general recognition of principles so peremptory as the above may at first appear to them; but, if so thinking, they would perhaps have underestimated the rapidity with which knowledge is now increasing as to the common interests and mutual duties of mankind in respect of dangerous infectious disease. Fourteen years ago, when the so-called cattle-plague or steppe-murrain was imported afresh, as a long-forgotten disease, into this country, and was found to affect very large pecuniary interests, primarily of the chief landowners of the United Kingdom, but secondarily of other classes, an immensely valuable stimulus was given to the education of the country, and especially of its legislature, in regard to the preventability of the infectious diseases. And the remarkable zeal and ability which have been shown in providing adequate laws and admirable administrative arrangements against the diffusion of steppe-murrain and other infectious diseases of Farm-Stock are not likely to be found permanently absent in relation to the interests of Human Life when once the true bearings of the subject shall have got to be popularly understood.

[Since the writing of the above, a year and a half ago, there seems to have been made, in one of the directions referred to, an important and most interesting addition to positive knowledge. In two recent numbers (July and October 15th) of the *Archiv für experimentelle Pathologie*, Professors Klebs and Tommasi-Crudeli report the particulars of a research recently conducted by them at Rome with regard to the essential cause of the ague which is endemic about that city; and, so far as can be judged from that account, the doctrine that ague-poison is a microphyte of malarious soil is no longer a mere matter of suspicion (as my text expresses it) but is a matter of experimental certainty. The investigators, namely, declare that they can isolate from malarious soils and their atmosphere definite microphytic forms capable of separate cultivation; and that, when successive generations of this "bacillus malarie" (as they name it) have been cultivated in successive quantities of an indifferent fluid, subcutaneous inoculation of rabbits with any final fluid in which the *bacillus* is germinating will give ague to the subjects of the experiment. The argument of my text (in the section relating to the natural history of contagia) may suggest how extremely interesting it would be to ascertain whether, when animals are under influence of the *bacillus malarie*, the fever can be propagated from them to healthy animals by means of small transfusions of blood. —November 1879. J. S.]

ABERGAVERNYY.—The number of deaths registered in Abergavenny last year is the lowest on record. The death-rate is in the proportion of 17 per 1,000 of the estimated population, and its lowness is chiefly attributable to the almost entire immunity from zymotic diseases. Only eight deaths from this class occurred, and even among these there are two or three whose place in the class is questionable. Mr. Steel reports a still increasing improvement in the sanitary state of the town, especially in the habits of people dwelling in the closest and worst built courts. There still remains a good deal to do, and the questions of sewage disposal and trade nuisances seem to deserve the earnest attention of the local authority.

REIGATE (URBAN AND RURAL).—The borough of Reigate, as has already been shown in Dr. Thorne Thorne's report on the recent epidemic of typhoid fever at Caterham, is usually remarkably healthy. The rate of mortality during the five years of Mr. Jacob's appointment has been lower in this district than in any other portion of the combination. Last year, it was 14.3 per 1,000, or slightly (.2) above the average of the last five years. The deaths (16) from zymotic diseases were very much fewer than in any of the four preceding years, and were at the low rate of 9 per 1,000. Three cases of small-pox are recorded; four cases of scarlet fever in Reigate and a small outbreak at the Earlswood Asylum; attacks of diphtheria in two houses; two cases of enteric fever; and a somewhat extended prevalence of whooping-cough. In all these cases, the proper remedial measures were taken. Some of the polluted wells that still remain in the district have been closed, and a supply taken from the water companies. Sewerage and house-drainage are also progressing satisfactorily. The rural district of Reigate is also a very healthy one, the death-rate being 15.3 per 1,000 of the estimated population. The deaths from zymotic diseases were 10 per cent. of the deaths from all causes, and at the rate of 1.6 per 1,000. Small-pox, measles, and whooping-cough were not prevalent in any part of the district. Thirty-nine cases of scarlatina, 28 of diphtheria, and 10 of enteric fever were reported. As regards diphtheria, Mr. Jacob could, in many instances, find no connection between the cases, though one outbreak (at Horley) was clearly traceable to the school. Schemes for the sewage disposal of Merstham and Horley are now in a forward state, and a code of by-laws has probably by this time received the sanction of the Local Government Board.

