[EXTRA SESSION.]

Commonwealth of Massachusetts.

EVIDENCE.

THURSDAY, May 31, 1860.

The Legislative Committee on the Pleuro-Pneumonia held their first meeting in the hall of the House on Thursday, May 31, at 12, M.

The following gentlemen of the House and Senate constitute the Committee:—

Messrs. NASH, of Hampshire, *Chairman;*
COLE, of Berkshire,
GORHAM, of Worcester,
OSGOOD, of Essex,
FISHER, of Norfolk,
WHITING, of Plymouth, and
COOK, of Worcester,
Of the Senate.

Messrs. ELDRIDGE, of Canton,
WENTWORTH, of Lowell,
THOMPSON, of Nantucket,
FULLER, of Whately,
GRIFFIN, of Malden,
GIFFORD, of Provincetown,
PARSONS, of Northampton,
CHOATE, of Salem,
JENKS, of North Brookfield,
GARDNER, of Swanzey,
SHURTLEFF, of North Chelsea,
GAY, of Springfield,
WOODMAN, of Charlestown, and
SCOVILL, of Sheffield,
Of the House.

Mr. Parsons, of Northampton, was chosen Secretary.

On motion of Mr. Gifford, of Provincetown, it was voted to employ a phonographic reporter; and Mr. J. M. W. Yerrinton was nominated and unanimously elected.

It was voted to ask the attendance of the Commissioners at the next meeting, and to give a hearing to all persons desiring to be heard.

The Committee then adjourned till 2½ o'clock.

Afternoon Session.

Thursday, May 31.

The Chairman called the Committee to order at the hour to which it had adjourned, and requested a motion as to the order of further proceedings.

Mr. Wentworth.—I move that the Chairman of the Commission be requested to state to the Committee what, in his judgment, is necessary to be done in the present state of the cattle disease, by the State.

The motion was carried, and Mr. Paoli Lathrop was according invited to make the statement asked for.

Mr. Lathrop.—Mr. Walker will give you, gentlemen, a history of the introduction of this disease and its progress thus far.

Mr. Wentworth.—Mr. Chairman, we don't want to take up the introduction of the disease, or the progress of it, for we have that in writing. What we want to know is the desire of the Commissioners in the present state of the case, what they want of the legislature.

Mr. Walker.—It is thought by the Board that it may be proper, as a sort of connecting history of the whole case, that the facts should be stated in regard to the introduction of the disease, because these facts will show something what ought to be done. They will show the nature of the disease, and whether it is contagious or not; upon those facts legislation must be based.

The disease was introduced into North Brookfield from Belmont. Mr. Curtis Stoddard, a young man of North Brookfield, went down the very last of June, last year, and purchased three calves of Mr. Chenery, of Belmont. He brought those calves up in the cars to Brookfield. On their way from the depot up to his house, about five miles, one of the calves was observed to falter, and when he got to his house, it seemed to be sick; and in two or three days exhibited very great illness, so much so that his father came along, and thinking he could take care of it better, took the calf home. He took it to his own barn, where there were about forty head of cattle, but it grew no better, and his son went up and brought it back to his own house. In about ten days after that, it died. His father, who had had the calf four days, in about a fortnight afterwards observed that one of his oxen was sick, and it grew sick very fast and died. Two weeks after, a second was taken sick and died. Then a third was taken and died, the interval growing a little wider from the attack of one animal to that of another, until he had lost eight oxen and cows. Young Stoddard lost no animal by the infection, that is, no one died on his hands, prior to the appointment of this Commission. About the first of November,—for reasons independent of this disease, which I don't suppose he then knew the nature of,—he sold off his stock. He sold off eleven heifers or young animals, and retained nine of the most valuable himself, which shows that he did not then know any thing was the matter with them. These nine were four oxen and five young cattle. The four he took to his father's, three of the others he took to his uncle's, and the two remaining he took to his father-in-law,—distributing them all among his friends, which furnishes another proof that he did not suppose he was doing any mischief. He disposed of his herd in that way. From this auction, these eleven animals went in different directions, and wherever they went, they scattered the infection. Without a single failure, the disease has followed those cattle,—in one case, more than two hundred cattle having been infected by one which was sold at Curtis Stoddard's auction, when he was entirely ignorant of the disease.

When the Commission was appointed, they went and examined his cattle, and were satisfied that they were diseased,—at

least, some of them. They examined his father's herd, and found that they were very much diseased; and when we came to kill Curtis Stoddard's cattle, seven of the nine head were diseased. Two were not condemned, because the law says cattle not appearing to be diseased shall be appraised. Nevertheless, it proved that these animals were diseased; so that his whole herd were affected. In regard to Leonard Stoddard's cattle,—he lost fourteen of his animals before the Commissioners went to his place. They took eighteen more, which were all diseased,—most of them very bad cases indeed—extreme cases. That left eight head which were not condemned because not appearing to be diseased. Here I remark, that when the disease is under the shoulder blade, it cannot be detected by percussion. The physicians did not say the animal is not diseased, but—"we do not perceive sufficient evidence to condemn." Such animals were to be paid for, on the ground of not appearing to be diseased. Nevertheless, it is proper to state that the remaining eight which were not condemned were suspected to be diseased, and we told Mr. Stoddard that we had the impression that they were diseased, nowithstanding appearances. He says: "There is a three-year old heifer that has never faltered at all—she has never manifested the slightest disease; if you will kill her, and she is diseased, I shall make up my mind that I have not a well animal in my stalls." We killed that animal, and she was badly diseased. Thus the two first herds were all infected by the disease; and in the last of Curtis Stoddard's oxen which we killed, we found a cyst in the lungs of each. One of these lungs is now in this building, never having been cut open, and medical men can see the cyst which it contains.

I have said in what manner Mr. Curtis Stoddard's cattle spread the infection. In regard to Mr. Leonard Stoddard,—in the first place, he kept six or eight oxen which he employed in teaming. He was drawing some lumber, and stopped over night with his oxen at Mr. Needham's. Needham lost his whole herd. He lost eight or ten of them, and the rest were in a terrible condition. Seven or eight more were condemned, and his whole herd was destroyed, in consequence of Mr. Stoddard's stopping with him over night. Mr. Stoddard sold an animal to Mr. Woodis, of New Braintree. He had twenty-

three fine cows. It ruined his herd utterly. Seven or eight animals died before the Commissioners got there. Mr. L. Stoddard sold a yoke of cattle to Mr. Olmstead, one of his neighbors, who had a very good herd of cattle. They stayed only five days in his hands, when they passed over to Mr. Doane. In those five days, they had so infected his herd that it was one of the most severe instances of disease that we have had. One-third were condemned, and another third were passed over as sound, whether they were so or not. They did not appear to be diseased. The cattle that were passed from Mr. Stoddard through Mr. Olmstead to Mr. Doane, Mr. Doane lent to go to a moving of a building from Oakham to North Brookfield. They were put in with twenty-two yoke of cattle, and employed a day and a half. It has proved since that the whole of these cattle took the contagion. They belonged to eleven different herds. Of course they carried it into eleven different herds—and each of these herds formed a new focus from which the disease spread. Now, in these two ways, the disease has spread in different directions. But when the Commissioners first commenced, they had no idea that the disease extended further than those herds where there were animals sick. And hence their ideas, and the ideas of those who petitioned for the law, did not extend at all to so large a number of herds as have since proved to be diseased; because they only judged of those who manifested disease. As soon as we begun in that circle, we found a second circle of infection, and another outside of that; and by that time it had branched off in various directions to various towns. It assumed such proportions that it was very evident that the Commissioners had not the funds to perform the operations required by the law. The law confines the Commissioners to one operation,—killing and burying. No discretionary power is given at all. Well, now, the Commissioners became entirely dissatisfied with that condition of things, because other measures, besides merely killing and burying, are quite as necessary and important. And when they arrived at that point, and discovered to what extent the infection had spread, they stopped killing the herds, and I believe there has not been a herd killed for twenty days. The policy was then changed to circumscribing the disease, by isolating the herds just as fast as possible,

and as surely as possible. A man's herd has been exposed. There is no other way but to go there and examine it, and take the diseased animals away; then he knows the animals are diseased, and his neighbors know it. That has been the business of the Commissioners for the last twenty days; and the fact that the Commissioners had no discretionary power whatever, and that they were entirely circumscribed in their means, and that it was hard for the farmers to lose their stock and not to be paid for it, induced them to petition the Governor, in connection with the Board of Agriculture, for the calling of a session of the legislature, to take measures for the extinction of the disease. The other gentlemen of the Commission will state what our wishes are in the matter, and I will not take up the time any further.

A Member.—I would like to inquire, Mr. Walker, where the disease came from to this Mr. Chenery in Belmont?

Mr. Walker.—I understand,—and I believe there is no doubt about the fact,—that he imported it from Holland, in which country it has existed two hundred years.

Q.—Have you obtained any facts relative to his herd?

A.—Perhaps I should have alluded to that. Soon after the appointment of the Commissioners, they went to Mr. Chenery's herd, and found that he had them all kept close in his barn, and they were apparently safe, as far as giving infection to other herds was concerned. He thought that his cattle had got well, pretty much, except one animal which was sick. We killed the one that was sick, and it proved to be very badly diseased. We killed one which had been cured twice, and that was in about as bad a state. We killed another and found that diseased, but we went no further at that time, mainly for the reason that his herd was safe, while all around us there was imminent danger.

The great drawback was the fact that before the Commission was appointed, cattle began to go out to grass. The trouble and expense and loss would have been much less if we could have begun operations before the cattle left the barns.

Mr. Griffin.—I would like to ask Mr. Walker what remedy he proposes.

Mr. Walker.—I think my associates have some measures to propose, and I will say nothing at present on that point.

A MEMBER.—I would like to ask if any animal has recovered?

Mr. WALKER.—Animals have recovered in this way. We went to Mr. Woodis, and he says: "There is an animal that has recovered. It has been sick twice—very sick—but it is now all over it." It was an ox, apparently well, ruminating and gaining flesh. Now I think all that was true; it appeared so. Our surgeons examined it, and said the animal was diseased. It was killed, and both lungs were found in a hopeless case—very badly diseased indeed. Yet the animal looked well, and was gaining flesh. We found a great many cases of that kind, where animals appeared to be in a way of recovery, and yet, when they were killed, it appeared that they were hopelessly diseased.

Q.—Have you formed any opinion in regard to the time necessary for the development of disease after exposure?

A.—In regard to that, we have not. And it is the want of knowledge of the laws of the disease that is the great obstacle to our operations, and it is the most alarming fact in regard to the disease, that it don't seem to be understood at all in this country, or even in Europe, where they have had it for two hundred years.

Q.—Is there no literature of the disease at all?

A.—I presume that there may be some; but I understand that there is no single book on the subject. There are articles in the Transactions of the London Philosophical Society, and of the Royal Society of Great Britain; but I believe there is no distinct work on the subject.

Q.—Has killing been the only remedy known in Europe?

A.—That is the general remedy. I have got in a memorandum book a great many statements in relation to that, where it has been adopted. If it is worth while to give them at this point, I will read some of them.

In the year 1714, when it was introduced into England, one of the rules adopted was, that "all such cows as are now in the possession of certain persons mentioned, be bought, killed and burned; or, at least, that the sick be killed and burned—that the sound be kept isolated—and that such as sickened or died of this distemper be burned." In point of fact, they were burned or destroyed,—all of them that were

affected,—and as I understand, the disease was exterminated for the time being.

The orders and regulations that had so fully succeeded in England were enacted and adopted in Belgium, and in Flanders and Piccardy, in France, and succeeded. And within twenty years, they have been adopted in Switzerland, and succeeded. In 1774, when the contagion was carried through Bordeaux to the south of France from Holland,—and in every case we trace this disease to Holland, no country having received it from any other source,—after attempts to cure had failed, the disease was stopped by the killing system, as in Great Britain.

Mr. WENTWORTH.—From what authority do you read?

Mr. WALKER.—Well, Sir, I read extracts from the Transactions of the London Philosophical Society, or else from the Agricultural Transactions of the Royal Society.

Mr. WENTWORTH.—Where do you find it—in a newspaper?

Mr. WALKER.—It comes from a newspaper, but I have seen the gentleman who prepared the article, and have read the authorities myself, and presume it is none the worse for coming through a newspaper. I have taken such good authorities as I could, the literature not being very abundant.

Q.—Who prepared those articles?

A.—Mr. Leander Wetherell, who is connected with the *Cultivator* of this city—I suppose I may state, although I am not authorized to do so.

The disease became naturalized in Denmark, and the practice of inoculation was adopted there and in Holland, but it seems it has not succeeded. Of three hundred cattle inoculated in one instance, not a sixth part were saved.

I have here an extract from a letter of Mr. Josiah Stickney, of this city, who is now in England, and he advocates the adoption of the English course. This comes pretty fresh from a man of education and talent, who has gone abroad for the purpose of collecting information on this very matter. He thinks the cow is less likely to be destroyed, from her greater tenacity of life. So far as we know in this country, bulls seem to be the most proof against the disease. They are much less liable to show severe cases of the disease, than other animals of the same race.

A gentleman in Australia imported an animal which proved to be diseased, when his neighbors agreed to share the loss from the slaughter of his entire stock; and at last accounts, this course had proved successful.

The mortality in England is determined at about sixty per cent. of all that are exposed to the disease. But the facts that are adduced here in evidence show that even a much larger proportion die. Mr. Ratcliffe, out of two hundred lost one hundred and twenty; another gentleman lost sixty-two out of seventy-two; another, thirty-eight out of eighty-seven.

A MEMBER.—I would like to ask, how long after the exporting of these cattle from Holland, the disease broke out in Mr. Chenery's herd?

Mr. WALKER.—They arrived here in the early part of the spring of 1859, and the disease appeared in about two months; in fact, some of the animals were sick when they got here. Two of them were carried in trucks to his barn; but they had no idea then what the disease was. These calves of Mr. Stoddard's left there on the 27th or 28th of June.

Q.—What became of the cows first sick?

A.—I have not kept so close a run of them, but I believe one or two of them died, and the other two are still alive.

Q.—What is your estimate of the value of inoculation.

A.—That is a subject that should be gone into separately. Gentlemen are here who will give you all the facts better than I can. I believe that in Europe, the general opinion is that it is not a success; that is, the remedy is almost as bad as the disease. They cut off the animal's tail, take a piece of diseased lung, and insert it under the skin near the shoulder; it causes a terrible inflammation, a part of the tail rots off, and frequently the whole; the body is ulcerated and covered with offensive sores; and it is a terrible remedy even in those cases where it succeeds, and in all cases it is very uncertain and very unpleasant. It is a very different matter from the inoculation of the kine pock.

Dr. GEORGE B. LORING, one of the Commissioners, being invited to speak, said:—

Mr. Chairman, I hardly know what course the Committee will require the Commissioners to take in making their statement. The request made by a member of the Committee just now, was that the Commissioners should say what their desire was in petitioning for an extraordinary session of the legislature. I can state that in a very few words, Sir, or, with the leave of the Committee, I will go on and present the whole matter of this disease, as it appears to the Commissioners, who have investigated it here, not only from their own observation, but from the best information that they can acquire from Europe, or those portions of Europe in which the disease originated. It will take a very few words to do the former of these things; it will take a great many words, and many tedious ones, I fear, to do the latter. I am perfectly willing to submit the whole thing to the Committee. If they desire me to simply state what our wishes and desires are, I will do so; and if they wish to know what our views are and the best information we have on the subject, I will give that. It shall be for the Committee to decide.

The CHAIRMAN.—The Committee would like to have you state simply, at present, the desires of the Commission.

Dr. LORING.—I would say that the Commissioners found the Act under which they were empowered to operate, for the extirpation of the disease called pleuro-pneumonia, wholly insufficient to enable them to accomplish their object. In the first place, the appropriation was not sufficient. The disease was found extended over so large a territory that it was beyond the physical power of any three men to cope with it. They wished authority to employ agents to assist them legally in the transaction of their business. They further desired power to prevent the spread of the disease by isolation, which was not furnished them. And I mean by isolation, cutting off herds from adjoining herds by various means,—by shutting them up in stalls, or by confining them in pastures, cut off from adjoining pastures. They wished power to disinfect all buildings which had been exposed to the disease—barns and hay. These were the chief points which led the Commissioners to appeal to the State Board of Agriculture, and next, in connection with

that Board and numerous citizens, to petition the governor for an extra session of the legislature.

The Commissioners have in their hands, Sir, some propositions made by highly respectable and influential gentlemen in the Commonwealth, which they simply submit to the Committee, touching an Act which would empower the Commissioners, so far as their experience teaches them, to go on in their work. One of these papers has been drawn up by ex-governor Lincoln, another has been prepared in the office of the attorney-general, and a third has been furnished by the Rev. Mr. Sewall, a member of the State Board of Agriculture. These papers contain the whole proposition of the Commissioners, with regard to the Act which they desire to have passed by the legislature.

Mr. F. W. Bird, of Walpole, addressed the Chair, but was interrupted by a member of the Committee.

A Member.—Not knowing whether the symptoms of the disease are described in the Report of the Commissioners, I, for one, should be happy to be informed in that matter, and I move that Dr. Loring be requested to give a statement in regard to that point.

[No action was taken on this motion.]

Mr. Bird.—Mr. Chairman, I have nothing to say except to state to the Committee, that, representing certain remonstrants, I came in this morning, and, learning that the Remonstrance which I had presented had not been referred to the Committee, —that is, not having passed the Senate,—I supposed that, in accordance with usage, there would be no public hearing until the papers were regularly before the Committee. I inquired of my friend, the Chairman of the Committee on the part of the House, and he said he supposed that public notice would be given to remonstrants and others who wished to be heard. Accordingly, I started to go home, but accidentally met a friend who informed me that the Committee was in session this afternoon. I appear in behalf of the remonstrants with whom I am particularly connected, and it may not be improper to say, having been in consultation with some of its eminent members, in behalf of the members of the Massachusetts Medical Society,

who were active in bringing the matter to the attention of the Society yesterday, with whom I agreed to appear before the Committee to-morrow, and whom I informed that there would undoubtedly be no hearing until to-morrow. I do not desire to go on now. I am sure those gentlemen of the Medical Society are very desirous of appearing. Dr. Bowditch desired to appear,—only it seemed necessary that their Memorial should come before the Committee regularly, through the legislature. That being the case, they supposed the hearing would not take place till to-morrow. It seems to me a matter of a great deal of importance. Unprepared as I am, at present, to represent the remonstrants, and not having counsel as I had intended to have, and feeling that the statements made here, if they are made as evidence, should be scrutinized,—not that I mean to say that the Committee are not capable of scrutinizing them,—we should like to have the opportunity of scrutinizing the statements of the Commissioners, and I only wish to say that I reserve the rights of the remonstrants in this respect, till they appear before you at the proper time.

The Chairman.—It would be out of the jurisdiction of the Committee to act upon the Remonstrance of Mr. Bird, and others, until it comes properly before them.

Mr. Wentworth.—When I made the motion to hear the Commissioners upon the subject of their desire in calling the legislature together, I supposed that we should confine ourselves pretty much to what they considered necessary for us to do, this afternoon. I had understood that the Massachusetts Medical Society had raised a committee for the purpose of memorializing the two Houses upon this subject, and it occurred to me that that committee should be present when any medical testimony was gone into on behalf of the Commissioners. And I suppose it would be very desirable for the Committee, as well as for gentlemen adverse to the course pursued by the Commissioners to be present, so as to learn what they could, from their description of the disease and their mode of treating it. I think it would be better myself to defer going into the medical part of this examination to-day, and to give an opportunity for the Memorial of Mr. Bird to be referred to us in concurrence,

and also the Memorial of the Medical Society, before we go into that part of the case; and in my judgment we had better confine ourselves to the views and wants of the Commissioners, with such other testimony as they may think proper to give in relation of the history of their transactions, and defer the testimony in relation to the disease and its progress until to-morrow. We shall want to know from the Commission how they propose to treat the disease in future, and the amount of money they want of the legislature, in their judgment, in order to enable them to carry out the views they entertain. I think it would be best to confine the Commissioners to these points at present, and to defer the other part of it until the Memorials are presented to us, and the parties representing them are ready to take part in the examination.

By request, Dr. Loring here read to the Committee the three propositions, alluded to in his remarks, from ex-governor Lincoln, Rev. Mr. Sewall, and Mr. Choate of the Attorney-General's office, as follows:—

Worcester, May 28, 1860.

Charles L. Flint, Esq., *Secretary of the Board of Agriculture.*

My Dear Sir,—I have the deepest solicitude in regard to the action of the legislature, in the matter upon which it is specially convened. The most vigorous and effective measures should be immediately prosecuted to arrest the alarming progress of the disease among cattle, which now threatens the destruction of our herds, and the utter prostration of the agricultural interest of the country. Nor do I distrust the success of proper efforts to this end. It is by no means an impossible, or even a very difficult thing, to protect our healthy stocks by isolation on our farms. Let authority be given, if it does not now exist, to require owners of cattle, in infected places, to inclose lots in the middle of their farms, or in places secure from communication, for the keeping of their cattle; or where this cannot be done, to keep them, by *soiling* in their stables. At this season of the year, no great hardship would attend such requirements, and by a rigorous prohibition of the removal of animals from place to place, by driving them on the highways, or by transportation by the cars, the further dissemination of the disease may be prevented. *It should be arrested by whatever human effort it may be accomplished.*

The hazard of entire loss is a sore temptation, with unscrupulous men, to dispose of their animals, which if not *known* to be diseased, have yet

been exposed to contagion. To relieve the public mind from anxiety on this subject, and at the same time to afford additional security against infection, some measure of legislation seems called for, and I have ventured, in a very hasty manner, to propose an enactment to that end. The accompanying sheets are not offered as a precise form to be adopted, but are respectfully submitted to you, as a suggestion, *substantially*, of what might be proposed for the action of the legislature.

I cannot but suggest, also, the *expediency*, if not *absolute necessity*, of enlarging the number and powers of the Commissioners. The field of duty and the labor required to its prompt and effectual discharge, are altogether beyond the physical ability of any three persons to its accomplishment. I think there should be Commissions for large geographical divisions of the Commonwealth.

I pray you, pardon, for my interest in the cause, the liberty I take in thus addressing you, and be assured of the great regard with which

I am, very truly,

Your obliged and obedient servant,

LEVI LINCOLN.

SECTION —. *Be it further enacted*, That no animal of the ox genus slaughtered for food within this Commonwealth, shall in any part or parts of such animal, be offered for sale, until such animal, after the slaughter thereof, with the viscera of such animal, shall be examined by some competent person to be appointed for such purpose, in the manner hereinafter provided; and upon the examination and certificate of such person, it shall be certified to the person slaughtering the same, or the owner thereof, that the flesh of such animal is healthy and fit for human food and sustenance. And if any person shall sell, or offer for sale, within this Commonwealth, the flesh, tallow, hide, horns, or any other part or parts of any animal of the ox genus slaughtered for food, without first causing such animal, and the viscera thereof, to be examined, and obtaining a certificate of the healthy condition of such animal at the time of its slaughter, from the person appointed to such service, he shall be deemed guilty of a misdemeanor, and upon conviction thereof in any court of competent jurisdiction, shall be liable to a fine, to the use of the Commonwealth, of not less than dollars, nor more than dollars, or to imprisonment in the common jail of the county, for a term not less than months, nor more than , in the discretion of the court before which the conviction shall be.

SECTION —. *Be it further enacted*, That it shall be the duty of the mayor and aldermen of every city, and the selectmen of every town in

this Commonwealth, in their respective cities and towns, within two days after receiving notice of the passage of this act, to appoint for their cities and towns respectively, one or more, not exceeding three for any city or town, skilful and competent persons to examine all such animals as are described in the last foregoing section in the manner therein provided after the same are slaughtered; and it shall be the duty of each or either of the persons so appointed, upon application to him therefor, to examine the carcass and viscera of such animals, and if the same are found healthy, and the flesh fit and proper for human food and sustenance, to make and deliver to the persons slaughtering the same, or the owner thereof, a certificate of such examination, and his judgment thereon. And for such examination, the person so making the same shall be entitled to receive of the person making application therefor, or of the owner of such animal, for every animal of one year old, or over that age, the sum of one dollar, and for every animal under one year of age, fifty cents; and if more than one animal shall be examined at the same time and place, one-half the above compensation for each animal so examined after the first. And it shall be the duty of the secretary of the Commonwealth, as soon as may be after the passage of this act, to transmit a printed copy thereof to the clerk of each city and town in the Commonwealth.

Section —. Any person who shall sell, or offer for sale, milk from any diseased or unhealthy animal, knowing that the animal from which such milk was taken was diseased or unhealthy, shall forfeit for each instance of such offence, the sum of dollars, to be recovered on complaint before the police court of any city or town, or any trial justice having jurisdiction of offences within the county; one-half of the penalty to the use of the complainant, and the other half to the use of the city or town in which such sale, or offer of sale, shall be made.

Section —. This act shall take effect from and after its passage.

Proposition of Rev. Charles C. Sewall.

An Act respecting the Disease among Cattle, called Pleuro-Pneumonia.

Be it enacted, &c.

Section 1. The commissioners that have been or may hereafter be appointed by the governor shall have full power to establish any and all suitable regulations in this Commonwealth for the suppression or extinction of the disease among cattle called pleuro-pneumonia; to cause all cattle which may have been exposed to, or exhibit symptoms of, the aforesaid disease, to be forthwith killed and buried, and the premises where such cattle have been kept to be thoroughly cleansed and purified;

and to make such order in relation to the further use and occupation of such premises as, in their opinion, may be necessary.

Section 2. The commissioners shall cause all cattle which, in their opinion, should be killed, to be appraised by two competent judges, under oath, at a fair market value, and the amount of such appraisement shall be allowed and paid out of the treasury of this Commonwealth to the owner or owners thereof.

Section 3. Any person in this Commonwealth having any cattle in his possession, care, or keeping, which shall at any time have been exposed to, or shall exhibit symptoms of, the aforesaid disease, shall be and is hereby required to give notice thereof, within twenty-four hours from and after his knowledge of the same, to the selectmen of any town or to the mayor and aldermen of any city of which such person may be an inhabitant or in which he may have a residence, under penalty for withholding such notice of a sum not exceeding dollars, or of imprisonment in the county jail for a term not exceeding months.

Section 4. It shall be the duty and it is hereby required of the selectmen of any town and of the mayor and aldermen of any city, having notice of the existance of any such exposure or disease among cattle, from the owner or owners or keeper thereof, or from any other source, to inform the commissioners of the same, within twenty-four hours thereafter, under a penalty for the neglect or omission of such duty, of a sum not exceeding dollars, or imprisonment in the county jail for a term not exceeding months.

Section 5. The commissioners shall and are hereby authorized to prohibit the transportation of cattle by railroad or otherwise, into, from, or through any portion of the Commonwealth, where the aforesaid disease may be known to exist or have existed or any portion contiguous thereto, except under such restrictions and regulations as, in their opinion, may be necessary—which restrictions and regulations shall be forthwith made known by posting them in suitable conspicuous places in every town and city of the Commonwealth.

Section 6. The commissioners shall have power and are hereby authorized to take and hold possession of such land or lands in any town or city of this Commonwealth, from which, in their opinion, it may be necessary to exclude all cattle of any description, or within which it may be necessary to inclose the same, for such time as the public safety shall demand. And they shall cause an appraisement to be made of the rent of such lands by the assessors of any town or city wherein such lands are situated, and the amount of such appraisement shall be forthwith allowed and paid out the treasury of the Commonwealth to the owner or owners thereof.

SECTION 7. Any person in this Commonwealth who shall violate or knowingly disregard any order or direction of the commissioners aforesaid, or who shall remove, sell, or otherwise dispose of any cattle which he knows, or has good cause to suspect, have been exposed to the aforesaid disease, shall forfeit and pay unto the treasurer of the Commonwealth a sum not exceeding five hundred dollars.

SECTION 8. The commissioners shall duly certify all allowances to be made under the second and sixth sections of this act, and all other expenses incurred by them, or under their direction, in the discharge of their trust, to the governor and council, and the governor is hereby authorized to draw his warrant therefor upon the treasury.

SECTION 9. All acts and parts of acts inconsistent herewith are hereby repealed.

SECTION 10. This act shall take effect from and after its passage [and continue in force for the term of one year thereafter and no longer.]

Proposition of William G. Choate, Esq.

SECTION 1. The selectmen of every town and the mayor and aldermen of every city may, and if directed so to do by the commissioners appointed under the one hundred and ninety-second chapter of the acts of the present year, or a majority of them in writing, shall establish at some convenient place in such town or city a hospital or quarantine, to which shall be taken all cattle, sick or diseased within said town or city with the pleuro-pneumonia, or suspected to be so diseased, and all cattle ordered to be taken thereto by the said commissioners or either of them; and the same shall be maintained until the said commissioners shall authorize the discontinuance thereof, at the expense of such town or city.

SECTION 2. The selectmen of any town and the mayor and aldermen of any city may, and if directed so to do by said commissioners or the majority of them in writing shall, prohibit the passage through said town, or from or to said town or city to or from any other place, or between different parts of such town or city, of any neat cattle, and shall post up a notice of such prohibition in not less than four public places in said town or city, and may arrest and detain at the cost of the owners, all cattle found passing in violation thereof, and may take all other necessary measures for the enforcement of such prohibition.

SECTION 3. The said commissioners, or a majority of them, may make such orders in relation to the mode of securing cattle during their passage from place to place within the whole or any part of the Commonwealth, and in relation to the treatment of diseased cattle, as they

shall deem necessary or expedient to prevent the communication of said disease and to effect its cure or extirpation; and the selectmen shall take all necessary measures to carry into effect the orders of the commissioners, and such orders shall be published in such newspaper or newspapers in the several counties in which they are to take effect, as said commissioners shall order.

SECTION 4. Every town maintaining a hospital as aforesaid may recover the actual expense of the keeping and treatment of any cattle therein of the owner thereof in an action of contract.

SECTION 5. Whoever knows, or has reason to suspect the existence of said disease among the cattle in his possession, or under his care, shall forthwith give notice thereof to the selectmen or mayor and aldermen of the town or city.

SECTION 6. Whoever knowingly violates the provisions of this act or the act to which this is in addition, or fails to comply forthwith with the lawful orders of said commissioners, or drives or carries, or attempts to drive or carry any neat cattle to or from any place prohibited as aforesaid, except by the license in writing of said commissioners or one of them, shall be punished by fine not more than five hundred dollars, and imprisonment not more than one year for each offence.

SECTION 7. Said commissioners, or the majority of them, may prohibit or regulate as aforesaid the transportation of neat cattle, to or from place to place within the Commonwealth, on any railroad, canal, steam-boat, vessel, or other vehicle of transport; and any corporation violating their orders shall forfeit a sum not exceeding five hundred dollars for each creature so unlawfully transported, and the officers, agents, servants or persons acting in behalf of such corporation shall also be subject to the penalties of the preceding section.

SECTION 8. Any city or town, whose officers shall neglect or refuse to establish or maintain such hospital or quarantine, after they shall be ordered so to do, as aforesaid, shall forfeit a sum not less than one hundred nor more than five hundred dollars for each day's neglect.

SECTION 9. Whoever sells, barters, or offers for sale or barter, or attempts to sell or barter, any neat cattle sick or diseased with the pleuro-pneumonia, or which he has reason to suspect to be so diseased, or to have been exposed to said disease, except with the license of said commissioners, or either of them, or who sells or barters, offers for sale or barter, or attempts in any way to dispose of the flesh of any such sick or diseased creature, except in the mode prescribed by such commissioners, shall be punished by fine not more than one thousand dollars, and imprisonment in the county jail not more than three years.

SECTION 10. Nothing in this act shall be deemed to impair the powers given to said commissioners by the act to which this is in addition.

Said commissioners may also cause any cattle, that are sick of said disease, or suspected to be so sick, or that have been exposed thereto, to be isolated and kept apart from all other cattle, either on the premises of the owner or elsewhere. They may also employ all subordinate agents necessary or expedient for the discharge of their duties. They may also cause to be destroyed any hay or fodder, and other things, and disinfect buildings, building materials and fixtures which they shall deem necessary to prevent the spread or secure the extirpation of said disease.

A MEMBER.—Dr. Loring, how much money do you think is required?

Dr. LORING.—The report which the Commission have made to the governor states that they have killed 842 cattle; and an estimate carefully made shows that there are a thousand head of cattle which must either be killed or isolated for such a length of time as to satisfy parties that they have no disease about them. The amount already expended in the appraisal, is a little rising twenty thousand dollars. That is the amount of the appraisal for cattle already killed. The expenses of this matter are not included in that; what they are we do not know precisely. We have been obliged to employ men to assist us, and to pay farmers for killing and burying cattle, as it seemed a hardship for them not to be paid. The amount of money required, it is impossible to estimate accurately.

Q.—How much money has been involved in these operations?

A.—I suppose from twenty-five to thirty thousand dollars. The appropriation of the last legislature was ten thousand dollars. When that sum had been expended, the Commissioners waited upon the governor and upon the Board of Agriculture to ask for instructions, advice and coöperation. At that time, a paper was drawn up proposing to establish a guaranty fund, in order to enable the Commissioners to proceed, and to secure in some way the payment to the farmers of the losses they incurred. When that fund had reached ten or fifteen thousand dollars, a paper was drawn up and signed by all the farmers who lost their cattle, stating that they were perfectly willing that the Commissioners should go forward and prosecute their duty, and they would wait for their pay till the legislature could pass upon the matter, not relinquishing their rights to this guaranty

fund. The farmers were uniformly anxious that the matter should progress, and to run their proportion of the risk.

I heard the amount of appropriation required stated at $100,000. I should be very much astonished if $50,000 did not finish the business,—besides what has been expended. I have no question that that would cover the whole amount of expense as it stands to-day. I will state the reasons why I have no sort of doubt of the speedy extermination of the disease. I am satisfied that on the western line of this disease the progress has stopped. On the line of the road running from West Brookfield to Ware, with the exception of a number of cattle which were killed in Pelham,—a very ordinary farming town, where there are few cattle,—and which were driven over from Brookfield, I don't think the disease has gone. In Pelham, I think it has entirely stopped. I understand there is a little fear about it to-day, but nothing decisive. But on the road from West Brookfield to Ware, the disease is thoroughly exterminated, I have no doubt. And the Commissioners have every reason to suppose the disease may be entirely eradicated by proper measures.

Q.—Have any measures been taken to disinfect any premises?

A.—The farmers who had lost their cattle were requested to whitewash their barns inside thoroughly, and the Commission, —before I was put upon it,—purchased some tons of a disinfecting powder for the advantage of the persons who wished to use it. In some places, that disinfecting process has gone on. We advised it to be done as early as possible, in order that they might put their hay into their barns this coming season.

Q.—Do you know of any disease beyond the Connecticut River?

A.—I have no idea of a case west of the Connecticut River, and I have no idea of any in Essex County, where it has been reported. A great many cases brought to the notice of the Commissioners are mere matters of suspicion. I am satisfied that no case has occurred in the Commonwealth or out of it that cannot be traced directly to exposure.

Q.—Have you in all cases allowed damages when you killed cattle?

A.—So far as my knowledge goes, we have.

Q.—Whether they were diseased or not?

A.—Oh no, Sir. The practice adopted by the Commissioners was, whenever a herd of cattle was found exposed, the cattle were appraised, and a surgeon was appointed to pass judgment upon the number of diseased animals. After that judgment, the remaining animals that were pronounced sound, were killed, and passed,—as in the case of Mr. Stoddard,—to the credit of the owner, after an appraisement made by three persons.

Q.—In point of fact were those animals sound or unsound?

A.—That is a very difficult question to answer. The Commission have been as careful and stringent as possible in pronouncing upon diseased animals. They have been entirely unwilling to incur any excessive debt on the part of the Commonwealth; and they have been very unwilling that any farmer should suffer unnecessarily. My impression is that nearly all the animals that have been pronounced sound were so.

Q.—In point of fact, were those animals, after the surgeon passed upon them, and you took those that he pronounced unsound and killed them,—were the others sound?

A.—They were so far as I know.

Q.—Were they examined?

A.—They were not examined. The law provides that they shall not be. The law provides that the cattle shall be appraised and judgment passed upon them. We had not that matter at our option.

Q.—In the appraising of these cattle were imported prices placed upon that kind of stock?

A.—No, Sir, not that I am aware of. I was not put upon the Commission until after the visit to Chenery's herd, and don't know what was paid him. Of course, the fair market prices were paid for all cattle. Mr. Walker says the average is about thirty-three dollars a head. That is not very high, considering it is a very good cattle region about Brookfield, and that a great portion of those killed were oxen.

Q.—Do I understand you, Dr. Loring, that these cattle that were judged to be healthy, but had been exposed and were subsequently killed, have not been examined?

A.—They have not been examined.

Q.—Out of the eight hundred, what portion were diseased and what were not?

A.—The Commissioners have no accurate knowledge of what number of that eight hundred and forty were actually diseased and what were not. They have nothing except the pronouncement of the surgeon himself. But I would state to the Committee that in no case has an animal been pronounced diseased that it was not found so.

Q.—I would ask how many animals have died of the disease?

A.—About seventy. Some little statement may be interesting as to the condition of the disease when it was found by the Commissioners. You will remember that it has been in Mr. Chenery's herd more than a year. It is now a year, the 23d of May, since the diseased cattle were brought into this country. The probability is, that Mr. Chenery has not a sound animal in his whole stables. He has lost thirty, and has thirty left, and the probability is that not one sound animal remains. On the 28th of June, the disease was transported from Mr. Chenery's herd to Mr. Stoddard's, in North Brookfield. Mr. Stoddard has had it therefore in his stable nearly a year, or had, when his herd was exterminated. I forget the precise number, but Mr. Stoddard had lost a great many cattle, I think fifteen. A very large portion of the remainder were condemned as diseased, and the condition of the remainder I would not attempt to say any thing about, although I have my opinion about it. The herds exposed to Mr. Stoddard's, last autumn, or when they came to the fall feeding,—all those herds exposed early in the autumn, presented unmistakable and very extensive signs of disease. Now let us come down to a period more recent. Animals exposed to animals brought from Mr. Stoddard's, on the first of November, and transported from one place to another, and carrying the exposure with them, in the early part of the winter, presented slight marks of the disease. The longer it lodges in a region, the more decided and fixed it is; so that it is in one solid mass in North Brookfield, apparent, distinct and unequivocal. Any body can find it there to any extent; and it radiates from that point, more or less, according to the time it has been carried. Now the Commissioners have thought and they still think, that there is no reason why Pelham, for instance, after a year's exposure, should not be as badly off as North Brookfield,—or why any other town where the seed had been sown and the crop ripened, should not be just as badly off as North Brookfield.

Q.—You believe that the disease can be exterminated?

A.—I have no question about it. I think it is one of those distinct and certain things that can be traced and stopped exactly where it is carried. I have no question about the success of the Commissioners, if they are empowered to put a stop to the disease wherever they encounter it.

Q.—Is contagion the only mode of communicating it?

A.—I conceive that it is, entirely. I have never seen a case that I could not trace to Stoddard's or Chenery's herd. The question was asked me in the Board of Agriculture, by Dr. Bartlett, Why the disease did not extend from Mr. Chenery's barn in Belmont? Why has not Mr. Chenery's herd sent it about? My reply was, he has sent it about. He shut it up in his own barn, but he took an animal out of his barn and carried it to North Brookfield, and it broke out there as if he had thrown a fire-brand into a powder barrel. No man can lock an epidemic up in his house. You cannot monopolize such a thing as that. It is because it is a contagious disease, that it has not gone from Mr. Chenery, except when he sent an animal into a neighborhood where cattle come in contact and are passing back and forth continually.

Mr. Chairman, there is a gentleman here, who has had a good deal of observation in regard to this disease, and I think it would be interesting to hear what his experience has been. I allude to the Rev. Mr. Lindley, from South Africa, who has seen the disease there to a considerable extent.

Adjourned to twelve o'clock, Friday.

SECOND DAY.

FRIDAY, June 1.

The Committee met at 12¼ o'clock, and the examination of Dr. Loring was resumed.

Mr. WENTWORTH.—How many cattle have been killed that had, in the judgment of their owners, commenced to recover?

A.—That, it is impossible for me to tell.

Q.—So far as you know?

A.—I cannot give any sort of a statement about it, because the owners themselves were wholly ignorant; they expressed no opinion upon the matter.

Q.—Do you not know of cases in which cattle have been killed, where you have been informed by their owners that they had been sick, but were getting better, or appeared to be better?

A.—We have killed animals which had been sick, and in reference to which the owners expressed an opinion that they were getting better.

Q.—Was that opinion founded upon any facts disclosed to the Commission, such as improved appetite, appearing more lively, and so forth?

A.—No, Sir, it was merely an expression of opinion.

Q.—Did you examine them to see whether they were apparently getting better or not?

A.—Yes, Sir, we did. After they were killed we examined them to ascertain, as far as possible, the physical condition of the animal at that time. It was impossible for us to tell whether the animal was getting better or not, because we had no data to make our comparison upon.

Q.—Assuming the statements made by the owner with regard to the previous state of the creature to have been correct, was it not getting better, in your judgment?

A.—So far as external appearances went, the animal was getting better.

Q.—Were there many such cases?

A.—I remember only one or two.

Q.—On examining those animals, after they were killed, what was your opinion then?

A.—There were unmistakable marks of disease.

Q.—Did they bear the appearance of recovery?

A.—They bore the appearance of having the disease in a circumscribed condition, if I may use the term. I can cite a case in illustration, if it would be satisfactory to the Committee to hear it.

Mr. WENTWORTH. State whatever you deem important in regard to the matter.

WITNESS.—I remember one case, distinctly. It was that of the ox belonging to Mr. Doane, which has been referred to as having imparted the disease to twenty-three yoke of oxen in one team. That ox was killed, perhaps within a month. He had, early in the winter, given indications of the disease, such as we found there, and the question which arose at the time he was killed, was what the appearance of the disease would be in that animal. As he was led out of the stable, any one would have said, "He looks well enough." He was a very good looking red ox—a large Devon, a breed known to all farmers; that is a sufficient description of the animal. Any body would have bought him to fatten, either in stall or pasture. He had, however, at the same time, a slightly pinched appearance, and any one would have said, "He don't look exactly right, either;" and an acute observer of cattle would have hesitated about buying him. He was taken out and killed by the Commissioners, because it was known that he had been exposed to the disease—he having been in Mr. Leonard Stoddard's yard when the disease was there—and it was understood and known that he had imparted the disease to the twenty-three yoke of oxen in the team. Now the question with the Commissioners was, after the animal was slaughtered, what condition of the disease shall we find in those lungs? Will it be that usually found in the early stages of the disease which has been called hepatization? Will there be the effusion of serum ordinarily found in the chest? Will there be hypertrophy of the lungs, very commonly found? Will there be thickening of the serous covering of the lungs, and will they be in a state of carnification, or will it be that circumscribed condition of the disease, in its chronic form, where nature seems to have separated the diseased part from a healthy part, and impacted it there during the life of the animal? The general external appearance of the lungs was that of fair health. The serous covering of the lungs was a little thickened and there was not that pink, healthy appearance which is usually found in the healthy subject. Upon cutting into the lower lobe of the right lung, a portion of diseased matter, perhaps half as large again as a goose egg, was found enclosed in a fibrous sac about the strength and consistency of the lining membrane of a fowl's gizzard, and about the same color. This substance, of the size

of which I speak, resembled decayed Stilton cheese in color, but considerably harder in consistency. It was entirely separated from the rest of the lung. On splitting the cyst open, it fell out and lay there, a mass of curdy looking matter, for the inspection of the Commissioners. That was a case, Sir, in which the disease was there in the lung of the animal, and it stopped there.

Q.—Did the rest of the lung appear to be healthy ?

A.—Fairly healthy ; when the lung was cut, the fine nice crepitas which is heard in cutting the healthy lung, and which every butcher and surgeon knows, was not to be heard. A portion of the lower lobe of the lung, in which this disease had been isolated, cut much like the muscle of young veal or chicken ; not with that fine, sharp, clear crackling which you hear when cutting a healthy lung.

Q.—Would you not think it probable that that diseased portion might have been absorbed, and eventually removed ?

A.—No, Sir, I think it would have remained there to the end of the animal's life. I do not think there were any absorbents at work upon it. I think nature had set up a process of separation, had accomplished it, had enclosed the disease in a sac, and there left it. I think every surgeon and physician would see an analogy between such a case as that and the case, frequently found in the human subject, of an old tuberculous deposit in the lungs, which may have been there forty years, and have been carried through life without any apparent injury to the person in whose lung it existed.

Q.—And you believe this ox would have lived without being materially injured ?

A.—I have no doubt he would have lived, but whether he would have been an able-bodied ox would have been a question. I think that a man who was about purchasing an ox would rather have one with healthy than diseased lungs. I suppose that a farmer would not be very apt to select a man to work for him who had had hemorrhage of the lungs, or who had tubercles in them.

Q.—Do you think he would have communicated the disease ?

A.—That is a very important question, and it is one that we have investigated to the extent of our ability. We have endeavored to ascertain from the authorities in Europe, and

from observations made here, the point of time when the disease fails to be communicable. I will state a case which came under our observation on that point. A yoke of oxen were sent from West Brookfield, from a part near the depot to the northern portion of the town, somewhere about the 20th of January. They were sent to the farm of Mr. Gleason. He kept them until the last of March. He had noticed that they did not do very well, and sold them to Mr. Makepeace, I think on the 28th of March. Mr. Makepeace took them home, and when he got them there, he found the oxen were lame. Supposing that the lameness was owing to their want of shoeing, or to improper shoeing, he had the shoes set, but the near ox was still crippled. Mr. Makepeace worked him through the spring, but he did not thrive; he took on no flesh, and when I saw him he was in precisely the condition that farmers usually describe as "foundered." Many men would have said, that ox has been mealed too hard; he is good for nothing, and never will be. The animals, as I stated, went into Mr. Gleason's hands about the 20th of January, as near as we could ascertain. We found the disease in Mr. Gleason's barn. Any man would have said that some disease was there. We found the Pleuro-Pneumonia was there, I should think about the last of April. The question came up, "How came it there? Was it from this same yoke of oxen, or from some other animal?" He had purchased a doubtful cow that had been carried there, and we thought it might be attributable to her. Mr. Gleason's herd was taken possession of by the Commissioners. We followed that yoke of oxen to Mr. Makepeace's, and there they were killed. They presented a very extensive—almost carnification—hepatization of the lungs. The lungs did not collapse; their weight was more than twice the ordinary weight of healthy lungs. They were almost impervious to air. The bronchial tubes would admit a very little air; but the air cells seemed to be entirely destroyed. There was but a very small portion of either lung which was in such a condition as to be of any practical service to the animal. Then the question immediately arose, whether Mr. Makepeace had got Pleuro-Penumonia in his herd. These cattle had unquestionably given it to Mr. Gleason's herd some time between the twentieth of January and the last of March. Now the question arose, whether they

had given it to Mr. Makepeace's herd, into which they had gone after they had been a long time sick, and in what is unquestionably to be called a chronic condition of the disease. Mr. Makepeace's herd was examined in the best way in our power—by percussion and auscultation—by the best tests we had. They were examined as far as their natural signs were concerned—the appearance of their coats and eyes—every thing about them by which a farmer would judge of his cattle. There was no cough; they looked well; but in order to put the matter to a still further test, the cow that looked poorer than the rest was taken out and killed, and she gave no signs of disease in the lungs.

The solution seemed to be this—that these animals were in Mr. Gleason's herd while suffering under an active condition of the disease, and communicated it; and that they went into the other herd with the chronic condition of the disease, without the ability to impart it. This was the conclusion to which we arrived. We have had no other case that would enable us to test the question. I do not say that our conclusion is correct, because the best investigators in Europe have been unable to decide at what stage the disease becomes incommunicable.

Q.—As a medical man, taking the first case, do you not think that the ox had passed the period when he could communicate the disease?

A.—I should think he had.

Q.—In your judgment, what is the latent period of this disease?

A.—If you mean, the time between the taking of the disease and its appearance, it is impossible to tell. Mr. Chenery's animals, which were brought from Holland to this country, had the disease with them. They were some of them in such a condition when they were landed here, that it was with difficulty they could be taken to his farm at Belmont. Mr. Chenery's calves, that he sent to North Brookfield, communicated the disease to Mr. Stoddard's oxen within a fortnight, and in ten days, three of his cattle were dead. An animal sent from Holland, or some part of Europe, to Australia, (the account is authentic, and published in an official document,) carried the disease with him the whole passage, without presenting any appearance of it; it broke out when he arrived there, and was

communicated to the herd of a person there, which herd was exterminated, and the disease stopped. I do not know what the time is. I do not know that it is possible to ascertain it.

Q.—Has there been, at any stage of the disease, an examination of the blood?

A.—I do not know that there has been, either here or any where. I never have heard of any.

Q.—In your judgment, is not the blood primarily affected with this disease?

A.—I have no question about it myself. I suppose it would depend somewhat upon the medical theory adopted, as to whether fluids or solids take the disease.

Q.—If it be communicable by inoculation, must not the blood be affected by it?

A.—I should have no doubt of it. I would not say positively. I should suppose that some fluid in the body must be affected.

Q.—In your judgment, is the disease a contagious one?

A.—Entirely so.

Q.—How is it communicated? by the breath, the saliva, or actual contact?

A.—I have no doubt that it is communicated by the breath, by the exhalations from the bodies of the animals, and by the mucous discharges, if there are any, from the throat and nose of the diseased animals. I think the opinion expressed by examiners in Europe is entirely correct—that a diseased animal, confined in a close apartment with any number of sound animals, will infect the air of that apartment in such a way that all the animals in it that are capable of taking the disease, will take it.

Q.—In your judgment, is this disease pleuro-pneumonia?

A.—Yes, Sir, as far as I can ascertain from the best examination I have been able to give.

Q.—Have you ever known any disease of the lungs or pleura to be contagious in the animal kingdom?

A.—I have never known any other disease of the lungs to be contagious in the animal kingdom. I mean, specific disease of the lungs. The disease has been named pleuro-pneumonia, but it is not the common pleuro-pneumonia. It has been designated in Europe, exudative and contagious pleuro-pneumonia, in order to distinguish it from that form of pleuro-pneumonia which is pure acute disease of the pleura and lungs. The

name was given it in order to signify what organs were diseased.

Q.—In what is it distinguishable from the real pleuro-pneumonia, as known to physicians?

A.—I should think that the general point of distinction was this—that in this disease, the inflammation was what might be called sub-acute inflammation, whereas, in the common form, the inflammation is acute and rapid. I think it is doubtful whether this disease would ever end in that form which is called gray hepatization, in which the body of the lung is filled with pints of pus, following on the acute stage of the inflammation. That is as near as I can come at it, for my medical knowledge has been some time out of practice. I have never seen that form of red hepatization which is seen in common pneumonia. In this case, the hepatization was more confined—not lobular, not confined to the lobes of the lungs; but I have seen it stratified, so to speak; a stratum of hepatization and a stratum of healthy lung.

Q.—Do you know whether the pleura or the lung is first affected?

A.—No, Sir, I do not.

Q.—Have you ever seen the lungs in this disease when they were inflamed, and if so, what was the nature of the inflammation?

A.—I have seen them when they presented the appearance of being inflamed. It is that form of inflammation which is designated as sub-acute. They presented the appearance of congestion. I have seen the pleura in that condition. I have seen some of the most beautiful specimens of the ordinary injected appearance of the serous membranes in this disease.

Q.—How would you describe this inflammation? Was it local or general?

A.—In some cases, it would be diffused over the whole of the lungs; in other cases, it would be circumscribed and limited to the lungs themselves. I have seen lungs in which the hepatization—call it inflammation or not—or injected condition, was very much diffused, and others in which it was very much circumscribed.

Q.—Have you seen any matter generated by this disease escape from the cattle?

A.—No, Sir.

Q.—Have you seen any matter in the lungs themselves—any pus?

A.—In one case, I saw an abscess which discharged pus.

Q.—What was the character of the pus?

A.—It had the appearance of ordinary pus, which has been confined in an abscess; what might be called scrofulous.

Q.—Have you seen many such instances?

A.—I do not remember more than one or two.

Q.—Have you had an opportunity to see cattle killed frequently?

A.—Yes, Sir.

Q.—Is it an uncommon thing to find unhealthy lungs in cattle that pass for healthy?

A.—I should think it was.

Q.—Did you ever see one?

A.—I never saw a diseased lung taken from an animal killed in a healthy beef condition.

Q.—Have you never heard of any such case?

A.—No, Sir.

Q.—At what time did you take your place upon the Commission?

A.—I forget, Sir. I think about the 12th of April; perhaps after the middle of April.

Q.—Since you have been on the Commission, has any thing been done with a view to curing any of these cases?

A.—No, Sir.

Q.—Has any examination been made by the Commission with a view to attempting a cure?

A.—No, Sir.

Q.—Previous to your taking a place upon the Commission, do you know whether any attempt was made to cure, or any examination made with a view to attempting a cure?

A.—No, Sir; I never heard of any. I do not understand that the attempt has been made in any case.

Q.—Do you know whether any attempts to cure were made before the Commission was appointed?

A.—I know that attempts were made by private parties. But the Commissioners, acting under the appointment of the legislature, did not conceive it to come within their province to

attempt to cure the disease. We conceived it to be our duty to extirpate it.

Q.—And you have no plan to propose other than the course you have adopted?

A.—The plan of the Commissioners is contained in the act which was submitted as coming from the attorney-general's office.

Q.—You would say that that is your general plan, subject to such variations as might be suggested?

A.—Yes, Sir.

Mr. Woodman.—What is the view of the Commission in reference to the establishment of a hospital for the study of the disease and its cure?

A.—I am not aware what the view of the entire Commission is; I am perfectly free to express my own opinion upon it —that it is utterly futile. I never saw a cow that I thought was worth curing, if the expense of her cure would cost more than thirty dollars. I don't understand that the effecting of a cure would insure us against the spread of the disease. That is the point we want to come at.

Mr. Wentworth.—Suppose an ox worth a hundred dollars?

A.—That would change the question, unless the surgeon's bill ran up above that amount. Such bills increase rapidly.

Q.—Do you understand that cases without treatment are always fatal?

A.—No, Sir, they are not.

Q.—What proportion are fatal without treatment?

A.—It depends somewhat upon the locality, and the condition of the animal.

Q.—Suppose them to be favorable in both cases?

A.—I should suppose that from fifteen to twenty per cent. would be fatal; that is, taking the most favorable conditions under which the disease exists. In the present condition of the disease on the south side of the Thames, it is said that ninety-five per cent. are fatal.

Q.—Would it not pay to isolate the herds?

A.—I have no question that it would, where it could be done in an economical and proper manner. The Commission

ers, in one or two instances, have done so, and they have no doubt that they have done it with success. They did it on their own responsibility, as a matter of economy to the Commonwealth.

Q.—Do you not think the system of isolation might be carried out?

A.—Possibly it might be adopted with good effect; it would depend altogether upon how long a time the system of isolation would have to be maintained. If adopted by the Commonwealth, it would be attended with such large expense, that it would be impracticable. I think it would be more practicable to kill, and certainly it would be more effectual. To illustrate this, suppose that on the 29th of last June, Mr. Chenery had killed his whole herd, instead of sending the calves to North Brookfield; I suppose no one doubts that that would have ended the disease. Now if that is true, it would be equally true in regard to the present state of the disease on a much larger scale. I have no doubt that a combination of extirpation and isolation would stay the progress of the disease.

Q.—Was not Mr. Chenery's herd isolated?

A.—It has been very generally isolated. He is not a cattle dealer. He purchased his stock and has kept it very much to itself. Since the disease has been raging to such an extent, he has been compelled to keep the whole herd, and with the exception of the animals sent to North Brookfield, and one cow which I know of his having sold to a gentleman near him, (which cow is now sick,) no animal has been sent out.

Q.—There has been no instance of any communication between his cattle and any of his neighbor's herds?

A.—No, Sir.

Q.—Is it unusual to find marks of disease in animals that are uncommonly fat?

A.—I have seen marks of disease in the liver, but not in the lungs.

Q.—Any in the heart?

A.—I do not remember any in the heart, but I have seen the liver of very fat oxen, (stall-fed cattle particularly,) very much diseased.

Q.—What would be the effect upon the beef, if the animal were killed before any manifestation of the disease?

A.—That is a very difficult question to answer. I should suppose the effect of it, certainly, could not be beneficial.

Q.—Supposing it to be a poison in the blood?

A.—Then certainly it could not be beneficial. It is possible, however, that the effects of heat in cooking might dispel the poison, so that the meat might be nutritious and healthful.

Q.—What is the limit of distance within which the disease is communicated?

A.—I have no doubt that the distance within which it is communicable depends very much upon the state of the atmosphere and the condition of the animal. When the diseased animal is kept in a barn, it may be carried a hundred feet or more. It has been carried across a road, with a slight wind and in a heavy state of the atmosphere. Precisely how far it may be carried, I cannot say,—perhaps two or three rods.

Q.—Is it communicable by the clothing of the attendants?

A.—That is a question I cannot answer. It is supposed to be so in Europe. It is thought that the herdsmen who attend cattle badly diseased may carry the disease in their clothing, and they are therefore kept away from healthy herds. I have been very careful, myself, after having been into barns where the disease existed,—of course in the spring of the year they are very hot, and with a good deal of moisture,—to take off the clothes I wore there, and put on others before I went into my own stables, feeling that it was an act of safety, at least.

Q.—What number of cattle did Mr. Chenery's herd consist of?

A.—It consisted, originally, of sixty head of cattle. I think he has lost about thirty, and has about thirty left.

Q.—That would be about 50 per cent. of fatality in his herd?

A.—Yes, Sir. There were two or three cattle that were found very much diseased, and that would unquestionably have died if they had not been killed.

Q.—Do you know whether that herd has been subjected to medical treatment?

