and with a few in his dormitory from 9.45 P.M. to 7.30 A.M. After this he was separated absolutely from his school-fellows in the sanatorium throughout his illness.

Now let us analyse the damage he occasioned during those few hours. I should state, however, *in limine*, that the type of mumps was severe; and although every boy was kept in bed for eight clear days (never even putting foot to the floor for any purpose) more cases of orchitis occurred than I have ever witnessed when such care is observed. A large epidemic—66 cases—ensued but none of them was of the same severity of type. I only give the period of incubation from the one source of infection, for after this it was difficult to trace it. It is usually asserted that it is in the dormitories, where eight or nine hours are passed, that the largest number are infected. Yet the originator of this group of cases slept in a dormitory with nine school fellows and only infected two of them (marked with an *), four of them having previously suffered from the malady, while three escaped.

The Period of Incubation of a Group of Cases of Mumps and its Orchitis.

Name.	Date of attack of mumps.	Day from infection.	Date of commencement of orchitis.	Day of illness.
G.	Jan. 18th, the originator of this group of cases.			
1. H.	Feb. 3rd.	Sixteenth.	Feb. 6th.	Third.
2. P.	" "	"	" 11th.	Eighth.
3. S.*	" "	"	" 13th.	Tenth.
4. D.	" "	"	—	—
5. T.	" "	"	Feb. 10th.	Seventh.
6. J.*	" 4th.	Seventeenth.	—	—
7. P.	" "	"	—	—
8. B.	" "	"	—	—
9 C.	" "	"	Feb. 11th.	Seventh.
10 F.	" "	"	—	—
11 R. T.	" "	"	—	—
12 P.	" "	"	Feb. 12th.	Eighth.
13 M.	" "	"	—	—
14 B.	" 5th.	Eighteenth.	—	—
15 C.	" "	"	—	—
16 W.	" "	"	—	—
17 B.	" "	"	—	—
18 F.	" "	"	Feb. 13th.	Eighth.
19 C.	" "	"	—	—
20 W.	" 6th.	Nineteenth.	Feb. 14th.	Eighth.
21 G.	" "	"	—	—
22 M.	" "	"	Feb. 13th.	Seventh.
23 M.	" 10th.	Twenty-third.	—	—

Thus 37·5 per cent. of these cases suffered from orchitis—a larger number than had ever previously occurred in my experience—and 77·77 per cent. on the seventh to the eighth day. I am, Sirs, yours faithfully,
March 16th, 1906 CLEMENT DUKES, M.D. Lond.

ETHYL CHLORIDE AS A GENERAL ANÆSTHETIC.

To the Editors of THE LANCET.

SIRS,—Having read various articles recently in THE LANCET regarding the use of ethyl chloride as a general anæsthetic, and having myself had the opportunity of frequently administering it, I feel justified in craving the courtesy of your space in order to state my results and to give a few opinions regarding the use of the gas. I may say that I am the more inclined to do this on account of the dread in which I have noticed a great many medical men, both at hospital and in practice, hold ethyl chloride as a general anæsthetic.

During recent months I have administered chloride of ethyl several hundreds of times and I have never had the least trouble during or after its administration. These results I am inclined to attribute more, in spite of the strong opinion to the contrary, to the safety of the gas than to any particular skill on my part in administering it. I have given it to people of both sexes and of all ages and have found it as satisfactory in young children and old persons as in

middle-aged men and women. A large percentage of patients to whom I have administered the gas have been placed under the anæsthetic for the extraction of teeth and I have found the rapidity of the production of anæsthesia a great advantage when compared with that of nitrous oxide gas. The length of the anæsthesia, too, is convenient, the simplicity of administration and the far smaller expense all giving it a decided advantage over nitrous oxide. For the operation of the removal of tonsils and adenoids I have found it invaluable, the length of the anæsthesia being amply sufficient for any experienced operator to remove both tonsils and thoroughly to scrape the posterior pharyngeal wall with a Gottstein's curette. In these cases I have been discussing ethyl chloride as an anæsthetic for a short period. I have never gone in for the routine practice of giving it for prolonged anæsthesia by the open method, but with the ordinary mask I have kept a patient under for over 20 minutes with no inconvenience. I cannot say I have found the after-effects of chloride of ethyl at all distressing and vomiting has occurred only in a large minority of cases and then never of a serious character.