REMARKS ON ANIMAL VACCINATION.

A Paper read at the Conference of the Parliamentary Bills Committee of the British Medical Association, on Animal Vaccination, December 4th, 1879.*

By E. WARLOMONT, M.D.,
Member of the Royal Academy of Medicine of Belgium; Director of the State Vaccine Institute at Brussels, etc.

It is not, however, sufficient that the lymph from the calf be harmless, it must be inoculable and preservative against small-pox. It must be inoculable. But how can it be otherwise? What was it, in fact, but animal lymph which caused Jenner to make his immortal discovery? When Jenner had the inspiration which has made his name revered, what had he before his eyes but a person who, having had the abraded epidermis brought into contact with an animal-vaccine pustule, had been inoculated with the matter of that pustule? This person had thus unwittingly practised upon himself animal vaccination. Animal vaccination is, then, the progenitor of all other vaccination, present, past, and future, and if it had not been efficacious, vaccination would have been unknown. Of what use is it to push demonstration further? Animal vaccination is—and we profit by this opportunity to define it—nothing else than the maintenance of original vaccinia (cow-pox) upon its native soil, where it is cultivated by successive inoculations, without ever having recourse to other sources. It is not, as our opponents are pleased to say, thinking thus to overthrow our proceedings, lymph taken from children, and replanted on calves (a method known as "retrovaccination"), but lymph which has never received any other aliment than that of its native soil. How then could it lose its qualities, and what analogy could be pleaded for the belief that it was necessary that such virus, to live and prosper, should be cultivated upon strange soil?

Perhaps I shall be asked to adduce, in support of my statements, comparative statistics. I should regret that such a request were addressed to me, for I should feel bound to take no notice whatever of it. I would never consent to furnish as proof such an element of appreciation, because I should reject it as coming from my opponents. Comparative statistics are only valuable when all the conditions of a proper comparison can be brought together. It would be necessary, to arrive at any reliable result in this respect, to have identity of vaccinated subjects, identity of condition in the lymph employed (living or preserved, in the dry or liquid form), identity of the age of the lymph; identity, moreover, of the capacity and judgment of the experimenters. I shall wound no one, I think, by remarking how very important it would be, in such a question, for statisticians to put on one side their predilections, and also what difficulty there would be in getting identity of conditions, all of which are indispensable. Comparative statistics, even if they were made by Aristides, would only satisfy himself.

It would not be quite the same with statistics giving absolute results, since they cannot be left to the caprice of opinion. The subjoined appears to me to unite the good qualities of absolute exactitude and truth. In 1870 and 1871, thirty-six of the most trustworthy physicians of Belgium acquainted me with the results obtained by them in vaccinations and revaccinations with animal lymph on points sent out by the State Vaccine Institute at Brussels. These are the results: 1. In vaccinations, out of a total of 500 cases, 479 successes, or 96 per cent.; 2. In revaccinations, out of a total of 5,425 cases, 3,419 successes, or 62 per cent., a result superior to those shewn in all statistics. In England, the best vaccinators only calculate upon 90 per cent. of success in vaccinations performed with human lymph in tubes, and 95 per cent. with the same lymph on ivory points.

I shall here finish what I have to say about statistics, without wishing on that account to cast any doubt or disparagement on the comparative figures adduced by men whose character I respect too much to think of doubting their sincerity.

But it is not sufficient to adduce figures: it is necessary to know at what time and under what conditions they have been brought together.

* Concluded from page 928 of last number.