A.—Yes, Sir. It was submitted to the care of a very skilful veterinary surgeon all last winter. He brought me a portion of the lung of one of the diseased animals last November, sometime before there was any excitement in regard to the matter, and before I was aware that there was any contagious

disease there. The condition of the lung was very peculiar. I did not understand it then, and I had never seen any thing like it before.

Q.—Were there any adhesions of matter in that case?

A.—No, Sir, there were not.

Q.—Where there are adhesions as the result of the disease, it implies a more active state of inflammation in the earlier stages?

A.—Yes, Sir.

Q.—Do you make a distinction between congestion and hepatization?

A.—I used that word [congestion] because I knew it would convey to all medical men a more perfect understanding of the condition of the lungs.

Q.—Would not a congested condition of the lungs more clearly imply a bad condition of the blood of the animal?

A.—That it would be impossible to tell. A purely congested condition of the lungs would imply pulmonary apoplexy. The adhesion of the substance of the lungs, in this disease, would indicate that it was not pure congestion.

Q.—In your judgment, would the daily secretions convey the disease?

A.—I should suppose they might, Sir.

Q.—If the cyst contained the diseased portion, may it not have been softened, and then absorbed and carried off?

A.—I do not think the inner lining of the cyst had any active force in it, but simply circulation enough to keep it in existence. I do not think there were any absorbents in it. I think the whole circulation was cut off by the disease from the contents of this cyst, just exactly as if, instead of this diseased matter being separated, it had been a portion of bone lying there, or a bullet. It was precisely in the condition of a foreign body lying there.

Mr. Bird.—I understand that the Commissioners are not agreed upon either of the three plans that have been submitted.

A.—I am not aware that they differ materially in regard to the plan I have mentioned. Perhaps some small items may be objectionable.

Q.—As a matter of fact, the Commissioners have not agreed upon any plan?

A.—We have not. We supposed that was for the committee and the legislature to decide. We have our own views upon the matter. I am not sure that the Report indicates an intention of killing exclusively. I think it speaks of isolation.

Q.—Have you ever seen any effusions in the cavity of the pleura?

A.—Yes, Sir, they are very common in these cases.

Q.—One hundred and eighty-five animals are reported as having been killed. How many of those, upon subsequent examination, appeared to be diseased?

A.—They were not all examined, but all that were examined were diseased.

Q.—How many were examined?

A.—The larger portion of them.

Q.—How many?

A.—I do not know, Sir.

Q.—How many, do you think, did you examine yourself?

A.—I do not know, Sir.

Q.—In what stage of this disease do you regard it as contagious?

A.—I have stated already that I was not aware of any particular stage in which the disease is not contagious. I am satisfied that it is contagious in all its early stages, and I have the opinion of eminent surgeons in Europe to sustain me. How long it continues I do not know. Whenever the disease is in an active condition, it is contagious; and I think there are some facts which tend to show that while the disease may not exist in one portion of the lung, there may be in the other portion of the lung, or in the other lung, an active disease going on which may be contagious.

Q.—Has the meat of these diseased cattle been used for food in Europe?

A.—The meat is used for food in some parts of England. The laws of England, as at present existing, are by no manner of means satisfactory either to the farming community or to scientific men, and it has been advised there that animals found diseased should be fattened at once.

Q.—Would it not be impossible to fatten a diseased animal?

A.—It would be impossible to fatten an animal much diseased, but I have no doubt that the custom of high feeding

adopted in England, and the slow progress of the disease, would allow a quick and rapid process of fattening.

Mr. BIRD.—You say that in some cases the disease has been communicated across a road?

A.—Yes, Sir, I think so. I think there is no question that the animals exposed to the disease in North Brookfield, were exposed at that distance. Animals were found diseased, and badly diseased, who took it standing in the road while the diseased animals were in a barn on the other side.

Mr. BIRD.—In other words, they had the disease after they had stood in the road?

A.—Yes, Sir, I am willing to take that correction. Every man knows that the atmosphere round a barn is filled with effluvia from it; and I have no doubt that if a disease were in a barn, the poison would come out in that effluvia and communicate the disease.

Q.—Do you know of any cattle having been in a position to take the disease that have not taken it?

A.—Yes, Sir, the contagion is not universal. There are cases where animals have been exposed directly to the disease, and have not taken it.

Q.—What is the general opinion as to what condition of the system would insure the animal from the disease?

A.—It is impossible to say.

Q.—In your opinion, does the communication of the disease depend more upon the contagiousness of the air or the pre-existing condition of the animal?

A.—Upon neither, but upon a combination of both.

Q.—You state that in certain cases, this disease has evidently been carried in the system weeks, months, perhaps years—long voyages at any rate. If that is the case, how can we know that we have ever arrested the progress of this disease here? For instance, suppose you isolate the diseased animals and at the end of a year conclude that the disease has been eradicated; it may be that four months before, it has been communicated to other animals.

A.—All that is very true. We can simply judge by results. We may infer that a portion of territory which has been exempt

from disease a certain number of months will continue to be exempt.

Q.—Have you ever known of any cases of the disease arising spontaneously?

A.—No, Sir, I never have. I never have seen any case that could not be traced to Mr. Chenery's herd, either directly or indirectly.

Q.—Did you make any other examination which disclosed phenomena similar to those which you particularly describe here—the partially coagulated matter enclosed in a cyst?

A.—Yes, Sir, we had several such cases. It is a common phenomenon. I do not know that I should say common—it is not unusual.

Q.—Did you read the letter of Mr. Campbell in the *Journal* of yesterday?

A.—Yes, Sir.

Q.—Do you recognize the same disease in the one he describes?

A.—No, Sir. The disease he describes is very different from this.

Q.—What did you think of his treatment?

A.—I thought it was very well for the disease he was treating.

Q.—Do you know of its having been tried on cattle affected with this disease?

A.—I think it was tried by Dr. Saunders upon Mr. Chenery's cattle last year. I think he told me that was the course he pursued. Before I was aware of what the disease was, I frequently saw him and conversed with him in regard to it.

Q.—How is the disease treated in Europe?

A.—The disease in Europe is looked upon like the steppe-murrain, the rinderpest, and all such diseases, and is now treated there by sanitary regulations, as far as possible.

Mr. Bird.—This is the same disease that is found in Europe?

A.—Yes, Sir.

Q.—Then they have given up the practice of killing?

A.—No, Sir. I said sanitary regulations. Among these are very stringent ones for killing the cattle in some countries.

Mr. Andrew.—Do you know whether they limit the killing to cattle actually diseased, or kill those that have been exposed?

A.—They kill both, and pay for both.

Q.—Do they kill all, as a general practice?

A.—No, Sir, they isolate and kill.

Q.—What is the rule of discrimination?

A.—They have adopted none.

Q.—Then experience has developed no consistent system?

A.—The best system, as near as I can ascertain, is that adopted in Denmark; and in that country, it has checked the progress of the disease, and, it is supposed, would have exterminated it, had there not been further importations of cattle from Holland. The precise method of isolation which they adopt—whether they shut the animals up in barns, or enclose them in fields—I do not know.

Q.—Where does this rule begin and leave off?

A.—They isolate animals said to be or found to be in a sound condition. They kill all animals that are diseased, or that have been so far exposed as to be considered badly diseased.

Q.—Then, if they kill all those which are diseased, and which have been exposed, directly and immediately to infection, what are left to be isolated?

A.—That is decided, as near as I can ascertain, by what is called the district veterinary surgeon.

Q.—And his judgment is applied to each particular case?

A.—Yes, Sir.

Q.—And not governed by any general rule of science?

A.—I suppose a district veterinary surgeon of Denmark would apply all the science he had.

Q.—But there is no rule that can be stated for the adoption of other people?

A.—None, except the rule I have already stated. Medicine, as near as I can ascertain, is a less exact science than law.

Q.—What I wanted to find out was, whether, as yet, experience has developed any rule of general application, so that men who should study the matter scientifically, in the light of other men's experience, would be able to see how far to extirpate, and how far to isolate cattle disease and how far the two could be modified, and how far you could apply either or both to any class of cases?

A.—I think that any intelligent man who had read the history of the disease, and the accounts which have been published, would be able to apply to individual cases the information he had obtained.

Q.—Is there any general statement of the result to which science and experience have arrived that could be made, so that intelligent men could understand it?

A.—I do not know that I have stated it so that intelligent men could understand it, but I have stated one. I stated that the system adopted in Denmark was one of isolation and extirpation; that diseased animals were killed; that those animals that were supposed to have been so exposed as to carry the disease, were also killed, and that healthy animals that had been exposed were isolated and kept away from other herds to prevent even a chance of their conveying the disease to others. That is the plan adopted in Denmark.

Q.—Is that plan in print?

A.—It is in print. The accounts are found in English journals.

Q.—You speak of isolation—do you mean the isolation of individual animals?

A.—I suppose it depends upon the particular case. The probability is, that the system is the shutting up of the herd of the person where the disease is found to exist, either in a barn or a field. I suppose it would not be individual isolation, unless the man had only one animal. That system we have adopted in North Brookfield.

Q.—Would that not endanger the whole?

A.—The danger would be, that the disease would spread through that herd, and the advantage would be, that it would not go beyond it. To illustrate: we found that the herd of Mr. Gleason was diseased, and the animals were killed. It was found that the pastures around Mr. Gleason's had contained about thirty or forty head of cattle. They had been thus near to Mr. Gleason's about a year. The Commissioners inferred from the length of time which had passed, that the contagion could not have gone beyond that field. So they took the animals from the pastures surrounding Mr. Gleason's, and put them into his, and, in that way, shut out the whole disease from the surrounding country. That was one process of isolation.

There have been other cases, where we have had one single cow shut up, and others where parties have agreed to keep their animals shut up.

Mr. WENTWORTH.—Do you know what degree of exposure to the infection constitutes such a liability to take the disease as would bring the animal within the rule requiring him to be killed?

A.—No, Sir; I do not think there is any rule that can be followed.

Q.—How long has that system been in existence in Denmark?

A.—I think it was adopted in 1845, for the first time. There had been appearances of the disease for a few months, that had troubled the farmers and the government a great deal, and in 1845, this system was adopted, and adopted with entire success; so much so, as I have said, that the best investigators of the matter have come to the conclusion, that had no more cattle been brought into Denmark, they would have had no more of the disease. They attribute the recent appearance of the disease to one hundred and eighty oxen brought into the country from Hungary.

Q.—Has the Danish system commended itself to the English mind, so that it has been adopted there?

A.—It has commended itself to the mind of the English farmers, and the English Commissioners, who have investigated the matter; it has not so far recommended itself to English legislators that they have adopted it.

Q.—Have not the British Parliament taken this matter up?

A.—They have had commissioners to investigate it repeatedly, and they have certain acts in reference to it. I cannot state precisely what those acts are, because I have not been able to ascertain; I have seen an act passed in 1744 applicable to a disease then prevalent in England—the rinderpest—a virulent and contagious disease. The question of recent legislation has been, whether they should increase the stringency of the acts already existing.

Q.—If this disease is contagious, is it not also epidemic?

A.—It is not supposed to be so. It has given no indications of being so in this country, and none that I am aware of in Europe.

Q.—Then you feel very confident that if any measures could be adopted that would restrain the contagion, it has no epidemic character that would make it dangerous?

A.—I should be perfectly willing that a herd of mine should be within half a mile of a diseased herd, if no diseased animal could come into it.

Q.—Has that question arisen in the investigations which have taken place in England or in this country, and have scientific men of experience come to a conclusion as to whether it is epidemic or not?

A.—The best science that has been applied to it has agreed that it is not epidemic.

Q.—Still there are two sides to the question, are there not?

A.—I don't think there are; I have never heard but one. The question, in fact, has not been discussed in Europe, at all. So far as I have investigated, I have not found any authority on the other side.

Q.—Then you consider it a well defined and clearly established contagious disease, as much so as the smallpox?

A.—Yes, Sir; and all the analogies, all comparisons that are made in Europe, are made with such diseases as the smallpox.

Mr. BIRD.—Did you ever see any cases of the disease known as "hog cholera?"

A.—I never have seen any.

Mr. BIRD.—I have been told that that is the same disease as this—that it affects the lungs in the same way.

WITNESS.—I have never seen any cases of that disease; I have only seen newspaper statements that the lungs were diseased.

Mr. ANDREW.—When you come to this question of the contagiousness of the disease, is it at all certain how the infection is communicated; whether its communication in all the three different methods is possible, or whether it is uncertain as to which of the methods of communication is really operative?

A.—I should think it was uncertain.

Q.—Then the result of experience is simply this: that it is concluded that the disease is contagious; that different methods are presented by which it may possibly be communicated, but it is not

certain which actually communicates it? The three methods are, by the breath, by the exhalations from the body, and by contact with the mucous discharges. Now, is it certain that it is communicated by any one, or any two, or by all three of these, or is it merely an inference from the possibility of communication being made through all these methods?

A.—I look upon either of these as simply a modified form of the others. I do not mean to say they are identical, but I mean to say it is impossible to tell the way in which any contagious disease is imparted.

Q.—You infer that in the case where it was communicated from across the highway, it must have been by the exhalations?

A.—I should think there must be going out from such diseased animals what might be called morbific poisons.

Q.—They would go out too from the mucous discharges?

A.—I have no doubt that the diseases might be taken from the mucous discharges at the same time. That is, I suppose that if a diseased animal is tied up in a stable and fed in a stall, he would communicate the same disease to a healthy animal brought in and tied up at the same stall.

Q.—Then this proves that it may be communicated without contact?

A.—Yes, Sir.

Q.—Can you give any reason why this disease of the lungs is regarded as contagious, while other diseases of the lungs are not contagious, and have no feature corresponding with contagious diseases?

A.—There is no way of accounting for it that I know of. The argument at Amherst was, that this disease could not be contagious, because there is no disease of the lungs that is contagious. There was no answer to be made to it. If they were satisfied with the reasoning, we were.

Mr. Bird.—The reasoning seemed one way, the facts the other.

A.—We thought so, Sir.

Mr. Andrew.—Now, do you think, that in truth and in fact, setting aside what may be learned from abroad, we have had

in this country, facts enough from which to deduce a theory on the question of contagiousness?

A.—I think we have had facts enough to show us that the disease is contagious. I think those facts cannot be set aside.

Q.—There has been no effort made through New England generally to ascertain whether sporadic cases have existed?

A.—No, Sir; but there have been a great many examinations of cases which, if they were pleuro-pneumonia, would have been sporadic, and in no case has it proved to be the disease.

Q.—How many cases of certainly established pleuro-pneumonia have existed, to your knowledge?

A.—The Report states one hundred and eighty-five animals killed, a large portion of which were found to be diseased. Thirty animals died in Mr. Chenery's herd, and, I believe, about fifty in North Brookfield and thereabouts.

Q.—Do you speak now of cases in which the existence of that particular form of disease was established by actual examination?

A.—Yes, Sir; I speak of the cases where *post mortem* examinations indicated the presence of the disease.

Q.—How many cases have existed in this Commonwealth in which *post mortem* examinations have been resorted to, and in which, by that examination, the existence of that disease has been ascertained?

A.—I should think about two hundred.

Q.—That includes animals which have been killed, as well as those which have died naturally?

A.—Yes, Sir.

Q.—How many have died naturally, in which the disease has been known to exist?

A.—From seventy to eighty.

Q.—Who made most of those examinations beside yourself?

A.—Dr. Dadd was with us. Dr. Saunders has made some examinations, and Dr. Thayer, Dr. Bates, and Dr. Martin.

Adjourned to half past two o'clock.

AFTERNOON SESSION.

FRIDAY, June 1.

The Committee met at 2½ o'clock.

By invitation of the Chairman, Rev. Daniel Lindley, lately a missionary in South Africa, made some statements to the Committee.

He said:—I wish to stand before this honorable Committee in the light of a witness, simply, to testify to facts, but not to give opinions. However, if after I shall have made my statement, gentlemen should be pleased to ask my opinion, if I have one, I will give it, and if not, I will say so. I will make my statements as succinct and brief as I can, and if I should not be distinctly apprehended by any member of this honorable Committee, I hope he will then and there interrupt me.

This disease, whatever be its name, and it has different names, was introduced into South Africa about six years ago,—I don't now remember the precise date,—but it is about six years. It was introduced from Holland, imported in the body of a bull. A gentleman in Cape Town, wishing to improve his stock, made that importation, and with it that disease which has been to South Africa the severest scourge that has ever fallen on its property interest. It was about six weeks after the animal landed,—he having been on board the vessel on the passage about two months,—before any sign of sickness appeared in him. At the time, it was not suspected that the disease was the lung contagion, so long known in Holland. However, he died. He communicated that disease to a considerable number of cattle, and before they became aware of the evil that threatened them, it had been scattered about very extensively. The question may arise in the minds of the Committee—Why was it not at once exterminated there, as you propose to have it here. The answer to this question will be found in this statement that I must make, in order that you may understand the circumstances of that country. If you will imagine New England, and a great part of the United States, divested of its woods, its forests, leaving here and there thickets and jungles, and a grass country, that is without fences, without any enclosures, and all this country spread over with cattle by the thousand,—for the

property of the inhabitants of the country consists in cattle and in sheep,—and over all the country cattle are grazing by the thousand. I have seen 1,600 in one herd, but generally the herds are from one to five hundred. In those parts of the country, where the lions and tigers have been exterminated, these cattle are allowed to roam night and day where they please, and they wander considerable distances, sometimes miles around. In addition to that, all the produce of the country that is brought to market, whether to supply the city of Cape Town, or Port Elizabeth, or other towns lying along the coast, is brought down from the interior in large wagons, drawn by oxen. All the goods imported into the country and taken inland are conveyed on these wagons, drawn by oxen; and to each wagon the custom of the country gives six pairs of oxen.

The country is large,—it being from Cape Town to the extremity of any civilization in the interior, twelve hundred miles, and across the plains to where I live 1,200 miles more. Well, this country is passed through, up and down, crosswise, and backward and forward, by hundreds of wagons and thousands of cattle every day. They have no railroads, no rivers,—no other way of transporting goods from one point to another, but this ox wagon. Well, they are great sheep raisers in this country,—having five to ten thousand sheep in a flock,—and I have seen as many as fourteen thousand in one flock. Their clips of wool are all sent down in these wagons to the coast.

In a country of this kind, where there are so many cattle, and where every thing is done by means of cattle, and they are travelling night and day, there is no possibility of killing out this disease by extirpation. The seed had been so widely disseminated before the people knew what the matter was, that such a system was looked upon as hopeless, and the government adopted no measure to stay it, and every man was left to look out for his own interests. I will say, that after it had got fairly spread abroad to a considerable extent, the inhabitants very generally resorted to inoculation. And I will say in passing, that we are indebted to that for about all the cattle we have left. We should have been flat on the ground, and no man could have got to the coast with his products or returned with his merchandise. Inoculation has saved us what we have, after

six years. The disease was still at work when I came away, about a year ago, but was more under subjection. It has killed hundreds and thousands of cattle, and I can assure you, gentlemen, that where it has come into a flock, it has not left more than five out of a hundred. I was happily surprised when I heard Dr. Loring state that in the past year, in this State, not more than twenty per cent. had died.

With us, when an animal is known as having the disease, we look upon it as already dead. And I can affirm, without hesitation, that where it has got into a herd of cattle, not more than five out of a hundred have been spared. Occasionally, one has passed through, and has not had the disease at all; and a few on the other hand,—two or three in a hundred,—have recovered, and no more. I know of one man who had five hundred head of cattle, and that disease got in, and he had not five left. If I speak with emphasis, it is because I have had sad experience; and I have been afraid that the good citizens of Massachusetts might not be aware of the evil that I do most firmly believe threatens their property interest more than any thing that ever threatened it yet.

The disease has spread in every direction from Holland, and by contagion. I will give you facts on this matter of contagion. Well-meaning men, men of science, and who hold high and influential stations, said it was not contagious, that it was impossible for a lung disease to be contagious, and through their influence some herds suffered, that might, to my certain knowledge, have been saved. One of the commissioners appointed was a man of some science, and he said, "Poh! poh! it cannot be contagious," and the cattle were left, and the consequence is, it has spread over all the country around them.

I will tell you how the disease came to my particular neighborhood. A native went out as a pedlar over the Cathumba Mountains, into the interior nearly 300 miles. There he took cattle in payment for goods. He brought down a herd of oxen to the eastern coast. While on the way down, some of his oxen became sick and he quietly put them out of the way, for he could travel one or two days, perhaps, and not see a single person, and the dead cattle were not likely to attract attention. He had that failing which we can pardon in others as we see

it in ourselves, that he cared a little more for himself than he did for his neighbors. He put the sick oxen out of the way, and brought down the rest and sold them. They were bought by a gentleman who had about 120 oxen. The pedlar's cattle, looking apparently well, were put into that herd. Well, presently the disease broke out. It was in that instance that this doctor had the influence to prevent the slaughter of that herd, because he said the affection was not contagious. Well, these cattle were running about in the neighborhood,—out on the plain twenty miles square, without fence and without tree, save here and there a bush,—where were grazing thousands of cattle, and they ran just where they pleased. From this flock, the contagion was communicated to all the cattle in the region. Oxen were travelling through the country every day—at least a hundred passing in a day—and in that way it was carried widely through the country. Until it was brought from a contaminated region in the interior, by these oxen, the disease had never been within three hundred miles of us. I might give a thousand facts just equal to this, but I am mentioning what occurred in my neighborhood.

The disease had not crossed to the northward, to the Ungani River, until this happened: A man wished to convey a boat from Port Natal to a place about sixty miles to the northward. He put the boat on a wagon and took his six yoke of oxen to draw it. He travelled one day, and camped just outside of a village through which he had passed. In the morning he found one of his oxen sick. He had camped on a piece of ground where cattle grazed every day, and in a place where the people had thought themselves safe. Finding his ox sick, he quietly took him and his mate out of the wagon and leaving them there, started on. These oxen remained through the day, and mixed with the many cattle owned in that village. The second day after they had been there, it was discovered that there was a sick ox in the field. The inhabitants were all out at once; they killed the ox, and from the description, they saw that he had the disease they had dreaded. They immediately inoculated their cattle, and saved a goodly number of them. Now, in regard to that, I wish to make this statement. I made a statement, which was honestly reported, I suppose, but mistakenly as a statement, that they had saved 90 per cent.; in

some instances not more than 30 per cent. Between this and 90 is probably the average percentage saved. In that case, I mentioned that there was a clear, distinct instance, where the sickness had been brought from the interior three hundred miles; and in the last case it was carried twenty miles.

Another instance: two natives were trading, and brought the disease from the country where they went, two hundred miles, and set it down in a perfectly healthy region in a herd of about eighty cattle, and there it spread, and they were every one carried off.

Another fact, one with which I had to do myself. A native, a stupid heathen native, was working for an Englishman in an infected region. He took his pay in cattle,—two calves, I think, a year or a year and a half old. He carried them into a healthy district where the disease had been kept out, and within twenty miles of which it was not known. Presently these calves fell sick and died, and the cattle with which they were placed began to be sick. I had in my service a young man belonging to that village. That was twelve miles from where I lived. A messenger came to this young man to say—"your cattle are sick." When I heard that, I began to inquire if any cattle had been brought from within the infected region to his kraal. They said such-a-one, naming the native before mentioned, had been working with a man and taken two head of cattle for his pay; he came back a little more than two months ago with these cattle, and they took sick and died; and now our other cattle are sick. I saw at once what the matter was,—for I knew that the region which these two cattle were taken from was wholly contaminated. I said, "your cattle will all die—you ought to tell your neighbors to keep their cattle away from you." I asked him further if his cattle had mixed with other cattle, and he said, "there are three kraals that have mixed with ours." So it was too late, and the result was that they *all* died. I suppose that in those four herds there were from one hundred to one hundred and thirty head of cattle, and they every one died. Well, I told the young man whom I sent, to go and warn his neighbors. He did so, and they took their cattle in the opposite direction to grass, and for two years before I came away not a single head of the cattle around them had taken the disease. Just those that were

exposed to the contagion, and no others, died. The neighbors' cattle continued in a state of perfect health for two years after those four herds—one hundred or one hundred and thirty head—had died right out there in the heart of a healthy region, a region as large as a county. I cannot doubt that the disease was communicated by contagion, and that if the animals can be cut off, the disease will be kept off. It was kept off in the region in which I lived, in this way. The chief with whom I have lived occupies a considerable extent of territory, and he is fortunately fortified on one side by a range of mountains, and on the other by a precipice some hundred feet in height. He had assembled his tribe for another purpose, and wanting my advice in reference to some political difficulties, he sent a messenger to tell me of his trouble. I went to him, and after that matter was settled, I took occasion to tell him that the sickness was within some forty miles of us. I told him what the disease had done and would do, and I said to him, "there is just one thing to do, and that is, to keep your cattle where they are and not allow any to go out or come in." Well, the people there love their cattle, as they say, better than they love their lives. They took the alarm, and every effort that was made, on the part of any one, to bring cattle into the country, was immediately and stoutly resisted. The intruder was met with spear and shield, and threatened with death and destruction to himself and his cattle if he came a step farther, and so was made to go back. Only half a mile off, within sight of these cattle, dead animals were lying unburied that had been exposed to this contagion. The disease was brought there by the oxen of an individual who had been into the interior, and when he came home his oxen died. They communicated the disease to all the cattle in that neighborhood, and I never saw more complete destruction. There was not a single head left in all those kraals. Those cattle came up to within half a mile of our boundary, and you could look down and see herds of them lying dead. That was three years ago, and yet, when I came away, the disease had not got one inch over that line.

These are facts that I have seen and know, and in that country, if you should ask us, "Is the disease communicated by contagion?" we would say "Yes" and we should just as soon doubt that the sun made daylight. There are thousands upon thousands

of facts to prove it. We have no more questions to ask on that subject. You will see how widely the disease might spread in a country like that, where cattle are so abundant, where the travel is continued day and night, and where thousands of oxen are on the road every twenty-four hours. It has been to that country a great scourge. Thousands and hundreds of thousands of cattle have died, and many of the people have been made poor by the ravages of the disease, and the only hope they have of securing a comfortable subsistence, and recovering a comfortable position in respect to property, is through sheep. They have given up all idea of grazing cattle, and are now turning their attention to sheep; for the disease is so widely spread, that they have no hope that it will ever be exterminated.

I do not know that I have any thing further to state. I might repeat hundreds and hundreds of facts of precisely this character. If I have appeared earnest in my statements—somewhat as if I was making a speech, which is, perhaps, my profession—I hope you will not attribute it to any other motive than a wish to make you fear as I think you ought to fear. Massachusetts has enacted some glorious history, whereof you have famous monuments; and I hope that pluck will not be wanting now.

Mr. WALKER.—How soon after exposure does the disease appear?

A.—The time varies; anywhere along from two to four months; but I should say, most commonly ten or eleven weeks after exposure.

Q.—What weather seems to hasten it?

A.—We have noticed very little difference there; our climate is so uniform that we cannot give an opinion on that score. On my station, I have never seen frost.

Q.—Over what extent of country did the disease spread?

A.—It has spread from Cape Town across the country 1,200 miles; and now it has gone up the north-east coast some 1,400 miles from Cape Town. Before I left, it had gone inland beyond where we could get any account of the people, certainly 1,300 or 1,400 miles, and it may be 2,000.

Q.—Have you personally examined those diseased animals?

A.—I have seen parts of the lungs at times; but the disease had been so long in the country before it reached us—we were some 1,200 miles from its starting point, and it was nearly two years before it got there—and the matter had been so much discussed, and so much had been written about it, that we thought we knew all about it, and when cattle were taken sick, they were shot and buried without any examination.

Q.—Were no efforts made to effect a cure?

A.—Some efforts were made, and some men professed to have discovered nostrums that would cure it; but they lasted no longer than some patent medicines do. We looked upon a remedy for the disease as no better than a remedy for the dead.

Q.—Still, I understand you to say that inoculation was partially successful?

A.—It was, Sir; I will describe it briefly. I would say, in the first place, that inoculation was practised by every man for himself; and the heathen, who know nothing of such diseases, and who know so little that they believe that when the sun goes down to-night it will be a dead sun, and that a new one will rise to-morrow—thousands of such men inoculated their oxen by taking a part of the lung of an animal that had died of the disease, and been dead some days, and put that into the living animal; they, of course, have not had much success. Others have taken an animal that was sick, but not very sick, killed him, and immediately cut him open and taken a part of the lung not very seriously affected, but still there was disease in it, and inoculated with that. Some animals were inoculated in the dewlap. They generally died. Others inoculated just at the end of the tail by cutting a hole just under the hair, just so as to draw blood, and depositing in it a little piece of the lung about as large as a common sized bird-shot; and that was left for ten, twelve, and even fifteen days sometimes. They watched the animal closely, and when the end of the tail began to swell and feel hot, they cut it off, hoping thereby to cut off some of the virus. I don't know whether that did any good or not; but some have recovered. The animal appeared to be unwell; in some cases the tail would be swollen and stiff up to the body; but presently it would recover. In some other cases the swelling goes into the rectum, which becomes enlarged

to an inch and a half, and large tumors were formed about the roots of the tail. When it becomes so bad as that, the animal will die; but some men cut and slash them in a terrific manner, if you will allow me to use a strong word. But what is peculiar about these cuts is, that while you would think the animal would bleed to death, the wound bleeds but a few drops and then stops. There is no suppuration; it looks like dried meat; and so it will remain for days and days. The owners take very strong suds and wash it four or five times a day, so as to bring it into such a state that it will secrete matter; and if they can do that, the animal will recover; if not, he will die. The cattle that have been preserved by inoculation are all the cattle we have left there. In a country like that, where there are no fences, and where there is such a constant going of cattle, many persons felt obliged to inoculate. Farmers in the interior, having, perhaps, some of them, three thousand dollars' worth of wool in their house, living four hundred miles from the seaboard where it was to be sold, did not dare to leave home unless their oxen were inoculated; and when they inoculated them, they inoculated the whole herd. Every man felt obliged to inoculate his cattle, and did so, except some two or three strong-headed men, who did not believe inoculation would do any good. One such man is a neighbor of mine. He left it to fate; and out of 120 head, I believe he had but three or four left.

Q.—Do you know whether the disease produced by inoculation would communicate disease to the cattle with which they come in contact?

A.—No, Sir. Where one was inoculated, we took it for granted that there we had the real disease, and all were inoculated.

Q.—Did mortification ever proceed from inoculation?

A.—Nothing that appeared so. The swelling goes up into the intestines, and the animal becomes bound, costive. But I did not see any animals treated scientifically that had been inoculated. I have no doubt that many would have been saved if they had been properly inoculated; but I had most to do with those ignorant people who had no knowledge whatever.

Q.—Is the disease that you had in your country the same as that about which we are inquiring?

A.—Yes, Sir. I had an interview with Mr. Walker and some other gentlemen the other day, and they asked me to describe the disease as it existed in Africa. I told them it appears so and so, in the various stages. Mr. Walker sat opposite to me, and, slapping me on the leg, said, "That's it—that's it." They are identical; there is no doubt about it. The appearance of the lung differs in various stages. It is a disease that goes on gradually. There is no enemy more stealthy, none that goes to work more secretly than that does. You may kill an animal in the very first stages, when it appears perfectly well, and, no doubt, feels perfectly well, and you will find a spot in the lungs not bigger than half a dollar, but it is unlike the spots that appear in a healthy animal. If you cut it open, you will see that the air cells are becoming hepatized; it is becoming solid; and that little bit serves no purpose at all in the animal's body. If you had waited a week, the spot would have been larger; two weeks, and it would have been larger still. Some animals go off in a galloping consumption; others retain the poison longer before it becomes manifest, and they hang on to life with marvellous tenacity; but they become thin, but they may live on and cough, cough, cough, for three months after they are taken sick.

Q.—What are the first symptoms?

A.—The first are, the hair of the animal stands up, and we hear a little cough, as if it had got something in its throat that tickled it. Other cattle have such a cough and you will hardly be able to distinguish it, but you will observe that this is not momentary, but is kept up day and night; and then this gets to be a cough that comes from the very bottom of the lungs—a very severe cough—which continues until the animal dies.

Q.—What are the general appearances?

A.—You see it only in the staring of the hair, in a quickness of breathing, and a dryness of the nose and mouth.

Q.—No discharge from the nostrils?

A.—Not at first. That does not come on until after the disease has made considerable progress in the lung.

Q.—You stated that many animals died from inoculation. Were the lungs ever examined?

A.—Yes, Sir. I have seen many; the lungs were not affected in the least. There is a mystery about it that I cannot explain.

Mr. WENTWORTH.—Did you say that you have seen these cattle running at the nose?

A.—Yes, and froth at the mouth. There was considerable secretion flowing from the nose and mouth, and sometimes they coughed out lumps of this. As one man expressed it, "There is one, coughing out his lungs."

Q.—Is there suppuration of the lungs?

A.—There is, in many instances; and I should say that with us it was far more common than Dr. Loring stated it to be here. Sometimes, when you cut open the lung, you find a cyst; but it holds pus, and it is not curded as Dr. Loring described. You cut down through a rotten lung, and wonder how the animal could live under such circumstances.

Q.—Have you seen any animal in this country affected by the disease?

A.—I have not.

Q.—Have you seen any lung said to be affected with the disease?

A.—Yes, Sir; I have; in a room adjoining that of the Secretary of the Board of Agriculture.

Q.—Does the condition of that lung resemble those you have seen in Africa?

A.—Yes, Sir; it resembles, exactly, many that I have seen there. But, as I said, there was not, in the African cases, that cyst, containing a large lump of coagulated matter, which is seen here. More lungs have pus in them, in that country, instead of the curdy substance which is here found enclosed in the cyst.

Q.—How does the animal behave, in Africa,—aside from coughing?

A.—At first, it will feed, some; you will notice, however, that it is not so full as the other animals, when it comes home, at night; but the feeding falls off, until the animal gets so that he stands in one spot almost all the time, and becomes more and more feverish. If the wind blows, it will seek some place to get out of it, and stand there for several days; it will go off a little way, to get water, but will return to that spot. It will stand almost all the time. It seems to oppress the animal to lie down; and it stands day and night, with its neck stretched out so as to give the air direct access to the lungs.

Q.—Does it associate with the other cattle?

A.—No; it leaves them.

Q.—How do you account for the lungs not being affected, in cases where the animal dies in consequence of the inoculation?

A.—There is no accounting for it. The whole thing is contrary to all common-sense and all science and all human experience. I can only say, that my oxen were inoculated, and lived through it, though thousands of cattle, within half a mile of them, died of the disease. I cannot account for it.

Q.—Then the disease communicated by inoculation does not affect the lungs?

A.—No, Sir.

Q.—Do you know of any instances where it did?

A.—No. I have known instances of this kind. Many persons, after their cattle had been exposed to the disease, and it had begun to take effect upon the lungs, have inoculated them, and they have afterwards died. It was hard to tell, in such cases, whether the animal died of the disease, or from the inoculation. Inoculation is simply a preventive; it will not cure the disease, and if the animal has been exposed for any length of time, inoculation will do no good.

Q.—Did you ever know an animal to have the disease that had been inoculated?

A.—I have,—a good many; but the inoculation was done by men who have no intelligence. An intelligent, watchful man, will say to you, "I inoculated twenty; fifteen took, and five did not; I must re-inoculate those five";—and he does it, and saves his cattle. But with the heathen around me it was not so. They thought that if they had done the thing once, it was all right and sufficient; and then they took the consequences.

Q.—Did you ever see the lung of an animal that had died after having been inoculated?

A.—I have, many of them, I am sorry to say. They are not affected; I know that personally.

Q.—What seems to cause the death of those that die from inoculation?

A.—I should say that it was from inflammation, which causes a swelling, and that goes through the body, until it reaches the seat of vitality. Allow me to state, in reference to this matter, that some persons now buy up cattle which they

know have been exposed to the disease, which they get at a reduced price, for the purpose of inoculation, expecting to lose some of them, but to make a profit on those that live through the inoculation, for then the animal will bring three or four times the original price. And I would say, in reference to the station where I live, that, finding there was not room for our agriculture in that healthy region which had been kept free from the disease by the vigilance of the natives, we went out into the contaminated region—moved our houses and every thing. But my people were not willing to take their oxen out. They went and bought other oxen, that had been exposed, however recently, and inoculated them, and the most of them recovered. They did a good business at that; and then they would buy up the produce of the natives around, bring it up to the line with their oxen that had always remained in the healthy region, there they would unyoke them, and then bring these inoculated cattle up, yoke them, and go to market. So, with the help of two sets of oxen, they took their own produce to market, and also the produce of those around them.

Q.—After an animal is inoculated, does he become very sick?

A.—Some of them show very little sickness; some of them are worked all through it, but others become very sick.

Q.—What proportion become very sick?

A.—I have never paid such attention to the matter as would enable me to give you a reliable answer—a good many of them.

Q.—What length of time is required to go through this process of inoculation?

A.—Well, an ox will be dead or safe within twenty days after he is inoculated.

Q.—And then he recovers rapidly?

A.—He recovers rapidly. But I must say, gentlemen, that I do not know what would be the effect of inoculation in this climate on your cattle. I am speaking now only of my own neighborhood, and my own experience. I ought to add, having said so much about inoculation, that in Europe they think less favorably of it than we do in Africa.

Q.—Your cattle are out all the time?

A.—Yes; we have no barns, no stalls, no any thing.

Q.—What is the average temperature?

A.—The highest I have known was ninety-three degrees, the lowest forty-two degrees. But you will notice that I am speaking of my own locality. The land rises back of me, and there are frosts there at night in the month of July, with a very clear, bright air.

Q.—Do you know of any difference in the system of inoculation in the different climates?

A.—None whatever. There is, indeed, very little difference in the climate. There are frosts in the morning in some sections, but the middle of the day is warm and pleasant, and men walk about with their coats off. What I wanted to impress upon the minds of this Committee—for I feel an interest in this subject—was simply this: that we are satisfied that the disease is contagious; that we have facts which prove that if it can be isolated it will die out, and go no further; and if those persons who have cattle in the neighborhood of infected herds, if they know they are infected, would remove them, and let them get well, the disease would die out, I am sure. But I have no plan to propose, for I am too ignorant of the country to be able to offer one.

Mr. Bird.—What was the distance from you at which other cattle were kept? and what were the means of insulation?

A.—Cattle came up from the other side to the line, and died; and down the hill, a descent of half a mile, herds of cattle belonging to the natives were running, and have continued to run, for three years, with perfect safety. Whether quarter of a mile would have done it, I do not know, but half a mile has kept them safe. There was nothing to preserve the insulation but the watchfulness of the natives. All they have is in their cattle. The personal property of a native is not worth five shillings. They were very wide-awake, therefore, and if they saw a person coming down those hills, they ran out with spears and shields, and drove him back, and it came to be understood that no person was to pass through the country with cattle.

Q.—What are your reasons for supposing that the diseases are the same?

A.—One reason is, that they both came from Holland, or it is so stated. It was imported from Holland to Africa by a bull; and I may say that every one, excepting, perhaps, two or three

gentlemen, believe that the disease here was brought from Holland. I have inquired of the Commissioners, I have looked at the lungs, I have read all the accounts I could find, and the symptoms and the results are the same, with the exception that it is not so fatal here as it has been with us.

Q.—I understood you to say that you had not seen the whole lung of any animal in this country, but only a piece; is that the fact?

A.—I will tell you what I saw. I suppose it was a whole lung in a bucket. I did not take it out to examine the whole lung. I saw the diseased side of it, and where, if you cut into it, I have no doubt you would find the cyst of poisonous matter.

Q.—How many did you examine in Africa?

A.—A few. I have seen pieces of the lung there. I am not a professional man, and the disease had been examined so much when it reached us, that we thought we knew all about it. I cannot say more than I have said of this disease, imported from Holland into South Africa. I wish to state here, however, that we have a disease that I suppose is the true pleuro-pneumonia there, and sometimes it gets into a locality, and will kill a number of cattle—eight, ten, fifteen, out of thirty head,—just in one neighborhood, or in one place; and when this other disease first came into the country, the natives laughed at the white people, and thought they were wiser than we. "Pooh!" they said, "we know all about that; we have seen this disease before." But when an animal died of it, and the natives stood by to see it examined, they said, "Oh! no, we don't know any thing about that." As soon as they saw that, they said, "That is not the trouble with our cattle." There we have a disease, the pleuro-pneumonia, and it sometimes acts very strangely, and kills a great many cattle; but it does not go through the country, it is not contagious, nobody fears it.

Q.—What do you call this disease there?

A.—We call it by the Dutch name, which, literally translated, means a sickness of the lungs.

Q.—Have you had sufficient experience to be able to say that where inoculation is fatal, there is no disease of the lungs?

A.—I have seen many cut open and cut up, but I have not seen any disease of the lungs. I have seen lungs cut up and given to dogs, and some people eat the meat of animals that

have died from inoculation, and some the meat of animals that have died of the true disease; and I have seen the thing so often, that I think there can have been no disease of the lungs, though I have not cut into them as a scientific investigator would have done.

Q.—I understood you to say that you had taken some pains to inquire as to the disease as it exists here, and that you had seen it in Africa. Now, is there or not a very close resemblance in the symptoms and their results in the two countries?

A.—I am sure that the cases are the same. I have no more doubt of it than that I am standing here. I am sure that it is one and the same identical disease.

Q.—I will ask you about the appearance of the lungs, as exhibited in Africa, and as seen here in the State House. Is there that strong similarity which would, in itself, lead you to the conclusion that the disease must be identical?

A.—I suppose that if a scientific man had the two lungs before him at one and the same time, without knowing that one came from Africa and the other from North Brookfield, he would not hesitate to say that it was the same disease in both lungs. An unlearned and unscientific man might not see an exact similarity, especially if one was a lung in which the disease had been progressing eight weeks, and the other one in which the disease had existed only five weeks; but I think that a scientific man would say that the latter was behind hand, but was coming up to the other.

Q.—How far is there any similarity in the symptoms between the cases of the natural disease and the inoculated disease?

A.—Well, they have hardly any thing in common. They have no cough; they have a fever when they are inoculated; but the constitutional disturbance is not violent, and not extensive. The animal goes on grazing, and may be worked every day. Many oxen get well, and are made proof against the disease, that work every day.

Q.—When they have been inoculated, and go on from bad to worse, and finally die, what are the common symptoms?

A.—The disease goes up the tail, and goes into the animal, so that the intestines and rectum are much swollen. I have seen the rectum cut open when it seemed almost as large as my two fingers, and so swollen that it became almost a solid piece of meat.

Q.—Dark and firm?

A.—Yes, hard and firm; and then it is impossible for any thing to pass through the rectum.

Q.—Did you see the actual pleuro-pneumonia in Africa before the bull landed there?

A.—Yes, Sir.

Q.—How long before?

A.—It was at least a year, it may have been a year and a half; and at least three years before the sickness reached us.

Q.—You saw that in your neighborhood?

A.—Yes; one of my missionary brethren lost an ox by that disease, he was opened, and we examined him.

Q.—Was there any difference between the real disease and the inoculated?

A.—A very striking difference.

Q.—Can you not explain the difference readily?

A.—I do not believe I have science enough. The lung in the true pleuro-pneumonia was enlarged very much; but the disease was violent, and nature did not progress so far as to set up, as in almost every case of this disease here, a kind of defence against it. There was no cyst; there was no deposit of the cheesy matter that Dr. Loring spoke of. The animal was sick, I think, not twenty-four hours after the disease appeared. There was considerale serum deposited in the cavity of the chest. It was one of those common cases which had been known in the country for considerable time.

Evidence of Dr. O. Martin.

Dr. O. Martin, of Worcester.—I wrote to the Secretary of the Board of Agriculture when this Commission was appointed, that I wished he would give me an opportunity to examine the cases of cattle in Brookfield and New Braintree,—that having been my place of residence heretofore,—and Mr. Flint complied, and I went up there. I was there three days, testing the nature and character of the disease. I agreed, at that time, with the rest of the medical profession, in thinking that it was not contagious. I went there with the expectation of being able to prove that the disease was not contagious; but in investigating the cases, I was convinced of the contagious character of the disease

beyond a doubt. Every case I saw there, I was able to trace back to Mr. Chenery's herd. Then, in pushing my investigations, to satisfy myself of the character of the contagion, the next step seemed to be to ascertain the time of the incubation of the disease, because, if the time was four weeks, and there was exposure six weeks after the time of incubation had expired, the probability was that the disease would not be propagated, and it was necessary to arrive at that point, if we could. I pushed my investigations to a certain extent, and satisfied myself as well as I could in regard to it. Then I endeavored to satisfy myself of the fact of how long the diseased animal could propagate the disease, but I arrived at no definite conclusion. I examined twenty cases, I believe. I reported to the agricultural society in the city of Worcester nine or ten sample cases. I have that paper in my pocket, which is more concise than I could state it verbally; and if you wish, I will read you the report on those cases. It was intended to embody the results of my observations up to that time, and in fact, it contains the results of my observations as far as they have gone at all.

Case 1st.—*Autopsy.*—This cow had been sick nineteen days, was feeble, without much appetite, with diarrhœa, cough, shortness of breathing, hair staring, &c. Percussion dull over all left chest, respiration null. Killed by authority. We found several gallons of serum in left chest, by estimate, a thick fuzzy deposit of lymph over all the pleura-costalis. This lymph was an inch in thickness, resembling the velvety part of tripe, and quite firm. There was a firm deposit of lymph in the whole left lung, but more especially at its base, with strong adhesions to the diaphragm and pleura-costalis, near the spine. The lung was hard and brittle, like liver, near its base. No pus. Right lung and chest healthy. There seems to be no very good reason why this case should not have recovered, as one lung was sound. This herd belonged to Mr. Olmstead. It was two weeks after first diseased animal to the second, making the time of incubation two weeks. Mr. Olmstead wrote me that the fatal cases lived from ten to twenty-eight days, making duration of disease till death nineteen days as an average. Those that commenced recovery had the disease from twenty-one to thirty-five days.

Case 2nd.—This cow was taken very sick January 30. In fourteen days, she began to get better. Now, April 12th, she is gaining flesh, breathes well, hair healthy, gives ten quarts of milk a day, and in all

other respects bids fair for a healthy animal hereafter, except a slight cough. Percussion dull over base of the left lung, near the spine, and respiration feeble in same region.

Autopsy.—Left lung strongly adherent to diaphragm and costal-pleura—the long adhesions well smoothed off, pleura-costalis, shining and healthy. Also the surface of the lung where there were no adhesions, sound and right; all the lung white and free for the entrance of air, except the base, in which was a cyst containing a pint or two of pus. Loose in this pus was a hard mass, as large as a two quart measure, marble looking; when cut through its centre, like the brittle, hardened lung in the first case. It appeared as though a piece of lung was detached by suppuration, and inclosed in an air-tight cyst, by which decomposition was prevented. The other lung and chest were sound.

It is to be inferred, as there were adhesions, that there had been pleurisy, deposit of lymph and serum like the first case, and that nature had commenced the cure by absorbing the serum from the chest, and the lymph from the free pleural surface, and smoothed off every thing to a good working condition. The lump in the cyst was brittle and irregular on its surface, as though it was being dissolved in the pus. And no good reason can be given why nature should not consummate the work that she so wisely had begun.

Case 3d.—This cow had been sick fourteen days, was coughing and breathing badly, percussion dull over both chests, and respiration feeble. Killed.

Autopsy.—Both chests filled with water, deposits of lymph over all the pleura-costalis an inch thick, presenting the same velvety, fuzzy appearance as the first case. Both lungs were hardened at the base, and the left throughout its whole extent, and firmly adherent to diaphragm and costal-pleura near the spine.

The right lung had nearly one-third of its substance in a condition for the entrance of air, but this portion, even, was so compressed with the water that a few hours longer would have terminated the case fatally, without State aid. This case had not gone on far enough for the formation of the cyst, or pus.

Mr. Needham wrote me that about twenty-eight days intervened between the first and second case of disease in his herd, making the time of incubation in this herd about twenty-eight days instead of fourteen, as in Mr. Olmstead's.

The duration of disease in this herd seemed to be about two weeks till death or till commencement of recovery.

Case 4th.—A nice heifer belonging to Mr. Wilcox, in fair condition, eating well, only having a slight cough. Percussion dull over base of left lung.

Autopsy.—Base of left lung adherent to diaphragm, and costal-pleura; lung hardened. On cutting into base, found ulceration, and a head of Timothy grass four or five inches long. The animal every other way well.

Case 5th.—This cow was taken January 1st with a cough, difficulty of breathing, and the other symptoms of the disease, and continued sick till March 1st. On taking her out April 12th, to be slaughtered, she capered, stuck up her tail, snuffed and snorted, showing all the signs of feeling well and vigorous.

Autopsy.—Right lung firmly adherent to diaphragm and costal-pleura near the spine. Base of lung hardened, containing a cyst with a large lump of the size of a two-quart measure, floating in pus; outside of the lump was of a dirty yellow white, irregular, brittle and cheesy; the inside mottled, or divided into irregular squares, red like muscle, and breaking under the finger like liver. Costal pleura smooth, shiny, adhesions where there was motion, card-like and polished; no serum; lung apparently performing its function well, except for a short distance about the air-tight cyst, where it was still hardened. It would seem as though nature was intending to dissolve this lump and carry it off by absorption. The writer thinks she knows how, and would have done it, if she had been allowed sufficient time.

Case 6th.—Was taken December 15th, and was very sick; in three weeks she was well, except a cough, quite severe, and so continued till about the first of March, when she coughed harder and grew worse till seven days before she was killed, April 12th, when she brought forth a calf, and then commenced improving again.

Autopsy.—Right lung adherent to diaphragm and costal-pleura. At its base was a flabby, fluctuating cyst. In cutting into it, we found the lump breaking up by decomposition, and scenting badly. Every thing else normal. Did not some accident break through the cyst and let in the air when she grew worse? Would she not probably have overcome this disagreeable accident, and recovered in spite of it?

This cow's hair did not look well, like those in which the cyst was air-tight, but still she was beginning to eat well again, and appeared like recovery.

Case 7th.—This heifer had coughed slightly for six weeks, but the owner said he thought no one going into his herd would mind that any thing was ill with her. *Autopsy.*—Slight adhesions of lung to diaphragm. Near these adhesions are small cysts of the size of a walnut, containing pus and cheesy matter; about the cysts, a little way, the lung was hardened, say half an inch. There were several cysts, and they appeared as though the inflammation attacked only different lobes of the lung, leaving others healthy between—nature throwing out coagulable lymph

around the diseased lobe, and forming thereby an air-tight cyst, cutting around, by suppuration, the diseased lobe, so it could be carried off by absorption. This herd belonged to C. P. Huntington. Nine days after first cow died, the second case occurred. First cow was sick five weeks. The time of incubation could not be over six weeks—probably not over three weeks. These cows—one improved in eight, the other in three weeks.

CASE 8th.—This cow has been sick three weeks. Killed. *Autopsy.*—Large quantities of serum in left chest; lung adherent and hardened at base. On cutting into hardened lung, we found one side of lump separated from the lung, with pus between the lines of separation, with the forming coat of the cyst outside of the pus; the other side of lump was a part and parcel of the hardened lung which had not yet had time to commence separation. The pleura-costalis was covered with organized lymph to the thickness of an inch, with the usual characteristics. The right chest contained a small quantity of serum, and had several small, hardened red spots in that lung, with some tender, weak adhesions, but most of the right lung was healthy.

CASE 9th.—Sick four weeks. Killed. *Autopsy.*—Right lung hardened at base; adherent to diaphragm and costal-pleura; lump separated on one side only. Cyst beginning to form outside of separation; pus between cyst and lump, but in a very small quantity. These two cases settle the character of the lump, and the manner of the formation of the cyst. The lump being lung and lymph, cut out by suppuration; the cyst being organized lymph, smoothed off by suppuration, friction, &c.

CASE 10th.—Killed. Hair looked badly, but the cow, it was said, eat and appeared well. But this case occurred in Leonard Stoddard's herd, where we could get no reliable information. *Autopsy.*—Base of lung hardened; adherent to diaphragm; containing a cyst in which was a lump of the size of a quart measure, but little pus. This lump had air tubes running through it, which were not yet cut off by suppuration, and in one place the cyst was perforated by a bronchial tube, letting in the external air to the lump which was undergoing disorganization—smelling badly, and when cut into, not presenting that red, mottled, organized appearance of those cases with air-tight cysts. This case was the specimen exhibited at last medical meeting. These two cows with perforated cysts, although improving, did not present the bright, active, healthy appearance of those recovering with air-tight cysts.

Quite a number more cases were examined, but these ten present all the different phases of those examined. One or two cases are needed of an early stage of the disease, to settle the point whether, in all cases, the primary disease is lung fever and the pleurisy—a continuation of the primary difficulty merely; and some six or eight cases dating five, six,

seven, eight months from attack, and so on till entire, final recovery. And such we expect to have an opportunity to examine in Mr. Chenery's herd. Some cases were sick most a year since, and are now apparently quite well; perhaps all that lump and pus are not gone yet. I believe many physicians think no severe case will ever recover, and some think none ever get entirely well. But it would be difficult to see why, as a rule, all single cases should not recover, and all double cases die.

The disease has been the most fatal in Mr. Chenery's herd—the herd the best fed and the warmest stalled—but he attributes the fatality, in part, to a want of sufficient ventilation.

The herds in North Brookfield and West Braintree, where all the fatal cases have occurred in these towns, consisted originally of

Leonard Stoddard's,	48	head,	of which	13	have died.
Alden Olmstead's,	23	"	"	7	"
C. P. Huntington's,	22	"	"	8	"
A. A. Needham's,	22	"	"	8	"
Alden Woodis's,	21	"	"	4	"
Total,	136	"	"	40	"

Or were killed to prevent a certain death. A little less than thirty per cent. of these herds have died.

From what the writer could learn, he was inclined to the opinion that almost every animal had the disease, but many of them so lightly as to be perceptible only on close observation. This estimate has excluded the calves. Most of the cows which had not calved before being sick lost their calves prematurely. Of the ability of nature to restore fully most of the cases in which she has made the attempt, the writer has no doubt. The probable time of incubation is from two to three weeks. The time of propagation about the same time. The acute stage of the disease lasting about three weeks.

I found that all reasoning from the human system to these cases failed. The disease does not follow any laws with which I am acquainted.

I would say, in addition to this, that we labored under a difficulty, inasmuch as we had never seen the disease; we had never had much acquaintance with healthy lungs. There is a part of the history of the cases that I have not described in that paper, because I was not certain in regard to it. When the animals were slaughtered, we found in the lungs something that we called red hepatization; but when I returned home, I went to the butchers and found some lungs which presented

the same appearance, and the butchers told me, that when they cut the wind-pipe and the blood did not flow freely, one lung was left heavy, and did not collapse, and cut in the way which we called indicative of disease in North Brookfield. Nevertheless, I have no doubt that there is an incipient stage of the disease which I have not seen, but which I presume the other gentlemen have. I made a mistake there, as I did in a number of other things, which I took for granted before I knew.

It seems to me that the important point to be settled, if in our power,—and as I am one of a committee of the medical society appointed to confer with this body, I will mention it now—is the time of incubation. The question of contagion is settled; the time of incubation is not settled. I *guess* it is within four weeks. Then the time, too, of propagation. You will see the necessity of settling these questions, if possible. If we knew anywhere within a week or two, the time of incubation, if an animal has been exposed to the disease, and it has not come down with it within six weeks, and the time of incubation is only four, he may be considered safe, and it is not necessary to slaughter him. If the time of propagation should only be during the active stage of the disease, which is the peculiar time of propagation of disease in the human species, then if this diseased animal, after six weeks, should go into a healthy herd, there would be no danger from that. That might help the matter along a little.

Then comes the question of recovery. Now, I wanted, and I presume that the Commissioners wanted, that some animals should be placed in Mr. Chenery's herd. That would throw immense light upon the subject, but the Commissioners thought they had no power to do it. If we could place a healthy animal in there, and it should remain so, it would show that the propagating period had passed, and that it would be ridiculous and absurd to kill them. I hope that Mr. Chenery's herd will not propagate the disease. I hope that herd will not be killed at once, but only a part of them. It is a very serious question whether any of these cases will recover. My hope in nature is strong, and not so strong in medicine as some others. I should like to have an opportunity of examining those cattle. If they can be kept and killed from time to time, it will settle the question of final recovery. I am in hopes that the disease will grow

less and less fatal, as such diseases are very likely to. I do not agree with the rest of the gentlemen, though they have had more opportunities of judging than I. I expect that some seventy per cent. will recover. My opinion may not be worth any thing to you. I think you have got to go by facts. There are opinions enough in the land for all of you. The facts I have stated you may depend upon.

Q.—What mode would you recommend ?

A.—Feed the animals well, use them well. If there is water in the chest, let it out. I should keep healthy cattle away from them. I might do something more than that to amuse my patients, but if one of my children had water on the chest, I should let it out, to make the child comfortable.

Q.—Would you advise killing the cattle, if they were diseased ?

A.—If the thing were left to me, to kill all the cattle that are diseased, I should do it forthwith. I think the only question is whether it can be exterminated; if it can be, it ought to be. Any amount of money that it would cost, would be a small consideration. I want to mention some facts that have come to my knowledge in reference to its probable extent. I have been informed that Mr. Stoddard's calves were exhibited in West Brookfield, at the cattle-show, and that they stood by the side of some of the most distinguished bulls in the Commonwealth. If that was so, it will make the question of extirpation a very difficult one. It would make the disease very much more extensive than I supposed it to be ; because, if that is the fact, there have been exposures in Worcester, and in different parts of the Commonwealth. Mr. Stoddard will be here, and can be examined.

Q.—How long ago was that ?

A.—That was last fall. In one herd, in which there is a bull that was there, there have been two deaths, but they were not by this disease—*of course.* There are a good many things that are "of course," when it is for a person's interest. That matter would be for the Committee to investigate.

Q.—You alluded to one case, where the cow was improving, and giving so many quarts of milk. Was the milk used in the house ?

A.—No ; they said it was given to the calves.

Q.—Did the milk given to the calves communicate the disease to them.

