

on the assumption that everyone has already been infected) "from adult to adult" (why "from adult" necessarily?), and adds, "I know of no evidence to support this theory." But is the evidence any stronger for the theory that adult pulmonary tuberculosis is always the result of the activation of tubercle bacilli which have been lying dormant since childhood? Dr. Hodson makes the curious admission that "adults in some parts of the world may become infected" (note not "reinfected"). But is there any reason to believe that Oslo nurses or South African miners—the examples he gives—are not just as liable to infection in early life as people in this country?

Dr. Hodson then brings forward the case of a woman who broke down eight years after her husband died of pulmonary tuberculosis, and asks triumphantly: "What, then, is the incubation period and how do the upholders of the idea of adult reinfection" (note again "reinfection") "distinguish which of the known and unknown contacts has passed on the microbe vicious enough to cause the breakdown in an individual already infected?" Surely there is confused thinking or confused writing here! In reply one might quote some words of Huxley in another controversy on beliefs: "Why should anyone be asked to say how he knows that which he does not know?" We do not profess to any certain knowledge of the modes of transmission of the tubercle bacilli or of the incubation period. We do, however, know that the bacillus is ubiquitous and that therefore infection may presumably be possible at any time without being *directly* traceable to a particular contact.

But what answer can Dr. Hodson himself give us? Is it so much easier to believe that the tubercle can lie dormant and after a period of several years suddenly wake to life at the stimulus of "excessive stresses and strains," as he would have us believe? The question of the incubation period, as distinguished from the period of dormancy, would still remain to be answered. As regards the case of the woman quoted, we should like to know how far she was kept under strict observation after (and even before) the death of her husband. Unless kept under strict observation over the whole period she might well have shown signs of tuberculosis long before she came to be diagnosed; which would be very germane to the question at issue.

Dr. Hodson declares: "Apart from patients with a family history I cannot remember a single one that knew of a contact with a case of pulmonary tuberculosis." It is naturally the family, or rather the household, which presents the most cases of obvious "contact," and until the theory in which Dr. Hodson is a believer is proved he is not entitled to exclude all such cases as having no bearing on the question of transmission of tuberculosis to an adult from close association with a case of pulmonary tuberculosis. Incidentally, has Dr. Hodson ever asked himself in what proportion of cases of infectious diseases—for example, diphtheria or scarlet fever—it is found possible to trace the case to any known contact?

In conclusion, I agree with Dr. Wallace (January 31, p. 163) that the onus of proof lies with Dr. Hodson, and that "until concrete proof that adult to adult" (though why necessarily from adult I fail to see) "reinfection is impossible, our present management of tuberculosis should not be relaxed." I am so cut off from the world at the moment of writing that I fear my letter will be very late in appearing. But I much hope that this question will not be allowed to drop without the views of other tuberculosis workers being heard.—I am, etc.,

E. WEATHERHEAD.

### Increase of Tuberculosis

SIR,—May I draw attention to the unreasonable nature of Dr. V. S. Hodson's views in his letters of January 17 (p. 89) and February 14 (p. 239). If a decrease in an individual's "resistance" can be responsible for awakening to activity a previously dormant endogenous focus of tuberculous infection, then it is surely unreasonable to suppose that such a decrease in resistance cannot also render an individual susceptible to exogenous infection, whether he already has or has not a dormant tuberculous focus.

As everyone cannot enjoy the felicity of uninterrupted perfect health, I deem it wiser to continue to regard cases of "open" tuberculosis as grave public dangers. Speaking for myself, I would not willingly undergo the risk of *heavy* contamination

with tubercle bacilli, even in my moments (fortunately frequent) of perfect health and unimpaired resistance; nor, I think, would any other medical man, including, I believe, Dr. Hodson, despite the statement at the end of his letter of February 14.—I am, etc.,

Stafford.

J. G. H. FREW.

### Amoebic Liver Abscess

SIR.—Some years ago there was admitted to East Fortune Sanatorium a young discharged time-serving soldier whose illness presented points of interest which are in many respects similar to those of the patient described by Lieut.-Colonel J. D. Sandes (February 7, p. 184). Unfortunately the history in our record notes is not detailed in the matter of dates, but this history at least is accurate.