We should wish to say a few words on this subject. Innovations, new methods, are not introduced without much groping in the dark and the committal of mistakes. Animal vaccination has not escaped these gropings or these faults. At the commencement, no doubt, it was not modest enough in its pretensions. From this have arisen attacks, which its adversaries have perhaps pushed too far; and from it especially have arisen the prejudices which are not yet extinct, and which justified to a certain degree the hesitation of early days. Thus, in certain circumstances, in certain places, the use of matter taken from vesicles the value of which had not yet been ascertained by experience, whether with regard to development or age; the employment of lymph too old or too young, or that of lymph taken from animals which did not fulfil the desired conditions—all this has given rise to disappointments, which have been used, not without some reason, but perhaps with too much eagerness, against the method.

As a matter of fact, animal vaccination has, in our minds, during the fourteen years that we have followed this tedious business, not without having removed all the objections to it, made, in our hands, daily progress, both with regard to the cultivation of the lymph, its results, and the preservation of the product. After having tried to preserve it in a liquid state, we were obliged to abandon this method, because the lymph coagulated in the tubes and could not be got out. We then adopted the method of keeping the animal lymph on ivory points, drying it rapidly, for delivery in this form to vaccinators. Nothing is more practical for expeditiousness and preservation. But it was necessary, in order to get from this lymph all the desired effect, to introduce the matter thus dried into sufficiently wide openings in the skin. On this account, the so-called English mode of scarifications has since been largely adopted.

This method, nevertheless, has itself had to encounter adversaries; it has been reproached, and will continue to be reproached, with causing exaggerated traumatism. Here, again, we have supplied the corrective: a "vaccinateur trephine", with which even the heaviest hand or the most presbyopic eyes can make a circular incision of a depth sufficiently small to protect from accident. Finally, we have borrowed from M. Chambon, who has been kind enough to communicate it to us, a most simple means of defibrinating the animal lymph before its introduction into the capillary tubes, and of thus preserving all its limpidity and its fluidity. For several years, we have thus furnished, to whomever has asked for it, liquid or dry lymph, thus allowing practitioners to choose for its insertion whichever method—scarifications or punctures—may suit them best. All these improvements should, it would seem, have annihilated an opposition which, having at first some measure of foundation, has now no *raison d'être*. It is, however, by no means so, as you know.

As to the vaccination of the calves, several years passed without my arriving at anything which was perfectly satisfactory to me. I vaccinated—and things happened at Brussels as they happened at Paris and Milan—an animal on a given day; then I vaccinated children from this animal on the fifth, sixth, seventh, or eighth day, according to the temperature, to the habits, and a little according to the convenience of the inhabitants. From the beginning, I have adopted the following course. I inoculated a calf on the Friday, and, on the Tuesday or Wednesday following, the children came to be vaccinated. Full of confidence in the results which I had at first obtained with good calves, I did not give sufficient attention to the variations which occur in eruptions; and it has more than once happened to me that, when the public had arrived at the station, I vaccinated without being sufficiently careful as to the choice of vesicles, of which, as further observation has taught me, some may be fit and others inert, or nearly so, in the same animal. On some children, the vesiculation was admirable; in others, it miscarried. The great fault of this arrangement was that, having caused often a large number of children to assemble for vaccination, and finding ourselves before an animal which did not supply us with all that we wanted, we performed the vaccinations nevertheless, and with lymph which we ought to have rejected. This practice has been the cause of a great depreciation of animal lymph—not at Brussels, because I soon stopped myself, pursuing my observations and correcting my practice, but elsewhere. At Paris, during the year that preceded the siege, and during the siege itself, grave faults were committed: often a single calf served for the vaccination of more subjects than it ought; and I think the conscience of those who proceeded in this way cannot be quite easy. When it was a question of vaccination, the evil was not so great; if they did not succeed, they began again. But it is quite different in re-vaccination. Then it is really a matter of conscience, and it is necessary to have lymph which can be thoroughly relied upon. What has passed at Paris continues, I must say, to weigh cruelly on animal vaccination in France, where it has not yet succeeded in resuscitating itself, notwithstanding honest and conscientious efforts.

