(6) Dr. Freedman works in a country with so-called socialized medicine and should know that there is no economic burden for the patient having an operation for varicose veins. There is no reason to admit patients with this disease to a hospital unless the patient has some concomitant disease necessitating prolonged postanaesthetic supervision.

Dr. Freedman admits that "the stripping sound is useful in removing isolated segments of vein as those under an ulcer or eczematous area." These are the conditions in which a stripper really is *not* necessary. In these cases the pertinent perforating veins should be ligated and, most often, varicose clusters should be left at the operation in order not to imperil proper wound healing. I think that Dr. Freedman is alone with his view, at least as long as it is founded on the arguments he presents.—I am, etc.,

KNUT HAEGER

Malmö, Sweden

Gonorrhoea in Obstetrics and Gynaecology

SIR,—Dr. R. A. Sparks's letter (22 June, p. 666) on this subject summarizes very helpfully and with factual support of recent origin what many of us have been saying for some years about the diagnosis of gonorrhoea. Especially gratifying is Dr. Sparks's report that the Royal College of Obstetricians and Gynaecologists is nowadays willing to approve a post in venereology for all or part of the elective year preceding the M.R.C.O.G. examination. This continues the college's characteristically rational approach to the realities of modern society.

There is, I believe, a need for more thought in this direction by young people holding the M.R.C.O.G. With the continued control of syphilis, the rising incidence of other well-established sexually transmitted diseases, and the arrival on the clinical scene of previously rare or unrecognized sexually transmitted infections the emphasis in the specialty of venereology is nowadays very much on what might be called medical gynaecology. The day when the holder of the F.R.C.S. or M.R.C.P. dominated venereology is passing. In my view the future lies with holders of the M.R.C.O.G. In the past decade four trainees with this qualification have passed through this department to consultant posts. The prospect for others is excellent.

Venereology is in the privileged position of being able to secure approval for supernumerary registrarships, especially if linked with a named and suitable candidate. Special provisions are available for women doctors holding the M.R.C.O.G. There is no doubt that if you are young, hold this qualification, and apply yourself to training in the subject a consultant post in venereology awaits you long before the average age of consultant appointments.—I am, etc.,

R. S. MORTON

Special Clinic,
Royal Infirmary,
Sheffield

SIR,—Dr. R. A. Sparks (22 June, p. 666) is right to stress the importance of correct microbiological techniques in the investigation of women with salpingitis for *Neisseria gonorrhoeae* and it is clearly important that the sexual contacts of women who are found

to have a gonococcal infection are traced and treated.

A further important point arises in connexion with follow-up. Clinical resolution of gonococcal salpingitis does not necessarily imply a microbiological cure, and cultures from the urethra, cervix, and anal canal should be negative for *N. gonorrhoeae* on at least three occasions over a period of two to three weeks before patients are pronounced cured. Relapse of the salpingitis may otherwise occur, or infection or reinfection of a sexual partner.

I suggest that women found to have a gonococcal salpingitis should be followed up by the gynaecologist in conjunction with the department of sexually transmitted diseases, where the facilities for the isolation of *N. gonorrhoeae* are readily available and where any contact tracing action can be undertaken.—I am, etc.,

J. T. WRIGHT

Department of Genito-Urinary Medicine,
University College Hospital,
London W.C.1

Tetracyclines after 25 Years

SIR,—It seems to me that in your leading article (25 May, p. 400) you may have overstated the risks involved in the administration of the tetracyclines to patients with latent kidney disease if by that you mean in the absence of azotaemia. In all the papers which you cited and in the comprehensive review by D'Arcy and Griffin¹ significant deterioration of renal function was found only in patients with pretreatment blood urea values greater than 60 mg/100 ml.

