to draw the attention of the profession to the fact that gonococcal infection of the pharynx occurs, that the clinical condition is sometimes severe and, contrary to the findings here, not always accompanied by genital infection, and also that our knowledge of the sexual practices of the population is scanty.—I am, etc.,

W. FOWLER

V.D. Department,
General Hospital,
Birmingham

Lithium and Weight Gain

SIR,—Your leading article "Drugs Causing Weight Gain" (2 February, p. 168) mentions lithium carbonate taken over months in the prophylaxis of manic-depressive disorders. Patients on this treatment stay on lithium for years and the long-term considerations could be stressed. It has been shown that generally the weight gain with prophylactic lithium occurs in the first six months, and after this initial increase the weight remains constant for many years.[1] There is also some, rather limited, evidence that this initial increase in weight represents solid weight rather than water and that it is probably a reversal of weight loss during recurrent psychiatric illnesses.[2] Lithium, in a few cases, at the beginning of treatment may cause increased drinking due to thirst. It is helpful to advise patients against drinks containing sugar.[3] One of the important factors causing patients, particularly women, to stop taking their lithium is a fear of an excessive increase in weight. Such a lapse in treatment is not so likely if the pattern of an initial gain to be followed by stabilization at a somewhat higher weight is explained to the patient and their relatives.

From a practical point of view when prescribing prophylactic lithium your advice about physical checks, which should include regular weighing, is very desirable. A patient's initial gain in weight followed by his maintaining a constant body weight with be generally reassuring. It would be unfortunate if largely unnecessary short-term considerations by patients, their relatives, and their doctors during the first few months of prophylactic lithium caused the treatment to be stopped.—I am, etc.,

R. J. KERRY

Northern General Hospital,
Sheffield

1 Kerry, R. J., Liebling, L. I., and Owen, G., *Acta Psychiatrica Scandinavica*, 1970, **46**, 238.
2 Kerry, R. J., and Owen, G., *Archives of General Psychiatry*, 1968, **22**, 301.
3 Furlong, F. W., *Canadian Psychiatric Association Journal*, 1973, **18**, 75.

Attitudes to Abortion

SIR,—You have been taken to task by a number of correspondents (4 May, p. 276; 11 May, p. 329) concerning your leading article "Attitudes to Abortion" (13 April, p. 69). They are mainly concerned with only one emotive aspect of the problem. There are others.

You say that "abortion . . . is a poor substitute for contraception as a means of stopping unwanted births." This is indeed true, but so much of our work is concerned with the failures of preventive medicine and until we can eliminate the congenital defects in the newborn, abolish the stress diseases, control all infections, deal with the cause of dental decay, and find some cause for cancer we shall have to accept a situation that is not ideal. One day, I hope, all children will be conceived with the willing consent of both parents, but meanwhile the Abortion Act of 1967 does enable the medical profession to alleviate a very great deal of mental and physical suffering and to mitigate the results of the failure of methods of contraception that are admittedly imperfect.

It is suggested by Professor H. C. McLaren (12 May, p. 329) that one termination of pregnancy every three months is all that a consultant "practising modern obstetrics, offering compassion and advice" should be doing. This statement is, with respect, that of someone living in another world. A study of some of the carefully compiled case histories of those patients presenting themselves to such organizations as the British Pregnancy Advisory Service would enable a balanced judgement to be made.—I am, etc.,

REX BINNING

Hove, Sussex

SIR,—I am sure that there are many doctors who share my great anxiety for the future of gynaecological departments and even possibly for the Royal College of Obstetricians and Gynaecologists itself if the Lane Report[1] is accepted by the Government and implemented.

For doctors generally it would mean that there could be no prospect of specializing in gynaecology for Roman Catholics or for those who hold similar views on the subject of abortion. Inside the royal colleges it could create endless divisions among Fellows and members. For nurses it would perpetuate the atmosphere we have felt since the Act has been on the statute book—a reluctance on the part of many to work in the gynaecological wards and theatres. Finally for the patients too there would be an unacceptable paradox when a woman having a threatened miscarriage or being investigated for infertility is nursed alongside one having an abortion for reasons other than strictly medical ones.