For ten years, I have proceeded on a plan which completely protects me from such mishaps. I always vaccinate two calves at once. They progress in a parallel course; if one of them give doubtful vesicles, the other gives good ones, which enables me to be always perfectly sure of my vaccinations. For twelve years, I have vaccinated each year from eight to twelve hundred children. Every one will corroborate my statement that, when I vaccinate fifty children in half an hour with living animal lymph by simple puncture, taking the lymph from selected punctures, of which I am perfectly sure, not one of these punctures fails. Thus, when I see the criticisms that are made, as much abroad as in this country—when I read parallel facts as to the two vaccinations, in which the parallel is always unfavourable to animal lymph—I shall be allowed, it is indeed my duty, to combat the objections made to the latter.

These objections are ranged under three heads: 1. The transmission of animal lymph, even from the calf to the arm direct, is less certain in its results than that of lymph taken from arm to arm; 2. Animal lymph does not keep so well as humanised lymph; 3. The transmission of lymph from calf to calf is liable to frequent interruptions, even in the hands of the best operators.

1. *The transmission even from calf to arm is uncertain.* This assertion is the result of an inquiry made ten years ago, when the method was yet in its infancy, and when experience had not yet laid down its principles. In 1869, it was not known, as it is now, that the vaccine vesicle gives a product which is more active in proportion as it is younger. In more than one institute, lymph was then still used which had been taken from vesicles seven or even eight days old. Now it is no longer the same; it is in the course of the fifth day from the inoculation that the lymph is taken, whether for preservation or for immediate vaccination; and since then the results have completely changed. As I said a moment ago, no puncture fails when all the conditions are observed. I shall refrain from bringing forward, in support of this, copious statistics; I prefer to call to witness all those who have watched my operations—Mr. Ernest Hart, for example, who has seen my results—I should prefer, in short, to confine myself to a simple affirmation, resting on my honour. This result is now so certain that the vaccinators of the Brussels district send to me, each week, children to convert into vaccinifers. There has not been a single instance of these children being sent back on account of failure. And here I would make an observation that must not be omitted. In the same way as animal lymph, that derived from the arms of children is more active the younger it is; but in the child it is difficult to have recourse to this early taking of lymph without occasioning pain, and perhaps unpleasant traumatism. Moreover, how much lymph can be got from a vesicle on a child before the end of the seventh day? It is with difficulty that one gets at the lymph, and however little pressure or probing is used, blood comes. If, then, it were shown that lymph is only really active when taken five or six days after inoculation—and opinion takes this direction—it would soon run short, not only because the crop would not be abundant enough, but because the greater proportion of mothers would refuse to allow the hardly developed vesicles to be touched. Neither of these difficulties hampers animal lymph, for the pincers express at the fifth day innumerable drops of the most limpid and active lymph from the vaccine vesicles on the animal.

2. *Animal lymph does not keep so well as human lymph.* This assertion, like the preceding, belongs to the legend of the past. From the first, it was never true as regards dry lymph on ivory points. During the twelve years that we have made use of it in this form, it has shown a longevity comparable to that of human lymph in whatever form it was kept, and it is to it that the Belgian physicians owe the success of which we have spoken above. It is to it also that many colleagues living in the most distant countries—India and America—have had recourse many times, with success, for renewing their stock from ours. But it has not been always the same with regard to lymph preserved in tubes. At the commencement of our work, our failure was such that we had one day to utter this cry of distress: "If we do not succeed in preserving liquid animal lymph better than before, tubes will be the ruin of the new method." And indeed the matter, taken directly and without any preparation from the pustule, rapidly coagulates in the tubes and cannot be expelled therefrom: to such a degree in fact, that for several years we only sent out points, failing constantly with lymph in tubes. It is different since, by a most simple method, we have introduced the animal lymph into the tubes only after it has been defibrinated. It is thus kept in a perfect state of limpidity, and maintains its activity so well (the tubes are closed without the use of heat) that we have been able lately to inoculate a calf without any failure with tubes which had been filled in this way several months before. Let us, then, hear no more of this old stereotyped argument, which, true ten years ago, is now obsolete and no longer deserves any credit. Animal lymph defibrinated and inserted with the needful care into tubes closed without

heat, by paraffin or an artificial capsule, keeps as long and as well as humanised lymph.

