assessments and examinations. Some astonishing facts came to light. Training methods and courses are very often indeed a hodge-podge of different devices. There may have been a good reason for every one of them when it was introduced, but they have come in at all sorts of different times and for all sorts of different reasons and purposes. Training schemes are far more often overloaded than under-loaded, and it is very common to find candidates kept for a long time learning things that the general run of them can perfectly well master in a short time, and for a short time on things which require more prolonged study. The result is staleness in some directions, under-training in others, and that most curious state of affairs in which it is accepted that a candidate's training must almost begin all over again when he goes from the school to his eventual career. In this country the only case I know of a thoroughly validated selection procedure from first to last was one in which selection and training were treated as a single problem. I can see no hope at all for a really successful issue to primary selection in the case of medicine unless medical authorities are prepared to submit existing training methods to a radical investigation and to take a very open-minded part in such investigation themselves.

5. It has become clear that follow-up technique and testing technique are not the same and in a serious investigation should almost certainly be the concerns of different groups. In general the more any type of investigation requires field study the more important it is that a leading part should be taken by people with interests, knowledge, and skill relevant to the field concerned. Whereas the kind of testing which is now under discussion is very largely a technical psychological problem, the follow-up is mainly a matter which is best directed by medical personnel. Fortunately there are now an increasing number of the latter who have also a satisfactory training in normal and experimental psychology. It is this kind of training which is required. Doubtless there are also psychiatric problems involved in the initial selection of medical students, but on the whole, though there is no complete agreement, the weight of recent experience is heavily against mixing up the psychiatric with the normal problems of selection.

6. Tests of intelligence in a general sense, as distinguished from those which have to do with highly specialized skills whether mental or bodily, must employ a variety of principles and a variety of media. The media commonly used are words, numbers, and visual—usually geometrical—forms. All three are required. The principles must include both structural and relational factors, and these have to be treated both analytically and constructively or synthetically. No set of tests which are merely repetitions of the same pattern of problem presented in varying complexity can be regarded as satisfactory.

7. There is impressive evidence, not as yet given much publicity, that the type of intelligence which deals efficiently with the symbols of words, numbers, and visual forms has no necessary relation to another type which deals directly with complex situations in terms of the immediate data of those situations. It seems highly probable that the latter type of intelligence may be very important for certain aspects of the practice of medicine, especially in regard to clinical skill. Any long-term investigation of the value of intelligence tests in initial medical selection should give due regard to this.

These are, I think, the main principles that have emerged from the work of the last few years. There are in addition a great many details which need attention and trial. It is not improbable that several of the conventions which, for very good reasons, are usually observed in the design and presentation of intelligence tests may undergo change. Some of these, for example, are the arrangement of test items in a regularly increasing order of difficulty, the use of one-way questions and answers only, and current practices about time and speed setting. These and other matters, including some of the statistical measures commonly demanded, should not be taken as settled finally.

Further, Prof. Smyth is certainly incautious in one respect. The assertion that coaching and practice make no difference to test verdicts runs a long way beyond established facts. But nobody yet knows for certain what exactly are the implications which the undoubted results of training and practice may have for intelligence test scores.

The practical conclusion seems to me to be this: the time is ripe for setting up a small but influential body of medical doctors and of psychologists with the necessary resources to implement any decisions they may make after discussion. This body should aim at a long-term policy of investigation and also at a short-term policy for immediate, but provisional, application. The latter ought not to be at all impossible provided the probable ratio of candidates to places in those medical schools which would take part in it is known.—I am, etc.,

The Psychological Laboratory, Cambridge.

F. C. BARTLETT.

Classification of Psychological Disorders

SIR,—Instead of criticizing each other on the problems of nomenclature would it not be better if we frankly admitted that it is not possible to pigeon-hole every patient neatly under some simple diagnostic label? This is not the fault of psychiatrists but is due to the complexity of human nature, which exhibits infinite variations. The unique individuality cannot be stretched so as to align accurately upon one of a limited number of Procrustean patterns. By all means let us teach students the distinguishing features of "typical reactions"—depressive, hysteric, anxiety, and so forth—but let us also insist that in a considerable number of cases we have to deal with "mixed reactions," because personalities are mixtures of many artificially separated types.

With the neurotic it is far more important to obtain a complete record of his biographical development than to label a small cross-section of his life. A psychiatrist who is able to say, for example, that the patient is reacting to frustration of occupational ambitions and a state of marital disharmony in accordance with certain behaviour patterns conditioned by various childhood and adolescent experiences is being more practical, and will be able to do something far more useful for his sufferer, than any pundit who is happy only when he can exhibit a clear-cut diagnosis. It is unfortunate, therefore, that some of our over-enthusiastic E.C.T. exponents seem to consider it more important to attach hastily a diagnostic tag to their patients than to make a painstaking study of life histories; hence the statistical fallacies concerning which Dr. Dillon very rightly issues a timely warning (Oct. 19, p. 588).—I am, etc.,

Basingstoke.

