

Correspondence.

"Audi alteram partem."

OUR VACCINE LYMPH.

To the Editor of THE LANCET.

SIR,—As you support the object which I have in view—namely, the provision of State facilities for the optional use of animal lymph for vaccination, I have no desire to quarrel with you respecting matters of detail. I did not therefore consider it necessary to reply to your article in THE LANCET of the 20th ult., in which you ridiculed the views that I advanced as to the impairment of the protective powers of the old Jennerian lymph in use in this country, and as to the variolous nature of the so-called variolo-vaccinic lymph produced by Messrs. Ceely and Badcock, by the inoculation of the cow with variolous matter. I see, however, that in last week's LANCET you again return to the charge; and as I conceive that, were I to allow your reiterated assertions to pass unchallenged, they might mislead your readers, I must ask you to allow me to correct them. In the first place, however, permit me to point out that my arguments on these points are perfectly germane to the subject in dispute. When I advocate the use of animal lymph, I am answered with Dr. Seaton's statistics, which show that in proportion to the number of points of insertion animal lymph does not produce the same number of vesicles as the humanised lymph current in this country. I could quote half a dozen authorities to the effect that this inferiority does not exist; but I do not wish to obscure the real question at issue in any dispute as to the weight to be attached to this or that authority, when it can be decided much more satisfactorily on its own merits. I therefore say that it is puerile to enter into details as to the number of vesicles produced. What we want to know is the relative amount of protection against small-pox conferred by the two methods of vaccination. Dr. Warlomont assures us that, after having vaccinated with animal lymph for now nearly twelve years, he has been unable to learn of a single case, and the Belgian Academy of Medicine has been unable to learn of a single case, in which a person vaccinated with his lymph has been attacked with small-pox. Dr. Martin, who has had an enormous experience with animal lymph in the United States, repeats the same assertion. On the other hand, during the recent epidemic (1876–7–8–9), of 15,117 persons, treated in the several small-pox hospitals under the control of the Metropolitan Asylum District Board, 11,412 had been vaccinated (Mr. Jebb's letter in *Times* of last Nov. 8th). In the early days of vaccination, when the lymph was fresh, death in such cases was almost unknown. In Copenhagen, for example, a city at that time of 100,000 well-vaccinated inhabitants, there was not a single death from small-pox from 1811 to 1823 inclusive (Simon's Report, Appendix, p. 171). Taking 8000 cases of post-vaccinal small-pox recorded in London, Edinburgh, France, and Copenhagen, between 1818 and 1830, I find that the mortality was 1 per cent. In the 11,412 cases just referred to, observed during the recent epidemic, it was 8·8 per cent. In the fifteen or twenty years immediately succeeding Jenner's great discovery, when the lymph in use was still comparatively fresh, the universal practice was to make only one insertion on each arm, or often one on one arm only. Dr. Marston showed very conclusively that in the time embraced in his observations (1836–51), one or two insertions did not confer sufficient protection, and that among persons with one or two vaccinal cicatrices attacked with small-pox, 6·21 per cent. died, whereas of those with three or more, only 1·3 per cent. succumbed. Coming down a little later, Dr. Seaton has classified 6905 cases of post-vaccinal small-pox which occurred in London during the epidemic of 1870–73, and there the mortality among persons with only one or two cicatrices had risen to 10·4 per cent., while in persons with three or more cicatrices it was 6·4. What is the use of wrangling over details as to relative numbers of insertions and vesicles, when we have before us facts like these—facts which show that the mortality in post-vaccinal small-pox observed in the two epidemics of the past decade has been about nine and a half times as great as it was in 8000 cases observed between 1818 and 1830?

