable of being produced by other diseases,—e.g., localized meningitis, tubercular tumors, tetanus, reflex epilepsy, acute mania. A case has very recently come under my notice in which from injury to the finger with a piece of brick tetanoid symptoms were developed, with well-marked sputtering of saliva, which is considered so typical a symptom of hydrophobia. In one case, which I saw in association with Dr. Burns and Dr. Wood, the autopsy revealed distinct pachymeningitis; and in another



case chronic brain-lesions were found which were sufficient to account for the symptoms. The high percentage (eighty to ninety per cent.) of instances in which persons are bitten by rabid dogs and yet do not go mad is another point of importance. Even experimental inoculation succeeds in only about one-fourth the cases. As regards the influence of fear, I may mention the cases of two ladies, both patients of mine, who cannot endure the sight of a dog, and who are thrown into great excitement at the mere mention of the animal. In another case, a patient actually barked like a dog, spat, sputtered, and had clonic spasms, symptoms which wholly ceased on threatening to use the actual cautery.

That cauterization applied to recent bites has apparently been efficacious in preventing infection is no argument; for the same applications have been of great effect in epilepsy.

The long period of incubation is a serious difficulty. If we take vaccine virus as a typical virus, we can get an idea of how extraordinary the supposed long incubation of hydrophobia is. Vaccine loses its power within a few months at most. It seems very difficult to suppose that the virus of hydrophobia can remain for years locked up in the system and then suddenly break out with violence. The case of the soldier alluded to by Dr. Wood is not absolute proof of long incubation. Other infection is possible,—syphilis, for instance; and no proof is adduced that the dog which did the biting was mad.

Dr. Moon: The paper presented this evening is as gratifying as the remarks of Dr. Wood on "The Pathology of Hydrophobia" a few weeks ago were unsatisfactory,—this being a fair and creditable presentation of the subject, while Dr. Wood might have been describing a typical case of acute mania as not infrequently seen. The pathological specimens exhibited could very readily be ascribed to a diseased condition of the brain which would induce mania.

There is already in the public mind too much misapprehension on this subject, which should not be encouraged by physicians.

Dr. Dercum: The virus-theory may be unsatisfactory, but it is the best. It is very difficult to see how mere reflex from afferent nerves could produce the symptoms observed in hydrophobia in dogs, especially the melancholia. Another fact in favor of the specificity of the poison is the frequent success in inoculation. Some dogs have been inoculated only after several trials; but these failures are dependent on differences in the susceptibility of the animals. The period of incubation is, it is true, very variable and often long; but then the periods of incubation of all diseases are subject to much variation.

Dr. William R. D. Blackwood: That there is such a disease in dogs I believe from experience, but as to its occurrence in the human species I do not know personally, although I

do not believe that the thousands of physicians who have treated and described the disease before our time were all fools, or that the hundreds of people who have died from rabies were all victims of their imagination; neither do I believe that the most ardent admirer of the fright and imagination theory would deliberately allow a mad dog to bite him, and then depend upon his superior wisdom and lack of imagination to avert the possible and probable development of hydrophobia. In 1868 a rabid dog entered the camp where I was stationed, and, after snapping at various other dogs, he bit—*first*, a small lap-dog, *second*, a woman, and *third*, another small dog. The mad dog was then killed. The woman had two wounds, which completely penetrated the webbed portion between the thumb and index-finger of her left hand, and I at once cauterized the punctures by pouring nitric acid into them, *the acid passing freely through each* and dropping from the under side of them. The two bitten dogs were cooped up, and *both died* from hydrophobia; but the woman remained well when I last heard from her, ten years afterwards. The peripheral irritation from the severe bite or the severer nitric acid did not develop hydrophobia, yet there must have been virus present; for the animals bitten *before* and *after* her wounding died from rabies.

I also believe the disease to originate spontaneously in dogs. Some time after the occurrence mentioned above, a small cur-dog became offensive through biting strangers and visitors to the post, although he did not ordinarily bite those belonging to it. The dog was tied up securely for a time, and I believe he had abundance of water and proper food. He did not, however, have sexual intercourse, which may or may not have had anything to do with his case. This dog went mad; and I caused him to bite another animal for experiment. Both animals died. No rabid animals had visited camp since the time when the woman was bitten, and the dog now referred to was not then born: hence the inference plainly is that this case of rabies arose spontaneously.

I cannot see any difficulty about the virus lying long dormant in the system before breaking out. Syphilis, apparently eradicated, will show itself many years afterwards without a second inoculation. Diseases of all kinds require longer or shorter periods of development after exposure to them in different persons.