Of course one can understand the desire to use the training and expertise of the gynaecologist to ensure "safe" abortions throughout the country, in which there is said to be a majority demand for such a service. But for job satisfaction on the part of doctors and nurses and for peace in the hospitals I am absolutely convinced that beds allocated for the purpose should be separate from the gynaecological ward and staffed by those who agree with this type of practice.

In other words, I suggest a new subspecialty—gynaecology (abortion).—I am, etc.,

J. C. MILLER

Croydon

1 *Report of the Committee on the Working of the Abortion Act*, Cmnd 5538. London, H.M.S.O.

SIR,—Professor H. C. McLaren (11 May, p. 329) is surely overstating his case when he tries to imply any parallel between the state atrocities of the Nazis and official recognition here of the reality of the need for abortion and the essentially humanitarian nature of the service it provides when abortion is inevitable.

Conscience must be seen to have its positive face. The "conscience clause" cannot be regarded as merely a negative one allowing only the right of abstention from helping the distressed. It also entitles those who are humanly concerned enough to wish to do so the right to ensure, within the law, proper medical help for those who would otherwise pass into the hands of the backstreet abortionists, with all the awful consequences of this that Professor McLaren and I, and all our generation, know very well. Hospital administrators have certainly a right on behalf of the public to stand on the side of such a positive interpretation of the "conscience clause" and for the intention of the Abortion Act as law. Trying to cripple the working of the Act seems to me almost as unworthy as profiteering from it. Those, and particularly those like Professor McLaren, who have a long and wide experience in hospital wards of the distress, damage, and not infrequently death resulting from septic abortions performed outside and who, despite it, think as he appears to do perhaps have a need to unblinker the function of conscience to give it a wider and more human sweep.

The trouble is that our natural humanity is hedged about with doctrines. Once it was thought salutary to the soul to scourge the body or to torture to death in the name of religion. We have a need still to shed all such doctrines and let the human spirit shine through. Unfortunately, the old coin is still being offered by some today as if it was valid tender without question, though very many now doubt, with reason, that it is gold. Perhaps, in our profession today we have a need to apply the touchstone more courageously and more emphatically to such coin, whoever it is presents it.—I am, etc.,

N. A. CHISHOLM

London N.W.3

Vaccination of Smallpox Contacts

SIR,—In reply to Dr. D. J. Bauer's letter (23 March, p. 576) we would like to stress that we stated (17 November 1973, p. 423) that "it is generally accepted that *successful* vaccination within up to about 48 hours after exposure will *usually* protect contacts against smallpox." We do not consider that there is anything *absolute* in the effectiveness of smallpox vaccination, and its success depends on many factors.

The efficacy of vaccination following exposure could be accurately determined only by carefully controlled studies, but these will never be done because of the known effectiveness of the procedure, which could not be withheld from any exposed person.

We have again reviewed the references quoted by Dr. Bauer but cannot reconcile his table with the published data. In his table there is no indication whether "day of primary vaccination or revaccination" relates to the day of onset of illness or to the day of contact. Only in Hanna's book[1] and the paper of Lyons and Dixon[2] is it possible to deduce reasonably accurate information of the day of vaccination and the day of exposure. In the data of Smith[3] and Cramb[4] the date of vaccination and the date of onset are stated and only an estimate of the day of vaccination in relation to contact can be made. Even so our analysis of the data confirms our original observation. For some reason Dr. Bauer does not include the data from

Lyons and Dixon[2] in his table but states that "four cases occurred among 40 patients in a hospital ward who were exposed for three hours only and successfully vaccinated within eight hours." Lyons and Dixon stated, however, that vaccination was "undoubtedly positive" in only three of the four and pointed out that in vaccinations done at that time the lesions were "rather slow in becoming vesicular and this may be a determining factor" in the failure to prevent smallpox in those vaccinated. The vaccine they used appears to have been of relatively low potency or the vaccination technique was poor, for about 10% of contacts required a second attempt. But there is another point about these three failures, for the four patients concerned could not be considered immunologically normal individuals. One of them "suffered from constitutional eczema, the second from sclerosing lipo-granuloma . . . the third was convalescent from hepatitis and the fourth was convalescent from pneumonia." We never suggested that impotent lymph would protect the immunologically abnormal.

