the observed phenomena of aerial convection (assuming that the aerial theory is correct) might partly depend on temporary conditions of ebb or flow of infectivity. Both in its shortness of duration and in its excess of flow, a "spring tide" struck me as not an inapt simile for the special outbursts; but "The nautical[1] analogy," says Dr. Sisley, "does not help" him. I am sorry. The literature of the subject is full of references to epidemic "waves," and such terms certainly have been useful in my own reading—or, at least, I have imagined so.

Dr. Sisley says I have "Brought no evidence to show that the virus is more intense at the beginning of an epidemic." That is undoubtedly so; indeed, I am inclined to believe that the virus is usually less intense then. I spoke, however, not of the "beginning of an epidemic," but of "a rising epidemic," which is quite a different thing. As for the facts, they may not belong to every outbreak[2] everywhere; but those to which I referred are contained in Mr. Power's Reports, regarding which I had said in an earlier part of my paper: "I am assuming that the members of the Epidemiological Society are well acquainted with the general lines of the Fulham investigation." The special outbursts *did* occur during the rise of the epidemics. Here, again, is it allowable to go back to first principles? All nature is most active and vigorous in its spring time, man and animals in theirs, and why not the contagia of infectious diseases? Next, as to the "concentration of acute cases as centres of infection," Dr. Sisley presumes that all I mean is "that the worse the cases are, and the more there are of them in a given place, the greater will be the amount of infective matter in that place." But this is not exactly all that was in my mind. In these days of investigation regarding attenuation and intensification of virus, one thinks of something more than a mere adding of vesicle to vesicle and pustule to pustule as sources of variolous infection, and it is not easy to read Dr. Thompson's suggestive article in Stevenson and Murphy's "Hygiene" or Dr. Sweeting's paper on Post-Scarlatinal Diphtheria[3] without being led to speculate (for it is only speculation) whether the aggregation of acute cases of small-pox in a hospital ward or elsewhere may not result in "an altered quality or increased potency," rendering the disease "more readily transmissible and more easily diffusible." (To save critical comment, let me say that I am not complimenting Dr. Thompson and Dr. Sweeting, or hinting that they were the first to write on this subject.)

What I meant with reference to Haygarth and the atmospheric conveyability of moist small-pox matter was that moist matter could not well become disengaged from its source and get into the atmosphere. As it stands in my paper the statement is so brief as to be hazy, but if Dr. Sisley will turn to Stevenson and Murphy's "Hygiene," vol. ii., p. 400, he will see that I referred to an experiment in which a number of children were set round a small table, on the centre of which was placed a dossil of cotton soaked in variolous pus constantly renewed. Finally, as to a curious assumption made by Dr. Sisley on a very trifling point, I did not say that Dr. Whitelegge was the first to call attention to the matter with which I associated his name, and I cannot even see that I implied it. As a matter of fact, my reason for mentioning his Milroy lectures was the very opposite; it was because they contained, so far as I knew, the *latest*, and not the earliest, exposition of the point in question. I am obliged to Dr. Sisley for giving me the opportunity of trying to clear up (though even yet there is no lack of fog) some points which seem to have been left in specially Cimmerian darkness, but at the same time an apology is due to you for this letter having run to such unexpected length.

I am, Sirs, yours truly,

Glasgow, Feb. 27th, 1894. JOHN C. MCVAIL.

'THE NATURE AND TREATMENT OF ASPHYXIA NEONATORUM."

To the Editors of THE LANCET.

SIRS,—In reading Dr. Alexander Morison's interesting paper on the above subject I cannot help feeling that there are one or two points in the physiology of asphyxia neontorum which might bear a little speculative reflection. What, for instance, is the condition of the trachea in an infant who has never breathed? One cannot conceive that the trachea is filled to the glottis with fluid, for were this so the first inspiration would inhale a tracheaful of mucus into the bronchi and inevitably produce suffocation. Still less can one imagine the trachea to contain air. The most probable condition is that the two surfaces of the trachea are in contact, or separated only by a small quantity of mucus. The posterior muscular wall is flaccid enough to admit of its apposition to the curve of the cartilage, and the first inspiration separating the surfaces permits the access of air to the lungs. Now, supposing the child to be born semi-asphyxiated—that is, to have made no initial effort at inspiration—Sylvester's method is, as I have found it, in many cases useless and, if practised, does no more than draw up the abdominal contents into the thorax. And the reason for this is quite plain, for inasmuch as, the glottis being closed, there is no air in the trachea, the air pressure on the glottis is no greater than on any other part of the chest, and consequently there is no inducement for air to enter the lungs. This, however, is entirely changed should a voluntary inspiration have taken place and the trachea become patent, for then no more satisfactory method than Sylvester's could be devised. Hence the *raison d'être* of placing the mouth of the operator to that of the child and more or less forcibly inflating the chest, although it is true that the stomach often becomes inflated at the same time. Acting on this suggestion I find no better method of commencing artificial respiration in an infant moribund from asphyxia than intubation with a catheter carefully performed, for when the chest is once inflated it is easy to carry on respiration on Sylvester's lines.

I am, Sirs, yours truly,

Cheniston-gardens, W., Feb. 28th, 1894. E. A. BARTON.

[1] Why "nautical"? Spring tides are independent of sailors and ship

[2] E.g, Oldham?

[3] Transactions Epidemiological Society, vol. xii.

"THE CORONER'S COURT."

To the Editors of THE LANCET.

SIRS,—It is inevitable that a medical coroner should occasionally offend some of his brethren. Dr. Love's letter, in THE LANCET of Feb. 10th, is a case in point. He is called on Jan. 31st to a man who has cut his throat at Mitcham, attends to him, and orders his removal to Croydon Infirmary. On Feb. 3rd the man dies, having been for the last three days of his life under the care of Mr. Wilson, the superintendent of the infirmary. Then arises a nice question, Which medical man shall be summoned to the inquest? Alas, in such a simple case the coroner cannot call both. He calls impartially the practitioner who is, in his opinion, best able to help the jury to ascertain the actual cause of death—viz., the infirmary superintendent. Whereat Dr. Love writes off to you that he has "a grievance"; that the coroner "has not dealt justly with him," &c. But if the coroner had summoned Dr. Love he would have given "a grievance" to Mr. Wilson, who in his turn would have wanted to "air his grievance" in your columns, and to show how the hard-worked Poor-law medical officer "does not receive much consideration even from those who ought to know better." Not long since one man knocked another down. A medical man was called, who ordered and superintended the injured man's removal to the hospital, where he died shortly after admission. As the case was manifestly one of manslaughter or murder, and desiring to have the evidence as complete and accurate as possible, I ordered the medical man to make a post-mortem examination and attend the inquest; but, being on very friendly terms with him, I suggested—and he cordially accepted the hint—that the house surgeon should be invited to join him in making the examination, and that the fee of two guineas should be divided between them, though the house surgeon was entitled to no fee. When my officer invited the house surgeon to join the medical man in the mortuary, he not only refused to attend, but was highly offended and reported the matter to the hospital committee, as though I had been guilty of a grave offence in sending a gentleman not on the hospital staff to examine a corpse lying in their mortuary. This case, read in conjunction with Dr. Love's, shows how impossible it is to please everybody, and how ready some people are to discover a grievance and to blame a coroner. In all similar cases all that can be reasonably required of a coroner is that he shall act impartially and call before him the surgeon who, from his knowledge of the circumstances, seems to be best able to help the jury in their inquiry.

In THE LANCET of Feb. 3rd there is a note on "Inquest Fees," to the effect that the Croydon county court judge