he is already doing more abortions than he would like and any increase would be unwelcome. But obstetrics perhaps more than most clinical subjects requires people who care deeply for their patients. We need, and can surely absorb, potential obstetricians of ability and integrity without rejecting those who do not happen to agree with current permissive trends.

Like Mr. Vartan I do terminate pregnancies and indeed I believe that, within the legal framework and after due consultation, the patient herself must ultimately make the decision. But to restrict obstetric training to those of one creed or code who will terminate unwanted pregnancies could be as damaging for the general public in the long run as coercing unwilling obstetricians to do what they believe to be wrong. The basis of medical ethics is to do what one believes to be right, and there must be room for all shades of opinion; a "closed shop" attitude in medicine is neither right nor healthy.

The long-term solution to the abortion problem is to encourage a greater sense of personal responsibility in preventing unwanted conception. The short-term solution is to increase the facilities for terminations within the Health Service performed by those of adequate training who believe on equally good ethical grounds that abortions should be more freely undertaken. The obvious corollary is greater provision of National Health Service family planning, including vasectomy and tubal ligation.—I am, etc.,

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SIR,—My old friend Mr. C. K. Vartan (26 June, p. 767) is a distinguished obstetrician. I can understand his irritation at being saddled with 600 abortions a year with 600 babies born at the same unit. He feels sincerely that the "suffering of a woman bearing an unwanted child" is best relieved by abortion. No doubt he offers modern alternatives—social and medical support—to have these rejected. No money changes hands. The doctor and his nurses hate the operation, but do it. Twice a day, year in, year out.

But the advice to keep out of obstetrics unless one accepts this kind of life is surely not to be taken seriously. No trainees should be asked or compelled to carry out an abortion for his chief, unless of course he agrees with him, so that a young man or woman need not be deterred. Obstetrics and gynaecology, care of the mother and her baby, is the most fascinating of specialties. I forecast that as in Rumania, Hungary, and Czechoslovakia "permissive" abortion will soon give place to abortion when the patient's life is at risk. Meantime, all a young trainee has to do if he wants to keep clear of the abortion epidemic is to choose his senior colleagues with care.—I am, etc.

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B.M.A. Policy on Smoking

SIR,—The Council of the B.M.A. are to be congratulated on making such a direct and unequivocal report on smoking (*Supplement*, 8 May, p. 91). It gives encouragement to many both inside and outside the profession who have waited for doctors to take a more forthright stand on smoking.

It is impressive that many patients will stop smoking with comparative ease provided they are given direct instruction to do so. I warmly support recommendation (15). "Doctors should never miss an opportunity to discourage smoking even when the clinical condition is one not at present known to be related to smoking".

Many hospital committees are concerned about the large amount of smoking in many hospital wards. I should like to see all hospital medical committees consider recommendation (18). "Cigarettes and tobacco should not be advertised or sold on hospital premises—for example, in hospital shops, canteens, trolleys, or slot machines".

I trust the Representative Body will give overwhelming support to this outstanding document, which is strongly supported by the Council and members of ASH.—I am, etc.,

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Confidentiality

SIR,—I refer to the meeting of the General Medical Services Committee (*Supplement*, 3 July, p. 13), in which Dr. D. L. Williams had said that it was "necessary for the Association to be seen to be giving thorough consideration to the problems of confidentiality and secrecy between patient and doctor."

Recently I wrote reports to two general practitioners who had sent men to V.D. clinics. In both cases the doctors disclosed my "confidential" information to the patients' wives. How can this leak be stopped?—I am, etc.,

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Fingerprint Changes in Coeliac Disease

SIR,—We were much interested in the fingerprint study by Dr. T. J. David and his colleagues (5 December 1970, p. 594) which suggested a high degree of correlation between ridge atrophy and the clinical state of patients with coeliac disease. Since their series included only two children with untreated coeliac disease, we have made a similar study of patients attending the gastrointestinal clinic at the Royal Hospital for Sick Children, Edinburgh.

Fig. 1

Fig. 2

FIG. 1—Normal print of infant with untreated coeliac disease.
FIG. 2—Treated coeliac showing normal print.

In every case the diagnosis of coeliac disease was supported by a jejunal biopsy showing subtotal villous atrophy. In all, 23 such patients were examined. Of these, six were new patients entirely untreated. Twelve were judged to be completely well following many months of careful dietary management. A further five continued to have evidence of active disease, clearly attributable to failure to adhere strictly to the prescribed gluten-free diet. In addition, fingerprints were also taken from 18 patients suffering from cystic fibrosis as well as 25 normal children and 50 mothers (all reportedly well) accompanying the children at the clinic.