Coming now to my assertion that M. Chauveau's experiments prove that the variolo-vaccinic lymph produced by the inoculation of cows with small-pox, and put in circulation by Messrs. Ceely and Badcock, is simply small-pox, I venture to say, that if you will read Mr. Ceely's own account of his experiments, you will arrive at the same opinion, without the assistance of M. Chauveau at all; at least, you will arrive at this conclusion, that whatever the so-called variolo-vaccinic lymph was, it was not cow-pock. If, then, it be not small-pox, we have no facts, whatever before us to justify us in asserting that inoculation with this lymph exercises that protective power against small-pox which is exercised by inoculation with small-pox or cow-pock virus. Mr. Ceely's book is, I believe, out of print, and, though I have a copy by me as I write, I refer you for a quotation of his description of his experiences to Dr. Ballard's prize essay, or M. Bousquet's *Nouveau Traité de la Vaccine* (Paris, 1848), where, at p. 444, you will find a very clear and succinct summary of the results produced. Mr. Ceely, having succeeded in inoculating two cows with small-pox, proceeded to vaccinate with the product. The first experiment was accidental. Mr. Ceely's assistant, Mr. Taylor, having pricked his hand with the lancet employed in collecting lymph from the cow, there resulted a vesicle, fever, and an eruption, which Mr. Ceely calls roseola. In the subsequent cases the vesicles were undistinguishable from those of genuine cow-pock; "but," I quote for the sake of brevity from M. Bousquet, "the general symptoms were very different. The primary symptoms comprised, with infants, sleeplessness, agitation, loss of appetite, vomiting, and diarrhoea. Towards the ninth day few escaped fever, and this fever was accompanied with vomiting, delirium, diarrhoea, &c. Adults suffered still more; headache, shivering, anorexia, anxiety, nausea, powerlessness to quit bed, &c. But neither on infants nor on adults was anything observed which resembled variola or varioloid; only roseola lichen and an eruption of vesicles such as one sees in chicken pock. Finally, adds Mr. Ceely, the eruption remains always local; only one often sees supernumerary vesicles near the pusules. In one case, however, one of these vesicles was found on the shoulder and another on the neck, and in two others they appeared on the abdomen. And in spite of all this," adds M. Bousquet, "Mr. Ceely persists in maintaining that vaccinia is only small-pox modified by transmission through the cow!" Then M. Bousquet goes on to compare the results obtained by other experimenters who had worked in the same groove. First he takes Thiele's experiences. In his cases the fever showed itself twice—on the second or third day, and again on the eleventh to the fourteenth, as in inoculated small-pox; and when Thiele neglected to temper the strength of his virus with some drops of milk, there supervened a veritable small-pox, with fever and its two eruptions. Then M. Verheyen, proceeds M. Bousquet, reports in the *Memoirs of the Royal Belgian Academy of Medicine*, 1847, p. 133, that in a certain case variolous matter inoculated on the cow had produced, when re-inoculated on man, "variola to which nothing was wanting." M. Gaultier, he goes on to say, reported that one of the infants experimented upon had died of confluent small-pox; and he adds the following paragraph, on which I should be glad if any of your correspondents could throw some additional light:—"The same thing," he writes, "has just happened in England. There also variolous matter, having been inoculated on the cow, was retransferred to infants. The first had only a local eruption, but on pursuing the experiments it was found that in some cases perfect variola occurred, so that the authorities forbade the use of the virus under penalty of 300 francs fine"—(Op. cit., p. 550-1). As to the similarity of the vesicle produced by this vaccino-variolic lymph with that produced by cow-pock, there is nothing in that. As M. Bousquet remarks, and as anyone may see by referring to Jenner's petition (quoted in Mr. Simon's Report, Appendix, p. 2), Jenner himself was struck with the close resemblance which existed between the vaccine vesicle which he produced and those produced by inoculation; and a number of the most celebrated inoculators (quoted by Bousquet, p. 449) state "that when, as was common, there was no secondary fever, the variola limited itself to the points of insertion." After this I need hardly add that Bousquet does not believe in the vaccino-variolic lymph; but it may be necessary for me to state (so intense do I find the prevailing ignorance regarding the literature of vaccination) that Bousquet was one of the greatest authorities on the subject which the present half-century has

produced—a man of cosmopolitan reputation, a man whom I may describe as the Trousseau or Bretonneau of variola and vaccination. Well, what Chauveau ten years ago did was to prove by a most exhaustive series of experiments what Bousquet and many others had been convinced of for years before—namely, that variolo-vaccine lymph was not cow-pock, and that it was small-pox; and this he did in such a way as enabled him to explain the discrepancies exhibited in the results obtained by different previous experimenters. Where Ceely had only succeeded in inoculating two cows, Chauveau inoculated seventeen, in fifteen of which the success of the operation was proved by subsequent insusceptibility to cow-pock inoculation. With the variolo-vaccine matter from these animals he inoculated a child on March 14th, and obtained vesicles undistinguishable from cow-pock. But on the 23rd there was fever, vomiting, and an eruption on the face and trunk, which on the 24th had developed into “very fine variolous pustules, mostly umbilicated, forming a semi-confluent eruption.” A second child inoculated from this one had a secondary eruption consisting of fifteen pustules on the face and other parts of the body. Fear of creating a centre of infection in the hospital where the experiments were conducted prevented them being pushed further in this direction. But, having reinoculated on the cow lymph taken from the variolo-vaccinic vesicles on the arm of the children, which, as I have said, were undistinguishable from cow-pock, it produced, not cow-pock, but the totally different bovine small-pox eruption. Now if a chemist finds two substances, undistinguishable in outward appearance, to act in a totally different manner in the presence of a given reagent, he has no hesitation in pronouncing them different. This was precisely what Chauveau found on testing the variolo-vaccinic, as against the cow-pock vesicle, with a physiological reagent. From the reaction of the one substance the chemist says it is so and so, and from the different reaction of the other he declares it to be something else. This is again precisely what Chauveau did. The secondary symptoms noted by Ceely and Thiele themselves sufficiently, to my mind, show that the disease that they succeeded in producing was not cow-pock; and that, with its delirium, its vomiting, and its secondary eruptions, it bore a most suspicious resemblance to inoculated small-pox. Chauveau demonstrated that it was inoculated small-pox. The lymph so produced is still going the round in the country, and it either still preserves its powers of infection or it has lost them—probably through that degeneration of the virus which inoculators observed to occur, through successive inoculations from the primary vesicle. If it retains its powers of infection there is every reason to believe that inoculation with it confers precisely the same protection against small-pox as inoculation with ordinary variolous matter. If it has degenerated so as to have lost its infective power, there is nothing to assure us that it retains its power of protection. Your article speaks of Chauveau's results as conflicting with those obtained by Mr. Marson and Professor Simmons. Certainly they did, even as the latter conflicted with those of Messrs. Ceely and Badcock. For Mr. Marson and Prof. Simmons, when they inoculated cows with small-pox, simply failed to obtain any results at all; or rather they failed to recognise the significance of the very slight and local eruption which characterises inoculated small-pox in the cow. Chauveau himself failed to recognise it, until its specific nature was revealed to him by the accidental discovery that it destroyed susceptibility to subsequent inoculations of cow-pock. Having made this discovery, which had escaped Ceely on the one hand and Marson on the other, he inoculated twelve animals without one failure, and with uniform results, and from these he proceeded with the experiments which I have just summarised.