3. The third objection remains: *the transmission of lymph from calf to calf is liable to frequent interruptions, even in the hands of the best operators.* In order to reply to this, I would ask permission to reproduce the following passage from a report which I addressed some months ago to the Minister of the Interior at Brussels, on the administration of the State Vaccine Institute, the direction of which is confided to me. The question which was put to me by the Government was this: "The aim of the State Vaccine Institute has been principally to promote the production and gratuitous distribution throughout the whole country of vaccine lymph. To what extent has this aim been attained?" I quote from my reply: "To answer this question, it will be sufficient to inform you, as to the first point, that, since the establishment was created [its formation dates from the 11th July 1868] it has continuously produced, without a day of intermission, all the lymph necessary for the requirements, however extensive they might be, of vaccination and revaccination throughout the country. The production of lymph has not then been promoted merely, it has been realised without interruption and without limits. As to the second point, there is not a doctor in Belgium who, at whatever moment he might require it, has not been gratuitously put in possession of lymph asked for by him for the renewal of his stock. In what measure have practitioners profited by this permission? Their demands have increased from year to year; so much so that, of nearly 2,000 medical men practising in Belgium, all of whom do not vaccinate, rather more than 1,200 have, during the year 1878, had recourse either once or more to our establishment with a view to the renewal of their stock. The regularity and promptitude of dispatch have left nothing to be desired. With very rare exceptions, the lymph asked for by letter has been sent by return of post, and practitioners have been thus put in possession, within twenty-four hours, even in most remote parts of the country, of the animal lymph asked for by them. Many times, under urgent circumstances, doctors have made demands by telegraph. Never have these demands remained an hour uncompleted with. The service to the public has been performed with the same rapidity and punctuality. Once, during the serious small-pox epidemic of 1870-1, which brought requisitions without number—as many as five hundred a day for a considerable period—the same punctuality and promptitude characterised the despatch. I may be permitted here to say incidentally, as an example of the immense service rendered to the country by the State Vaccine Institute, that revaccination, that prophylactic measure of so great an importance as to be equalled by no other in the vast field of public or private hygiene, is one of its best results; and that this fact dates from the day when the service was created. Up to that time, revaccination had only been recommended, or at least demanded exceptionally. To escape a danger that they preferred to doubt, people refused to submit themselves to an operation of which they were pleased, on the other hand, to exaggerate the peril. Now, the repugnance to the insertion of human lymph has given place, conquered by animal lymph, to favour. In Belgium, revaccination has now taken its place in the customs of the intelligent part of the community, who are henceforth quite ready to obey the word of command that we have given to the public, and which we have formulated as follows:—'Revaccination is a safe rampart against small-pox, if the resistance it ensures be proportionate to the violence of the attack'. Now, a recent successful revaccination gives this impregnable resistance. It is necessary, then, to be revaccinated every time that there is an epidemic of small-pox in the neighbourhood, and the more scrupulously in proportion as the epidemic is intense. For all those who follow this prescription, and the number of them is now great, small-pox,—that most terrible of all maladies—has no danger. The security given to families ought to be carried to the credit of the Institute as an inestimable benefit."

I shall here conclude this quotation, which I have used as a preface to my reply to the objection under discussion; but, before making this reply, I shall be allowed, I think, to remark that England, in systematically rejecting animal vaccination, as it has officially done up to the present, has thus neglected to deprive revaccination of the principal obstacle which has been obstinately thrown in the path of its general acceptance, and that it has by this neglect committed a fault for which public opinion has, perhaps, the right to reproach it. However this may be, the objection founded on the difficulties of execution is disposed of by the facts already stated. If he who speaks to you has been able during more than ten years to preserve his animal vaccination service from all interruption, the pursuit of a similar object and the execution of a similar programme cannot be beyond human power. What has been done in Belgium can be done in England. But will the organisation and the direction of such a service be an easy task? Very far

from the truth would he be who had such a notion. One can, indeed, hardly realise all the zeal, the care, the incessant supervision exacted by such an enterprise, and in default of which all the wheels of the machine will stop at once.