Dr. Bauer notes that "simultaneous presence of a successful vaccination lesion and the rash of smallpox is a familiar occurrence." This is not disputed and there is abundant evidence of the value of vaccination in mitigating the severity of the disease when performed at any time after infection up to the date of onset, and even afterwards.[1]

When it comes to Dr. Bauer's own data[5] we must conclude either that he and his colleagues were dealing with a unique epidemiological situation or else that they were supplied with very incomplete data. They found that the incidence of secondary cases among primary household contacts was 4·17% (113 of 2,710); this is at variance with the usual finding that about 30-35% of unvaccinated household contacts develop the disease. Bauer *et al.*[5] state that when vaccination is done early "there may be a 10% failure rate even with potent vaccine." This again is at variance with the present experience of those engaged in the global eradication of smallpox, which indicates that nearly 100% take rates are obtained with competent vaccinators using freeze dried vaccine. Dr. Bauer's experience with revaccination in Madras, as reported in his letter, was apparently based on an abnormal epidemiological situation or a poor case-finding system and the use of substandard vaccine, since it appeared that it was best for patients if they were not revaccinated after exposure; they fared less well if revaccinated unsuccessfully and had the greatest risk of experiencing smallpox if they were revaccinated successfully. With regard to primary vaccination Bauer *et al.*[5] stated that "the incidence of smallpox among the untreated contacts who received primary vaccination after contact appears to show that this procedure confers protection against smallpox *even if it fails*" (our italics).

All the available published and unpublished data which we have seen and the outcome of discussions and correspondence which we have had with experts in Europe and the U.S.A. confirm that what we stated in our letter of 17 November accurately reflects the situation.—We are, etc.,

G. BOUSFIELD
GEORGE DICK

Rowhook Medical Society,
Chequers Inn,
Rowhook, Sussex

1 Hanna, W., *Studies in Smallpox and Vaccination.* Bristol, Wright, 1913.
2 Lyons, J., and Dixon, C. W., *Medical Officer,* 1953, **90**, 293.
3 Smith, C. S., *British Medical Journal,* 1948, **1**, 139.
4 Cramb, R., *Public Health,* 1951, **64**, 123.
5 Bauer, D. J., *et al., American Journal of Epidemiology,* 1969, **90**, 130.

Unsuspected Cytomegalic Mononucleosis

SIR,—In your leading article on this subject (2 March, p. 340) you state: "Tests for Epstein-Barr virus antibody are negative, as is the Paul-Bunnell test for heterophile antibody."

The cytomegalovirus (CMV) mononucleosis syndrome occurs almost exclusively in adults. By the age of 20 most individuals have antibodies to Epstein-Barr (EB) virus, as demonstrated by the indirect immunofluorescence technique introduced by the Henles. After the primary infection IgG antibodies to EB virus capsid antigens remain detectable throughout life. Of 19 patients with CMV mononucleosis, all had antibodies of this category to EB virus.[2] Recently, convenient methods for routine determination of IgM antibodies to EB virus have been developed.[3 4] Unlike IgG antibodies, IgM antibodies as a rule disappear within 2-3 months after the primary infection and therefore make specific aetiological diagnosis possible. Tests for IgM antibodies to EB virus were made in 15 consecutive cases of CMV mononucleosis treated at this hospital; the results were negative in all but one case. Thus occasionally double infections with CMV and EB viruses[5] as well as with CMV and toxoplasma[1] do occur even in patients without a history of blood transfusions.—We are, etc.,

E. KLEMOLA

Department of Medicine,
Aurora Hospital,
Helsinki

J. NIKOSKELAINEN

Department of Virology,
University of Turku,
Finland

1 Klemola, E., *et al., Acta Medica Scandinavica,* 1967, **182**, 311.
2 Klemola, E., *et al., Journal of Infectious Diseases,* 1970, **121**, 608.
3 Nikoskelainen, J., *Virological and Epidemiological Studies on Epstein-Barr Virus Infections.* Academic Dissertation, University of Turku, Turku, 1973.
4 Schmitz, H., and Scherer, M., *Archiv für die gesamte Virusforschung,* 1972, **37**, 332.
5 Wahren, B., *American Journal of Pathology,* 1969, **52**, 303.

