simply as Rh,rh, a name which clearly indicates positive reactions with sera anti-rh', anti-Rh₀, and anti-hr', and negative reactions with anti-rh''.

12. No simple unified tables regarding the types and their heredity involving the other proposed systems of notations are available for use.

13. The C-D-E and other proposed notations are not mentioned in most standard laboratory texts, and are therefore unintelligible to the average reader.

—I am, etc.,

New York.

A. S. WIENER.

REFERENCES

- ¹ Wiener, A. S., "Theory and Nomenclature of the Rh Types, Subtypes, and Genotypes," *British Medical Journal*, 1946, 1, 982.
- ² — "Rh System in the Chimpanzee," *Science*, 1946, 104, 578.
- ³ — "Nomenclature of the Rh Factors," *Lancet*, 1948, 1, 343.
- ⁴ — *Blood Groups and Transfusion*, 3rd edition, 1943. C. C. Thomas, Springfield, Ill.

Sceptis Scientifica

SIR,—In his interesting address Prof. Geoffrey Jefferson (Feb. 28, p. 379) asks, "Could anything be learned from the structure and workings of the brain?" From the context this seems to refer to the relations of the brain to mind. He answers the question by saying that "to this old question we have no modern answer."

The *Proceedings of the Royal Society of Edinburgh*, Section B (Biology), 1943-4, Vol. 62, Part I, p. 86, gives at least one "modern answer."—I am, etc.,

Bristol.

R. J. A. BERRY.

** The reference is to an address entitled "Some Recent Advances in the Study of the Brain as the Implement of the Mind" which was read by Prof. Berry to the Royal Society of Edinburgh on June 5, 1944.—Ed., *B.M.J.*

SIR,—I read Prof. Geoffrey Jefferson's article (Feb. 28, p. 379) with such keen appreciation and enjoyment that it seems ungrateful to suggest that the issue might have been made clearer in the final paragraphs. The use of words as an economy of thought, to which the human race owes so much, is sometimes more confusing than helpful. Prof. Jefferson writes, "There are other large fields of knowledge besides Science." The meaning of these words as they stand alone is not very clear; but the important underlying meaning, obvious from the context, is that scientific technique is suitable for the investigation of some but not all of our total experiences.

The confusion arising from the use of the term "Science," at one time in its literal sense of knowledge, and at another in its acquired sense of a definite technique (deduction combined with induction, with special emphasis on the appeal to experience), has established a sort of vicious circle. Science in its meaning of scientific technique has had dramatic triumphs in the investigation of certain fields of our total experience, such as chemistry and physics, and has given us valuable knowledge of these subjects. These great successes naturally raised hopes that Science would be equally efficient in supplying knowledge in all other fields. But if we employ a word which means by derivation "Knowledge," not only for the knowledge obtained by a special technique but also for the technique itself, it is an easy step by equating all knowledge with the special technique to prejudice the whole issue and to obscure the truth, emphasized by Prof. Jefferson, that scientific technique as a means of winning knowledge is not infallible nor even suitable in all fields of our total experience.

This belief in the total infallibility of scientific method has become firmly established in the popular mind, largely owing to the dual meaning referred to above which has become attached to the term "Science," and it is this misconception which is threatening our civilization with disaster to-day.

As scientific technique invades the higher fields of biology and psychology it is compelled willy nilly, in order to maintain its output of success, to appropriate more and more to its use those techniques such as intuition and introspection which are suitable to these higher levels, and we are sometimes surprised to realize that these higher techniques have now been quietly incorporated in the scientific method. Unfortunately the highest techniques of all, meditation and love, have not yet been officially adopted, although to the writer it would seem just as much as error of technique to attack the problems of, for instance, social psychology by the scientific method, unaided by meditation and love, as to endeavour to analyse or weigh a chemical substance by meditation and love unaided by the scientific method.

Of course there is not the slightest objection to combining all techniques, including meditation and love, under the heading of "Scientific Technique" if we wish; but we might then just as well call this all-embracing technique "Religion." This brings us back to our old friend and enemy, the use of words.—I am, etc.,

London, S.E.21.

M. H. K. KANE.

Temperature Records and Ovarian Activity

SIR,—The letter by Dr. P. M. F. Bishop (April 10, p. 706) reflects certain common misconceptions concerning the significance of temperature records in the study of infecundity. May I attempt to clarify the position within the space available lest a valuable clinical method be employed without due regard for either its potentialities or its limitations? Dr. Bishop asks four questions.

Question No. 1: Does the basal temperature pattern denote whether ovulation has taken place?—The occurrence of a diphasic temperature cycle indicates that a phase of oestrogenic secretion has been followed by a phase of progesterone secretion. The rise of temperature caused by progesterone is usually, but not invariably, associated with, or follows upon, ovulation. But progesterone may be secreted without actual liberation of an ovum (e.g., in the case of retained ova or of luteal cysts), so that the rise is no absolute proof of ovulation. Conversely, ovulation may occur without luteinization, so that the absence of the typical rise in temperature may not prove the absence of ovulation. The condition of the endometrium also depends on ovarian secretion, not on ovulation itself. But the association between ovulation and a diphasic temperature curve is so high that in practice it has been used by a number of independent workers for timing of intercourse or of artificial insemination.