It is especially necessary that care be taken to inoculate new animals at the precise moment when the vesicles of the vaccifer are in all their activity, in default of which the risk is incurred of only having an unsatisfactory vesiculation, or even a totally abortive one. It is necessary to watch with a constant solicitude the health of the animal in use, the loss of which when the stock has but one branch would stop the whole of the working. It is necessary, in short, to collect the lymph oneself, so as to take it from good vesicles and to avoid the irregularities and the faults of inexperienced or unscrupulous assistants. My experience has shown me the importance of care even in the very smallest trifles.

During the year of my novitiate, only one calf was, as a rule, brought each week to the establishment and inoculated. But it happened more than once that the single animal, on which rested all the hope of the work, gave defective or insufficient results; perhaps because it was in poor or bad health, or from some other cause. From this arose great annoyance and great mischief, against which it was necessary to be constantly on the watch. Since 1869, the Direction has taken the excellent course of always maintaining a double source of supply by means of two calves each week; moreover, it has made arrangements with a foreign correspondent, who every week sends it some tubes from his own supply. In this way, interruptions in the working are almost impossible. Since these measures were taken, the Director of the Brussels Institute can go to rest with a light heart, and without being condemned at the first failure to be buried alive after the fashion of negligent or unfaithful vestals. Thus, so far from wishing to deny the difficulties of the enterprise, we are the first to recognise them. It is because they are real that the greater number of isolated experimenters, having only their zeal and insufficient experience to support their efforts, have almost invariably failed, to the great detriment of the reputation of the method, which they hastened to impugn. Animal vaccination was worth nothing, they said; when it was only those who performed it—and performed it badly—who should be held responsible for its constant unsuccess.

From this have arisen premature judgments, which continue to weigh upon a method as yet imperfectly understood. We formally lodge an appeal against the conclusions of Dr. Seaton's report of 1869; but we think it right to state the spirit in which this appeal has been made, and the grounds upon which it rests. We are only doing our duty in paying merited respect to the high impartiality, the wisdom, the great experience of the author of that report. His deductions spring naturally from the facts ascertained by his researches; but let us say at once that what might have been correct ten years ago is now so no longer. The method has thrown off its swaddling clothes; it is no longer a child, but a full grown man, who has outgrown all the imperfections of youth.

Animal vaccination can, then, proceed henceforward with a firm step; but on the condition that it is confided to trusty guides. These guides should possess three essential qualities—experience, accuracy, and the sinews of every enterprise, "ways and means". Confide the creation of a vaccine institute to inexperienced hands, and you will condemn it beforehand to sterility; put it in able hands, and success is assured, if you do not cripple its resources. And let it not be thought that the necessary expenditure for such an institution attains considerable proportions. I estimate that, for an annual expenditure of a thousand pounds, London could keep up a first-class vaccine institute, and derive immeasurable advantage from it.

There still remain two points for me to touch upon. We have already seen that animal vaccination has been reproached with being uncertain in result; in other words, with wanting in activity. We know what this allegation is worth. But there are others who reproach it with having too great an activity. An university professor, otherwise a very estimable man, repeats every year to his pupils in the same unvarying tone, that humanised lymph is preferable to animal lymph, because the latter is too strong, because it gives rise to phlegmons, etc. What can we understand by all this, if not that he has confounded the method in its principle and in its applications? If he had compared the two lymphs by employing them according to the same method, this notion would never have possessed him; but he has been spoken to about the scarifications made for the purpose of utilising the lymph on points, and the disasters (!) which they cause. He has been shown pustules made by means of lymph inserted into deep, wide, and painful incisions, sometimes ingeniously made with the badly sharpened end of the ivory points themselves. And he has believed all this, and has arrived at the conclusions which have just been quoted.