This matter is of particular concern to physicians with an interest in chest disease as these drugs have been frequently used on a long-term basis for the prevention of winter exacerbations of chronic bronchitis, and their use in certain cases was recommended following a large-scale British controlled trial.² No monitoring of renal function took place in this trial but there appear to have been no episodes of what could be construed as deterioration due to renal failure. In a recent study of my own³ among a small group of chronic bronchitics there was no evidence of a rise in blood urea values during continuous tetracycline administration over a period of 3-6 months. Furthermore, though it had been arranged to exclude patients from this study with a pretreatment blood urea level greater than 50 mg/100 ml, in the event none of a group of 23 chronic bronchitics had to be excluded for this reason.

This suggests to me that the long-term administration of tetracyclines to chronic bronchitics is not contraindicated because of problems of renal toxicity, as the risk of deterioration with a normal blood urea seems extremely slight and the frequency of frank renal insufficiency in this disease may in fact be quite low. This is not to deny that physicians should be aware of this possibility when a patient on tetracycline deteriorates unexpectedly.—I am, etc.,

G. C. FERGUSON

General Hospital,
Northampton

¹ D'Arcy, P. F., and Griffin, J. P., *Iatrogenic Disease*, p. 124. London, Oxford University Press, 1972.

² Johnston, R. N., et al., *British Medical Journal*, 1969, 4, 265.

³ Ferguson, G. C., *British Journal of Clinical Practice*, 1974, 28, 131.

Types of Emphysema

SIR,—Your leading article (15 June, p. 571) merits elaboration. From the pathological standpoint confusion between types of emphysema commonly arises for two reasons. Firstly, the basic process is often obscured at necropsy by secondary disruptive changes which produce gross architectural distortion, and secondly, pigmentation may be incidental to urban residence rather than the result of occupational exposure to dust inhalation.

Three-dimensional examination of early lesions in the lungs of individuals dying from non-respiratory conditions, in addition to emphysematous lesions which have contributed to or been primarily responsible for death, permits a clearer appreciation of morphology in both the industrial and the non-industrial population. As a result it has proved possible to recognize types of emphysema by the distribution and the form of the enlargement within the lung acinus, and a revised classification on this basis has recently been given. This microanatomical approach has the advantage of indicating mechanisms that are evidently involved in the pathogenesis of the different varieties of emphysema.¹

In lungs fixed by distension the emphysematous changes are typically either (1) a panacinar pattern in which all the territory supplied by terminal bronchioles is enlarged (with distal acinar emphysema probably representing a less extensive change of the same nature), so giving a more or less uniform appearance throughout lobules, or (2) circumscribed emphysematous spaces set in more or less normal lobular parenchyma. To describe all such circumscribed lesions as centriacinar is inaccurate microanatomically and obscures the pathogenic mechanisms concerned. Two basic factors evidently contribute to the development of circumscribed lesions: (a) dust accumulation, as in coal workers, where alveoli around the proximal part of the acinus are consolidated, and (b) inflammation which disrupts alveoli at an early stage and may affect the population at large and to which cigarette smoking contributes.² In North-east England proximal acinar emphysema attributable to inflammation (formerly referred to as centrilobular emphysema) is rarely encountered, the lesion being seen at a more advanced stage when acinar boundaries have been transgressed and the emphysema, though still circumscribed, is properly described as irregular in type. There is little doubt that a chronic bronchioloalveolitis is responsible for these changes. Review of earlier material from South Wales indicates that examples formerly regarded as affecting only the proximal portion of the acinus are in reality irregular in type.