Samuel Gee, Aretaeus, and the Coeliac Affection

SIR,—Samuel Gee's description of coeliac disease in 1888[1] was certainly an important milestone, but the view expressed by Drs. B. Dowd and J. Walker-Smith (6 April, p. 45) that this was the first significant progress since the time of Aretaeus is somewhat unjust to the physicians of the intervening 1,800 years. It is likely, for example, that the disease was recognized by Mathew Baillie in 1814 in his "Observations on a Particular Species of Purging."[2] Describing the stools, Baillie wrote: "They are pale in colour as if lime were mixed with water, are very frothy like yeast at the top, and often smell very sour." He stated that "patients labouring under this complaint have generally more or less of a sallow countenance, and are thin, but not very much emaciated. . . . The bowels, however, are often more or less distended with wind." He noted that the condition could persist for several years, with periodic exacerbations.

Both Baillie and Gee recorded that many adult patients had previously lived abroad in warm climates and both appear to have regarded the conditions later called tropical sprue and non-tropical sprue as the same disease.[3] Baillie's observations probably included patients with steatorrhoea of various causes other than coeliac disease, but the same is most probably also true of some of Gee's cases.

Gee realized that "if the patient can be cured at all it must be by means of diet" and he proposed treatment with artificially digested food. Baillie had earlier made the more relevant observation that "some patients have appeared to derive considerable advantage from living almost entirely upon rice."

Credit must be accorded to Gee for the recognition that coeliac disease commonly affects children. However, in historical perspective his real achievement was perhaps not his excellent clinical description but rather his acumen in designating the syndrome with a name resurrected from the writings of Aretaeus.—I am, etc.,

R. M. LEWKONIA

Edinburgh

1 Gee, S. J., *St. Bartholomew's Hospital Reports,* 1888, **24**, 17.
2 Baillie, M., *Medical Transactions of the Royal College of Physicians,* 1815, **5**, 166.
3 Cooke, W. T., and Asquith, P., *Clinics in Gastroenterology,* 1974, **3**, 3.

SIR,—Though Drs. B. Dowd and J. Walker-Smith (6 April, p. 45) quote Gee's statement that "if the patient can be cured at all, it must be by means of diet" they make no reference to several particular dietary observations that appear in his 1888 monograph.[1] He states that the allowance of farinaceous food must be small, and also describes "a child who was fed upon a quart of the best Dutch mussels daily, throve wonderfully, but relapsed when the season for mussels was over; next season he could not be prevailed upon to take them. This is an experiment which I have not yet been able to repeat." Samuel Gee may therefore be credited with the first documentation of improvement following the introduction of a gluten-free diet, with relapse upon return to a gluten-containing diet.—I am, etc.,

P. BAKER

University Department of Medicine,
Bristol Royal Infirmary,
Bristol

1 Gee, S. J., *St. Bartholomew's Hospital Reports,* 1888, **24**, 17.

Waiting for Brodrick

SIR,—Perhaps the major stumbling block to governmental adoption of the Brodrick Report is the undoubted additional work load that it would impose on pathologists. This problem would be particularly acute in the already overworked regional hospitals distant from departments of forensic medicine or well-staffed teaching centres. Already some regional consultants must be compromising their intellectual honesty in performing such large numbers of necropsies daily.

By far the largest proportion of forensic necropsies are for sudden unexpected deaths in middle-aged or elderly subjects, predominantly male. The vast majority of these have a perfectly natural cause of death. Is it really necessary to perform a necropsy in all of these cases? A representative sample of as small as 5% would allow a perfectly satisfactory statistical analysis of disease trends in addition to a substantial reduction in necropsy work.

I suggest that on referal to a coroner cases could easily be divided by his pathologist into one of two groups. The first would comprise those cases in which a necropsy is manifestly necessary. Clearly these would include trauma cases of all kinds; self-poisoning; death in children, young, and early middle-aged adults; or any case in which the medical or social aspects