of the minutiae beforehand. That is so of all the sciences, even of mathematics.

Finally, I did not say that Trotter was a greater man than Francis Bacon, who had in fact anticipated many of his views. The art of tight and lucid compression of thinking was unknown in Bacon's day. And, for me at least, his lovely patches of illuminating thought are imbedded in vast expanses of tedious and repetitive writing. It was Baudelaire who said in a letter to Fraisse on Feb. 2, 1860, that long poems were written by those who had not the talent to compose short ones. How true that is.—I am, etc.,

Manchester.

GEOFFREY JEFFERSON.

Rectal and Vaginal Temperatures

SIR,—Dr. Mary Barton (March 20, p. 567) advocates recording rectal as opposed to oral temperatures for the determination of ovulation and the assessment of ovarian activity. Half her patients prefer the rectal method to the oral because they are afraid of biting the thermometer in half if they put it in the mouth. As the result of our experience we have adopted the oral method because our patients find the rectal method distasteful and because we believe the vaginal method to be dangerous. (One of my patients had a portion of the thermometer surgically removed from the bladder, having been instructed to take her temperature per vaginam by the gynaecologist whom she consulted before coming to me.)

All who have studied the subject agree that the basal temperature pattern has considerable significance. What everyone now wants to know is, (1) Does the basal temperature pattern denote whether ovulation has taken place? (2) Can it indicate exactly when ovulation takes place? (3) What information does it provide as to the degree of ovarian activity? (4) Is it likely to give more accurate information if the temperature is recorded rectally than if it is taken by mouth?

1. Our experience at the Fertility and Endocrine Clinics of the Chelsea Hospital for Women has been that if a typical biphasic temperature pattern is obtained (i.e., temperatures recorded as being relatively lower in the pre-ovular than in the post-ovular phase) ovulation in that cycle may reasonably be assumed. In the absence of this typical biphasic pattern we would not feel justified in presuming that ovulation had not taken place without the evidence provided by an endometrial biopsy on the first day of the bleeding which terminated that particular cycle.

2. We would hesitate in the majority of cycles with a biphasic temperature pattern to fix the date of ovulation within 3 or 4 days of the "ovular dip" or subsequent rise in temperature.

3. We would hesitate to express an opinion on the degree of ovarian activity, other than in typical ovular cycles, on our analysis of the temperature records.

4. It is our impression that oral temperatures are just as likely to produce the typical ovular biphasic pattern as rectal temperature records.

In other words we believe that the only significant finding is the typical biphasic pattern which indicates that ovulation has taken place in that particular cycle, and that we obtain that record even though the temperature is taken by mouth.

Here we obviously disagree with Dr. Barton, who claims much more for basal temperature recording. How can our differences be settled? Clearly only by a careful analysis of a statistically adequate number of cycles, and an observation of a series of cycles in which simultaneous oral and rectal temperatures have been taken.

We have been recording basal temperatures fairly conscientiously in a number of our patients over a period of about three years. We probably have records of about 500 cycles. Dr. Barton, however, has "during the past five years been using temperature readings whenever possible in thousands of cycles" (March 20, p. 567), and has "collected temperature records in several hundred women during the past seven years" (Barton, M., and Wiesner, B. P., *Lancet*, 1945, 2, 663). If this is the case she is obviously in possession of data which should be capable of answering these four questions beyond statistical doubt. It is therefore unfortunate that "for reasons of space, neither complete presentation of data nor their statistical evaluation" could be attempted in her 1945 *Lancet* paper, and that her recent communication in the *Journal* was an item of correspondence necessarily short and dogmatic. Despite the fact that her *Lancet* article occupies 5½ pages—a very generous allowance of space in 1945—she does not give one single definite figure concerning the number of cases in the various groups she discusses.

She resorts instead to such statements as "in our series of observed pregnancies (numbering about 150, and including nearly 90 cases recorded for two months or longer after conception). . . ."

In view of the very important potential significance of temperature records in relation to ovulation and ovarian function I believe the time has come for Dr. Barton with her unique experience of this method to make a scientific contribution to the elucidation of this problem rather than contenting herself with dogmatic generalizations which, though interesting, can hardly claim the authority which she obviously should be in a position to command.—I am, etc.,

London, W.1.

P. M. F. BISHOP.

The Pain of Labour

SIR,—It has recently been stated in your correspondence columns and elsewhere that uterine contractions of the first stage of labour "should be painless." Whether or not this be true is perhaps a matter for philosophical and theological argument rather than a basis for obstetric practice. The solid fact remains that the great majority of women who have passed through this experience agree that it is attended by a varying amount of pain whose characteristics and distribution are more or less uniform.

