

together with a diagram which agrees very closely with that given in the present article. This operation was extensively used by surgeons for the closure of skull defects during the war, and while I am obviously completely unable to subscribe to Mr. Jones's claim to novelty or priority, I can, as a result of my experience, heartily endorse his statement that the method is a useful one, although in many instances equally good results are obtained if the pedicle is abandoned and the graft of the outer table is inserted free.—I am, etc.,

London, W.1, May 6th.

NORMAN C. LAKE.

Convalescent Homes and Vaccination

SIR,—In the springtime it is natural for a doctor's thoughts to turn to the amelioration of the lot of ailing children. Convalescent homes customarily make antecedent vaccination a condition of admission; this I consider is not now necessary.

All reasonable people know that vaccination protects from small-pox, but the great majority do not, by repeated vaccination, keep themselves protected against small-pox. Why? Because they realize that vaccination is an insurance proposition and that the individual risk of being exposed to serious small-pox infection is infinitesimal, and so they trust our excellent health service both to detect any small-pox which may occur and to protect them from infection, should occasion arise, by vaccination, etc. The authorities of convalescent homes have not this trusting spirit despite the fact that the hospitals have it, for hospitals do not make antecedent vaccination a condition of admission, although those on waiting lists would have plenty of time in which to so occupy themselves.

What does this antecedent vaccination condition entail on the children? As only about half the babies born are vaccinated in infancy, about half the children for whom convalescent treatment is recommended are either vaccinated immediately prior to admission to the home or are refused admission. The reason for insisting on the vaccination of the child must be either in the interest of the child population of the home or in the interest of the unvaccinated child. If the first, it is largely illusory. The unvaccinated child cannot start small-pox in the home unless he was already incubating the disease before arrival; if he is incubating small-pox, vaccination will not stop the development of that disease unless performed within the first three days of the incubation period, while the small-pox will not prevent the vaccination from taking unless the child at the time of vaccination was already suffering from the prodromal symptoms of small-pox.

A table from my 1923 Gloucester Small-pox Hospital Report illustrates this:

Small-pox Cases Vaccinated after Catching the Disease

	No. of Days between Vaccination and Appearance of Small-pox Rash														Total	
	0	1	2	3	4	5	6	7	8	9	10	11	12	13		14
Cases where vaccination took ...						2	1	2		3		2	1			11
Cases where it did not take ...	1	2	1	1												5
															16	

Thus, as a preventive of the introduction of small-pox into a home successful vaccination cannot be relied on unless it is done a fortnight before admission, but this time-condition is not enforced. It appears, then, that the second reason accounts for the rule—that is, that the homes require all their children to be vaccinated to

guard them from the small-pox to which they may be exposed during their stay of a month or less in the home. Is this reasonable? What is the risk? We know that for a generation the risk of variola major has been negligible, not primarily because of its infrequent introduction into this country, but because of the adequate treatment of the introduced cases and their contacts, made possible by the Infectious Diseases Notification Act of 1889. We know that variola minor, prevalent in certain areas since 1923, now seems to be disappearing, that it is a trivial disease for which vaccination has been refused by very large numbers of contacts, and that the disease is of low infectivity—for example, it must have been introduced year after year into our seaside resorts, and yet it has never spread in them. It seems erroneous to suppose that during a month's stay at a convalescent home the risk of contracting small-pox of either form is a sufficient one to justify making the vaccination of all entrants obligatory. When one realizes that these children are ailing and are admitted for the restoration of their health, to subject them to another disease, even though a mild one—vaccination—seems in the circumstances to be reprehensible.

A paragraph of the 1929 Vaccination Order merits the attention of convalescent home authorities. After stating that post-vaccinal nervous disease occurs mainly among children of school age and adolescents who had never previously been vaccinated, it concludes as follows:

"The Minister is of opinion that, in the present state of knowledge, and so long as the small-pox prevalent in this country retains its present mild character, it is not generally expedient to press for the vaccination of persons of these ages who have not previously been vaccinated unless they have been in personal contact with a case of small-pox or directly exposed to small-pox infection."

At the present time, variola minor, the mild small-pox referred to, is no longer prevalent; the last variola major cases were the *Tuscania* ones of some years ago. The chances of exposure to small-pox infection in this country are, indeed, very remote. Surely the time is opportune for the reconsideration of convalescent home policy in regard to vaccination. I hope they will disregard the vaccinal condition of their little patients save in the presence of small-pox.—I am, etc.,

R. W. JAMESON, M.R.C.S., D.P.H.,

West Wickham, April 25th. Barrister-at-Law.

Aspiration of Empyema in Children

SIR,—In the *Journal* of December 10th, 1932 (p. 1067) is written: "These two points—the question of air in the pleural cavity and the large masses of fibrin—are the main criticisms of the aspiration method." I would add to these a major objection that has not been overlooked by the authors of the paper under review—prolonged delay in recovery from an advantageous localization of a general infection. I can confidently relegate the two points mentioned to their proper place as difficulties associated with a method that is tedious because wanting in radical features.

The alarming mortality revealed by the American Empyema Commission impelled surgeons to resort to closed or intermittent drainage by aspiration. Immediately the mortality fell, and the low figure of 4 per cent. was obtained early but not sustained. It cannot be claimed that a mortality now reported as running into double figures is satisfying. The deaths are due to the risks attached to the slow design of the technique chosen. Its temporizing feature involves the patient in a struggle against a condition demanding salutary relief. The long stay in hospital invites cross and intercurrent infections, which are so often fatal.