I am not a dogmatist, and am content to leave humanised lymph to those who prefer it. I, for many reasons, prefer animal lymph, and claim the same facilities for its use for myself which, until we have amply tested both, I am willing to accord to others in respect of the article of their choice. But when my opponents turn round and attempt to choke me off with peddling arguments about cost of calves, proportion of vesicles to punctures, and so on, I hardly care to waste time in confuting them with the experience of other countries on these points of detail. I answer boldly, cow-pock lymph direct from the calf protects against small-pox, as the lymph used by the early vaccinators protected against small-pox. Your lymph protects against small-pox, and against death by small-pox, to an

infinitely smaller extent. You don't even know what an indeterminably large portion of your lymph-stock is—whether it is not small-pox pure and simple, or whether, if modified so as to be innocuous to the general community, it is not utterly fallacious as a protection against small-pox. What we have to aim at is, not a mass of petty details which the early vaccinators cared nothing about, but the results obtained by them in the first ten or fifteen years of the present century. In those days the work was performed in what we should now consider an utterly inefficient manner, and often by unskilled men; *but they worked with good lymph*. The result was that for long post-vaccinal small-pox was of the rarest occurrence, and when it did occur the death-rate, instead of being one in every ten or eleven persons attacked, as has been the case in the epidemics of the decade just concluded, was for many years less than one in every hundred.

I am, Sir, yours very sincerely,

CHARLES CAMERON, M.D., M.P.

Glasgow, January 6th, 1880.

ON THE DIAGNOSIS OF HYDROPHOBIA.

To the Editor of THE LANCET.

SIR,—In my remarks on rabies, or hydrophobia, I have pointed out most of the difficulties in connexion with a true diagnosis; and, further, I think I have established by my statistics that there are cases on record of recovery from the disease. Mr. Moore has quoted, in the very interesting observation published in THE LANCET of December 13th, a passage from my book bearing on this point. Whatever name we may give the disease from which his patient recovered, there cannot be a doubt that the group of symptoms he describes are sufficiently pathognomonic, and that, if unchecked by remedial treatment, death would have been the result. As Mr. Moore justly observes, the fact of the patient recovering is regarded by some as *prima facie* evidence that the patient was not suffering from true hydrophobia, but from an affection either simulative or caused by some morbid change in the medulla oblongata.

In order to obviate in the future all disputes, I offer what I think would be a crucial test, derived from my experience with rabies in animals.

The West Riding of Yorkshire has suffered to a considerable extent from rabid dogs, and the police authorities have been compelled to destroy both rabid and suspected dogs; and, living in the district, I have been able to obtain many animals for experiment and dissections. In order to test the condition of the dog, I have collected in small tubes, like those in a feeding-bottle, the saliva taken both from living animals and from those recently killed. By injecting the saliva into rabbits the actual existence of rabies will be found out by the development of the disease in that animal or not.

I would extend the same method to the human being. Breschet and Magendie proved that the human saliva is capable of producing rabies in animals. Maurice Raynaud has more recently confirmed this evidence. I would therefore advise all practitioners attending a presumed case of hydrophobia to collect the saliva each day in a tube, to seal it hermetically, and to have it tested on some of the lower animals as soon as possible.

At present there is under treatment a child, with most suspicious symptoms, bitten by a rabid dog thirty-three days ago, and I have arranged with the medical attendant to adopt my suggestion. It is premature to enter into the particulars of this case until the result is known, which shall be duly forwarded for publication in THE LANCET.

I remain, Sir, your obedient servant,

T. M. DOLAN, F.R.C.S. ED.

Halifax, Yorks, Dec. 16th, 1879.

* * Mr. Dolan's suggestion that the diagnosis of every case of hydrophobia should be rendered, as far as possible, certain, by ascertaining whether the saliva is capable of communicating the disease to one of the lower animals, is of great importance. There can be no doubt, from the experiments to which he refers, that such a test would often supply information of the greatest value. We do not at present know whether the saliva of the human subject will communicate rabies with certainty in all stages of the