Dr. O'Hara: I do not think that syphilis is fairly comparable to hydrophobia, for the former is transmissible directly from one human being to another, but the latter is not. Possibly but little confidence can be placed in the decisions made from the superficial examinations of pension surgeons, and therefore the case of the French soldier which Dr. Wood had mentioned is not unobjection-



able. Dr. Agnew and I saw a case which presented the typical symptoms of hydrophobia: Dr. Agnew considered it a brain-tumor, but the autopsy did not reveal any gross lesion. Many physicians would have put it down as hydrophobia, unacquainted with the previous history. The clinical symptoms of hydrophobia were very marked at one time, but the case closed with the symptoms of *acute paralytic dementia*. Hydrophobic symptoms are not very uncommon in many brain-diseases.

Dr. Bartholow: The disease hydrophobia admits of a large latitude of opinion, as has been well shown by Dr. Dulles's paper. Two groups of cases are presented. In one group imagination or morbid fear is the causative influence. All the objective symptoms of hydrophobia may be produced in this way. Persons who have been bitten by dogs brood and worry over the matter, closely study every symptom as seen in cases, until they develop a condition of the most serious character. The influence of the mind over the body under such circumstances is well illustrated by a case recorded in Tuke's work on the Influence of the Mind over the Body. Two brothers were bitten by the same dog, and shortly after this event they separated, one going to Belgium, the other remaining in France. The one in France was a few days after seized with hydrophobia, and died. The other, after a long absence, returned to France, learned for the first time of the manner of his brother's death, was shortly seized with hydrophobia, and died. Numerous cases have been recorded, in both sexes, having all the objective phenomena of hydrophobia, produced merely by mental causes, by fear, and by mimicry or morbid irritation. These constitute a distinct group. We have thus a neurosis having all the ordinary characteristics of hydrophobia. I am a firm believer, also, in the existence of a morbid entity, to which we apply the term hydrophobia, which is propagated by a distinct virus that is inoculable. We have knowledge of the fact that the saliva and other secretions may become toxic. Montgomery records a case in which an infant was poisoned by nursing at the breast of the mother while she was in a very high passion. Sternberg has shown that human saliva may become poisonous; and authentic instances are known of the communication of hydrophobia from man to man by bites given in states of tremendous passion. The recent discoveries in regard to the ptomaines, alkaloidal poisons produced during the decay of animal tissue, will throw some light on this subject. In the case referred to by Dr. Blackwood we have evidence of how close confinement and deprivation of customary indulgences can unfavorably affect an animal; and in the same manner a dog chased or annoyed in the street may be so affected that its saliva will become dangerous.

As regards the length of the period of

incubation, we must remember that this period varies in all diseases. If the period of incubation is two weeks,—which is not unusual,—why not two months, or two years?

Dr. Hamilton: I recall the following case, which may be interesting in this connection. A man had fallen against a rail and hurt his shin severely, but continued at work for two or three days. The wound inflamed and suppurated, and then tetanoid symptoms set in. To these hydrophobic symptoms supervened, shuddering, and going into spasms whenever an effort was made to drink water. This condition continued about thirty hours, when death ensued. The patient had never been bitten by a dog.

Dr. Wittig: About forty years ago I saw a case in which a young man who had been sleeping with a dog found his mouth covered with foam, and foam on the mouth of the dog also. The young man was very much alarmed when I saw him, but he recovered under the use of calomel and diaphoretics associated with a remedy that is used in Russian practice,—the *genista*. I was told that vesicles had appeared under the tongue and had been cauterized. In another case, a young man was bitten in the finger, caustic was applied, but the patient got very much frightened and sent for me. I used remedies similar to those in the case just mentioned, and the patient got well. Dr. Heger, of Vienna, has said that in his opinion the saliva is not the cause of the disease, but it is a lesion of a nerve causing peripheral irritation. I incline, however, to the opinion that it is a virus, and that it may become latent and lie dormant for some time in the system.

Dr. W. S. Stewart: In Westmoreland County, in this State, lives a family of physicians who claim to have a remedy for hydrophobia, and people come to them from all points for treatment. In one case, a patient of my preceptor's, feeling symptoms of an alarming kind, went to one of these doctors, and received from him one pill, to be taken on arriving home. This pill on being opened was found to be made of bread-crumbs and to contain a piece of paper bearing the word *abracadabra*. This pill, taken with restriction as to stimulants and excitement of all kinds, constituted the treatment, whether the cases were real or imaginary.

Dr. Dulles, in closing the discussion, said: I will endeavor to reply to some of the points raised, in the order in which they came up; for time will not permit a systematic reply.

In the first place, I must call attention to the vagueness of some of the statements of Dr. Wood. His figures in regard to successful artificial inoculations are not to be accepted without question. If his statement that sixty-six per cent. of experimental inoculations of rabies are successful is true of the Laboratory of the University of Pennsylvania, then



the success there is about four times as great as has been attained elsewhere. In the famous experiments of Hertwig, of Berlin, to which reference is always made in this connection, only about one-quarter were followed by what are called "positive results," and some of these were duplicated experiments on the same animal. Again, the statement that ninety-nine per cent. of cases of hydrophobia can be traced to the bite of a mad dog is also incorrect, even if one puts faith in testimony drawn out, as it often is, after suspicious symptoms arise. It is often the purest assumption to say that the dog that gave the bite was rabid. It is a necessary part of the inoculation theory, but it is not supported by good proof. A very common history of such cases is that a strange dog ran up, snapped at the person, and then ran away and was lost, nothing being known about it before or afterwards.