And now one word as to the method I have found the most beneficial for administering the gas. I squirt into the bag of the ordinary ethyl chloride inhaler supplied by the instrument makers from 2·5 to 5 cubic centimetres of ethyl chloride from one of Messrs. Duncan and Flockhart's flasks—2·5 cubic centimetres for children, 3 cubic centimetres for women, and up to 5 cubic centimetres for men. I then place the bag over the face until anæsthesia is produced. This nearly always is accomplished within a minute, the usual period being three-quarters of a minute, although it varies from four or five breaths to the full minute. The disadvantage of this method is the sudden exhibition of the full amount of the gas to the patient, with a consequent choking sensation, and fighting for breath during the first few seconds. The alternative is to squirt into the bag, or through wire gauze into the face-piece, small quantities, such as one cubic centimetre, from time to time, but as one cannot prevent the entrance of air, however little, the anæsthesia is delayed, and I have noticed in some cases it lasts for a shorter period. I have also found ethyl chloride of great value in the commencement of anæsthesia under ether, introducing it into the bag by means of tipping up a small glass tube containing five cubic centimetres of the gas and allowing the ethyl chloride to run through a rubber tube directly into the bag of a Clover's inhaler.

I feel that if more medical men who have had experience of administering chloride of ethyl would publish their results a great deal would be done to remove what I consider the prejudice of many of our profession to the gas. I consider it should at least be given an extensive trial.

I am, Sirs, yours faithfully,
Blackheath, S.E., March 13th, 1906. HAROLD MOWAT.

THE PROGNOSIS AND TREATMENT OF CROUPOUS PNEUMONIA.

To the Editors of THE LANCET.

SIRS,—In THE LANCET of March 3rd, p. 589, I read with great interest Dr. C. H. Cattle's paper on Pneumonia. He mentions a case of pneumonia in a man, aged 73 years, with a temperature chart showing rigors, which he very rightly describes as being extremely rare in this disease. The patient quoted previously suffered from prostatic trouble and during the attack of pneumonia was catheterised. I should like to ask Dr. Cattle what was the condition of the urine? Unless this excretion was normal I should be inclined to suggest that the state of the bladder and urine was more likely to be the cause of the rigors rather than the pneumonia.

I am, Sirs, yours faithfully,
St. Mary Cray, Kent, March 17th, 1906. J. M. BENNION.

THE STERILISATION OF MILK.

To the Editors of THE LANCET.

SIRS,—Quite recently, and with very great interest, I have read the detailed and scientific report of Professor R. Tanner Hewlett on the Budde process for sterilising milk which appeared in your issue of Jan. 27th, 1906, p. 209. The Budde process seems to me to be a very genuine one and great credit is due to the discoverer, yet clearly it leaves much to be desired compared with such a process as that known as the

"vacca" process, an account of which I remember to have read in your valuable journal. The great drawback to milk treated by the Budde process is that it only remains sweet for a period of from eight to 14 days, whilst your report, coupled with that of Dr. Klein, of the "vacca" milk in your issue of March 8th, 1902, which I have before me, states that the latter will keep indefinitely in bottle—in fact, some of the samples on which you report you state to be quite a month old. The "vacca" milk is, in fact, absolutely sterile and yet neither heat in the ordinary way nor preservatives are employed. The Budde process, on the contrary, according to Professor Hewlett, does not kill spores.

From Professor Hewlett's account of the Budde process I cannot help thinking that the results must prove to be rather uncertain. The effect of this method must be largely dependent upon the quantity of "catalase" which is present in the fresh milk. This quantity varies in fresh milk and decreases as the milk gets older, from the time it has been drawn from the cow. This must necessarily cause an element of uncertainty in the process. In any case it must be admitted that in the Budde process a preservative is added to the milk in the shape of peroxide of hydrogen, which may or may not be harmless. In Germany, at any rate, the addition of peroxide of hydrogen to fluids or food is prohibited. By treating milk by the Budde process there is not always the certainty that the peroxide of hydrogen has decomposed because the same quantity of catalase is not always present and it is impossible to find this out each time in a practical methodical way. I am pleased to note that Professor Hewlett admits this himself where he says that it may prove necessary to add yeast enzymes artificially to the milk.

Having made the preservation of milk the object of many years' study I was naturally interested in this article and I trust you will allow me to point out what I consider to be its scientific imperfections. However ingenious the idea may appear it is not on a level, in my opinion, with the vacca process which you have previously described, which, while completely preserving the milk, adds nothing to it or disturbs its nourishing qualities, as is the case by the ordinary methods of sterilising milk.

I am, Sirs, yours faithfully,

Amsterdam, March 14th, 1906.

Dr. F. BASENAU.

THE HAMPSTEAD GENERAL HOSPITAL.

To the Editors of THE LANCET.

SIRS,—The management of the Hampstead General Hospital has been the subject of much criticism by the local medical profession for some time past and the matter has been coming to a head gradually in the course of the past year or two. The hospital was originally founded by a local general practitioner as a home hospital, where individual local practitioners could themselves continue, in more satisfactory circumstances, the treatment commenced in the patients' homes. The hospital was in considerable difficulties for some years; finally it was only by the united efforts of the local profession that it was kept open.