A.—I don't know. I could not make much out of them. I did not estimate the calves, and the per cent. might vary, if they were brought into the estimate. My accounts, as far as they go, correspond with the accounts of the Commissioners, except that I did not take the calves in. I was there three days with the Commissioners; I have not seen the cattle since.

Q.—Where do you think the disease first originates, in the lung, or pleura?

A.—I do not know. I think it must originate in the lung, because I suppose it is communicated through the air, and will be likely to lodge in the place with which it first comes in contact.

Q.—Did you find any thing in the autopsy to indicate where the disease originated?

A.—No; only that I found inflammation of the lung and none of the pleura, and I have not heard of any case where there was inflammation of the pleura and none of the lung. I don't believe I know any thing more that will be of advantage to you.

Q.—How long do you think the period of incubation is?

A.—From two to four weeks, as near as I can tell.

Q.—How long the period of propagation?

A.—I said I could not tell. That is the thing I should like to know.

Q.—What is your opinion about the power to propagate, where the disease has apparently suspended its work, and nature is going on with the process of forming the cyst?

A.—I have guessed, in my own mind, that after the cyst was formed, the time of propagation was passed; but that is a matter of guess, which I should have put much more confidence in, if I could have had the control of Mr. Chenery's herd. It seems that the law is, that nature makes the attempt to surround the disease and cut it off, and then makes an attempt to absorb it and carry it off. In the longest cases, the lump is dissolved the most, and is the most brittle and easiest broken. It grows smaller, in proportion to the size of the cyst. There is evidently an effort to make it brittle, and absorb it. That is contrary to the law in the human system. We do not expect

absorption there; but I think the absorption vessels are very much more powerful in animals than in man. As we go down in the scale, that power increases. I am very well satisfied that an Irish child will recover from an injury that a Yankee will not. I suppose my Irish friends will not take any offence at that, as I date from that race myself.

Q.—I understood you to say that this lump becomes absolutely separated?

A.—Yes, Sir; it looks like a piece cut out. The inside of it looks alive. I presume it is preserved in the cyst, just as people preserve meats in preserving cans.

Q.—What is your idea as to how the absorbents are to take up the lump?

A.—By solution in the pus, and then the pus will be taken up by the absorbents, and carried off.

Q.—I believe you said that in a certain case, you found a ramification of the trachea that penetrated into the lump and passed through it? How does the disease appear to operate there?

A.—It begins by blocking up the bronchial tubes with coagulble lymph, so that they are air-tight, and cuts them off; but this was only where the sack was not perfectly air-tight.

Mr. Andrew.—If I understand you, you are of the opinion that some commission ought to have entire control over a diseased herd like that of Mr. Chenery's, and use them at pleasure, for purposes of scientific research, and to have the power to introduce healthy animals into diseased herds, and to experiment by that means as well as others?

A.—Yes; I think the interests of the Commonwealth require that. You know that one man is fitted for one thing and another for another. You want a commission to investigate the disease, whose peculiarities of character are such as will lead them to make a complete investigation; to take nothing on trust; but investigate it as far as it can be investigated. I do not believe that you will be able to exterminate the disease; and therefore I think that while it may be well to make the attempt, it should be coupled with other means, so that, if it should not succeed, the greatest amount of light may be thrown upon the disease, in order to protect the Commonwealth. It seems to me

the greatest scourge that has ever happened to this country, of a material kind.

Q.—How many have died of the disease?

A.—Forty had died before the Commissioners were appointed; then there were ten or fifteen that would have died, which were killed—say fifty-five full grown cattle. I excluded calves from my calculation.

Q.—What do you think of the attempt to form any opinion of the disease in this country from facts, or statements supposed to be facts, describing the disease in other countries?

A.—I had only opportunity to look at the facts in connection with the disease in this country; I should like to have had time to read up in regard to it in other countries, but I had not.

Q.—I believe that every thing in nature produces its own likeness, even if the climate be different?

A.—Every thing that has the power of propagating its kind. We do not expect a horse carried to Africa to produce a mule. I am opinion that if you inoculate and produce pleuro-pneumonia, another creature would take pleuro-pneumonia from the animal inoculated.

Q.—But you must first prove the paternity?

A.—I think you must.

Mr. Bird.—Dr. Thayer and Dr. Bates, speaking of this disease, say: "The same animal will show all the different stages of the disease; red hepatization, dark spots, and an effusion of serum." I understood you to say that red hepatization was rather the result of knocking the animal on the head?

A.—When I saw the cases at West Brookfield, I said it was red hepatization; but afterwards, on examining those Illinois cattle, I found the same appearance, which the butchers said was the result of the peculiar manner of killing them. There were dark spots in the lungs, the result of very free bleeding.

Q.—Did you find these dark spots in the lungs of the Illinois cattle?

A.—Yes, Sir.

Q.—The two first stages here described then, you found in the animals from Illinois?

A.—Yes.

Q.—But not the effusion of serum?

A.—No, Sir.

Evidence of Dr. George Bates.

GEORGE BATES, M. D. I am happy to indorse the report of Dr. Martin, with the exception of his opinions, and particularly his opinion on the recovery of the animals. I will take, for instance, the first case in the report, in which the doctor said that red hepatization extended to the whole volume of the lungs. In that case, there was a large quantity of effused serum. I was of opinion that that animal could not have recovered from the disease, and I am of that opinion still. So in case eight. This was a case of extensive disease, which was passing into the state of gray hepatization. I cannot conceive that the absorbent system could have removed the diseased matter in this sack. I believe it could not have done it in nineteen out of twenty cases I have seen.

Q.—Do you believe the process of absorption had commenced?

A.—The process of separation had commenced. The lump was not entirely, but partially separated. I recollect one case where it was nearly separated, but not entirely. In regard to the question of contagion, my opinion is that it is a contagious disease purely. All the experience that I have had in the matter contributes to convince me that it is purely a contagious disease. I am ready to respond to any inquiries that may be made, which I would prefer to do, rather than make any definite statement.

Q.—While this process of the formation of the cyst was going on, the cattle appeared to be in a healthier condition,—appeared to be doing well, improving, gaining flesh?

A.—They did, Sir.

Q.—Have you any idea at what period the formation of the cyst commences?

A.—It is difficult for me or any one to know when the formation of the cyst commences. I should infer that it commenced early in the disease, from the appearance of the cyst in different stages. The cyst is very small when the amount of disease is limited and circumscribed.

Q.—We have it in testimony that cattle which had been very sick, but which the owners thought were well, and which appeared well, when killed and examined, were found to have these cysts?

A.—In quite a number of cases. The attending adhesions show that there had been inflammation.

Q.—Do you think those animals would have died ?

A.—I think they must have died.

Q.—Was there any case in which there was simple inflammation, without any lump or cavity ?

A.—Yes ; the first case reported by Dr. Martin. That lung was in a state of simple inflammation, passing on to that stage which we call red hepatization. No lump was found.

Q.—How long had that animal been sick ?

A.—I think, fourteen days.

Q.—How long have you been with the Commissioners ?

A.—I went with them on their first visit, and remained with them two days ; and I have been with them subsequently five days.

Q.—If you found a case where the animal had been sick, refusing to eat, and the hair staring, and found that, in the progress of the disease, the hair became natural, and the appetite became good, should you not have had hopes that it would recover ?

A.—If an examination of the chest showed an improvement, I might, but not otherwise. From physical appearances, you might think the animal would recover, but from auscultation, you would be convinced to the contrary.

Q.—To what should you attribute the change in the outward condition of the animal and the appetite ?

A.—Merely to a change in the state of the disease.

Q.—And you could not look for any good result from such a change ?

A.—Not unless it was a very healthy animal. Animals that are well cared for resist disease better than those that are poorly kept, or thin of flesh, or in a bad condition, or in an exposed situation.

Q.—Would the fact that an animal had been well kept before it had the disease and afterwards, increase your confidence in its recovery ?

A.—It would. I should think the animal had more vitality to withstand the disease.

Q.—Something has been said about animals being feverish. Have you witnessed feverish symptoms—a quickening of the pulse, and heat?

A.—I did not examine the pulse in many instances; I did in one or two, with Dr. Thayer. He called my attention to the acceleration of the pulse, and heat; and there evidently was that feverish heat which usually accompanies fevers in individuals.

Q.—Was that in an early stage of the disease?

A.—In about twelve or fourteen days.

Q.—Supposing that you are right, and that the disease is an inflammatory one, how would it naturally be after the inflammatory symptoms had passed off? Would not the animal be more likely to have an appetite, and gain flesh and strength?

A.—It would.

Q.—And now suppose that the disease goes on progressing, would not this very lump be a constant source of irritation there, and be likely to produce a return of the same symptoms?

A.—I think it would, and perhaps the source of symptoms worse than the first.

Q.—Is it an uncommon thing in the human subject for inflammation to be attended with some fever and loss of appetite, and then for suppuration to take place, and until the amount of suppuration is so great as to produce disturbance in the system, for the patient to appear decidedly better?

A.—No, Sir, it is quite common.

Q.—Did you make any attempt at curing those animals?

A.—No, Sir.

Q.—Did any body, in your presence?

A.—No, not that I ever noticed. All the animals that I have seen have not appeared worth an attempt at cure.

Q.—Do you not think that in some of the cases mentioned by Dr. Martin, the animals would have recovered?

A.—I think some of them would; but I think a majority of them, as reported, would have died.

Q.—You do not agree with him, then?

A.—I do not, in that particular.

Q.—Did you assist in any of those examinations or autopsies?

A.—I did. At all of those which the doctor has reported, I was present.

Q.—In how many others?

A.—I am not able to state the number definitely. I should judge I had witnessed the deaths and examinations of some fifty creatures.

Q.—Of the one hundred and eighty-five animals, reported by the Commissioners to be diseased, how many did you examine?

A.—I am not able to give you a definite reply, Sir.

Evidence of Winthrop W. Chenery.

Mr. Winthrop W. Chenery.—I can only state that I sent to Holland for these cattle—three cows and one heifer. They arrived here the 23d of May, 1859. Two of the cows were found to be in a bad condition. The first died at the end of a week, and was killed and buried; the second one died two days afterwards, and was also buried. The other two were apparently well at the time. About the 20th of June, the third cow was found to be sick. She was confined in a pen in my cow stable, with some twenty or thirty head of cattle in the same room, which is fifty feet square. She died the 29th of June, about nine days after she was taken sick. (I do not pretend to give exact dates.) The next cow found to be diseased was taken in August. This cow was imported from Holland in 1852. She died about a fortnight from the time she was taken, in the same manner, and from that time, for two months, I had them dying constantly; but I kept no record of them, and, therefore, I cannot give the dates. I lost, between that time and the first of November, nearly all that I have lost.

Q.—Did you examine these creatures to ascertain what ailed them?

A.—Some of them were examined. Dr. Dadd examined the second one that died. I was away at the time, and did not witness the examination. Others were examined afterwards, by Drs. Wood, Saunders, and Thayer.

Q.—Did they, at that time, pronounce this the disease that now prevails?

A.—No, Sir, I think not. I understood that there was a difference of opinion about it. The three calves that have been spoken of as having been sent to North Brookfield, left the place on the 29th of June—the same day the first cow died of the disease.

Q.—Were they calved by these cows that you imported?

A.—No, Sir. They were sired by a Dutch bull imported in 1857.

Q.—What has been the condition of your herd since that time?

A.—My herd look well, very well, generally. There is no appearance of disease, except in three or four cases,—except, perhaps, an occasional cough.

Q.—Have three or four of them been diseased?

A.—There are several of them that had slight symptoms of disease, but they are now apparently well. One in particular that was very sick—one of the first that was sick, I think, last September—has, for the last three or four months, grown remarkably, looks remarkably well, but has a very bad cough.

Q.—Cow or heifer?

A.—Heifer.

Q.—How old?

A.—It is a yearling.

Q.—Does it appear to be improving in its condition?

A.—Yes, Sir. The same is the case with a yoke of oxen that were slightly sick. They work daily as much as any cattle.

Q.—Were you at home when these cattle arrived here?

A.—Yes, Sir.

Q.—Did you take the opinions of surgeons upon the sick cattle?

A.—Yes, Sir.

Q.—Who did you consult?

A.—Dr. Dadd, of this city.

Q.—Did he give you an opinion as to the disease at that time?

A.—He did not give the opinion that it was a contagious disease at that time. The opinion, at that time, was that the death of the two cows was caused by ill treatment on the passage.

Q.—Was that Dr. Dadd's opinion?

A.—It was his opinion, and my opinion, and is still my opinion. I don't think they had the disease at all; they showed no symptoms of it.

Q.—When the next cow died, was any surgeon called?

A.—Yes, Sir; the same surgeon.

Q.—What did he pronounce that?

A.—I think he called it pleuro-pneumonia. I did not charge my memory with it at the time, but my impression is, that that was what he called it. Nothing was said about its being contagious.

Q.—Who has attended upon your herd since that time, if any one?

A.—Dr. Wood, of Boston, attended the mammoth cow that has been spoken of, and Dr. Saunders and Dr. Thayer also attended with him.

Q.—Have your cattle been kept isolated from all others?

A.—Yes, they have strictly, for the last forty or fifty days, and were strictly during the time we thought the disease contagious, last fall.

Q.—Have you thought it not contagious since last fall?

A.—Yes, Sir. We thought it was caused by a want of ventilation in the barn, up to the time of the North Brookfield excitement. Since that time, we have had no doubt that it was contagious.

Q.—Whose theory was it that the disease was owing to bad ventilation, the surgeons' or your own?

A.—I think it was first suggested by Dr. Saunders.

Q.—What did you think about it?

A.—I agreed with him that it was so, at one time; I have altered my mind. I have no question now that it is contagious.

Q.—How long after the importation was it before the calves went to Brookfield?

A.—One went in six days. The cow that went to South Malden, I understand from my farmer, left the barn on the third day of July.

Q.—Was she sick?

A.—She has been reported by the Commissioners as sick.

Q.—Does your herd consist of imported stock entirely, or originating from imported stock?

A.—Yes, all I have now. I had, at the time this sickness commenced, two natives on the place.

Q.—How long was it after the cow went to South Malden, before this disease manifested itself?

A.—Seven months, I think. I only know the common report as to the time of her sickness. That was the cow that stood next to the cow that was first sick.

Q.—Has she been slaughtered ?

A.—I think not.

Q.—How many have you lost in all ?

A.—Thirty, including the three that were slaughtered.

Q.—Have you formed any opinion as to whether stock of imported origin are more liable to take this disease than native stock ?

A.—I think not. The proportion of my Dutch stock that have lived is greater than that of the others.

Q.—Have any other animals been sick there [South Malden] ?

A.—I think not. The cow has been kept alone.

Q.—Have any been sick in your vicinity ?

A.—No, Sir.

Q.—How long were these animals on the voyage ?

A.—I believe forty-seven days.

Q.—When did you commence the isolation of your herd from other animals in your neighborhood ?

A.—I commenced it about the first of September, or the last week in August.

Q.—Previous to that time, were any precautions taken ?

A.—No, Sir.

Q.—What was the fact as to their intercourse with other animals in the vicinity ?

A.—There was no intercourse, probably, before or after. My place is so isolated, that there is no opportunity for communication.

Q.—How near do any other animals come to you ?

A.—I should think one hundred rods. The pastures come together during summer, nothing but a common stone wall dividing my cattle from other cattle.

Q.—Do you or not keep a bull that serves the cows in that vicinity ?

A.—Yes, Sir.

Q.—As a matter of fact, that was your practice ?

A.—Yes, Sir.

Q.—Animals came to your place ?

A.—Yes, Sir.

Q.—Was that bull diseased?

A.—No, Sir; there never have been any signs of it?

Q.—He was with the herd?

A.—Yes, Sir; but at the time of service, the cow is not put in with the general herd.

Q.—Was that bull born abroad or here?

A.—He was born in Holland, and brought here in 1857.

Q.—Were those two cows sick before they arrived here?

A.—One of them had been mutilated on the voyage; one had not been able to stand for twenty days before her arrival.

Q.—That you did not attribute to the disease?

A.—No, Sir.

Q.—Did they both die?

A.—I killed one to put her out of misery.

Q.—That was the one you carried home on trucks?

A.—Yes, Sir.

Q.—Were those cows purchased by an agent of yours, or did you order them?

A.—An agent was employed specially to purchase them. They were purchased in the north of Holland.

Q.—It was not known that there was any disease, at that time, in that portion of the country?

A.—No; it was known that there was no disease there at that time. I have ascertained that since. There was a disease similar to that in the kingdom, but not in that vicinity.

Q.—Was the disease at Rotterdam?

A.—The animals were kept in Rotterdam some days previous to shipment, and probably took the disease there.

Q.—Was the disease there?

A.—I do not know that it was; it is usually there.

Q.—Where was the bull kept?

A.—It was kept in the centre of a lot of cows—a dozen; all of which have died.

Q.—How many cattle have you sold, to go away from your place, say since May of last year?

A.—The three calves that went to Brookfield and the cow that went to South Malden, that is all. I ought to say, perhaps, that I gave away one other, a bull calf, before the cow was taken sick, that I date the disease from. It went to the McLean Asylum, in Somerville. That was on the thirteenth of

June. He is still living, and I had him examined by a surgeon, a few days ago, who pronounced him perfectly well.

Q.—You considered the animals you sent to Brookfield and Somerville in perfect health?

A.—The bull calf went away before the cow was sick. The Brookfield calves went before we knew the nature of the disease. The calf that went to the insane asylum was not in the barn with the sick cow at all; the two that went to Brookfield were.

Q.—Did I understand you to say that in the case of the third cow that died, the lungs were examined?

A.—I think they were not.

Q.—Were the symptoms in her case like those that occurred in the cattle that died afterwards.

A.—They were.

Adjourned till Monday, at 9, A. M.

THIRD DAY.

MONDAY, June 4.

Session resumed at 9¼, A. M.

The chairman stated that Dr. Thayer was one of the physicians who had attended the slaughter of the animals, in most cases, and would give such information as was in his possession, of the history of the disease.

Evidence of Dr. E. F. Thayer.

Dr. THAYER.—My attention was first called to this disease in November last, at the farm of Mr. Chenery, in Belmont. There were, at that time, two animals taken with the disease, in an acute form. I went there with two other gentlemen, surgeons, and examined the animals. They were treated, and in a few days partially recovered. I have since had an opportunity of making a *post mortem* examination of one of those two animals. I found, upon examination, there were no traces of acute disease present. In one lung of one animal there was a portion of consolidated lung, evidently the result of a prior attack of

the disease. She was killed on account, however, of a diseased eye, not because she was sick. She was ruminating, and appeared well, with the exception of a malignant tumor, which we supposed to be a cancer of the eye.

In a day or two after his mammoth cow was attacked, and my attention was immediately called to her, in consultation. She was treated. In a few days after, the Durham cow had a severe attack, the herdsman saying she was attacked as violently as any animal last summer, when the disease raged in its intensity. She was treated vigorously, and apparently recovered. The mammoth cow was treated for a space of five and a half weeks, and died.

Question.—How treated?

Answer.—In the first instance, with laxatives and counter-irritation, and afterwards with tonics and stimulants.

Q.—What do you mean by counter-irritation?

A.—Blisters and setons.

Q.—Setons in the side, or neck?

A.—Both, Sir; both sides and dewlap.

Q.—When was she taken with the disease?

A.—The mammoth cow was taken, December 2d.

Q.—That was the first case treated for pleuro-pneumonia?

A.—No, Sir; we had treated the two other cases.

Q.—But the one that had a diseased eye?

A.—She was treated for pleuro-pneumonia, and apparently recovered. She was killed on account of a diseased eye, as she was not wanted for breeding purposes.

Q.—How long was this after the disease had apparently disappeared?

A.—This was the first that I knew of it. Twenty-four animals had then died.

The Durham cow, as I have before stated, received active treatment,—counter-irritation,—and as she evidently had the chronic form of the disease, and as the herdsman also stated that she had had a previous attack, when some mustard was applied to her sides, she was put under the influence of the hydriodate of potassa. This was in December. She was taken a few days after the mammoth cow was attacked. Dr. Wood was the principal in the case, and had the active treatment of the animal. I was called there as counsel. I had an oppor-

tunity, afterward, to examine, in the presence of the Commissioners and some others, this Durham cow, which was killed in April. On opening the thorax, evidence of disease was manifest on both sides. Adhesions had taken place. There was some consolidation of the posterior portion of the lung, and a cyst, of some five to eight inches in length, containing a small portion of pus.

The mammoth cow died in five and a half weeks from the date of the attack. Upon examination, the most extensive disease of both lungs was manifest. The anterior portion, as we term it, to the sternum and ribs, was very extensively diseased. The weight of the lungs, we were unable to take, as the animal was presented, as a specimen, to Professor Agassiz, and the examination was not as satisfactory as I should have wished. Yet the greater part of the lungs were taken out, and were estimated to weigh sixty pounds.

Q.—Do you say there was a cavity in each lung?

A.—There was a cavity in one lung, but adhesion in both.

Q.—The Durham cow?

A.—Yes, Sir. The mammoth cow's lungs were a mass of disease, and, more particularly, the left,—effusion of lymph and serum. The serum had probably become absorbed; but there was a great effusion of lymph, both into the lobules and into the interstitial tissues beneath the pleura, and consolidation of the lungs.

At the time this occurred, in December, I almost every day examined one or more animals which had recovered or were supposed to have recovered. There was a Dutch heifer, so called, that, upon examination, showed very extensive disease in the chronic form. Some of this hydriodate was given her,—merely experimental,—and she died the day previous to the death of the mammoth cow. The most extensive disease was found in her lungs, but of a character showing longer-continued disease. There were, in the cavities, pus—in fact, the lungs were both a general disorganized mass. She must have lived for some time upon a very small amount of healthy lung-tissue. In fact, she could eat but very little without incessantly coughing.

Q.—Was that an imported heifer?

A.—She was by a former importation, I think.

Q.—Did these cysts, that contained no separate portion of the lung, but some pus, communicate with the main branches of the trachea ?

A.—They did not communicate with the branches of the trachea or bronchial tubes, in the Durham cow, as was the case with the calf examined on Saturday. That was the first instance I had ever seen, where there was cutting off, as it were, of the bronchial tube, and connection with the sac.

Q.—Then if there had been pus to a great amount in these latter sacs, and it had been removed, it must have been removed by absorption ?

A.—Yes, Sir ; I think so, decidedly.

Q.—When did you first become acquainted with Mr. Chenery's herd ?

A.—In November.

Q.—In regard to those animals upon which you used the medicine,—how long after the disease appeared in them was it, before the remedy was applied ? I refer to all of them of which you have been speaking.

A.—I think the next day after they were observed to be ill, in the first two cases—in fact, in all of them ; there was but one night intervening, I think, between the attack and the application of the remedies.

Q.—Do you speak that from your own knowledge, Sir ? I thought you were merely consulting physician.

A.—That was what was reported to me, "that they were taken yesterday," when I got there. The time of their attack is a question we almost always ask.

Q.—Did you observe any febrile action in these creatures, at the commencement ?

A.—Yes, Sir. They differed somewhat. The mammoth had decided febrile action ; should have stated that three or four or five days previous, parturition had taken place with her. She was not known to be ill at all, till Saturday prior to calving. She appeared a little amiss, which, as they supposed, was in consequence of that. And the second day after calving—these dates I have, but haven't them in my mind perfectly—the mammoth cow was observed to have those peculiar signs of disease, and our attention was called to her. She had decidedly

febrile action. She had, what is rather unusual in cases that I have seen in Brookfield, a quick, hard, wiry pulse.

Q.—Was blood-letting among the remedies proposed?

A.—No, Sir.

Q.—Did this calf from the mammoth cow die?

A.—No, Sir; it is now living. I saw it on Saturday.

Q.—Does it exhibit any appearance of having the disease now?

A.—Not the slightest, unless you may except a little enlargement of the knees at the time of birth. That is, apparently, something of a scrofulous character; we do not know what causes it, or whether or not it has any connection with that disease.

Q.—Did you observe whether, when these cattle were attacked, they appeared to have rigors, chills, and such symptoms as in the human species usually precede febrile action?

A.—I think that has not been observed here. But we have that report from England, that it is observed, if they are closely watched, that they have occasional rigor.

Q.—Have you ever seen, in the course of your practice, any lungs exhibiting the same appearance, on *post mortem* examination, as these?

A.—No, Sir; not the slightest. There is a peculiarity in the appearance of these lungs, that I have never found prior to the occurrence of this disease.

Q.—You are satisfied that there have been no cases of pleuro-pneumonia among cattle in this country before?

A.—Not of this "contagious pleuro-pneumonia," as we term it. We have often true pleuro-pneumonia in cattle. This is not true pleuro-pneumonia.

Q.—How early did you discover the contagious disease?

A.—I followed it up to Brookfield, and traced the disease from the herd at Belmont, in one form or another, which brought me to a satisfactory conclusion that if was contagious.

Q.—What time of year was that?

A.—I went to Brookfield in March.

Q.—But did you make up your mind in March, that it was contagious?

A.—I did upon my first inquiry as to how a particular animal got it.

Q.—Was that in March?

A.—Yes, Sir. All that I knew before, was from evidence that I had read in my veterinary works.

Q.—But you did not recognize the genuine contagious pneumonia till the month of March?

A.—Yes, Sir; I did at Mr. Chenery's, but I did not know personally where it had proved contagious here, until this time, excepting from the report of Mr. Chenery.

Q.—Then you did not recognize it as the disease that has prevailed in Europe and in other places?

A.—I did at first.

Q.—Did you pronounce it so to Mr. Chenery?

A.—I did, Sir.

Q.—At what time?

A.—November.

Q.—Did you caution them at that time against exposing other animals?

A.—Well, Sir, we were divided in opinion in regard to it, and Mr. Chenery was present most of the time, and heard the conversation. I objected to it. I recollect that another told him he would as lief put his own cow into Mr. Chenery's barn as not. Mr. Chenery knew my opinion; but he knew that of another who differed from me, and what his mind was I do not know.

Q.—Who was the other?

A.—Dr. Saunders, of Salem.

Q.—He did not think it contagious?

A.—He did not think it the disease. He attributed the disease entirely to the want of ventilation of the building.

Q.—Do you know whether Dr. Saunders has changed his mind, in relation to the disease, now?

A.—I do not know. I have not conversed with him, recently; I have not met him.

Dr. Choate.—I saw him, two or three weeks since, and he then seemed to be aware of its contagious nature.

Q.—[*To Witness.*] Have you been in the habit of treating cases of common pleuro-pneumonia,—that is, simple inflammation,—in cattle?

A.—We have, once in a while, a case; they are rather rare. I have been called to several such cases.

Q.—What is the usual result in such cases?

A.—If called in season, there is no trouble in treating them.

Q.—What is the proper treatment in the early stage?

A.—According to its indications. If decidedly of a febrile character,—hard pulse, quick breathing, crepitating sound in the lung, and so forth,—if called in season, it should be treated with bleeding, and such medicines as are indicated at the time. If that stage is passed, we cannot bleed them. As I said before, the cases are rare, compared to those in horses. It is a disease peculiar to this climate, in horses, every spring.

Q.—Have you been accustomed to see animals slaughtered for beef, and examined them?

A.—I have; and since this occurred, I have taken several opportunities to visit slaughter-houses, for that purpose.

Q.—Is it common to find the lungs diseased of cattle killed for beef, that are apparently well?

A.—I have not been able to find, in market, diseased lungs in cattle, this year. I have, some years previous, seen them, but then not, perhaps, thinking so much of it. I merely thought it the effect of some diseased lung. But we occasionally find, perhaps, a slight adhesion of one lobe to another,—or one little spot, or something of that kind,—which may have been caused by close contact in cars, and want of ventilation, or the result of some slight attack having occurred heretofore. I have not seen many diseased lungs in slaughter-houses.

Q.—Diseased lungs of horses are frequent,—are they not?

A.—Yes, Sir; but the horse is a kind of animal that is driven rapidly, and is more liable to diseased lungs than cattle.

Q.—What do you understand by "contagious disease?" You have pronounced this contagious pleuro-pneumonia; and the common pleuro-pneumonia is not contagious. What is the difference? I do not mean the difference in symptoms.

A.—I have always called this *infectious*.

Q.—We will not go into nice distinctions; but what do you mean when you say that this is a contagious, or infectious disease?

A.—I recognize contagion to be the communication of the disease by touch. If an animal touches another, it might be

called contagion. Infectious,—he should receive it either from the air, or from the breath of the animal.

Q.—But, in a medical point of view, what makes a disease contagious, or infectious? What do you mean when you pronounce this contagious or infectious, and other forms not contagious or infectious?

A.—I do not know that I exactly understand you; but my idea is this,—that it is well known that ordinary pleuro-pneumonia will not convey disease to another animal standing by the side of the sick one. But in this case, it is, to my mind, perfectly satisfactorily proved that if an animal stands near enough to breathe the air from the other, if at all susceptible to the disease, it takes it.

Q.—What constitutes susceptibility? Do you say this disease is contagious or infectious under all circumstances, irrespective of the physiological habits or conditions of the subject within a certain distance?

A.—My opinion is, so far as I have have been able to judge, that there are a very few animals not susceptible to it, under any circumstances. They seem to be so, at least. An animal was killed, which was pronounced, by the surgeon, sound. It ruminated, and had no appearance, not the slightest, of disease, or even of ever having had the disease. There was neither upon auscultation, or percussion, any appearance of the disease, and the creature was bright and healthy. The medical men called for an animal pronounced sound, and this one was selected. She was examined, and not the slightest trace of disease was found upon her lungs, in any particular.

Q.—Where was that animal taken from?

A.—I think, from Mr. Olmstead's.

Q.—How long had she been exposed?

A.—She was said to have been in his herd all the winter; and he had about as sick a herd as any man.

Q.—Did any one suspect she had the disease, before she was killed?

A.—No, Sir, I think not.

Q.—Do you call this disease contagious, or infectious?

A.—I think it infectious, as I use the word.

Q.—Within what distance do you call it infectious?

A.—I think, if in a very tight barn, it would give it through the barn, if the barn was battened.

Q.—But you have said you consider it infectious, irrespective of the physiological conditions of the animal.

A.—I do not consider that a physiological condition. If put in a close room, and obliged to inhale the air at ten feet off, the animal would take the disease; but if there was a good current of air passing between the two, she would not take it.

Q.—Then, how far is this infectious virus controlled by these other things?

A.—I think, considerably.

Q.—How much?

A.—I think an animal which stands within two or three feet of it, would be sure to get it; and I think, with a free, open ventilation of the building, at a distance of ten or fifteen feet, it might escape.

Q.—Is not what you say in regard to this, true in respect of contagious and infectious diseases, as to their effect upon individuals coming within range?

A.—It depends, of course, upon the physiological condition of the animal. I believe the itch connot be contracted without touch, but I do not know of any other instance.

Q.—What is the law of contagion or infection? Is it so that medical men will risk their reputation upon a positive statement such as this: "Here is an animal with a disease which we call infectious; we will risk our reputation upon the assertion that, under all possible conditions of certain animals, it will communicate the disease, at a certain distance?"

A.—I should think there might be exceptions.

Q.—The number of exceptions would settle the question whether the disease was most affected by the virus, or other considerations?

A.—I consider the infectiousness of this disease settled by the amount of the poisoned air inhaled.

Q.—Then that depends upon the amount of the admixture of the pure air?

A.—Yes, Sir.

Q.—What we want to get at is this: in considering this question of the contagiousness or infectiousness of the disease,

does the liability to disease depend upon the condition of the subject, or the morbific potency of the virus?

A.—I don't know that I can give a satisfactory opinion upon that; for I believe that both have some share in determining the matter. The more an animal is predisposed to the disease, the more likely it would be to take it.

Q.—Is it not a doubtful question?

A.—Yes, Sir; it is.

Q.—But you believe that every animal with this disease makes a certain amount of poisoned atmosphere about it?

A.—Yes, Sir.

Q.—If the animal should be put in a close barn with a dozen cows or oxen, the poison in the atmosphere would be more intense?

A.—Decidedly so.

Q.—If the wind blew freely through the barn, it would carry off a large portion of it?

A.—A large portion of it.

Q.—Do you consider smallpox contagious?

A.—I suppose it is.

Q.—If a certain number of persons are exposed to the smallpox, is it not a matter of notoriety that a certain portion will take it, and certain others will not?

A.—Undoubtedly, a few will not take it.

Q.—Do we know the physiological or other condition which prevents their taking it?

A.—I do not, Sir.

Q.—Do you know that any body pretends to know?

A.—I do not, Sir.

Q.—Do you or do you not mean to say that the evidence of contagion in this disease is similar to what we know of contagion in smallpox?

A.—I should think it very similar.

Q.—What was the condition of Mr. Chenery's building as to ventilation?

A.—He had what we call a "ventilator," going up through, of considerable size.

Q.—How many cattle were kept in this room?

A.—Forty-two. The room was fifty feet square, its height about eight feet.

Q.—Which should you think the more contagious,—if we may use the comparison,—smallpox, or this disease?

A.—Well, Sir, I could not answer that with any satisfaction to the public or myself, for many are protected by vaccination.

Q.—Independent of vaccination?

A.—My experience has been small. I should hardly think there was much difference, if they were confined in a close place, under similar circumstances. I don't know, however; I really could not answer that question satisfactorily to myself. I have not reflected upon it. In fact, I know but little about smallpox.

Q.—What, in your opinion, is the best mode of extirpation,—by general slaughter or isolation,—or any other mode?

A.—In answer to that question, I would observe, that under the present aspect of the case, my opinion would be, perhaps, now, that it would be best to form a territory for it, thoroughly and efficiently protected, to isolate the cattle, and then to have those cattle closely watched, and as soon as a sick one is found, have her taken out and slaughtered, and follow that up for a great length of time. Otherwise, I cannot see any efficient way of extirpating the disease, but by slaughter.

Q.—How long, in your judgment,—if you have formed any opinion upon it,—should exposed or diseased animals be isolated, before they are permitted to go at large?

A.—I should never let them go at large, but should have them fattened and killed. I should never allow them to go with healthy animals. I should never permit them to mix with healthy cattle.

Q.—How far would you keep them from other animals?

A.—I should never permit them to associate freely.

Q.—You would not permit them to be in the same herd?

A.—They might be in the same herd, but I would not permit them to come together.

Q.—What constitutes the exposure on account of which you think it necessary to insulate the cattle? Within what distance shall my cow have been of a diseased animal, in order to make it necessary that I should shut her up?

A.—I should say, being in the same herd—in the same yard,—so that they could get their noses together, and the healthy cow

get the breath of the diseased one, would be sufficient to constitute exposure.

Q.—Have you examined the cow that went to South Malden ?

A.—I have ; but not under the most favorable circumstances. She was in the road, feeding, and was caught and held. I examined her by auscultation, which indicated tuberculation of one or both lungs. Her natural signs, the general appearance of the animal and looks of the eye, the skin and hair, showed that she had disease, and the medical signs certainly did.

Q.—How long was it after that animal went from Chenery's, before the symptoms of disease developed themselves there ?

A.—The committee of the agricultural society called upon the gentleman who bought her,—Mr. Marsden. He stated that he brought her home on the 30th day of July, and that she was in perfect health until February, when she was sick. She breathed short, discharged from the eyes and nose, I think, was very sick, and did not take any food, or ruminate, for about ten days or a fortnight. That was his statement.

Q.—Is it considered possible that that animal could have been exposed to the disease at any time after she was taken from Mr. Chenery's ?

A.—Not if what they said is correct. I should think not.

Q.—The Malden cow is still living ?

A.—So far as I know.

Q.—What is her condition ?

A.—She was poor, her skin pretty tight to her ribs, her eye looking dull, and she had a dejected appearance, though she was giving six quarts of milk per day.

Q.—When was that examination made ?

A.—About a fortnight since. I cannot give dates. I make the examinations, and my hands are bloody, so that I cannot keep a record. I have to make these statements from memory. The Committee, or Commissioners, have a record of all these things, which can be called for.

Q.—I understand you to say that a creature which had been once exposed, could never be safely permitted to mix with other cattle ?

A.—That is my opinion.

Q.—Upon what account ?

A.—From observation in two or three cases I could name, or at least in two, from the reports which I have in my veterinary journals, where it is stated that the disease breaks out in the sporadic form, I am convinced now that the animal is liable to another attack of the disease, whenever any exciting cause occurs to develop it.

Q.—How can you tell whether the creature did take the disease by exposure? You say an animal once exposed must never be permitted to mix with other cattle?

A.—I think not, because the disease is so insidious and stealthy in its character, that an animal may have considerable disease going on, without its being known to an ordinary observer.

Q.—All its life?

A.—I think it perhaps so.

Q.—Is that true of any other contagious disease?

A.—Not of any that I know of.

Q.—Then you think that contagious period may continue through all the life.

A.—It may continue a very long time, so long that I do not know when to fix a period that would be safe. Therefore, I advise that the animal should be fattened and killed for beef.

Q.—What do you think of vaccination?

A.—I know nothing of it particularly, and have to take my opinion from a report of the commission sent to Belgium, and from just reading those reports I came to the conclusion that it was perfectly valueless.

Q.—Did you hear Mr. Lindley on Friday?

A.—No, Sir; I did not.

Q.—In point of economy, which, in your opinion, is the more expensive—killing the cattle and paying for them, or attempting to isolate and cure them?

A.—Animals sick, I think it would be cheapest, as far as dollars and cents are concerned, to kill and bury them.

Q.—If they were exposed, what would you do?

A.—Isolate and watch them—and have it done efficiently, or it is of no value.

Q.—Would you proceed to fat them immediately?

A.—I would, Sir.

Q.—Do you know any thing about the rumor circulated, on Saturday, about Mr. Chenery's, as to that red ox which appeared, now, to be healthy, but had the disease in December, recently communicating the disease, when it went off after hay?

A.—Yes, Sir.

Q.—State the facts, if you know them.

A.—That ox, when I went there, in November,—both oxen, indeed, but one particularly,—was evidently suffering from the effect of the disease. This spring, they have evidently been gaining in flesh and condition; in fact, they have altered in appearance very markedly. On the 11th of April, I think it was, they went to Mr. Wellington's barn to get hay. They backed into the barn. There were, in the barn, five animals,—three cows and two heifers. The noses of those oxen came very near these animals. Sometime in May, Mr. Wellington notified the Committee, and I was sent there to examine the animals. One animal had a calf, but yet, showed considerable excitement in the system. We could not form any definite opinion as to whether they were diseased, or not. They were put under an injunction. On last Saturday, I visited them myself. The day before, a calf of one of the cows showed very quick breathing, and alarmed the man who was keeping her. We found the calf breathing with some difficulty; but yet, being in a cellar where it is rather damp, we did not know but this might have been caused by the animal's taking cold.

Q.—How old was the calf?

A.—Five weeks. The cow showed some bronchial or tubular respiration,—not very marked. She was in pretty good condition.

Q.—Was the cow in the cellar?

A.—Yes, Sir. The day we examined her before, she showed some quickening of respiration, but as she had had a calf, and seemed to be anxious about it, we thought it might be attributable to that.

Q.—Can you say that the disease was communicated to her by the oxen?

A.—I cannot. I did not know that the oxen had the disease then.

Q.—Do you know that they had it now ?

A.—No, Sir. The report says, that when the calf was killed, it was found to be diseased.

Q.—This whole story about their going for the hay is merely hearsay, is it not ?

A.—I know nothing more than that my attention was called to the animals.

Q.—Why can't you answer the question ? Is it hearsay or fact ?

A.—A good deal of what I say I have to take from reports. I went there and examined the animals at the request of the Commissioners.

Q.—Was the story of their going after a load of hay a matter of hearsay, or do you know it as a fact ?

A.—It was a matter of hearsay.

Q.—Who first told you the story ?

A.—Mr. Wellington first told me. I will state, as corroboratory, that he had already been to Governor Banks, and the governor had informed him that he should send one of us out to see him.

Q.—What was the condition of the cellar in which this calf was kept ? Would it have been likely to take cold there ?

A.—I should say so. It is a cold cellar—two-thirds of it under ground.

Q.—What is the size of the cellar ?

A.—Sixty feet by forty, and some seven or eight feet high.

Q.—Do you know who performed the autopsy on the calf killed Saturday ?

A.—I do not, but I understand that Dr. Loring was present.

Q.—This was the cellar used for the manure of the barn ?

A.—Yes, Sir.

Q.—Was it all open ?

A.—Yes. She was shut up in a pen. Mr. Wellington told me that the animal had not appeared well, and he was a little frightened, and put her down there to have her out of the way.

Q.—Do you know how many months the oxen had the disease before they were sent to the barn ?

A.—I know they were sick prior to my first visit, in November.

Q.—How many months would have elapsed between that time and the time they went to the barn ?

A.—Six months, certainly.

Q.—Were these oxen regarded as cured at the time they went after the hay?

A.—I do not know. They were at work, and had been worked for some months. In March, I visited Brookfield, in company with Dr. Wood, and was taken first to a barn which was then occupied, a portion of it, by Mr. Curtis Stoddard, who bought the calves at Belmont. There we found an animal sick. We then proceeded to Mr. Alden Woodis's yard, and there we found an ox very sick, and we were both of the opinion that he could not recover. Several other animals were evidently affected by the disease, in various stages. Upon inquiry we were informed—not by Mr. Woodis himself that day, but he afterwards corroborated it—that Mr. Stoddard asked the privilege of putting his oxen into Mr. Woodis's barn, to be kept over night, while he was "logging," as it is called, in that neighborhood, to save the transit over the road, and while there these oxen sickened, and one of them had to be helped up in the morning once or twice. A few weeks after, his animals began to be sick, and one was sick at that time, and very sick. This was in New Braintree. We then went to Mr. Olmstead's, in Brookfield. We found there a very sick cow—so sick that we thought she could not recover; and upon examining all the animals, we found many very sick, and for our especial gratification a calf was taken out and killed, and we found both the chronic and acute forms of disease exhibited in a very marked degree. The calf, we were told, was four weeks old. Three weeks afterwards, I saw the cow that we thought could not live, and she was better—was ruminating, and there was a better appearance of the eyes and hair. A few days after she was taken out and killed, and her left lung was found to be entirely solidified, and weighed sixteen pounds.

Q.—What would it weigh in a healthy condition of the lungs?

A.—Some weigh two and three-quarters pounds, and some come up to three pounds. This was a single lung, separated from the trachea or windpipe.

Q.—What was the state of the other lung in this case?

A.—It was nearly healthy, and weighed four and a half pounds. The anterior portion of it was consolidated.

Q.—What you call red hepatization?

A.—No, Sir; rather a confused lymph. I went out at that time on purpose to examine the situation of the farms, and satisfy myself perfectly with regard to the contagious, or infectious, (as I call it) nature of the disease. I found the barns well built, with perfect drainage, and most of them pretty well ventilated, some of them very well, by the natural openings in the boards; but Mr. Olmstead's was decidedly close for a country barn; and the disease raged in his herd as extensively as in any herd that I have witnessed. Mr. Olmstead purchased a pair of oxen of Mr. Leonard Stoddard, and kept them in his barn but five days, when he sold them to a neighbor, after which his animals commenced being attacked by the disease with great violence and considerable fatality.

Q.—Have you any personal knowledge of the manner in which Mr. Chenery's cattle were kept through last summer—whether they run in the fields adjoining which his neighbor's cattle ran all the summer through?

A.—No, Sir, I have no personal knowledge of it.

Mr. Chenery, being in the Hall, at the request of some members of the Committee, stated the facts in regard to this matter. He said, "They did run, with cattle in the adjoining pastures, for several weeks."

Q.—Was the disease communicated to any of the cattle running in the fields adjoining yours?

A.—It has not been, so far as I know.

Q.—Were the pastures fenced in such a way as to prevent the cattle from getting their noses and heads together?

A.—They were not. We had no cattle running in the pastures that we supposed to be diseased at that time.

Q.—They were cattle that had been exposed?

A.—Yes, Sir.

Dr. Thayer resumed. The particular manner in which Mr. Needham had the disease communicated to his herd, I do not distinctly recollect, although I heard it stated; but those were the places where the disease raged with the greatest violence. I spent the day in examinations of that character, and in examining that calf, and I could come to no other conclusion

than that it was the disease called pleuro-pneumonia, and that it was highly contagious, or infectious, as I term it.

Q.—I understand you to say that you would be in favor of destroying diseased cattle and of isolating those that have been exposed; but I understand you to say that you would not be willing to have such cattle mix with those not diseased. What benefit, then, would the isolation be?

A.—To stop the extension of the disease, and fat them, so that they might be killed for beef, after they had been exposed. The reason is that the disease is so stealthy in its character that I should never dare to risk allowing cattle that had been exposed to mix with others. I will state, in illustration, that I was instructed to examine an animal in Brookfield, near Sturbridge, that left the herd of Mr. Stoddard on the 23d of October. She was sold at auction, and was finally purchased, in November, by Mr. Nichols, who bought her to supply him with milk during the winter, thinking he should get four or five quarts a day, but he only got three pints. So he tried to feed her so that she would give more milk, and then to fat her; but she would not fatten to any extent. I killed her in May, and, on examination, she showed no extensive hepatization, but the most spots I ever found,—some twenty or thirty, and there was some hypertrophy of the lung; it weighed nine and three-quarters pounds. The animal had the disease in an acute form, and had been exposed in no other way, so far as I could learn; and yet there was great difficulty in fattening her. That was seven months after she left the herd, and never had shown any acute signs of the disease, or communicated it to others in the herd, probably; but she had been in this ailing condition herself, without the ability to fatten.

Q.—Should those animals be fattened and killed, would you suppose that the meat would communicate the disease?

A.—I think not.

Q.—Were you present at the slaughter of Mr. Olmstead's stags?

A.—No, Sir.

Q.—Do you think it possible to fatten animals that are diseased?

A.—Perhaps they may recover sufficient to be fattened, in a sufficient length of time. From the appearance of this cow, I

should think it would, in many cases, cost more than it would come to. However, I should attempt it.

Q.—What is now the condition of Mr. Chenery's cattle that had the disease ?

A.—They are in tolerably good condition.

Q.—Do you think there would be any difficulty in fattening them ?

A.—I should think from the experience and observation I have had, that it would be difficult to fatten them for first class beef; they might be fattened and sold for middling beef.

Q.—Where have you seen the attempt made to fatten cattle in such a condition as Mr. Chenery's ?

A.—This animal in Brookfield or Sturbridge—it was near the boundary line, and I don't know which town it was in. That was the only instance. Mr. Chenery is a good feeder, and his cattle have the best of care; but yet the animals that are known to have been diseased have certainly not thrived. That is all my experience. They have not appeared like good, healthy animals since the disease.

Q.—Are not his working cattle in good condition ?

A.—Now they are, but through the winter, they looked dejected and poor.

Q.—And from the fact that they were poor in the winter, but are now in good condition, do you not think, Sir, they might be fattened ?

A.—I think they might. I have no doubt of it.

Q.—Do you think the beef would be healthy ?

A.—I think it would.

Q.—Do you think the beef would be salable in market, if it was known that these animals had been exposed ?

A.—I think not, Sir.

Q.—You would buy it ?

A.—I should, if I couldn't get any other; but I should prefer beef which I knew had not been diseased at all. But yet, I do not think any harm at all would arise to a person, from eating that beef,—decidedly not.

Q.—What allowance would you make in the valuation if the animal had been exposed ?

Witness.—For fattening ?

Q.—For any purpose.

A.—Now, it cannot be sold at any price, to any intelligent person.

Q.—What is the creature worth, when she has been exposed to the disease?

A.—Just what she is worth to fatten, if well cured.

Q.—One-half, or the whole?

A.—Under the present excited state of feeling, she would be almost valueless, except for fattening. That is all I can say.

Q.—You spoke of some examinations you had made, of indications of acute and chronic forms of the disease. What do you mean by "indications of the acute form of disease" in the *post mortem?*

A.—The red hepatization.

Q.—Were those cases where the animals were diseased, or where you suspected them of disease?

A.—I do not recollect that I have mentioned both the acute and chronic.

Q.—Yes, Sir; in the calf.

A.—That is correct. The calf had consolidated lung tissue: almost the whole of one lung was entirely solid.

Q.—But what do you regard as indications of acute disease?

A.—This is the chronic stage; and I was going to show what I considered the acute. It had effusion of serum in the thorax. That is decidedly an acute symptom, and a very prominent one.

Q.—Peculiar to this disease, and existing in no other?

A.—It exists in this; but it is peculiar to another, also. It exists in ordinary pleuro-pneumonia.

Q.—Then that does not necessarily indicate pleuro-pneumonia?

A.—No; but with the solid portion of the lungs it would.

Q.—What are the acute symptoms?

A.—Some hypertrophy of the lungs; that is, a dilatation of lobules, a separation of the coloring matter of the blood, beneath the pleura, into the interstitial tissue, and effusion, all taken together, I should consider of the acute stage.

Q.—Of the early stage?

A.—Of the early stage.

Q.—What do you mean by red hepatization?

A.—I mean, a change taking place in the tissue, arising from a deposit of the red particles of the blood.

Q.—You found it had taken place?

A.—Yes, Sir; by dissection. I have dissected off the pleura, and found it free; whereas, the sub-pleural tissue had this appearance.

Q.—Dr. Martin says he found what he called red hepatization, and that afterward he found the same symptom in healthy animals, killed at Worcester, where it was only the consequence of the arresting of the blood.

A.—I have followed that up closely, myself, Sir; and I have found a marked difference between cadaveric change in the healthy animal and one with this acute disease. It is decidedly different. The red hepatization extends further, and is isolated, as it were, in patches, oftentimes, till it becomes more general. We have that symptom described by eminent men; and I have distinctly found it, although I am not a microscopist, and cannot speak from experience microscopically.

Q.—Dr. Martin washed it, and found it washed entirely clean.

A.—I have tried it several times, and in cases I called red hepatization, I could not wash it.

Q.—How many of these cases?

A.—I kept no record. I report to the Commissioner, or agent present.

Q.—Who has the record?

A.—Mr. Walker, I think. I have taken some, where I have gone alone, and have been obliged to take notes, and returned them to the Commission.

Q.—How many cases of diseased animals did you have, to examine?

A.—I should think from sixty to a hundred. I do not know exactly,—fifty, sixty, seventy.

Q.—How many examinations have you made, yourself, of which you have made a record, or taken notes to which you are willing to stand professionally?

Witness.—In company with others?

Q.—No,—your own minutes.

A.—Sometimes another physician was present, and he might report, or I, or both. I alone have made, perhaps, from twenty to forty,—I cannot tell very nearly.

Q.—And of those you have no record?

A.—I have kept no record. I know that a record was taken at the time.

Q.—You have never examined the record that was made?

A.—No, Sir.

Q.—Have you ever seen or heard of a report like this, which has appeared in the newspapers: A Frenchman eat the meat of a diseased animal, without inconvenience, while a cow that eat the swill of the meat took the disease?

A.—I read the report. It was a Frenchman who lived near Mr. Olmstead. Mr. Olmstead informed me that the cow came to one of his stags and smelt of him, and took the disease from his nose. I attribute the disease of the cow to exposure to the stag.

Q.—What part of the animal is first attacked?

A.—The lung, in my opinion.

Q.—That is the primary seat of the disease?

A.—I think so, decidedly.

Q.—And not the blood?

A.—I think so, decidedly; but then, the blood is at all times in the lungs.

Q.—Yes; but the question is, where does the disease first attach itself—to the blood or the lungs?

A.—I think, to the lungs.

Q.—Has the blood of any animal ever been examined?

A.—Blood has been put into vials on one or two occasions, but whether it has been examined or not, I do not know.

Q.—Has the milk of cows affected by the disease ever been tested?

A.—Not to my knowledge.

Q.—How do you account for the success of inoculation, if the disease attacks the lungs in the first instance?

A.—I don't believe in the success of inoculation, to begin with.

Q.—Has it ever been attempted in this country?

A.—Not to my knowledge.

Q.—What treatment did you adopt in the Brookfield cases?

A.—None, Sir; I was not called there as a practitioner.

Q.—What treatment have you used with Mr. Chenery's herd?

A.—I was present, and coincided in the treatment, as I said before—counter-irritation, attending to the general condition of the animals, the bowels, &c.

Q.—"Attending" does not seem to be *any* treatment, but merely watching?

A.—If the animal was costive, I gave an aperient, or laxative; if too lax, something that would work the other way. The treatment was a little sulphate of magnesia, at first; afterwards, thoroughwort tea was administered, the sides were blistered, and the dewlap plugged.

Q.—Has any case been treated for disease in the blood, on the ground that the disease first attacked the blood?

A.—Not that I know of.

Q.—Have you treated any case of recent exposure, before the development of the disease, in any way?

A.—No, Sir.

Q.—Did you make any inquiry as to what had been the keeping of those animals in North Brookfield through the winter? Were the cattle fed as they are commonly in the country?

A.—I presume so; I do not know any thing to the contrary.

Q.—Will you give an account of the animal which you slaughtered at Mr. Chenery's on Saturday?—the history of that animal, from the beginning?

A.—As far as I can. I was not acquainted with Mr. Chenery's herd until last November. I then examined the animal, and found evident marks of disease. The animal coughed frequently, and upon auscultation, a decided solidification and loss of the use of the lung was manifested. I have seen her several times since. She has been gradually improving, although at all times, auscultation manifested the presence of disease in the right lung. Percussion also indicated adhesions, as the sound was dull from the right side. On Saturday, she was slaughtered. Upon attempting to take off the ribs on that side, they were found firmly attached, by a very tough, fibrous band, between the pleura-costalis and the ribs, and the pleura and lungs. That was divided, and then we found the lungs adhering to the diaphragm, to the posterior and anterior part, very firmly. We separated it from the bronchial tubes, near the root, and took it out. Afterwards, it was laid open, and a

cyst was found in it, some eight, nine, or ten inches long, with a connection with the large bronchial tube; and in that and in the bronchial tube was a kind of mucous. A very small portion of that lung only was what we should call a healthy lung tissue—scarcely any.

Q.—Was there an inch square any where?

A.—Not in any one place.

Q.—Was there any healthy division of the lobes, as there usually is in a healthy lung?

A.—One large lobe could not be distinguished—two seemed to be absent.

Q.—Do you know, from Mr. Chenery, when this animal became sick?

A.—He said, in September?

Q.—Did he tell you how the animal was at that time?

A.—Nothing further than that she was quite sick.

Q.—What has been the condition of that animal, from the first of your seeing it, in November, to the time of its slaughter?

A.—It looked somewhat debilitated during the autumn and winter, and would always cough a short time after eating. This spring she evidently very decidedly improved in condition.

Q.—Had she a good appetite?

A.—Yes, and ruminated; and yet, after eating a little, she would almost incessantly cough.

Evidence of Prof. W. S. Clark.

Prof. W. S. Clark, of Amherst, was then called upon. He said:—

I have no new facts to communicate, and my opinions may be of but little value, as I am not a professional veterinarian, nor a commissioner, nor a legislator; and yet, I am greatly interested in this thing. Not many weeks ago, I was in blissful ignorance respecting this disease, and I would to God I could go back to that same condition;—but, having been called upon by the Secretary of the Board of Agriculture, as President of the Hampshire Agricultural Society, to meet the Commissioners, and investigate the condition of things there, I went, and like many others who have gone there, had my eyes opened, and opened so thoroughly that I cannot shut them close enough to exclude the scenes that I there witnessed, day nor

night. I shall not attempt to depict the wretchedness of the people in that unfortunate dairy district where this terrific scourge has wrought its legitimate effects; and I wish that every man who is active in this matter, that every man here, might go to Brookfield, and enter one of the farm-houses——

Mr. WENTWORTH.—Mr. Chairman, this Committee have met here to hear evidence, and not to hear speeches. Our time is precious, the time of the State is precious, and our expenses are large. I think, if the gentleman has any testimony to give to the Committee, that is one thing, but I do not think it comes within the line of our duty to meet here and hear addresses to the Committee and the audience indiscriminately. If the gentleman has any facts or opinions to give in reference to the subject under consideration, it would be very proper for us to hear them; but to hold a meeting for lectures is something that I think we have no right to do. I should be glad to hear any opinions or facts the gentleman may have to offer, but beyond that, I think we have no right to go.

Prof. CLARK.—The practical question is, whether we shall allow the disease to take its own course, or extirpate it. The Commissioners have acted with great energy and discretion, and have the confidence of the public, especially of that portion of the public among whom they have acted, who are thoroughly acquainted with the manner in which they have discharged their duty. But an opposition has been manifested by some—and by the very men who ought to have been leaders in this work of extirpation—that has been deeply felt by the Commission, and been a source of very great anxiety to them. I had supposed that sound sense and true philosophy were the characteristics of the learned physicians of Massachusetts, but I must confess that my faith has been sadly shaken, by the course they have taken in regard to this disease.

Mr. ELDRIDGE.—It seems to me that this is not the legitimate course for the Committee to pursue. If the gentleman is here for the purpose of stating certain facts, that is one thing. If he has been put upon the stand by the Commissioners as a witness, let them examine him; but, as Chairman of the Committee on the part of the House, I am not disposed to allow this course to be pursued any longer.

Mr. LATHROP.—The Commissioners do not propose to make any examinations at all.

Mr. ELDRIDGE. It seems to me, if the Commissioners have no questions to ask, and the Committee have no questions to ask, the gentleman is not wanted.

Mr. WHITING.—Was he summoned as a witness?

The CHAIRMAN.—Yes.

Q.—Do you know any thing about the identity of this disease with that known in Europe and Africa as pleuro-pneumonia?

A.—No, Sir.

The CHAIRMAN.—Has the witness any suggestions to make as to the best way of extirpating the disease, which he wishes to state to the Committee?

