Chloroform from the same bottle had been administered to a case by me on the previous day, and was given to another patient afterwards without any disturbing effect. The amount administered in this case was not accurately measured, but could not have exceeded half an ounce; and the patient was completely, but by no means deeply, under when the alarming symptoms arose. I am inclined to believe that respiration and circulation ceased simultaneously; but I am of opinion that the symptoms were due to respiratory cessation, aided by the upward displacement of the diaphragm from the abdominal fluid, the heart's cessation being secondary. I would call particular attention to the following—(1) The normal size of the pupils; (2) the position of the tongue; (3) the sudden development of the condition; (4) the lividity of the face throughout in the entire absence of obvious signs of laboured breathing; (5) the early arrest of the heart's action, as discovered by auscultation and puncture; (6) the complete failure from the first to make air enter or leave the chest by any artificial methods. I may add that during a fair experience of anæsthetics, both in the hands of others and myself, I have never known alarming symptoms come on so suddenly and all efforts from the first prove so absolutely futile; and this case, in my opinion, lends support to the view, now so commonly held, that in the vast majority of cases of death under anæsthesia respiratory failure plays the leading part.—I am, Sirs, yours obediently,

F. MORTIMER ROWLAND, M.B. Cantab.,
Resident Medical Officer, City Infirmary.

Birmingham, Dec., 1892.

To the Editors of THE LANCET.

SIRS,—It is a far cry from you to where I am writing, but after waiting in vain for some more doughty champion of chloroform to appear, especially one from the Edinburgh school, I venture to raise my feeble voice, impelled thereto by what seems to me only just and fair to the merits of this useful servant of our profession, but one which appears liable to become decidedly unruly in use, and unusually fatal to our unhappy patients. I take it that the conclusions of the Hyderabad Commission have settled it that the respiratory centres are the first in order to be affected, and the rule to watch the respiration is a sound one. I make bold to broadly lay it down that there are two classes, and two only, where death occurs under the use of this anæsthetic, and these classes are—(1) unsuitable cases, and (2) unsuitable apparatus.

1. *Unsuitable cases.*—As regards these I opine that we are all agreed that they consist of those where some organic disease of the heart and lungs exists, likely to be influenced by the drug, and to these we should perhaps add any affection of the centres presiding over the circulation and respiration.

2. *Unsuitable apparatus.*—Here I have been struck by the remarkable preponderance of the use of some one of the numerous inhalers in fatal cases. It is my firm conviction that if all apparatus are carefully deposited in the lumber room, out of the reach of the scientific mechanic, we would be gratified by a decided decrease in the number of deaths reported. Roughly speaking I think it will be found on an analysis of every case of death from chloroform narcosis that where death has not been the outcome of organic disease, the existence of which was unascertained, unsuspected or overlooked, it has been the result of the use of some inhaler or other. For my part I have ever since my student days used the folded towel applied over mouth and nose, ensuring a due admixture of air with the vapour. If ever I did have recourse to any other method I soon saw reason to return to the time-honoured, simple and useful towel or napkin folded crosswise. Keeping this towel well before the nose and mouth I keep my eyes upon the respiratory movements and regulate the administration accordingly. I feel as I write this that it must seem very platitudinous, as it has been urged and recommended again and again, but apparently to deaf or preoccupied ears. I have never had since a student any mishap in giving it, and I have given it to all sorts of cases, and numberless times and often, when far away from medical assistance, alone. I therefore cannot help wondering how it is that all the deaths occur which one sees recorded. As an improvement to your present heading, then, I would suggest "Deaths under Inhaler." In thus speaking of chloroform I do not of course pretend that it is any safer than ether, and do not decry the latter; but I only desire to put the saddle on the real delinquent, and in this case I am persuaded the inhaler, patent or otherwise, is to blame. The tendency of all true

science is to the simplification of treatment and the methods of practice.

Hoping that this protest will meet with the attention the subject merits, though conscious of how much better a case many another could present,

I remain, yours sincerely,

Orange, Oct. 5th, 1892. G. A. VAN SOMEREN, M.D. Edin.

THE USE OF ANTISEPTICS IN THE PREVENTION OF SCARLET FEVER AND OF SEPTICÆMIA.

To the Editors of THE LANCET.

SIRS,—The abridged report in the last issue of THE LANCET of my remarks at the Harveian Society on Mr. Tait's paper on Peritonitis is apt to leave an erroneous impression, which I trust you will afford me this opportunity of correcting. Inasmuch as scarlet fever spreads from patient to patient, my experience at the General Lying-in Hospital was, as you state, analogous to that of Mr. Tait in operation cases; but I differ from him *in toto* as to the channel whereby the patients are likely to have received infection, and, consequently, the deductions to be drawn from that experience are entirely at variance with the opinions advanced by him. Mr. Tait relates how, in the days when he used complete Listerian precautions, spray and everything else, he inadvertently carried scarlet fever from one patient to eleven others, on whom he operated in succession; and, assuming that he transmitted the infection at the time of the operation by direct inoculation, he puts this experience forward as evidence against the value of antiseptics to prevent not only scarlet fever but septicæmia also. From this view I entirely dissent, and for the following reason. Scarlet fever, as is generally recognised, spreads through the atmosphere, and the poison obtains entrance to the patient's system through the respiratory passages. It is not necessary, therefore, to assume gratuitously that, because a patient has been operated upon, the poison has of necessity been introduced through the wound. The very fact that three of Mr. Tait's nurses were also infected points the finger in the right direction. On this ground I would submit that antiseptics, however efficiently employed with a view to prevent direct inoculation, would not in themselves prevent infection of the patient by scarlet fever through the ordinary channels, either at the time of the operation or subsequently. On the other hand, if efficiently carried out, they will destroy, and therefore prevent, the introduction of the poison of scarlet fever and of septicæmia through the wound. I have shown how in the General Lying-in Hospital, in which for the last eleven years the service has been conducted upon Listerian principles, though scarlet fever spread from patient to patient, septicæmia as a cause both of illness and of death had been practically eliminated; and though scarlet fever spread, septic processes obtained no hold even in the scarlet fever cases. The precautions adopted aimed at and succeeded in destroying septic matter which might otherwise be carried by direct inoculation through wounds in the genital passage; but these same antiseptic precautions were obviously insufficient, and were never intended, unless supplemented, to prevent the spread of scarlet fever and of other poisons which permeate the atmosphere and find a door of entry through the respiratory passages. This experience I have related in detail in my paper on Scarlet Fever during Pregnancy and in the Puerperal State, published in vol. xxx. of the Obstetrical Transactions.

I am, Sirs, yours faithfully,

Weymouth-street, W., Nov. 23rd, 1892.

ROBERT BOXALL.

LACTIC ACID IN TUBERCULOUS ULCERATION.

To the Editors of THE LANCET.

SIRS,—The reputation enjoyed by lactic acid as a curative agent in tuberculous ulceration of mucous membranes is well borne out by Dr. Percy Kidd's interesting case reported in THE LANCET of Nov. 19th. That this drug has a curative effect in such cases must be the conviction of all who have employed it persistently and efficiently in laryngeal phthisis. Indeed, even where the laryngeal mischief is extensive and the lungs are affected, we believe that, if accompanied by sufficiently large doses of creasote, it may be applied with advantage either by means of a cotton-wool brush or in the