Deep pain, whether arising from a viscus or other structure, is felt in more superficial structures of the same sclerotome. While the female genital tract, like other visceral structures, shows no evidence of segmentation, its nerve supply is necessarily derived from the segments through which it passes. The upper end of the tract, formed from the Müllerian ducts, begins in the embryo at the level of the eleventh thoracic to second lumbar segments, from which it receives its nerve supply. The vagina and perhaps the cervical part of the uterus are probably developed as an up-growth from the urogenital sinus, and are innervated from the pelvic nerves (S2–S4). It would appear that the intervening segmental nerves (L3–S1) take no part in supplying these organs, being devoted to the formation of the great plexuses to the lower limb.^{2,3} The sites at which labour pain is felt might be accurately predicted from these considerations. It occurs in the upper lumbar and lower sacral sclerotomes without radiation into the areas served by L4–S2. It is admitted that the uterus can be cut or burnt, the cervix cauterized, pierced, or grasped with vulsellum forceps, and no pain results, but similar insults can be equally painlessly inflicted upon other abdominal viscera from which spontaneous pain is known to arise.⁴ In each case only the "appropriate stimulus" induces pain. For the cervix such stimulus is stretching; for the uterine body it may be ischaemia.

Meigs⁵ found that even after resection of the superior hypogastric plexus pain can be produced in the lower sacral areas by cervical dilatation. It is during the last two-fifths of the first stage that sacral pain is most intense.⁶ During pregnancy, on abdominal palpation uterine contractions are frequently noted, but these are never sustained or intense. When labour has begun the pain is not noticed at the same time as contraction begins but follows 10–20 seconds later, reaches its acme, then rapidly fades a few seconds after the uterus has relaxed. The tortuous course of the intra-uterine vessels must render them particularly liable to obliteration by myometrial contraction. These observations, and considerations of the effects of muscle ischaemia elsewhere,⁴ suggest that this is the "appropriate stimulus."

Of all sensations pain is the most susceptible to modification.⁷ It is a perception subject to influence by cortical control, associated impulses and ideas, and the emotional state. Discriminating sensations normally exert an inhibiting effect on impulses subserving pain, so that unless these impulses are so strong as to dominate the entire sensory pattern, pain as such may not register in consciousness. The state of the sensorium at any particular moment is the product of all stimuli being experienced. Further, pain may be much modified or even abolished by suggestion. It is therefore apparent that the degree to which pain is perceived during labour may be modified by a number of variable factors—the emotional pattern, the degree of confidence reposed in medical attendants, and the amount of healthy interest taken in the physiological experience through which the mother is passing. This last may dominate the sensorium so completely that little pain is noted.

With the onset of the second stage of labour marked change is often apparent. Cervical distension is replaced by vaginal and perineal stretching as a cause of pain, but, the woman's attention now being directed to purposive activity, such pain may be felt

less severely. Clearly muscular tenseness from anxiety or any other cause will augment pain.*

From the foregoing it is contended that pain felt in labour is a physiological response to uterine ischaemia, cervical dilatation, and stretching of the passages. As in pain from other sources, perception may be modified by the emotional state, which is doubly important in that it may influence actual uterine functioning. While therefore certain obstetricians are able to conduct relatively painless childbirth without anaesthesia, this is a tribute to their enthusiasm, personality, skill, and zeal rather than a proof that childbirth is "normally" free from pain. The great majority of parturient women will continue to require some form of analgesia, local or general, and no vague philosophical concepts should be allowed to interfere with progress in this direction.—I am, etc.,

London, W.2.

J. DONALDSON CRAIG.

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Harrison's Grooves

SIR.—The article by Drs. J. Naish and H. R. E. Wallis (March 20, p. 541) dealing with the above subject and its possible aetiology prompts me to ask if the cause of the condition is primarily in the chest at all; if the chest condition may not be secondary to the abdominal distension from infantile hearty feeding; and if the repeated abdominal distension might not be alone sufficient to account for, on a purely mechanical basis, the ultimate production of the grooves.

The abdominal distension that can occur as a natural and normal phenomenon after a feed in a healthy baby has to be seen to be fully appreciated. The following points should be considered: the frequency of the feeds; their repetition several times daily for months; the fact that the feeds are most often given with the baby in a sitting or a semi-sitting position; that, at least in the early months and towards the completion of the feed and even without the baby being in the constrained position previously mentioned, the lower ribs can actually be seen splaying out as if to allow the upper abdomen to accommodate the increased abdominal contents with the production of temporary grooves (or the makings of them); and that, fed in the position mentioned, the breathing of the baby can sometimes be seen to become temporarily embarrassed even to the point of becoming noisy—the accessory respiratory muscles coming into play—and perhaps the face red.