Question No. 2: Can it indicate exactly when ovulation takes place?—Experience with artificial insemination and with laparotomies timed in relation to the temperature cycle (cf. Greulich, W. W., Morris, E. S., and Black, M. E., *Proc. of the Conference on Problems of Human Fertility*, 1943, p. 37) show that ovulation, which of course is not necessarily an instantaneous process, tends to coincide with the thermal shift from the low to the high level. The accuracy of timing depends among other factors on the rapidity of the rise and is, therefore, less reliable in cases where the shift is of the gradual ("staircase") type.

Question No. 3: What information does it provide as to the degree of ovarian activity?—Injection of natural oestrogens tends to lower the level of the temperature. In women with anovular cycles and a high level of oestrogenic secretion (e.g., in certain cases of glandular-cystic hyperplasia or ovarian cysts) the waking temperature remains permanently at or below the level characteristic for the follicular phase; injections of progesterone raise the waking temperature to the level normally prevailing during the luteal phase according to the dose given. Again, during the early phase of pregnancy when the progesterone level is high the temperature is also high. When the progesterone level falls during pregnancy (e.g., in imminent abortion, or during the fourth month of gestation) the temperature drops, but rises again in response to progesterone injections. These are but some of the facts which indicate that the actual level of the waking temperature, as well as its relative variations, reflects quantitative variations, and not only the phases, of ovarian activity.

Question No. 4: Is it likely to give more accurate information if the temperature is recorded rectally than if it is taken by mouth?—The oral temperature is apt to give occasional deceptively low readings, due to extraneous factors such as mouth-breathing, to which the rectal reading is not susceptible. This means that in any individual case the oral temperature may show a misleading value, so that the subject might miss a critical date, if I may use this expression.

Dr. Bishop also contrasts the 500 records collected at the Fertility and Endocrine Clinic of the Chelsea Hospital for Women during the past three years with the large number to which I referred. Perhaps there is some confusion between number of women observed and number of cycles recorded. It has been my practice to ask my patients to record the waking temperature for not less than three months, and quite generally until they have reached the fourth month of pregnancy or their case has been discontinued. In one case illustrated in the original communication (*Lancet*, 1945, 2, 663, Fig. 6) 26 successive cycle records from one patient were reproduced, and more than 1,500 records gathered from private practice were consulted in developing the argument of that communication. Of course since then the work has been much extended by myself and others. It is a fact long recognized that successive menstrual cycles differ from each other with respect to occurrence of ovulation, time of ovulation, duration of luteal phase, etc., and it is quite insufficient in the treatment of infecundity to record only one cycle or occasional

cycles. Continuous records are part of the proper background for the diagnosis and treatment of infecundity, also for the supervision of pregnancy in its early stages.

Concerning the statistical evaluation of the extensive data available some results have already been published (see Fig. 2 in the paper quoted; also Figs. 100 A-D inclusive, in Siegler, S. L., *Fertility in Women*, London, 1944; also Palmer, A., 1942, *Surg. Gynec. Obstet.*, 75, 768). These show mean *rectal* readings on suitable grids. Further statistical evaluation of data will be useful in some respects, but Dr. Bishop overrates the function of statistics if he thinks that the statistical evaluation of temperature readings taken by themselves can solve such questions as to whether ovulation is denoted by any given pattern. They can only be used for this purpose when related to some different and direct indicator of ovulation such as findings at laparotomy.

I would add that my experience is not "unique," as can be readily ascertained by anyone who cares to avail himself of the extensive literature on the subject which has come into being in the last ten years.—I am, etc.,

London, W.1.

MARY BARTON.

Identification of Medical Documents

SIR.—It appears to me that Prof. Lancelot Hogben and others (April 3, p. 632) are trying to make a mountain out of a mole-hill. We are, presumably, on the eve of a National Health Service. If code numbers can be supplied to death certificates, why not to birth? These could be used for the medical histories.

I have been told that Records Officers have experienced difficulty in obtaining National Registration Card numbers, but after 30 years' experience of giving identification numbers to my patients I am convinced that where the Records Officer may fail the doctor never will if he is worth his salt.—I am, etc.,

Halifax.

A. GARVIE.

Treatment of Phlebothrombosis

SIR.—Mr. Hamilton Bailey's article (March 27, p. 594) is both timely and welcome, and it should serve as a stimulus to the earlier detection of phlebothrombosis. It would be a pity, however, if the impression is gained that all such cases should be treated by interruption of the vein.