From a home hospital this institution has advanced, or degenerated, into a "general hospital," with a large out-patient department, a limited staff of general practitioners, a few (pay) beds for the patients of practitioners not on the staff, and a management on which the outside profession is unrepresented. This divorce between the management and the local profession is greatly to be deprecated on every ground. The hospital has recently become possessed of a very large donation and it is rumoured that it is about to receive another. It has been transferred to a new specially erected building quite recently (and objections might be easily made to the choice of site). The lines upon which the hospital authorities have been recently developing that institution indicate further alterations in the near future and, indeed, there are rumours, apparently well founded, which foreshadow further drastic changes.

This letter is not for the purpose of opening a discussion in your columns, Sirs, but in order to request the general practitioners of Hampstead and its neighbourhood to keep an open mind upon the subject in view of the proposal to call a meeting of the local profession in the course of the next few weeks to consider the whole matter. I will, therefore, merely observe that there appears to be a consensus of medical opinion that an out-patient department in this neighbourhood is neither necessary nor desirable; that if any hospital be required in Hampstead it should be one of the nature of a home hospital; that the appointment of a

small privileged staff is unfair to the patients of other practitioners, as, indeed, it is to the practitioners themselves (though the staff, by all reports, do their work in a very satisfactory manner); and that the appointment of a staff of consultants, which is rumoured as a possibility, in place of the existing general practitioner staff is opposed both to the original objects of the founder and to the needs of the locality.

I am, Sirs, yours faithfully,

F. R. HUMPHREYS,

Chairman, Hampstead Division, British Medical Association.

Fellows-road, N.W., March 20th, 1906.

THE ADMINISTRATION OF ADRENALIN IN CASES OF FAILURE OF THE CIRCULATION.

To the Editors of THE LANCET.

SIRS,—Mr. J. P. Lockhart Mummery's comments on the case I published in THE LANCET of March 3rd did me the service of calling my attention to his extremely valuable articles in the issues of March 18th and April 1st, 1905. I quite see that theoretically the ideal method of administering the drug is intravenously. He, however, states that even "in cases of failing circulation adrenalin if injected under the skin will have a very poor chance of reaching the peripheral circulation." In my case, however, the pulse at the wrist was not to be felt and yet the recovery of the patient proves that it did reach the circulation. Of course, it may be said that the hypodermic needle pierced a fair size vein, but the same almost immediate effect (about 20 seconds elapsing before a change was noticed) occurred after each injection. The point I wish to emphasise is that in suitable cases an injection of adrenalin hypodermically may be of the utmost service. As a general practitioner I have constantly come across cases in which I now see that its use might have been of the greatest value. In many of these, circumstances would not have permitted intravenous infusion. It consequently seems to me that if some manufacturer would place on the market sealed capsules containing suitable doses capable of being carried in the hypodermic pocket-case he would be fulfilling a want. Mr. Mummery also finds the action of adrenalin very transitory. In my case, however, weakening of the pulse began quite 15 minutes after the first injection and was not marked enough to call for a second injection till 30 minutes after it. It is true that I gave 10 minims orally, but Mr. Mummery says this can have had no effect. The remaining injections were given at about two-hour intervals according as the pulse required it; that is to say the effect appeared to last about that length of time. One small point more: do the words "vaso-motor stimulant" imply a nervous action? It happened that I knew the action of adrenalin was direct, and unless these words imply action through a nerve, which it does not seem to me they do, surely it is not right to say that "adrenalin can in no sense be called a vaso-motor stimulant."

I am, Sirs, yours faithfully,

Chiswick, W., March 19th, 1906.

H. O. BUTLER.

THE DANGERS OF THE FLUTE-KEY FLASK FOR ADMINISTRATION OF ETHYL CHLORIDE.

To the Editors of THE LANCET.

SIRS,—I beg to thank you for inserting my letter in THE LANCET of March 17th on the dangers of the flute-key flask. I regret that I have made an error. Instead of "(3) Messrs. Sumner's measure," it should be "Messrs. Reynolds and Branson's 'Antivolat.'" I should be glad if you would correct this.—I am, Sirs, yours faithfully,

THOMAS Y. SIMPSON.

Royal Albert Hospital, Devonport, March 19th, 1906.

THE BATTERSEA BOROUGH COUNCIL AND ITS MEDICAL OFFICER OF HEALTH.

To the Editors of THE LANCET.

SIRS,—I observe in the list of duties proposed by the Battersea borough council for the medical officer of health to be appointed by them that this officer must "issue all instructions and communications to sanitary inspectors