I. ATKIN.

Goitre without Bias

SIR,—Few there are, I think, who would deny that surgery as performed by an expert is the method of choice in the treatment of thyrotoxicosis. Nevertheless there are some features which call for discussion. In the first place I am sure that the average doctor would require a lot of convincing if he were told that all the mild adolescent colloid goitres he sees should be submitted to surgery. He would look back across the years and ask himself how many of these goitres have really become toxic, and how few indeed have become malignant. In support of this idea Garlock¹ states that about 2% of thyroid enlargements are malignant, and he informs us that Smith, Pool, and Olcott² emphasize that it may be that undue weight is being attached to the probability of cancer developing in an adenoma. He remarks trenchantly:

"As one reviews the statistics of three or four decades ago, when goitre operations were unusual and patients harbouring adenomata did not accept surgical intervention as freely as to-day, one is impressed by the fact that few cancers of the thyroid were noted, far fewer than would be expected did adenomata present the marked tendency towards malignancy which is generally assumed to-day."

Turning back the pages, we find that in Allbutt and Rolleston's *System of Medicine* it is stated that 50% of patients recovered completely on rest and sedatives, while Osler³ informs us that "Statistics are misleading, as only the severe cases come under hospital treatment. Probably 65% of the patients make a good recovery, and 10% to 12% die." It is well perhaps to remind ourselves that a majority of patients did recover by various methods of treatment, while admitting that these results did not compare with good modern surgery.

With regard to thiouracil, and the new methyl and propyl derivatives which are much less toxic than the former, it is surely too early to condemn them out of hand. It is true to say that many patients have shown complete remissions, which have lasted so far under the administration of these compounds combined with rest in bed for a few weeks at the outset. Admittedly it does require a lot of experience to determine which patients may be suitable for medical measures, and above all when to advise operation so as to give the surgeon a fair chance. Much could be said did space permit on the question

¹ Nelson's *Loose Leaf System of Medicine*.

² Smith, L. W., Pool, E. H., and Olcott, C. T., "Malignant Disease of Thyroid Gland: a Clinico-pathological Analysis of 54 Cases of Thyroid Malignancy." *Amer. J. Cancer*, 1934.

³ *Text Book of Medicine*, Osler and McCrae.

of diagnosis, but the all important thing is to bear the condition in mind, especially where there is undue nervousness with restlessness, unexplained wasting, or diarrhoea, or the presence in mid-life of tachycardia or an abnormal rhythm. The difference in the response to iodine and methyl-thiouracil on the one hand and sedatives on the other may be strikingly suggestive; and lastly in the few as yet undetermined cases we should remember Osler's dictum, "Diagnosis in the borderline cases depends more on careful study and observation than on any single test."—I am, etc.,

Clifton, Bristol.

FREDERICK SUTTON.

Carbon Dioxide Absorption

SIR,—Mr. A. Charles King (Oct. 12, p. 536) gives an interesting historical survey of anaesthetic apparatus, in the course of which he states: "In 1932 the first apparatus proper in this country was made for Frankis Evans, and this was followed by alternatives designed by Shipway, Harris, Halton, Primrose, Gillies, and others."

On reading this, I referred to my own records on this matter and find that I have a patent specification applied for on Oct. 16, 1931, No. 391,176, covering circuit breathing combined with an inflatable pharyngeal tube. A further patent specification, No. 419,679, dealing with to-and-fro breathing was applied for on June 30, 1933. Messrs. Barr and Stroud made my third machine, which was ordered from them on April 8 and delivered on May 20, 1932. The fifth machine of my series, home-made, was described at a meeting of the Anaesthetic Section of the Royal Society of Medicine, I think in the spring of 1934, while a subsequent machine was that used at the Annual Meeting of the British Medical Association held at Bournemouth in 1934.

I offer these facts as supplementing what Mr. King has given from his records, and should it turn out on investigation that a prior claim has been established through these facts, then the Glasgow Royal Infirmary must be regarded as the home of carbon dioxide absorption technique as at present practised.—I am, etc.,

Glasgow Royal Infirmary.

W. B. PRIMROSE.

Recent Advances in Anaesthesia

SIR,—In Dr. Langton Hewer's most interesting article (Oct. 12, p. 531), I note that he dates recent advances as those occurring between 1914–18 and the present time. He is thus able to do full justice to the advances in premedication, to the new volatile anaesthetics, to technique and apparatus, and even to the most recent adjunct to anaesthesia, curare. It is therefore all the more surprising that no reference whatever is made to intravenous anaesthesia, which has almost supplanted the older methods in certain types of operation—for this great advance was made within the period specified.