A.—Yes, Sir. I have the belief, in the first place,—and I think that is the thing to be settled in the beginning,—that the disease is contagious, and, therefore, I believe it is possible to do something; and the thing to be done, in my judgment, is to carry out the provisions of the Act that was passed—namely, to extirpate the disease. If there is any thing to be done, it is extirpation. It is not to find a method of prevention, not to find a remedy, but it is to get rid of the whole thing; and if I knew a remedy that would cure three-quarters of the diseased animals in the State, I would not, for a thousand dollars, state the fact, for it would only paralyze the efforts of those who are now trying to get it out of the country. The true method of procedure is clear enough—it may not be to every one, but it is to me. Every animal that is sick, or has been sick, should be killed; and if I had it in my power, there should not be an animal of that description alive twenty-four hours from this time. There are many points with regard to this disease which are entirely unsettled. No man can tell what exposures consist in, how long the seeds of the disease remain in the system, whether an animal will ever get well, or whether the disease is hereditary or not. With all these doubtful points, it seems to me that all the action of the Committee should be based upon one fact, namely, that if you kill the diseased cattle, and bury them five feet under ground, they will not give it to others. No man knows any thing else that will stop the infection, therefore I would have them all killed—those, on the one hand,

that have been diseased, and, on the other, those that have been exposed. In cases of certain exposure, I fully believe that the cheapest and surest way of escaping danger is to kill them. But when it is not known certainly that they have been exposed, I would have every suspected animal taken and placed under guard, perfectly isolated from all other animals, and very carefully watched, and as fast as any symptoms of the disease are manifested, I would have the animals killed. It seems to me that it would be for the interest of the State of Massachusetts, alone, to have all the cattle between the Connecticut River and Massachusetts Bay killed, and put under ground, rather than have the disease go West.

Q.—I understand you to say that no one can tell that this disease is not hereditary. If it is hereditary, how are you going to secure the country by slaughtering the animals?

A.—We are in doubt in regard to all these matters, and the only thing is to do what we know how to do, according to our ability; and if, having done our best, the thing gets the upper-hand of us, we must surrender at discretion.

Mr. Bird.—I would like to inquire whether Prof. Clark has had a medical education?

A.—Not a very perfect one; I do not profess to be a doctor. I inherited it, somewhat.

Q.—"The seventh son of a seventh son?"

A.—About that, Sir.

Q.—That is all there is of it?

A.—Yes, Sir.

Dr. Loring.—Something has been said here about the feeling of the Commissioners in reference to the opposition which has been manifested to their course. I want to say, that the Commissioners did not expect to go on with their work without opposition, and they feel that medical and professional gentlemen have a perfect right to call their conduct in question. We have no doubt that it has been done honestly, and with the firm conviction that they were doing the best they could for the interest of the Commonwealth. While we have our own opinions, which we have freely expressed, we are anxious that all those who differ from us should express their opinions before the Committee, and should have a fair opportunity to say what

they see fit in this case. The other members of the Commission join with me in saying, that we do this without the slightest feeling, and that we never have entertained the slightest feeling in regard to those gentlemen who have differed from us in opinion.

Dr. Loring then gave a description of the calf killed at Lexington on Saturday. He said: We were very particular to have the calf killed by a butcher, in the usual way, in order that it might be thoroughly bled, as some question has arisen in regard to what is the condition of the lungs growing out of imperfect bleeding. The lung presented the usual appearances found in recent cases of the disease, so far as the experience of the Commissioners goes. There were upon the external surface numerous dark, livid spots. Those spots, when cut into, were found to extend into the substance of the lung, from a line to perhaps an eighth of an inch. In the substance of the lung, especially in the lower lobe, we found portions of lung, about as large as a walnut, which were, to a certain extent, broken down or disintegrated. There were dark, livid spots of hepatization upon it, and so easily broken, that the substance came all to pieces in the attempt to scrape off the blood with a knife. It was very evidently, to use a common term, on the verge of rottenness.

Q.—Did you ever try to wash off the blood?

A.—No, Sir. We tried a portion of the lung, evidently healthy, in which the small vessels contained blood, and from this the knife would very readily remove every appearance of blood, without the substance giving way under it.

The reason why we were called upon to examine this calf was this: the mother of the calf had been exposed, and within two days, Mr. Wellington stated that the calf had exhibited some symptoms—difficulty and thickness of breathing—and he suspected him to be diseased. Previous to the autopsy, the examination from physical signs, so called, presented what is called bronchial respiration—that is, the sound of the passage of the air through the bronchial tubes is transmitted by solid substance to the ribs, whereas, in a healthy condition of the lungs, there is a respiratory murmur. There was a slight dulness of sound.

Q.—Do you think that the calf had this disease.

A.—I do. If desired by the Committee, I will read a very interesting account of the cow killed on Saturday by Dr. Bates of Worcester, at Holden.

[The Committee signified a desire to hear the account at a future time.]

Q.—Might not this have been an ordinary case of pleuro-pneumonia, such as is not uncommon here in the country.

A.—It would not have been in a human subject an ordinary case of pleuro-pneumonia.

Q.—Might not the disease have been brought on by the calf being kept in a damp place?

A.—If it had been an ordinary case of pleuro-pneumonia, it might have been brought on in that way; but it did not present that pathological appearance which is presented in the human subject after so short an attack of pleuro-pneumonia.

Q.—How old was the animal?

A.—Four months.

Q.—How long had the calf been affected?

A.—He said not more than two days.

Q.—Did you examine the mother?

A.—Yes; we examined the mother by auscultation and percussion, and we endeavored to ascertain the physical signs.

Q.—What did you discover?

A.—There were certain appearances about the cow which would be interesting to a scientific man. Mr. Lathrop, who is a very acute observer, has noticed that the hair of diseased animals is very apt to stare and turn the wrong way. This cow presented that appearance, and upon examination, this bronchial respiration was indicated, and over a portion of the right lung, I think, there was dulness on percussion.

Q.—Do you mean you think there was dulness, or that it was over the right lung?

A.—I think it was over the right lung.

Q.—Do you think this disease affects the blood or the lungs first?

A.—That is rather a nice question to answer.

Q.—A pretty important one?

A.—I don't think so, because it is not yet settled whether disease attacks fluids or solids. It was an old mooted question when I was a student, and I suppose it is now. I remember

that, when I was a student, my votes were given entirely in favor of the theory that fluids and not solids were attacked.

Q.—How is it since you have grown up?

A.—I have quit practice and gone to farming; but I have never modified my opinion since.

Q.—If this disease attacks the blood first, might not something be administered to the cattle that had been exposed, to prevent its showing itself, and drive it off?

A.—I am free to confess that I agree entirely with Dr. Holmes in his views with regard to specific medicines. I doubt if any remedy could be applied which would be of any special service.

Q.—Might you not get something that would affect the blood and reach this disease?

A.—There are many medicines administered that unquestionably affect the blood; at any rate, they affect the vital condition of the animal or man.

Q.—Have any of these been tried, to your knowledge?

A.—I have received, I suppose, twenty letters containing accounts of specifics for this disease, but they were of such a nature that they evidently were not worthy of any attention. They were quack medicines, unquestionably. And when I learned, from the testimony that Dr. Simonds gave before the British Parliament, that the disease was incurable, I took it for granted that he said so for good reasons.

Q.—You say, that in your opinion, those medicines were quack medicines. Have you had any evidence that they were quack medicines?

A.—If the Committee should care to read the letters, they would agree with me. One recommends that iron should be given in large doses, another, that salt and soda, another that salt should be injected into the nostrils—and injections of all sorts and kinds.

Mr. BIRD.—Are not these very much the same kind of medicines that were tried in the cholera by the regular faculty?

A.—Yes, Sir; I think the cholera rather beat the faculty.

Mr. BIRD.—I think this disease rather baffles them.

WITNESS.—I think so. In regard to this calf, I think she might have taken the disease congenitally—that is, in the mother.

Q.—Is it not a very dangerous thing for a medical man to say that a disease called infectious and contagious will be infallibly communicated to all that come into connection with it?

A.—Yes. There is a susceptibility—I think no one has doubted that.

Q.—Whether the probability of communication depends on the condition of the subject, or upon the intensity of the morbific influence, or the virus, or whatever it may be called?

A.—That is a very hard question to settle.

Q.—Is not that a question necessary to be settled, in order to determine whether to kill the diseased cattle, or subject them to isolation?

A.—I do not think it would bear upon that point at all. You cannot tell how far susceptibility extends.

Q.—Unless you know in advance with absolute, or at least reasonable certainty, that a given percentage of an exposed herd must die, why kill?

A.—Upon the same principle, that if I had a house with the floors of pine, the staircases of oak, and the doors of mahogany, and it should take fire, I should not wait to see which wood would burn first. I would put out the fire if I could, by pulling down an ell.

Q.—Do you think some cattle are not susceptible to this disease?

A.—I think that is pretty well established.

Q.—What proportion?

A.—The percentage has never been fixed.

Q.—Are there any marks by which you can tell which are and which are not liable?

A.—No, Sir; no more than there are in human beings, marks by which you can tell who are liable to the smallpox, and who not.

Q.—Then there is no way of settling the question, to be of practical value?

A.—No, Sir.

Q.—Would not certain diatetic treatment or certain physiological conditions affect the susceptibility of the animal?

A.—That I do not know.

Q.—You think that whether an animal is kept on poor food or proper food makes no difference?

A.—I should not think that was a physiological condition; I should think it was sanitary.

Q.—Is not the susceptibility to disease controlled by external causes,—by the diet and habits of the animal?

A.—I think an animal in ill health would be more likely to take the disease. I have no doubt that certain atmospheric influences would affect it, and yet some animals escape under all circumstances.

Q.—It comes to this, then, that there is not much known about it?

A.—Yes, Sir.

Q.—Have the Commissioners kept a record of the autopsies that have been made?

A.—Yes, Sir; they have kept a record of all their proceedings. I have it at home, and the Committee can have it at any time.

Dr. Loring here read the communication of Doctor Bates, in reference to the animal killed at Holden on Saturday, before alluded to, as follows:—

WORCESTER, June 3, 1860.

Messrs. WALKER, LATHROP, and LORING, *Commissioners, &c.*

Gentlemen,—At the solicitation of William S. Lincoln, Esq., I this day accompanied him to Holden, Mass., and proceeded to the examination of several cattle, which have been exposed to "pleuro-pneumonia epizootic," by contact with the Dike cow, which animal was killed and examined by Dr. Thayer and John Brooks, Esq.

One cow, owned by Lyman Rice, exposed May 14, twenty-one days since, presenting apparently healthy conditions, was examined, and presented dulness over region of right lung, tenderness of spine, slight cough, respirations eighty per minute, heart's action much accelerated, had coughed several days, feeding in pasture. Examination after death revealed a pint of effused serum in cavity of right chest, right lung inflamed over quite the entire surface of the pleura, with slight attachments confined to nearly the whole circumference of lung. Near the superior posterior portion of lung was presented a portion of lung the size of a dollar, hardened in texture, and changed in color, and presenting unequivocal indications of true disease. The internal structure

thickened, and presenting rapid progress of inflammation. Bronchial tubes in contact with disease, slightly inflamed, and covered or lined with tenacious mucus.

The right lung weighed five pounds six ounces. The left lung presented healthy condition, and weighed four pounds four ounces.

Heart presented the softened and flacid condition, common in the disease, the left auricle and ventricle hypertrophied or enlarged, and weighed seven pounds.

Several minute black objects were discovered in the right lung, within the texture of the lung, and diseased portions similar to those discovered in animals at Brookfield, and two were removed from the surface of the lung directly beneath the pleura, and discoverable through the pleura, which, on examination with the microscope, presented the appearances of a segment of metalic leaf, dark ground, and metalic appearance of iron pyrites. Specimens preserved.

I was extremely gratified with the results of examination, as they afforded undeniable proof of correct diagnosis in early stage of disease and process of development.

Respectfully yours,

J. N. Bates.

Q.—Some gentlemen of the Committee would like to know the expenses of the Commission. Can you tell us what those expenses are, independent of the $20,400?

A.—No, Sir. We have not got the bills of the persons we necessarily employed to dig the holes for the cattle, and certain horse hire, but suppose that $25,000 would include all.

Q.—Including the compensation of the Commissioners?

A.—That is my impression, and Mr. Lathrop agrees with me. I was informed in the outset, by the governor, that our compensation would be very small. Whether he considered it a labor of love, or not, I do not know.

Q.—Are there any herds of cattle still under the direction of the Commissioners?

A.—Yes, Sir; they are waiting the action of the legislature.

Q.—At what expense?

A.—There are very few under any expense. There is one pasture in which there are forty head of cattle, for which the Commissioners agreed to pay a small amount for pasturage. I think that is all.

Q.—I would like to have you state, as near as you can, the extent of territory over which those thousand head, which the Commissioners have ascertained to have been exposed, extend; whether a line cannot be run round it; whether it is confined to one county, or how?

A.—I think there are about ten or twelve miles square, in the centre of the county of Worcester, in and about North Brookfield, in which the geographical limit can be ascertained definitely. Outside of that there are narrower bounds. I should suppose that a very small circuit about Hubbardston, perhaps not more than half a mile, would cover that. I consider the question in Pelham settled.

Q.—In your judgment, would it be judicious and wise for the legislature to define a line, and pass an enactment prohibiting ingress and egress, for the time being, of cattle over that line, embracing this infected district, on the principle of isolation?

A.—Yes, Sir, I think it might; but I would not say that legislation should stop there. A line might be defined, but what law should be passed would be best left to the Commissioners, or whoever has charge.

Q.—Do you know any thing about the statements, that exposed cattle have been driven to Northfield, Wendell and Amherst?

A.—We have had no authentic accounts of diseased cattle in Amherst. One animal was killed, but on examination, the disease did not appear to be pleuro-pneumonia, exudative and contagious. I understand that about three hundred head of cattle have been exposed in Northfield, if this disease was pleuro-pneumonia.

Mr. Fisher.—It is stated that Mr. Wellington said, that his calves went up to New Hampshire with one hundred and fifty others, and that these were scattered about in various localities.

A.—I am perfectly aware that there is a good deal of excitement about this matter, very naturally, in various sections of the New England States. I think that the utmost care, therefore, should be taken in investigating the history of the exposure, at the outset, in all cases brought to the notice of the Committee, the Legislature, or the Commission.

Q.—How long since there have been any deaths in what you call the infected districts?

A.—I do not remember.

Mr. Lathrop.—The last I knew was the 11th or 12th of April.

Evidence of William S. Lincoln.

Mr. William S. Lincoln, of Worcester, was then called, and asked to state his views as to what should be done in view of the nature and ravages of this disease. He said: From the opportunities I have had of judging of the disease in Brookfield, and other opportunities I have had of reading accounts of the disease in Europe, I should, in cases of well-ascertained exposure, kill.

Q.—You would not expect any advantage from attempting to cure them, on the whole?

A.—I cannot say that I should except any, from the personal experience I have had of it, and I certainly could not from the reading which I have had upon the subject. I will state one fact, if you will allow me to do so. I was at Holden yesterday. The cow that was killed there was a case of exposure to the cow that was driven to Pepperell. There are about fifty animals there, within a small circuit, that have been exposed to these two animals, both of which are now dead. Whether it has extended beyond Holden into Worcester is a matter about which we are all in the dark. Two oxen, which have been exposed to these cows, it is now promised, shall not go into Worcester, but they have been in, since this exposure, almost every day, passing, of course, more or less animals on the way, and standing in our wood marts.

Q.—Did you say both were killed?

A.—One was killed on Friday, and the other on Saturday. There is a third cow, which is shut up.

Q.—What do you mean by "well ascertained exposure?"

A.—Such cases as that of the cow which is termed in Holden, "the Pepperell cow," which was driven from North Brookfield to Pepperell, and the case of the Dike cow.

Q.—Do you mean that they must have been in the same building and the same herd with a diseased animal?

A.—Not necessarily so; but where there has been any communication; as, for instance, in the case of the Dike cow, where it was known that she put her nose over the boards of the fence, and communicated with the cattle on the other side.

Q.—What makes a case of exposure?

A.—I suppose it would depend upon the air and the wind, somewhat. I suppose the disease would not be communicated to animals against the wind as far as with it; nor, probably, so far in a bright, fair day, as in a heavy day.

Q.—Do you know to what extent, if any, cattle owners, whose animals have been diseased, have sold cattle out of the town?

A.—I have no knowledge. The Pepperell cow went from North Brookfield, and there have been other cases from North Brookfield, but I have not been able to ascertain any case where a cow, suspected of having been exposed, has been sold, except from that town.

Q.—Are you perfectly acquainted with the diseases of cattle?

A.—Well, I have that general acquaintance which a man has who follows farming, and is in daily communication with his cattle—nothing more.

Q.—Have you been with the Commissioners?

A.—I have been in Brookfield somewhere from ten days to a fortnight.

Q.—In the employ of the Commissioners?

A.—Not at all. I happen to have the honor of holding the office of President of the Agricultural Society in our county, and I went up at the request of a number of the trustees, (they having had some doubts about the disease,) for the purpose of satisfying myself, as much as any thing, Sir. I should like to say that the Hon. Mr. Brooks and myself were instructed by the Worcester Society, at its meeting, to express to the Committee of the legislature the opinion of the Society as to the means which should be adopted to remove this disease, which, in the opinion of those who had observed it, and in the unanimous opinion of the Society, was that of extirpation, which the Commission have followed.

Q.—I would like to inquire whether you believe there would be any opposition in Worcester County if the Commission should be empowered to encircle the county, and prevent all egress

or ingress of cattle? Do you believe the people would heartily coincide?

A.—I have no doubt of it. I can state one fact, which I know from communication with the officers of towns, that in some towns the inhabitants have taken that responsibility, without authority of law, and the vote of the town is uniformly respected.

Q.—Do you think that any claims for damages would be presented?

A.—I can only speak for myself. I should not.

Mr. Bird.—If you believed the disease was neither contagious nor infectious, and there was no necessity for such a course, and you were subjected to a loss of a thousand dollars, should you not think you were entitled to damages?

A.—I do not know what I should think in a case which does not exist. I believe it to be contagious, and therefore cannot answer what I should do if I held to a different opinion.

Winthrop W. Chenery.—(Recalled.)

Q.—Did all the four cattle imported by you die?

A.—No, Sir.

Q.—How many are alive now?

A.—One.

Q.—The first two you thought died from injuries?

A.—Yes, Sir.

Q.—When did the third die?

A.—On the 29th of June last.

Q.—And what day did they arrive there?

A.—The 23d of May.

Q.—When did you observe the first symptom of disease in other cattle than those imported at that time?

A.—It was about the first week in August, I should judge.

Q.—When did the first one die?

A.—On the 20th of August. This was a cow imported in 1852.

Q.—When those calves were sold to go to North Brookfield, did you suspect the existence of disease in them?

A.—No, Sir.

Q.—How many of them have died?

A.—I understand they are all dead; but I don't know the fact of my own knowledge.

Q.—After those imported cattle arrived, how were they kept?

A.—They were kept in a pasture.

Q.—But those three that died—were they kept in a pasture, or in a barn?

A.—In a barn.

Q.—With the other cattle?

A.—No, Sir.

Q.—Is your herd now under the control of the Commissioners?

A.—I do not know. We take care of them. I understand we are not allowed to make use of them. We feed them. I don't know who will pay for it.

Q.—Any bargain made about it?

A.—No, Sir.

Q.—Do you consider that you have any claim upon any body?

Witness.—Do you mean for keeping the cattle?

Mr. Bird.—For every thing.

A.—Twelve or fifteen thousand dollars—fifteen, I should think.

Q.—You think you are equitably entitled to that amount?

A.—Yes, Sir.

Q.—And that is for what?

A.—For the destruction of the value of the property, and the keeping.

Q.—You are keeping the cattle separated, under instructions from the Commissioners?

A.—Yes, Sir.

A Member.—I would like to inquire whether your cattle ran in a pasture adjoining the fields of your neighbors, who had stock with which they might communicate? And were not some of your cattle taken sick, and taken out of the pasture as soon as known to be sick?

A.—There was, one.

Q.—On what do you rest your claim?

A.—I had a valuable lot of cattle on the 15th of April; on the 16th, the State came there, and reported a large number of them sick, thus destroying their value. Their value is for breeding purposes, not for feeding.

Q.—You did not consider them sick?

A.—No, Sir; only a few of them.

Q.—But has it not turned out that they were sick?

A.—No, Sir.

Q.—They are improving, are they?

A.—They appear well, very well.

Q.—Do you say that any appraisal was made of those cattle, either before or after they were killed?

A.—I am not now speaking of those that were killed.

Q.—What is your opinion of the ox that went after the hay, in regard to his communicating the disease to the cow and calves?

A.—If Mr. Wellington's cattle had the disease, I have no doubt that they obtained it in that way.

Q.—Still, the ox looked healthy?

A.—Yes, Sir.

Q.—If that is so, is there not a strict propriety in the Commissioners enjoining you, and keeping your cattle thus inclosed?

A.—I think so.

Q.—Would you think yourself entitled to damages; for injury to the value of your cattle, when, in point of fact, they could communicate the pleuro-pneumonia to other cattle?

A.—I have no doubt that the State should take my cattle, if the public good requires it, but I believe it should pay me for them.

A.—Did the confinement of those cattle that are now living, merely, and the reporting that they are diseased, so injure their value, that you consider yourself entitled to damages?

A.—I suppose that there may be two or three animals in that state, but the most valuable ones the Commissioners themselves do not call diseased.

Q.—Did the Commissioners, when they went to your place in April, propose killing your cattle that were still living?

A.—I do not know what their views were.

Q.—Would the mere fact of the Commissioners confining them affect their value, if the public believed they were sick?

A.—The public did not believe it, and do not believe it now. I consider that their value was injured by reporting them diseased, when they were not in the least damaged.

Q.—What was the object of the Commissioners in their first visit?

A.—I suppose they went there to examine the cattle, and satisfy themselves in regard to the disease that was supposed to be there at the time.

Q.—There was a suspicion in the minds of the community that the disease was there previous to the visit of the Commissioners, was there not?

A.—Undoubtedly, in some places; but I don't know about its being general.

Q.—The Commissioners went there under a strong impression that the disease existed there?

A.—I suppose they did.

Q.—Do you conceive that their visit there damaged you to the amount of ten or fifteen thousand dollars?

A.—I do, decidedly.

Q.—Do you look upon your cattle as being any more extensively diseased after the visit of the Commissioners than before?

A.—No, Sir; but I believe that it would be hard work to get the community to buy any of them, or to use them for any purpose.

Q.—Do you think that, after the history of the calves that went from Belmont to North Brookfield, had been published all over the State, and even throughout New England, the people would be very anxious to purchase such animals?

A.—No, Sir.

Q.—You don't think any farmers or cattle-breeders would be anxious, after the reputation that those calves had given to the herd, to have any cattle from it?

A.—No, Sir; not if it was thought that the disease still existed there.

Q.—Do you remember the condition of the two or three animals that were sick when the Commissioners visited you?

A.—Yes, Sir. The Durham cow had been slightly sick, and showed traces of disease, but, in the opinion of the physicians present, it would recover. The Ayrshire heifer was slightly diseased, but, in the opinion of the physicians, would recover.

The third was a dark Devon heifer, born two months before her time, with a crooked leg, swollen at the knee, which I understood to be the reason that she was selected for slaughter, to ascertain whether any connection existed between this pleuro-pneumonia and this swelling of the knees. She was killed, and proved to be badly diseased.

Q.—Was any animal killed there which, in the opinion of any intelligent witness, was considered past cure?

A.—I heard no such opinion expressed, though the presumption is that the last one would have died.

Q.—You think so yourself?

A.—Yes, Sir, I should think so.

Q.—Are there any examples in your herd now that have such strong traces of the disease that you, as a cattle-breeder, would consider them worthless?

A.—Yes, Sir.

Q.—Do you know many?

A.—I have but two in my mind. There may be others.

Q.—Should you think the animal which was killed on Saturday would have been a good animal to breed from?

A.—No, Sir; it would not.

Q.—Should you think that the small black heifer that had been treated with hydriotate of potassa, that had swollen knees was a good animal to breed from?

A.—No, Sir—independent of the disease.

Q.—Had the two heifers killed by order of the Commissioners shown decided marks of the disease before they were slaughtered?

A.—No, Sir; very slight. I noticed a cough occasionally.

Q.—Would you, at the time of the Commissioners' visit, have sold any of your cattle for breeders at any price?

A.—No, Sir; not at that time.

Q.—Why not?

A.—On account of the suspicion attached to them; and aside from that, I thought there was a possibility of imparting it.

Q.—I understood you to say that it would be hard work for you to dispose of any of your herd, in consequence of the report which the Commissioners have made. Don't you think it ought to be hard work to dispose of cattle under those cir-

cumstances ? Should you be willing to buy cattle, at any price, situated as your animals are ? Should you be willing to propagate from cattle under such circumstances ?

A.—I don't know as I understand what you mean.

Q.—I understand that you keep your cattle for breeding purposes ; that that was their main value, and that you claim damages on the ground that the report of the Commissioners made it impossible for you to derive any benefit from them as breeders. Now the question is, whether you would think it right to propagate cattle under such circumstances ?

A.—If you mean, whether it would be proper to propagate animals for sale from diseased animals, I should not. I will say, to illustrate, that I have a bull, considered perfectly well, for all purposes, never been diseased, never likely to be, that brought me in an income of five hundred dollars a year, which would have undoubtedly increased from year to year. This report destroyed his value, as well as that of half a dozen others that I have coming on—none of them diseased. Of course, I do not expect much patronage for that bull, under those circumstances, which I should have had, had it not been for the fact that the Commissioners went there and made this trouble. I am not complaining that the Commissioners did go there ; I think it was all right ; but I think I should be paid.

Q.—Do you think the visit of the Commissioners has damaged the value of that bull ?

A.—Decidedly, Sir.

Q.—Do you think the visit of the Commissioners or the existence of the disease diminished the value of the bull ?

A.—The visit of the Commissioners.

Q.—Had there been no visit of the Commissioners, do you think your bull would have been as profitable this summer as last summer ?

A.—Yes, provided I had allowed him to serve.

Q.—Would you have allowed him to serve ?

A.—No, Sir ; not this summer.

Q.—Would you, while the suspicion of the disease, under such circumstances, existed in your herd ?

A.—No, Sir.

Q.—Have you any doubt that the whole trouble in Brookfield from the disease among the cattle emanated from your herd?

A.—No, Sir. I have no doubt of it.

Q.—What is the fact in regard to the mortality of imported cattle in the process of acclimation?

A.—I have not lost any before.

Q.—How many have you imported?

A.—I don't know. About a dozen all together.

The CHAIRMAN here stated that the case, on the part of the Committee, was considered through, and Hon. FRANCIS W. BIRD, of Walpole, addressed the Committee in behalf of the remonstrants. He said:—

Mr. Chairman: In opening the case for the remonstrants, or so far as I represent the remonstrants, let me say that we are entirely satisfied with the case as it now stands. I think, however, we are entitled to some information from these Commissioners upon certain points as to the expense of the Commission. We get from the Report of the Commissioners very meagre details as to the costs of the Commission and how they have been incurred. When I had a little something to do with affairs at the State House, we never allowed a bill of any kind to be paid—a bill could not pass the Council Chamber—unless it included all the items of expenditure—the cost of blacking boots at Holmes' Hole, and the cost of being shaved at Edgartown: and it seems to me that we ought to have had something more in regard to their expenses thus far. I would like to know the cost of the Commission—the sum paid for cattle killed, specifying whose they were, where they were, and what they were; and as they report that nothing was paid for diseased cattle, the number of such cattle killed and their probable value, in case the party should bring a claim against the State; for I have no doubt that if they had taken my cattle for public uses, whether diseased or not, I should make them pay for it, if there was any law to do it. I want to know, also, the cost of the present measures which have been adopted for isolating the cattle kept under restraint; there must be a good deal of cost attending that. After this panic is over, and these

parties have become satisfied,—as it seems to me they must become satisfied,—that it is mainly the result of a foolish and insane excitement, and that they have consented to incur these expenses under its influence, I have no doubt they will come in and demand damages from somebody,—the Commissioners or the Commonwealth,—and they are entitled to them; and some sort of an estimate ought to be made on this point. These are items on which, as it seems to me, the Commissioners should furnish fuller information to the Committee and the community, than we have yet; and, as a tax-payer, I object to paying my proportion of the taxes, without seeing something more about it. Then, I hope the Commissioners will be required to furnish a report of the examinations they made of diseased cattle. We have had nothing but oral statements, and those only in very rare cases made by the parties who make the examinations. I have tried to ascertain how many cases of autopsy individual examiners would state they made, and, as you all remember, the answers have been very loose and unsatisfactory.

We need, at the same time, more accurate information as to the character of the disease, before you can report to the legislature any plan as to its treatment hereafter. If the disease is positively and strictly contagious, then it would seem as though the Commissioners might, to some extent, be justified in their treatment by killing.

I will read, Mr. Chairman, the Remonstrance under which I appear.

To the Legislature of Massachusetts:—

The undersigned, tax-payers in this State, respectfully remonstrate against any appropriation of money for the purpose of staying the spread of the (so-called) cattle disease.

1st. Because it is not proved that said disease is either contagious or infectious; and unless the disease has one or both of these characteristics, all attempts to arrest its progress by destroying the cattle are worse than useless.

2d. Because the legislation authorizing the killing of the cattle is a departure from the legitimate province of legislation, all experience agreeing to show that the remedy of an evil like this is more economically and more surely secured when left to intelligent individual interests than by governmental interference.

3d. Because, of the cattle which have had the disease, or have been exposed to contagion or infection, and have not been killed, the proportion of deaths has been less than that of cases of clearly developed disease in those which have been slaughtered, thus proving either the shallowness of the Commissioners' diagnosis, or the impossibility of a reliable one, and proving especially that it is safer as well as cheaper to give cattle a chance for life, than to kill them.

4th. Because the legislature has no right to authorize the destruction of private property, except as a public or common nuisance; and for these contingencies, existing laws, deliberately passed, and carefully guarding personal rights, adequately provide, and because our Bill of Rights guaranties that "the property of any individual shall be appropriated to public uses" only "when the public exigencies require it," and then "he shall receive reasonable compensation therefor."

5th. Because the dogmatic assumptions of the contagious or infectious character of the disease, tend only to create and increase a panic, which inflicts greater injury upon the property and industry of the community than would reasonably be feared from the disease itself, and because there is every reason to believe that the healthful feed and genial weather of summer will do more to check the disease than the empiric's nostrums or the butcher's knife.

Your memorialists therefore respectfully pray, that no more money be applied to any such quixotic and mischievous purpose, and that the Act which has created or found such sanguinary executioners, be repealed.

And as in duty bound will ever pray.

F. W. Bird.
Andrew Bird.
Horatio N. Godbold.
D. F. Grover.
G. C. Park.
George Cox.
Henry E. Achorn.
Jabez Sumner, Jr.
T. W. Kennedy.
G. W. Johnson.
Martin Cashin.
Sam'l Bird.
William S. Johnson.
James Smith.
J. N. Fisher.
Lemuel Allen.
J. G. Hartshorn.
Chester Morse.
Wm. Kingsbury.

Walpole, May 28th, 1860.

Mr. Bird also presented the following Memorial from the Massachusetts Medical Society:—

At the Annual Meeting of the Massachusetts Medical Society, held on Wednesday, May 30, 1860, the following preamble and resolution were unanimously adopted.

Whereas, a disease, the nature of which is not well understood, and the treatment of which has been to a remarkable degree unsuccessful, is now prevailing among the cattle in this Commonwealth; and whereas, the legislature is convened this day to consider this specific object;

Therefore, Resolved, That a committee of nine from the Massachusetts Medical Society be appointed by the Chair, to urge upon the legislature the establishment of a Scientific Commission to investigate said disease.

A true copy. Attest: JOHN B. ALLEY,
Recording Secretary.

The Chair appointed on this Committee:—

Drs. Jacob Bigelow, *Boston.*
George Hayward, *Boston.*
Henry I. Bowditch, *Boston.*
John B. S. Jackson, *Boston.*
Oramel Martin, *Worcester.*
John C. Bartlett, *Chelmsford.*
Johnson Gardner, *Pawtucket.*
Calvin P. Fiske, *Sturbridge.*
John G. Metcalf, *Mendon.*

Mr. Bird then stated, that he had that morning received a note from Dr. Henry I. Bowditch, (who had been summoned as a witness,) stating that he had been suddenly called away to New Haven, upon an important professional case, and should not be able to attend.

Testimony of Dr. Charles M. Wood, of Boston.

Mr. BIRD.—Did you accompany the Commissioners to North Brookfield?

A.—I did not.

Q.—What is your connection with this case, Sir?

A.—My first visit to North Brookfield was with the selectmen of the town. I also saw Mr. Chenery's cattle and examined all the herd on the 26th of October last.

Q.—What was the condition of that herd, at that time?

A.—I was then accompanied by Dr. Saunders, of Salem. I introduced myself to Mr. Chenery at Mr. Hale's stable in Sudbury Street, and told him that I understood he had lost several cattle by a disease not then understood. He told me he had

lost several cattle, and had one animal very sick, which he would like to have any person see, who could do any thing for her. I visited the herd the next day, and saw an ox that had been sick some days with the disease. He was lying in a small pen in the south-east corner of the barn. I examined him very carefully, as I thought. He was much emaciated, his hair stared, and he was groaning somewhat when lying, and when urged to rise, he made several efforts, accompanied by loud grunts. His symptoms were labored respiration, accompanied, in inspiration, with groaning, and in expiration, with grunts. I told Mr. Chenery that there was no possible chance of his recovery, and he was immediately led out into the corner of the field, and knocked on the head, and, assisted by Dr. Saunders, I made a *post mortem* examination. I found the left lung almost black, quite solid, and about three times its natural size, with a yellow, tough substance intervening between the pleura-pulmonaris and the pleura-costalis, about an inch in thickness, with some slight infusion in the cavity of the chest. The opposite lung was also much diseased, but not so extensively. The liver had lost its natural color, and appeared a disintegrated and degenerated mass, giving evidence of long-standing disease. There were some adhesions, to be sure, to the right lung, but not so extensive as to the left. I have the minutes of the autopsy, but they are not with me; and from memory I may not, perhaps, state the matter with perfect correctness.

Having never seen a case of what is termed pleuro-pneumonia, I was at a loss to understand what the disease could be, and, for further experience in the matter, I stated to Mr. Chenery that if any other case should happen, I would be happy to have him let me know, as I should be willing to visit the place and treat the case without putting him to any unnecessary expense in the matter. He called on me early in November, and said that he had two animals sick, one taken that morning, the other the day previous. It being a day when Dr. Saunders was in the city, I mentioned the matter to him, and he agreed to accompany me. Dr. Thayer was also there. We found the four-months-old Ayrshire calf, and the three-year-old Devon heifer, sick. The symptoms of pleurisy in the four-months-old calf were well marked; they were also pretty

well marked in the three-year-old heifer. There was a difference of opinion between my friend Thayer and myself in regard to the heifer. He believed there was some pneumonia in her, I did not. I attended them from day to day, for some ten days, with very active treatment, and they apparently recovered.

Q.—What was the treatment?

A.—Counter-irritation, with tonics and stimulants. It being a case of torpidity and debility, I thought that the treatment indicated. After some ten or twelve days, the appetite returned, rumination was restored, their coats looked well, and they apparently recovered. The three-year old heifer having a malignant disease of the eyes, I suggested to Mr. Chenery to have her destroyed, to which he readily consented. I agreed to notify my friends, Doctors Saunders and Thayer, and to go out the first fair day. However, on the second day of December, Mr. Chenery came into town and requested me to go out and see the mammoth cow, as she was very sick. I should have said, on our visit two or three days previous, there were some slight symptoms of sickness on her part, but as it was just about the time of the cow's calving, I said I thought it was owing to parturition. She calved Wednesday night; on Thursday they gave her sulphate of magnesia, some thoroughwort tea, and some stimulant—gin, I believe. Friday morning, Mr. Chenery told me that the cow was very sick, and wished me to go out, and take Dr. Saunders with me. I did so, and pronounced her laboring under a chronic disease of the lungs; acute symptoms, however, had supervened on that condition. I treated her, from time to time, by counter-irritation, such as setons and packing the dewlap, and with remedies,—tonics and stimulants,—for about three weeks. About that time diarrhœa set in, when I did not wish to give her medicine, and I requested that one pint of domestic flour and half a pint of oatmeal should be mixed for her, and after that she became costive, and needed laxatives during her life, which lasted two weeks longer. She died, if I mistake not, on the morning of the ninth of January. Some seven or eight hours after, I, with Doctors Saunders and Thayer, made an autopsy of her, and found the lungs extensively diseased, and very much enlarged. We supposed them to weigh about sixty pounds. Their usual weight, in a healthy condition, would not have exceeded ten pounds, and it was an immensely

large cow, even at that. They were enlarged sufficiently to fill both cavities of the chest; they were consolidated, and adhered to the anterior and superior portion of the chest by a small band, some two or three inches in width, and eight or ten in length, and very firmly, so much so that it required the strength of a pretty strong man to remove them, and break up the adhesion. The liver was somewhat diseased; the heart was large, but apparently healthy.

I should have stated, perhaps, that on destroying the three-year-old heifer, and examining her, we found the respiratory organs healthy, with one exception; the anterior portion of the left lung was consolidated for some five inches in length and from four to four and a half in width, in its widest part. That consolidated part had the appearance of sloughing away. It appeared as if a band had been passed around it and tightly drawn, so much so that when a man took hold of it and pulled it off, it appeared to be detached from the healthy portion of the lung, and was only held by the pleura-pulmonaris.

During the time that I was visiting Mr. Chenery's herd, the Durham cow became sick, violently so. The attendant said she was as sick as any animal he had ever seen. I attended her, and in ten or twelve days she became convalescent, and continued to live and do very well, until some weeks ago, when she was destroyed. On examination, there was found but very slight disease of the lungs, some adhesions, but nothing indicating serious trouble.

Q.—How came she to be destroyed?

A.—She was destroyed by the Commissioners, I understood. I do not know the fact.

Q.—Does that cover your knowledge of Mr. Chenery's cattle?

A.—No: I was present at the examination last Saturday, and saw two animals destroyed. One was a black and white calf, which I had seen on the 26th of February, it being then in the barn. It was very ill-looking at that time, and had a very peculiar and frequent cough, with an impaired appetite, and almost a cessation of rumination. I saw it from time to time and examined it very frequently; and when I saw it on Saturday last, I made up my mind that there was still disease in the right lung. I had never discovered any in the left lung,

and I was somewhat surprised at the description which Dr. Martin gave of what he expected to find. He did not find what he expected to, but he found the right lung diseased; the left lung, I think, was healthy. The animal had improved very much indeed, and it was a very well-looking animal. The breathing appeared normal, but disease could be detected on the right side by auscultation and percussion. My attention was called many times to the animal, and accompanied by Dr. Thayer, I had examined it many times. Dr. Saunders also examined it. The cow which was killed on Saturday was bought in December,—I should think on the third or fourth,—for the purpose of raising a calf from the mammoth cow. I saw and examined her on Saturday, and she gave to me evidences of marked acute disease,—the best specimen I have seen, except one, of any that I have attended or seen examined, of acute pleuro-pneumonia.

Q.—Was it still in progress?

A.—Yes, Sir.

Q.—Is that all you have to say about Mr. Chenery's herd?

A.—I believe it is.

Q.—Then in regard to North Brookfield?

A.—I was called upon by a gentleman from North Brookfield about the 22d or 23d of February last, who stated that he was instructed to call and see me in relation to some disease which they had among the cattle at North Brookfield. I think it was Thursday or Friday that I received the call, because I recollect stating to a gentleman that I had sent a communication to London the day previous in relation to Mr. Chenery's cattle. I went to Brookfield on the Monday following, the 29th, and visited several farms in that town and in New Braintree. I think the first herd of cattle I examined was at Mr. Olmstead's, in New Braintree. They sent me to the barn by myself, and wished me to discover which were the diseased cattle, if any there were. I walked through the barn, first in front of the cattle, and then behind them, and brought out five or six that were diseased. He said he had a little bull that he wanted me to examine; and with other physicians who accompanied me,—Doctors Porter, Tyler and Seavey,—I examined him, by ausculation and percussion, and I pronounced him diseased. I was next taken into a shed on the other end of the building,

to see a diseased heifer. I stated my opinion that there was no possibility of her recovery, and he consented to have her destroyed and examined in the presence of these other physicians. The left lung presented similar appearances to those already described,—a deposit of lymph between the lung and the walls of the chest,—and firmly adherent. The symptoms were the same, loss of appetite, cessation of rumination, &c. Upon examination, the right lung was found to be slightly diseased. I next went to Mr. Needham's, and there I saw a number of cattle diseased. In order to ascertain whether the disease was contagious or not, I asked which was the cow that had it first, and which of the others had taken it. He pointed out the positions of those that had taken it,—jumping from one part of the barn to another, some of the most remote ones having caught it, while others nearer had not. He said he had lost five cattle. We saw one fine looking cow, that was looking a little ill, but it was getting late in the afternoon, so that she was not killed for examination. She died on the following Saturday.

I then went to Mr. Woodis's barn, and there I saw several cows which manifested disease. I also examined an ox which I pronounced sick, and he also had a red heifer standing outside of the barn which I rather advised him to destroy. But some gentlemen there thought I did not know any more than any body else about the disease, and he declined to kill her. She died in a few days.

We next went to Mr. Wilcox's, and from Mr. Wilcox's to Mr. Stoddard's. He had a white calf that he wished destroyed and examined. I examined several head of cattle there, and in one case I discovered an effusion. The chest was filled with lymph. I was surprised to find no lung there, or only a small portion of the lung tissue. The animal was poor and weak, and could scarcely walk from the barn to the pasture.

I next went to Mr. Huntington's, and there I saw some six head of cattle, all of which I pronounced sick with this disease. He stated to me that he had lost some six or eight, and that this number had been removed to another place, where they had been kept but a short time, and were returned. He had kept them by themselves, not knowing, however, at that time, that any of the others were sick.

From that time to March, I heard no more of it, but on the 22d of March, I accompanied Dr. Thayer on a visit to different herds, and I ascertained that no animal that I had pronounced ill on examination was living. They had all died or were beyond the power of recovery. That we were told by all. I went to visit some herds that I had not before, and made some particular inquiry as to a yoke of oxen sold by Mr. Stoddard to Mr. Olmstead. I followed them up. But before leaving Mr. Stoddard, I obtained his consent—having in his absence examined two or three cattle that had been treated by a man from Spencer, and also a calf four weeks old then lying in the barn, and having pronounced the latter irrecoverable—I obtained his consent to the killing of that calf. I examined it, and found a very beautiful specimen of the disease—the best I had seen. This calf presented the appearance of acute and also of chronic disease. The lung was apparently consolidated, yet there were symptoms of acute pneumonia at that moment. There was what is called interlobular pneumonia, with a small tissue between the lobules. That was thickened to the extent of the eighth of an inch, and was easily broken down. It had the marbled appearance so characteristic of the disease. That is, I may say, the great characteristic of the disease. All the authorities that I have been able to find state that as the principal characteristic generally observed. That was the only case I had ever seen of the kind. I saw it again on Saturday.

Q.—You saw this marbleized appearance?

A.—Yes, Sir; and I called Dr. Dalton's attention particularly to that on Saturday last, and took a portion of the lung to Dr. Bowditch Saturday evening for his examination. I have not had an opportunity of seeing him since.

Q.—Do you mean to say that that is the general characteristic of the disease?

A.—That is its characteristic.

Q.—Then all cases ought to present the same appearance?

A.—I think they do. It was slightly apparent in the case of the ox destroyed on the 26th of February. There was a slight checkered appearance, but it was not well marked. This calf that I speak of, as but four weeks old, was beautifully marked; and in this cow which was destroyed on Saturday, it was appar-

ent, though not so well marked as in the calf. That is the distinguishing symptom between this contagious or infectious pneumonia and common pneumonia. I have seen a number of these cattle which have been examined, that I have seen no symptoms of pleuro-pneumonia about, according to the authorities that I have read upon the subject.

Q.—What is the disease with which you have found the other animals sick?

A.—I look upon this pneumonia to be the result of a specific disease, which I am unable to account for or to give a name to.

Q.—If I understand you correctly, in these two cases, you found the distinguishing mark or feature of pleuro-pneumonia, which is the marbled appearance of the surface of the lungs.

A.—Yes, Sir; and these are the only two cases where I have seen it well marked.

Q.—In the other cases, what inference do you draw as to the non-discovery of that appearance?

A.—If I had been called to examine those cases, not having any knowledge of the existence of the disease, I should have said it was pneumonia, or pleuro-pneumonia, if you like; it is a very common disease.

Q.—The marbled appearance you have never seen exhibited in any other cases?

A.—No, Sir; except the one I saw on the 26th of February, where it was very slight. It looked like a piece of cloth, shaded.

Q.—Well, suppose you don't find it; what is the inference you draw?

A.—Well, I hardly know how to answer that question. If I were unaware, as I before stated, that a disease of the character existed, I should say that the animal had the pleuro-pneumonia—the common pleuro-pneumonia, which is not regarded by any body as contagious or infectious.

Q.—May not that marbled appearance be apparent in the incipient stage; and in the other cases, the disease be farther advanced?

A.—I do not know; but I gather from all the authorities I read, that the appearance is the great characteristic of the disease.

Q.—What is the appearance of the disease in its early stages?

A.—I could not say; but this calf was only four weeks old, and whether it took the disease *in utero*, or after birth, is a question I cannot answer; at any rate, the disease was well marked.

Mr. BIRD.—Excuse me—the butchers in our town tell me that the majority of cattle they kill have just such appearances about their lungs.

A.—The great majority of cattle they kill have diseased lungs. I can assure you of that.

Q.—What is your profession?

A.—I am a veterinary surgeon.

Q.—How long have you been in practice?

A.—I have been twenty-eight years in practice in this State, and I have been something like forty years in practice in all.

Q.—Among cattle particularly?

A.—I have attended thousands of cattle and horses, both; more extensively in cattle.

Q.—Where were you educated?

A.—In England.

Q.—Have you ever seen this disease previous to these cases?

A.—I have never seen lungs which exhibited the appearances mentioned in these three cases.

Q.—Separating these cases from the rest—have you ever seen it?

A.—Very commonly. It is very common to find deposits of lymph, and to have cattle die of the disease.

Q.—Have you found heretofore this extensive disintegration of the lung and the presence of this hard lump.

A.—I have found the lung consolidated, but have never found this lump.

Q.—Have you seen cases where this lump was found, or any thing corresponding to it?

A.—In the case of a cow of Mr. Chenery's which had an attack of the disease, but lived along for some time. Accompanied by Dr. Thayer, I visited her repeatedly. She was very badly diseased, and died, I think, on the eighth of January. The mammoth cow died in the morning, and they laid side by side when Drs. Saunders and Thayer arrived. We made a

slight examination of the old Dutch cow, as she was called, probably because she was not very good looking. We found the lungs extensively diseased, there being extensive excavations in them, into which you might put an inkstand. We found no lump. In one or two cases I have taken out a piece of lung tissue as large as my thumb, which appeared to be enfolded in a kind of capsule, and still it was not detached from the lung. That I believe we saw when I examined a cow in Brookfield.

Q.—Have you assisted in the autopsy of any case of previous disease from which the animal had recovered, or had apparently recovered ?

A.—I assisted in the examination of a Durham cow about six weeks ago, and I made the observation to Mr. Fay, that if he wished to know any thing of the disease, it would be well to destroy some animal that had been sick, and another that had been exposed, but had not yet shown any symptoms. They said the Durham cow had been "cured twice." She had had a slight attack sometime in September, and she showed the marks of applications of mustard then made on her side to relieve her. They thought she had recovered. During our visit, she was severely attacked; she had labored respiration, and the sighing inspiration and the grunt I have described; her skin was adherent, rumination suspended, the eye sunken in its orbit, and the whole aspect ill. She apparently recovered. Upon examination after death, she was found much diseased, and there were some slight adhesions in the right lung only, but there were no apparent lumps.

Q.—May not that have been a case where the application of medicine stayed the progress of the disease ?

A.—I have no doubt that the active treatment she was subjected to, arrested the progress of the disease.

Q.—You have not seen any case which presented a lump enclosed in a cyst as described by Dr. Martin ?

A.—I have not seen any thing of that kind. In one case,—I don't now recollect precisely where,—but in one case which I examined, there were one or two small lumps, a condensation of the lung tissue, enveloped in a kind of capsule. It was not detached, nor was the pleuro-pulmonaris interfered with.

Q.—You spoke of excavations in the lung; were those surrounded by capsules?

A.—They were not. The pleuro-pulmonaris in those cases was destroyed, and there was an opening in the lung.

Q.—Which appeared to be the result of ulceration?

A.—Yes, Sir.

Mr. ANDREW.—Doctor, taking into consideration, as a part of all your knowledge upon this subject, that which you have acquired from the literature of the disease, that which you have acquired from the statements and testimony of other gentlemen here, as the result of immediate observation, and also that which you have learned by your own practice as a physician, and by your examination into morbid symptoms after death,—putting it all together, is it or is it not your opinion, that this disease which you have been treating and which has been exhibited to other persons, is the same disease which you think existed in the other cases where the marbled appearances were present?

A.—It is. I take it as a whole from the general symptoms, and the general appearances of cattle as described; and I know of no better authority than Professor Simonds of London,—taking his description of the symptoms, I think it is the case; but I am at a loss to understand the extent of communication, or the length of time required to give the disease. There is a great variance there.

Q.—Have you any doubt of its being highly contagious?

A.—I have had strong reasons to believe it is contagious, or infectious, or both; and I have had some strong reasons for believing the contrary.

Q.—Do you make any distinction between contagion and infection?

A.—Yes, Sir. I think if a healthy subject were placed in a room which had been occupied by a diseased one, and it caught the disease from the atmosphere of the place, it would be infection. If a healthy animal were placed in the same room with a sick one, and caught the disease, it would be contact. And if this is a contagious disease, it has seemed extraordinary to me that there should be a herd of animals placed in a barn, and that the disease should slip from number one to number

five or number ten, when the animal directly beside it does not catch it.

Q.—May it not be that the susceptibility of number five and number ten may be greater?

A.—It may be; the vital and resistant forces of some animals are certainly greater than those of others.

Q.—And where the vital force is strong, can they resist it altogether?

A.—That is the only way that I can account for the resistance of some of Mr. Chenery's animals. A large calf of his is six months old; it came from a diseased cow, and occupied the same place and sucked upon her four days. He was afterwards placed upon another cow. That cow was destroyed on Saturday, and gave evidence of acute disease. That calf has, to the present time, resisted the disease, having been constantly exposed thus far.

Q.—You consider him very tough?

A.—I suppose I must.

Q.—Were these herds turned out during the day and mingled promiscuously?

A.—I understand they were when the weather permitted. The disease might have been communicated in that way.

Q.—Is there ever any discharge from the mouth or nose?

A.—I have seen it in one case. Mr. Chenery, however, frequently alluded to this symptom. There was a discharge from the mouth and nostrils of the large cow. Mr. Chenery said that before that occurred, he had hopes of her recovery, but when he perceived it, he gave up all hope.

Q.—As the result of your experience and observation, what is the best mode of annihilating the disease?

A.—Isolation. On my first visit to Brookfield, I was called upon to address the meeting on the subject of the disease. I was asked if I considered it contagious or infectious. I hesitated to give an opinion; I was unwilling to stand alone. I said, "If you will give me statistical evidence when the calves were brought from Belmont, and how they became diseased, I will take that, together with what I know of the subject, and the other authorities that I can look up, and then give you an opinion."

I would call your attention to one fact. Mr. Chenery had three calves which were born in the spring of 1858. He had also two calves which were born in the fall of 1858. All of them were kept in the barn during the winter. In the spring of 1859, two calves, from imported stock, fell in the barn, exposed to the miasma there. When old enough to put to grass, he had five calves put out at Lexington, and three or four pastured on a hill sixty or eighty rods from the barn. During the time the smaller number were on this hill, those two were taken from the barn and put with them. A month afterwards, they were put with those at Lexington. Some three or four weeks afterwards, one of those calves was discovered dead. The living one was taken home, and remained a few days, and the balance were brought home in the fall, and have remained apparently well ever since.

Q.—You recommend isolation ?

A.—Yes, Sir.

Q.—To what distance ?

A.—As far as half a mile.

Q.—Have you arrived at the conclusion that the disease is contagious or infectious ?

A.—I have.

Q.—Then why do you fix the distance at half a mile ?

A.—Because I think it is not epidemic. I do not think the virus can be transmitted at that distance.

Q.—Is there any medical treatment, of a preventive or remedial character, which can be recommended to be resorted to, in case the animal has been exposed, but in which the disease is not yet disclosed ?

A.—I think that by proper medical treatment and the adoption of proper sanitary measures, there is a fair prospect that the disease may be prevented.

Q.—Do the symptoms always appear in the first stage of the disease well marked ?

A.—They do not.

Q.—Where there is suspicion of disease, what sort of treatment would you recommend ?

A.—Tonic stimulants would perhaps be best.

Q.—You think the disease could be cured at its commencement by right means ?

A.—I think so.

Q.—What means should you adopt where cattle had been exposed, merely?

A.—A pure atmosphere, with necessary regard to dietetics.

Q.—Do you think disease which had been produced by a specific poison could be prevented?

A.—No, Sir—not *prevented*, but perhaps modified and managed by medical treatment.

Q.—Have any remedies been found in Europe?

A.—Various remedies have been suggested, and are said to have cured. All the authorities of Europe admit that the disease in the first stage may be cured. I think that on the continent they cure as many as 33 or 37 per cent.

Q.—Have premiums been offered for a remedy?

A.—I don't know about that. Premiums have been offered for the best essays on the subject by the British government, and various essays have been written.

Adjourned.

Afternoon Session.

Monday, June 4.

Hearing resumed at 3¼, P. M.

Examination of Dr. Charles M. Wood, continued.

Dr. Loring.—You seemed to think, Doctor, that there was a great deal of disease of the lungs found in butchers' cattle, in the ordinary mode of butchering. What do you suppose to be the cause of the disease?

Answer.—I think it the result of the manner in which they are housed and fed.

Q.—Then you think the disease would be found in cattle, on the farm from which they came, and is not incident to the driving, or the killing them in slaughter-houses?

A.—To a certain extent, it may be the result of driving them; but from the mode in which they are brought to our markets now, it would be hardly fair to state that as a fact. I know that all fat animals are liable to disease, both of the liver and lungs.

Q.—Do you think that such disease as you find in the lungs of slaughtered cattle, would be found in store cattle, fed as they ordinarily are?

A.—I should think it not so likely. I think, as I have before stated, it is the result of confinement and their mode of feeding.

Q.—It would not be found, then, in a herd of thirty or forty ordinary milch cows?

A.—I think not, Sir. I think the ordinary milch cow is subject to disease of the chest, but not of that character.

Q.—Do you think this disease has presented itself, in any form, in its early stages, as what may be called, actually, of an inflammatory character?

A.—I have never seen it.

Q.—Mr. Chenery's cattle, you said, you treated with tonics and stimulants?

A.—The first animal of Mr. Chenery's to which my attention was called, was the four-months-old calf. The symptoms were very active. I treated it by a gentle counter-irritation, and, secondly, by tonics and stimulants. In the active stage of the disease, I gave it neither tonics nor stimulants.

Q.—You said you treated it so because you thought it a disease of debility?

A.—No, Sir; I stated no such thing. I stated that, it being, in my opinion, a state of torpidity and debility, I thought tonics and stimulants were indicated.

Q.—However, it does not present itself, so far as you have seen it, in its early stages, like a violent case of pleuro-pneumonia?

A.—The only early cases I have seen were those of Mr. Chenery's, as I have stated to you,—which I diagnosed as pleuritic. There was a difference, as I have stated, between Dr. Thayer and myself, he thinking that the three-years-old heifer had pneumonic symptoms, while I did not.

Q.—You said, in the morning, you thought isolation was the best mode of stopping the progress of this disease. How long do you think isolation would be necessary?

A.—I think, perhaps several months; I could not say how long.

Q.—I suppose you have not thought enough of the system of isolation, to know to what length of time it should be continued?

A.—I have not.

Q.—Have you any idea of the time when the infection of this disease passes away?

A.—I have not. That is a matter I was desirous to ascertain.

Q.—Can you conceive of any cases in which you would kill an animal, in order to stop the progress of this disease?

A.—I should recommend the destruction of an animal at any time, if satisfied it was past recovery. But as far as the ability to communicate the disease is concerned, of that I should not be able to judge, knowing nothing of the length of incubation, and nothing of the time of propagation.

Q.—Then you would kill the animal, simply in order to get it out of the way, and not to stop the progress of the disease?

A.—Taking a general view of it, that might, perhaps, answer the purpose well enough; but I should kill an animal, if, upon examination, I thought it beyond any possible chance of recovery.

Q.—If you had an animal of your own, that you knew had the disease, and which had exposed the herd, and you had reason to suppose that by killing it you might stop the progress of the disease among your neighbor's herd, would you kill the diseased animal and those exposed to it?

A.—In those I suspected of disease, I should like to see if there was not any possible chance of recovery; but that portion of the herd which I was perfectly satisfied could not recover, I should entertain no objection to having destroyed.

Q.—Then as a matter of scientific investigation, you would be glad to know how far you could treat the disease?

A.—Yes, Sir.

Q.—But as a matter of practical benefit to the farming community, and get rid of an evil to them?

A.—That I know nothing about. I am not satisfied that the disease is not curable, if taken in an early stage; and for that reason I should recommend isolation. That was my object in so stating.

Q.—Do you know of any scientific authority in Europe, which says the disease is curable in its first stages?

A.—In its first stage, if I understand it, they all say it is curable; but they say that the first stage only is curable, and it requires pretty careful treatment to remove it then.

Q.—Has any man said it is incurable?

A.—Every person, perhaps, has said it is incurable after the first stage.

Q.—Do you know whether any testimony has been given by Professor Simonds, to the Committee of Parliament, to the effect that he, from his investigation, considered the disease incurable?

A.—I could not answer that question. I have in my possession the testimony of Professor Simonds, and his Reports to the Agricultural Society, but I do not know whether or not he has made a statement to the effect you mention. I have not had an opportunity to refer to it.

