As to the value of the test, that depends upon its efficacy as a guide to treatment, and this evaluation the author has not attempted, despite the title of his article. His has been an academic exercise. Had he proceeded to test the practical issue as others have done, along lines which I have described elsewhere (*N.Y. St. J. Med.*, 1944, **44**, 489; *Arch. Otolaryng., Chicago*, 1949, **49**, 151; ibid., 1949, **50**, 564; ibid., 1950, **51**, 149), I venture to think that he might have finished among those who, coming to scoff, remained to pray.—I am, etc.,

New York MILES ATKINSON.

Mr. Bevan

SIR,—May I be allowed to comment on your leading article (April 5, p. 749) on "Bevan's Book"? Having read Mr. Bevan's book I can only regard your leader as being an undignified, misleading, and unjust personal attack on Mr. Bevan, unworthy of publication in a professional and scientific journal such as the *British Medical Journal*.

It is unworthy in its failure to comment objectively and without bias on Mr. Bevan's chapter on the National Health Service. It is misleading in its quotation of extracts out of their context; for example, your extract about "the innumerable harpies who battened on the sick" quoted in support of your accusation that Mr. Bevan besmirches those who looked after the sick before 1948. When read in its context this clearly refers to the quacks and purveyors of patent medicines and cures and not, as you represent, to the medical profession. It is unjust in its failure to draw attention to Mr. Bevan's account of his negotiations with the B.M.A., to his statement, for example, that he "suggested a graduated system of capitation payments which would be highest in the medium ranges and lower in the higher." It is undignified in its whole tenor and tone and above all in its terminal childish attempt to prove Mr. Bevan's political adolescence.

The N.H.S. patients and doctors have too long been bedevilled by the incompatibility of the B.M.A. and Mr. Bevan, and articles like this can surely do nothing but harm. I am sure I am voicing the sentiments of a great many younger practitioners when I say that, though deeply conscious of our indebtedness to the efforts of the General Medical Services Committee of the B.M.A. for their successful and prolonged efforts on our behalf, we have the uneasy feeling that wiser direction of the early stages of our entrance into the N.H.S. would have avoided much of the difficulty and delay.

In conclusion, may I say I am not a supporter of either Mr. Bevan or the Labour Party. I am local secretary of the B.M.A., but articles of the spirit and quality of this leading article almost shame me into resignation.—I am, etc.,

Cornwall ERIC TOWNSEND.

Rh in Practice

SIR,—In your issue of March 22 (p. 667) you drew attention to the recent publication of the Medical Research Council's Memorandum No. 27, *The Rh Blood Groups and their Clinical Effects*, which is a revision of the earlier Memorandum No. 19. You described the changes as very minor and as consisting chiefly of bringing the memorandum up to date with recent references. I feel that by this statement you may inadvertently have misled some of your readers. It is true that much of the text, particularly that of the section on Rh groups, is unchanged, but important alterations have been made in the sections on clinical considerations and on Rh testing.

In the clinical section we should like to draw special attention to two main changes, the results of recent controlled trials. Premature induction of labour is stated to be inadvisable as a routine treatment for women whose serum contains anti-Rh, and exchange rather than simple transfusion is now recommended without reservation as the treatment of choice for affected infants.

In the section on Rh testing numerous changes have been made as the result of the experience of the past four or five years. The very important pages on direct matching tests have been rewritten, as have most of those on the trypsin test and on the preparation of anti-human-globulin serum.

Although these and other changes affect only a small number of pages, I think you will agree that they are of considerable practical importance.—I am, etc.,

Medical Research Council, S.W.1. SHEILA SMITH, Publications Officer.

Explosion During Anaesthetic

SIR,—I have read with interest the account in your Medico-Legal column (January 19, p. 168) of an explosion during anaesthesia.

Your correspondent states that the agents used in this case were "flaxedil," thiopentone, and oxygen. I feel that the occurrence of an explosion, as opposed to a fire, under these circumstances requires some explanation, as none of the agents used is explosive. Oxygen supports combustion but will not explode. One is therefore led to surmise that some explosive agent such as ether or cyclopropane must have found entry into the machine and that the resulting mixture was ignited by a static spark. I have personal knowledge of one such explosion which could be explained only by the fact that a trace of explosive gas had remained in the anaesthesia machine from a previous case. There may be pockets of cyclopropane or of ether vapour lying within the corrugations of the rubber hose. To my mind this is the only explanation of this occurrence—namely, that some traces of an explosive agent were still somewhere in the anaesthetic machine from a previous case.

Two points of interest emerge from the report of this mishap. First, one would be well advised when using an anaesthetic machine for supplementation with oxygen to "wash out" the tubing and breathing bag of the machine by means of a large flow of oxygen before applying the mask to the patient's face; or else to use a separate oxygen unit for this purpose. Secondly, it probably is never advisable to proceed with the operation after an explosion has occurred, however apparently satisfactory the patient's condition may be at the moment.—I am, etc.,

University of Illinois, Chicago. GORDON M. WYANT.

POINTS FROM LETTERS

Hyperpiesis and Hyperpiesia

Dr. D. M. CAMERON (Bristol) writes: At the risk of being called pedantic, may I plead for a return to hyperpiesia for sustained high blood pressure and hyperpiesis for a raised blood pressure? If I remember right, the original work on blood pressure was carried out by Sir Clifford Allbutt. He gave us these names. They satisfied us for many years, and I cannot understand why in the last five years or so the hybrid word has displaced the original, and now finds favour even in editorials. Since we owe to Sir Clifford the clinical thermometer we daily use, why discard the name he gave the disease? Or if the moderns find the extra syllables tiresome, and every little sign or syndrome seems to be appropriated nowadays, might we call it Allbutt's disease?

Sulphamerazine Still Dangerous

Dr. R. S. SAXTON (Brighton) writes: I am glad to see that the campaign against sulphamerazine has been revived. In my opinion the use of this drug, for which there are many adequate substitutes, should be prohibited. . . . A nurse had tonsillitis, and received only six tablets (3 g.) of sulphamerazine in 12 hours, during which she had had an alkaline mixture and a high fluid intake under the watchful eye of the home sister. The patient developed complete anuria, which was subsequently relieved by ureteric catheterization. . . . Another patient had been given the recommended doses of "cremomerazine" as a prophylactic against complications of measles, which she had. She developed anuria.

Correction

In the paper, "Daraprim (B.W. 50–63)—a new Antimalarial," by L. G. Goodwin (April 5, p. 732), the heading of Table II states the dosage to be "twice daily." This should, of course, read "twice *weekly*."