not asked for reversal. And Mr Winston's conclusion that it "seems unwise to sterilise women under 30 particularly immediately after pregnancy or if their marriage is in jeopardy" is therefore difficult to justify and has a disconcertingly paternalistic ring. It also implies that the stability or otherwise of a marriage should be a prime concern of obstetricians when women request sterilisation.

But what disturbed me most about Mr Winston's article was his statement that "most women in this survey had been told that termination would not be undertaken without sterilisation." I wonder if this form of ultimatum, which Mr Winston condemns, is likely to increase now this operation is performed under a fee-for-service system?

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Haematuria

SIR,—The helpful article by Mr P J R Boyd on haematuria (13 August, p 445) omitted one harmless cause of this symptom.

In 1963 a healthy 16-year-old boy suddenly and painlessly passed urine with fresh blood intimately intermixed. There were no other symptoms or clinical signs. Microscopy revealed the expected masses of red blood cells and no organisms, but did show also, unexpectedly, masses of the sharp refractile Maltese crosses of oxalate crystals. It seemed possible that these crystals had cut the urinary tract causing bleeding. Retrospective history-taking then revealed that he had, a few hours before, eaten three enormous helpings of red currants. This clinched the diagnosis, to my satisfaction, of haematuria due to oxaluria and the patient was spared all urological investigations. He has had no symptoms in the 14 years since. This supports Mr Boyd's two priorities in his article of (a) an accurate history and (b) a microscope available to the general practitioner.

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Adverse effects of lithium treatment

SIR,—In recent years major tranquillisers have been used singly or combined with lithium in the treatment of primary affective illness and affective disorders in schizophrenia. Cohen and Cohen¹ and Loudon and Waring² have reported toxic neurological reactions to the combined use of lithium and haloperidol and the latter have recommended caution when using haloperidol in doses greater than 40 mg/day with plasma lithium levels in excess of 1 mmol (mEq)/l. In the light of their reports and your leading article (6 August, p 346) I should like to report the following two cases in patients with long psychiatric histories.

A 48-year-old woman with unstable bipolar psychosis was maintained on lithium carbonate 1000 mg, the serum level being steady at 0.9 mmol (mEq)/l. Over a six-week period she received 75 mg flupenthixol decanoate and a few days after the last dose of 20 mg marked Parkinsonism (especially akinesia) developed. The drugs were stopped and nine days later she was symptom-free.

A 41-year-old woman with mild hypomania was given haloperidol 1.5 mg twice daily and 0.75 mg at noon. On the day the haloperidol was started she

noticed tingling in her fingers and later complained of episodes of marked pallor or cyanosis of the fingers. These changes were noticed by nursing staff and the haloperidol was stopped, when she complained of marked retrosternal pain. She was being maintained on lithium carbonate 1200 mg daily and the serum level was 0.5 mmol (mEq)/l. The symptoms disappeared and have not recurred though other major tranquillisers have been prescribed with the lithium. No evidence of disease associated with Raynaud's phenomenon has been detected and the ECG is normal.

The first case suggests that flupenthixol should also be used with caution in patients receiving lithium. The time relationship in the second between the symptoms and the use of haloperidol suggests that more than chance may be behind the phenomenon, though haloperidol normally inhibits vasoconstriction.³ A further trial of haloperidol has not been given on account of the retrosternal pain.

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¹ Cohen, W J, and Cohen, N H, *Journal of the American Medical Association*, 1974, **230**, 1283.
² Loudon, J B, and Waring, H, *Lancet*, 1976, **2**, 1088.
³ Johnstone, M, *Anaesthesia*, 1967, **22**, 3.

Poor-risk prostatectomy

SIR,—Your leading article (6 August, p 347), reviewing the paper by Sach and Marshall¹ contains many points with which urologists would agree. It is perhaps a little too critical for Sach and Marshall themselves to make the point that "direct comparison between series is difficult because of the number of variables involved and the difficulty of being certain that patients have been graded in a similar manner."