Q.—You can refer to it, if necessary?

A.—Yes, Sir.

Dr. Loring.—I am satisfied upon the point myself, because I have found he did say so.

Witness.—Did he say so in his first or second Report, or did he make an allusion to that effect in his address to the Farmers' Club in Norfolk?

Dr. Loring.—No, Sir. I think he stated so in his examination before the Committee. I think you stated that you thought there was a remedy by which the progress of the disease could be prevented.

A.—I did not say I thought there was a remedy. I said there had been no effort made to find a remedy by which it might be prevented.

Q.—Do you think there is a remedy that can prevent the progress of the disease through a herd, after it has once appeared among them?

A.—If it is a specific disease, I should suppose none but a specific remedy could have any effect, and that I am unacquainted with, and no one has found it, to my knowledge.

Q.—Do you think the progress of a contagious disease, generally,—the smallpox, for instance,—could be prevented by a therapeutic agent,—a medicinal agent?

A.—No, Sir; because I look upon it as a self-limiting disease; but I think it might be modified in its character by a medical agency.

Q.—I do not mean its progress through an individual system, but through a community?

A.—No; I do not think it could.

Q.—Suppose a herd of thirty cattle had got the disease among them, do you think there is any remedy which would stop the progress of the disease through that herd, if it had once broken out there?

A.—I do not know that I am led to suppose the contrary. I do not know that fact myself.

Q.—I suppose the usual law with regard to contagious disease would lead you to suppose there was not a remedy?

A.—There is a remedy for contagious diseases.

Q.—None that would prevent the contagion?

A.—No, Sir. But there are remedies for contagious diseases in the human subject, and I am led to believe that there can be some found for the lower animals.

Q.—You think this disease is peculiar, and not ordinary pleuro-pneumonia?

A.—I think so. It does not look like what is commonly called pleuro-pneumonia. I think that is neither contagious nor infectious.

[The examination here became general.]

Q.—Where does this disease attack the animal,—in the blood or lungs?

A.—I cannot tell whether the disease is produced by inhalation or absorption,—whether through the circulation or whether inhaled into the lungs.

Q.—As far as you know, has any attempt been made to ascertain that?

A.—None that I know of.

Q.—You would recommend isolation for the purpose of treating the disease?

A.—Of experiment.

Q.—To see what cure could be devised for it?

A.—Exactly, Sir.

Q.—Not with a view to this particular year alone, but for science generally?

A.—That is it.

Q.—How long, in your judgment, should isolation be continued, to be safe?

A.—It might be necessary for several months, perhaps. It would be very difficult, perhaps, to answer that question; for, the testimony I have heard to-day, or Saturday, shows that a cow has been removed from Mr. Chenery's, and remained for several months in the town to which she was removed, before the disease was discovered; and yet another has caught the disease from Mr. Chenery's oxen, that were sick last September.

Q.—Should you be willing, after isolation had been continued for some weeks or months, to permit the animal to mix with other cattle?

A.—I should prefer months to weeks or days.

Q.—Would it be safe after months?

A.—After several months, it might be safe,—I cannot tell.

Q.—What would be the value of these cattle, after having been through all this process?

A.—I cannot tell that.

Q.—Would they be worth as much?

A.—I cannot say; I cannot tell how the disease would terminate. They might, and might not, be. If the disease was simply functional, I think, myself, in case of recovery, they would be worth nearly as much. If an organic disease, perhaps they might be lessened in value.

Q.—What is the general rule as to the period of the incubation and propagation of contagious or infectious diseases?

A.—I do not know. There is a limit, I believe, to all contagious diseases, in the human subject; but I know nothing about that.

Q.—Does not the fact of the long period of incubation and propagation, in all these cases, suggest a doubt in your mind, as to the reality of contagion or infection?

A.—Well, that is the ground upon which I base my doubts.

Q.—Are you acquainted with contagious or infectious diseases in cattle?

A.—I know of none.—Yes, we do, to be sure, know that scarlatina, in the horse, is an infectious disease.

Q.—Do you know of any disease in neat cattle, that you so regard?

A.—No, Sir; I know of no contagious disease in cattle, except the one just alluded to.

Q.—And if this is contagious, it is purely exceptional?

A.—Yes, Sir; I should think so.

Q.—Is the murrain contagious?

A.—It is a disease I am little acquainted with. I remember seeing a case or two when quite a boy; but I have seen none of that kind in this country. That is an abdominal disease, however; whereas this is a disease of the respiratory organs.

Q.—You say that the contagiousness, for instance, of small-pox, cannot be affected by any treatment?

A.—I look upon it as a self-limiting disease. It might be shortened by dietetics and medical agency, perhaps. Still, it has, in ordinary language, to "run" a certain time.

Q.—You do not mean to be understood to say that small-pox will act, in subjects of contagion, in those who are exposed under all circumstances, with precisely the same effect, whatever their habits are?

A.—That would depend upon their location and their peculiar idiosyncracies, or susceptibility.

Q.—To what extent, in your judgment, is the contagiousness affected by these circumstances?

A.—It depends upon the locality and susceptibility of the patient.

Q.—It depends upon conditions which it is very difficult to ascertain *a priori?*

A.—Yes, Sir.

Q.—Has this disease ever been made a subject of theory, and regular scientific examination, by discriminating and careful minds, in Europe, so as to have been reduced to any rule whatsoever, touching any of its peculiarities?

A.—I know that some few years since, the British Agricultural Society appointed Professor J. B. Simonds, professor in the Veterinary College, to travel on the Continent to obtain information as to the effect of inoculation, *et cætera*, in regard to the matter. He gave a great deal of information upon the subject in his reports. Those reports I have had; but the disease did not exist in this country, and I had not the occasion, nor did I care to take the trouble, to read them.

Q.—You spoke of having addressed a communication to London. Have you received any reply to it?

A.—I have the communication in my pocket.

Q.—Is it of any value for the purposes of this Committee?

A.—I don't know, Sir.

Q.—What is it?

A.—It is with relation to the disease in Mr. Chenery's herd. I gave the disease no name. I spoke of a fatal disease prevailing in this section of the country at that time; and they headed the article "*Pleuro-pneumonia, as it has appeared in America.*" I was unwilling to give the disease a name, for I had never seen it; nor was I willing to call it pleuro-pneumonia then. At that time, I had seen nothing more of it than I had seen in Mr. Chenery's herd. I afterward visited Brookfield, and then thought it necessary to write again, stating I should modify some opinions expressed in the preceding article, as I had since had further opportunity of observation, which led me to other conclusions. They acknowledged the receipt of it. They have sent me no reply. They published my first article. I expect to hear nothing from the second article for the next month. I had an article published in their work. The paper in my possession has nothing from them, but contains my article.

Q.—Have you an impression that on the premises infected with this disease, a disinfectant would be of any use?

A.—I suggested to Mr. Chenery, when I first went there, the use of a disinfectant or something of the kind. He took that precaution, since which time he has had very little trouble with his cattle. There are some suffering with latent disease of the chest.

Dr. Loring.—You know very well the authority of Dr. Willems, of Belgium, I suppose, as the best on the Continent?

A.—Yes, Sir, I do.

Q.—An opinion from him would be considered by veterinarians, as a valuable opinion?

A.—It has been doubted by Professor Simonds, in his reports.

Q.—I suppose, then, that Willems and Simonds would agree about as well as doctors generally agree?

Dr. Loring.—I find the following opinion of Dr. Willems, which I will read:—

"All alterative medicines, however curative they may be, are powerless in setting an obstacle to the evil, and in repairing the considerable losses which it occasions every day. The beasts which are *cured by treatment* fall away rapidly, and recover but slowly, and with difficulty, from the attack they have sustained."

Q.—Has any of your experience in the treatment of these animals sustained this opinion of Dr. Willems?

A.—No; I treated some of Dr. Chenery's animals, and they improved.

Q.—I know. But there were some which we saw on Saturday, that you would not consider healthy, sound animals?

A.—I examined the calf, on Saturday, which I had treated for Mr. Chenery in November last. It was examined by several medical gentlemen, at the same time, who were unwilling to state they could detect any disease in the chest, although its external appearance is not very good. The animal feeds well, and appears in good spirits, and is improved in condition—that is to say, in flesh—since I last saw it.

Evidence of Hon. Henry F. French.

Q.—Do you know any thing of this reported case, at Exeter, New Hampshire, of pleuro-pneumonia?

A.—I saw, in a paper published at Exeter,—*The American Ballot*,—printed, I think, the 31st of May, a statement that cases of this disease had occurred in that town. It gave the names of the owners of the cattle, in two instances, as Mr. William Gooch, and Joshua Getchell, stating that each of them had lost a cow by pleuro-pneumonia, or by a disease supposed to be that. And I saw that statement copied, I think, this morning, or yesterday, in the *Herald;* and the report has gone abroad in the community. I have heard it stated, in the cars in which I am daily travelling; and have also heard another case spoken of,—that of a cow belonging to Dr. Perry, of Exeter.

I was at Exeter on Saturday, spent the day there, and charged myself with investigating the matter, so far as necessary, in order that if the report was incorrect, it might be corrected. I saw Dr. Perry, a surgeon of great experience, and asked him as to the cow belonging to him, said to have been attacked with the disease. He said that, so far as he had read the state-

ments in the papers, in regard to the disease, his cow had manifested no symptom of it; that it was a case, not uncommon, of stiffening of the limbs and trouble in the chest, which lasted a few days; that the cow was nearly recovered, and had gone to pasture; and that there was no symptom of this disease, so far as he knew. I dropped the matter there, supposing the doctor more competent than myself to give an opinion upon it.

I saw Mr. Gooch, whose house is the next to my own, in Exeter. I saw him last evening. I got the facts from others, before; but I received this statement from him, reliably. He says he bought the cow spoken of, about a week before she died. She died, I think, last Thursday. He bought her at Hampton Falls, of a woman of the name of Sanborn, I think, and took her to Exeter, seven miles. She had a calf about three weeks old at the time. She was very feeble, as he said, and now believes, from the effect of scouring a good deal,—which could not be stopped. He took her to his barn, and she continued very feeble for several days, having no symptoms, that I could recognize, from his statement, of any thing but such as you would expect from mere debility. He turned her out to pasture, and being strange to the pasture, she crossed a swamp, where sensible cattle would not have gone, and got mired. She was there some ten hours, before she finally was got out, in the night. She was well enough to eat hay and corn when in there. He found her lying there, next morning; and he thought she was good for nothing, and knocked her on the head. She was given to a man, there, who gave half of her body to the pigs; and the other half was buried. I cannot hear of any other cases on the road by which she came. I think that is all there is of that case.

The other case was that of the animal of Mr. Getchell,—a cow he had owned but a few weeks, but had bought in the neighborhood. That cow was in pasture with others. (This was the cow more particularly suspected.) She was at pasture until a week ago last Friday,—the last Friday in May,—and giving a good quantity of milk. She then had a calf nearly three months old. Her milk failed in part, on that day. On Saturday, she gave very little milk—was nearly dry. This statement was taken from the mouth of a man who, if he

did not kill the cow, examined her after she died, and who keeps a tin-shop, I believe. He does not profess to be a doctor of cattle particularly, but he comes in in extreme cases. But I have taken the statement in his language; and I think the Committee will be able to judge as well as I, if I give the statement as he gave it to me. I afterwards saw others, and his statement was confirmed. He says that on Sunday he gave her a pound of Epsom salts and bled her.

Judge French then read the following notes of the statement of Mr. Allen :—

"Getchell's cow. At pasture, Friday, (last Friday in May); milk failed in part. Saturday, nearly dry. Sunday, gave her 1 lb. Epsom salts, and bled her. Saw her Monday, apparently in distress,—groaned when she breathed—not quick. Gave her thoroughwort tea—also, ½ pint rum, and three table-spoonfuls of mustard. She did not lie down much, if any, till Monday. Gave her injections. Thought it a stoppage. She lay down and got up, several times,—no trouble in lying down, but was uneasy. No symptoms of having difficulty. She died Wednesday, five days from the day she was first noticed to fail in the milk. Opened her—lungs sound, except as irritated by pouring gruel down her throat. Died in four minutes after pouring gruel down her throat. Found Indian meal in her lungs, from the gruel,—lungs perfect, except a little inflammation near the windpipe. The 'peck' or 'manifold' small paunch was hard as a stave of a flour barrel. Could not cut the feed in it with a knife. From third to big stomach, or paunch, empty. Kidneys inflamed, probably by falling several times."

He says there was a considerable quantity of this meal in her lungs and windpipe.

No scientific man saw the cow after she was killed. There were some bystanders, whom he did not think of sufficient authority to refer me to. I saw a man who had three cows in the same pasture,—a Mr. Haley, a butcher, and who has a considerable number of cattle. He says he saw this cow under treatment, several times. He saw what this man called her stoppage. He thought there were no symptoms of this disease. His cows, which were with her, were perfectly well. I cannot find any man except the man who keeps this tin-shop, who attended the *post mortem* examination.

My own impression is, that there is not the least cause for alarm as regards the town. I made this statement here,

because I thought the legislature might find themselves much embarrassed in setting bounds to the disease, if it had made a step fifty miles eastward, at once.

Q.—You have no reason to suppose the disease to exist there?

A.—I have no reason to suppose the disease to be in that part of New Hampshire, at all. It is said to be in Hillsborough; but that matter is under investigation.

Dr. Jacob Bigelow called.

Mr. ANDREW.—Have you given to this subject of malignant pleuro-pneumonia,—by whatever name it might be called,—in neat cattle, any special attention of your own, outside of that which you have given as an auditor of this testimony?

A.—I have no experimental knowledge on this subject, having never seen a case of the present epidemic. But I have seen something of former epidemics, among men and cattle. I have attended to a part of the testimony, which I have happened to hear, and have read some other things in the newspapers and elsewhere.

Q.—Be kind enough to give the Committee the result of your reflections upon the subject.

A.—I have bestowed some thought upon the subject, and have arrived at the general conclusion that, although the investigation now going on has elicited many useful facts in regard to the disease, yet the most important points to be learned are not yet arrived at. There are certain fundamental considerations which should govern any investigation of this sort, and which I believe remain yet to be settled. And one of the first and most important of these is the great question of the contagiousness of the disease. I am aware that most of the world are now reported as believing that the disease is contagious; and I am aware that most of those who have started on the present inquiry, and most of the witnesses whom I have had the pleasure to hear, have begun, not so much an inquiry whether the disease is contagious, as with the foregone conclusion that it is contagious. Some gentlemen, I do recollect, stated that they began with an unbiased mind, and have gone on with their inquiry until they have arrived at the con-

viction that this disease is contagious. But I find, from the course that the investigations have generally taken, that the inquirers have started by taking it for granted that the disease is contagious. And as the weight of arguments, and of supposed proofs, has hitherto been on the side of contagion, I will, without committing myself to any opinion, for I do not know, any more than any gentleman here knows, as to whether the disease is contagious or not, venture to suggest a few inquiries, which may be put for what they are worth, in the opposite scale to that of the contagion of the disease.

In the first place, the inquiry in regard to the new outbreaks of the disease, has generally been, to trace what I may perhaps be allowed to call its pedigree, to ask where it came from, and not to inquire into the previous and more important question, whether it came at all,—in other words, whether it did not spring up and originate on the spot. Now we well know that epidemics, and among them some of the most formidable, the most extensive, and most deadly epidemics, are not contagious at all. Contagious diseases are those which are communicable from one individual to another. Non-contagious epidemics are those which are communicated to a large number of individuals, either simultaneously or consecutively, from some cause, irrespectively of each other. To make this plain, I will suppose that one or several wells in a place have been poisoned. The people who drink the water of those wells will be sick, but they do not make each other sick, nor will they make others sick by communicating the poison to them. Now I conceive there are some reasons which may go toward showing that this disease, supposed to be communicated by importation from other countries, is not so communicated in reality. The inquiry seems to me to have been, thus far, a one-sided inquiry. Investigations have been made, and facts noted, in regard to the few animals that have been imported, and which have been taken sick in this country afterwards, and happened to be, or were, among the first cases. But in the same inquiry a multitude of other cases have been overlooked and ignored. Cattle have been imported from the most infected countries in Europe, and have brought with them no disease whatever. I am not in possession of statistics to

enable me to say how many cattle are imported into the United States. I do not know whether, in a year, there are hundreds or thousands, but I presume the number is very large; but of the number which come from England or Holland, and other infected countries, the proportion of cattle which remain healthy is immeasurably greater than of those which become diseased. The disease, we are told, now exists all over the world, in every quarter of the globe. And among the cases existing there must have been first cases; for, even if the disease be contagious, it must have had a beginning,—it must have sprung up in England, in Holland, Germany, or somewhere. And if it may spring up once, spontaneously, why may it not spring up twice, or a dozen times? And why may it not spring up in Belmont, or in Brookfield, as well as in any other now infected part of the globe?

In times of popular panic, all widely spread epidemics are believed, by the populace at large, to be contagious. This has been the case in regard to cholera in modern times, in regard to leprosy in old times, in regard to certain pestilential fevers in various times. And the public have acted upon this persuasion, and sick individuals have been avoided, neglected, and fled from, as prolific fountains of contagious disease. And yet these epidemics are now known and admitted, by the intelligent part of the medical profession, to be not contagious.

Two things are necessary to the spreading even of a contagious disease. One of these is the presence of contagion; and the other is the presence of what physicians call predisposition, or susceptibility on the part of the community, to take the disease. And unless both these conditions are present, the disease cannot spread. The most contagious diseases with which we are acquainted,—for example, small-pox, and measles,—are always present in all large cities. And they do not extend so as to be considered in the character of epidemics, except once in a certain number of years, and the reason that they do not thus spread at one time as well as another, is that the predisposition to take the disease is wanting.

Every epidemic disease has its rise, and climax, and decline, after which it goes out for the time, and becomes matter of

history. And such, I presume, will one day be the case with the cattle disease now prevailing here. This is the fact in regard to the epidemic prevalence of cholera, of influenza, of the murrain among cattle, and the rot among sheep. They all have their rise, climax, and decline, and after the susceptible individuals have had them, and died or got well, they then go out for want of fuel, precisely as a conflagration in a city sometimes goes out after it has burnt up the wooden houses, leaving the brick houses standing. As to the epidemic diseases which are not contagious, such as the cholera and influenza, as I have just mentioned, they travel across continents and across oceans, in some vehicle with which we are not acquainted, and of which mankind now know as little as they did of the cause of lightning and thunder before the discovery of electricity. Perhaps this cause will one day be discovered. All I have now to say is that, to my knowledge, it has not been discovered.

It seems to me that the question of the contagiousness of this disease, which is all-important, for it certainly lies at the foundation of all useful practice, and of the great question connected with the arresting of the disease,—it seems to me that the question of contagiousness may be tested by an experiment of the following kind. Not by collecting public rumors and sudden impressions or convictions of credulous individuals, but by taking a certain number,—say, ten or twenty, of healthy cattle, placing them in a healthy district, and then turning in among them a certain number of diseased cattle. That will lead, I think, to a useful result. If the exposed cattle shall all take the disease, and die of it, we may then infer that the epidemic is contagious, and deadly in its character. If, on the contrary, it shall turn out that none of them get the disease, then we shall be justified in drawing exactly the opposite conclusion. And, lastly, if a part of them shall get the disease, and the rest shall not, we shall have a gauge, and index, by which we may judge of the average risk and danger of the disease. And if it turns out that only ten per cent. of the exposed cattle shall get the disease, surely we have no reason to kill off twenty per cent. for fear they should get or give it.

I do not think of any other suggestions, at present, to make, but shall be happy to answer any questions which gentlemen may propose.

I will merely state, in regard to the management and treatment of this epidemic, which I think has got to go out by and by, as its predecessors have done,—so far as I can learn, that the question at issue is as to the propriety of slaughtering the infected and suspected individuals among the cattle of the Commonwealth at large. I have not made up my mind in favor of the propriety of this measure. No doubt, if all the cattle in the State are slaughtered, the disease will stop for a time. But as we must have beef, and must have milk, we shall immediately go to importing other cattle, and then, if contagious, we may get the disease again, as we got it before. And, moreover, if it should turn out that half the cattle in the State are slaughtered, and only the remaining moiety left, I should not consider, even if the disease should then stop, that it was proved to be contagious, for, in the first place, the remaining cattle might still get a similar disease from an epidemic cause, such as I have already stated, independent of contagion. And, lastly, if it did stop, I should not infer that, necessarily, the result had any thing to do with the supposed cause. If I were to state an instance in illustration of this point, I would say that in the dark ages, when men were ignorant and credulous, they attempted to arrest epidemics by hanging Jews and burning witches; and if this practice did not prove effectual in stopping the disease, they proceeded to execute more Jews and witches, until it did stop. But, I believe, it does not follow, and would not in the mind of any reasonable man now-a-days, follow, that these two events stood, to each other, in the relation of cause and effect.

Q.—Have you ever seen, yourself, either now or at previous times, any cause of disease in animals corresponding to this malignant pleuro-pneumonia?

A.—I have seen cases of disease in animals, and sometimes where the disease was prevalent among the animals of a great farm. I have known herds of cattle to have numbers affected, and droves of swine, also; but I have never made a study of such disease, inasmuch as it never has prevailed, within my

knowledge, to such an extent as to excite particular curiosity in regard to a minute examination of its symptoms and anatomical character.

Q.—Then are you able to say, Doctor, whether these symptoms of disease, both in the living and dead subject, which have been disclosed in the testimony, are new to New England, or not?

A.—I am not able to say. I have no knowledge on that subject. What are called sporadic cases, single cases, may have occurred for all that I know for all time. But they have not been subjected to a systematic examination so as to identify them, and to know whether they are the same or different.

Q.—Have you examined the accounts of this malignant disease as it has appeared in England and on the Continent?

A.—I have not.

Q.—Are you able to pass an opinion as to this question,—whether the facts thus far disclosed from the examination made here in Massachusetts are numerous or exact enough to enable any one to draw a satisfactory inference as to the real character of the disease, touching its contagiousness or infectiousness?

A.—They have been numerous enough to justify one in drawing inferences in regard to its pathological character; that is, in regard to its symptoms and morbid appearances after death. As to its contagiousness, I have already stated that I do not think the observations sufficiently extensive.

Q.—Can you give the Committee any opinions which would be needful to them touching the probability of this disease,—whatever it may be,—being liable to substantial reduction by medical treatment. I am aware this is entirely outside of your practice,—but as a student of natural science generally?

A.—I think that, like other epidemics, it could be alleviated, palliated, and rendered more safe, by what I should call a natural, salutary treatment, but I know of no very violent or heroic measures that would be likely to arrest or lessen its intensity or extent.

Q.—To make a practical application, I will suppose a case. Supposing that the disease itself is a contagion, and supposing the contagion to be present in a herd of thirty cattle so long that you know all of those cattle to have been subjected to the

possible influences of the contagion. You would infer, I suppose, as to some percentage of that thing, that the susceptibility of the contagion was also present. Then what would be your advice to the proprietor of the thirty cattle as to what he should do with all of them, before there has been any appearance of active disease in any one of them. And then in the next place what would you advise to be done for those who began to manifest symptoms?

A.—I should pursue the same course, if it were practicable, with cattle, that I should pursue with men, or that men would pursue in regard to themselves. I would remove, if possible, those that had been exposed, to a healthy district. I am not sure that this would be practicable in view of the supposed conflicting interests of the owners of the cattle and their neighbors. I do know that if a man is in Norfolk or New Orleans when the yellow fever breaks out, he comes, if he can, to Boston, or some other healthy district. So a man who resides in a fever and ague district, if he has got the disease, or expects to get it, immediately leaves the place, and goes into a non-malarious country, a country where he is not subject to the influence which brought it on where he was.

Q.—That would not apply to contagious diseases like measles or smallpox. Suppose that this disease is contagious in animals, as measles or smallpox are in men, and a man's herd had been subjected to contact with diseased animals, so that if a susceptibility was present in the herd, the inference would naturally follow that some of them would be sick, is there any course of treatment which would seem to you to be wiser, so far as you know, than any other?

A.—The first course would be to get the animals into another locality. If that could not be done, I should put them in some situation where they would enjoy free air. I should keep them upon a very moderate diet, such as would neither overfeed nor underfeed them, and I should let them take their chance. I do not know of any medication which would be relied on to stop the progress of individual cases.

Q.—What should you think of the slaughter of all of them, on the ground that they had been subjected to disease?

A —I should infer that there would be no case of the disease left if they were all slaughtered. But it would do no certain good to wait until some were taken sick, and then slaughter them.

Q.—There is then no medium ground if you adopt any system of killing between where you begin and where you stop, until you have killed all?

A.—I cannot give positive information on that point. My presumption is that it would be a very difficult thing to carry a middle course into profitable execution, because most contagious diseases are contagious from beginning to end, and to slaughter an animal affected with disease after the actual symptoms of that disease have shown themselves, is to shut the door after the steed is stolen—premising that the disease is contagious.

Q.—Suppose the disease is contagious, the only logically consistent system would be to slaughter every animal which had been possibly exposed to the contagion, would it not?

A.—If that theory is adopted as a rule of practice, I should say that to be thorough, the only way is to kill all animals showing symptoms of disease, and all which by exposure had become candidates for it.

Q —Whether you have any means of forming an opinion as to the probable effect of isolation; supposing you draw a cordon around a certain space called an infected district,—whether it is reasonable that keeping up non-intercourse between animals outside and inside, the contagion could be stopped,—assuming it to be contagion?

A.—I think it would have a good effect. It would satisfy the requirement of public opinion. I do not know of any way of isolating cattle so as to keep them from some communication. An infected herd may be shut up from danger, but as the disease may appear in other places, I do not know how the legislation of one town can produce a non-intercourse with another town or State. Suppose, for example, that Massachusetts should pass laws as stringent as possible to cut off all intercourse between the cattle of this State and those of Rhode Island and Connecticut, what is to hinder the cattle from going to the boundary fence and putting their noses together.

Q.—Suppose you made a neutral line?

A.—That may be done if you produce the coöperation of other States; and I think that would be very satisfactory to public opinion.

Q.—Don't you think it worth while, now we have got this disease among us, to ascertain whether it be curable or not?

A.—By all means.

Q.—Would you advise that as a medical man?

A.—I would advise as a medical man that squads or small herds of cattle should be made the subjects of different experimental treatment; that half a dozen be treated in one method, and half a dozen in another method, and another half a dozen in a third method; and that the result be obesrved.

Q.—You think it would be advisable to have a commission appointed for the purpose of making this investigation or inquiry?

A.—I should think it might result in very useful information, provided it is made by individuals qualified to judge, and of impartial character—so that their minds might not be settled upon any thing beforehand.

Q.—How, in your opinion, is this disease communicated to the animal? through the blood or directly from the lungs?

A.—That I am wholly unable to say. I have formed no opinion upon it. If it is a contagious disease, it is conveyed by something received from the cattle; if it is not a contagious discase, it is produced by some morbific influences derived from elsewhere. What part it invades first is of secondary consequence.

Q.—Suppose it to be in the air, what part of the animal would be likely to be first affected,—the blood or the tissues?

A.—That I am utterly unable to answer. Nobody knows. It is easy to answer questions on conjecture. If you produce proof, the answer would be on a very different ground.

Dr. Loring.—Doctor, if you had two herds of cattle consisting of ten each, and they were half a mile apart; and to improve your stock, you purchased during the last year a creature of Mr. Chenery, and that creature should have become sick in the herd with which you herded it, and you soon

found that the rest of that herd were becoming sick,—would you remove these sick ones to your well herd half a mile off, to give them a chance to recover?

A.—Considering the question of contagion still open and unsettled, I might as a precautionary measure, to cover a possible risk, make such removal. If it were a matter of indifference, or mere curiosity, I do not know that I would.

Q.—Have you heard the testimony relative to the sale of calves by Mr. Chenery, and their removal to Brookfield?

A.—I have heard something about it.

Q.—I mean the testimony that in every case the disease has been traced to one origin. On the supposition that you can confide in that testimony, would you call it epidemic or contagious?

A.—I should suspend my opinion until I got further evidence; knowing that cholera and yellow fever, and many other epidemics not known to be contagious, were traced with as much minuteness and exactitude from family to family, and from man to man, as these cases of the cattle now are.

Q.—Is that so—that epidemics are traced? Do they not rise with exposure?

A.—Some epidemics do and some do not.

Q.—Is it not the common history of epidemics that they rise irrespective of connection with the disease.

A.—Smallpox is an epidemic supposed to be communicated only by contagion. Cholera is an epidemic communicated by other causes than contagion.

Q.—I understood you to make that distinction,—that epidemic diseases originated from other diseases aside from contact.

A.—The term epidemic is a general term, including both contagious and non-contagious diseases. Epidemics are diseases that spread over a whole community. One class is contagious and another class is not contagious.

Q.—Can there be any more conclusive evidence that a disease extending over quite an extent of territory originated in contagion, these being traced distinctly to one origin in every case?

A.—No better than to subject it to a new and decisive experiment, such as I have suggested, to see whether the converse of the rule will operate or not.

Q.—If you find that calves from the same herd are sent in other directions, and the disease is not communicated?

A.—I should entertain the same views, and want more evidence.

Q.—If twelve well cattle should be placed with twelve diseased ones in a healthy location, and all the well ones should take the disease, would it be proof that the disease was contagious?

A.—As I have already said, I should think it strong presumptive proof. There are two things necessary to prove a contagious affection; one is the existence of contagion, and the other is that the animals were so susceptible as to take it.

Q.—I suppose these animals were susceptible, and did not take it, would that prove it was not a contagious disease?

A.—We cannot tell in advance whether they are susceptible; but if we could, such facts on either side would be strong presumptive evidence of the contagion or non-contagion of the disease.

Q.—You think, Dr. Bigelow, that this question of contagion is entirely open?

A.—I am aware that the preponderating opinion of the world seems to be in favor of the contagion; but to my mind this opinion does not prove the contagion any more than that multitudes of prevalent erroneous doctrines may be considered as proved.

Q.—I suppose in modern times,—especially in our enlightened community,—it is not deemed necessary in scientific investigation to set aside all the testimony of creditable witnesses, is it?

A.—Not to set it aside, but to give it all the examination it deserves; and whether it comes from one or a hundred persons, I should say that every man has a right to form his own opinion from the intrinsic merits of the case itself.

Q.—Well, when you say there have been panics and superstitions in regard to epidemics, originating in former times

you don't mean that the intellectual condition of mankind is the same, and that disease is less understood than it was then?

A.—As a general fact, the intellectual condition of mankind and their information are improved, and disease is in some respects better understood, but in others I am sorry to say it is not better understood.

Q.—I suppose that if this disease has prevailed for a great number of years, and been submitted to scientific investigation, as far as science can apply to the diseases of animals, the testimony of those who have investigated it would be considered worthy of considerable weight?

A.—It is of course worthy of receiving a certain amount of candid attention, but it is well known that there is as much error in medical science as there is in theological or political science. A great deal of it is matter of opinion, and not of positive demonstrable fact.

Q.—I suppose you are aware that some very interesting experiments in certain parts of Europe in regard to the contagious character of this disease were made by scientific persons there, somewhat analogous to the proposition made by yourself, with regard to shutting up diseased animals in healthy places?

A.—I am not acquainted with satisfactory experiments made in any part of the world. If the experiments have been made which I suggested, I should take the result of it as leading to pretty strong presumptive proof.

Mr. Bird.—Nothing of the kind has been proved here, however.

Dr. Loring.—I am simply endeavoring to ascertain what scientific experiments have been made any where.

Mr. Bird.—I do not think, Mr. Chairman, it is hardly fair to bring in any knowledge which he may have of matters elsewhere; they are not before the Committee.

Dr. Loring.—In speaking of this question of contagiousness, Doctor, you suggested that there were importations constantly to this country, and that it was very extraordinary that cases of this disease should be so rare under those circumstances.

I suppose you are aware that the number of cattle imported from Holland to this country is small,—that the Dutch animal is not a favorite of American importers?

A.—I stated that I am not in possession of statistical facts, to enable me to say how many animals are imported from one country or another. But since I came into this room, I have asked the opinion of half a dozen gentlemen supposed to have, and whom I found to have more or less knowledge, of the amount and the number of cattle introduced by importation, and I have found that they all of them seemed to express an opinion that the number was quite large, and that they were imported from various countries; I do not know how many from one country or another. But I understand that the disease has existed in all quarters of the globe, and all countries, from here to the antipodes, although I did not take the trouble to single out any particular country from the rest.

Dr. LORING.—Some interesting facts might be given as to the importation of cattle into this country, showing that the cattle which are favorites with breeders and importers, seldom come from infected districts.

[*To Dr. Bigelow.*]—Have you any knowledge that the disease does exist in every quarter of the globe?

A.—No further than I have read in the newspapers, and heard in this room. It exists in America, in Great Britain, Holland, Denmark, Africa and Australia, and I know not how many other countries. I presume there can be little doubt that it has existed in intermediate countries, and in Asia, the remaining quarter of the globe.

Q.—Are you aware of the knowledge which we have had here in regard to the instances in Africa of this disease, that the testimony shows that it was carried into Africa from a distant and infected district, and so far as any stop has been put to it, it has been by burning it out, by isolation?

A.—I do not know of any other or different facts, in regard to the introduction of the disease into Africa, than that at one time were believed to exist in regard to the introduction of the cholera into the United States. We know that that disease was a subject of great alarm. Passengers were prohibited

from landing from New York, and in some of the New England States, and as that procedure was founded in error, the other may also prove to be. Some of the statements that I have seen or heard in the course of this hearing, appear to me not very probable. I noticed that it was stated that the disease prevailed at one time in Denmark, and then it stopped,—how and when, and in what manner, I am unable to say,—and that it afterwards broke out in consequence of a cow being brought from Hungary that had the disease. It appeared to me more probable that the disease had remained in Denmark all the while, or was indigenous there.

Dr. Loring.—I made the statement that the bringing of one hundred and eighty oxen from Hungary caused the re-appearance of the disease in Denmark. The application of stringent laws in Denmark had satisfied the farmers that the disease was nearly eradicated. In regard to Australia, doctor, are there any statements with regard to the existence of the disease in Australia that you are aware of, other than those in the newspapers? Is it not a fact that all the knowledge we have of that country, goes to show that the seed of the disease got into Australia, but was destroyed so soon that it never spread?

A.—When I read the newspaper account, the only thing that struck me was the extreme improbability that this disease could have been carried in the body of a cow, on a three-months' passage from Europe to Australia, and that it could then be communicated from that animal.

Q.—Did you ever hear of the disease in South America?

A.—I never did.

Q.—You referred to the analogy between hanging witches and Jews in times of superstition to cure distempers. Do you think there is any analogy between such a piece of barbarous superstition as that is, and an intelligent and enlightened attempt to remove disease by killing the disease-breeding cattle?

A.—The question whether any proceeding is enlightened or intelligent depends not so much upon the qualifications of the individuals to judge rightly, as upon the manner in which they apply their knowledge to practice. I believe it to be a fact that

people are as much terrified in the most civilized times in regard to epidemics, as they are in the most ignorant and unenlightened age of the world. If I may be allowed to relate an anecdote, I will do so. When the cholera first broke out in New York, where it destroyed some three thousand people, in the height of the epidemic, the city government of this city despatched a commission consisting of three physicians, to investigate the disease, and report what they thought expedient to be done. I was one of the commission. After residing in New York three days, we returned in a steam-boat, the last which came from New York that season, to Providence. When we arrived a mile below Providence, a boat came to us with orders from the Board of Health of Providence to hold no communication with the shore.

We sent repeated applications to be permitted to land and get home, for we were impatient and tired; but we were kept there against our will the whole day, at anchor in the stream below the town of Providence; and about once in an hour a boat came down to us to tell us that the Board of Health were still in session in the court house, and that the court house was surrounded by a mob, and that the Board of Health was disposed to allow us to land, but the mob kept sending in petitions not to permit this pestilential epidemic to enter into their State. At length a despatch came to us that we might land at Seekonk, in the State of Massachusetts, but that we should not land in Rhode Island. I do not know of any thing in the dark ages much less intelligent than that.

Q.—Then you think there is some analogy between hanging Jews and witches who do not communicate disease, and killing cattle which are spreading disease in every direction?

A.—Both are expedient only so far as it gratifies popular excitement.

Q.—If you had a herd of animals, and introduced among them a diseased one, and you found by experience that that disease passed from that animal into others—you would not hesitate to distribute your animals about the community?

A.—In obedience to the popular will and a desire to accommodate it, and not to outrage any strong prejudices of the community, I should not.

Q.—I am not speaking of popular prejudices; but as a man and good citizen and a good farmer, what would you do if you had a disease brought into your herd, and it went from one animal to another, step by step, and you saw that the disease went by contagion—without the application of the nicest scientific principles—should you consider it a barbarous prejudice to shut up your cattle?

A.—If I was convinced that the disease was brought there by contagion, I should not allow them to go elsewhere. If not, I should endeavor to get at such evidence as I might to settle the question.

Q.—I suppose you would be unable to say whether animals would be fit for breeding,—for the purposes of reproduction,—in this diseased state?

A.—I am not clear on that point; we know that children are sometimes born of parents in their last illness, and those children grow up to be healthy, useful citizens.

Dr. Loring.—That point has been put here once before, and a comparison make between men and animals; and it has been stated that there were men who had but one lung, and were useful for all the practical purposes of human life. That may be true. The question arises whether there is any difference between intellectual and accountable being, and a simple animal, who is good for nothing except the service he will render to man. It seems to me that there is, and I don't want to ask any question on that point.

Mr. Bird.—I want to ask the Doctor whether, in his opinion, a cow or bull is affected by an acute attack, so that one lung is destroyed, leaving the other entirely healthy, whether the progeny of such animals would or would not be healthy?

A.—That involves further inquiry. Among mankind we know that there are certain diseases that are hereditary.

Q.—Well, in case of an actual attack of pleuro-pneumonia, what would be the effect in that respect?

A.—If the animal had the disease and was recovered, I do not see why he is not entitled to have as healthy progeny as any other animal. If I understand the disease, it is capable, in some instances, of recovery. After he gets well, with one-

half of a lung or a whole lung disabled, then what remains to him is an infirmity and not a disease. He is like a man with one limb gone ; he is good for various purposes, though he may not walk very well.

Q.—Would he be as good for procreating purposes ?

A.—I submit that to the Committee.

Q.—I mean a brute—not a man.

A.—Well, what a man may do, a brute may do, I suppose.

Q.—Do you think it would be practicable or safe to remove any animals diseased with a contagious disease, that is, if you had a suspicion that animals had a contagious disease do you think it would be safe to remove them to some other district ?

A.—I think it would be safe to remove them into a healthy district providing their disease is contagious, if all other cattle are removed from the same locality.

Q.—You don't imagine that private enterprise could do this thing ?

A.—I should think it proper that the Commonwealth should institute experiments that might throw light upon the epidemic.

Dr. Loring.—You thought that free air, and a moderate diet might have a beneficial effect upon the disease ?

A.—Nothing strikes me as more probable, with a removal of the diseased animals into as different a situation as possible.

Q.—Suppose you were to enter a country barn that reminded you of the once fashionable "ventilation gossamer hat," filled with meadow hay, you would conceive that there was free air and moderate diet; and suppose you found the disease very extensively in such a barn as that ?

A.—I should be inclined to try another barn ; for example, a colder after a warmer one.

Q.—Well, suppose you were in a warm and tight, well-clapboarded barn, with plenty of good English hay, and you found the disease still worse, what would you do under the circumstances ?

A.—I should either give it up and let the cattle die, or should try a new experiment.

Dr. Loring.—Well, we did try a new experiment, we killed them—because those were the facts we found, to a remarkable extent, in North Brookfield.

Mr. Andrew.—Doctor, I think I have heard it stated that no pulmonary disease in the human system is contagious. Is that true?

A.—I know of no disease of the lungs, or generally speaking of the chest, which is contagious. There are some diseases,—a very few in number, which have appeared to be epidemic to a small extent,—such as what was once called typhoid pneumonia. But I do not know of any evidence that such diseases are contagious, or that there is any evidence that there is any analogy between these and pleuro-pneumonia, which occurs in mankind.

Q.—Supposing it to be true that no disease of the lungs or chest, in man, is contagious, is it or is it not possible to draw any inference from that fact touching the contagiousness of lung or chest diseases in neat cattle?

A.—I should consider the evidence to be very strong evidence. As far as it goes, the analogy leads to the justification of the belief that what is not contagious in one race of beings may not be in another. You have to jump too wide a gap between the human being and the brute creation to suppose otherwise. No such inference could be justified with certainty; it is at most a probability.

Dr. Loring.—When you were speaking of the question of contagiousness, you thought it might be decided by taking ten or twenty diseased cattle and putting them into a healthy place. It occurred to me that precisely that experiment had been tried at Brookfield; otherwise, how do you account for the jump of the disease from Belmont to North Brookfield?

A.—I am not qualified to give decisive information nearer than this. I see two methods by which it could have appeared at North Brookfield. One of these is contagion; the other is indigenous origin, arising in North Brookfield as it has done in some other places.

Q.—It seems to be possible that it may have been indigenous in Belmont and North Brookfield?

A.—Yes, Sir.

Mr. Bird.—Dr. Bigelow, is it or is it not safe to place any material reliance upon supposed analogies between this disease and similar diseases in foreign countries?

A.—If upon accurate comparison of the accounts given and reports made of the two diseases, they closely resemble each other, I should say it was justifiable. Not otherwise.

Q.—What diseases, within your knowledge of the medical world, are regarded to be contagious other than cutaneous diseases?

A.—Smallpox, measles, hooping-cough, the ship fever or typhus fever. The typhoid fever we do not commonly consider as contagious. Ship fever is, and as an example of it, the late Dr. Moriarty, brother of the present Dr. Moriarty, physician on Deer Island, took the disease and died, having had communication with patients there.

Q.—Are not cases of ship fever transferred from ships to Deer Island?

A.—They are, Sir, and if there is a predisposition existing to take the disease, it will spread. If not, it will not spread. I have known cases of ship fever to the number of seventeen, to be taken at one time to the House of Industry at South Boston, and not a secondary case occurred there.

Q.—Does not that pretty nearly destroy the theory of its contagiousness?

A.—It shows that there was no predisposition there. It is pretty generally acknowledged that ship fever has a contagious element, and may be communicated to predisposed persons.

Q.—Well, as a general rule, it is only eruptive diseases that are contagious, is it not?

A.—No, Sir; I am not prepared to say so. The hooping-cough is considered contagious, and I think I might add, mumps, and perhaps some others. There are various diseases in regard to which the medical profession are not agreed, whether they are contagious or not. I have said that these diseases are generally conceived to be contagious; I do not positively assert myself that they are.

Q.—Then it is very difficult to say positively how far the contagious virus is affected by susceptibility, or by other conditions, so as to determine that in given cases it will absolutely be communicated.

A.—There are causes in existence which we cannot measure, and do not know at the time; and we cannot foretell the result in certain cases of exposure.

Dr. Loring.—Ship fever is considered contagious, generally, is it not?

A.—Yes, Sir.

Q.—Well, now, ship fever requires susceptibility. Would you consider that an argument against its contagion, or was the fact that I never took ship fever when attending it in the hospital, an argument against its contagiousness?

A.—That would be no argument against its contagiousness.

Q.—Would not the law that applies to ship fever apply to all contagious diseases? Is there any reason to doubt that?

A.—I presume it does.

Evidence of A. B. Wilton, of Dorchester.

Q.—What is your profession?

A.—I am a doctor of horses and cattle.

Q.—Have you had any cases in your practice, of pleuro-pneumonia among cattle?

A.—I can state to you the symptoms; the name I cannot tell so well. Other persons may call it what they see fit. I have had animals that commenced with giving less milk, loss of appetite, and loss of flesh. Some of them stood with their shoulders stooping and their backs humped up, eyes stupid, and laborious breathing in some cases.

Q.—What cases have you had very recently?

A.—I guess I had one at 'Squire Draper's, about a fortnight ago. The animal got better. There were two animals sick, and one of them died, but not of this disease. She was turned into green feed, overloaded her stomach, became sick, and being feeble, we concluded to kill her. Dr. Cushing, of Dorchester, examined her.

Q.—What was the condition of the lungs?

A.—On one side of the left lung there was an adhesion to the side, and the lung was of a darker color than in a healthy animal. Dr. Cushing, who knows more about the matter than I do, said that she had been sick, as near as I understood him, with this disease, but had recovered, and did not die of it.

Q.—Did you find any cyst in the lungs?

A.—There was no pus, at all.

Q.—Any hardened matter?

A.—Well, a small portion of the liver was hardened.

Q.—You think it was what is called pleuro-pneumonia?

A.—I think, from the symptoms I have seen described in the newspapers, and heard talked about, that it was that disease.

Q.—When was this cow taken sick?

A.—I should think about the last of March. Mr. Draper had one sick before, but she recovered, and is now getting on flesh, and is doing well.

Dr. William Saunders, of Salem, was called to the stand, but Mr. Bird stated (a large number of the Committee having left the hall) that he objected to going on with only a minority of the Committee present; but if it was the desire of the Committee, he would go on.

The roll was called, and the door-keeper was instructed to require the attendance of the absentees.

Evidence of Dr. Saunders.

Q.—What is your profession?

A.—I am a veterinary surgeon.

Q.—How long have you been in practice?

A.—Over twenty years.

Q.—Are you acquainted with any facts connected with this matter now under investigation?

A.—I have seen the herd of Mr. Chenery, in Belmont.

Q.—Did you hear the testimony of Dr. Wood?

A.—Only a small portion of it.

Q.—Please to state to the Committee what you know in regard to the matter?

A.—I know but very little. All I have seen, are some few cattle of Mr. Chenery's, when I was present at the *post mortem* examination.

Q.—When?

A.—I was present at the examination of the large cow, and an ox previous to that, and, I think, one or two head of cattle besides.

Q.—When was that?

A.—I cannot tell the dates. Dr. Wood was present at the same time.

Q.—In your opinion, what was the disease?

A.—I think a disease of the lungs, evidently.

Q.—What?

A.—There was a doubt in mind at the time, what the disease really was. It was called pleuro-pneumonia. There had evidently been pleurisy, and pneumonia, too. I can say of one animal that I saw examined, that I never saw an animal with lungs that presented the same appearance. That was the case of the ox that was slaughtered. I think I showed Dr. Loring a portion of the lungs at my house.

Q.—What was the difference between them and the other lungs which you had seen?

A.—These had a streaked, checkered appearance, different from any case of acute pneumonia.

Q.—Do you think the difference one of kind or degree?

A.—Both.

Q.—Was it a difference in the kind of disease, or in the degree?

A.—A difference in the kind of disease.

Q.—In your practice, you say you have never seen any such case?

A.—Not that I remember.

Q.—How many did you examine there?

A.—Some five or six.

Q.—Have you ever seen a case resembling the others?

A.—I think I have, partially.

Q.—Upon the whole, from what you saw, and from what you have learned, what opinion have you formed in regard to the contagiousness of this disease?

A.—Well, I did not think, at that time, that it was contagious. I considered it a disease more of location than any thing else.

Q.—What do you mean by that?

A.—I thought it was a disease caused by the cattle being kept in a close place, breathing impure air—a large number of cattle in a small space. But since that I have come to the conclusion that it must be a contagious disease, from the reports I have read, and from the reports made by medical men.

Q.—Who were they?

A.—Dr. Wood, Dr. Thayer, and Dr. Loring.

Dr. Loring.—I understand the witness to say that he regards this disease, from his own observation, and from reports presented to him from other physicians, as contagious. Now, as that seems to be the point in issue in his examination, I have no questions to ask. I would merely inquire of Dr. Saunders if he ever saw any lung brought to the condition, by any ordinary disease with which he is acquainted, in which that lung was presented to him for examination last winter?

A.—No, Sir; I so stated at the time.

Q.—What distinction do you make between contagious and infectious diseases?

A.—I suppose there is a distinction, but I consider it rather a fine one, and I don't think I am alone in that opinion. I would not like to attempt to define it myself, because I don't think I could do it justice.

Q.—Have you noticed any cases in which the liver was examined particularly in the *post mortem* examination?

A.—Yes, Sir; the liver of the ox, I think, was examined at the time I saw him opened.

Q.—How did it appear?

A.—It was rather in a diseased state. I think it was the ox, but I would not be positive.

Q.—On the ground that this is a contagious disease—admitting, from all the facts that you have gathered, that it is contagious—do you look upon it as a constitutional disease, or a local disease, confined to the lungs, and that whatever constitutional disease there may be grows out of the local difficulty in the lungs?

A.—I should think it was. I should think that the disease of the liver might have been consecutive—followed.

Hon. Amasa Walker.—If Mr. Bird has got through for the present, I would say that I was requested to appear before this Committee in behalf of the Worcester South Agricultural Society. You have the letter of authority in your possession, Sir, or the clerk has, and I would thank the clerk to read it.

The clerk read the letter, as follows:—

STURBRIDGE, May 30, 1860.

Hon. AMASA WALKER—Dear Sir,—At a meeting of the Executive Committee of the Worcester South Agricultural Society, held in Agricultural Hall, in Sturbridge, the 29th inst., the following votes were passed:—

"*Voted*, To appoint a special agent to appear before the legislature in behalf of said Society and co-operate with the members of the legislature within the limits of this Society in securing the most effective enactments for the suppression of the disease among cattle called pleuro-pneumonia.

"*Voted*, That Hon. Amasa Walker, of North Brookfield, be invited to act as such agent in securing the object above stated."

We earnestly hope your engagements will allow you to comply with the wishes of the Executive Committee in efforts to secure all the protection that law can confer, against the ravages of the terrible scourge among cattle in this vicinity. Very respectfully yours,

D. WIGHT, Jr., *Rec. Sec.*

Mr. Walker continued:—

The interest which attaches to this expression of the views of that society depends much upon this circumstance, that they are situated in the locality of this disease, while, at the same time, they are a little removed from the centre, so that they may be considered as likely to look at it a little more impartially, and with cooler judgment, than those who are situated in the immediate neighborhood of it. A fortnight ago last Saturday, I attended a special meeting of the society, called with reference to this disease. They had sent an invitation to the Commissioners to be present at that meeting. The Commissioners requested me to attend, and I did so. I found a very full meeting—farmers, and all others belonging to the society. Several professional men were present, and among others, a lawyer—one of the principal lawyers of that neighborhood. The question was brought up, and a resolution offered, to the effect, "that the members of this society memorialize the legislature for an efficient law upon the subject, giving the Commissioners authority to adopt such measures as shall seem to them most expedient for the public safety, in reference to this disease." This legal gentleman got up and moved "that the society petition the legislature to abolish the law and the Commission." He defended his motion in a speech of three-quar-

ters of an hour, at least, I should think,—a very able speech indeed; that is, he went into the question in *extenso*, and with great particularity, and argued his case like an able lawyer. Several gentlemen then rose—farmers, physicians, and others, and spoke upon the other side; and then his motion was put, and there was just one vote in its favor, and that was his own. The question then came up on the motion for a petition to the legislature in favor of the law, and there was but one single vote against it, and that was this same gentleman's.

I mention this to show the perfect unanimity among the people of that section in reference to this matter. There was a very full meeting of all classes—farmers were there, physicians were there, professional men and others, and they were all united, to a man, in the most earnest and hearty sentiment in favor of petitioning the legislature for an effective law. They knew what the Commissioners had done, and another vote that they passed with equal unanimity, was, that they approved the action of the Commissioners.

Mr. Andrew.—Is not that like taking a vote for president in the cars?

Mr. Walker.—It should not be considered so, for they were met together on a very serious matter. They had come together to consider a matter that immediately concerned their personal interests. No question could be more serious to them than the security of their cattle, for that is a great thing to a farmer. They had met to consider this question in a serious and solemn manner, many persons speaking, and apparently with cordial unanimity, and a good deal of feeling; and I regard it as the expression of the feeling of the Worcester County South Agricultural Society on this subject. Several towns were represented—Sturbridge, Charlton, Brookfield, North Brookfield, and others—and the vote was unanimous, as I said before, with the exception I mentioned, and there was a great deal of earnest feeling manifested. And that was done after I had expressed the views of the Commissioners—that we wanted a law which should give discretionary powers to the Commissioners, and which, instead of compelling them to kill whole herds, should enable them to separate the herds, isolate them, and

do whatever they thought necessary to secure the safety of the public. They understood the subject fully, they deliberated on it carefully, anxiously, and they voted with great unanimity. I think their opinion should have weight, coming from so important a section of the State, and from a society which is a thriving one, and has a great deal of influence in that part of the county—a society whose members own more good cattle than the members of any other society in the Commonwealth. Of course they have great interest in this matter, and I hope the prayer of the petitioners will be granted. I speak now simply as a member and representative of that Society, and not as a Commissioner.

To the Honorable Senate and House of Representatives of the Commonwealth of Massachusetts :—

The undersigned, president and secretary of the Worcester South Agricultural Society, would respectfully represent, that whereas, the disease of cattle, called "pleuro-pneumonia," has been spreading to an alarming degree, within the limits of said society, and whereas the most active, persevering and judicious efforts of the Commissioners, to the full extent of the legal authority invested in them, have failed to arrest said disease,

Therefore, at a meeting of said society, duly called, and held the 26th instant, "to see if the society will adopt any measures to aid in staying the progress of the cattle disease now prevailing, and known as pleuro-pneumonia," it was voted that the society, through their president and secretary, memorialize the legislature to make the most *effective* provisions by legislative enactments, for the suppression of said disease.

All which is most humbly and prayerfully submitted.

Calvin P. Fiske, *President.*

David Wight, Jr., *Secretary.*

Sturbridge, May 28, 1860.

Mr. Walker.—I would ask that the President of the Worcester Centre Agricultural Society be requested to state the views of that society, and any action which may have been taken by them on this subject.

The motion was put and carried, and

Wm. S. Lincoln, Esq., in compliance with this request, said :—I can only say, in addition to what has been said this morning, that at a very full meeting of our society,—fuller

than I ever saw before—called after an unusually long notice, that the members might have time to make arrangements to attend conveniently—after a long discussion, in which different members of the society, some of whom had been with the Commission in its later operations, expressed the opinion that there was no possible way to eradicate the disease save by killing,—the society expressed that as their opinion, unanimously; and they instructed the Hon. Mr. Brooks and myself to make that representation to the legislature, as the expression of their opinion—that that was the only practical way to remove the disease from our midst, at any expense—and it was stated very frankly, that the expense would probably be very great. The members of that society will certainly cheerfully bear their proportion. I can only say, that since that time, the disease has shown itself in other localities in the county, and there is still more serious feeling in regard to it.

Hon. John Brooks, of Princeton.—I will only say, in addition to what has been said by my colleague, that I think the feelings of the people in that section of the State demand that some strong measures should be taken to arrest the progress of the disease, at any expense, even (I have heard it suggested) as high as a million of money. They would cheerfully pay their proportion of a tax of a million dollars, and be better satisfied even if they did not by the expenditure of that large sum arrest the disease, than if they had not made the attempt to arrest it. I have no doubt that is the feeling through the whole county of Worcester. I know that it is in the town where I live, where we are largely interested in stock, and have as good stock, perhaps, as any other town in the county—getting as large premiums at the County Fairs. In 1857–'58, when I was in the legislature, I voted for a State tax of $999,999, which comes pretty near a million of money. The proportion of the town of Princeton was somewhere between $900 and $1,000; and the people all say they would rather pay such a tax than not take some vigorous measures to arrest the disease. I think if the Commissioners should be authorized to go on and do any thing they saw fit, they would cheerfully pay a tax of $1,000 rather than not make the attempt. We have already got $500 subscribed in our small town, voluntarily, to back up the Commissioners and

their action, and this is a pretty good proof that we could get a thousand very easily.

Adjourned to Tuesday, at 9 o'clock, A. M.

THIRD DAY.

TUESDAY, June 5.

Hearing resumed at 9¼, A. M.

Dr. Calvin Ellis called.

Q.—What is your profession?

A.—I am a physician.

Q.—Where?

A.—In this city.

Q.—Have you made any examinations in connection with this disease of pleuro-pneumonia among cattle? and if so, please state what they have been.

A.—I have made very few indeed, and they have been confined to fragments of lungs, which have been brought me, which were said to have come from animals which died of this disease which is under discussion.

Q.—Do you know where it was that they died?

A.—I cannot be sure of the exact place where the cattle were sick, at the time I received the specimens, as the first was received two months ago. I think the last came from Belmont, I received them from Dr. Bowditch, who received them from Dr. Dalton.

Q.—Where did you get the others?

A.—One was brought here from Dr. Dadd, and one came from Dr. Jackson, by the hands of another person. It was brought by a medical student. This was an entire lung, the only lung I have seen.

Q.—Where did that come from?

A.—I cannot tell; it was stated, at the time, but I have forgotten.

Q.—Did not Dr. Dadd state where those brought by him came from?

A.—I think he did, at the time, but I have forgotten.

Q.—Were they reported to be specimens from cases of pleuro-pneumonia?

A.—They were, all of them, stated to me to be specimens from animals dying with the pleuro-pneumonia.

Q.—State the appearances.

A.—The appearances have all been different from each other.

Q.—Were these examinations made with the naked eye, or microscopically?

A.—Both with the naked eye and with the microscope. One specimen, an entire lung, said to be that of an animal in the earliest stage of the disease, was remarkable only for the great vascularity of it; there was more blood in some parts than others, and a peculiar mottling caused by that. This local discoloration was such as I have seen in human beings, where there was no disease suspected to be in the lungs. I do not mean to say there was no disease here. I am unacquainted with the organs of animals, and it may be that upon seeing a series of specimens, this might prove to be, as was said, the disease in an early stage, but it was simply a fulness of certain vessels; but I am not able anatomically to distinguish the natural appearance after death from the appearance caused by disease.

Q.—Did you make any examinations to see whether this discoloration was the result of inflammation, or simply of coagulation?

A.—I simply examined them with a microscope, and noticed nothing remarkable, so far as I could judge.

Q.—Does this apply to all of them?