He was a Regular soldier from 1926 to 1934, and during that period he served for five and a half years in India. It is in respect of these five and a half years that we have no accurate dates. He was discharged some time during 1934 and was perfectly well until January, 1936, when he began to have attacks of nausea and vomiting. During February pain appeared in the lower part of his right axilla, and at the beginning of May he was admitted to a local sanatorium with a diagnosis of pleurisy. The diagnosis was not questioned until the end of the first week in June, when the abdomen began to swell. He was then transferred to us for diagnosis and treatment. He was in a very bad way—emaciated, extremely weak, had a hectic flush, was febrile (99° to 101.5° F.), and had a rapid pulse. His liver was huge, and the skin over the bulging abdominal portion was pale, glossy, and oedematous. The organ was not tender and the spleen was not palpable. The diagnosis of liver abscess was fairly obvious, and the abscess was easily located by puncture through the anterior abdominal wall. The fluid withdrawn was bloodstained and slightly greenish, but on the following day I inserted a needle into an area of dense basal dullness and drew off typical tropical abscess pus. Our treatment will not meet with Colonel Sandes's approval. The abscess was drained transpleurally after resection of 2½ in. of the tenth right rib posteriorly, and the best part of a gallon of pus escaped at the operation. Several pints escaped during the following day, and on the day after that again the drainage tube drained nothing but pure bile. He was given intensive treatment which included emetine by injection, stovarsol by mouth, and yatren enemata, and his recovery was rapid and continuous. He was discharged from the sanatorium six months after the operation, a big, sturdy healthy man. Cultures of the pus were sterile and smears showed necrotic debris. No pus cells were seen and amoebae were not found.

During his five and a half years in India this man had several attacks of diarrhoea, but he was never in hospital and none of the attacks was severe. Unfortunately the time interval between his return from India and the onset of his illness in January, 1936, is not known to me, but there was presumably an interval of at least two years of good health.—I am, etc.,

East Fortune Sanatorium, Scotland.

C. CAMERON.

### "Ether Convulsions"

SIR.—It may seem unexpected that a psychiatrist should ask for permission to speak in the discussion on this subject; but since Meduna introduced convulsions as treatment of certain mental disorders eight years ago the induction of epileptiform seizures and kindred conditions of cortical irritation has become a household activity of every psychiatric hospital and clinic. On both theory and practice of this therapy a vast literature exists, from which there is ample proof that the clinical effects as well as the beneficial results are produced by cerebral anoxia. Convulsant drugs and electric current are the usual methods, but some authors have applied inhalation of pure nitrous oxide or of nitrogen with more or less identical results (Himwich, H. E., *et al.*, *Proc. Soc. exp. Biol. Med.*, 1938, 39, 367; Fraser, R., and Reitmann, F., *J. Neurol. Psychiat.*, 1939, 2, 125; Fogel, E. I., and Gray, L. P., *Amer. J. Psychiat.*, 1940, 97, 677). Thus the incident which the anaesthetist and the surgeon dread and try to avoid is what the psychiatrist seeks and provokes. The subject of interest is the same, and my plea is for pooling observations between two branches of medicine. The study of the so-called ether convulsion cannot

but profit from the experiences of psychiatrists who have used a great variety of means to produce convulsions, including hypoglycaemia; they have the opportunity to observe the premonitory movements and the whole sequel of signs in artificial epilepsy at their ease.

The following observations are of interest in respect of the theories of ether convulsions. Atropine is regularly given in psychiatric cases to combat vomiting after convulsant drugs; only single twitchings, but never a full seizure, can be evoked by external—"neurogenic"—stimulation if the amount of the drug or of the electric current administered is below the convulsant threshold. The difference between the psychiatric patient in good physical health and a patient with high temperature on the operation table is obvious, but does not seem so great that a combined effort should not yield instructive results.—I am, etc.,

Crichton Royal, Dumfries.

W. MAYER-GROSS.

### Spinal Anaesthesia

SIR.—Having used percaine on a series of cases running into several thousands, I beg to plead for a flexibility of technique which should be associated with anaesthesia no less than with surgery: no single method is ideal for every case.

I have found the Etherington-Wilson technique to be the most accurate and rapid method for obtaining a high block, failure to achieve which is invariably due to neglect of details or faulty technique; but as this method is reasonably simple I cannot regard failure from that cause as being a contraindication. There is also one advantage which has not been stressed: there is no need to test for anaesthesia providing paraesthesias, notably "pins and needles" in the feet, are present.

The use of  $N_2O$  and  $O_2$  in a closed circuit assists in controlling respiration and in the avoidance of vomiting, which is either anoxic or reflex; the lightness of the anaesthesia required permits the employment of adequate oxygen and precludes the necessity for recourse to an agent of 100% potency such as cyclopropane, which, being both scarce and costly, should be kept for use by itself. Pentothal, owing to its depressant effect on respiration, should not be used save in the small amount sometimes desirable before spinal puncture in an apprehensive patient. In spite of the aspersions cast on it, a wheal at the site of the puncture will be found to decrease discomfort to a marked degree. I can thoroughly endorse all observations concerning the value of phedracin (Ciba 2020), which is best used intravenously.

For low blocks the same precision is not obtainable by means of the Etherington-Wilson technique as by the Howard Jones method with the hypobaric solution, or the various methods which employ the hyperbaric ("heavy") solution. Where the latter is seen to its greatest advantage is in the anaesthesia of the "saddle area" obtained by the "Sitting Bull" technique (Maxson) with 1 c.cm. of the "heavy" percaine, representing one of the least traumatic anaesthetic procedures possible. However, for surgeons who prefer to do anal operations on patients in the prone position an equally satisfactory result is obtainable with the "light" solution, which can be injected when the patient is in position, no further movement of the patient or even of the towels being necessary. It can thus be seen that both solutions have their own particular advantages; but both are highly susceptible to alkali contamination, the result of which is more readily visible in the case of the heavy solution, precipitation being immediate and obvious.