Failure to distinguish circumscribed forms of emphysema by three-dimensional study has confused the parts played by inflammation and dust accumulation. Urban dwellers develop irregular emphysema and the inflammatory response encloses dust-laden macrophages. Coal workers may also develop irregular emphysema which likewise exhibits pigmentation from industrial exposure, but here personal pollution is the likely explanation, dust accumulation, though heavier than in town dwellers, being of little importance. Coal workers may, however, acquire a simple dust lesion with which proximal acinar emphysema may be associated. Several

factors, including developmental and genetic, may underlie panacinar emphysema, which sometimes affects coal workers and becomes pigmented. Acquired fibrocystic disease, which in coal workers is often heavily pigmented, must also be distinguished from emphysema. On present evidence it is difficult to envisage a primary role for alveolar macrophages in the genesis of emphysema. Even in α_1 -antitrypsin deficiency, an uncommon state in which leucocytic protease activity may be directed against connective tissue, other factors may co-operate in the genesis of emphysema. The occurrence of alveolar fenestrae and capillary damage may merely represent the early stages of secondary non-specific disruption, which when advanced and widespread and possibly complicated by infection renders it difficult to assess the relative significance of the different pathogenetic processes in emphysema.—I am, etc.,

A. G. HEPPLESTON

Department of Pathology,
University of Newcastle upon Tyne

- ¹ Heppleston, A. G., *Annals of the New York Academy of Sciences*, 1972, **200**, 347.
² Ryder, R. C., Dunnill, M. S., and Anderson, J. A., *Journal of Pathology*, 1971, **104**, 59.

Psychiatry in the Soviet Union

SIR,—Dr. G. Morozov offers us evidence (6 July, p. 40) of the objectiveness of Soviet forensic psychiatry by referring to five dissidents whose mental illness was confirmed by an international team of psychiatrists. We must accept that judgement with respect. Unfortunately, as we are not told how or by whom these five patients were selected it proves nothing about the diagnosis made on other dissidents. Consequently it cannot refute in any way the complaints from distinguished Soviet citizens that many dissidents are still being interned in mental hospitals purely on account of their dissent.—I am, etc.,

ALISTAIR KING

Blackhall, Edinburgh

Sex for Medical Students

SIR,—Dr. Ronald Fletcher's analysis of the problems surrounding the introduction of teaching on sex and family planning, outlined in your leading article (29 June, p. 686), is as true for those subjects as it is for many other important aspects of medicine which are still outside the normal medical curriculum.

The problems, though, are not just lack of space in the curriculum and resistance by departments but also a complete lack of communication, not only between the departments themselves but also between them and the students they purport to teach. A telling example of this was in my own medical school, where the department of community medicine gave a lecture on the population crisis followed no less than three months later by a lecture from the department of obstetrics and gynaecology on the subject of contraception, to all intents and purposes a completely separate entity.

The introduction of a multidisciplinary course in contraceptive methods, sex counselling, demography, and population dynamics should not just be an excuse for more lectures. It is hardly surprising that medical students faced with slide after slide of

statistical evidence find more stimulation in a cup of coffee. More use must be made of teaching aids, films, and discussion groups related to clinical situations before students should be expected to take an active interest.

In fact, an extracurricular course on "Understanding Sex" was organized by students at Guy's consisting of lectures, discussion groups, and a film. It was well attended and appreciated by the students, which perhaps indicates that medical students, contrary to the picture of an un-receptive group needing to be cajoled into attendance, feel that sex education does occupy an important place in their course that is not yet filled.—I am, etc.,

T. J. GOULDER
Medical Student

Guy's Hospital,
London S.E.1

Jaundice after Halothane

SIR,—We wish to comment on the letters from Dr. W. H. W. Inman and Professor W. W. Mushin (27 April, p. 220) and Dr. D. Mansel-Jones (p. 221).

In their report (5 January, p. 5) Dr. Inman and Professor Mushin note that "no technique is available which will with certainty distinguish viral hepatitis from post-anaesthetic jaundice," while in their letters they state that "sufficient information to exclude other causes of jaundice was obtained for 111 (85%) of the patients." We are unable to reconcile these apparently contradictory statements. The three points made by Dr. Mansel-Jones in the fourth section of his letter apply to adverse reactions in general, but they should not apply to this specific one.

