

described are not available; but I had hoped that the article, incomplete though it was owing to space restrictions, would have at least suggested that many pregnancies in tuberculous women are unnecessarily terminated.

I feel that it is very seldom safe to be dogmatic in dealing with tuberculosis, and while presenting this letter to refute Dr. Logg's accusation of "precipitate deductions," venture to commend "cautious comment" to him in subjects where much research still remains to be done.—I am, etc.,

Braintree.

R. C. COHEN.

Anaesthetics, Old and New

SIR,—Recent publications in the *Journal* tend to revive the impression that all is not yet well in the world of anaesthesia. May I, who have been a teacher of anaesthetics for over thirty years, be permitted to express my views on this subject.

Dr. Spoor (March 11, p. 374) writes: "The G.P. uses chloroform and will continue to use chloroform until something equally powerful, convenient, pleasant, and with less toxic is discovered." I hope not, and I know that the many students I have taught will not. Dr. Spoor's letter has been ably answered by Dr. Dale (March 25, p. 434), but if Dr. Spoor would like to have further information as to why chloroform is condemned he might be interested to read "The Present Position of Chloroform" (*Lancet*, June 12, 1926). In 1925 it was agreed at a meeting of the Scottish Society of Anaesthetists that "in the light of our knowledge of the properties of chloroform, to proceed to induce full surgical anaesthesia supplied only with the means of administering chloroform is unjustifiable." To-day that opinion holds good.

Dr. Kemp, in his article on the aetiology and treatment of convulsions during anaesthesia (April 1, p. 447), suggests that a convulsion during anaesthesia is due to interference with cell respiration in the brain resulting from a condition of anoxaemia. So far, indeed, it is the only acceptable explanation of this phenomenon. Dr. Kemp, however, fails to explain (1) why there were no recorded cases before 1924, and (2) why cases occur with a certain frequency in the records of some administrators and not at all in those of others. These two facts definitely support the view that recent methods and manner of employing anaesthetics are important factors in causing convulsions.

Open ether is not a very recent method, and, as a matter of fact, is relatively rarely the method employed when ether convulsions occur. In a series of over 20,000 cases of open ether I have not seen a case of ether convulsions. Yet Dr. Kemp states that "prophylactic treatment consists in avoiding open and semi-open administration of ether." What is meant by "semi-open" I do not know. Open ether, as I understand it, is a perhalation method with mask and drop-bottle and no limitation of atmospheric air. His first case of convulsions (Case 1, 1924) is reported as occurring during "anaesthesia with ether vapour, with oxygen and semi-open technique with mask and damp towels." I could well believe that damp towels, suitably employed, could be depended upon to bring about a condition of anoxaemia and convulsions. His Case 4 (1943) is typical of the complications and risks associated with modern methods: "pentothal-intratracheal nitrous oxide-ether . . . closed filter circuit . . . patient in second plane of third stage of anaesthesia . . . pulse 110, colour dusky." All this for a skin-graft operation requiring a simple gas-oxygen administration, and perfectly illustrating the manner in which the misuse of the modern machine leads to anoxaemia and convulsions.

What exactly is the value to the patient of the technique adopted in the above case? It is a method, too, which corresponds to that employed in many other places—e.g., cranial nerve palsies following general anaesthesia (see Humphrey and McClelland, and Carden, March 4, pp. 315 and 319). Dr. Carden refers to the closed-circuit carbon dioxide absorption technique as being almost a routine practice of most anaesthetists, its outstanding advantage being that a quiet operation field is ensured by the diminished respiratory excursions. This is certainly an indirect advantage to the patient, for the surgeon is able to operate more swiftly and with less trauma. I am not aware of any other advantages to the patient, and have not infrequently observed profuse sweating and con-

sequent dehydration. I look upon the closed circuit as an unfortunate necessity when cyclopropane is indicated as the anaesthetic agent to be employed. One must not forget, too, that surgical shock is associated with a low blood pressure due to a falling-off of the cardiac output. The cardiac output depends upon the venous return, and the venous return is greatly assisted by the respiratory excursions. Sufficiently quiet respirations can be assured by appropriate premedication.

It is very difficult to assess the value to the patient of any particular anaesthetic technique, so much must depend upon the skill and experience of the administrator. A method which saves time and trouble, entertains the administrator, and satisfies the surgeon, may not be the best for the immediate and post-operative welfare of the patient.—I am, etc.,

Dundee.

ARTHUR MILLS.

Medical Research

SIR,—May I add my voice to that of Mr. Malcolm Donaldson, who in the *Journal* of April 8 makes a plea for medical research as a career, and clamours for better conditions and some encouragement for those who show aptitude and interest and a bent for research work.

