

Measures had been adopted to equalise the number of battalions at home and abroad, but circumstances in connexion with Egypt had not yet admitted of this being fully effected. The progress made in recruiting for the army is reported to be most satisfactory. We hope in next week's number of THE LANCET to advert more in detail to some of the points which are of more immediate interest to the medical service.

#### THE GIBRALTAR DRAINAGE SCHEME.

The committee on the Gibraltar drainage question have recommended in their report to the Colonial Office that scheme which proposes to have its outfall at Europa Point, at the southern aspect of the rock, by which the discharged sewage will go straight out to sea. Several proposals have been put forward, but it is considered by the committee that this scheme will consume less time and labour in its execution and be preferable to that which proposes to tunnel a passage through the rock. It will be remembered that this question of the drainage of Gibraltar gave rise to a good deal of controversy and discussion some time ago, and that a deputation from the residents and civilian local authorities came to this country in order to represent their views to the Government. The subject is one of obvious importance to the health of both the garrison and the civil population of Gibraltar.

A sum of Rs. 23,000 has been provided in the military budget estimates of 1894-95 for grants-in-aid for cantonment hospitals in the Madras Presidency, including Burmah.

The *Indian Medical Record* states that the appointment of Secretary to the Principal Medical Officer in India at Simla, held at present by Surgeon-Lieutenant-Colonel Exham, A.M.S., is to be abolished, the second Secretary, an Indian medical officer, taking over the dual duties.

## Correspondence.

"Audi alteram partem."

### "AERIAL CONVECTION OF SMALL-POX FROM HOSPITALS."

To the Editors of THE LANCET.

SIRS,—I wish to thank Dr. McVail for his reply to the questions suggested by his paper on the above subject. The questions were so numerous, and, for the sake of succinctness, so briefly expressed, that my letter has, I find, an air of abruptness which might possibly give an erroneous impression as to the value I attach to Dr. McVail's paper. And, as my questions referred only to points which appeared obscure, I hope you will allow me to say here that it seemed to me that at the adjourned discussion at the Epidemiological Society hardly sufficient importance was attached by some of the speakers either to the strength of the evidence which Dr. McVail brought forward or to the importance of the conclusion to which he came—namely, that small-pox infection may spread to a greater distance than is usually supposed. I find that Dr. McVail takes exception to certain parts of my letter. He asks in a footnote why I applied the term "nautical" to one of his analogies. It was a mistake. Nautical people, I have noticed, often make use of analogies referring to tides and waves, but no nautical person would be likely to speak of waves as constituting flood tides. I must apologise to Dr. McVail for having made a mistake as to what he meant in reference to conclusions arrived at by Dr. Whitelegge. The observation that at the beginning of an epidemic in a previously unaffected population there are more susceptible people than there are later on in the epidemic is the point to which I referred. I did not assert that Dr. McVail said that this was an original observation of Dr. Whitelegge, but I did suggest that he implied it. I thought, and still think, that in so far as Dr. McVail referred to this observation in speaking of "Dr. Whitelegge's conclusions" he implied that the conclusion was Dr. Whitelegge's.

With regard to the question raised as to the contagium of small-pox, there was a verbal error in my query. Dr. McVail has kindly pointed this out and given some interesting speculations as to the intensity of the virus at the beginning of an epidemic and during a rising epidemic. On this subject I have no wish to make critical comment, for medical specula-

tions are to me of interest in so far only as they are supported by facts, and the facts are not given by Dr. McVail. Nothing is further from my wishes than to enter into an argumentative discussion; but, whilst thanking Dr. McVail for the trouble he has taken in replying to my letter, and whilst readily crediting him with a desire to enlighten me, I must confess that he has not done so. This may possibly be due to obtuseness on my part, but I think he will readily admit that he has not answered the questions I asked, which arise out of his conclusion that "there must apparently be intensity of virus depending on minor waves of epidemicity referring to particular days or weeks, these waves constituting flood tides of infectivity." The question is: "What are possible minor waves of epidemicity?" Dr. McVail has not explained what he means by waves of epidemicity, and, as I have not been fortunate in putting Dr. McVail's figurative expressions into ordinary language, I should be really glad to learn what he means. If there are possible minor waves of epidemicity, how do they modify the intensity of the virus? This, I confess, is incomprehensible to me, and Dr. McVail has not explained what he means. In what way do minor waves of epidemicity refer to particular days or weeks? And how do minor waves of epidemicity constitute flood tides of infectivity? I shall be really grateful if Dr. McVail will explain these points. I can assure him I am not alone in being unable to understand what he means. He points out in his letter that the literature of the subject is full of reference to epidemic waves, and that he has found them useful. For my part, I admit that expressions concerning "epidemic waves," "epidemic constitution," and such like have seemed to me to darken counsel by words without knowledge.<sup>1</sup> In the case of the older writers such expressions were certainly used as a cloak to, rather than as a confession of, ignorance. I have no doubt that Dr. McVail, however, attaches a definite meaning to the expressions he uses, and that they are, therefore, capable of being put into simple words. The letter Dr. McVail wrote in answer to mine does not do this. He says that rhythm is a law of nature; that this law applies to individual cases of disease, and especially to infectious disease; and that epidemics are examples of the law of rhythm. I will not venture to comment on these curious assertions more than to say that they do not answer the questions I ventured to ask.