Fig. 3

Fig. 4

FIG. 3—Cystic fibrosis patient. Print shows "white lines."
FIG. 4—Normal housewife showing "white lines."

The fingerprint examinations were carried out by the Identification Branch, Edinburgh City Police. The techniques in the main were those directed by the Home Office (1960).[1] For small infants, however, Bristol Black was used and impressions taken on white Sellotape. The prints were examined for ridge atrophy and white lines as described by Dr. David and colleagues. Prints of four subjects are illustrated in the Figures. The results are summarized in the Table.

Coeliac Patients	Number	Number with White Lines	Number with Ridge Atrophy
Untreated ..	6	0	0
Partially Treated	5	2	0
Treated and Well	12	3	0
Non-coeliac Subjects			
Normal Children	25	7	0
Mothers ..	50	32	0
Cystic Fibrosis Patients ..	18	3	0

Our findings therefore do not support the suggestion that a correlation exists between fingerprint patterns and the clinical state of children with coeliac disease. Certainly we cannot agree with Dr. David and his colleagues that abnormal fingerprints are acceptable as a diagnostic feature in new cases of coeliac disease or as a measure of response to a gluten-free diet. Examination of the material presented in their paper leads us to doubt the validity of their observations. The fingerprints reproduced in their article do not show convincing ridge atrophy. Ridges can be seen in every case. Such lack of definition as there is can probably be attributed to poor technique, since the appearance illustrated is that produced when too much ink is used or when exces-

sive sweat causes incomplete recording of the finger impression. Further, "white lines" are common in normal people, particularly in those following certain occupations—for example, bricklayers, cement workers and housewives. These lines quickly disappear during periods of interruption of the relevant employment. Improvements observed during periods of treatment probably relate to concomitant change in occupation rather than to the dietary management.—We are, etc.,

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1 Home Office, *Instructions in the Method of Taking Finger and Palm Prints*, London, H.M.S.O., 1960.

Halothane Hepatitis

SIR,—Though perhaps not surprised I was nevertheless a little disheartened to read the various letters (17 April, p. 166 and 29 May, p. 523) which followed the publication of our paper on halothane hepatitis (20 February, p. 448). For the same old arguments based on clinical impression and selected papers from the literature are again put forward. The findings of the National Halothane Study[1] based on a retrospective analysis of data from 34 centres have to be interpreted with considerable caution, as was stressed in the original publication, and on many occasions since by some members of the panel.

There can be no doubt about the existence of halothane hepatitis as a distinct entity. I will not repeat the various features summarized in our paper which distinguish it from viral hepatitis. They include immunological abnormalities as well as distinct differences in histological appearances on both light and electron microscopy. These have also been the subject of a recent and detailed appraisal by Professor Sheila Sherlock.[2] Even more damning evidence comes from the effects observed in patients, anaesthetists, and even a worker in a factory making halothane, when re-challenged with this agent. The differences between the histological appearances of viral hepatitis and of that due to halothane are also stressed in a recent paper from America[3] in which the authors refute the hypothesis that halothane hepatitis represents a coincidental viral hepatitis or aggravation of a viral infection by the anaesthetic. The appreciation of these histological changes will of course depend on the experience of the pathologist in this field. Indeed, this is well illustrated by the letter of Dr. J. D. Hill (17 April, p. 166) in which he questions the diagnosis of one of the cases included on our report partly on the basis of a histological report described as showing advanced biliary cirrhosis. But when this material was most carefully reviewed by us and by independent observers the appearances were clearly those of submassive hepatic necrosis with some areas of surviving tissue, entirely consistent with a halothane aetiology. Of course it may be difficult to prove the diagnosis in an individual patient for the relevant tests are often not done at the right time and many of the milder reactions probably pass unrecognized.

If all those clinicians, pathologists, and immunologists with special experience of liver disease are all agreed on the existence of halothane hepatitis, and I doubt if there is a single hepatologist throughout the world who is not, why do anaesthetists not accept it and then tackle the problem in a more constructive manner? Although the overall incidence of fatal hepatic necrosis following its use may indeed be low, two-thirds of the fatal cases have followed multiple exposures and it is these which could have been prevented. An acceptance of its existence together with more detailed observation and laboratory investigation of patients having multiple anaesthesia would allow diagnosis of the first and often milder reaction—a warning sign not to be ignored. Even if such a reaction cannot be proved beyond doubt surely it is only wise to ensure that these patients are not exposed again to halothane? Such a policy would in no way negate the setting-up of a properly and controlled prospective trial with adequate laboratory and other objective criteria. Initially the trial should be restricted to certain diagnostic groups such as those patients likely to need multiple anaesthesia, who are at the greatest risk.—I am, etc.,

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1 Committee on Anaesthesia, National Academy of Sciences and National Research Council, *Journal of the American Medical Association*, 1966, **197**, 775.
2 Sherlock, S., *Gut*, 1971, **12**, 324.
3 Uzunalimoglu, B., Yardley, J. H., and Boitnott, J. K., *American Journal of Pathology*, 1970, **61**, 457.