Most urologists would agree with the benefits of endoscopic resection, but a large gland does pose some problems, particularly in the unfit subject. You are a little naive when you talk of careful selection and of the benefits of operating "early rather than late." We all have our fair share of geriatric unfit customers and one can sympathise with our Adelaide colleagues that the majority were admitted in acute or chronic retention. Those urological units accepting all emergencies from a closed catchment area and working on a restricted bed allocation, worsened by nursing shortages, are all too conscious of these difficulties.

Sach and Marshall look for other methods to obtain better results and in the context of the grossly unfit patient may I suggest that greater attention be paid in the future to the use of cryosurgery of the prostate? It is always difficult to assess the numbers of patients condemned to a permanent catheter drainage. Certainly I have performed successful cryosurgery with very good functional results on respiratory cripples condemned to years of catheter existence in centres staffed by competent and enthusiastic resectionists, which I agree, to quote your leading article, is a "counsel of despair."

Properly performed and using quite a simple technique² cryosurgery takes only 10–20 min of total theatre time and can be performed under a short general anaesthetic or local intraurethral or regional anaesthesia. Depending on social conditions the patient may be discharged home within 48 h draining through an indwelling catheter. Good and lasting functional results can be obtained after catheter removal a few weeks later.

I have preferred to use probes using liquid nitrogen and currently we have no experience of lower temperatures with the nitrous oxide probe advocated by Wright.³

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¹ Sach, R, and Marshall, V R, *British Journal of Surgery*, 1977, **64**, 210.
² Green, N A, *Annals of the Royal College of Surgeons of England*, 1977, **59**, 288.

³ Wright, B, *British Journal of Urology*, 1976, **48**, 203.

Occupational scabies

SIR,—Drs R J G Rycroft and C D Calnan (30 July, p 303) emphasise the importance of overcrowding on board ship as an important factor in the spread of scabies rather than the sexual transmission so frequently favoured. It may not be widely known that a perfect control experiment occurred in 1945 to refute the opinion given during the war by the Advisory Committee on Scabies of the Ministry of Health that "the disinfection of clothing and blankets may reasonably be dispensed with."¹

The Third Infantry Division was transferred from Belgium to Egypt after the cessation of hostilities in Europe, half by air and half by sea. Among those arriving by ship after some three weeks large numbers had severe scabies, while none of the airborne troops were affected. Any "fraternising" in Germany and Belgium would have been equally enjoyed by both groups. The seaborne group, however, had slept in very dirty blankets which had previously been used by German prisoners of war, among whom we had seen many cases of severe crusted scabies.

This outbreak fits very well with the susceptible 15–30 age group, the epidemic which ended in 1949, and the overcrowding on board ship mentioned by Drs Rycroft and Calnan. Perhaps we should think of scabies less readily as a venereal condition and more often as a possible occupational disease.

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¹ Mellanby, K, *Scabies*, Oxford War Manual, pp 42 and 50. London, Oxford University Press, 1943.

Liver tumours and the pill

SIR,—In your leading article on this subject (6 August, p 345) you state that hepatic artery ligation will help to control bleeding, a complication of these tumours, and that "in at least one instance arterial ligation led to necrosis of the tumour."¹ In 1974 my colleagues and I reported that hepatic artery ligation combined with portal vein ligation resulted in the immediate shrinkage of a large, unresectable, vascular tumour, enlargement of which may have been due to prolonged oestrogen administration, and permitted complete removal of the tumour.²

Secondly, as you point out, ultrasonography may be useful to diagnose these tumours² and is an established method of diagnosing and differentiating solid from cystic lesions in the liver.³ It is available in most centres. Surely any patient who has sufficient reason to undergo a liver biopsy should now have the benefit of ultrasound, instead of "liver scan first,"⁴ to warn of the presence of cystic or vascular lesions or dilated bile ducts, which are likely to result in morbid complications after

needle puncture,^{6,7} and to demarcate solid lesions for biopsy?