I am, Sirs, yours truly,

RICHARD SISLEY.

York-street, Portman-square, March 6th, 1894.

### KITCHEN BOILER EXPLOSIONS.

To the Editors of THE LANCET.

SIRS,—My attention has been drawn to two articles on the above subject which appeared in THE LANCET, one on Jan. 13th and the other on Jan. 27th, and everyone will agree with you in desiring the prevention of these sad catastrophes, which, though not on so large a scale and therefore not so impressive as the explosions of ordinary steam boilers, are yet very fatal, while they have unfortunately an importance of their own, inasmuch as they invade the sanctity of the home and strike terror into the family circle. So numerous are they that I have by me a list of thirty-eight explosions which occurred in the short interval between Friday, Jan. 5th, and Tuesday, Jan. 9th, killing twelve persons and injuring thirty-three others. As the Manchester Steam Users' Association has for the last thirty years been paying attention to this subject, visiting the scene of the explosions and examining the fragments in order to arrive at the cause of the disaster, while, in addition, it has instituted several experiments to ascertain the effect of charging red-hot boilers with cold water, it may be of assistance in this matter if I give you the results of its experience. The Manchester Steam Users' Association has invariably found on investigating kitchen boiler explosions that the circulating pipes between the boiler and the overhead cistern have been stopped up, the stoppage arising in some cases from the pipes being choked with ice, and in others from the stop-taps in the pipes being closed and the thoroughfare thereby shut off. In consequence of this the pressure has been "bottled up" and has gone on accumulating until it has become too strong for the boiler, and explosion has resulted. While the explosions due to the choking of the circulating pipes with ice are, of course,

confined to the winter time, those due to the shutting of the stop-taps in the circulating pipes may occur at all seasons, and several have occurred in the summer time. This clearly shows that they cannot be due to the dropping of a little ice into a red-hot boiler, while, as the stop-taps have been found to be closed after the explosion, neither can they be due to the readmission of the feed.

In 1867 the Manchester Steam Users' Association conducted a series of experiments in order to ascertain the effect by actual test of injecting cold water into red-hot kitchen boilers. For this purpose it made three such boilers, two of copper and one of cast-iron, red-hot, and then injected cold water. No explosion, however, took place, the boilers were not stirred from their seat, neither did they tremble nor evince the slightest signs of internal commotion. In 1879 the Manchester Steam Users' Association tried a series of red-hot furnace crown experiments with a full-sized Lancashire boiler, 27 ft. 9 in. in length, 7 ft. in diameter in the shell, and 3 ft. in the furnace tubes. The furnace crowns were heated to redness and then cold water showered upon them through a perforated pipe. This was done repeatedly. When the safety valves were blowing off at 25 lb. on the inch no appreciable rise of pressure took place, but when the pressure in the boiler was only at 6 lb. and the safety valves were shut down the pressure rose from 6 lb. to 27 lb., after which it gradually fell. The rise, however, was not instantaneous, but took three-quarters of a minute. These experiments with kitchen boilers, as well as those with the Lancashire boiler, plainly show that the idea that the injection of a little cold water into a red-hot boiler is accompanied with an instantaneous and irresistible generation of steam, equal in force to the explosion of gunpowder, is unfounded. Further, I have before me the fragments of a stone bottle which was placed in an oven when full of water, and with the cork tied down, in order that it might be heated and used as a foot-warmer by an invalid. Suddenly, as the family were sitting round the fire, the bottle exploded, blowing out a portion of the oven, killing a little girl, severely scalding a little boy, and also scalding their mother. In this case it must be clear that the frost had nothing to do with the explosion, that there was no dropping of ice into the bottle when empty and heated, as is supposed to be the case when kitchen boiler explosions occur, nor was there any readmission of the feed. The bottle burst simply from a gradually accumulating pressure, which had no opportunity of escaping, as the cork was tied down, just as an ordinary steam boiler bursts when the safety valve is overloaded. From the numerous examinations of exploded boilers the Manchester Steam Users' Association has made, as well as from the experiments it has conducted, it has come to the conclusion that kitchen boiler explosions are not due to the thaw but to the frost, that they are not due to the instantaneous generation of steam, but to a gradual accumulation which a small safety valve is able to relieve.