Various agencies have been set up in the course of time for the cultivation or encouragement of medical research—academies under Government patronage, learned and professional societies formed by interested individuals, special endowment funds the trustees of which distribute income in the form of subventions, and, finally, research professorships occupied by individuals who enjoy the advantages of university associations while freed from ordinary university routine. The final phase in the development of research was the outright institute for medical research established for the express purpose of expediting the increase of knowledge in the field of medicine.

In the past, as Mr. Donaldson points out, recently qualified men with a bent for research too often had to give up hope of developing it for lack of means of support during an apprenticeship. Later came the epoch when grants were obtainable from various sources, but their acceptance usually implied some obligation to collect observations or experimental results for publication. It is now realized that the time spent on immature effort of this kind can be better applied, and that allowance for an unproductive period may yield far better fruit in the long run.

With this view in mind the Government in 1920 set up under the jurisdiction of the Privy Council the Medical Research Council, through which young research workers are maintained in various laboratories and clinics throughout the United Kingdom. In most places where active investigation is going on the beneficent and intelligent co-operation of the Medical Research Council may be discovered. In addition to its distribution of subventions to research workers in universities, hospitals, and kindred institutions throughout Great Britain, the Council with part of its modest funds runs the National Institute for Medical Research at Hampstead. There are very few medical men who are familiar with the manifold activities of the M.R.C. or with the mechanism and general principles guiding its actions. These are set out at some length in the *Lancet* (Aug. 6, 1938), and anybody interested in medical research should make a point of becoming familiar with the workings of the M.R.C.

The financial side of medical research, however, is unsatisfactory. The annual grant from the State Treasury to the M.R.C. is now £215,000. In addition to its public funds, the Council are in a position to help directly or indirectly by advice in the disposal of private endowments for medical research. These they receive from various sources. At present the State provides but meagre financial support for medical research, and though the country is still conservatively inclined, British medicine is moving towards the larger conception of scientific medicine. Now is the time for the profession to organize and clamour for more money with which to conduct research work. A mere £215,000 is, of course, inadequate, and the Government can afford a much larger sum.—I am, etc.,

G. R. W. N. LUNTZ.

* * It should be added that it was in 1920 that the Medical Research Council received its present title and constitution. The move to provide public money for research was made in

1911, in Mr. Lloyd George's National Health Insurance Scheme. It was in 1913 that the Medical Research Committee (continued in 1920 as the M.R. Council) was established to administer the funds for medical research provided under the National Health Insurance Act.—Ed., *B.M.J.*

Babies in Glass Cages

SIR,—I was careful in my previous letter not to express a personal opinion regarding the use of glass cubicles, and merely recorded what I had been told in Vienna, where the idea originated. This was that the idea had been abandoned because it gave poor results owing to the children being lonely. It appears that Dr. Baar wants my opinion, so he shall have it. First, he states that the use of cubicles had been abandoned in Vienna because von Pirquet was dead. Surely this is a fantastic point of view. Has insulin been abandoned since Banting's death? Has the use of the arsenicals been given up since Ehrlich died? If ideas are good they do not die with their originator. Again, he states that "the majority of patients in babies' wards are not yet at a sociable age." My dictionary states that "sociable" means "disposed to associate with others." Are not babies disposed to associate with their mothers and others? Do not young mammals crowd together or press close to their mothers? This is so universal that it might be considered instinctual. Separation must be intolerable to a child—more so than to the adult. It might be said with truth that never again will the child be so sociable as it is in its babyhood. To place it in solitary confinement in a glass cubicle seems contrary to its instinctual reactions and is likely to produce harmful results.—I am, etc.,

London, W.1.

CLIFFORD ALLEN.

Medical Boarding for the Merchant Navy

SIR,—I have read with complete agreement and much satisfaction the letters on the above subject by Dr. H. M. Royds Jones (Dec. 25, 1943, p. 831) and Dr. E. L. Caldwell Smith (Jan. 15, p. 96).

My own experience of the Merchant Navy is of a mere three years' duration, but in that short time I have found that one rapidly gets to know the more permanent members of one's crew, and even though some are not A1 their departures from positive health are known and can be dealt with. Such men constitute no problem; but every new man may have a chronic duodenal ulcer or a progressive suppurative otitis media—to quote but two types of recent headache—and in existing circumstances, when a busy "signing-on" day permits of only cursory examination, the condition can and will be discovered only when the sufferer reports sick—after sailing.