Since 1944 I have been on the look-out for this condition and have performed the operation in a number of cases. I have been impressed at the number of times the condition can be spotted if one remembers to look at the legs after the fourth post-operative day. My own feeling is that the large majority of cases are best treated with anticoagulants. I have tried to define, for my own benefit, the indications for operation as against anticoagulative therapy, and I have come to regard them as follows:

1. Where anticoagulants are not available—this was so in three battle casualties.
2. Where haemorrhage follows the use of anticoagulants. In a recent case of spreading thrombosis and pulmonary infarction on the 7th day following prostatectomy, profuse haemorrhage occurred from the prostatic bed when heparin was given.
3. Where anticoagulant treatment would have to be maintained over many months and active leg movements are prohibited, as is the case where phlebothrombosis develops during heart disease.
4. Where there is recurrent phlebothrombosis—e.g., in 1943 and 1944 repeated small emboli with pulmonary infarction occurred at intervals of a few months in a serving soldier. Each attack was severe enough to require hospitalization. Venous interruption has given him complete relief.
5. Where there is obvious arterial disease contraindicating anticoagulants. In one case I have seen cerebral haemorrhage follow their use.
6. Where extensive ulcerating lesions are present—e.g., ulcerative colitis from which haemorrhage is to be feared.

In the actual operative procedure I have made it a practice to expose the external iliac vein and not the femoral. There is some evidence that the collateral circulation is more efficient here than below Poupart's ligament.

I have found a glass tube of a diameter a little less than that of the external iliac at this site to be the most efficient extractor of clot. The tube must be angulated 135° at about 6 in. from the tip. A powerful suction apparatus is required, and the glass tube gently pushed up inside the vein, thus detaching the clot where it is adherent

to the intima. As the clot gradually becomes free it can be watched passing up inside the glass tube until the upper limit of attachment is reached, when it is suddenly sucked down the tube, often in one piece.

It is wise to have the patient half sitting-up during this stage of the operation, and as soon as the clot is free he should be instructed to hold his breath so that the blood flows back down the vein. Pain in the calves usually disappears within 24 hours of the operation.

I have never regretted carrying out this operation, though I have once regretted that I did not tie the opposite vein as well. At the same time I think it would be unwise if the vein was ligated in every case of deep thrombosis. In the majority of cases all that is needed is treatment with heparin and dicoumarol. The difficulty which arises is to determine the cases in which vein interruption is preferable.—I am, etc.,

Bristol, 3.

W. M. CAPPER.

Harrison's Grooves

SIR.—The article by Drs. J. Naish and H. R. E. Wallis (March 20, p. 541) on the significance of Harrison's grooves is an interesting work of research into the historical aspects of the problem. It is rather difficult, however, to agree with them in their explanation of the mode of occurrence of this clinical feature. If the ribs buckled along the line of separation of the "vertebro-sternal" from the "vertebro-chondral" ribs such an effect would be produced maximally in cases where the intercostal muscles are strongest and the negative intrathoracic pressure most evident. In complicated pneumonia there is very little alteration in the intrathoracic negative pressure; certainly there is no increase in the elastic recoil of the lungs in this disease. If bronchiectasis followed the pneumonia a varying amount of elastic tissue will have been lost, leaving the patient with less elastic tissue than previously. In any case if one is concerned chiefly with a disproportion between the negative pressure in the thorax and overacting chest muscles, one is still met with the fact that the diaphragm helps to fix the anterior portions of the lowest ribs, whose lateral portions tend to move outwards.

I agree with Dr. R. Good (April 10, p. 707) that an analysis of the problem should include a consideration of the question of position, though it is felt that he does not sufficiently stress the significance thereof. On the other hand it is difficult to agree that feeding is a matter of much importance. Feeding *per se* affects 100% of children; Harrison's grooves are not found in more than about 10%, and in any case they develop more commonly in children after illnesses in which anorexia, and therefore a diminished food intake, are clinical features. This of course does not rule out the part played by abdominal distension whether by aerophagy or by fermentation, in the intestines of a weakened infant.

However, I think more light will be thrown upon this clinical sign by considering in greater detail the incorrect posture in which many patients are nursed during several phases of their illnesses. The so-called "upright posture" frequently consists of flexion of the head and neck, of the upper part of the chest upon the lower, of the abdomen, of the knees, yet minimal flexion of the hips. It is enforced yet concealed by the tucking in of the bedclothes, which are rendered heaviest and tightest under the chin and over the shoulders in a patient who has partly slipped down—i.e., the custom of "tidying the bed" or making the patient "comfortable."

Where the best of nursing facilities exist—and certainly in the best of children's wards—such a posture is practically unknown. In many other children's hospitals, and in more than one adult ward, if understaffed, this "decubitus *en flexion*" has been encountered with somewhat more than slight frequency. It explains why *lumbar* rather than *sacral* pads of oedema sometimes develop in patients suffering from cardiac failure who are treated in bed in this manner.

An examination of conditions obtaining in the patients' homes will support this point of view. Especially in the case of the poorer ones—from which many of the patients observed by Drs. Naish and Wallis came—and particularly where the sick child has been nursed in its parents' bed because other facilities are even less suitable, the posture described above is the usual finding. It is perpetuated when the debilitated convalescent child or one suffering from repeated attacks of asthma or recurrent mild infections is made to rest in a high and large adult's arm-chair with only his head, shoulders, and sacrum in contact with any support.

The result—as Dr. Good pointed out, displaces anteriorly and outwards the soft lower ribs by the action of the tense upper abdominal contents. It diminishes respiratory excursion and mini-