With this view the Manchester Steam Users' Association recommends that a safety valve should be fitted to every kitchen boiler, and considers none so simple or reliable as one of dead weight construction without either guide spindle or wing, so that there is no fear of its sticking fast. This should be fixed as near the boiler as possible, the connecting pipe not being more than six inches in length. It should be enclosed in a small cast-iron box with a hinged door so as to be protected from injury, while the door would admit of its being examined whenever that was necessary. To see that the passage through the pipe connecting the boiler to the safety valve is open the valve should be lifted occasionally, and if the water squirts out it will show that this is so. Should a little piece of grit get under the valve and prevent its reseating and cause it to leak slightly the valve may be lifted again, so as to blow the piece of grit away, or the valve may be turned round so as to grind it out. I have known these valves to work for years without giving trouble, and am told that one maker alone has sold about 10,000 of them.

In conclusion, I would strongly recommend every householder to have a safety valve fixed to his kitchen boiler before the occurrence of frost for his own safety and peace of mind. It can be procured for 10s. 6d., and fixed complete for about 20s. The publication of this letter will, I trust, assist in preventing a recurrence of these alarming disasters.

I am, Sirs, yours faithfully,

LAVINGTON E. FLETCHER, Chief Engineer.

Manchester Steam Users' Association, 9, Mount-street,  
Albert-square, Manchester, Feb. 19th, 1894.

\*\* We print this practical letter with great pleasure, and

think it only right to add that the Manchester Steam Users' Association is not a company working for profits, and has no motive other than a philanthropic one for desiring the public to adopt the safety valves recommended by Mr. Fletcher, their chief engineer. Pressure on our space and the somewhat lengthy nature of Mr. Fletcher's communication have prevented earlier insertion.—ED. L.

## "CHRONIC IRRITATION IN CANCER."

To the Editors of THE LANCET.

SIRS,—It is an old saying that there is nothing new under the sun. The history of medical doctrines certainly often endorses this. Again and again, as the years roll by, the same old doctrines, in slightly varied form, wax and wane. In connexion with modern ideas as to the pathogeny of cancer and other neoplasms there is no more striking fact than the widely-prevalent effort, amounting to a pandemic, now being made to resurrectionise the doctrine of Broussais as an adjunct to the microbe theory. That great man's name has, I believe, never before been prominently mentioned in this connexion, but the order of the day to regard cancer, sarcoma, and other neoplasms as the outcome of chronic inflammation precisely reproduces his doctrine. The wide range and the extreme simplicity of this brilliant generalisation are certainly most captivating. I should be really delighted to be able to accept it, not only on this account, but also because the germ theory would thereby be rendered paramount in pathology. Unfortunately, however, I am obliged first of all to consider the facts. For ten years—regardless of theoretical considerations—I have diligently recorded and classified facts relating to cancer and other neoplasms; and I find that these cannot be harmonised with this conception. In what I have to say on this important subject I hope I shall be able to express myself in such a way as not to be misunderstood. I am persuaded that the facts justify the belief that repeated irritations of long duration and moderate intensity are, in a certain proportion of cases, the precursors of neoplasia. Further, I think that we are justified in believing that parts thus chronically irritated may thereby be rendered more apt to take a neoplastic action than they otherwise would have been. Thus may we account for the great relative frequency with which certain cutaneous cancers are met with in chimney sweeps and other workers in irritant substances—e.g., tar, paraffin, &c.; but this conclusion is, I submit, as far as the facts justify us in going, a totally different thing from admitting that chronic inflammatory lesions are the necessary antecedents of cancer. It is against this doctrine that I protest; for, so far as I have been able to ascertain, the facts are decidedly against it. In my former letter I referred only to cutaneous cancers, because Volkmann's data are also based on cancers of this part; but I should not have expressed myself so emphatically had not these indications been fully confirmed by careful inquiries as to the association of the onset of cancer with pre-existing local disease in other parts of the body.

In the case of the female breast no inflammatory lesion seems more likely to be the precursor of cancer than chronic mastitis, and since this disease gives rise to tumour-like induration its presence can hardly be overlooked; yet, how rarely does mammary cancer ever appear to start in connexion with such indurations. I have met with this conjunction only twice in 137 cases, and among the numerous cases analysed by Gross,<sup>1</sup> in only 49 out of 907 (5.4 per cent.) did the cancer appear to develop in this way. Hence it appears to me to be improbable that chronic mastitis plays an important part in the genesis of mammary cancer. It tends to confirm this view that cancer of the breast is relatively of almost as frequent occurrence in the single as in the married. This is proved by the following facts. According to Gross,<sup>2</sup> among 1545 women with mammary cancer 85.5 per cent. had married and 14.5 per cent. were single; other estimates give the proportion of married at about 80 per cent. In the general population, according to Farr,<sup>3</sup> only 79 per cent. of women over twenty-four years of age ever marry, and in London the proportion of single women is higher. This shows the folly of regarding marriage as *per se* a cause of cancer, as some maintain, monogamy, according to these views,

<sup>1</sup> International Journal of Medical Sciences, March, 1883, p. 222.

<sup>2</sup> *Op. cit.*, p. 220.

<sup>3</sup> Vital Statistics, p. 20.