

in all cases be taken from a healthy child, as it is in the last degree important to be confident that we have lymph upon which we can rely as certain to run its course, pure and simple, producing a definite effect upon the constitution. The affiliation of skin diseases &c. occurring after vaccination upon that operation, affords abundant examples of the *post ergo propter* line of argument, and, *toto cælo*, differs from the proofs of constitutional contamination by syphilis, scrofula, &c.

Until the source of syphilitic inoculation in the case of the child Chiabrera has been determined by the committee, it is, perhaps, idle to speculate, or to attempt to found a practical rule, upon the hypothesis that has been offered with regard to the non-transmission of syphilis by lymph, and its transmission by the "peri-vaccinal" blood. As it stands, it is simply an assumption that the blood-globules only can be the medium of contagion. Professor Hebra's experiments contradict it,* and the ordinary mode of infection by sexual intercourse is opposed to the view; so also is the practice of vaccination itself, in which a disease is conveyed into the system by vaccine lymph alone, although that lymph is frequently and unavoidably mixed with a few blood-globules; notwithstanding which, past experience is, as I have endeavoured to show, against syphilitic contamination thereby, which could not have been the case had blood been the special medium of contamination. This theory, if admitted, would open the door to still greater carelessness in vaccination than we have already too much occasion to lament. It would come to be believed that lymph might be taken indiscriminately from any child, totally regardless of its health or constitution, provided only blood-globules were not taken with the lymph. Every experienced vaccinator well knows that many vaccine vesicles are so flat as to make it almost impossible, even in the most careful hands, to avoid the intermixture of blood. For reasons above given, irrespective of all questions about syphilization, it is important to use only good, healthy, reliable lymph.

The unspeakable importance of the topic under discussion will, I trust, be sufficient excuse for my obtruding again upon your space while asking for evidence to support the conclusion you have laid down. Your words will travel beyond the limits of the profession, and will, I much fear, be used by the opponents of vaccination to strengthen prejudices already existing, and to aid the efforts now being made to obtain a retrograde legislation upon vaccination.

I remain, Sir, your obedient servant,

Upper Holloway, Dec. 2nd, 1861. W. B. KESTEVEN, F.R.C.S.

* * We would suggest to our correspondent an unprejudiced consideration of the evidence brought forward in the recent treatise of M. Rollet.—ED. L.

ON THE EARLY TREATMENT OF CHOLERA.

To the Editor of THE LANCET.

SIR,—You are aware that in 1853 and 1854, assisted by the valuable information afforded by the Registrar-General, I was enabled to point out to the medical profession and to the public that cholera is not a disease *sui generis*, independent of, and a superfoetation on, diarrhoea, but that in every case the attack of spasms, vomiting, purging, &c. (the attack of cholera) is preceded by a diarrhoea for a few hours, or for a few days, or for a few weeks; and that if the disease is scientifically attended to in this diarrhoeal stage, this diarrhoea can easily be cured, the developed stage of the disease thereby prevented, and life thereby will not be endangered. And I also pointed out that it is impossible that an individual in perfect health can be at once struck down by spasms, vomiting, &c.—by cholera, without his labouring under a diarrhoea, and that the reports published of persons being suddenly struck down by cholera, while a moment before they were in good health, arose from the fact, that the public and the medical profession were not aware till it was pointed out to them in 1853 and 1854, that almost the whole serum of the blood may be drained away by a painless diarrhoea; that the heart may have ceased to beat, and the blood may have ceased to circulate; the individual may be past all human aid, yet he is walking about for business or for pleasure, unaware that he has anything serious the matter with him, and all this before he is attacked with spasms, vomiting, &c.—before he is attacked with cholera.

From these facts you will see at once how important, how essential it is to attend, when cholera is about, to the slightest deviation from the normal action of the bowels.

* Professor Hebra inoculated four persons with the blood of patients suffering secondary syphilis, and after fifty-two days found no result.—Simon's Report, p. 133.

There was another important pathological fact derived from the above inquiry—that when cholera is about to break out in a locality, and when it has already broken out, half, one-third, or one fourth of the usual dose of purgative medicine too often will bring on diarrhoea, followed by cholera and death. Therefore, from these facts these important deductions may be drawn: That it is impossible to attend too early to the diarrhoeal stage in cholera, and that purgatives are contra-indicated in any dose, and in any stage of this disease.

As you have seen by the public papers, cholera is making sad ravages in India, and the army has particularly suffered. As the etiology and the pathology of the disease have never been scientifically studied in India; as they still continue to believe that it is a disease *sui generis*, independent of, and a superfoetation on, diarrhoea; as, consequently, the medical treatment of the disease is still empirical; and as I have seen by the papers that the Indian Government had appointed a Commission of five members to inquire into the rise and progress of this outbreak; in the interest of the public I took the liberty to point out to the Minister of State for India, and to the Minister for War here, the importance of having the etiology and the pathology of the disease scientifically studied in India. At the same time I also pointed out to them that the present Commission appointed by the Indian Government—where three of the gentlemen out of the five composing the Commission were completely unacquainted with medical science, who could not form and who could not give an opinion on a question so strictly medical—would be an injury, not a benefit, to the public. To these representations I have received answers from both Ministers; but I am led to believe, from these answers, that the opportunity will be allowed to pass away, that the disease will not be scientifically studied in India, and that the next outbreak will find the public as little prepared to meet this scourge as they were in 1817. In the army there we shall have a repetition of the horrors of the Crimea, and those under which the army is now suffering, repeated.

Therefore, Sir, in the interest of humanity, let me solicit a place for this letter in THE LANCET, in the hope that the medical profession, to whom the public look in the hour of sickness and in the hour of danger, may join their efforts, and point out to those administering the public weal, that it is their duty to protect the public from pestilence.

I have served, and I have seen the disease, in an army in the field before the enemy, and I have seen the disease in an army in quarters. I am aware how difficult it is to deal with the disease in an army, in the field, and before an enemy; but I am also aware how easy it is to deal with the disease in an army in quarters, and I have no hesitation in saying, that if the etiology and the pathology of the disease had been understood in India, and if the military and the medical officers had known and had done their duty, the country would not have had to deplore the loss of so many valuable lives.

I remain, Sir, your obedient servant,

Bruton-street, Nov. 1861.

D. MACLOUGHLIN, M.D.,
Member of the Legion of Honour.

"SORE HEELS."

To the Editor of THE LANCET.

SIR,—In a recent number of your journal, I see some remarks made by Mr. Skey on treatment of fractures of the tibia and fibula, singly or both together, by suspension, after the plan advocated by Mr. Luke, and variously modified by Dr. Bell Salter and others. Mr. Skey states that it is apt to produce troublesome sores on the heel, for which reason he does not recommend it.

Knowing the great weight attached to all that Mr. Skey writes, from his personal repute and his position at St. Bartholomew's, I deem it necessary to trouble you with these remarks, because the question involves a matter of fact, and such a statement, if uncontradicted, is likely to bring an excellent method of practice into disrepute.

Mr. Luke's very simple and ingenious plan has been in use at the London Hospital for upwards of twenty years. I find from our published statistics that the fractures of one or both bones of the leg admitted here are considerably over 150 a year (sometimes nearly 200). Almost every case during the period referred to has been "slung;" and I can confidently appeal to every surgeon, dresser, and patient concerned to support me in the assertion that "sore heels" are a rarity at the London Hospital. The cases include simple, comminuted, and compound fractures. The secret (if there be one) of success lies in the placing a pad just above the heel, so that that part may