Jarman and Abel (*Lancet*, 1933, 2, 18) appear to have been the first in this country to describe the use of evipan; Dickson Wright (*Lancet*, 1935, 1, 1,040) drew attention to his apparatus for giving intravenous anaesthesia; and finally Jarman and Abel (*Lancet*, 1936, 1, 422 and 600) give an account of the action of pentothal sodium in over one thousand cases. I feel that to complete your account of recent advances in anaesthesia these facts should be added.—I am, etc.,

London, W.1.

J. JOHNSTON ABRAHAM.

John Snow—Anaesthetist and Epidemiologist

SIR,—In praise of Snow's perspicacity your contributor (Oct. 12, p. 535) writes, "Where Snow was in advance even of Farr and Simon was in his firm conviction that the ingestion of water polluted with a presumably living contagium was the only method of epidemic dissemination." I fear that the emphasis given by the italics may tend to deprive Snow of credit for that other remarkable and original, but less well known, inductive inference relating to the epidemiology of cholera which H. Harold Scott in *Some Notable Epidemics* records as follows:

"Yet another shrewd observation of Dr. Snow. A friend told him that, having placed in his room in summer when flies were abundant an infusion of quassia to kill them, he could often taste the bitter quassia on his bread-and-butter. If then flies could convey

quassia from a saucer to food they could as readily transfer fragments of cholera dejecta present on linen. He cited this as another possible mode of contracting infection."

—I am, etc.,

London, S.W.5.

IAN E. MCCrackEN.

SIR,—“It may not be generally realized how profound were many of Snow's views” writes Mr. Charles King (Oct. 12, p. 536), and the otherwise excellent *Anaesthesia* Number of the *Journal* does little to remedy this. In the column written on John Snow, only half a paragraph refers to his work in anaesthesia. Of the pioneers, none contributed so much, in observation or theory—the first to record the sign of intercostal paralysis and the first to postulate interference with oxygenation as the cause of narcotic action; in practice—the expiratory valve, wide-bore tubing, warmed ether bath, and “safety” chloroform inhaler; and in experimental work—forty-two pharmacological animal experiments, using, besides ether and chloroform, ethylene, amylene, refrigeration, and even endotracheal intubation. Again quoting Mr. King: “The idea of the carbon dioxide technique had entered the fertile brain of John Snow.” Yet he died in 1858, only twelve years after Morton's demonstration.

By reprinting *Snow on Cholera* America has made sure that his fame as an epidemiologist will live. In this centenary year, could not a British house be persuaded to republish the all too rarely seen *On Chloroform and Other Anaesthetics*?

—I am, etc.,

London, S.W.16.

JOHN IVES.

Penicillin Injections and the Boiling of Syringes

SIR,—One is alarmed at the letter by Dr. P. Finch (Oct. 19, p. 589) who, because he is informed that ethyl alcohol does not inactivate penicillin, states that he is employing industrial spirit for syringe sterilization as boiling wastes a lot of time. One of the lessons that should have been learnt from World War II is that syringes cannot be sterilized adequately by fluid disinfectants and that when such methods are used syringe-transmitted hepatitis becomes rife, especially when intravenous injections are being employed. By early 1944 the jaundice rate in syphilis patients attending Service venereal disease clinics had reached a very high percentage and was only controlled when the boiling of syringes was made compulsory. The incidence of syringe-transmitted jaundice is much lower after intramuscular injections, but cases following penicillin therapy are far from unknown and will become more frequent if the precaution of boiling is in any way relaxed.—I am, etc.,

A.M.D. 10, War Office, S.W.1.

R. R. WILLCOX.

Potency of Penicillin

SIR,—Penicillin has now become a commonplace and is used by thousands of doctors. It is admittedly the greatest advance in treatment of many bacterial infections that formerly were regarded as almost hopeless, and in lesser degrees of infection markedly shortens the period of recovery and convalescence. But there is one snag. There is no test that the ordinary practitioner can use to determine whether any given sample of penicillin has or has not lost its potency. We are told that under certain conditions of temperature watery solutions will remain active for 3–4 days and that oily solutions will last a week. But this is not definite enough and I am convinced that much inactive penicillin is being both injected and inhaled, thus often causing great disappointment. I hope some simple test of efficacy will soon be discovered and described.—I am, etc.,

London, S.W.1.

DESMOND MACMANUS.

Transmesenteric Hernia

SIR,—Dr. R. B. Leech's letter (Sept. 7, p. 339) on the subject of mesenteric hernia adds yet another example to the ever-increasing list of reported cases. With the cases of Peter Martin (*British Medical Journal*, 1946, 1, 238) and James Moroney (*Ibid.*, 329), the views I expressed on the aetiology (*Brit. J. Surg.*, 1944, 31, 275) are confirmed in that all three patients showed a defect of the mesentery near the caecum,