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- ¹ Christopherson, W M, and Mays, E T, *Journal of the National Cancer Institute*, 1977, **58**, 167.
² Morley, J E, et al, *South African Medical Journal*, 1974, **48**, 695.
³ McCarthy, C F, et al, *Gut*, 1969, **10**, 904.
⁴ Goudie, E G, Myers, J B, and Morley, J E, *South African Medical Journal*, 1974, **48**, 203.
⁵ Conn, H O, *Gastroenterology*, 1974, **67**, 375.
⁶ Geist, D C, *Archives of Surgery*, 1955, **71**, 867.
⁷ Conn, H O, *Gastroenterology*, 1975, **68**, 817.

"The Scientific Journal"

SIR,—The only logical conclusion to be drawn from Dr Michael O'Donnell's comments about *The Scientific Journal: Editorial Policies and Practices* (2 April, p 886) is that he read neither the preface nor the rest of the book (in violation of a cardinal rule for book reviewers) before presuming to make a judgment. Let us examine specific statements of his.

(1) "Doctors in search of practical advice [about writing] will have to dig harder to find it in Lois DeBailey's book..." The stated purpose of the book was to provide guidelines for editors of scholarly journals, not to provide practical advice to physicians about how to write articles. It is therefore inappropriate to evaluate the book, as Dr O'Donnell has, in reference to another book with a different purpose and different audience. An intelligent reader would not, in fact, have to "dig" very hard at all to find a great deal of practical advice on the stated subject; all he need do is spot-check the labelled text or consult the table of contents or the detailed index. He will find practical advice regarding the administrative and editorial responsibilities of the editor and his staff; all aspects of the reviewing system for various genres of manuscript (including book reviews); advertising; copy-editing; journal cover, page identification, masthead; indexing; and binding practices.

(2) Although Dr O'Donnell alleges the book "has too many gaps," he does not indicate the scope or contents of the book and cites only two presumed gaps: inadequate treatment of libel and copyright. The preface clearly states that "we claim no...pretense at completeness. We hope, however, that the present treatment will...stimulate further exploration of the subject." A book designed as the preface states this one was could not—and should not—give a detailed explication of complex libel and copyright laws for every country in the world. Had these topics been covered comprehensively, Dr O'Donnell would, in fact, have been justified if he pointed to an imbalance in treatment and emphasis.

(3) "This neutered approach, hereby christened the However Syndrome, often leads to banality..." Clichés aside, what Dr O'Donnell calls "neutered" consists of a descriptive analysis of certain issues (again, as announced in the preface) when rigid prescription was not desirable. The passage he considers "banal" offers the practical advice to editors to maintain a uniform format for each issue but dictates no fixed order of journal components. Are we to conclude from his objection to the "neutered approach" that he wants dogmatic rules?

(4) "Occasionally," writes Dr O'Donnell, "when the text seems to rebel against the bland consensus, the syndrome leads to highly questionable dogma." He does not explain how the "However Syndrome" can lead to dogma. Dr O'Donnell's idea of dogma: "Although a catchy editorial title may attract attention, the title should above all be informative." He maintains that "if the title catches the eye and gets the reader to read the informative editorial beneath, it has done its job." Assuming that catching a reader's eye is getting him to read the material is naive; many a reader passes over a cute

but cryptic title. And what about problems of indexing and retrievability? Are editors to ignore these? Such a position reeks of irresponsibility.

(5) Does he, or does he not, want rules? Dr O'Donnell cannot seem to make up his mind. First he complains about a descriptive rather than a prescriptive approach to certain topics and then he decries the publication of what he considers to be dogma. "Above all," he writes, "[the editor] tries not to live by rules alone..." Quoting, once again, from the preface of *The Scientific Journal*: "Because we are aware there is no 'right' policy on most editorial matters, we have tried not to prescribe rules, but have, instead, explored various facets of the problems that confront the editor in his daily work." Implicit in the text throughout the book is the recognition that different types and purposes of different journals will require varying policies and practices.