I had the opportunity over a lengthy period some time ago of closely and repeatedly observing the feeding of a baby from two weeks after its birth. Its parents were of the professional classes and there was no question of any dietary, vitamin, or other deficiency. The health of the child was excellent. I repeatedly observed the phenomena already described without at the time appreciating their significance, and the Harrison's grooves became permanent. There was no ascertainable cause for the production of the grooves other than the repeated post-prandial distended stomach.

Also, as would appear in most cases, at least judging from what one was taught and what one can read in textbooks, the onset of the condition is so insidious that it escapes the eye of the fond mother, who consequently never brings it to the clinic for that reason alone. In the case cited the condition though relatively slight was present as a permanency before I was aware of the fact that it had actually become so.

The association of the condition in other children with chronic respiratory affections such as asthma and diminished lung expansion in post-pneumonic states could conceivably be explained on the same hypothesis—or at least it would act as a large contributing factor. For feeding to the baby, as well as having a hunger-satisfying aim, has also the very important psychological function of allaying any anxiety experienced by the child. A child will feed, if circumstances are favourable to feeding, not only because it feels hungry but also at times because it feels anxious—i.e., it will drink its food in certain (psychological) circumstances when there is no (physical) necessity for it doing so—and any chronic respiratory affection, such as asthma, does have much associated anxiety. Thus the extra feeding such a child will take, if given it, in an endeavour to allay the anxiety felt will also accentuate the abdominal distension with outward movement of the lower ribs and with upward pressure on the lungs which are physically abnormal.

To attempt to generalize from one case is very rash, and the foregoing hypothesis—the insidious development from a naturally occurring cause repeatedly acting in a mechanical manner over a lengthy period—is merely thrown out as a passing suggestion for a possible line of investigation to one with sufficient interest, time, mechanical knowledge, and x-ray facilities.

But the hypothesis, tentative though it is, seems to fit without undue stretching most of the facts—such as, to mention a few, its occurrence in children who are otherwise physically normal and who have enjoyed good previous health. Naish and Wallis's finding that 77% of their unilateral cases in the control group were left-sided, and the same authors' reference to Jenner's observation that the grooves are level with the upper surfaces of the liver, spleen, and stomach—and would seriously have to be reckoned with before being disposed of as invalid. Individual variations in different babies in the way of volume of amounts taken at each feed in proportion to the body volume, the frequency of the feeding, constitutional factors in the way of bony, cartilaginous, and relative muscle strengths and their interacting and relative preponderances, accidental catastrophic factors of lung affections (such as asthma), and infections (such as pneumonic and post-pneumonic) with diminished lung expansion—all these would, of course, also signify, especially since the normal biological capacities of the alimentary system are not primarily affected in these conditions.—I am, etc.,

Shotts, Lanarkshire.

R. GOOD.

Where Are We Going?

SIR.—There must be many of the older clinicians who have been thinking on the lines of Dr. Ffrangcon Roberts's excellent article (March 13, p. 485) and deploring the present state of medicine as he does. As with a disease—and it is a disease of modern medical practice—it is as well to consider the main aetiological causes.

To my mind there is a predisposing cause and an exciting cause. The former is the present method of medical education, whereby the more intelligent entry to our profession passes his school certificate at the age of 14, and hereafter devotes the rest of his education to science—an excellent training for a laboratory boy but a very poor one for clinical work, which, as we all know, is a very inexact science demanding close observation and deductive reasoning. The exciting cause is that most of the younger members of the hospital staffs have served as specialists in the Services. The medical branches of the Services are administered by a class many of whom got where they are by their extreme adroitness at "covering themselves." As Dr. Roberts points out, so many of these investigations ordered are not for the benefit of the patient but are used as a method of covering the clinician. So the administrators of the Services by orders and regulations have tied the hands of the specialists to such an extent that what clinical acumen they possessed has been wasted and allowed to die away.

If and when the State Medical Service starts, I cannot see the administrators altering their outlook, an important point so far overlooked.—I am, etc.,

Bristol.

RICHARD C. CLARKE.

Bone Conduction in Otosclerosis.

SIR.—In two recent numbers of the *Journal* letters have appeared regarding this subject, from which diametrically opposite inferences are intended to be drawn. In the issue of March 27 an increase is alluded to as a myth. To me it appears to be a mathematical necessity.

The sound waves which generate hearing would, in a medium such as that of the endolymph or perilymph, be between 10 and 200 ft. in length, and if they passed through the cochlea would be virtually in the same phase on both aspects of the basilar membrane and produce equal pressure upon them. The movements of fluid which are universally recognized as the cause of the sensation are therefore in response to, and are not actual, waves. Presumably the same movements are produced by bone conduction.

A second opening, to permit of the movement, was one of the earliest features of the ear to be recognized as essential. In