So there is not enough activity on one side, and too much activity on the other. All this would be unpleasant if we had not, to cut the matter short, the high authority of Dr. Seaton, which cannot be impugned. Dr. Seaton says: "Neither in the size of the vesicle, nor in the intensity of the local symptoms, nor in the length of period which the eruption takes to run its course, is there any notable difference" between animal and humanised lymph. And this is the truth. If any do not share in this opinion, it is because they have observed badly and have not understood, for example, that if very young animal lymph give rise to a luxuriant eruption upon which we must congratulate ourselves, it would doubtless be the same if human lymph equally well chosen were made use of, and that the contrary would take place under opposite circumstances.

Let us pass to the conclusions. Does animal vaccination preserve from small-pox as does humanised vaccination? Up to the present, no one has contested this, and we have no taste for fighting windmills. We confine ourselves, therefore, to what we wrote five years ago on this subject. "Out of more than ten thousand children vaccinated at Brussels with animal lymph, from 1865 to 1870, and who went through the terrible epidemic of small-pox which in 1870 and 1871 frightened the world, not a single one was, to my knowledge, reported as having been attacked by the disease. The same immunity was shared by those—a much larger number—whom I had revaccinated, and who, at the same period were living in epidemic centres." Three years later, in 1878, wishing to have a clear conscience, we appealed to our colleagues, at a meeting of the Belgian Academy of Medicine, in the following terms. "I have said before that no such case had been reported to me. I repeat it; and, up to the present, none of the numerous practitioners that I have questioned on the subject have contradicted me. Have there not been any? It seems to me impossible. However this may be, I appeal to the medical officers of hospitals and charities to clear up the point, which, on account of the deductions to be drawn from it, requires to be strictly verified." This appeal has remained, and still remains, without reply. Such a silence is what I hope to inspire in you as the most eloquent testimony in favour of the method of which I am the most confident defender.

In spite of all this, there are yet found men, high in scientific and public opinion, who, relying upon distant experiences, badly done and badly construed, and upon opinions which they maintain because they have not the courage to abandon them, persist in repeating, every time that they have the chance, that animal vaccination is a bad thing. Let us content ourselves with answering them—relying on the reasons that we have just brought forward—with this other formula, which is just as distinct and is perfectly true, that animal vaccination is a good thing.

I will conclude by stating precisely the position that I recommend should be assigned to animal vaccination. This position has not the exclusive character that might be inferred. In the recent report which I addressed to the Minister of the Interior of Belgium, and to which I have referred above, I have thus frankly explained it. The State Vaccine Institute, whilst being the supporting column of vaccination in the country, ought not to have any thought or claim of substituting itself for the traditional practices. Vaccination from arm-to-arm, strong in its ancient rights, is, and will long yet remain, the greatest strength against small-pox, and nothing ought to be omitted to encourage and regulate it. Animal vaccination ought now only to be its faithful auxiliary, but an auxiliary so useful, that it would be as unjustifiable to pass it by as to desire to upset suddenly the classical method.

These are, gentlemen, the considerations which I have had the honour of placing before you. If I am permitted to come from so far to bring them, it is not because I had great confidence in my feeble resources. But I have thought that your kindness would not fail me; and I remember especially that I am in the country where one of its greatest orators, I think, Mr. Gladstone, has said those charming words, which come so appositely to screen my insufficiency: "The success of the orator is in the feeling, not of him who speaks, but of him who listens."

TOTTENHAM.—This is a very practical little report, and shows much real progress. The death-rate for 1878 was 17.3, the total number of deaths being 590. Of these, 97 were from zymotic diseases, giving a rate of 2.8 per 1,000. Enteric fever caused 7 deaths, scarlatina 21, diarrhoea 26, diphtheria 9, whooping-cough 19, measles 13, and small-pox 2. One long-standing trade nuisance has been got rid of, and another is much less offensive. Hille's process of treatment of sewage, which has been in constant use for three years, is well spoken of, and the sludge is removed without difficulty. The water has been most favourably reported on each month by Dr. Frankland, though a constant supply is a desideratum. A disinfecting oven and a proper mortuary seem to be wanted in this rapidly-growing district.