Until I read Dr. Caldwell Smith's letter I was not aware that the "Pool" kept medical records of any sort, but in view of this fact I think that the procedure which I have recently adopted in dealing with the genuinely sick men may recommend itself to Dr. Royds Jones and many other ship surgeons who find themselves faced with the same problem.

My procedure is to notify the Pool medical officer by personal letter at the end of each voyage of the names and ratings of all men who in my opinion are unfit for sea—e.g., epileptics, peptic ulcers, etc.—and at the same time to refer them to their own doctors, telling the men of all the steps taken. In this way a man cannot complain of high-handed treatment by the ship surgeon, as he comes into the care of his own doctor; or, if he has no doctor, he can be referred directly to the hospital of his choice for investigation and treatment. Thus, in theory at least, the Pool knows the men who should not be permitted to sign on again without careful overhaul, and as a central organization should be able positively to prevent known unfit cases from going to sea. This scheme will not prevent unfit men getting to sea *once*, but should prevent recurrences. Without prejudice, I must add that I have never yet received any acknowledgment of my communications from the Pool; so whether they are at all appreciated I do not know.

One man referred in the above way some six months ago recently rejoined my ship a much fitter specimen altogether—he was clinically a duodenal ulcer—and he tells me that

he spent three weeks in hospital, but in confirmation or negation of my diagnosis I have had no word. I am prompted, therefore, to the following plea: that the same procedure be adopted in dealing with ship surgeons' as with G.P.s' cases, and a note of the diagnosis and treatment sent by the hospital or doctor concerned. As such patients may apply for a position in the ship at a later date the information is of great importance. I do not imply that such notes are never sent, but their absence is a noticeable feature. A record card which should accompany each man from ship to ship is a necessity, but in the meantime the suggestions made above may be of some help.

So much for ships carrying a surgeon. But what about the greater number that sail without? Would it be possible for the Pool to supply a questionnaire as to the health of the crew, which would be filled in at the end of each voyage by the surgeon if carried, or by the chief officer (the cargo-ship "surgeon") if not, in which leading questions as to men with symptoms of the commoner incapacitating complaints would provide answers whereby the "Pool" medical officer could investigate more thoroughly any suspicious cases?—I am, etc.,

A. E. K. SALVI,
Ship Surgeon.

Pavlov's Theories

SIR,—May I comment upon your two recent leading articles dealing with Pavlovian physiology and neurosis—namely, "Pavlovian Physiology and War Neurosis" (*Journal*, Aug. 14, 1943, p. 205) and "Behaviour and Neurosis" (Oct. 16, 1943, p. 487). These articles make the criticism that Pavlov's concepts of cortical function and their application to the problems of human behaviour and neurosis are vitiated by his failure to "think in terms of the meaning of stimuli," in "psychological" as well as "physiological" terms, and describes this failure as unscientific and biased.

If brain be the physical organ of mind, then either psychological and physiological phenomena are related precisely and interdependently or their relationship is non-existent, doubtful, or variable. Physiological experiment and deduction in the first case need take no special separate cognizance of the parallel phenomena of consciousness. In the second place, confusion of psychological concepts, definitions, and phenomena must surely be rigidly avoided. It would seem that it is the attitude of your leading articles and not that adopted by Pavlov which is "hardly in the spirit of science."

The statement that Pavlov refused to admit that the behaviour of experimental animals might be determined largely by physiological needs, aversions, conflicts, or other internal motivations is most misleading. He specifically noted that intrusion of strangers, hunger, satiety, desire for micturition, fear, neurosis, confinement, and external distraction profoundly modified his results, and not the least admirable feature of his work is the technique introduced to cope with these complications. Masserman's experiments are apparently made under less rigid conditions in which "the animal is not placed in complete isolation and isolated from every stimulus but the experimental one." The inference drawn that his experiments and conclusions are therefore more scientifically valid is difficult to follow.

It is stated in reference to the application of Pavlovian principles to social phenomena that "this *may* be good physiology but as a contribution to the all-round understanding of life this kind of wild analogy is a step backward into the materialistic abyss." If it is good physiology honest men must take that step; if not, then disproof will be on scientific grounds and not upon threats of an abyss or criticism of Pavlov's philosophy of life. Surely at this stage of scientific thought the fear that spiritual or psychological realities may be destroyed by demonstration of physical and chemical mechanisms for vital phenomena can be dismissed as mere vulgar error. One may deny in Pavlov's work the accuracy of his experiments, his scientific probity, the correctness of his generalizations, or their applicability to human affairs. One should, however, demonstrate grounds for so doing. To state that his speculations as to the cortical processes of Christian martyrs reach "the zenith of absurdity" without demonstration of the absurdity of fact or logic involved is simply to