(6) The ambiguity continues. Having just stated that "writing, editing, or driving...are no occupations for slavish rule followers," Dr O'Donnell proceeds to recommend six threadbare rules from Orwell, four of which begin with "never."

Several other reviewers and many distinguished editors and publishers, who have adopted *The Scientific Journal* as a standard reference book, obviously disagree with Dr O'Donnell. His remarks seem, in fact, to disclose more about the state of his own knowledge of the subject and of the art of book reviewing than they do about *The Scientific Journal*. Valid criticism of a book is useful, but unsupported—and unsupportable—statements based on inadequate reading of a book are a disservice.

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Dextrostix-Eyetone system for blood glucose measurement

SIR,—We agree with Dr M A Preece and Mr R G Newall (16 July, p 152) that the Dextrostix-Eyetone system (DES) is practical and quite accurate for the measurement of the blood glucose concentration not only for the insulin test, but also for the routine control of diabetic patients. We think, however, that the standard incubation time for Dextrostix as recommended by the manufacturer (60 s) is not always the best. In order to get the best correlation with the glucose-oxidase method the incubation time must be different under different conditions.

We measured blood glucose with the glucose-oxidase method and the DES in 96 blood samples from diabetic patients with blood glucose values ranging from 3.9 to 19.4 mmol/l (70 to 350 mg/100 ml). Every sample was measured three times with DES, each one with a different incubation time, 60 s (recommended by the manufacturer), 65 s and 70 s. Although prolonged incubation gives higher glucose values, the correlation between the glucose-oxidase values and the corresponding DES values for each incubation

time was highly significant; the correlation coefficient at 60 s was 0.94, at 65 s 0.95, and at 70 s 0.96, the P values for all three correlations being <0.001.

On the other hand, using the paired *t* test to compare glucose-oxidase values with DES values we got the results shown on the accompanying table. Thus it is clear that while there is a significant difference between the glucose values measured by the glucose-oxidase method and the DES with 60 s and 65 s incubation there is no such difference with 70 s incubation.

We have noticed that the "best" incubation time may vary from batch to batch of Dextrostix and may be influenced by the temperature in the laboratory and other unknown factors. So it is our practice to test every new batch and the same batch frequently in order to determine the appropriate incubation time for the best correlation with the glucose-oxidase method. Doing this for the past three years we have used incubation times ranging from 55 to 80 s and we have always obtained very good results. Using the appropriate incubation time DES becomes a very accurate method not only for glucose measurements in everyday practice but also for the diagnosis of diabetes by the oral glucose tolerance test.

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SIR,—We thank Dr A M Bold and his colleagues for their comments (13 August, p 459) on our paper (16 July, p 152) on the use of the Dextrostix-Eyetone system during the insulin tolerance test.¹ Their two main points cover adequate training and regular quality control procedures.

We emphasised the former point in our paper and we certainly endorse the latter. At the time of our studies there was no suitable quality control procedure for the Dextrostix-Eyetone system in the UK. However, a Dextrostix calibration and control set is now available from Ames Company. This consists of a calibration standard and three control solutions containing known quantities of glucose. The set is designed to calibrate and control Dextrostix when used with the Eyetone or Ames reflectance meter. A control solution is also available for Dextrostix when used visually.

Not only will this quality control procedure overcome the principal objection raised by Dr Bold and his colleagues, but it should also help to eliminate the carelessness observed by them.

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Comparison of glucose-oxidase values with DES values after different incubation times

	Glucose-oxidase value	DES value after incubation for		
		60 s	65 s	70 s
Mean $\pm$ SE (mmol/l)	8.98 $\pm$ 0.44	7.83 $\pm$ 0.42	8.24 $\pm$ 0.44	8.73 $\pm$ 0.46
<i>t</i>		7.93	5.19	1.66
P		<0.001	<0.001	>0.05

Conversion: SI to traditional units—Glucose: 1 mmol/l $\approx$ 18 mg/100 ml.