Sleep of Enuretics

SIR,—In his review on childhood enuresis (26 December, p. 787) Dr. R. Meadow unfortunately perpetuated some erroneous observations when he stated, "E.E.G. studies have shown that wetting usually occurs when sleep is light or the child is awake." These views arose from the reports of Ditman[1] and Bental.[2] However, Pierce[3] found that enuresis occurred during slow wave sleep.

Sleep is composed of two contrasting physiological states which alternate regularly. Slow wave sleep is a cyclical state created by transitions between contiguous stages of slow wave sleep, demarcated by spindling fast activity (sleep spindles) and large slow waves in the encephalogram. Stage I or drowsiness, contains minimal slow waves, while Stage IV—the trough of the cycle—contains maximal slow activity. Normally there are four or five cycles during a night's sleep. Between cycles, episodes of rapid eye movement (R.E.M.) sleep occur. The bulk of dreaming occurs in R.E.M. sleep.

Gastaut[4-6] has described an "enuretic episode" in children. Beginning in Stage III or IV slow wave sleep—that is, near the trough of the cycle, small body movements and muscle twitches associated with respiratory irregularities and decreased skin resistance, preceded micturition.

However, as Schiff[7] described enuretic episodes in three Army recruits during drowsiness, I have investigated the sleep of a group of six adolescent subjects (aged 12-19) and three adult subjects (aged 22-35). A total of 19 enuretic episodes were recorded during 47 nights. Initially bed electrodes were used to signal enuresis, but latterly a soft rubber collecting tube system was used. Without exception, enuresis occurred during slow wave sleep, generally starting in Stage III or IV. Tachycardia and muscle twitches preceded enuresis.

Enuresis in the adult and adolescent does not appear to be different from that of the child. Gastaut[6] and Broughton[9] have demonstrated physiological differences between enuretic children and controls. Bladder contractions in the normal child are limited to body movements and arousal. However, in enuretic children' spontaneous bladder contractions were more common in Stage IV sleep and a series of contractions preceded micturition. Broughton also found increased pulse rates during sleep in enuretic children. Using cerebral evoked potentials, he was also able to show a "carry over" effect from sleep into arousal, which may account for the stories that enuretic children can be very difficult to rouse.—I am, etc.,

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1 Ditman, K. S., and Blinn, K. A., *American Journal of Psychiatry*, 1955, **111**, 913
2 Bental, E., *Journal of Psychosomatic Research*, 1961, **5**, 116
3 Pierce, C. M., Whitman, R. M., Maas, J. W., and Gay, M L., *Archives of General Psychiatry*, 1961, **4**, 166.
4 Gastaut, H., and Broughton, R. J., *Revue de Neurologie*, 1963, **109**. 247.
5 Broughton, R. J., and Gastaut, H., *Electroencephalography and Clinical Neurophysiology*, 1964, **16**, 625.
6 Gastaut, H., and Broughton, R J., *Recent Advances in Biological Psychiatry*, Vol. 7, p. 197, New York, Plenum Press, 1965.
7 Schiff, S. K., *Journal of Nervous and Mental Diseases*, 1965, **140**, 397.
8 Broughton, R. J., *Science*, 1968, **159**, 1070.

Control of Paraquat

SIR,—We have recently treated two patients with paraquat poisoning. The first patient, a man of 35, deliberately ingested 2 g of paraquat as Weedol granules. He is well and back at work two months after admission. The second, a man of 23, accidentally ingested a mouthful of commercial concentrate and is now recovering rapidly 49 days after admission. A more detailed report will be available after a longer period of follow-up.

If the concentrate is not going to be made unavailable for general use as Dr. A. A. H. Lawson (26 June, p. 767) suggests, we feel strongly that the manufacturers and users of paraquat-containing weedkillers must be compelled to provide more adequate safeguards against accident, and that its extremely poisonous nature must be emphasized on all containers. However, the above cases and other reported survivals suggest that the outlook is not as gloomy as expressed in the lay and medical press. Headlines such as "Doomed—a boy who drank pop bottle poison" and phrases such as "waiting unsuspecting for death" (*Sun*, 15 and 17 June 1971) caused unnecessary anxiety to our two patients.—We are, etc.,

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