To reserve spinal anaesthesia for the robust seems as ill judged as to inflict it on the moribund, there being a great number of intermediate cases in which only the most pusillanimous would deny their patient the advantages of a spinal anaesthetic owing to an element of risk. It is our function to assess which are legitimate risks and which are not, and it will be found that the mortality bears an inverse ratio to the experience of the anaesthetist.

There are two main contraindications to spinal anaesthesia. These are cardiovascular degeneration, and general decrepitude from any cause; but before deciding against spinal anaesthesia the increased facility, which means speed in operating in a quiet abdomen, and the remote mortality associated with the volatile anaesthetic agents should be considered; a death after a general anaesthetic is as suggestive of an error in judgment

as is a death on the table under a spinal anaesthetic. Finally, I would plead for an eclectic attitude in technique and for the retention of both "light" and "heavy" percaine in the anaesthetist's armamentarium.—I am, etc.,

H. W. LOFTUS DALE,  
Hon. Anaesthetist, Royal Bucks Hospital;  
Anaesthetist, E.M.S.

Aylesbury.

### Heavy Percaine in Spinal Anaesthesia

SIR.—I should like to say how much I have appreciated Dr. Mushin's recent article. His views are practically identical with those I have formed on a series of 150 cases. Hypobaric techniques have been predominantly popular wherever I have worked, but realizing the greater comfort to the patient and the greater ease of administration of hyperbaric percaine I have used it now for the past eighteen months.

Many anaesthetists omit to turn the patient into the prone position when using "light" percaine because they realize it is uncomfortable for the patient, especially those who are in pain from perforated gastric ulcer or who have such conditions as colostomy or marked abdominal distension. It is also a cumbersome procedure. As laid down by Howard Jones, however, it must be done to ensure adequate sensory loss in upper abdominal operations, though it may be disregarded in operations below the umbilicus. It is, however, in just those painful upper abdominal operations that it is so necessary. In order to avoid this, I have on occasion seen the patient tilted in extreme head-down tilt with nurses and orderlies hanging on to the patient's shoulders. Or else they may be subjected to a similar extreme tilt in the reverse Trendelenburg, again with willing hands holding the patient from slipping on to the floor. Again, I have seen the bridge of the table raised in the small of the back, which certainly increases the discomfort of the patient. Again, patients much prefer to have their heads slightly raised on a pillow during an operation performed under a spinal anaesthetic, especially when they remain conscious. Light percaine technique is safer when the head is kept low, whereas in heavy percaine anaesthesia the head is slightly raised. Finally, except for a slight tilt of from  $5^\circ$  to  $10^\circ$  there is absolutely no shifting of the patient under heavy percaine once the lumbar puncture has been performed. There is also less chance of the needle becoming dislodged when using a small quantity of fluid, as in heavy percaine technique, than there is when introducing five or six times as much fluid as in light percaine block, with less possibility of deposition of percaine outside the theca leading to partial failure. In other words, heavy percaine is less cumbersome to use and more comfortable to the patient, the second quality being more important than the first. It was particularly interesting to compare Dr. Mushin's article with that read at the Royal Society of Medicine by Dr. Frankis Evans last year. The former gives us the maximum doses to be used, the latter the minimum.

With regard to premedication, I agree most emphatically in condemning heavy respiratory depressants before a spinal anaesthetic. My choice lies between omnopon and scopolamine and nembutal and morphine. For urgent operations I premedicate with omnopon gr. 1/3 and scopolamine gr. 1/150, and give a small dose of pentothal on the table prior to the lumbar puncture if the condition is painful, like a perforated gastric ulcer in a robust subject. For prearranged cases I give nembutal gr. 3 two and a half hours before operation, with morphine gr. 1/6 later, depending on the action of the nembutal. Both types of case receive  $N_2O+O_2$  when the operation takes place in the upper abdomen.  $CO_2$  absorption, of course, should be combined to control the depth of respiration. Cases for spinal anaesthesia must be chosen carefully. The upper abdomen must not be considered as a bar to spinal. That is akin to saying, "I do not use chloroform because I am afraid of it or it is dangerous." Most gastrectomies do better under endotracheal inhalation anaesthesia, but one occasionally experiences the robust subject who simply will not relax under inhalation anaesthesia, and here it is so important to achieve relaxation before the peritoneum is opened, otherwise it retracts from the wound and is again "difficult" when it comes to sewing up.

With regard to sterilizing of instruments, I have had some disappointments using distilled water, even when HCl has been added, probably because the bowls coming from ordinary