We are concerned that Dr. Inman and Professor Mushin note in their report that the more rapid onset of jaundice after multiple exposures to halothane provides "strong evidence of a cause-effect relationship between the use of halothane and jaundice." As the ability to differentiate halothane hepatitis from viral hepatitis was essential in the generation of their data, this conclusion is to some extent the result of circular reasoning. It is essential to remember that Klatskin¹ and Trey *et al.*² stressed the importance of multiple exposures to halothane in 1967 and 1968 respectively. Klatskin's uncontrolled data also suggested that there might be a shorter latent period to the onset of jaundice after multiple exposures to halothane compared with that after single exposures to the drug. It is interesting to observe from table II in Dr. Inman's and Professor Mushin's report that the percentage of cases in which the patient suffered multiple exposures to halothane rose in 1969 (1964-1968=78%, 1969-1972=90%, χ^2 test $P < 0.01$). In view of this it would be important to know if the picture relating to the rapidity of onset of jaundice also changed in 1969.

Dr. Inman and Professor Mushin have compared the risk of death in any one year after multiple exposures to halothane with the risk of "death in any one year from infectious hepatitis and acute or subacute hepatic necrosis in the general population." It would have been more appropriate—if indeed it is appropriate to compare numerical data derived from cases submitted voluntarily with anything—to compare the former with the risk of death in the same

year after multiple exposures to non-halothane anaesthetics.

The Mann-Whitney U test seems satisfactory, but while acknowledging that the result would remain "statistically significant" would it not have been more appropriate to use the two-tailed test? We would also like to ask Dr. Inman and Professor Mushin why they assumed that the multiple exposures to halothane rather than another variable, such as the medical condition requiring the additional surgery or the exposure to multiple operations, was the cause of the statistically significant difference. The population exposed to single administrations of halothane also differed from that formed by pooling the patients exposed to two and three administrations of the drug with regard to sex distribution (χ^2 test $P < 0.01$). Perhaps the populations differed in several other respects as well.—We are, etc.,

MICHAEL H. M. DYKES

Department of Drugs,
American Medical Association, and
Department of Anesthesia,
Northwestern University,
Chicago, Illinois

MARY JEAN BACH

Department of Anesthesia,
Northwestern University,
Chicago, Illinois

- ¹ Klatskin, G., in *Toxicity of Anesthetics*, ed. B. R. Fink p. 159. Baltimore, Williams and Wilkins, 1968.

- ² Trey, C., *et al.*, *New England Journal of Medicine*, 1968, **279**, 798.

Infections in Asplenic Adults

SIR,—It is of interest that one of the adults with *Haemophilus influenzae* meningitis described by Dr. Susannah J. Eykyn and her colleagues (1 June, p. 463) had previously undergone splenectomy. The authors indicate that splenectomy has been mentioned as a predisposing condition, and I would like to emphasize certain points about the risk of life-threatening infection in asplenic adults which have a bearing on management.

As in children, asplenic adults have a peculiar susceptibility to fulminant pneumococcal infection presenting as meningitis or the Waterhouse-Friderichsen syndrome,¹ with a mortality of 60% in the 40 cases reported in the literature.² In recent reports of serious infection following splenectomy for staging of Hodgekin's disease *H. influenzae* infection has also been prominent, though less frequent than *Streptococcus pneumoniae*, and these two organisms account for the large majority of reported cases of serious sepsis in asplenic patients. The risk is said to be maximal in the first 2-3 years after splenectomy,^{3,4} but this is based on paediatric experience, which is biased by short follow-up after splenectomy.⁵ Excluding those who were followed up for only 2-3 years after splenectomy, eight of 20 cases of serious pneumococcal infection in asplenic adults occurred more than five years after splenectomy.² Fourteen (70%) occurred later than two years after splenectomy, suggesting that penicillin prophylaxis for two years⁶ is unlikely to solve the problem.

These devastating infections are an uncommon complication of the asplenic state, and it seems that the practical approach to management is prompt and effective treatment when they arise. It has been suggested that increased awareness of the risk may have contributed to the lower mortality in