A.—This was only one specimen. The specimens, as I have stated, differed entirely from each other.

Q.—Were any of these reported to be, or did they appear to be, from examination, in the advanced stages of the disease?

A.—Yes, Sir; some of the others. The first was a specimen brought to me by a Mr. Hazelton.

I will state the persons from whom they were received, and then say where they came from. I merely examined them as specimens of the disease, and not knowing I should be called upon to testify. Further, I did not interest myself in the exact localities from which they came.

Q.—Was the specimen you just described, the whole lung?

A.—I saw the whole lung, but limited portions of the lung only were affected in this way. This was reported to be a recent case.

Q.—Pass on to the next case.

A.—The specimens brought by Dr. Dalton, I think, from Belmont, which I received from Dr. Bowditch, presented appearances entirely different from these which I have described. And they were not the same. I found two different diseases, apparently, in these specimens which came from Dr. Dalton.

Q.—Did you have more than one specimen from Dr. Dalton?

A.—I think there were three or four pieces, some healthy and some diseased.

Q.—From the same, or different animals?

A.—They were from two animals. There were portions of healthy lungs, I think, in these specimens. I had two specimens of healthy lungs, and two of diseased; but I think the diseased portions came from the same animal. But I will not testify as to that; I will simply state what I saw in these specimens.

Dr. Loring.—I will simply state, to the Committee, that Dr. Wood, who furnished the specimens, says the diseased specimens came from different animals,—a piece from each of the cows killed.

Witness.—I will say that I did not examine these with reference to any examination here, or I should have been more accurate in my information with regard to their source. One of the diseased portions was light-colored, and contained a great many firm nodules, to the feeling. I expected, on incision, to find that the substance of the lung was filled with some solidified interior: but the nodules seemed to be formed of healthy tissue, surrounded by dense fibrous substance, as if the cellular tissues had become thickened.

The other diseased portion was red, and contained quite a number of small, yellow nodules. In these nodules I found, on microscopic examination, pulmonary tissue infiltrated with small globular corpuscules, resembling those we see in inflammations.

In the other specimen, handed me by Dr. Dadd a number of weeks since, the appearances were entirely different from those which I have described, and different from any thing I have

ever seen. In that, yellow, or brownish-yellow masses, of various sizes, lay in the substance of the lung, free, but surrounded by a dense fibrous membrane.

Q.—Do you call that a cyst?

A.—That is what would be considered a cyst. These yellow masses which lay within the substance of the lung, proved to be the tissue of the lung itself, filled with the same granular matter as that which I have previously described.

This is, I think, all I have to communicate upon the subject. It is all the information I have.

Q.—Does that membrane, or cyst, exclude the air?

A.—I should say it would. I noticed no openings. I did not examine it very closely, to see if there were any. It appeared to be closed.

Q.—What would become of that enclosed matter?

A.—It is impossible for me to say. I never saw any thing of the kind.

Q.—What is the medical theory in regard to such formations in the lungs, supposing the animal recovers? Does it become absorbed?

A.—I never have known of just such a substance in the lung, before: but I should say, if the animal did recover, making use of such information as I have about human beings, that this would become drier, shrivel in size, and, perhaps, become calcareous, and remain surrounded by this cyst.

Q.—You do not think it would be absorbed?

A.—I don't know how far it would go: to a certain extent, it might be absorbed.

Dr. LORING.—I was asked if I thought the lining membrane of this cyst was capable of absorbing this substance. I thought not. What do you think?

Witness.—I think, to a certain extent, the watery parts would exude. I do not think the absorbents would be active enough to take up the whole of this substance.

Q.—Did you examine the sac, or covering, to see whether it was provided with absorbents?

A.—I did not.

Q.—Is this cyst, with the contents in it, wholly exceptional in the morbid anatomy of the lungs, so far as you know?

A.—Oh, you have cysts in the lungs.

Q.—Enclosing such a substance, I mean.

A.—I have never seen any thing of the kind, before; it was entirely new to me.

Q.—Is there nothing corresponding to it, in what has been disclosed by the morbid anatomy of either men or animals, heretofore?

A.—You may have portions of the lung separated in human beings; they may become gangrenous, and separated; but I refer to this peculiar cyst surrounding this dense mass, without any gangrene, or any bad odor, which in ordinary cases attend the formation of a cyst in a human being.

Q.—It was not a tubercle?

A.—Not as I understand it.

Q.—You never saw an entire lung?

A.—I have seen one entire lung; but, as I have stated, I could not, from the knowledge I have of the human subject, at least, say that this lung was diseased at all.

Q.—Did you make a section, and divide that entire lung, or only examine it externally?

A.—Oh, yes, Sir; I divided it, and cut into various parts, to ascertain its character.

Q.—I suppose the law that tuberculous disease of the lungs is confined more to the upper than lower portion of the lungs, usually, is true?

A.—It certainly is true.

Q.—Was this the upper or lower portion of the lung?

A.—As far as I can remember, the red portions appeared to be scattered throughout the lung.

Q.—But this cyst?

A.—It was not stated to me. I could not tell from the appearance of it. I have never seen an entire lung which enabled me to determine that.

Dr. Loring.—My observation is, that this lies in the lower lobe, not in the upper, and, therefore, differs from any thing in the human subject.

Witness.—From what I saw, I thought it more like pneumonia than any thing else I had seen. The external membrane was thickened, in one case,—decidedly like pleurisy.

Q.—You said one of the diseased portions was light-colored, and contained many firm nodules. How do you account for

the light color? By the distribution of nodules through it, or was it the general appearance of the lung?

A.—It contained less blood than the other. I cannot explain that: but the firmness was very peculiar, and was not explained by the presence of any masses of the size you would expect. But the tissue of the lung itself appeared to be enclosed in this thickened, fibrous membrane.

Q.—It had more density than ordinary lungs?

A.—Yes, Sir.

Q.—It cut differently under the knife?

A.—Yes, Sir.

Q.—There was nothing of that fine crepitus you usually find in cutting into a healthy lung?

A.—I cannot tell, as to that. The lung contained air. It appeared to affect the inter-cellular substance.

Dr. Loring.—I have stated that, in cases I have seen, it reminded me of the coating of a young, not thoroughly organized muscle, as of young veal, or chicken. It had the same kind of solid—semi-solid—consistence.

Q.—Does that example I have given correspond with your observation?

A.—Yes. It was different from any healthy lung I have seen in any human being.

Q.—Have you ever seen any thing in the human subject, analogous to this,—a portion of the lung surrounded by a dense, fibrous substance?

A.—I do not think I have; I have no such recollection.

Q.—Should you think this the result of acute and active inflammation, so far as you can judge?

A.—This of which we have just been speaking did not appear to be the result of an acute disease.

Q.—Should you not think it the result of sub-acute disease?

A.—I did not, really, see any sign of inflammation in this small portion. It looked as if it might have been the result of inflammation.

Q.—Do you think that one of these stages—that is, this condition of this light-colored lung containing these nodules—could have passed on to that stage in which you found the cyst?

A.—I should think not, from the appearance.

Q.—Did it occur to you that this first specimen might be of the same disease, more diffused throughout the body of the lung, than that of specimens which had the cyst, and was confined and circumscribed ?

A.—I saw no sign of any process in the small specimens I have spoken of, which would lead me to believe active changes were going on, at all. It was entirely new to me, and I could not connect them at all.

Q.—Any thing in the contents of this cyst, like this substance ?

A.—I think not.

Q.—They seemed to be of different substance, entirely ?

A.—Yes. That is, these nodules, which lay very near the cyst, were firm and dense masses. It was only upon close examination that they seemed to bear any resemblance to pulmonary tissues, and then only by showing blood-vessels, and so forth.

Q.—Do you think this was one stage of either of these two other forms of disease which you have described ?

A.—I should say it might be an early stage of a form of disease which would terminate, perhaps, in separation. That is, the lung was undergoing the same change, in limited portions, which it had undergone in the other specimens, where the tissue was separate.

Q.—You should think, then, one an early, and the other a late stage of the disease ?

A.—It might be. They were entirely different from each other. It is possible.

Q.—By what process can one portion of the tissue of an organ become separated from another portion, in the living system ? What is the process of nature which would produce that separation ?

A.—That is done by the absorption of a certain tissue on the confines of the diseased and healthy parts. It is a mystery, just how it is done; but that is the result. A line of tissue is absorbed, really, so that one part is separated from another.

Q.—Usually, where that process commences, in nature, what is the final disposition of nature, relative to the cast-off and separated portions ? Does it become absorbed down and cast off by absorption, or otherwise ?

A.—That depends entirely upon the tissue separated. If in bone, in one way; if in the soft parts, in another.

Q.—What is the ordinary way in the soft parts?

A.—As I have stated, I have seen nothing which was precisely like this specimen of which I was speaking. Usually, where the part is separated in the soft tissues of the body, it becomes gangrenous, and will mostly liquefy.

Q.—And then is removed by absorption, unless it is situated where, by an abscess, it might be absorbed?

A.—Yes, Sir; it might be discharged by an abscess, or parts of it may be absorbed.

Q.—Had you been familiar with the appearance of the lungs of healthy animals, before making these examinations?

A.—No, Sir; not at all.

Q.—You never had examined any?

A.—No, Sir.

Q.—What should you think would be the chance of the recovery of an animal that contained this detached portion of lung,—say, a very considerable portion of one lung?

A.—I should think, where large portions were separated, the chance would be very small. Nature works, sometimes, in a remarkable manner. But I should say, where the disease had been extensive, the chance would be very small.

Q.—Suppose one lung only was affected by it; do you think the chance of recovery very small, then?

A.—Human beings may live with one lung—what amounts to one lung, or, perhaps, less.

Q.—But *do* they not?

A.—They do.

Q.—How with the animal creation?

A.—I know nothing about them.

Q.—Do you think a man could do a good day's work, with one lung, or a half one?

A.—Well, they do a great deal, certainly.

Q.—Have you ever seen an able-bodied, healthy, hearty man, doing a fair day's work as a laborer, that you were aware had only one lung?

A.—Yes, Sir; I remember one case, in particular, where a man died of an acute attack of pleurisy. Just before that, he had been employed as a laborer, doing at least hard work,

in some manufacturing establishment. Years before, he had had an attack of pleurisy, which had absolutely destroyed the whole, or almost the whole, of one lung. There was nothing of any consequence retained. There had been a remarkable change in the chest. It had adapted itself so that there was a very dense formation of fibrous tissue between the chest and the lung itself, partly filled up by that, and partly by the lung.

Q.—What should you infer, as a general rule?

A.—As a general rule,—that a person with one lung is not as well off as a person with two.

Q.—I suppose it would depend somewhat upon the cause of the destruction of the lung—whether done by an acute disease, like pleurisy, or a chronic disease, like phthisis?

A.—Very decidedly.

Q.—The character of the disease would, then, depend very essentially upon the constitution of the patient?

A.—Yes, Sir.

Q.—In the case you name, might not the full amount of labor which this man performed have had something to do with the last attack, which carried him off,—predisposing him to it?

A.—I cannot state how that is. I don't see why it should.

Q.—If he had but one lung left, and had pleurisy in that, he would be a little more likely to die?

A.—Oh, yes; but it is a question of predisposition to pleurisy.

Q.—I refer to the excessive labor as exhausting his vital energies?

A.—That simply implies a very active interchange between the external air and that which passes through the lungs,—and pleurisy is external to the lung.

Q.—Dr. Martin gave it as his opinion, the other day, that these cysts indicated unusual activity of the recuperative function of nature, in saving the rest of the lung, and restoring to health. What do you think about that?

A.—I think, certainly, that they indicate a great effort of nature to restore.

Q.—And that, as far as he knew, this was more active in the lower class of animals than in man.

A.—I know nothing about that.

Q.—Upon the whole, in these cases of diseased lungs, what conclusion have you come to?—that they were all affected by the same, or different diseases?

A.—I could come to no conclusion. I had detached portions brought me, of which the appearances were entirely different. Any conclusion of mine as to the identity of the disease must be based upon information brought to me from outside. I could give no opinion, except that the inflammation, which is limited in one case, here, might terminate in a separation.

Evidence of John E. Chaffee.

Witness.—About sixty days ago, a cow was driven into our town, from Brookfield, and she was taken sick, and put up at a barn where a gentleman had a cow, and the latter animal was exposed, from that Brookfield cow. Fifty days after this cow was exposed, it was killed. Dr. Thayer was there from Boston, and Mr. Brooks, of Princeton. The cow was examined before it was killed; and after it was killed, Dr. Thayer stated it was a marked case of the disease. About twenty days ago, that cow went to pasture with other cattle; and in twenty days, we slaughtered another cow, that had marked signs of the disease.

Q.—Which cow went to pasture with other cattle?

A.—The one that was afterwards killed in presence of Dr. Thayer. There were other cattle in the pasture.

We slaughtered another cow. This cow was examined by Dr. Bates, together with Mr. Lincoln, the President of the Agricultural Society, and that cow was pronounced by Dr. Bates to be diseased. We accordingly had it slaughtered, and found that it was diseased—in the first stages, as stated by Dr. Bates.

Q.—What became of the Brookfield cow?

A.—It was sold from Brookfield, and went to Pepperell. The papers say this cow has been killed in Pepperell. I have heard she was diseased, and had been killed.

The second cow we killed in Holden. She had symptoms of the disease. The first cow did not show any marked symptoms; but the citizens of the town did not feel satisfied to let her remain without satisfying themselves, and they accordingly had her slaughtered.

Q.—And she showed no marks?

A.—Not outwardly—not particularly.

Q.—Did she inwardly?

A.—She did. One of her lungs was very much diseased. Dr. Thayer examined the diseased lung.

Q.—Was the cow that went to Pepperell sick at Pepperell?

A.—She was. That was the reason she was put up at that barn.

Q.—Was she sick when she went to Pepperell?

A.—I can't say, Sir.

Q.—How far is your place from Brookfield?

A.—Perhaps twenty miles.

Many cattle in our town have now been exposed by this Holden cow, which took the disease from the Brookfield cow; and the disease now seems to be fast spreading in the town.

Q.—What evidence is there that it is fast spreading?

A.—There have been some symptoms—what the physicians call symptoms—of the disease; and many cattle have been exposed from the first cow.

Q.—What are the symptoms?

A.—Symptoms that physicians give. I do not profess to know, myself; I am not a physician.

Q.—How do you know there are such symptoms?

A.—I take it from physicians who have stated that before me.

Q.—How many cattle were there in the pasture where this Holden cow went?

A.—I think in the pasture there were six; but in going to pasture, other cattle have been exposed in the roads.

Q.—Did those six belong to one herd?

A.—No, Sir.

Q.—Did they merely belong to individuals who keep cows in this common pasture?

A.—There was one who had three or four cattle; and there were three others, I think.

Q.—Did they belong to four herds, or to three herds?

A.—To four different herds, I think. I don't know that there were more than three.

Q.—Where are, now, those six cattle?

A.—Two or three of them we keep in barns, and the others are where no other cattle can get to them.

Q.—They are, then, isolated ?

A.—Yes, Sir.

Q.—Do you know of any other cattle except those six ?

A.—I have not seen them, myself. It has been reported to us that there are other cattle that have been exposed, by way of oxen that were in teams, passing in the road. And I was informed that in two different herds there are cattle that have showed the symptoms.

Q.—You mean, they were exposed by being in teams, on the road, when these cattle were passing them ?

A.—Yes, Sir. And those herds have showed symptoms of the disease.

Q.—Are you one of the selectmen ?

A.—Yes, Sir.

Q.—Did these reports come to you as selectman ?

A.—Yes, Sir.

Q.—Did they come from an accredited source ?

A.—Yes, Sir.

Q.—When did these reports come ?

A.—Yesterday.

Q.—How did they come ?

A.—The owners of the herds came and reported them.

Q.—Had they taken means to isolate them ?

A.—Yes, Sir, they had.

There seems to be, on the contrary, on the part of many people of our town, a disposition, rather than to isolate their cattle, or keep them at home, to let them go at large. On the Sabbath day, one or two herds were running at large in the roads, and one of the herds that had been exposed to the first cow.

Q.—Why was that done ?

A.—The gentleman who owned them thought there was no danger, and made light of it when we tried to persuade him to keep them enclosed. Yesterday, he concluded it was best to keep them enclosed, and has determined to do so.

Q.—Did he not believe the disease was contagious ?

A.—Not until two or three days ago.

Q.—Did he think there was any disease?

A.—He did not have much faith in it.

Q.—He did not know his cattle were sick?

A.—No, Sir. Two days ago, his cattle were exposed to the diseased cattle. The sick cows were in the yard, and two or three of them came together. There was a fence between them.

Yesterday, a foreigner was determined to drive a creature through the town, and came through this part of the town where this disease is, resolved to go through to an adjoining town. We had to use considerable effort to prevent his going. He finally consented not to go. He threatened to go last night.

Q.—How many cattle do you consider have been exposed, in and about Holden?

A.—I cannot tell, exactly. Perhaps there may be fifty, or more.

Q.—How many are reported as having been exposed?

A.—From the first cow? We think there may, perhaps, be a dozen of them.

Q.—Do you mean, the first cow killed, or the Pepperell cow?

A.—I mean the first cow we killed,—the Holden cow. And if the other cattle were capable of giving the disease, many others have been exposed.

Q.—How extensive are the reports upon the subject, so far as they are authentic? How many are reported to have been exposed?

A.—I should not be able to say.

Q.—Did this Holden cow stand in the barn with the West Brookfield cow?

A.—It did, Sir.

Q.—How long were they there together?

A.—One night. They stood about ten or twelve feet apart, with three or four partitions, like horse-stalls, between them.

Q.—Did the stalls cover up the whole room?

A.—No, Sir. About like horse-stalls,—partial partitions.

Q.—How much territory do you think has been exposed?

A.—From two to three miles, one way.

—On one road?

A.—Nearly all. Very near one road. It extends to the line of Princeton. We had no power with us to use any efforts or means to stop the spread of the disease.

Q.—Do you believe that isolation could be effected perfectly? Can the people be trusted?

A.—I should think not, Sir. We should hardly know where to commence.

Q.—Could it not be done, if you had the power?

A.—I don't know but it could; but we should hardly know where to commence.

Q.—Commence where you find it.

A.—We should hardly know where to leave off, then.

Q.—The cow you speak of, from Brookfield, was not kept at Holden, at all?

A.—No, Sir.

Q.—Taken to Pepperell, by a man who bought it in Pepperell?

A.—I understand so.

Q.—Merely stopped over night?

A.—Yes, Sir.

Q.—What would you like to have the power to do?

A.—We would like to have some power given to the town, or selectmen, that, so far as we may be able, we might stop the spread of the disease, and keep it from going out of town, or prevent it from coming into town.

Q.—What particular things do you advise you should be authorized to do?

A.—We hardly know, Sir, what to advise. We would like some speedy action.

Q.—Does any thing but killing or isolation suggest itself to you?

A.—I don't know that there is, for this disease. We want to know how to prevent its going further.

Evidence of John Brooks.

Witness.—I was selected by the Commissioners, and went the fifth day of May to visit Pepperell, and trace back the Brookfield cow to her starting from Brookfield. I found that on the tenth day of April she travelled to Holden, and stayed

there over night. She was tied up in a barn with a cow in Holden, belonging to Benjamin Dyke.

Q.—Only one creature?

A.—There were two creatures, but one was only a calf. The distance she stood from Mr. Dyke's cow was about twelve or fourteen feet. The intervening space was occupied by two horse-stalls, involving a necessity of two partitions,—the air circulating freely around those partitions. She then, on the eleventh, in the morning, started for Pepperell, and was found to be fatigued and giving out; and the man stopped her at North Lancaster, at Mr. Sabin Woodbury's house, put her in an open shed, and she stood there two nights and one day. The floor she had was rough, and was five or six feet below the sill of a barn where a cow stood belonging to Mr. Woodbury, about forty or fifty feet distance from this diseased cow. She stood there two nights and one day. The cow was killed on the twenty-fifth day of May.

Q.—Did she go to Pepperell when she next started?

A.—Yes, Sir; she next went to Pepperell, to Mr. Wood's. We killed her, and found her diseased. We went back to Mr. Sabin Woodbury's, examined his cow, found that she had the disease, put an injunction upon her, and left her, till last Thursday. We went to Mr. Dyke's, and could discover no symptoms of disease in his cow, at the time, and left her, putting an injunction upon her. A few days afterwards, there was some alarm, and the Commissioners directed me to go to Lancaster, and slaughter Mr. Woodbury's cow. They were much alarmed at Lancaster, and wanted to know as to the facts of the case. I got a contract with the selectmen to pay Mr. Woodbury, as at Pepperell, and to wait for the reimbursement of the town for the next legislative session,—if they ever got any. We found the cow partially diseased. There were marks of the disease, Dr. Thayer thought, but not very fully developed.

On Thursday afternoon, I went to Holden, and destroyed that cow there. She was very considerably diseased. The people of Holden became alarmed; and we went through the same process,—killing Mr. Dyke's cow, and they agreeing to wait until the legislature should pay the town or not, as they saw fit.

That was the history of the Brookfield cow and her exposure to other cows.

There were three or four other cows, when I went to Holden, in the pasture with the Dyke cow. She appeared so well, I thought it unnecessary to take any precautions with her, then.

The calf that stood in the barn with the Holden cow was killed, but did not appear to be diseased at all.

Q.—How near did the cow at Lancaster stand?

A.—From thirty to forty feet, and six feet above, and inside the barn. That seems to prove the fact which the Commissioners have observed,—that the nearer an animal stands to the diseased creature, the more likely it will be to take the disease, and the more violently. I think that is the fact observed in Europe, that the cow standing nearer the diseased animal had it more violently than the one further off. And this was the case at Lancaster.

Q.—That was not the testimony of Dr. Wood.

A.—That was, I believe, the testimony of Dr. Thayer; and I believed it to be the testimony of all the physicians.

Q.—Did this Holden cow exhibit external marks of the disease?

A.—Not so directly. There were external marks. She drooled a little, and stood with her legs rather wide apart, and seemed to be reluctant to walk—walked as if confined in the shoulders, or as if she had a soreness in the chest. There were no short breathings. Dr. Thayer, however, could state it better than I could. The cow did give marks of drooling, and a little running at the eyes.

Q.—She was so free from disease as to make it right and equitable that her value should be paid for?

A.—That is what the doctor thought. He could not discover, at first, positive marks of disease in the cow; but, after killing her, there appeared extensive marks of disease in her.

Q.—How with the Lancaster cow?

A.—She showed marks of disease by short breathing. Her respirations were twenty-two or twenty-four. Her breathing was short, and she evidently had disease of the lungs. Dr. Thayer will give you the autopsy of the cow, better than I can.

Q.—How long was it, after the Lancaster cow was exposed, before she was killed.

A.—She was exposed on the 11th day of April, and last Thursday she was killed. She was driven from Brookfield on the 10th, from Holden to Lancaster on the 11th. That commenced her exposure at Mr. Woodbury's, and she stayed there one day and two nights.

Q.—The cow was bought in Brookfield?

A.—Bought in Brookfield by Mr. Wood. There are two brothers of that name, one in Brookfield and one in Pepperell; and the one in Brookfield sold this cow to his brother in Pepperell.

Q.—Did he know that the cow was exposed?

A.—I suppose so; for he wrote to his brother that it had been exposed, and that he had better take care.

Q.—Did he know it at the time?

A.—No; he did not know it at the time. That cow at Pepperell exposed some other animals, and we have not yet heard whether or not they have been sick.

Q.—You say he did not know at the time, that this cow had been exposed in West Brookfield. Had not the disease prevailed?

A.—I believe it had prevailed. It had prevailed in North Brookfield. I don't know as to that, however; I speak from remembrance.

Q.—Had there been cases at North Brookfield?

A.—I suppose so; I know nothing of the fact.

Q.—Then you do not know that even the Pepperell cow had been exposed before leaving Brookfield?

A.—I know nothing about it except from hearsay, that the cow came from Mr. Wood's, in Brookfield, and was afterwards destroyed.

Q.—At the time the cow was sold, no one knew that it had been exposed?

A.—I don't know, Sir.

Q.—You don't know that it had been exposed at all?

A.—No, Sir; I don't know it. I suppose there is no doubt about it.

Q.—Has there been, to your knowledge, any disposition on the part of persons owning cattle that have been exposed, to sell them and get rid of them?

A.—No, Sir; there has not any such disposition, to my knowledge.

Q.—Have you attended, Mr. Brooks, many of these examinations and appraisals of cattle that have been made?

A.—No, Sir; only three—those I have named.

Evidence of Dr. E. F. Thayer, (recalled.)

Mr. BIRD.—You examined this Lancaster cow?

A.—I did.

Q.—What were the indications of disease at the autopsy?

A.—I could not decide it to be a decided case of disease. There was some slight abnormal appearance of the lungs, but not sufficient to decide it a decided case.

Q.—What were the indications before death?

A.—The respirations were twenty-three a minute, and evidently, on the right side, were tubular.

Q.—What were the external indications?

A.—The external indications were that she was healthy, but I expected to find some disease of the lungs.

Q.—Any enlargement of the lungs?

A.—The lungs were a little enlarged.

Q.—Do you know how long she had been exposed?

A.—The report was that she was left there on the 11th of April over night, in a shed: not put into the barn at all. It was said, at one time, that she had drank out of a bucket used by the cattle there, but I don't think that could readily convey the disease. Two months previous, parturition had taken place; she had a hard time, and had been feeble ever since. The first time I was there, she was worse than when I saw her the second time; she was then evidently improving. There were only a few days between the visits.

Q.—What was the appearance of the heart?

A.—It was flabby and flaccid.

Q.—You do not pronounce it a case of the disease?

A.—No, Sir.

Dr. LORING.—You did so regard it, before she was killed?

A.—I expected to find it so.

Q.—You found somewhat the same symptoms?

A.—Somewhat the same.

Q.—Did you not consider, from the symptoms, that this was probably the disease?

A.—I did, Sir.

Q.—Under these circumstances,—if you knew an animal had been exposed to this disease called pleuro-pneumonia, if you were satisfied of it, by your own observation, and you afterwards found disease in the lungs, both by physical signs before death, or by autopsy, of course after death, would you not infer that the disease in the cow was pleuro-pneumonia?

A.—I should, of course.

Mr. WENTWORTH.—You would not infer it, unless the facts warranted it?

A.—That was what he [Dr. Loring] said.

Dr. LORING.—I meant to ask, if you know that an animal has been exposed to pleuro-pneumonia, and you find it a difficult case to examine before death, and the autopsy is not precisely satisfactory, but, at the same time, there is disease of the lungs, whether you would not infer that that disease,—hidden, concealed as it was,—was pleuro-pneumonia?

A.—From my knowledge of the character of the disease, I certainly should, so insidious is it in its character.

Dr. CHOATE.—I wish to ask if the symptoms and appearances in that case, both before and after the autopsy, were not such as you would expect to find in a case of the commencement of pleuro-pneumonia?

A.—There was an absence of something that I expected to find?

Q.—But so far as they went?

A.—So far as they went, they were. There was some slight appearance of what I was fearful might be the disease, yet I could not pronounce it decidedly so. I would state the fact, that the inter-lobular tissue had a slightly marbled appearance, and this I regard as a true indication of the disease.

Q.—Did you weigh the lungs?

A.—Yes; they were light.

Q.—Do you believe that that was a case of the disease called pleuro-pneumonia, derived from contagion?

A.—In my report, I have stated that it was unsatisfactory; that I could not decide it to be so; but that the appearances presented by the *post mortem* examination might be accounted

for by the feeble condition of the animal. I cannot state it any more decidedly.

Q.—Where is your report?

A.—It is in the office of the Secretary of the Board of Agriculture.

Q.—What appearances did you observe on the slaughter of the Holliston cow, last week?

A.—On removing the left lung, several spots of red hepatization appeared; also, a slight hypertrophy, or enlargement, of that lung. We removed the right lung, and found it considerably enlarged, and the middle and inferior portions were studded with red spots of hepatization—a very marked case.

As some questions have been asked in reference to her appearance before death, I will state, that on my arrival there, the animal was in the road, and there were a very large number of people present. She was driven into the barn, but I was unable to make any satisfactory examination. She appeared decidedly excited, and I could not take her respirations at all. They were not regular. She would turn to one man and another and snuff, and turn to me and snuff. I could not get a decidedly regular breathing. There was slight tubular respiration in a portion of the right lung, but I accounted for that on the ground of her excitement. The sound of the air going through the bronchial tubes was greater than you would naturally expect in an animal when in a quiet state. I could not form the opinion that she was diseased, and I hoped, from the appearance of the stable,—two or three stalls intervening between the animals, a distance of some ten feet,—that she would not be diseased. Three physicians were present, and examined the case with me.

We afterwards went to a farm where a cow had died, and the owner, fearful that it might be reported that his herd was affected with this disease, came for me to go and examine the case. I found the animal lying on the sternum and abdomen, against a partition. On opening the abdomen, there was no appearance of disease; but on opening the uterus, a very large quantity of fluid—an unusual quantity—ran out; the fetus appeared to have been dead for many days; the skin puffed up to its utmost capacity by the disengagement of gas from the fluids, and perhaps the solids. I then took out the lungs,

and although, from the position in which the cow laid, there was congestion of the blood, yet the appearance was so markedly different from the other case, that the physicians were satisfied that there was a difference between the disease in the one case and in the other. They were satisfied that the first animal was quite extensively diseased.

Mr. LATHROP then stated that Mr. Chenery, being the unfortunate man who imported the stock which brought this disease, and having had the most experience in regard to it, would give his opinion as to the best way of extirpating it.

Mr. CHENERY.—I have a very decided opinion about it; perhaps not well founded, but founded upon my own experience, mainly, and upon the evidence I have heard here. My opinion is, that the best way is to slaughter every herd that is known to have diseased animals in it, at once, and to isolate thoroughly all those herds that are suspected, but not known to have diseased animals among them.

Q.—That is, those that have been exposed?

A.—Yes, Sir; that have been "exposed;" that is the term that seems to be used here.

Q.—What would you do with those herds that you knew had been exposed?

A.—I would slaughter the herds in which there were known to be diseased animals.

Q.—Then you would slaughter indiscriminately a whole herd, if there was one diseased animal in it?

A.—I would, decidedly. I don't see what use it would be to slaughter one.

Q.—Have you not got an animal that has not been diseased? Do you want to kill him?

A.—I do not want to kill him. My opinion was asked, and I gave it, without reference to myself.

Q.—Would you think it good policy to kill cattle that were isolated before they exhibited any symptoms of the disease? Why not wait until they showed symptoms?

A.—I would isolate herds that are suspected to have the disease, and if any animal became diseased, I would kill the whole herd.

Q.—Notwithstanding you know, from your own experience, that some animals do not take the disease?

A.—Yes, Sir.

Q.—Do you not think that we owe it to ourselves and to the world to examine into the disease, and see whether it cannot be cured?

A.—Yes, Sir; I think that would be well.

Q.—How would you do that, without isolating and watching the animals?

A.—I would do it by obtaining an island, if possible, and putting a diseased herd upon it, and introducing, from time to time, well cattle among them.

A.—For the purposes of experiment, I understand you to say that you would recommend procuring an island?

A.—Yes, Sir.

Q.—Would it be wise ever to let the cattle off the island?

A.—I should think that an experiment that did not extend over one or more years would not be satisfactory.

Q.—I suppose it would be satisfactory when it was ascertained that the disease could be cured?

A.—Yes, Sir; but I don't think it could be ascertained under a year or two, from the experience I have had. If the cow that went from my herd to Malden carried the disease seven months, it seems to me that is conclusive.

Q.—But suppose you ascertain that you can cure the disease, then the experiment would have succeeded, would it not?—you would not continue the isolation then?

A.—No, Sir; but, as I said, I think it would take a year or more to ascertain that fact.

Q.—How many cattle out of a hundred would be worth curing, if that length of time were required?

A.—I don't know.

Q.—Would not the risk from attempting to isolate them, from the danger of their breaking out, &c., be such as to render it safer to destroy them?

A.—I think it would.

Q.—Taking the view you have expressed, what value do you put upon your cattle?

A.—I am not prepared to answer that question.

Q.—If you could have your way, would you allow any animals to come from the island that might have recovered, while any diseased animals were upon it?

A.—No, Sir. I suppose the isolation to be for the purpose of experiment, not to save the animals.

Evidence of Dr. Samuel A. Green, of Boston.

Mr. ANDREW.—Have you made any investigations in reference to this disease?

A.—I have made such investigations as I could from books, but have not had an opportunity to see a diseased animal; I have seen several specimens of the lung.

Q.—How many specimens of the lung have you seen?

A.—Three.

Q.—Were they all alike, or did they represent the disease in different stages of its progress?

A.—I should think at different stages of progress, although they had been soaked in alcohol,—all of them,—which rendered them not so good specimens as they would have been if they had been more fresh.

Q.—Could you ascertain if they were morbid specimens, with what disease the animal was afflicted in its lifetime?

A.—They were morbid specimens; more than that I cannot say.

Q.—Not distinct and clear enough to enable you to form an opinion?

A.—No, Sir.

Q.—You have stated that you have examined this subject in books. How extensively is this subject of malignant pleuro-pneumonia treated in books?

A.—As far as I can judge, the accounts are exceedingly meagre, and very much scattered.

Q.—Both as regards the Continental and English?

A.—I have seen some French books, but mostly English. I would state that it is a subject that I looked into more for my own gratification than any thing else; I never dreamed of being called upon until this morning. It is something that I have had but very little conversation about with others.

Q.—Is there any treatise on the subject, or is all that is to be found in scattered leaves?

A.—It is in scattered leaves, and by persons who entertained different views.

Q.—Is this the fact, Doctor?—that as yet no theory touching the origin, progress, history and treatment of the disease has been agreed upon by men of science?

A.—I should say so, from what I know.

Q.—Have the French writers decided in regard to the curability of the disease?

A.—I don't think they advance any very definite opinion in regard to it.

Q.—Have you examined several different authorities both in English and in French?

A.—Yes, Sir.

Q.—Have any of them treated the subject of inoculation?

A.—That has been mentioned.

Q.—Do they agree upon that?

A.—I cannot say, Sir.

Q.—As to the contagiousness of the disease, what is known and decided?

A.—I should judge that to be an open question, on the authorities. I think Dr. Livingstone speaks of it in his African Expedition, and although it is some time since I looked at it, I think he does not agree to its contagiousness. I think, however, that his account refers to cases where horses and mules were affected—not neat cattle.

Q.—What part of Africa did he find it in?

A.—I cannot state, Sir. I think he considers it an epidemic.

Q.—As among horses?

A.—Yes.

Q.—This disease has prevailed on the banks of the Thames, has it not?

A.—It has prevailed in England. I cannot say about the banks of the Thames.

Q.—Is there any substantial agreement among English writers upon the subject?

A.—I should think there was great diversity of opinion.

Q.—As to curability and contagiousness?

A.—Yes, Sir; and as to the percentage of mortality, I found different accounts.

Q.—How widely do they differ as to the percentage of mortality?

A.—I think I have seen statements of over fifty per cent. recovering, and then, again, I have seen it stated at a very low rate. If I had had any thought that I should be called upon, I should have refreshed my memory, but it is something I have not looked into, to any great extent, since last winter, when the disease first made its appearance.

Mr. Bird.—Upon the whole, doctor, from what you know and have read, and from what you know of the facts in regard to the disease in this country, what should you recommend as the best course to be pursued?

A.—It is very difficult to say. There is considerable *prima facie* evidence of its contagiousness, but, then again, I don't think the proof is conclusive.

Q.—Contagious or not (that being an open question), should you recommend killing or isolation?

A.—I should not recommend killing, rather isolation.

Q.—What extent of neutral ground should be preserved between diseased cattle, and cattle in contiguous regions that are considered healthy?

A.—I should want to know more of the details of the disease, before I answered that question.

Q.—What is the general law of diseases known to be contagious, or infectious, as to distance?

A.—Under ordinary circumstances, I should not think it would be a great distance.

Q.—What is "a great distance"?—a rod, ten rods, or half a mile?

A.—I think it would be safe to call it half a mile, although I have no idea that it would be carried through the atmosphere to any thing like that extent, to be contagious.

Q.—Do you know of any well-authenticated case of disease that went that distance, or any where near it?

A.—No, Sir.

Dr. Loring.—Did you subject the specimens of lung which you saw to any very accurate examination?

A.—No, Sir.

Q.—Could you tell whether you had ever seen similar specimens coming from the human subject?

A.—These had been soaked in alcohol, and had become very much hardened, indurated, and in that respect they differed from the specimens I have seen from the human subject.

Q.—Those have been recent specimens?

A.—Yes, Sir.

Q.—Are you aware to what extent this disease has been investigated by competent persons in Europe?

A.—No, Sir.

Q.—Have you ever read the statement of Dr. Willems, of Belgium?

A.—I have not read the original, but I have seen extracts from it.

Q.—Have you ever seen the reports of the Scientific Commission appointed by the Minister of Agriculture in France?

A.—No, Sir.

Q.—Have you read the account of the examination of Prof. Simonds by a Committee of Parliament?

A.—No, Sir.

Q.—Have you ever read, in the transactions of the Royal Society, an elaborate essay upon inoculation?

A.—I have seen it, but I cannot say that I have read it. I have glanced over it.

Q.—Are you aware of the history of the disease in Denmark, as it has been explored there?

A.—I have seen accounts of it.

Mr. Charles L. Flint, Secretary of the State Board of Agriculture, was asked what his sources of information had been, and to give his views in regard to the matter, and said:—

I am in the constant receipt, officially, of a great many of the highest scientific and agricultural journals published in Europe: *The Journal d'Agriculture Pratique*, of France—considered one of the best—*The Journal of the Royal Agricultural Society*, and *The Farmers' Magazine*, London,—*The Journal of the High-*

land and Agricultural Society, Scotland, &c.; and I have also had frequent exchanges with the French government, receiving from them official documents and papers in regard to public investigations; and when this disease was first made public, of course my attention and interest were excited and attracted to the subject in a degree which they had never been before. I have informed myself so far as I have been able, with regard to its nature, with regard to its history in Europe, and I have also been with the Commissioners through all their earlier investigations, spending several days in North Brookfield and New Braintree.

Q.—Where did the disease originate, according to the accounts?

A.—There is a difference of opinion whether it can be connected, directly, with what is called the "steppe-murrain," which originally came up from Tartary to Italy, &c., or whether it is a specific disease, which originated in Europe, of a comparatively recent date; but this fact is established, that it was for a considerable time centred in Piedmont, round the Jura mountains, from which, about 1840, it began to spread down upon the lower countries of France, Belgium and Holland. In 1840 Dr. Delafond, a distinguished veterinary surgeon connected with the veterinary school of Alfort, was sent into the Seine Inferieure, one of the departments of France, where the disease had manifested itself, and after 1840 he was commissioned for several successive years to go into different departments, to investigate and struggle against this disease. The conclusion to which he arrived, and which is expressed in a recent work on the *Dairy Cow*, by a celebrated French author, is, that after the first stage it is incurable. It is generally admitted, as far as I am informed, by veterinarians and practical men, that after the first stage it is incurable.

Q.—What is the first stage?

A.—The period of incubation, commencing with exposure. The disease has been properly described by gentlemen here as a very insidious and stealthy one. The first stage is of course very obscure. The disease is almost universally admitted among practical men in Europe to be contagious. The length of time that should be considered the first stage, is somewhat unsettled. I do not think the limits of it have been or could

be fully defined, but in general, it might be called the period of incubation, which lasts perhaps from two to four weeks, and then is followed by what might be called the inflammatory stage. The next stage is where it gets hold of the organs and produces some disorganization. The symptoms in the first stage are very obscure; they are so obscure that they might not be detected even by a very careful scientific examination; and cases have come within my own observation where the disease must have lingered upon the animal several months before it was at all suspected or perceived by the owners, but upon careful examination by a veterinary surgeon, (and I have assisted in such examinations in many cases,) the lungs would be found decidedly diseased; and in nearly every case the *post mortem* has verified the previous examination. I suppose that the only curable stage is the first, or inflammatory one, and that practically it is incurable. Preventive measures are the truly effective ones.

Q.—It is not merely functional, but organical?

A.—*Organic*, it appears to me, after the first or inflammatory stage. I have no interest or feeling in the matter, except to have the truth and the facts made known, and I am so confirmed in my own opinion from having read the reports upon the disease, and from having followed it up in its details, that I am anxious that efficient and prompt measures should be taken in regard to it.

Q.—How are your impressions in regard to its being contagious and not epidemic?

A.—My opinion is perfectly settled. I may say that I *know* that it is contagious; and I have no idea that it is epidemic. That is the almost universal opinion among scientific and practical men in Europe, and the editors of agricultural journals. The *North British Agriculturist* has recently come out with an article on the local signs of the disease, as they have appeared here, and has taken up the details here of the cases where it has been carried from Belmont to North Brookfield, and has given that as an instance to prove conclusively that the disease is contagious. There was, on its first appearance, a difference of opinion in the minds of some as to its contagious character.

Q.—In your observation or reading, have you been convinced in regard to any preventive that can be administered, after exposure, before disease has taken place?

A.—No, Sir; I have not. I, of course, have heard Mr. Lindley, (who was early introduced to me by Dr. Anderson,) as to the results of inoculation; but on that point I had been previously informed by the reports of Dr. Willems, who discovered that process; since the disease has raged here, many have came to me with specifics, which they considered effective; but, as one of the Commissioners has said, [Dr. Loring,] their character was so apparent that I did not think the Committee would look at them.

Q.—Did they allow you to analyze the medicines?

A.—No, Sir; they kept them a profound secret, because they expected to make a great deal of money by them. One went so far as to say, that the Veterinary School of Berlin had not discovered his cure, although they had investigated the disease in every possible way.

Q.—Is there any systematic treatise on this subject, extant?

A.—No, Sir; I think not. There is, as Dr. Green remarked, a great deal of information scattered all through the different medical and scientific journals on this subject, but it is in an exceedingly fragmentary form. In the *Journal of the Royal Agricultural Society*, there are several elaborate, complete, and full papers, illustrating the condition of the lungs in different stages, compared with healthy lungs. These I have in my office, but I do not think the information has ever been collected together. There is a small popular treatise on it by M'Gillavray.

Q.—Has there ever been, any where in the world, any thorough and systematic treatment of the disease, under the direction of learned men, who had the power to do what was necessary to be done, in order to subject healthy animals to the contagion, and unhealthy animals to scientific treatment, for the purpose of cure?

A.—Yes, Sir. There was a commission established by the Belgian government and by the government of France, and I think by the Dutch government also, for the purpose of investigating and treating the disease by way of inoculation. That has been thoroughly tried and investigated, by learned, able, and scientific commissioners, appointed by those governments. The reports of those commissioners, or extracts from them, are to be found in the *London Veterinarian*, and other works, and

the results, as you have all heard in the testimony here, are not satisfactory.

Q.—Did you hear Mr. Lindley's testimony ?

A.—Yes, Sir.

Q.—What do you think of the results in Africa ?

A.—I have no doubt he stated what is strictly true in regard to the results there; but they are more favorable than have been shown in Europe. Whether this is owing to the climate, or to the fact that they have tried it on a more extensive scale, I cannot say. I showed Mr. Lindley the reports of those commissioners, and after looking over them carefully, he said that if they should try inoculation in Africa, under such circumstances, they should not expect any better results. He stated to me that they did not expect any good results from inoculating animals after they had been exposed any considerable time to the infection. Many of the animals used by the Belgian commissioners for their experiments were taken from herds where the existence of the infection had been known for a considerable length of time.

Mr. ANDREW.—Then, supposing it to be a contagious disease, inoculation was not tried by those commissioners early enough to give it an opportunity to gain a start upon the infection ?

A.—That is what I understand to be the opinion of Mr. Lindley.

Q.—Then, so far as that is concerned, it has not been thoroughly and scientifically tried in Europe ?

A.—I think, myself, it has been pretty thoroughly tried. Although Mr. Willems, the discoverer, has met with a great deal of opposition, and it has not, perhaps, been tried so thoroughly and completely as it ought to have been, still, it has been pretty thoroughly tried—there is no doubt about it. In those cases where the cattle have never been exposed, the result has been perhaps, more favorable in Europe ; still, there has been a considerable percentage of loss by inoculation,—so great, perhaps, that it would not be thought expedient to try it here.

Q.—Should you expect any good results from inoculation, after the disease had made its appearance, any more than from vaccination after smallpox had commenced ?

A.—No, Sir. But I do not understand that to be exactly the case. After infection had been known to be in a herd, animals were taken from such a herd and inoculated, with those results.

A MEMBER.—Vaccination will prevent the smallpox after exposure.

Witness.—That is because the operation of the virus is more rapid than the progress of the disease itself, and such is the case in inoculation if performed immediately after exposure.

Q.—Would it not be wise to try that, among other remedial means, in case of the isolation of animals?

A.—Well, Sir; this disease is so well known to be very dangerous and fatal that I should not be in favor of tampering with it, except by way of abstract scientific investigation. Every sensible man would admit the importance of that. Practically, I do not think the results would be of any value. In an abstractly scientific point of view they might be interesting and valuable.

Q.—According to Mr. Lindley, a large per cent. of animals were saved in Africa.

A.—Yes, Sir; no doubt.

Q.—What is now the course of treatment adopted in Europe for the first stages?

A.—Well, Sir, in many cases, they destroy the animals entirely. That course was taken a few years ago by the Belgian government, and they succeeded in completely eradicating the disease. It was introduced again by an importation from Holland, and they still have it. In England, the location and manner of keeping cattle is so much more isolated than on our small farms here, that the disease is not so likely to spread there as with us, where every body is continually trading and exchanging stock. It is spread there by the fairs and markets.

Q.—But they have the disease in England?

A.—Yes, Sir; it was introduced in 1841.

Q.—Now, if the disease appears in an English gentleman's herd and the herd is large and isolated, what does he do upon its first appearance?

A.—I know that in some cases they have destroyed their herds, in others treatment has been resorted to.

Q.—I want to ascertain if experiment, in England, has led, at last, to any substantial agreement as to the treatment of the disease, when it appears in their herds?

A.—I do not think that there is any uniform mode of treatment among individuals, but in some cases within my knowledge, when animals have been taken sick, the healthy animals have been immediately removed from them, and the effort has been made to keep them completely isolated. The diseased ones are ordinarily knocked in the head, as not being worth curing. In mild cases, however, they are fattened for beef.

Q.—Have you a distinct recollection of many places where the disease appeared and has been annihilated by killing?

A.—It was annihilated entirely in a part of Switzerland, twenty years ago, by precisely this course, and also in Belgium, and in some other localities.

Q.—How long did it take to annihilate it by the destruction of the animals?

A.—I cannot tell you. It would depend entirely upon the number of animals that had been exposed.

Q.—You cannot tell how long it took, either in Switzerland or Belgium?

A.—No, Sir.

Q.—You say that this disease has existed in England twenty years, and still continues. Can you tell the reason why the cattle have not all been swept off?

A.—Because they are isolated, and kept away from the contagion.

Q.—What is to prevent the herds from being exterminated, when it appears among them?

A.—I think there are few cases where it has appeared in which they have not been exterminated. I do not know of any. It is given up as an incurable disease, or, supposing it could be apparently cured, it would be of little practical value because it is admitted that it can never be perfectly cured. The disease lingers on for a long time in a latent or chronic state, ready to break out upon the occurrence of any exciting cause.

Q.—Do you know of any system of treatment that has been adopted?

A.—As I remarked before, I do not think there is any uniform system of treatment.

Q.—So far as you can see, then, men are indebted to Divine Providence, rather than to any skill of their own in treating it?

A.—Yes, Sir. As I have before stated, the first or inflammatory stage may be susceptible of control, but afterwards, I think they would be dependent upon a special Providence for any good result.

Q.—What is the treatment used in Europe?

A.—A part is dietetic. I cannot give the detail, but in the inflammatory stage bleeding is often resorted to, counter-irritants, &c. Other remedies have been recommended by veterinary surgeons.

Q.—Have there not been some herds in England in which the disease instead of being destroyed by the knife, has been checked, or put an end to, by some sort of remedial treatment?

A.—That I cannot tell. I do not know whether it is so or not, but I know that comparatively few cases of actual pleuro-pneumonia ever recover there under treatment.

Q.—Do you know whether any herds have been voluntarily extirpated by their owners, to put an end to the disease?

A.—Yes, Sir; such cases are reported in the British journals.

Q.—What good does that do to the rest of mankind, if the English herds are so isolated that the disease would not be communicated from one man's herd to another's? Is it not much like cutting off a man's head to save his life?

A.—It is ordinarily recognized as a fact in Europe, that if the disease has got hold of a herd, it is as good as destroyed. I think that is the result of the good judgment of practical men. Some fat and kill them immediately for beef.

Q.—Would not a small percentage be left?

A.—Yes, Sir; but no breeder would be willing to breed from animals that had a suspicion of the disease.

Q.—How many herds do you know of in England that have been destroyed, as a means of preventing the extension of the disease?

A.—I cannot state the number of cases, but I know that in some cases the disease swept over considerable territory.

Q.—Are there any books where the evidence can be found marshalled, so that a man could make up a tabular statement of the number of herds destroyed by the knife ?

A.—I do not know.

Q.—Do you know whether the number destroyed in Switzerland was accurately given ?

A.—I do not recollect.

Q.—Is there any place where a man can find the number of herds destroyed by the disease where it has appeared, or is there any evidence that any one herd was ever destroyed in England ?

A.—Yes. Statements are given in the journal of the Royal Agricultural Society. The tone of the journals has been that complete destruction, where the disease has got hold of a herd, is to be recommended. I know that is the prevailing tone of the highest and most authoritative agricultural journals ; but as to giving the details of the animals or herds that have died, I could not pretend to give them from memory without referring to the reports.

Q.—There is no precise knowledge on the subject ?

A.—There is, in regard to the investigations, that have taken place in England and Scotland and Holland, as definite information as you could expect under the circumstances. All the symptoms, the ordinary modes of treatment, that have been adopted, the places where herds have been destroyed by order of the government, the investigations by the commissioners appointed by the Belgium, Dutch, and French governments,—all these things are known and before the public, and have been for some time. The progress and pathology of the disease are also known.

Q.—I want to know your opinion in regard to the economy of a course recommended to be pursued. Suppose isolation could save many animals, would the value of the animals, after they had been saved, justify the expense ?

A.—If any animals in the herd that it was proposed to isolate had been decidedly exposed, and diseased, I should say not. In further answer to your question, if you will allow me, I will make one remark. There is a very common fallacy in reasoning from this disease of pleuro-pneumonia in the human subject and in cattle. For instance ; we wish to use the butter,

the milk, and the cheese, which are the product of these animals. Now, it is almost universally admitted, by agriculturists and practical men, that this disease, even though apparently cured in an animal that has been attacked, is never in fact wholly cured. Now, if a man loses half his lung, as you have been told by one of the physicians in this city, he may live on; he may do office work; he may go to his counting-room and get along with his head work; but *he would never be considered a healthy man.* He may live along, and perform light duties for some years, but he is never considered a healthy man. Now, suppose you should save a cow; upon the same testimony, that cow could never be regarded as a healthy cow; no one would be willing to use the milk, the butter, and the cheese, that were made from her while alive, or to eat the beef when killed, and no sensible man would be willing to use her as a breeder.

Q.—What length of time do you consider would be necessary to secure the public from the ravages of an animal that had the disease in its constitution?

A.—Well, Sir, I have not formed any opinion upon that point, but I am inclined to agree with Mr. Chenery, and others, who have testified upon that point, that it would be a long time before it would be safe to allow such an animal to be brought among healthy animals—never, so far as my knowledge goes.

Q.—Would the cattle, if they had been saved, have been worth the money that it cost?

A.—I think not.

Mr. ANDREW.—What do we know?

A.—They know more in Europe than we do.

Q.—What do they know in Europe?

A.—I could show you if I had the reports here. The disease is very stealthy and insidious, and for all practical purposes is incurable. That they know. That is the result of the investigations of Collot, of Delafond, of Waters, and of Professor Simonds, one of the highest agricultural and veterinary writers in Europe, selected by the British government to go to the Continent and investigate this disease.

Q.—I want to know whether the result of your observation and study, thus far, is not simply this, that in regard to the

disease called pleuro-pneumonia in cattle, there is a point beyond which it is ascertained to be incurable?

A.—Yes, Sir, but the point is pretty well ascertained.

Q.—That is the most you can say?

A.—No, Sir; not the most; but I should say that you could say that, with perfect positiveness.

Q.—Where that point is you do not know?

A.—I do not think any one is competent to define it precisely, but for all practical purposes it is well known.

Q.—But is it also probable, that up to some point, the disease is curable?

A.—Yes, in the very first stage, the animal's life may be saved in some cases.

Q.—But how far it remains curable is one of the questions of science not yet solved?

A.—I should hardly say that it was very indefinite, from my knowledge of veterinary writers. They say, almost invariably, that after the first stage, the symptoms of which are given in detail, it is incurable, and that the first stage is so stealthy and so insidious, that it is impossible for an ordinary farmer and practical man to tell whether an animal had that disease or any other; so that when the animal has passed into the second stage, and the symptoms become more marked and positive, the disease is incurable.

Q.—Has any Commission ever tried a hundred animals to see whether any of them were curable, in what is called the second stage?

A.—I do not know whether any one has taken a hundred cattle, but I know that a great many cattle have been treated in France, and Scotland, and England, and have been treated with no favorable result.

Q.—No recoveries?

A.—Recoveries would be rare, complete ones, never. The testimony is, that after the inflammatory stage, the disease is incurable, and that no favorable results have been obtained.

Q.—You have no confidence in any remedies?

A.—I know that remedies have been used in Europe, such as bleeding, in the early stage of the disease; but the testimony, so far as I am informed, is that after the early stage, bleeding is injurious.

Q.—What is the success of treatment in the early stages?

A.—It is somewhat controlled by bleeding, counter-irritants, and other remedies.

Q.—What is your impression, from what you have learned, in relation to inoculation—that it produces the same disease?

A.—No, Sir; it is a somewhat similar disease. As I understand this disease, which we call pleuro-pneumonia, it is a disease which attacks the respiratory organs. As I understand, inoculation produces a disease in the system of the animal, not so directly in the lungs, but a similar disease, which passes all through the system.

Q.—Do you learn that a disease produced by inoculation, results fatally itself or that it fails to protect from the true disease?

A.—I learn that in a considerable number of cases, it produces death, but in those cases where it does not, it is the salvation, almost, of an animal from taking the infection, on going into an infected herd.

Q.—I understand you to express a deep interest in the protection of the industrial interests of this country. I want to know what your opinion is, in relation to circumscribing the infected districts, and preventing ingress and egress of animals?

A.—My judgment has been—and I have given the subject a great deal of thought and reflection—that among other things that ought to be done, in our circumstances here, is to make the Connecticut River a boundary on one side, over which no animal should be taken, under severe penalties; and then, perhaps, the State line, or the Merrimac River, on the other side. Some such stringent legislation is necessary to confine it within its present limits.

Q.—But what would be done with the infected animals?

A.—That would be an isolation upon a large scale; and then I should say, that authority might be given to the officers of the several towns, to isolate them, and also the herds. But if it did not go further, there would be no beneficial result. There would be a disposition to tamper with the disease; you would have ignorance, and disbelief, and every thing else to contend with; and even in the ordinary isolation of a herd, there would be danger of breaking down the fences, which would require very careful watchfulness to guard against.

Q.—I understand you to say that this disease has been in England some twenty years?

A.—Yes, Sir.

Q.—It is there now?

A.—It is, as I understand, in some places. I do not understand it to be generally in England, but it has been known, for instance, within the last year, in London, south of the Thames, and has carried off a very large per cent. of the cows. It was reported in the *London Veterinarian*, I think, for April, that as large a percentage as ninety-five were carried off by this disease; but then, I do not mean to say that it was general over England. I understand that the condition of the country is such, that there is a sort of natural isolation of farms there, so that the stock can be kept separate, and that the herds of cattle are kept separate to a great extent; and also that there has been, from time to time, a destruction of herds, where the disease has appeared, but yet, the disease has not been extirpated from the island.

Q.—Now, I want to know what reason you have for thinking that the same thing cannot be done here, by either or both, of those processes?

A.—We have a great many instances, some of which have been brought to the knowledge of the Committee, where very stringent measures have been taken, and have resulted favorably. Suppose Mr. Chenery had killed his entire herd, instead of sending a portion of them to North Brookfield. I suppose no one will doubt that the disease would have been extirpated. Now we have the same thing on a much larger scale. The labor is herculean now, compared with what it would have been then. I would remark, that there is, in the method of farming adopted in England, greater facilities for isolation, and, as a general thing, the animals belonging to large owners, are considerably more isolated than ours.

Dr. Loring.—Do you not know that the legislation in England is considered by farmers and veterinary surgeons, usually as insufficient?

A.—Yes, Sir.

Q.—And that, within the very last year, strenuous efforts have been made to obtain laws that should be more satisfactory?

A.—Yes, Sir.

Q.—Are you not aware, that in the examinations before a committee of Parliament, repeated statements to that effect were made, and that the regulations at present existing in the island were deemed insufficient?

A.—Yes, Sir.

Mr. ANDREW.—Don't you know that the landed interest has been more powerful than any other in England, ever since the Norman conquest?

A.—Yes, Sir.

Q.—Then, how do you explain the fact that they have not been able to protect themselves?

A.—That I cannot say.

Q.—Is not the answer to be found in this, that they are not agreed among themselves?

A.—That may be so to some extent; but they have been blocked and obstructed, in various ways. This disease, however, is not common over England; it is only known in certain localities, and of course does not arouse the interest of the nation as it would if it was more widely spread.

Q.—Who is the editor of the *North British Agriculturist?*

A.—That I cannot state.

Q.—Was the article to which you referred, editorial or a communication?

A.—It was editorial.

Q.—Do you know any thing about his sources of information?

A.—Nothing; only that he speaks as if he knew.

Q.—Does he not give his sources of information?