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I take it that the work of Drs. E. T. McEnery and Joseph Brennemann was designed to show by comparison the superiority of aspiration over all other methods, including variations of that technique. According to these authors aspiration has been on trial "for the last ten or fifteen years" and seems to have done its best. It is a poor best. The immediate effect of aspiration is relief and fall of temperature, but with re-accumulation there is relapse. The conditions alternate for weeks. The little patient is exhausted by an acute illness prolonged by a complication which we should welcome as an indication that the infection has been overcome and blocked, yet we treat the situation with vacillation.

At this point, to justify my statement, I will describe the pathological processes involved as I understand them. In the main, all forms of abscess, including empyema, conform to this description. After aspiration the relief of tension within the cavity is brief and incomplete, and, the tension still being positive, pus formation does not cease, but is actually excited by the tension, variable though it be. This statement may not conform to modern views, but I will supply ample evidence to support it. I fear that it is taught that re-accumulation is the result of bacterial activity, and all our efforts at cure are based on that supposition. It is not true. Admittedly, pus formation is originally excited by continued stimulation provided by the virus, but a point is reached at which this influence wanes and the abscess is said to be "ripe." Definite localization has been established. It is impossible to imagine how cure could result otherwise. Beyond this stage increase of size is due to the attempt on the part of the host to throw off the residue of a successful conflict. An attempt at physiological repair is made by adding polymorphs and lymph to the mass. These outpourings cease when the tension so caused is relieved by an exit being found or by the pus finding its way into yielding tissues. In the former case no more pus is formed, a serous discharge, small and lessening in quantity, taking its place. In the latter, pus production is merely suspended. These things could not happen if prevailing beliefs were true. In this description it is understood that secondary infection has been rigidly excluded from the draining abscess. Unless the discharge remains serous, reinfection has occurred, which is usual but reprehensible. *Hinc illae lachrimae!*

To prove the complete misconceptions held to-day in the pathology of abscess I will refer to the textbooks thereon. They say that, when an abscess ceases to spread a dense wall of cellular infiltration forms around it—"the so-called pyogenic membrane." Why "pyogenic" if it forms after the pus has arrived? This loose and utterly misleading description focuses the attention on the passive pus as the active influence to the exclusion of the potent energies of the granulation tissue which has been struggling all along to clean up a messy conflict. Of course, what happens is that granulation tissue forms first and pus exudes from its surface. This infiltration tissue is then rightly called "pyogenic." It is not a membrane. I think we can now agree on clinical and pathological grounds that the formation of granulation tissue is the first step towards cure. This conception must give us as surgeons great heart. When granulation tissue is found, it is the time to strike and cut to the core, but not to puncture, except to find the way.

My ground is now clear for a complaint that thoracotomy has not had a fair go. In defending it, I do not forget that a proof puncture is the first essential, and in very young infants one aspiration has been known to be followed by cure. The conclusion to-day is that thoracotomy, with or without rib resection, should only be resorted to when there is no other way out. In these circumstances the open operation must have a place in

the treatment of empyema. It is therefore our bounden duty to perform it under proper safeguards. Our great and only concern is secondary infection. It is an axiom wider than affairs surgical that evacuation must be attended by adequate protection to be safe. It is evident that the methods in use to-day are not safe, and in that case no surgeon has a right to perform the operation.

It is recorded by Drs. McEnery and Brennemann, and it is common knowledge, that pus evacuated through rupture into a bronchus results in rapid cure. Can we learn nothing from this?

The explanation is simple. The empyema is drained through a tube (the bronchus) that is bactericidal—in other words, protective against secondary infection. Surely we can provide an exit for the pus through a wound on the same principle—that it shall be received by a medium that is also bactericidal and therefore protective. But we must be watchful as to its efficacy from hour to hour. If that is too much for the busy practitioner, he must train someone to do it for him. By adopting the open method of drainage we respond axiomatically to a surgical emergency. The effect on the patient is dramatic. All constitutional symptoms rapidly abate, and if our precautions have been adequate there will be no relapses, but increase in appetite and well-being.

I could write a lot on the prevailing fear of collapse of the lung on opening the chest wall. As a matter of fact it expands and empties the pleural cavity of pus. Adhesions, which only form after secondary infection or prolonged and repeated aspirations, bind the lung down and in turn contract the chest wall, but these adhesions take weeks to form. By the open method the wound would have closed and the abscess healed long before this. Treatment by irrigation is the commonest cause of this disability.—I am, etc.,

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Hernia and Appendicectomy

SIR,—It was with great pleasure that I read in your issue of April 29th (p. 763) the protest of Mr. F. A. R. Stammers at the repetition of what I too believe to be the unfounded idea that the muscle-splitting incision for the operation of appendicectomy has any causal connexion with any subsequent appearance of an inguinal hernia. It is my practice to use this incision for appendicectomy in any case in which the diagnosis seems to be reasonably certain, and in which no special difficulties are looked for from adhesions or other cause. I have carefully looked for an undue proportion of inguinal hernias after many hundreds of these operations, but without result. It is, in my view, unfortunate that an attempt should be made to discredit what I believe to be the best incision for straightforward cases, and on grounds which are purely theoretical.—I am, etc.,

Clifton, May 1st.

C. A. MOORE.

Persistent Occipito-Posterior Cases

SIR,—Professor Bjornson's article in the *Journal* of February 25th raises once more the question of the treatment of persistent occipito-posterior positions of the vertex in labour. I should like to question the wisdom and the advisability of his treatment of these cases, and at the same time put forward a method which I have found uniformly simple and successful. Professor Bjornson's paper is illustrated by a figure which (Fig. 1) is gravely misleading. It shows the foetal head in the pelvic cavity, together with the whole thickness of the operator's wrist and forearm. This is impossible. I am stressing this inaccuracy because to do as Professor Bjornson writes means that the foetal head has to be pushed up out of