RESEARCHES

ON THE TEMPERATURE IN PHTHISIS AND SOME OTHER DISEASES:

WITH REMARKS ON THE GENERAL VALUE OF THERMOMETRIC
OBSERVATIONS IN DISEASED CONDITIONS.

*An Address delivered at the Opening of the Session of the Liverpool
Medical Institution, October 1878.*

By A. T. H. WATERS, M.D., F.R.C.P.,

Physician to, and Lecturer on Medicine at, the Liverpool Royal Infirmary;
President of the Institution; etc.*

As a contrast to the oscillations of temperature in acute phthisis, let me call your attention to those of acute capillary bronchitis. Under ordinary circumstances in bronchitis attacking the larger tubes, the temperature does not rise high; but, in capillary bronchitis, the rise is often considerable. It rarely, however, reaches the elevation of acute pneumonia, or even acute tubercular phthisis. There is, in some cases, a difficulty in making a differential diagnosis between acute capillary bronchitis, and acute phthisis. I think the thermometer affords important aid in such cases; and I have been able, by its use—by recognising that the temperature did not reach a great height, although it was sustained for many days at a rather high level—to give a favourable prognosis, although the physical signs were such as might have existed in acute phthisis.

Let me call your attention to Table No. VIII, which represents the temperature-movements in a case of severe capillary bronchitis. The oscillations were very decided; but you will observe that, unlike that which occurs in phthisis, or typhoid fever, the temperature was highest in the morning—much higher, indeed at 9 A.M. than at 9 P.M.; and this is not an isolated case; in several others of a like nature, I have found the temperature highest in the morning.

TABLE VIII.—*M. C., aged 28. Acute Capillary Bronchitis.*

Date.		Morning.	Evening.
February 7	.. .. .	—	101.2
" 8	.. .. .	100.6	100.8
" 9	.. .. .	101.5	101.7
" 10	.. .. .	102.0	102.3
" 11	.. .. .	103.2	99.0
" 12	.. .. .	104.0	100.0
" 13	.. .. .	103.0	99.0
" 14	.. .. .	103.2	99.6
" 15	.. .. .	102.6	99.2
" 16	.. .. .	102.8	99.1
" 17	.. .. .	101.6	100.0
" 18	.. .. .	100.2	99.6
" 19	.. .. .	98.6	99.0
" 20	.. .. .	98.6	98.6
" 21	.. .. .	98.0	98.6
" 22	.. .. .	98.6	98.4

Table No. IX represents the temperature in the case of a woman who was admitted into the Infirmary under my care. Her condition for several days was such that I was unable to form a correct opinion of the nature of her disease. From the temperature, pulse, and general aspect, as well as the physical signs, I feared that we had to deal with a case of acute tuberculous of the lungs. Soon, however, the temperature began to assume oscillations, which made me hope that no tubercular disease existed. The temperature was highest in the morning. Gradually the patient improved, and made a satisfactory recovery. The case turned out to be one of acute capillary bronchitis, with probably some localised pneumonia.

The differential diagnosis between phthisis with cavities, and some cases of bronchitis with dilated bronchi is by no means easy. Without attaching undue importance to thermometric observations, it cannot be doubted that, while the physical signs may be such as to admit of hesitation as to the exact nature of the affection, the thermometer may afford valuable aid. I have referred to the fact that, in capillary bronchitis, I have found that the temperature does not have the afternoon, or evening rise so common in phthisis; on the contrary, the temperature is highest in the morning. In the following case, which simulated in a great measure one of chronic phthisis, but which I diagnosed—I believe correctly, as far as could be judged by the course which the disease took—as one of bronchiectasis, I was aided much by the thermometer. The temperature never rose high; and, instead of having an evening exacerbation, had almost invariably an evening remission.

* Concluded from p. 808 of JOURNAL for November 22nd.