A.—No, Sir; it is only an ordinary editorial, taking the facts in regard to the disease, as shown here, as evidence that it is contagious.

Q.—But you stated that he spoke of tracing the cows from Belmont to North Brookfield; does he not state whether he quotes from the newspapers or not?

Q.—He quotes from the *Country Gentleman.* The editor commences his article with the words, "we learn from the *Country Gentleman,*" &c.

Q.—What is the *Country Gentleman?*

A.—It is one of the most thriving agricultural papers in this country.

Q.—Has the *Country Gentleman* had any fact which has not been brought before this Committee, so far as you know?

A.—I am not aware that it has. I think almost all the facts have been brought before the Committee.

Q.—As a matter of fact, the disease still continues in England. Do you know how far it is owing to importation?

A.—I don't know whether it is owing to importation in any special, particular instance of recent date.

Mr. WENTWORTH.—You say that in the first stages of this disease, on the European authorities, it is curable?

A.—It is supposed to be so.

Q.—And they give the specified treatment?

A.—Yes, Sir.

Q.—Would it not be advisable, then, to apply that treatment to cattle that have been exposed, as a precautionary measure?

A.—It would not be necessary, where cattle have been merely exposed. A man would not be willing to have forty cattle bled, because they had been within ten feet of a diseased animal.

Q.—Why cannot that treatment be followed up with exposed cattle?

A.—I suppose it might be; but I cannot say as to the effect of the treating comparatively healthy animals as if they were diseased, without knowing the fact. The difficulty is, that the disease, in its first stage, is so very insidious that the farmer does not know, in one case in a hundred, that his cattle are diseased.

Q.—But take a case of well established exposure; why not treat that?

A.—That would be a question for the veterinary surgeon. I should not like to try that course with animals simply exposed.

Q.—Would you recommend killing? Which mode would you prefer—attempting to cure in this way, or killing?

A.—In the case of a herd that had been simply exposed, without any disease in it, I should say, isolation was to be preferred. I think that might be tried with safety.

Q.—Why not with safety, then, where the herd had been exposed, and one death had ensued?

A.—Because there is scarcely one chance in a hundred, that the disease would not go through nearly the whole herd. Where exposure has been so direct in a herd, where one or more animals have died with this disease, the chances of any of the animals escaping are very few indeed; one or two may.

Q.—Then you do not mean to say "safely," but beneficially tried; because if isolation is safe in one case, it is in both cases?

A.—Hardly safe, because there are so many accidents to which the cattle may be exposed, the breaking down of fences, &c., &c.

Q.—You say, that in case of an exposed herd, isolation may be safe. Suppose they have taken the disease.

A.—What I mean to say is this: where there has been a mere exposure, where there has been no case of positive disease in a herd, but where a suspicion of exposure has existed simply,—where an animal, for instance, has passed by a diseased herd, a few feet off, and where you do not know that the herd will be sure to take it, or be positively diseased, I should hardly recommend the destroying of the herd in that case; but where the disease has entered a herd, and there is almost a certainty of its going through that herd, and making them all either comparatively or wholly worthless, whether for the purpose of breeding, or ordinary farming purposes, I should say it would be very unsafe, very injudicious, as a matter of economy.

Mr. Fisher.—The difficulty seems to be, that it is practically impossible to ascertain in what stage the disease exists in an animal or in a herd?

A.—Yes, Sir, for one not accustomed to the disease.

Q.—So that, on that ground it is exceedingly difficult to apply remedies?

A.—Yes, Sir.

Q.—Would it not be necessary, if you attempted to cure an animal when the first symptoms appeared, to keep the whole herd isolated until you got through the whole experiment?

A.—I think so.

Q.—Do you mean to stand by the statement you made, that it is difficult for any one to tell, when an animal is diseased in the early stage?

A.—Yes. It is not easy to detect the disease in the first stage, even by a surgeon.

Q.—Have you any particular knowledge upon this subject, except what you have derived from books?

A.—I was with the Commission, all through their investigations. I was present at every examination, and assisted in some cases. I have so much knowledge of it, that I think if I saw an animal opened I could tell whether it had the disease or not, unless it was in a very slightly developed case. The appearances are so very peculiar, and so specific, that I do not think any person, of ordinary intelligence, could fail to become acquainted with the appearances of the disease, after he had seen several cases.

Q.—You are not a surgeon, nor a physician, and had no occasion to investigate this subject, until the Commission was appointed?

A.—No, Sir.

Q.—Can you furnish the statistics of the mortality that has appeared in England?

A.—I can furnish the Committee with certain articles concerning the disease. Some of them have already been laid before the Committee, by Dr. Loring and the Commissioners.

Dr. Loring.—I would like to answer an inquiry that has been made here, but has not been answered. It is with regard to the history of this disease in any particular herd in England. I happen to have a report of Prof. Simonds upon inoculation, for the disease called pleuro-pneumonia in cattle. He gives the names of the men who assisted him in the examination. In a very few sentences, he makes a statement, which I have no doubt will be interesting to the Committee, as it has been to the Commissioners;

"On our first visit to Ruddington, Mr. Paget kindly placed at our disposal any number of animals we might select for the experiment of inoculation; and this notwithstanding he was in full possession of our

opinion as to the serious ill consequences which might attend the operation, as well as our doubts of its ultimately proving of any value as a prophylactic. From the history given, it appears that pleuro-pneumonia, which had prevailed more or less in the neighborhood of Nottingham since 1843, first showed itself in Mr. Paget's herd in August, 1849. The attack was very virulent, and between this time and Christmas of the following year, it carried off no less seventy animals. In 1851 thirty fell a sacrifice to the disease, and from January, 1852, to the end of November, when the experiments were commenced, thirty-two more animals were destroyed by it. We have thus a total loss of 132 animals from August, 1849, to November, 1852, inclusive; a period of little more than $3\frac{1}{4}$ years. From the changing state of the herd, the ratio of deaths to the number kept cannot now be ascertained, but it will be seen that the losses may be described as being ruinous in amount.

"Mr. Paget milks upon the average, sixty cows, for the supply of the town of Nottingham; besides which, he buys in from time to time, a number of animals to fatten, and also to supply the place of those which have been sacrificed to this and other diseases, so that he has from 90 to 100 head of cattle usually on his premises. It is necessary to state that the amount of loss is partly guarded against by feeding the animals liberally, and by having them killed as soon as they give the slightest indication of being affected with pleuro-pneumonia,—experience having shown the inutility of medical treatment."

The Chairman stated that he had received a communication from Dr. Martin, which was a continuation of his testimony given on Friday last, and was in relation to the cattle slaughtered at Belmont last Saturday.

On motion of Dr. Choate, it was voted, that the communication be printed in the Report.

To Hon. Mr. NASH, *Chairman of Legislative Committee*:—

CASE 11th.—June 2d, 1860, I visited W. W. Chenery's herd, at Belmont, with Legislative Committee, and the State Commissioners. Examined a beautiful black and white Dutch heifer, one year old. She was taken quite sick in September, 1859, and continued very sick about six weeks; then she commenced recovery. At present her eyes look bright, hair glossy and smooth, appetite good, presenting all the appearances to the eye of a healthy animal. She has coughed some ever since first attack. Percussion dull over right chest. In applying the ear, a coarse, mucous rattle can be heard at the base of right lung. Nothing unnatural about the external examination of left chest.

Autopsy.—Strong adhesion of the right lung to whole inside of chest, ribs, diaphragm, heart case, &c. The lung showed the effect of severe compression by the effusion of water, which water had been absorbed during the process of cure.

On opening into the lung, we found a large cyst, big enough to hold a quart or two. But in this cyst we found *no* lump, *no* pus; but we found an opening from it into the bronchial tubes. The question will be asked, what did that cyst contain? No *large* cyst of this kind, in the early stage of the disease, has ever been seen by the writer, without having for its contents a large lump of detached lung, surrounded by pus. And is it not a fair inference that this originally contained the same, and that the lump had undergone decomposition, and been discharged with the pus into the mouth by coughing, and swallowed into the stomach, that being the way discharges from the lungs of cattle go? The lung itself was lighter than water, and was beginning to be pervious to air all around the cyst, and had nearly regained its original pink color; the color in the acute state being dark, like liver. The sack in this case was smooth, and seemed to be lined by a mucous membrane, and performing its function, as we found mucus secreted from it. In that condition it (the sac) might remain without great injury, after being pushed into a much smaller space by the refilling and enlarging of the remainder of the lung, sufficient to fill that side of the chest—the cyst performing the duties of a dilated bronchial tube, showing the divine wisdom and power which presides over the laws that govern diseased action.

The lung showed no less ability to admit air from the consequences of compression, than is seen in numerous cases of pleurisy in children who die from the compression of effused water, that would have been saved by tapping, and as others have done, presenting like symptoms, the lung and chest being a year or two in regaining their full functions; the adhesions remain always. A person without experience would be likely to say, on seeing a man's lung compressed to the size of his fist, and appearing hard, that it would never get well, when nothing is more frequent in disease. If this lung contained a lump in its cyst, the lump did not go off by absorption, but by decomposition and discharge through the bronchial tubes. But the author expects to find in Mr. Chenery's herd, ultimately, a case or cases where an animal has commenced recovery and ceased coughing, showing an air tight cyst, with lump dissolved in pus and carried off by absorption, but at present he has no proof, only inference, this case not being in point. But this is the way that the medical faculty who have given the investigation of the disease care, think the cures are to take place, but your writer hopes and expects some will get well in this way, and some by absorption. In the two cases examined to-day, the heart was thinner and flabby, as usual; the *left*

ventricle thinner than right. I am informed that this is always the case; but personally I have not observed that the left ventricle is always the thinnest, not having directed my attention to that point; but the flabby state has been seen in all well-marked cases. The cow to be hereafter described in this paper, having but very little disease in its very early stage, showed a great thinning of the heart, and that this organ must be very early affected, but in what manner this is produced your deponent knoweth not; and whether it is peculiar to all cases of pleuro-pneumonia or not he is not quite certain. But that patients sick with pleurisy frequently die from a coagulum in the heart he does know, which would imply a feebleness in that organ.

This disease is strictly a supperative one. Its name should be supperative, or turuncular-pneumonia.

Case 12th.—Cow six years old; she was taken into Mr. Chenery's herd as step-mother and nurse for a calf born of the recently imported Dutch cow, which died. She was introduced into the herd December 1st, 1859, and is now presenting all the general appearances of health, excepting a slight cough. If our memory serves us right, the calf coughs also slightly, but he is a fat, healthy looking beauty.

Could detect no disease by auscultation and percussion.

Autopsy.—No adhesions, no serum, but at the base of the right lung was a darkish red spot, somewhat near three inches square. On cutting into it, we found it hard like liver, with the mucous membrane of the small bronchial tubes pushing out of the cut surface with their mouths filled with pus or mucus, giving this cut surface the appearance of being covered with innumerable small pustules, not quite as large as very small peas. There was another spot, but not as distinct, and it might not be disease, so it is not described. The rest of this lung, and the other, presented no marked appearance of disease. This was probably a case of mild contagious pleuro-pneumonia, in its very early stage; but the writer is not quite sure, as he has not yet traced the connecting link between this and more active cases later in their progress. If this be a case, it settles the origin of the disease to be in that pouting, swelled mucous membrane, shown on cut surface, extending to substance of the lung, and then to the pleura. There are probably other cases in this herd in the various states of advancement, some of which will give the disease.

By this and other mild cases, it will be seen how extremely difficult it will be to separate diseased from well cattle; probably some that are thought not susceptible running through the disease and giving it to others like worse cases. This herd does not show that all its cases have passed eight weeks from taking disease, the least possible time to run through the time for incubation and propagation.

The attention of the Committee and the Commissioners is especially called to the following conclusions, based upon the personal observation of the writer:—

All cases that die, die from the quantity not from the quality of the disease. They die from the local, not from the general effect. They die by the lung either being hardened so as to be impervious to air, or by the lung being compressed by the effusion of water, so that the air cannot enter it. A case may die from absorption of pus, but that is an accident, not the laws of the disease. All cases that do not die during the acute stage of the disease, go on regularly, step by step, towards recovery.

And that the well lung will take on the disease after the sick one has commenced to recover, I do not believe. It might as well be pretended that the skin in smallpox that had remained sound would take on the peculiar diseased action, after the first eruption had commenced healing. A cow that has had the disease once is probably forever protected from it after, as much so as the man who has had smallpox. And the belief that one lung may be having the incipient, and the other the advanced stage of the disease, may be the result of the observers not being sufficiently acquainted with disease to recognize the return to health.

All cases that the author has examined, that have died or were near death when killed, and all cases that have been described in public prints, had hardening of one or both lungs, and effusion in one or both chests; and wherever there is effusion, there is hardening of that lung, the effusion being the effect of the continuation of the original lung difficulty. Of course, where there is effusion in both chests, the animal must die; for if the water is removed by tapping, the harder the lungs are left. There is no sound lung to carry on the function of respiration, just as in confluent smallpox no healthy skin is left for perspiration, and the patient dies. But as a rule, where the hardening and effusion are confined to one side, if the animal dies, it dies from the well lung being compressed by the water from the other side. Such animals might and ought to be saved by tapping. Many a man and many an animal have gone down to an untimely grave, through the ignorance of the laws that govern animal life.

Therefore it is incumbent upon this legislature to see to the appointment of a scientific commission, composed of men, who by education, by the peculiar character of their minds, by experience in tracing cause and effect, by their standing with the community, will give confidence to their opinions, and quiet to the now uncertain and agitated state of public sentiment; to select men with educated minds, clear heads, and sound hearts; to clothe them with *unlimited* powers and *unlimited* means;

then they will either wipe this tremendous scourge from the land, or teach how to save most of the cattle.

Respectfully submitted.

ORAMEL MARTIN, M. D.,
One of the Committee of Mass. Med. Society.

Evidence of Dr. J. B. S. Jackson, of Boston.

Mr. ANDREW.—Have you given to the subject of malignant pneumonia in cattle any attention ?

A.—I have seen some two or three specimens that have been brought to the city, and one of them I examined carefully; the others I did not examine carefully.

Q.—Will you be kind enough to state to the Committee, with such order and arrangement as you think best, what investigations you made, and what results followed ?

A.—The specimen I examined carefully I found to be different from the disease pleuro-pneumonia, as it occurs in the human subject. It was quite remarkably different, so much so that I exhibited the specimen before a meeting of one of our medical societies, and my description of that particular specimen was published in the *Medical Journal*. It would be useless to attempt to describe, to non-professional persons, what the anatomical appearance was. Although I have examined many specimens, in the human subject, I have never found the appearance that I found in this case.

Q.—Did you ever examine any specimens of diseased neat cattle before ?

A.—There was a buffalo from the western country, that died in this city several years ago, and I examined that animal. It was a very strongly marked case of pleuro-pneumonia.

Q.—Was this characteristically different from that ?

A.—That was so long ago that I don't know whether the same peculiarities existed in that case, that I observed in this last case, or not. I think if it had, however, I should have been struck with it. This specimen, I would say, that interested me so much, was one brought to the hospital by Dr. Wood. There I saw it, and took a part of it home to examine.

Q.—Have you studied this subject of disease in cattle, either by reference to authorities or otherwise ?

A.—Not at all.

Q.—Have you any advice or opinion upon the subject, that you could give to the Committee?

A.—I have nothing to offer to them that I think of. I am perfectly free to give my opinion, so far as I can form it.

Q.—Have you any basis upon which you could form and express an opinion, touching the contagiousness or curability of this disease, or possibility of limiting or controlling it by any sort of treatment?

A.—The contagiousness, Sir, struck us, here, as a very singular feature in the case; it was something we had never heard of—a contagious pleuro-pneumonia. I supposed that it was that, and not a malignant fever of some form, in which pleuro-pneumonia is to be found in a large proportion of cases; when you would say, that the fever is the disease, and pleuro-pneumonia (to give it a name) is only a complication. The testimony is so conclusive, that it is admitted by many of the profession, that it must be contagious, on authority. With regard to curability, even, though it may be ever so contagious, as is the case with some diseases affecting the human subject, there is no reason *a priori*, why it might not, in a certain number of cases, be treated successfully. We understand, on the same general authority, that a very large proportion of cases are fatal—very, very large. We only get it from general rumor, but even if it is, some might be benefitted by treatment; I do not know, however, how far our method of treatment might operate. But as to what has been done for the animals, since the disease has been prevailing in this country, I do not know that I have heard of any thing except merely with a view to check its onward progress; animals having been killed in a large number of cases, as soon as it was ascertained that they had the disease upon them.

Q.—You have not examined the history of the subject in England and on the continent?

A.—No, Sir; I have not.

Q.—Do you know any means of checking the progress of a single case of disease which is contagious?

A.—Well, Sir, we know that smallpox is a very contagious disease, and that a large portion of cases of smallpox recover.

Q.—That is not the question, but whether you have the means of cutting it short during its progress?

A.—I suppose it may be that this, like many other diseases, is a self-limited disease (as they have been called by many); that is, it has a certain time. Smallpox and scarlet fever, when they get into the system, must run longer or shorter, and there are no active means, that we know of, that we can use, that will cut them short.

Q.—So far as analogy goes, would you not expect the same rule to apply to this disease, if it has been established to be contagious?

A.—Yes, Sir; if it is in the system, there are probably no means by which you can cut it short, so that the animal would be well in a few days.

Q.—Do you know whether the disease can be readily cured in its first stages?

A.—As I say, I know nothing whatever about it.

Q.—Have you examined any authorities?

A.—Not a single authority.

Q.—Never have seen a case of it?

A.—Never.

Q.—Only one lung?

A.—More than one—two or three; may have been four.

Q.—The opinion has been offered here that it is curable in one stage. Now, should you expect that a contagious disease was any more curable in one stage than another disease produced by specific poison, breathed into the system and affecting the vital organs?

A.—I believe I have answered that question twice before, that the disease, when it gets into the system, will have a run.

Q.—It has been asserted that the disease may be cut short, if taken in season?

A.—I cannot believe it.

Q.—The last witness said it was curable; that is, that the animal may recover under proper hygienic and medical treatment?

A.—I suppose that the chance of recovery would depend upon the amount of the dose of poison, which the animal has taken into his lungs. As in the case of smallpox, some will take a heavy dose of the poison, and others a light dose. It is

probable that those who had a heavy dose will die, whatever the treatment may be; those who have taken only a light dose will probably recover, under any suitable treatment. Then the question is, in regard to the intermediate cases. If they are taken in the early stages, and receive proper treatment, I don't see any reason, *a priori*, why a considerable number might not recover. If they were left to themselves, and if their diet was not attended to, their chance would, of course, be small; and if they had much febrile excitement, I don't see why the bleeding that we heard Mr. Flint refer to might not be used successfully. Treatment consists in the first place in what you might call nursing, and then medical treatment. I refer to bleeding, as well as the use of drugs. I don't know whether any drugs would be useful in such a case.

Q.—Do you know of any thing that will qualify the poison introduced into the human system, in a contagious disease? Is it possible to cut it short or qualify it in any way? Suppose the smallpox has been introduced, can you, in the first stage, cure that disease except by allowing it to go through a regular course of changes?

A.—If the person has never been vaccinated, and has been exposed to a bad case, it is almost a certainty that he will be diseased. Then let him attend to his condition, and when the disease comes on, let him be treated judiciously, in a hygienic way, and let any symptoms that arise be treated. If there is high febrile excitement, or diarrhœa, or constipation, let them be treated; but, for the disease itself, there is no remedy that we know of in the medical profession, that will touch it. I suppose that the same law applies to the diseases of animals.

Q.—My question was to ascertain whether this disease could be cured in any stage, or whether it must go through all the stages. Is not that the law of contagious diseases?

A.—Yes, Sir; and of a great many other diseases. I suppose that the disease would run through its course.

Q.—You suppose that this disease is caused by poison taken into the vital organs?

A.—I do not know much about the causes.

Q.—It is a poison?

A.—For the sake of giving it a name, we call it a poison.

Q.—And, when this poison is applied to the animal system, the general impression is, that it cannot be modified essentially ?

A.—I say that hygienic treatment will render the disease less virulent.

Mr. ANDREW.—Now, I would like to add up this sum the other way, and see if it will prove. The questions put to you by Dr. Choate have gone upon the assumption that this disease is a contagious one. Now, supposing it were proved to be true, that in a certain stage of the case it is curable, would or would not that tend to show that the disease is not contagious ?

A.—If you mean to ask whether if a dozen animals were taken in an early stage of the disease, and treated by a judicious veterinary surgeon, and got well, it would show that that disease was not contagious, I should say that it would not.

Q.—The answer you give assumes that the disease has its run, as the phrase is ; but supposing that it turns out, upon experiment, that the animals taken and treated medicinally, in a certain stage of the disease, may be cured, without the disease having a run, would that or not tend to show that the disease taken by them was not a contagious one ?

A.—Well, Sir, I do not feel prepared to give an answer. There are cases of dysentery, and diarrhœa, and some other cases that we are called to, that will be sometimes cut off very short by medical treatment. We may treat the patient almost heroically, and we are sometimes skilful enough, by active means to cut short the disease ; but in an ordinary case of fever—typhoid fever, yellow fever, the English ship fever, or any form of fever—and in cases of pleuro-pneumonia and pleurisy, it is a very, very rare thing for any one to take a patient and be able to cut the disease short, if of any sort of severity ; it will have a certain run, whether it is contagious, as in some forms of fever, or not in the slightest degree contagious, as is the case with pleuro-pneumonia.

Q.—Some of the witnesses have testified that this disease, in its first stage, is curable. I do not know what curable means, only I suppose that it means some way of getting the animal well. Supposing that it is curable in the first stage,—I don't suppose that it makes any particular difference whether it is cura-

ble by reason of its being cut short, or by controlling the disease, so that it shall not be very dangerous,—assuming that it be so, does the fact of the disease being contagious render it at all probable that it is not liable to control and limitation by proper hygienic and medical treatment?

A.—I think, Sir, that the question might almost be answered, at any rate, an answer might be inferred, from what I said before, that hygienic and medical treatment will control more or less, contagious diseases, as well as other diseases. It is well known, that in cases of smallpox, or measles, under judicious treatment, the patient often goes through the disease very kindly.

Q.—Have you not found, in treating measles, that the proper treatment, on the first appearance of the symptoms, may so modify the disease, that even the "run," may be cut short, and the patient go through the stages quicker, under proper treatment? In other words, have you not seen cases of measles in which the treatment had been neglected, prove protracted, and the disease have a longer run than in ordinary cases of measles?

A.—Certainly, Sir.

Q.—And the same rule will apply to other diseases as well as to measles?

A.—I suppose the disease will be likely to go through its "run" in a shorter time, and with less violence.

Dr. Loring.—Are there not diseases in the human system, whose progress may be stopped, at some certain point, and yet leave a serious organic difficulty behind them?

A.—Yes, Sir. I have a case in my mind now of scarlet fever, where the patient recovered from the disease, but is left with a diseased ear, and will probably be deaf for life.

Q.—I mean a disease of the organs; for instance, may there not be an ordinary case of pleuro-pneumonia, which would leave the lung in such a condition that it would be useless?

A.—Consumption might follow on as the sequel of pneumonia, and pleurisy may be attended with large effusion of fluid into the chest, which may make a very protracted case of it. The patient may finally sink from the disease, or may finally recover.

Q.—That you would not expect, when the disease appears to be cured?

A.—Not if the patient should apparently get entirely well; but there are a great many cases in which the patient is left in a weakly, feeble state, so that, although he may go about his business and consider himself well, a physician would see that he was not well.

Q.—Might there not remain in the lung, after an inflammatory disease, either pneumonia, or pleuro-pneumonia, a portion of the lung which would probably be useless to a person during his life?

A.—It is not, I suppose, by any means a rare occurrence, as you must know, that a portion of the lung will remain useless, after the individual has essentially recovered from the disease, but then, he has got enough lung left besides to work with.

Q.—In such a case as that, would it not be difficult to say that the patient is cured?

A.—He appears cured, to all practical purposes, but anatomically he may be in a diseased condition.

Mr. Andrew.—This question of cure, in medicine, is more a matter of comparison than otherwise, after the disease has once attacked the system, is it not?

A.—It is very well known, that a great many persons, after recovering, apparently, from disease, are left with a certain amount of disease behind,—a variety of diseases,—diseases of the chest, the abdomen, and the head. As there are a great many of us, who may have a good deal of disease in us, which does not manifest itself, so disease is left after sickness, which does not particularly manifest itself, nor interfere with a person's going about.

In answer to a question, which did not reach the reporter's ear, Dr. Jackson said, So far as I see, these gentlemen (the veterinary surgeons) recognize disease as we cannot begin to recognize it in the human subject. I wish we had the skill in diagnosis, that they seem to have.

A Member.—I would like to have you state in brief, the appearance of that lung which you saw?

A.—I will state, in a summary way, for the information of the physicians present, that the peculiarity consisted in the interlobular substance being primarily inflamed, and the vesicular structure becoming affected subsequently. It was an acute disease, and the interlobular cellular tissue was affected before, and to some little distance beyond, where the disease was observed in the vesicular structure; and it was only when the disease was advanced in the interlobular cellular tissue, that hepatization existed in the vesicular structure. To make it understood, to non-professional gentlemen, I will say, that the lungs, as you all know, are vesicular in their structure,—but if you examine them particularly, you will find that they are made up of little lobules side by side; and these last are connected together by loose cellular tissue. Now, in pneumonia in the human subject, the vesicular structure of the lungs seems to be the proper seat of the disease; whereas, in the animal that I examined, it was this cellular tissue, between the lobules, which was primarily affected, and then subsequently the vesicular structure.

Mr. ANDREW.—Dr. Jackson, have you considered the benefit of a special scientific investigation into this disease?

A.—I have not thought much about it, Sir, because I thought it was so self-evident, that the subject spoke for itself.

Q.—In what way, self-evident?

A.—In the first place, the natural history of the disease—how the disease originated—but many of these points have been ascertained; the proofs of contagion,—which, as many seem to suppose, is established; then the period of incubation—how soon an animal will be sick after it has been exposed; then the symptoms of the disease—that will enable the farmer, or a veterinary surgeon, to recognize it in its early stage; then the symptoms after the disease has got established; then the modes of treatment—the medical mode, and the hygienic mode, and the matter of isolation.

Q.—What experiment would you try, in regard to exposure, Sir?

A.—That is a point that I have thought of. Two or three cattle known to be well, might be taken, and put with an animal known to have the disease, to see if it will affect them,

and in order to ascertain what proportion of individuals exposed would take the disease. It is generally regarded by the profession, that all persons are not by any means subject to such diseases, for instance, as the yellow fever. One person in a family will be taken and go through the disease, and not another individual have it. But the grand object would be the trial of treatment.

Dr. Loring.—Do you suppose that the establishment of this knowledge in the community—the knowledge that you have just given in detail—would be of more value, than the establishment of the fact that the disease was eradicated,—that it was gone,—that there was no such disease here?

A.—No, Sir; that last is the great desideratum.

Q.—Suppose that Mr. Chenery's herd, for instance,—which seems to have been the seat, and the sole seat, of the disease in the country, for many months,—had been entirely destroyed in the outset, and the course of the disease had been stopped there? You would consider that of more value to the farming interests of the community, than the results of any scientific investigations made there?

A.—It would be self-evident to any one, a professional man or otherwise, that if Mr. Chenery's whole stock,—even if there were three hundred of them,—having this animal poison amongst them, and liable to communicate it (if it was known that there were no other cases of the disease in the country, except those in his herd)—if every individual had been sacrificed, every one would admit that that was the best thing to be done. But when the disease has gone abroad, is in other parts of the State, and is in other States, then we view it in a different light.

Q.—Exactly so; but would you not say, that the same rule, which would apply to them, would apply to the case now, if the facts could be ascertained?

A.—If it could be ascertained that a herd of fifty here, thirty there, and twenty in another place, were affected, and it was no where else, it would be better that all of those should be killed, than that millions and millions of cattle should be exposed. But I do not know how that could be ascertained.

Q.—Then the question comes down to one of the possibilities—whether it is possible to ascertain these facts or not?

A.—Whether it is possible to ascertain, whether the cattle throughout New England have been exposed or not. If you can settle that point, you would do a great deal.

Q.—You would think that worth trying for?

A.—Yes, if there was the slightest chance of success; but I don't see that there would be. When cattle are carried all about the country, taken from one farm to another, and hired by the day, I don't see that it would be possible to ascertain what individuals have been exposed.

Q.—It would depend very much on how far the trade extended from a given point—how far cattle were distributed from a given point?

A.—Yes. If you could keep the run of the trade, so that you could know how far cattle are bartered, and how far they are let out by the day, and so on, you might get at it.

Mr. WENTWORTH.—If you take into consideration the probable fact that this disease is contagious, and that we are liable to it, from our importations of cattle at all times, is it, or not, of the highest importance, that we should endeavor to ascertain its cure?

A.—It is, at any rate—contagious or not.

Evidence of Robert Wood.

Q.—What is your occupation?

A.—Veterinary surgeon.

Q.—State what observations you made at the examination of the two animals that were killed at Mr. Chenery's on Saturday, and in your examination of the disease generally?

A.—From my examination of these cases, and from what I have learned from my brother, Dr. Thayer, Dr. Dadd, and others, of the characteristics of this disease, I have no doubt of its being pleuro-pneumonia, as it is recognized in Europe. As regards the examination of the two animals on Saturday, I took what notice I could of them. I saw the lungs as they were presented to the persons standing about, and carried a part of them home and examined them again. I have no doubt that the heifer was then recovering from an attack of this

disease. There was no evidence ' me then of disease existing, other than altered structure, the result of disease. Of the cow, I think there were evidences of acute disease going on at the time.

A Member.—Mr. Chairman, I thought we had voted that no more testimony was advisable upon the progress of the disease.

Mr. Wentworth.—This evidence is as to the two cases last Saturday.

Q.—You have heard the evidence regarding the cases at Mr. Chenery's—have you any thing to add to that?

A.—I could only corroborate the testimony as to the exhibitions of disease there.

Q.—Whether it is curable or not, and whether it is desirable to treat it for a cure?

A.—I think not. If it is true that it is a specific disease, then there is no treatment that would cure it. We could only modify the effect of it.

Q.—Do you think it can be cured?

A.—No, Sir; but I think there may be a course of treatment that would modify the effect of the disease, rendering it less destructive to the body.

Q.—You don't understand me. I ask whether the animal can be cured—whether the disease can finally be overcome by treatment?

A.—I should have my doubts.

Q.—Whether an animal ever gets well?

A.—Yes, I have no doubt that there are many that get well. If properly cared for, the greater number would get well. As in all other diseases of this character, some generally recover.

Q.—Whether in your judgment it would be desirable to experiment with a view of ascertaining what is the best mode of treating cases?

A.—For the benefit of medical science, I think it would.

Q.—What do you think with reference to the farmers?

A.—It would be a question with me. We cannot judge now whether it would be policy or not, as regards dollars and cents.

That would depend upon experiment. It would depend upon what would be the cost, and how long they would require to be kept before the danger of their communicating the disease would cease.

Q.—If a cure could be found at moderate cost, of course it would be very desirable to discover it?

A.—Yes, Sir, undoubtedly.

Q.—And there is no mode of ascertaining that except by experiment?

A.—I think experiments might be conducted by isolating the animal properly, so that there would be no danger resulting from it.

Q.—Do you know what the proportion of mortality is in these cases? Have you examined to see how it has been in England?

A.—No, Sir,—not so as to make a correct statement. Various estimates have been made, but they do not agree. In some districts in all parts of Europe, but more especially in England, the mortality is greater than in others, depending upon the condition in which the animals are placed. I think that if animals that were sick could be placed in the most healthy atmosphere, and have the best and most wholesome food, and kindly care, the mortality would be less.

Q.—Do you think that would be attributable to the treatment, or to the fact that they inhaled less poison than others?

A.—The amount of poison would be less; and the treatment would tend to strengthen the forces of the body, and to resist the disease.

Q.—Would you expect to save the life of an animal or man who had inhaled a large quantity of poison, or who had been placed in circumstances favorable to such an event?

A.—I should have a stronger hope of saving him in those circumstances than if in an unhealthy atmosphere.

Q.—Don't you suppose the difference between a severe case and a slight one is owing to the amount of poison originally inhaled?

A.—Yes, Sir, undoubtedly.

Q.—You think mild cases would be affected more by remedies than severe ones?

A.—Undoubtedly. I think that if animals that have been slightly exposed could be isolated and have good care, there would be a stronger probability of recovery than if they were kept in the same place with other cattle, and had poor care.

Q.—I should like to ask if, in your experience in the care of cattle, it has not been more expensive to cure than it would to kill them? Would not the attempt to cure be profitless to the farmer?

A.—It may be so. I have not gone into any estimate to satisfy myself on that point. The duration of the poison in the system is a question with me. That must be settled before we can get at the value of curative measures. If we could cure a case in a short time, it would be profitable. If it took a long time, the cost would eat up the value of the cow, so that there would be no profit.

Q.—Do you think they could be entirely cured, so as to be useful for all purposes?

A.—I think it possible.

Q.—Do you think that one of these cured animals would sell for half price in the market?

A.—Yes; after people have become convinced that they are cured.

Q.—Mr. Wood, would the calf killed at Mr. Chenery's last Saturday, have been worth any thing as a steer for laboring purposes?

A.—I think that animal would have been worth something to work; not as much as one perfectly organized, but there would be a value in the animal, if it had been allowed to live.

Q.—For all purposes?

A.—Yes, Sir; for labor, marketing purposes, or breeding purposes.

Q.—A certain value?

A.—Yes, Sir; not the full value.

Q.—Do you think such an animal could have healthy progeny?

A.—I cannot see why an animal with but one lung cannot propagate its species as well as if entirely sound. I can conceive no law which would warrant me in saying that the disease would be transmitted to the progeny.

Q.—You do not know that this disease does not leave the system in the same condition that a man is left in with but one

lung? It has been stated that there was something peculiar in the morbid anatomy of the disease; and you do not know that the peculiarity is that it is a tubercular disease?

A.—I am strongly convinced that it is not. After cure, there is no evidence of the disease remaining in a form which could break forth anew. It is my impression that the organism would be sufficiently perfect to produce healthy progeny.

Q.—Are you not aware that it has been stated that in England, animals that were cured were considered comparatively useless?

A.—I am not.

Q.—Have you ever seen the lungs of a calf born of a cow diseased with the pleuro-pneumonia?

A.—I have seen a portion of one.

Q.—Did you find any disease in the calf?

A.—I found a little.

Q.—How do you suppose it acquired it?

A.—If a cow had this disease and lived and recovered simply with less organism as the result, and then gave birth to a calf, I cannot see how she could transmit the disease. I cannot realize the fact that it exists in her system.

Q.—Suppose the ravages of the disease have impaired the constitution of the animal, would the calf be likely to be impaired likewise?

A.—Undoubtedly, Sir.

Q.—Would you not think the vital force or energy considerably impaired, if one lung had been entirely broken down?

A.—Undoubtedly, Sir.

Q.—Don't you think that in the case of this calf, its vital force or energy was impaired by the condition of the lung of the cow in which it was found?

A.—I have no doubt it was in that individual case.

Q.—Then for breeding purposes you would not consider an animal as good as though it had never had the disease?

A.—It would be of value, but not of equal value.

Q.—You don't think the progeny would be affected if there was a recovery?

A.—If the cow has entirely recovered, having one sound lung, I do not see why she should give birth to a calf less in organism than though she had two lungs.

Evidence of Henry M. Hook.

Q.—Have you attended the examination of these cases at Mr. Chenery's?

A.—Yes, Sir.

Q.—Are you a physician?

A.—Yes, Sir.

Q.—What is your opinion upon the evidence here, as to whether the disease is contagious or not?

A.—From the evidence given, I should say the question was open, whether it was or was not a contagious disease; from the fact, as given in testimony, that the bull which was carried to South Africa was five months on its passage, in health, and was six weeks in the colony, in health, before disease was shown; from the fact that the animal carried to Australia was a long time on the voyage and was in health on arriving; from the fact, furthermore, that the stock imported by Mr. Chenery was taken from a healthy district, and there is no evidence shown that it was exposed to diseased stock, that it was sometime on the passage, and that the disease itself did not break out until sometime after arriving; from the fact, furthermore, that it appears in testimony given here to-day in relation to England, that it had prevailed there for twenty years, but only in a small district,—that it once ceased there and then broke out and destroyed a large proportion of a large stock,—that it has remained there for twenty years, in a small district, and no parliamentary enactments have been made to curtail it,—while if it had been a very virulent, contagious disease, it would have spread further and excited more attention; from the fact, furthermore, that a few years ago, I, and physicians generally, regarded typhoid fever, and dysentery, as contagious diseases, which I do not now regard as such at all, and that the cholera was so regarded; and doubts now exist as to the contagiousness even of scarlatina,—of the contagiousness of which I have seen no evidence for the last three or four years; and from the fact, furthermore, that all the epidemics that I have ever known the history of were at first regarded as contagious, and could be traced from one location to another as a matter of contagion. Then, on the other hand, there has been very strong proof that the disease is contagious, in its having been carried by Mr.

Chenery's stock from one place to another,—although it was carried to one place and did communicate the disease, and to another place and did not.

Q.—How, then, was it introduced into the well cattle?

A.—If it is a contagious disease, I should say it was communicated through the atmosphere, and the contagious agent,—what was derived from the body and the lungs of the diseased animal,—was inhaled into the lungs or the blood of the one that receives the disease.

Q.—In your judgment, would the blood be affected?

A.—I have no doubt that is the primary lesion.

Q.—Do you or do you not think this disease is in some degree a constitutional disease? Do you believe in its local nature altogether?

A.—I believe it is a constitutional disease—affecting the blood, and through the blood, that the lungs become involved. The natural location is in the lungs.

Q.—Do you think that the lung may be diseased, at the same time that other parts of the body remain perfectly sound?

A.—The lung may be diseased, and the other part of the system remain sound, throughout, if it is not a constitutional disease; otherwise not.

Q.—What evidence would convince you beyond a doubt of its contagious nature?

A.—Long and continued observation.

Q.—How long?

A.—Until the matter was conclusively settled throughout the community.

Q.—But then it may be settled as the nature of the epidemics you have alluded to were, and afterwards we may find we were mistaken?

A.—Very well; length of time has settled their nature, and they are only epidemic, not contagious. I lately travelled in southern Missouri and Texas, where they had a cattle disease which they called Texas fever. There was a great deal of strife between the different sections about that; and I found the people nearly divided as to its contagion, and it has existed there for eight or ten years, and the question is not yet settled.

Q.—But supposing that we began a course of investigation, to ascertain whether the disease is contagious or not. Suppos-

ing that the evidence from the beginning should uniformly go to prove that it was contagious, and could be traced from actual exposure,—so far the evidence would be on the side of contagion, would it not?

A.—So far, it would.

Q.—But you would not be fully satisfied?

A.—Not fully satisfied. There might be an epidemic or pandemic influence existing; and if one animal so diseased with the prevailing disease or epidemic, was brought in relation to a healthy animal who was predisposed on account of the epidemic, I believe the poison eliminated from the lungs of the diseased one, might be conducive to bring forth the disease in the lungs of the other, in the process of time. Therefore, I believe that some epidemics, in their origin, when they were violent, were to a degree contagious. I think typhoid fever was at one time contagious, but for the last five or eight years, I have not seen any thing to lead me to suppose it was.

Q.—Does not the same process of reasoning lead us to conclude that this disease is contagious?

A.—It may be so; but I would not put it on record that it was my opinion that it was contagious; for I consider the question still open.

Q.—In an exigency like the present, the testimony of observation up to this time being that the disease has been communicated, wherever it has been known to exist,—if action is called for,—it would be, in your judgment, the safest course for the public to be on the side of isolation?

A.—Most certainly.

Q.—You would think it, as a practical matter, best to treat this disease as though it were contagious?

A.—I should.

Q.—What do you think of the mode of treatment? Would you advise treating for a cure, or would you advise this wholesale slaughtering?

A.—So far as treatment goes, we have had similar attacks of scarlatina and measles, &c.; and we have no specifics for them. But I would treat this disease on general principles. If I found an animal hot and feverish, I would give cooling laxatives; if I found them wasting, I should use tonics, stimu-

lants, and nourishment; but as to specifics, it is out of reason to hope for cure.

Q.—Would you advise treating for a cure rather than killing indiscriminately, wherever a herd had been exposed?

A.—In my judgment, it would be better to leave the police regulation of the matter with each town, and recommend that they should have two farms set apart, one for those that were diseased, and one for those that had been exposed; and when any creature was so far exhausted with the disease as not to admit of any cure, I would slaughter it as a matter of pecuniary interest. To the others, I would give good diet and good air, and but little medicine.

Q.—Would you not do any thing?

A.—If I did any thing in the early stage, I should treat them upon what I should call the scientific idea of correcting the poison in the blood, namely, by antiseptics. I would begin with muriatic or chloric acid.

Q.—Would not that affect the milk of cows?

A.—Not injuriously.

Q.—In your judgment, Sir, would cattle thus affected be fit to eat, if they were fatted.

A.—I would not recommend that cattle that were sick so as to be emaciated should be used for food. But cattle that were in an improving condition, the health of which has become such that they are fattening, could not be deleterious. I judge from this fact,—that in a hospital at Marseilles, dogs were fed upon malignant tumors, and the blood of infectious diseases, and they grew fat and were healthy; but when the bile from many of these diseases was injected into the veins, they took the disease. And in the North, we know that the Laplanders eat dead whale with impunity and with improving health; and in some places, like the Amoor River, the inhabitants pile up fish till it putrifies, and then eat it, it being more stimulating than when taken in a fresh state. I believe that there is a great deal of prejudice in respect to food.

Q.—Then you believe in the antiseptic powers of the stomach?

A.—I do, if it is in a good, healthy condition.

Q.—Suppose this disease to be contagious, how far should you think the effluvia would extend?

A.—That would depend upon the location. If an animal was exposed to the disease, the wind blowing towards him, he would take it some considerable distance; but if the wind blew the other way, they would not take it in five rods, nor one. I have a case in my mind. I had a violent case of smallpox in a boarding-house. A girl was sick almost to death, and in the adjoining room, the boarders eat their meals; but no one took the disease. And from the testimony given, I don't take this disease to be as contagious as smallpox.

Q.—How many of the inmates of that house were vaccinated?

A.—I presume most of them were vaccinated.

Q.—Well, don't you believe that vaccination is reliable to prevent the disease?

A.—I believe it prevents smallpox, but not the varioloid.

Q.—As a general rule, what amount of isolation should you think would be necessary.

A.—If I had stock, I should want ten rods.

Q.—You have no doubt that a quarter of a mile would be sufficient?

A.—Ample.

The evidence here closed, and the Committee adjourned to three o'clock, P. M.

Afternoon Session.

Tuesday, June 5.

Met at three o'clock.

In the absence of the Chairman, Hon. Mr. Fisher, of Norfolk, was chosen Chairman *pro tem.*

The Chairman said the parties appearing before the Committee were now expected to present any arguments they might desire to offer.

Mr. Andrew.—I do not intend to trouble the Committee with an argument; but in consequence of having had placed in my hands by Mr. Wetherell a couple of treatises, or rather a medical treatise, called the "Stock Raiser's Manual," by Youatt, and a volume of the *Veterinarian*, in both of which I found some valuable matter bearing upon this disease, and the

results of observations and experiment, it struck me that I might perform some useful services by giving to the Committee the benefit of a few brief extracts from those portions of these books which touch upon this subject of pleuro-pneumonia. And I first call the attention of the Committee to "Youatt's Stock Raiser's Manual," edition of 1844. Under the title, "chronic pleurisy," or "chronic pleura-pneumonia," which appears on page 407, the writer refers to an article written by M. Lecoq, one of the teachers of the Veterinary School of Lyons, and although that appears to have been first published in 1833, Prof. Youatt reproduced it again in his edition of 1844, as if he had seen nothing of a later date to correct the opinions of Lecoq, entertained in 1833. The extracts from Lecoq's article occupy three pages of this book, but I will take up your time only to read two or three sentences on pages 408 and 409.

"M. Lecoq hazards some conjectures respecting the cause of this disease, which are very ingenious, and from which our breeders and graziers may derive some useful hints. He says that 'the graziers imagine that the animals bring the disease with them from their native country; and the traces of chronic disease which are found in them, even when they are slaughtered soon after their arrival, singularly confirm this opinion. Cattle that have been worked hard, and driven far, and somewhat too rapidly, are often attacked by diseases of the chest, which generally leave some dangerous traces behind them; and besides this, the breeders know their interest sufficiently well to get rid of those animals as soon as they can that have been affected with chest complaints.

"'The manner in which the journey is performed contributes much to revive the old disorder. The cattle purchased in Franche Compté are brought into Avesnes at two periods of the year—in the autumn and in the spring. Those which are brought in the autumn are more subject to the disease than those which arrive in the spring; and almost always, the years in which the malady is most prevalent are those in which the weather was bad during the journey of the beasts; and the disease is usually fatal in proportion to the badness of the weather.

"'The journey, also, is performed by two different routes—through Lorraine and Champagne, and often the disease appears only in the cattle that have arrived by one of these routes.

"'The manner in which the cattle are treated on their arrival, may contribute not a little to the development of the disease. They have,

perhaps, been driven a hundred leagues during bad weather; they have been half-starved on their journey, and they arrived famished and worn out, and, in fact, the greater part of them are lame. Calculating on their ravenous appetite, the graziers, instead of giving them wholesome food, make them consume the worst that the farm contains—all this is musty and mouldy; and it is often by the cough which the act of eating of such food necessarily produces, that the malady is first discovered.' "

He goes on to say that the treatment is the most unsatisfactory part of Lecoq's paper, and remarks that however skilful the treatment may be, the recoveries are very few. On page 409 is this language:—

"M. Lecoq finally enters into the question of the contagiousness of this disease. The farmers believe it to be contagious, and he is partly of their opinion. When a beast falls sick in the pasture, the others, after his removal, go and smell of the grass where he has lain, and which he has covered with his saliva; and after that, M. Lecoq has always seen new cases succeed to the first. He has also seen three cases in which the cattle of the country, perfectly well before, have fallen ill, and died with the same symptoms, except that they were more acute after they have been kept with pleuritic cattle. He, therefore, regards this affection as contagious; or at least, he imagines that, in the progress of the disease, the breath infects the air of a cow-house in which there were other animals already predisposed to this, or similar maladies. On the other hand, he acknowledges that many cases usually appear at the same time, and in cattle that have been widely separated from each other.

"M. Lecoq has very clearly stated the chief causes of this disease, in addition to which it has clearly an epidemic character. There are certain states of the atmosphere which call into action these lurking predispositions to disease, found most in the stranger cattle, but sometimes in the natives (for bad management, and hoose, and pleurisy exist too much every where,) but there is not yet sufficient evidence of the contagious nature of all these affections of respiratory organs. He, however, can never err who has recourse to the careful use of every precautionary measure."

So that down to 1833, the result of Lecoq's investigations, which are endorsed by Youatt as late as 1844, seems to have been this:—that the disease is thought to be "clearly epidemic," and there is a great deal to be said in favor of its being contagious. There have been coincidences which go to prove con-

tagion; and there have been many cases of disease appearing at the same time, though widely separated from each other.

The other authority to which I allude, is that contained in the *Veterinarian* for 1854, edited by Mr. Percivall—an article commencing on page 335 of vol. 27, entitled, "General Report of the Labors of the Scientific Commission, instituted by the Minister of Commerce, Agriculture and Public Works, for the investigation of the epizootic peripneumonia of cattle."

That was a French commission:—and the report, after giving some account of the history of the disease, under the head of "General Resumé of the Experiments instituted by the Scientific Commission on Peripneumonia," gives an account of experiments tried by the committee for ascertaining the influence which the organs of a healthy animal are capable of exercising in the course of cohabitation with animals of the same species suffering under peripneumonia.

"In instituting these experiments, the committee proposed the solution of the following questions:—

"1. Is epizootic peripneumonia susceptible of being transmitted by cohabitation from sick to sound animals?

"2. In the case where contagion is found operative in this manner, do *all* the animals of the kind living in the same habit of contagion contract the disease, or are there some who resist its influence? And, in the latter case, what proportion of animals fall sick, and what remain unaffected?

"3. Among those which contract the disease, how many recover their health, and in what conditions? How many sink from the disease?

"4. Are there any animals of the bovine species who prove decidedly opposed to the contagion of peripneumonia?

"5. Are animals of this species preserved for the future from being attainted with this disease, when after a first cohabitation they have presented no more than symptoms of slight indisposition, and that consisting principally in a cough more or less persistent?

"6. Are those animals who have contracted for the first time, more susceptible of taking the disease again?

"In order to obtain the solution of these questions, the committee have submitted to different proofs of cohabitation, 46 animals of the bovine breed, perfectly sound in health, and in such conditions of superintendence that they have never been exposed to the influence of contact of animals affected with peripneumonia.

"These 46 subjects of experiment have been disposed of as follows:—

20, at Pomeraye (first experiment).
2, at Charentonneau (second experiment).
13, at Maisons-Alfort (third experiment).
11, at Charentonneau (fourth experiment).

"Of this number,—
21 animals have appeared insusceptible to contagion in a first trial of cohabitation,
10 have manifested transient indisposition,
15 have taken the disease.

46 total.

"Of the 15 sick of peripneumonia contracted through cohabitation, 11 were cured, and 3 died.

Consequently, the number of resisting animals, to appearance, on the first trial of cohabitation, rose to . 45·65 out of 100
The animals insusceptible, to . . . . . 21·73 "
That of animals sick and cured, at . . . . 23·91 "
That of animals dead, at . . . . . 8·69 "

"But if, in place of reporting on the external appearances of animals exposed to cohabitation, we take into consideration the results afforded through the autopsies, which have demonstrated that six out of the eleven animals placed under experiment at the farm of Charentonneau (4th experiment) had contracted the disease, we should find that we must reckon six animals more as falling sick after cohabitation, and six at least as resisting (contagion), which gives, in point of fact, the following results:—

15 resisting, ...	32·61	out of 100.
10 insusceptible, ...	21·73	"
17 sick cured, ...	36·95	"
4 dead, ...	8·98	"
46	100·27	

"Of these, 42 animals who were exposed to the first proofs of cohabitation made at Pomperaye and Charentonneau, and which escaped with their health or recovery, 18 were submitted a second time to the same proofs, and of these 18, 4 a third time.

"These 18 animals became disposed of as follows:—

5 had contracted the disease at the end of the first cohabitation, and were cured,
9 proved refractory to the first influence of contagion,
4 experienced no other indisposition than that arising from the first cohabitation.

"As to the 4 animals who were submitted to the first cohabitation, they made part of the category of those who had contracted the disease from the first contact, and who were cured.

"None of the 18 subjects submitted to these fresh proofs, in such conditions, either contracted peripneumonia or presented even the slightest symptoms of indisposition.

"From results obtained from such experiments of cohabitation, the committee have drawn the following conclusions:—

"1st. That the epizootic peripneumonia of horned cattle is susceptible of transmitting itself through cohabitation, from sick animals to those in health of the same species.

"2d. That all animals exposed to contagion through cohabitation do not contract peripneumonia; there being some among them who thoroughly resist the contagious influence; and others who do but experience, under such influence, a slight indisposition and one of very short duration.

"3d. Among the animals who contracted the disease, some recovered, and obtained with their recovery every external appearance of health, while others succumbed.

"4th. Such animals as presented symptoms but of slight indisposition after a first cohabitation, appeared preserved by this trial, for the future, against other attacks of peripneumonia.

"5th. Animals who had been for once attacked with pneumonia, did not appear susceptible again of its influence.

"Such are the general conclusions which the committee believed itself authorized to draw from such experiments of contagion through cohabitation. As to the questions of ascertaining what may be, in a herd exposed to the influence of contagion, the relative proportions of animals remaining resistent to contagion, of those who become indisposed, and, lastly, of those who contract pneumonia—and among these last what is the relation of the dead to the recoveries,—the committee have not contemplated uniting so large an assemblage of facts, in order to come to a conclusion that might express absolutely the conditions habitually passing in practice. It has confined itself here to the ascertainment of the amounts resulting from particular experiments.

"From a summing up of these experiments, we find that 45 animals out of 100 have contracted peripneumonia through cohabitation, and that 24 have experienced slight indisposition: to resume, 65 have felt the influence of contagion in slight degrees, and 32 have shown themselves refractory to it.

"The proportion of animals who have recovered every appearance externally of health, after having experienced the disease, has been at the rate of 83 out of 100 sick, and that of those who have died, of 17 per cent."

I have taken the liberty to read this part of the Report of that Commission *in extenso*, for two reasons; first, because it illustrates what is something like a scientific investigation; and, secondly, because the result of the investigation, carefully and scientifically performed, is so encouraging to the farmer, and those interested in the preservation of stock.

I am not a farmer, Mr. President, although I was born and bred on a farm, and hope, some day, to be able to return to it. I suppose I have as much interest, in my feelings, in the prosperity and welfare of that part of the industrious and productive community, as any one: for, in truth, the only brother I have in the world is a farmer, and earns his living on a farm. And I do not know that I have any relative who is so unfortunate as to have to gain a living wholly apart from rural pursuits. I feel, therefore, greatly interested in the result of this experiment which is being made in Massachusetts; and, by reason of that interest, I have been the more willing to accept the invitation of some gentlemen, to contribute, if I may be able to do so, professionally and as their counsel, somewhat toward the success of the investigation of the Committee, by assisting its labors.

And I am always strongly impressed with this conviction, *a priori*, that there is no such thing as an absolutely remediless evil in the universe of God,—certainly no more in physical nature than in the world of morals and ideas. There is no falsehood, in thought, not susceptible of conviction and correction; there is no error in the domain of ideas, about which men may not be set right, and as to which they may not be sometime able to arrive at the truth, if they pursue its investigation. And there is no evil, existing in the material universe, contradictory to those ordinary laws—which are only the common and normal manifestation of Providence itself through the world of nature,—none, which is not curable. The evil is not itself a part of the law; it is only an exception to the law. And, therefore, I think there is no disease affecting man or animals or vegetation, which is not susceptible of being, to a greater or less extent, satisfactorily overcome. I do not mean to say that I am so much of an optimist that I believe that, this side of the millennium in the future, or this side of the fabled millennium of the past—the Golden Age—we can ever arrive at

a state of physical perfection,—of perfect material happiness in the physical world. But I do not believe that there is sent among men any evil which human science, human knowledge, carefully applied to the facts presented, may not combat, with an amount of practical success which will amply and completely reward all their exertions.

I do not believe that this disease sent among cattle is intended to destroy and sweep off from the face of the earth, nor from the face of any considerable portion of the earth, the whole bovine race, any more than I suppose that smallpox or malaria was sent with the intention, on the part of Providence, that it should extirpate humanity from the face of the globe.

Nor do I suppose, on the other hand, that the careless, impatient, thoughtless, or frightened observations of a few persons, limited to a few localities, made under unfavorable circumstances, in moments of excitement and panic, even though the observers may be persons of the greatest intelligence, and in communities of the greatest intelligence, will afford much more information, of a satisfactory and reliable character, than the information which we might derive from a committee of North American Indians who might report to us concerning the origin, progress, fatality, and means of cure of the smallpox, when it should be prevailing among them. The truth is, I suppose, that it is only after communities have had their attention called, for the first time, to such phenomena as this, that they ever begin carefully to investigate them; and it is only after such diseases have prevailed some considerable time, that they are able to accumulate an amount of knowledge sufficiently ample and sufficiently various to secure any useful generalization from these facts. And it seems to me that the dangerous error into which we in Massachusetts are liable now to fall, is the mistaking of coincidences for consequences. And that is an error the most natural into which the mind ever falls, when dealing with a variety of facts. Nothing is more deceptive than a series of coincidences from which we leap at once to the conclusion that the thing which followed after another thing was, of necessity, its consequence, and that the preceding fact was the cause of that which came after.

Now,—if the Committee will indulge me with making a single suggestion,—it seems to me quite clear that the result of this investigation, thus far, has convinced us all of the truth of what Mr. Walker said in the outset. In reply to the question, "Have you formed any opinion in regard to the time necessary for the development of the disease after exposure?"—his answer was, "In regard to that, we have not. And it is the want of knowledge of the laws of the disease, that is the great obstacle to our operations. And it is the most alarming fact in regard to the disease, that it does not seem to be understood at all in this country, nor even in Europe, where they have had it for two hundred years."

And Mr. Walker informed the Committee, also, that "the Commissioners became entirely dissatisfied with the current condition of things, because other measures beside merely killing and burying, are quite as necessary and important. And when they arrived at that point, and discovered to what extent the infection had spread, they stopped killing the herds."