The story credited to M. Colin is very interesting, but it has been exploded. M. Colin himself told it, with reserve, to the French Academy of Medicine; and it fell to pieces under the criticisms of that distinguished body. It gives no support to the theory of long incubations, but demonstrates the credulity of those who accepted it.

In reference to Mr. Youatt's views, it must be remembered that although Youatt is worthy of much respect as a veterinarian, yet he is not much of an authority in regard to the diseases of men. In regard to hydrophobia he held some peculiar views. For example, he believed in the preservative influence of delayed cauterization, and claimed that he had saved the lives of four hundred persons bitten by rabid dogs by cauterizing their wounds with nitrate of silver. He was altogether too credulous. Indeed, too much reliance has been placed upon the statements of the English veterinarians. Mr. Fleming, who is often quoted as an authority, and who wrote a book on this subject, recently acknowledged that he had seen in all his life only one case of hydrophobia, and that was not under his own care.

Dr. Blackwood, in his remarks, mentions an experience of his own in which a dog, being kept in restraint and being deprived of sexual indulgence, developed spontaneous rabies. Now, the testimony of the most reliable veterinarians is almost universally against the possibility of rabies arising in this way. Though I had not time to tell stories illustrative of the points dwelt upon in my paper, I will give one in this connection. M. Bouley, having long denied that there was any really trustworthy evidence of a spontaneous rabies, told the French Academy of Medicine, about twenty years ago, how he had recently thought he was mistaken. He had a bitch brought to him at Alfort by her owner, who said she was so valuable for breeding-purposes that she was never out of his sight. The last time she was in heat he refused her the male, and in

consequence rabies developed. M. Bouley showed the animal to his students, and confessed that with such positive evidence this must be regarded as a case of spontaneous rabies. In three days the bitch died, and at the autopsy "what did we find," said M. Bouley, "in this beast whose heat had not been satisfied, according to the positive assertion of her master? Four young ones in her belly. Behold how they write the history—of bitches!"

The story of the peasant of Languedoc, related by Dr. Bartholow, is a very familiar and oft-repeated one in the literature of hydrophobia. It varies a little in different accounts. The place to which the second brother is said to have travelled is not always the same. But the most constant thing about it is that it is used in a way diametrically opposite to that in which Dr. Bartholow has used it this evening. It is generally cited as an illustration of the effect of the imagination, and not considered a very valuable argument in favor of long incubations. The statement that Dr. Sternberg has found human saliva to be poisonous "under *peculiar* circumstances" is not correct. The fact is that Dr. Sternberg has been but one of several experimenters who have demonstrated that inoculation of certain of the lower animals with human saliva proves poisonous under *almost* all circumstances. Again, Dr. Bartholow spoke of cases of communication of hydrophobia from man to man as "well authenticated." After investigating the subject for fifteen months, I must say that I have not found a single case of this kind which deserves, or gets, serious consideration. Nor is there any scientific proof that rabies can be excited by deprivation of sexual gratification. Some very cruel experiments have been performed in France with the object of ascertaining whether or not hardships, solitary confinement, and the deprivation of food, of water, and of sexual indulgence would cause the development of rabies, and the result has been always negative.

It has been said by Dr. Bartholow, in answer to objections raised against the credibility of very long periods of incubation, that these vary in different diseases, being longer in some and shorter in others; and that "if there may be an incubation of two weeks, why not of two months or two years?" This is a dangerous style of argument. One might as reasonably say, "Men vary in height, and if a man's height may be six feet, why not sixty feet?" The periods of incubation in diseases that are fairly understood are always reasonable. A period of incubation is not a period of total inactivity, but simply the time required for a chemical poison to make its way through the system, or for a vital poison to multiply until it is capable of producing certain gross manifestations. Consistently with this fact, the longest periods of incubation which are well established are found to be measured by days



or weeks,—never by months or years. For incubations that exceed reasonable limits syphilis is sometimes cited. This has been done to-night. We are not here to discuss syphilis, or it might be interesting to consider the value of some of the assertions made in regard to it. But I call attention to the faulty method of arguing which this appeal to syphilis illustrates. One hypothesis is made

to rest upon something else, which itself is nothing but an hypothesis. Unfortunately, many of the arguments in support of the commonly-accepted theory of hydrophobia are of this sort, and but little of the evidence upon which it rests can be regarded as having *an accuracy and a reliability commensurate with the delicateness and gravity of the questions at issue.*"