Now, it seems to be the most natural remark, in view of this testimony, of the conclusions which follow from it, and of the opinion expressed by the Commissioners, that it is time that the laws of this disease were understood, or, at least, that a thorough, honest, scientific, and persistent effort should be made to discover them. And if there is any place in the world where that effort can be made with reasonably probable success, I think it is here, in Massachusetts. We are not encumbered here with the slow and ponderous legislation of older countries. The legislature is convened, here, upon a notice of a few days, and upon the application of a few persons; and the inquiries of intelligent minds, from every district in the Commonwealth, are brought directly to bear upon this subject, which is so interesting to the pecuniary interests of the larger portion of the people, and which involves so much of the welfare of the people. And we have, here, within this Commonwealth, men, too, who, by natural capacity, by long previous study, by aptitude for the inquiry, by willingness to enter into detail, and by the training which gives them facility in research, are preëminently fitted for the task of investigation, and whose presence among us would enable the government of Massachusetts to

secure, to this people and to humanity, the services of as able and as satisfactory a commission as could be organized any where in the scientific world. It even happens, if you will recollect, Mr. Chairman and gentlemen, that Massachusetts seems to have raked over, to have spread a drag-net, over the whole world of science, and have swept within its borders men of the highest capacity,—men not only of Yankee, not only of American, but of foreign birth. We have, lying behind us, so far as it is collected and preserved, the history of two hundred years of this disease. We have the benefit of the researches of men like Lecoq, by men such as those who have formed the French commission, by men such as have been, from time to time, commissioned by the British parliament, the Danish and Belgian governments; and we have the whole world of inquiry and research, over which to spread our investigations. Why, then, Mr. Chairman, should we not, with all this treasury of the past to draw from, with all these workers and delvers and thinkers in the world of science to press into the service, and with Massachusetts itself transformed, in the course of Providence, into a hospital, where the examination can be best carried on,—why should we not organize a commission, or series of commissions, charged with the duty, by actual, thorough, scientific experiment, of reducing this disease down to its laws? It must have laws. There is no such thing in the universe as even a disease, I suppose,—which is itself such a contradiction of nature,—not governed by some laws. The most abnormal thing, even, has a law: and this disease, as well as others, must be hemmed in within certain possible limitations of spread, of disaster, and of deadliness, which may be discovered, or the discovery of which may at least be approximated. It must be subject to some limitations; it must be subject to the possibility of some remedial process, which may, at least, by controlling, limit or alleviate, if not cure. One would suppose it must be, as smallpox, and many other diseases, are, capable of prevention. At least, it is worth while to ascertain, if possible, whether it may not be checked in its progress by some means, applied either directly to the infected or to the suspected subject; or by means of some disinfectant which may be applied to hundreds. But nobody knows, as yet, any thing in regard to this. Professor Youatt goes no further than Lecoq. In 1833, he

thought it was epidemic, and he "kind o'" thought it was contagious. He knew that animals brought in direct contact with others were likely to have the disease, and thought it was more severe in proportion to the nearness of the approach to the infected animal. And yet, he was obliged to confess that the disease did spring up spontaneously and sporadically, without any cause arising from any positive and known contagion whatsoever. Our Commissioners, and our own men of science, whether physicians, or others, who have given the subject some investigation during the short period which has illustrated the history of the disease here, in Massachusetts, have not been able, nor have they undertaken, nor has the legislature of Massachusetts, as yet, charged them with the duty of undertaking, to classify all cases, or to try experiments such as those which were tried by the French commission. The most they have undertaken to do, was to trace, if possible, from one case of the existence of disease, its own private history, back to some other case of existence of the same disease. But nobody has yet tried the experiment of operating in the counter direction, of proving the supposed rule of infection, or contagion, by subjecting any considerable number of sound and healthy animals to the presence and contact of supposed infection, or contagion, for the purpose of seeing whether or not the rule will work the other way. And unless that sort of careful, exact, and specific experimenting is pursued, the mind arrives, at last, at no more exact and scientific knowledge than did the English student of whom the old story is told,—that he undertook to walk the hospitals of Paris, for the purpose of observing disease, and especially fever; and, following after a learned physician, in his visits to his patients in the hospital, he observed, one day, a man sick with typhoid fever, and that the physician ordered him to be served with some frog soup. Thereupon he entered in his diary, "Patient sick with typhoid fever. Perscription, frog soup;" and he watched that patient until he was discharged, well; and then, in his diary, reported the case as cured, and generalized as follows: "Typhoid fever is cured by frog soup." He went home, to London, and in the first case of typhoid fever which came under his own immediate care, he administered the same remedy: and the coincidence in that case, whether it was a "consequence" or not I don't

know, not being a doctor, was that the patient died; and the student corrected his diary thus: "In typhoid fever, frog soup cures a Frenchman, but kills an Englishman." Now, that is a story of some old joker, which does, in truth, after all, illustrate the danger of attempting to generalize laws, or even rules, of the natural world, from any inconsiderable number of cases, and the danger of undertaking to draw your inferences in a direct line, without reversing your process. And there is also this danger, which the inquiry always encounters, the danger of being controlled, in our inquiries, as, I suppose, we almost always unconsciously are, by our own pre-conceptions. The most natural thing for all men, as all history shows, in the presence of a new and wide-spread disease, is to conclude, at once, that the disease has some characteristic not only alarming, but mysterious; and almost always, to suppose that it must, of necessity, be imparted from one person to another. And in the progress of the human mind, through all the ages of medical inquiry, down to the present, so far as I,—who have never read medical books, but only those which are comprehensible by laymen, and common minds like my own,—have been able to perceive, the tendency always is, not only to look upon the malady with alarm, but to suppose it to possess some peculiar and occult danger, in itself. And yet, it has almost always turned out, in the history of such cases, that, with the progress of observation and theory, of inquiry and experience, that after a few years have passed, the result has been to show that the disease was no more mysterious than is all disease. In a very small proportion, I believe, do they ever prove to be infectious or contagious. In a short time the panic goes by, and men resume their ordinary avocations, and their ordinary confidence in man and nature and God.

Now, I am not bold enough to think it possible for me to say any thing which would aid the wisdom of this Committee. Therefore, I do not assume the task of attempting to invite the Committee to any particular course of legislation, in the detail; but I beg leave to suggest, in the general, simply, that inquiry is the first duty, and that it seems to me there ought to be a commission, of ample powers, and ample in point of numbers as well as *in the personnel* of which it shall be composed, and ample in point of powers, to seize this opportunity of submit-

ting the disease to the fullest investigation possible to modern science; and, secondly, that this or some other commission should have some general powers, not too closely limited, some powers with a pretty wide margin, which they might be allowed to exercise, as the developments of days and weeks and months, during the recess of the legislature, may show the necessity, for the purpose of controlling, should their discretion thus dictate, the ingress, egress, and progress of cattle, whether healthy or diseased; and of making sanitary regulations and examinations; not, however, it seems to me, exercising itself, full and complete control over the whole domain of the Commonwealth, for that would seem hardly possible to any one Board of moderate size; but holding a sort of supervisory or appellate power over the local authorities. And I would suggest, that for this purpose, the board of selectmen for example, in each town, should be empowered to exercise a certain measure of control over the movement of cattle within and through their towns, and the management of diseased or exposed cattle within their respective domains. If that were done, you would have the local authorities directly interested to examine carefully within their own dominions, and under the influence of the public opinion of their own neighborhoods; and they, in their turn, if led too far astray by momentary panic or error in their neighborhoods, would be controlled by the appellate power of the Commission, and also, they would be directed by them, if found careless, lax, unfaithful, or unwilling to exercise their powers. If you had one Board to exercise this power, similar to the Commission now in existence, and another to pursue this subject as matter of scientific inquiry, perhaps these two Boards would mutually act and re-act upon each other, assisting the labors and studies of each other. I desire to speak diffidently as to the question whether it is best to create two separate commissions, or otherwise. Other gentlemen, of more experience and better judgment in these matters, will be able to suggest, upon this point, with more positiveness and pertinence than myself.

That something should be done, public expectation requires; that, if possible, a most exact and intelligent examination should be made, duty to ourselves, to the people, to posterity, to science and humanity, certainly demands. Whether there

should be any thing done like that which has hitherto been attempted, I mean the effort to limit this disease by the destruction of exposed animals, I certainly am compelled to doubt. Hopeless cases of disease, it is quite easy to see, can best be met by the destroying axe or knife; but as long as cases are hopeful, at least, so long as herds or individual animals are only suspected, it seems that a wise man,—a man who believes that nature is governed by laws, and that in the presence of civilized man almost every physical evil recedes when treated with a firm will and strong hand,—will be inclined seriously to doubt the policy of such a heroic remedy.

I am much obliged to the Committee for listening to these hasty and not well-condensed remarks of one who owes his privilege to speak solely to the sufferance and complaisance of the gentlemen whom he has had the honor to address.

Mr. Bird.—I have nothing to say, Mr. Chairman, except, that so far as I represent the Remonstrants, we are entirely satisfied with the position of the case as we are now. The examination has shown me that the further we go, the less we are satisfied we know about it. And, therefore, I have nothing to suggest or recommend, except to agree entirely with what has been said, in regard to further investigation before we spend more money.

Mr. Lathrop requested that the Commissioners should be allowed the privilege of presenting some additional remarks, through Dr. Loring.

The Chairman.—I suppose it is generally understood that the Commissioners will be properly entitled to close the hearing before the Committee. If there be any other person or party, that may desire to come before the Committee, the present is the proper time. Otherwise, the Committee will hear the Commissioners. After the Commissioners have made their statement, the public investigation before the Committee will be closed.

No other person appearing before the Committee,

Dr. George B. Loring, on behalf of the Commissioners, then addressed the Committee, as follows:—

Mr. Chairman, and Gentlemen of the Committee :—

I am very sorry to feel compelled to trespass any further upon your time. For I have been with you, here, for many days, through a long and tedious investigation ; and if the statement of the gentleman who preceded me is true,—that after all this trouble and all this expense, we have received no light upon this subject,—it seems to me an act of utter folly that we should waste any more time, to say nothing of money, in endeavoring to obtain that light. But, Mr. Chairman, I am not by any means satisfied that such is the case. I do not believe, Sir, that this is a panic, in the State of Massachusetts. I am not ready to believe that the citizens of Massachusetts are, as the gentleman who preceded me said, in the condition of North American Indians, in a state of terror ; or, as was stated here yesterday, that they are like the panic-stricken nations of old, who killed Jews and witches to stop distempers. I believe we are an intelligent, enlightened, cool, and dispassionate community, understanding what our pecuniary interests are, and abundantly capable of taking care of those interests. I am very sorry indeed, to ask you to listen to me : but I am satisfied that I can show you, not only that there is no panic here, but that the statement that there is nothing known of the disease, is not true. Much to my surprise, the testimony of one of the Commission, if I understood it, was given here, this afternoon, to the effect that the Commission ceased their labors for want of knowledge.* I wish the Committee, the State of Massachusetts, and the country generally, to understand, that the Commission stopped their labors solely because they wanted assistance and legislation from the Commonwealth of Massachusetts, to aid them in a work which they are entirely satisfied will be of the last importance to the agricultural interests of the Commonwealth,—the removal of this evil.

The Commissioners have in every possible way endeavored, in coincidence with the newspapers of the Commonwealth, to lay before the people all the facts contained in this curious history of disease,—how it began in Belmont, and was transplanted, almost in an hour, to Brookfield ; how it was hedged in

* See, at the close of Dr. Loring's address, remarks of Mr. Walker, correcting the misapprehension as to the effect of his testimony in this particular.

at Belmont, and how, step by step, it spread all over Worcester County. You have all these facts before you, in this State, now. Will you listen to me for a few moments, while I state in brief, what the facts are upon the continent of Europe, where the disease has been known almost from its inception.

I proceed to give the symptoms of the disease, as described by Prof. Simonds:—

"The early symptoms are not, as I have before observed, very easily recognized. They may sometimes consist of a little staring of the coat, a very slight cough, and a fastidious appetite; and these indications of disturbed health may show themselves more particularly at one period of the day than another. For example, in the first part of the morning supposing the animals are at pasture, you will find that they are standing under the hedge and not feeding so well as they will when the sun gets up; they will then be mingling with the herd, apparently in good health; or if animals are in sheds, we observe that at the first outset of the disease we have but little more evidence of their being affected than a slight cough. As the diseasea dvances,—and the rapidity of its advance may be governed by many secondary causes, such as living in a confined, badly ventilated, low, dirty building,—we find that a difficulty of breathing exists, the appetite is lost, the cough more frequent, the pulse increased, the coat staring, and so on. In a later stage the breathing will become more laborious, and the surface of the body will be irregular in its temperature. The horns, and the ears and legs may be cold while other parts of the body will be warm, rigors will now and then show themselves, the pulse will become more frequent, and the animal be found to grind its teeth as an expression of pain. The bowels at this stage of the disease become not unfrequently irregular, and in fact diarrhœa sets in, and in the still later stages of the disease the food which is in the stomach goes into a state of fermentation. The animal becomes tympanitic, the bowels more irregular, the body deadly cold, the pulse nearly indistinct and rapid; and in this condition it dies."

The disease at present existing in Massachusetts under the name of pleuro-pneumonia, appears to be identical with that which has been known in Europe for many years under the names of exudative and contagious pleuro-pneumonia—differing from the common pleuro-pneumonia chiefly in its contagious character.

According to the report of the scientific commission instituted by the Minister of Agriculture, Commerce, and Public Works of France, for the investigation of epizootic pleuro-pneumonia of cattle, this disease was "confined in former times to various isolated regions of the mountains of Piedmont, Switzerland, Franche Compté, the Jura, the Dauphine, the Vosges, the Pyrennees, and the Auvergne; and rarely made into the agricultural interests more than partial inroads, of which the public weal was hardly sensible," until after the year 1789. No contagious disease of cattle, so far as history shows, was pleuro-pneumonia, until that date. At that time, the barriers which had heretofore restricted the commercial relations between the different provinces of the country were removed. From this cause, and the moving of large droves of cattle from place to place, for the supply of troops during the state of warfare then existing, the disease was introduced into France, and still remains there, having extended over a large portion of that country. According to Dr. Willems, of Hasselt, "it has existed in Belgium since 1828, and came to that country from the south of Europe, from regions heretofore exposed, where it had existed for a very long time." It was introduced into Hasselt in 1836, by some cattle purchased in Flanders by the father of Dr. Willems, and is now known in that country under the name of exudative pleuro-pneumonia. Dr. W. also states "that the Belgian government spends yearly more than 100,000 francs ($20,000) as an indemnity for the infected cattle killed by the butchers. In the Low Countries, the losses are immense, and according to the reports of the French scientific commission on inoculation, "in two hundred and seventeen communes in the Northern Department alone, during a period of nineteen years, there were as many as as 52,000,000 francs ($10,400,000) paid as indemnity" for the loss of cattle by the disease. In the report of the Minister of Agriculture, &c., alluded to above, it is stated: "Nor are most other countries of Europe more free from this pest than our own. In Italy, Sardinia, Switzerland, Austria, Hanover, Sweden, Denmark, and of late in Holland and England, very considerable ravages have, the same as in France, inflicted public damage to an extent with difficulty to be repaired."

According to Prof. Simonds, Veterinary Professor of the Royal Agricultural Society of England, considered the highest authority on these matters, a gentleman of attainments and skill, the disease appeared in England about the year 1841, and has continued with greater or less virulence to the present time. The pestilence which was introduced into England in 1713, and again in 1744, by calves brought from Holland, is considered by the same authority to be identical with rinderpest or steppe murrain, a fatal and destructive malady still found in Central Europe. The disease now found in England, however, is the contagious pleuro-pneumonia, and it prevails in many parts of the kingdom. During the winter of 1859–60, "this fatal disease became very prevalent in the London dairies, more particularly on the south side of the Thames. The attacks are marked with much virulence, in a very great number of instances. And some of the public prints which have drawn attention to the matter, state that the fatality is as much as 95 per cent." So much for the history of the disease in Europe. The facts with regard to its introduction and progress in our own State are too well known to need repetition here.

Now, gentlemen, the first question which arises with reference to this disease, which has been discussed here from day to day, is,—is it contagious? We have had testimony after testimony produced in its behalf, many historical facts in its favor, some remarks and opinions against it. Now there seems to be, in all this testimony, abundant evidence, which has been repeatedly laid before the public, that it is contagious, at least in its course here. The opinions of European investigators, on this head, will be of great service to us, in our own explorations. Professors Norton and Simonds say, in regard to pleuro-pneumonia, that sanitary measures should be adopted to prevent the "*contagion.*" In the examination of Prof. Simonds before a committee of the British Parliament in 1857, he says, in answer to a question with regard to the contagious nature of the disease: "I think pleuro-pneumonia is equally contagious with glanders." In reply to the question, "Is it your opinion that from the very first moment the disease attacks the animal, it is contagious?" he replies, "I believe it is so." He says: "Pleuro-pneumonia I believe to be infectious; that is to say, exhalations arising from the diseased animal's body become dissemi-

nated through an apartment where a certain number of animals are placed, and some of the animals susceptible take it." This view of the question is sustained by many observations made in this country.

And I say, moreover, that wherever these exhalations are confined within barns either battened or kept close by clapboards, they are infinitely more dangerous than in those barns of which I spoke when I asked Dr. Bigelow if common country barns and meadow hay would cure the disease. He said that well-ventilated barns would cure it. I asked him if such a barn as these, with the wind blowing through it, would answer the purpose. He thought not. I asked him if warm barns and English hay, would cure it, and he said he did not know.

In the Report of the labors of the scientific commission, instituted by the Minister of Agriculture, &c., in France, the following statement is made. (I heard the gentleman who preceded me quote it, but I have it in brief, before me, and it goes directly to the result.) "From information collected by three of the inspectors, we learn that contagion was to be regarded as the principal cause and first producer of the malady, in localities where it prevailed, over a great extent of country." An experiment was instituted by this commission in order to decide the question whether the disease "can be transmitted from diseased to sound animals by cohabitation?" Forty-six animals were selected for the experiment, and the following were the results: twenty-one animals have appeared insusceptible to contagion in a first trial of cohabitation; ten have manifested transient indisposition; fifteen have taken the disease." And the general conclusions arrived at were: "that the epizootic peripneumonia of horned cattle is susceptible of transmitting itself, through cohabitation, from sick animals to those in health of the same species." Second. "That *all* animals exposed to contagion through cohabitation, do not contract peripneumonia, there being some among them which resist the contagious influence, and others which do but experience, under such influence, a slight indisposition, and one of very short duration." Third. "Among the animals which contracted the disease, some recovered, and obtained with their recovery every *external* appearance of health, while others succumbed." Fourth. "Such animals as presented symptoms

but of slight indisposition, appeared preserved by this trial, for the future, against other attacks of peripneumonia." Fifth. "Animals which had been for once attacked with peripneumonia, did not appear susceptible again to its influence."

In Holstein, Mecklenburg, Lubec and its territory, Hamburg and its territory, the belief in its contagion is so strong that the slaughtering of all diseased and exposed animals is considered the only effectual means of staying its progress.

In September, 1857, the disease appeared in Melbourne, Australia, having been brought there by cattle imported from Europe; and so thoroughly satisfied were the colonists of the contagion, after a most careful investigation, that the herd into which it was introduced were slaughtered at once; and it was resolved at a public meeting, after hearing the report of a veterinary surgeon ordered to examine the diseased herd, that, "It is the opinion of this meeting that the disease in question is contagious pleuro-pneumonia, and that at present it exists on Mr. Boadle's farm." And "that, as the disease called pleuro-pneumonia, if allowed to spread, will be very disastrous to the colony, this meeting is of the opinion that the cattle should be purchased, for the purpose of being destroyed." The measures proposed by the meeting were unanimously adopted.

Perhaps gentlemen will say that these people were insane, that they had a panic, that they ought to have called a scientific commission to sit around Mr. Boadle's farm, six months, and investigate the subject of the disease,—that the time had arrived when Australia could learn something about the disease and apply that knowledge to its cure,—that there was a golden opportunity, which Australia would lose if they extirpated the disease. No, gentlemen! the cattle were bought up and extirpation applied as the remedy for the disease, and it was cured. Do you not wish that this same remedy had been applied here, before the disease went from Belmont? Mr. Chenery said he was damaged to the extent of fifteen or sixteen thousand dollars, by the visit of the Commissioners to his barn. Would not the Commonwealth have given him a hundred thousand dollars, to have driven his cattle out of the barn, and slaughtered them, at the commencement of this history?

All these conclusions, drawn from the history of the disease in Europe, are founded upon facts. I do not know of a single

fact that has been stated here, I do not know of one brought up by the Commission, I do not know of one presented in the newspapers, or produced in any form whatever, that does not tally with these facts which I have collected from the history of the disease in Europe.

In the *Veterinarian* of 1845 is an abstract of the history of pleuro-pneumonia in the British empire, by Mr. Copeman, of Walpole, England, which will illustrate its contagious character there, and will remind many of its progress in this country :—

"This disease does not appear to have entered Scotland until April 1843, (*Veterinarian*, vol. xvi. p. 278). Mr. Fulton, of Wigtown, in a communication to Professor Dick, of Edinburgh, states that this disease had just made its appearance in that vicinity; and the professor, in his answer, does not mention having seen a single case, at least in Scotland. However, in the same volume, p. 282, we find that it has been close upon the borders for a long time. Mr. Carlisle, of Wigton, in Cumberland, thus writes: "Its first appearance in this part of the country was among some Irish cattle; and one or two of my employers purchased some of the infected ones, not knowing that any thing was amiss at the time. They were sent off to pasture on the farm, and, in two or three days, one of them was observed to be unwell. He was brought home, bled, and physicked, but soon died. Little notice was taken of him, and his remains unattended to. In the course of a short time, two more became ill: they were brought home, and also died. Presently the disease showed itself among his other stock. This was in December last, and he has since lost nearly thirty head of cattle, not more than five or six recovering. The treatment was strictly depletive."

"Thus we are led to infer that this disease commenced in Ireland; and, as most of the Irish cattle are imported at Liverpool, we first hear of its existence in that part of our isle. In a short time it finds its way into Cheshire, Shropshire, Staffordshire, and Middlesex; but is not heard of in any of these parts until 1842, although it had been raging in Ireland for nearly twelve months before. Thence it, not very rapidly, spread over almost every part of this country, but did not reach Scotland for nearly another twelve months.

"I shall now, in as few observations as possible, endeavor to give the history of most of the cases that have occurred in this neighborhood; and in doing this, I hope to be enabled to adduce sufficient facts to enable the most sceptical to appreciate the infectious or non-infectious character of this disease, and also to demonstrate the truth of the assertions, that pleuro-pneumonia was introduced into this country by Irish cattle.

"CASE 1.—The disease first made its appearance in this neighborhood in May, 1843, at an extensive farm, called Scot's-hall, in the occupation of Mr. C—, of D—. Mr. C. had purchased thirty year-old Irish cattle (steers) at Norwich stock market, in April. They were at the time in apparent health, and were sent to the marsh. About a fortnight after, one of them was observed to be unwell. He was taken home and died. On the following day, upon a *post mortem* examination it was evident that he had died of pleuro-pneumonia.

"On the 11th of May a second was attacked, and by the 1st of June five others. The treatment of all was strictly antiphilogistic, and they all died, after from a few days to a fortnight's illness. In an adjoining house, but separated from it by a yard and wall, the dairy cows, consisting of thirteen in number, were milked, and were at the time considered quite safe: however, on the 11th of June, one of them was attacked, and in a few days another. The Irish beasts were all coughing, and, on being closely examined, evident symptoms of pleuro-pneumonia were detected in nearly all of them. They, with the two cows, were immediately sent to another farm about a mile and a half distant from the marshes: every attention was here paid to their general comfort, and the medical treatment altogether altered, consisting of remedies similar to those advised in the following pages. Several of the Irish were many weeks in recovery, and the two cows died. As several of the other cows were evidently infected, Mr. C. determined upon selling them all. They were accordingly sent to market, and sold.

"Mr. C. had twenty-four short-horns in another marsh, and his neighbors had also in the surrounding marshes cattle of all ages, but not a single one was attacked with this disease during the summer.

"CASE 2.—Mr. G—, of H— Hill house, bought of a stock dealer at Norwich, fifty-three two-and-a-half-year-old short-horns on July 15th, 1843. They were in excellent condition, and to all appearance in health; they were sent home to his farm, and, on the following day, down to the marsh. On the next morning one of them was found dead, and in the course of a fortnight six others were attacked. Being forward in condition, they were sold to the butchers; but the lungs and chests of three of them were so much diseased, that the fore-quarters were of no value. The remaining forty-six were sold to the butchers, and, in a few weeks, all were killed. Several of them were hoosing. Their lungs were found more or less diseased, while the lungs of several others appeared perfectly sound.

"On the following Saturday, July 22d, Mr. G. bought twenty-three others, all nearly fat, and looking exceedingly healthy; but the man who drove them home observed that they were nearly all hoosing. They were placed in a marsh by themselves. On the 4th of August one of

them was attacked, and after a few days illness, died. After this several others were attacked, and, being fat, were slaughtered.

"Case 3.—Mr. F—, of N—, bought at Norwich stock market thirty Irish buds. After about a fortnight, July 29th, three of them were attacked with this disease, and, in the following month, fourteen others. They had been put into the yards with the cows, and several of them became infected. Fresh cases were occurring almost every week. Mr. F. finally determined to sell the whole off the farm. He then tarred the boards, washed them, &c. After several months he bought other neat stock, and all of them have been healthy.

"Case 4.—Mr. G—, of C—, bought at Norwich market, in July, 1843, fifteen year-old Irish steers. During the two following months, eight of them died of this disease. They were attended by a cow-leech. The treatment was strictly depletive. The remaining seven were sent to Norwich market, and sold. After this, two bullocks were attacked; they were Scots, nearly fat, and were slaughtered: the right lobe of one of their lungs weighed fifty-three pounds. In October two others were also attacked; here the beasts were lean: they were treated as hereafter directed, and both recovered.

"Case 5.—Mr. G—, of H— Hall, bought of an Irish stockhealer, on August 22d, twelve Irish beasts, consisting of nine year-old steers, and three two-year-old heifers. They were at the time in apparent health, and were not allowed to pasture with any other stock. On September 3d, one of the steers was found almost dead. He died on the following day. On the 27th, one of the heifers was attacked. I now closely examined all the rest, who were looking very fresh in condition; but, on putting them in quick motion, the respiration of two of them was much more than naturally increased, and they frequently coughed. These symptoms, with the characters of the sounds furnished by ausculation, confirmed, in my opinion, the existence of pleuro-pneumonia in its incipient stage; of which I apprised Mr. G., who, rather than hazard the safety of his other cattle, ordered the heifer to be immediately killed. On the same day he sold the remaining ten to a dealer, and, after passing through two or three hands, they again came into this neighborhood. Mr. F., of U., not more than two miles distant from Mr. G., bought them and turned them into his yards with his cows, &c. On the 6th of October, the two I have mentioned above, as being infected, were severely attacked, and died in a few days.

"The dealer who sold them to Mr. F. took the others back, and sent them to Norwich market, and sold them; but it was afterwards discovered that a cow and heifer which had been with them in the yards were infected; the cow being attacked on the 25th, and the heifer on the 29th of October. They, however, eventually recovered.

"CASE 6.—Mr. C—, of U—, bought six two-year-old Devon steers of a dealer early in September, 1843. They were turned upon a piece of aftermath. They were all observed to have a short, dry cough: still they improved in condition up to October the 8th, when one was attacked with pleuro-pneumonia, and on the following day the other five were become ill. They were all severely attacked. Two died, and the rest recovered. They have been at grass all the summer, and are now excellent beef.

"CASE 7.—Mr. N—, of Y— Hall, bought of a dealer at Norwich, October 7th, 1843, five short-horn bullocks, who were looking healthy and very fresh. In the following week he lost one, that lived only a few days after he was attacked; the others were frequently hoosing. He had them all killed by the butcher, and the right lung of all was found much diseased.

"CASE 8.—S. G—, Esq., of T—, bought at Norwich twenty. three-year-old Scots in good condition. In the latter part of November two of them died very suddenly of this disease, and, a few days afterwards, six others were attacked. One died, and the rest recovered. None of them had any communication with the other stock upon the farm; but, by accident, after the third beast was flayed, two of the cows and a heifer strayed into the place, and smelt at the skin and carcass, and very soon became almost frantic. They were kept apart from their companions, and, fifteen days afterwards, one of the cows was attacked with pleuro-pneumonia. On the twentieth day the heifer, and on the thirty-first day the other cow became ill. The last died, but the other two recovered.

"CASE 9.—Mrs. B—, of A—, purchased of a dealer, in November, six short-horn steers. About three weeks afterwards two of them were attacked. The first died, and the other was slaughtered. A cow and heifer that were tied up in an adjoining house became infected, and the cow, after a very protracted illness, died.

"CASE 10.—J. G. C—, Esq., of U—, on the 6th of January, 1844, bought eight three-year-old Irish beasts at Norwich market. They were put up to fatten at an off-farm, and were doing well up to the 15th of February, when one was attacked, and, after lingering about a fortnight, died. Three others were shortly afterwards attacked, but, as they were fair beef, were sent to the butchers: the right lung of one of these weighed nearly forty pounds, although he had been killed as soon as the disease was fully detected. As symptoms of the disease, i. e., hoosing, were present in the other four, they were taken to Norwich, and sold.

"CASE 11.—*July* 3*d*, 1844.—I was this morning requested by Mr. L., of L—, to attend a cow laboring under pleuro-pneumonia. Mr. L., who is a very intelligent farmer, gave me the following history of the rise and progress of this disease upon his farm. Early in December, 1843, he

purchased of a stockdealer two Irish bullocks, and tied them up to fatten with four out of a lot of eight short-horns bought of a neighbor. The other four were sent directly to another farm; and they all fattened and did well. He observed that the Irish beasts were frequently hoosing; still they got fat quickly, until the 20th of February, when one was attacked. He was immediately removed from the others, and died in a few days. Mr. L. now observed that the short-horns began hoosing, and on the 13th of March the one in the adjoining stall to the Irish beast refused his food. He was directly slaughtered. His left lung was very much enlarged, firmly adherent to his ribs, and that side of the chest contained about one gallon of fluid. On the 15th the other Irish bullock fell off his food, and he was slaughtered. In the course of the following month the other short-horns were attacked, and slaughtered. Mr. L., who, I might observe, was a firm non-infectionist, had placed in the adjoining yard a bull, a steer, and a heifer in calf; and after the death of the Irish beast the bull was tied up in his place. He was attacked next after the bullocks, and a short time after him the steer. They were both slaughtered. The heifer escaped, and has since calved. The stock buildings are all closely connected. Next, one of the cows, out of a dairy of thirteen, was attacked. This occurred on the 16th of June, and she died on the 20th. She was, during her illness, attended by the same cow-leech as Mr. G., of C—, in Case 4, and was treated on the same system. The cow to which my attention was now called, his man had bled twice. She was evidently fast sinking, and died on the following day. Mr. L. has since sold all the remaining cows.

"Observations: Norwich.—At this city the largest stockmarket in that part of the country is held on every Saturday; and at two seasons of the year, viz., autumn and spring, great numbers of cattle, particularly Irish, are brought to this market; in the former for the straw-yard, and in the latter for summer feeding in our marshy districts. They are brought here in large droves, and frequently several lots of cattle of different breeds belong to the same dealer. Although they are separated while in the market, they are frequently drifted and pastured altogether: hence cattle of every breed have become infected."

It has been proposed to treat this disease by medicinal agents. I am aware that the first impulse of mankind always is to find a remedy for every disease; but I am also aware that the older and wiser the medical profession grows, the less faith and confidence they have in specifics for disease. A fair-minded and honorable and acute scientific gentleman, one of the most so in America, stood here to-day and stated to you, in so many words,

that he did not know how far the profession of medicine could cure disease. Now we, who are moral and intellectual and accountable beings, may well live subject to such doubt as this. We have higher aims than animals, and we can afford to carry diseased bodies through this world, if we see fit. But not so with that portion of the agricultural interests of Massachusetts which is really the living wealth of every farm in Massachusetts. If gentlemen here come forward and say they have no remedy for the cancer eating at the vitals of Massachusetts, is it worth while to waste any more words about the question? I have never seen a remedial agent for it,—any thing which would economically, surely, and prudently stop the progress of the disease; and I never heard of any. And I would ask the farmers of Massachusetts whether it is worth while to spend sixty, seventy, or a hundred thousand dollars, in experiments which, for all the practical purposes of agriculture, cannot be worth one dollar. It seems to me, Sir, this is poor business, this attempt to cure the disease.

It is said no attempts have been made here, in Massachusetts, to cure the disease. And why? Simply and solely because the legislature which passed the Act of extirpation were wise enough to know that when it is possible to scotch a snake, it is best to do that, instead of trifling with him. They knew it was the best plan to root the disease out; and they appointed a Commission for that purpose. They knew that the best course was to eradicate it. It is said that it is a reproach to Massachusetts that there has been no attempt at the use of a medicinal cure. It is not a reproach to Massachusetts; it is a credit. What has been done in Europe? Does any body know what can be done in the way of treatment for this disease in cattle? Various remedies have been, but with so little success that the disease is pronounced, by Professors Morton and Simonds, to be incurable. The most approved practice in England, at the present day, is immediate extirpation.

Dr. Willems says, in a memorial to the Minister of the Interior of Belgium: "All curative measures, however curative they may be, are powerless in setting an obstacle to the evil; and in repairing the considerable losses which it occasions every day. The beasts which are *cured by treatment* fall away rapidly

and recover but slowly and with difficulty from the attack they have sustained."

The curative measure adopted in Holstein, Mecklenburg, Lubec, Hamburg, &c., where the disease prevails extensively, is immediate slaughter.

Each veterinarian in Europe seems to adopt his own peculiar practice. And there as here, every form of alkali, acid, salt, tonic and purgative, with external applications, has been recommended and tried in vain.

Blisters on the outside, and gin on the inside,—we have had them all recommended here; and, I believe, they have all been tried in vain. For I never saw a single animal which came through this process of external blistering with Spanish flies, and internal blistering with Holland gin, that has been pronounced cured.

Attempts have been made to stop the ravages of this disease. But how? They say we should stop it by inoculation. This operation of inoculation for pleuro-pneumonia is a curiosity. It is a very remarkable affair. You know, perfectly well, that inoculation was practiced for many years previous to the great discovery of Jenner, for the prevention of smallpox. But every physician knows that the mortality by inoculation for smallpox was so great that people were afraid to be inoculated; they were as willing to run their chance for the genuine disease as to have it put into their system by physicians! It was not until the discovery of the substitution, in the body, by legitimate laws, of one disease for another, according to the law that two diseases can hardly exist in the body at the same time, any more than two bodies can exist in the same space, that vaccination was introduced by Dr. Jenner, and succeeded. Now, let me tell you the difference,—let me tell you that there is no analogy between that vaccination, that scientific and valuable process introduced by Dr. Jenner, and this spurious process introduced by Dr. Willems for the prevention of the spread of this disease. Here is not another disease in the human system; but, according to all accounts, it is shown that the introduction of a decayed and rotten portion of the serum, from the lungs of a diseased animal, into the tail of a healthy animal, destroys the tail of the healthy animal.

This operation was introduced in the year 1850, by Dr. Willems, of Hasselt, Belgium, a district that has been peculiarly exposed to the disease. Since that time it has attracted the attention of all Europe, and various commissions have been established to investigate and inquire into the effects which it produces. Among others, the English government appointed Professor Simonds a commissioner to proceed to Hasselt and report upon the operation as practised there, and also to experiment on the matter in England. Extracts from Professor Simonds' report, will serve to show the best conclusions arrived at, amidst a mass of conflicting testimony.

"The commissioners here have spared no pains to arrive at the true value of the practice of inoculation, and their report, which extends over 176 pages 8vo., is full of most interesting and valuable details. In the majority of cases their experience fully coincides with our own, a fact to which we allude, in order to show the impartiality of their proceedings, and which we regret to see has been called in question!! It is unnecessary to select cases from their report, or to follow the commissioners through their scientific reasonings on the subject; and, therefore, we shall in this place content ourselves by giving the conclusions to which they have arrived.

"'From the preceding facts,' says the report, 'the commission concludes:—

"'That inoculation with the liquid extracted from a hepatized lung, the result of exudative pleuro-pneumonia, is not a certain preservative against the malady.

"'That the pneumonia succeeding inoculation may be produced several times in the same animal, which may or may not have been attacked with exudative pleuro-pneumonia.

"'That the two affections may exist together in the same individual and that considerable derangements are manifested in the inoculated part, whilst the morbid action of the lungs progresses towards a fatal termination.

"'As to the ascertaining whether inoculation really possesses a preservative power, and if so, in what proportion and for what length of time it imparts immunity to the animals subjected to it, these are questions which can only be solved by further experience.

"'Read and approved at a meeting of the commission.

"'Present—M. Verhezen, *President;* Bellefroid, Gluge, Theis, Deuterluigne, Sauveur, Thiernesse, *Members;* Fallot, Marinus, *Delegates from the Royal Academy of Medicine.*

"'BRUSSELS, February 6, 1853.'"

"On the 27th of November sixteen animals were selected for the operation; of these, twelve were inoculated on the under surface of the tail, near to its extremity, by *superficial* punctures, and four by *deep* punctures through the skin, after the manner of Dr. Willems. It is necessary, however, to add that these deep punctures were *cleanly made with a sharp lancet*, and not with a *bad-cutting 'double-edged' scalpel*, such as we saw *forcibly* thrust through the skin, and twisted about in the wound by Dr. Willems. This fact led to our remarking, in the former report, that 'surgical and scientific principles did not rule in these operations' on the Continent; and it is essential to allude to the circumstance again, because of the results which attended on these our first experiments.

"The material employed for the inoculation was the *serous fluid* pressed from a diseased lung, and of this two or three drops were placed in each wound. Care was taken to have this fluid as fresh as possible, and also that it should not come from a lung 'over diseased;' for which purpose we caused an animal to be killed in the early stage of pleuro-pneumonia, so that no untoward result might arise from a neglect of these precautions. We were assisted in these operations by Mr. H. Pyatt, veterinary surgeon, Nottingham, who is consulted by Mr. Paget in all cases requiring medical care, and who took a deep interest in these experiments. Mr. Pyatt also kindly undertook to watch the progress of events, and report to us as occasion seemed to require.

"It was decided to leave *fourteen* of the inoculated cows to mingle indiscriminately with the rest of the herd, but to remove *two* of them to an infirmary shed, into which diseased animals, as they were attacked, were taken, so as to expose them to the more direct influence of the contagion. This experiment was continued for several weeks, when it was discontinued, the animals during the time remaining unaffected.

"With *two* exceptions the inoculation *failed to produce the slightest effect;* and in these two animals it was not until the *fifteenth* day of the operation that the wounds inflamed. In consequence of this failure we determined to *re*-inoculate the cows, which was accordingly done on December 13th. *Twelve* only out of the fourteen were however operated upon, *two* being left to see if the previous inoculation would still take; Dr. Willems, in his Memoir, having stated that *a month* sometimes elapses before any local efforts are observed. No such phenomenon occurred in either of the cases, but, nevertheless, as one of these cows, after inoculation, was a little out of health for about a week, and both Mr. Paget and Mr. Pyatt thought this might possibly depend on the inoculation, it was determined not to repeat the operation upon her. This cow, up to the present time, June 1st, 1853, has continued well. This cannot but be considered as a decided instance of a *non*-inoculated

animal resisting for months, equally with those which were inoculated, the contagious influence of pleuro-pneumonia; for the continental authorities affirm, and in this we fully agree, that no constitutional effects can result from inoculation unless local morbid action is first produced. With regard to the other cow, she was subsequently *re*-inoculated, and lost her tail from the gangrenous inflammation which attended the operation.

"On one of the two *original* cases successfully inoculated, as it is ordinarily described, the inflammation was succeeded by ulceration of the parts adjacent to the puncture. It was feared that the animal's tail would be lost; such however did not prove to be the case. Further particulars, both with reference to this last-named cow, and also the *re*-inoculations, will best be learned by the following note received from Mr. Pyatt on December 17th:—

"'On Monday last, December 13, I went to Ruddington, and, in accordance with your directions, I *re*-inoculated twelve of the cows. Not the slightest effect was produced by the former operation, except in two cases. In one, No. 19, I found the tail swollen and very sore, with a scab about the size of a shilling covering the place of inoculation. I have seen this cow daily since Monday, and, although she appears to be perfectly well in health, the tail is now much more inflamed, and the wound looking so badly, that I fear in a few days the tail will slough. The *re*-inoculations were made from a *highly diseased lung*, and it seems to me they will all take, as the tails are now a little swollen and very sore when pressed.

(Signed) HENRY PYATT.'

"It will be seen from this letter that the fluid used for the *re*-inoculations was the product of a more advanced stage of pleuro-pneumonia; to this and also to the deep punctures made by Mr. Pyatt, the marked inflammation that speedily followed, or the success of the inoculation as it is designated, is to be attributed. On the same day that these twelve animals were *re*-inoculated two others were operated on, and on December 19th two more. These latter two were inoculated with *sero-purulent* fluid obtained from the inoculated places of other cows, being what is technically called '*a first remove.*' The animals bore the respective numbers of 10 and 21, these being the marks stamped upon their horns on purchase, and necessary to be made mention of for the purpose of identification.

"On the 23d of December we paid a second visit to Ruddington. The local effects of the operation, consisting of *ordinary inflammation, advancing with greater or less rapidity to suppuration*, were marked in all; but, comparatively speaking, they were slight in seven out of the

original cases. The two animals operated on the same day with the twelve, December 13th, presented a similar condition of the parts, as did also the *two inoculated by the first remove.*

"We selected *seven* of the most satisfactory cases from out of the fourteen inoculated *direct* with fluid from the lung, to give trial to *re*-inoculation. *On four of these the re-inoculation produced morbid action equally as great as the original inoculation;* on the others it failed. This fact, which is one of the first importance, we shall have again to allude to, and therefore we refrain from commenting upon it in this place. Between the 23d and the close of the month four more cows were inoculated by '*the first remove,*' and it was observed that more speedy action followed this method than that of *direct* inoculation with the exuded serum of the lung.

"During the month of December pleuro-pneumonia continued to show itself among the animals on the farm, and carried off no less than *seven* of them—six *non*-inoculated and one inoculated. The inoculated cow was, however, one of those which had been operated on by '*a first remove,*' on December 19th—No. 21. She was observed to be ill on the *fifth* day succeeding the operation, and an examination showed her to be the subject of pleuro-pneumonia. The disease advanced so rapidly that by the fourth day of her illness it was deemed prudent to have her destroyed. The autopsy confirmed the correctness of the diagnosis. Mr. Pyatt writes that the right lung weighed 30 lbs.

"Presuming inoculation does give security, this case must not be ranked among the exceptions or failures, for there cannot be a doubt that the animal was affected with pleuro-pneumonia, in its *incubative* stage, at the time she was inoculated. It should be noted, however, that the inoculation *took effect upon a diseased animal, and that its local action was in no way modified thereby;* facts totally at variance with the established laws of inoculation for diseases which are daily propagated in this manner. The question of inoculation proving abortive as a means of protection, because it was one of 'a first remove,' cannot be raised in this particular case, as it has been in others, from the facts connected with the time of the animal's illness. With regard to the point of non-protection from this manner of inoculation, we may remark that No. 10, operated upon the same day with No. 21, and likewise the four cows previously made mention of as being *also* inoculated by 'a first remove,' have now been several months on the premises without giving any evidence of disease. In this particular they agree with those inoculated directly from the lung; hence we may infer, that, if one is protective, so is the other. This point, however, will present itself for our examination again in the sequel of this report."

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"It was acknowledged, even in Hasselt, that they had had as little disease in some summers, prior to the employment of inoculation, as during the last when the system had reached its climax. In proof that inoculation was not the sole cause of this freedom, is the fact that the cattle of the distillers, who objected to have the operation performed, continued as healthy as those of others who did not so object. What we contend for is, that, as there are no specific *local* effects produced by inoculation, so *protection* does not depend on the special action of a special virus on the organism, as is the case with the vaccine and other similar diseases."

* * * * * *. * *

"1. That inoculations made by superficial punctures and simple erasions of the skin, invariably fail to produce any local inflammatory action, being the reverse of the case with regard to the vaccine disease, smallpox, and other specific affections, of which it is an indication of success.

"2. That the employment of *fresh* serous fluid, and a cleanly made, but *small* incision, during the continuance of a low temperature, will also almost always fail to produce even the slightest amount of inflammation.

"3. That deep punctures are followed by the ordinary phenomena *only* of such wounds, when containing some slightly irritating agent.

"4. That with a high temperature, roughly made incisions, and serous fluid a few days old, local ulceration and gangrene, producing occasionally the death of the patient, will follow inoculation.

"5. That the *sero-purulent matter*, taken from an inoculated sore, causes more speedy action than the *serum* obtained from a diseased lung, and that "*removes*" cannot be effected on scientific principles.

"6. That oxen are not only susceptible to the action of *a second*, but of *repeated inoculations* with the *serous exudation* of a diseased lung.

"7. That an animal inoculated with the serous exudation is *in no way protected even from the repeated action* of the sero-purulent fluid which is produced in the wound as a result of the operation.

"8. That animals not naturally the subjects of pleuro-pneumonia, such as donkeys, dogs, &c., are susceptible to the local action both of the serous exudation from the lung and the sero-purulent matter obtained from the inoculated wounds.

"9. That the serous fluid exuded from the lungs is not a specific "virus," or "lymph," as it is sometimes designated.

"10. That inoculations made with medicinal irritating agents will be followed by similar phenomena to those observed in inoculations with the exuded serum.

"11. That inoculation often acts as a simple issue, and that the security which at times the operation apparently affords, depends in part upon

this, but principally on the unknown causes which regulate the outbreak, spread, and cessation of epidemic diseases.

"12. That inoculation of cattle, as advocated and practised by Dr. Willems and others, is not founded on any known basis of science or ascertained law, with regard to the propagation of those diseases commonly called specific.

"13. That pleuro-pneumonia occurs at various periods of time, after a so-called successful inoculation.

"14. And lastly, that the severity of pleuro-pneumonia is in no way mitigated by previous inoculation, the disease proving equally rapid in its progress and fatal in its consequences, in an inoculated, as in an *un*-inoculated animal."

These, gentlemen, are facts and opinions with regard to inoculation, with regard to that scientific process which has been recommended here, as the means of curing,—of putting an end to this disease.

Dr. Reviglio, of Turin, pronounces against inoculation after repeated experiments of his own, and after examining many reports of commissioners appointed by the governments of Austria, Prussia, Holland, Belgium, France, England, and the Lombardo-Venetian Kingdom.

Even in Belgium where inoculation was first introduced, a commission recently established, has reported against its efficacy.

This is the history of the disease in Europe; this is its history here. You have the best opinions in Europe, upon inoculation. Now let us come to the practical part of the matter, to that part which concerns us here. I propose to lay before you, to a certain extent, the legislation which has been had upon the subject. Many laws have been passed and measures adopted for the prevention of the spread of the disease. In England the Parliament passed an act in 1744, under similar circumstances, providing—

1st, for the killing of all the infected animals, and burying them entire with the skins on, "slashed from head to tail," that they might not be used for the purposes of the manufacturer.

2d, for the burning of all the hay and straw used about the animals.

3d, for the cleaning and fumigating of the sheds, etc., and for no sound cattle to be put in them for two months after the removal of the diseased.

4th, for no recovered animal to be allowed to go near others for a month after its convalescence.

5th, for no diseased cattle to be driven to fairs or markets, nor for the flesh to be used for dogs, etc.

6th, for no healthy cattle to be removed from a farm where the disease had prevailed in less than a month after its disappearance.

And, lastly, orders were given for the notice of an outbreak to be immediately sent by the farmers to the proper authorities.

This does not apply to pleuro-pneumonia, but to the contagious diseases existing at the time, so long ago as in 1744.

And, in 1857, a "select committee on sheep and contagious diseases prevention bill," appointed by the British Parliament, inquired of Prof. Simonds his views on legislation on the subject, and he sustained vigorous measures.

It has been stated here, that there is a great deficiency in the laws of England in regard to contagious diseases; and a gentleman stated, this morning, as an evidence that the disease was not contagious, that the Parliament of England have not passed any law to prevent its spread. Whether he considered it a scientific, a medical, a legal, or a theological opinion, I did not ask him. Now, I wish distinctly to state, that there is a general law, passed in 1838, with extremely stringent provisions, precisely what has been passed in Massachusetts, but more in detail, and covering the whole ground of the spread of disease there. But Parliament has considered this sufficient to cover this whole ground; and while veterinary surgeons have asked for a specific law for the prevention of the spread of this disease, the members of Parliament have said, The general law is all-sufficient; and if it is applied in an efficient way, you can stop the progress of this disease among you. That is the reason why there is no law, in England, passed for the prevention of the spread of pleuro-pneumonia—not because it is not a contagious disease, for that had nothing to do with it, there, at all, and never had any thing to do with it.

In France, the Minister of Agriculture has recently issued an order, that no proprietor of cattle shall henceforth be indemnified for the loss of his animals from epidemic affections, unless he produces a certificate signed by the Prefect of the commune, that they were duly attended by an authorized veterinary surgeon.

In Denmark, a commission was appointed in 1845, to investigate the nature and consequences of pleuro-pneumonia. Professor Witt, joined with the department veterinary surgeon, and a surgeon and veterinary surgeon of the adjoining town and territory of Hamburg, formed a commission of inquiry. This commission ended its labors by recommending complete sequestration of the places where the disease existed, the immediate slaughter of all infected animals, and the ultimate killing of the whole herd upon its being found that fresh cases occurred. The diseased animals were to be buried with their skins on, but these cut in such a manner as to prevent their being surreptitiously disposed of, and their bodies were to be sprinkled over with chlorinated lime. The indemnity to the proprietor was to consist of the government paying two-thirds of the value of the diseased animals, and the full value of the healthy ones. Various other recommendations were made to secure the carrying into practice these extreme measures. Thus a proprietor was to be fined for not giving notice of his cattle being affected; and he was also not to be allowed to sell any animals off his farm until the department veterinary surgeon saw fit to give him a certificate of their being in a state of health.

These measures were at once adopted and are now in force. The effect is said to be that pleuro-pneumonia has more than once totally disappeared, and its subsequent appearance is attributed to fresh introduction from abroad. And its last appearance in Denmark, in 1843, is attributed to the purchase of one hundred and eighty oxen brought from Hungary, and suffered to graze on the islands and marsh lands of the Elbe.

Serious complaints have been made in England, that a Bill which has been reported has not passed Parliament.

The *Veterinarian* of April, 1860, contains a very able article on this subject, and calls for immediate legislative action, stating: "On all sides we are asked what can be done to cure the disease? and gloomily are we looked upon when we reply,

that, as a rule, it is incurable, but that it can frequently be prevented, and would be so, were sanitary measures adopted by the legislature, to limit the contagion."

So much for the history of the disease here, its course in Europe, the tone of legislation there, where it has existed in enlightened and not panic-stricken nations. Now, one word in regard to the action of the Commissioners here.

I have heard the terms "waste," "indiscriminate and wholesale slaughter," used here, as if this Commission, appointed by the legislature at its last session, had gone about the Commonwealth, thirsting for the blood of animals, whether sick or well. Now, gentlemen, it is not so. The records of the Commission, which I laid upon the table this morning, will show you that in all cases they have exercised their best judgment, not only in the killing of animals, but, really, that they have gone beyond the law, and taken upon themselves to sequestrate and isolate, wherever it seemed to be for the pecuniary benefit of the people of Massachusetts. They have endeavored, wherever possible, to consider the pecuniary interests of the Commonwealth, and the wants and interests of the agricultural region in which this disease existed and now exists. They have felt that this double duty devolved upon them, and they have endeavered in every way in their power, calmly, dispassionately, and judiciously to carry out the intentions of the legislature. Why, gentlemen, it seems as if the panic was all on the other side. It seems as if gentlemen were more afraid of the Commissioners than of the disease; as if the panic were not in regard to the pleuro-pneumonia, but in regard to the Commission of extirpation. When I read the Remonstrance which has been presented to the legislature, I was astonished that reasonable men, with the knowledge before them, easily to be obtained if they would but read, should come forward here and lay before an intelligent, enlightened, and not panic-stricken community, such assertions. On what grounds do they make them? They say:—

"Because it has not been proved that said disease is either contagious or infectious."

What will prove it? Will any thing prove it? Would these men believe it if they themselves had an attack of the pleuro-pneumonia, contagious and exudative? They also say:—

"Because the legislation authorizing the killing of the cattle is a departure from the legitimate province of legislation, all experience agreeing to show, that the remedy of an evil like this is more economically and more surely secured when left to intelligent individual interests than by govermental interference."

Just after this, they say:—

"Because the legislature has no right to authorize the destruction of private property, except as a public or common nuisance; and for these contingencies, existing laws, deliberately passed, and carefully guarding personal rights, adequately provide, and because our Bill of Rights guaranties that the property of any individual shall be appropriated to public uses only when the public exigencies require it, and then he shall receive reasonable compensation therefor."

Why,—is not that the foundation of the law, as it stands upon the statute-book? Is not pleuro-pneumonia a nuisance? Is it not a nuisance? Can the true state of the matter be defined in any way better than this,—that there is a nuisance, and that according to the Bill of Rights, and under the Constitution, and by the statute-book of Massachusetts, the legislature has a perfect right to pass such a law as this, for the removal of a nuisance? And when it is said that such acts as are provided for in this law shall not be done except by the State paying proper compensation therefor, I would ask if any man has raised a word against compensation. The cry has been upon the other side. It is said the farmers are too well paid,—that the Commissioners have taken care that the farmers shall get too much money. Is there any consistency in this, any science, any sound principle of legislation, any constitutional law? It seems to me that common sense, prudence, discretion, every thing which would teach men to be obedient to the laws, every thing which would teach them their own self-interest in ridding the community of such an evil, would plead against such a remonstrance.

Why, gentlemen, the Commissioners have been acting under the direction of the legislature, and with a view to the benefit of the people of the Commonwealth. They were appointed to carry out that provision of the Bill of Rights which says that private property shall not be taken except to abate a nuisance,

and that the owner shall then receive reasonable compensation therefor. And compensation, therefore, has been granted: and so entirely satisfied were the sufferers from this nuisance, of the intention and designs of the legislature which passed that Act, that they have come forward and signed an agreement to trust to your magnanimity to pay the bills. They have been entirely satisfied that they were laboring under an utterly intolerable nuisance, and, moreover, that the legislature of Massachusetts would help them out of this nuisance and sustain them in the abatement of it. I say they have come forward, not as matter of compromise; but every one whose cattle have been destroyed has said: "Gentlemen, we will abide by your judgment in regard to those you consider diseased: we are perfectly willing to lose them; and we will wait for our compensation until the legislature of Massachusetts passes an additional appropriation for that purpose." That is the prevailing feeling wherever the disease has existence; that is the prevailing feeling there where the disease was planted so long ago that it would have literally extirpated the cattle, itself, Commissioners or no Commissioners, by this time—they having simply hastened the work. Had they been stopped in their work, as they were threatened to be stopped, at one time, by the want of funds; had the farmers been unwilling to come forward freely, fairly, intelligently, and honestly, to sustain them in their course, nobody would ever have talked about the Commission extirpating—pleuro-pneumonia would have done its own work there.

Now, gentlemen, I have nothing to say with regard to all the investigations that can be made, of this matter. I would be glad to have it investigated. I have just as much respect for science as any man can have. It was the study of all the early part of my life. And when these gentlemen came upon the stand, here, and testified with regard to the scientific demands in this case, I saw my old teachers standing before me. I respect them for their untiring industry; I respect them for the efforts they are continually making to stop the progress of disease in this community; I respect them for the success they have met with in all the sanitary efforts they have made. I cannot help admiring them for the advantages they have furnished us, in modern times, in all matters of ventilation, of diet;

in all those things which go to arm the human race against disease. I respect them, too, for their ceaseless industry in searching out the fountains of disease. But they will tell you, every one of them, that when they have learned what marks disease makes upon the human body, and what pathological phenomena appear in a diseased organ, no human being has ever, beyond that been able to proceed,—that that is the boundary line, the *ultima thule*, past which no science has ever gone. And one of the most interesting and fascinating medical books I have ever read was written by a scientific gentleman of this city, a member of your Board of Agriculture, a gentleman of skill and intelligence, who comes forward to say, that all the scientific observations of disease can never enable us to ascertain, by these pathological facts, any remedy for the sickness,—that you may dissect, till the trump of doom has sounded, livers that are affected by the fever and ague, and that nothing that is discovered by such dissection will ever indicate the use of quinine as the remedial agent. While I regard science as one of the highest achievements of the human mind; while I admire my old teachers and the great extent of their attainments, I still say that before this malady they are as powerless as they have always been before the ravages of all contagious diseases in the human family. They have no power to stay it. It will "have its run," as Dr. Jackson said here to-day; and I am happy to state that his scientific testimony went to sustain the legislature of Massachusetts, and the Commission, in that act of extirpation which has been attempted here, and that he himself said that were it within the range of possibility,—about which he did not profess to decide,—he would not have science stop the operation, for one single instant. Now, gentlemen, shall we, *shall we*, in all this weakness of scientific investigation, in the face and eyes of this testimony which we have before us, shall we,—when we know that the disease if let alone, will ravage our Commonwealth,—shall we stop to establish hospitals, and all sorts of operations, for the benefit of scientific investigation? If any thing can be done, if any new laws can be ascertained, if any new light can be had upon the matter, in Heaven's name let us have them, and let us go to work and get rid of the disease. That is the object and intention of the Act of the last legislature; and while, as the

gentleman who preceded me said, I have nothing to recommend to the Committee, leaving it entirely to their wisdom to suggest the laws which shall control the operation, I do say that when the State of Massachusetts is well read in this disease, whether it be this month or next month, this year or next year, the legislature that enables the farmers to get rid of it will be considered to have conferred a greater benefit upon the Commonwealth of Massachusetts, than almost any legislature that has preceded or can come after it. It is for the agricultural interests, for the farmers, that we are at work here. And for the first time in my life, let me tell you, farmer as I am, I have learned what it is to work. I thought it was a pretty piece of business to contend against the thorns and thistles that have sprung up against mankind; I thought it was charming to engage in the occupations of husbandry, but when I was informed by the governor, that he had put me upon this Commission, with a prospect of hard work and small pay, I did not understand the Herculean task which was placed before me. And I call upon the legislature of Massachusetts to sustain myself and my brother Commissioners in the work which we have undertaken.

Mr. Walker.—I would here take the liberty to remark in reference to the supposition of my colleague, Dr. Loring, that I differed from the remainder of the Commission, in regard to the satisfactory character of our knowledge of the disease, I made no such remark as was attributed to me. I never said that the ignorance of the laws of the disease prevented our going forward. I merely said:—

"It is the want of knowledge of the laws of the disease that is the great obstacle to our operations; and it is the most alarming fact in regard to the disease, that it doesn't seem to be understood at all in this country, or even in Europe, where they have had it for two hundred years."

That was the remark I did make; and my colleague had not seen it, or he would not have alluded to it as he did.

The Committee then proceeded to consultation.