Sir,

Judy Gilley's paper on intimacy and terminal care made some important and often overlooked points but seemed lacking in breadth. The reader is left to infer a causal relationship between a lack of comfortable sexuality within a marriage and eventual admission of a dying spouse to hospital or hospice.

Many complex and intertwined factors determine where a patient dies; these are difficult either to define or measure. It is certainly ideal to have a spouse as loving nurse, but in the absence of this enviable situation other people may compensate. At best there will be blurring of family roles and the need for nuture within the dying person may be met by parent, sibling, child or dear friend. Physical intimacy in this setting is not so much a function of sexuality as a function of love. I would argue that Mr B.'s screams were not necessarily for a wife who could brush his hair but perhaps for anyone who loved him enough to brush his hair.

The important message these cases convey is that the anguish of losing a spouse accentuates the strengths and faults inherent in that relationship. There may be healing of old rifts or further destruction.

The article concludes by asking us to take the confessions of the carers as cues for the organization of appropriate care. But will this take into account the wishes and the rights of the dying person? These rights are only as real as respect and compassion permit them to be. Dying, like being born, is a time of extraordinary poignancy and importance; we in the primary care team are privileged in our involvement. Our role is to facilitate other carers in the realization of these rights while honouring patient, carers, and the bonds of trust between them. Willingness to accept a lack of detailed understanding of a relationship may reflect respect for the patient's privacy.

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Rubella prevention

Sir,

I was interested to read about the action being taken on rubella prevention in Northumberland (News, January *Journal,* p.47). As a trainee I decided to screen opportunistically for rubella immunity all women aged between 16 and 40 years who consulted me. I searched their medical records for documentation of either rubella vaccination or positive serology, and if no such information could be found I advised them to be tested for rubella antibodies. Those who agreed were venesected immediately. Women were only excluded from this survey if they had been sterilized, not if their husbands had been.

The records of 156 (57%) of the 274 women eligible for inclusion in the survey contained documentation of protection against rubella. Of the remaining 118 women, 11 refused to have rubella serology, 103 were seropositive and only four were seronegative. These four women were all aged over 30 years of age and none of them had ever been pregnant, but three of them were sexually active.

This survey suggests that the policy of vaccinating schoolgirls aged 11–14 years has been very effective in this area since it was introduced in 1970. However, it was necessary to serotest a large proportion of women in order to discover this as only 53% of women aged less than 30 years old had documentation of protection against rubella.

A slightly higher proportion (63%) of women aged 30–39 years old had documentation of rubella protection, usually in the form of antenatal serology. In contrast, of the 18 nulligravid 30–39 year old women in the survey, only four had documentation of rubella protection. Screening of this group of women was very rewarding, however, as it included the only four rubella susceptible women in the survey.

Many practices may feel that screening all their female patients for rubella immunity is not practical. It may be more realistic to concentrate one's efforts on sections of the population at highest risk. This survey suggests that nulligravid women aged 30 years or more are an appropriate target. As more women are pursuing active careers, and contraception has become more reliable, postponement of pregnancy until well into the fourth decade is chosen by an increasing proportion of women. For rubella infection to occur in any pregnancy is a tragedy, but for the elderly primigravida whose fertility is waning this must be even more devastating.

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Depression in the elderly

Sir,

Jack and colleagues (January *Journal,* p.20) are incorrect in stating that within the last two decades there have been no reports of British epidemiological studies on depression in the elderly. It seems remarkable that they should be unaware of the work of the US/UK study published in 1983[1] which interviewed 396 elderly Londoners and a similar number of old people in New York, detecting 'pervasive depression' in 12% and 13% respectively. An Edinburgh study published in 1984[2] found that 5% of a sample of 487 old people were depressed. More recent studies in Liverpool[3] and Clackmannan, Scotland[4] have given further information on the epidemiology of depression in old people in the UK which may not have been available to Jack and colleagues when submitting their manuscript.

It is difficult to know what to make of the results of the study itself. In the light of their comment that the geriatric depression scale[5] is not a diagnostic tool, it is doubtful whether the scale's originators would endorse its use as an instrument for actual case definition rather than possible case detection. Screening of general practice patients for depression will only help the planning of resource allocation if the significance of results obtained at screening is known. The authors are right to emphasize the importance of longitudinal studies of individuals who record high scores on screening instruments for depression, but individuals with low and intermediate scores should also be included for comparison. Only adequate follow-up studies will reveal the relevance of scale scores to clinical practice and service provision.

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References

1. Gurland B, Copeland J, Kuriansky J, *et al. The mind and mood of ageing.* London: Croom Helm, 1983.
2. Maule M, Milne J, Williamson J. Mental illness and physical health in older people. *Age Ageing* 1984; **13:** 349-356.
3. Copeland J, Dewey M, Wood N, *et al.* Range of mental illness among the elderly in the community: range in Liverpool using the GMS-AGECAT package. *Br J Psychiatry* 1987; **150:** 815-823.
4. Bond J. Psychiatric illness in later life. A study of prevalence in a Scottish population. *Int J Geriatr Psychiatry* 1987; **2:** 39-57.
5. Yesavage J, Brink T, Rose T, *et al.* Development and validation of a geriatric depression screening scale: a preliminary report. *J Psychiatr Res* 1983; **17:** 37-49.

Sir,

I must defend Dr Ames' allegation that we made an incorrect statement. In fact we were quoting Dr Henderson and Pro-

fessor Kay's comment (ref. 2 in our article) 'there have been no reports of British epidemiological studies on this subject in the past two decades'. This was further supported by Dr McDonald of the Institute of Psychiatry in his publication of 1986 (ref. 9).

We compared our results with a study on depression in London published in 1986 which is more recent than the study in London quoted by Dr Ames. Our paper was submitted for publication in 1986 and, as Dr Ames suggests, could not possibly have referred to the studies in Liverpool and Scotland, published in 1987.

I maintain that the clinical diagnosis of depression should not be made on questionnaire results alone. Professor Goldberg and associate (ref 17) appear to support my view.

I thank Dr Ames for his support for further epidemiological studies and for informing me of the recent publications on the subject.

I wish to report a printing error in the text. A sentence in the second paragraph of the discussion should read: 'However, *their* study was selective and did not include those patients...', rather than *'our* study' which confused some readers who brought the error to my attention.

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Bacteriology of a rural practice

Sir,

I was most interested to read Ditchburn and colleagues' retrospective bacteriology survey (March *Journal,* p.110). However, I do not entirely agree with their proposed antibiotic regimen.

Most general practitioners are faced with having to prescribe an antibiotic before bacteriological results are available, based on the most likely causative organism. With respect to urinary tract infections, in this study *Escherichia coli* and *Klebsiella pneumoniae* were the commonest causative organisms, *E. coli* being six times commoner than any other organism. As 95% of these organisms were sensitive to nitrofurantoin and 79% to trimethoprim it would appear that either drug would be a reasonable choice to cure a majority of patients while awaiting bacterial culture results.

The advantage of trimethoprim over nitrofurantoin is that it is well absorbed, attaining high concentrations in blood and other tissues, and therefore effective in patients at risk of developing an ascending pyelonephritis. Trimethoprim is one of very few antibiotics which penetrates prostatic tissue in therapeutic concentration and thus is very useful in this difficult therapeutic area.[1]

Either nitrofurantoin or trimethoprim may safely be used for long-term prophylaxis in children with anatomical abnormalities of the urinary tract which predispose to infection — the commonest organisms involved again being *E. coli* and *K. pneumoniae.*[2] The high blood concentrations achieved with trimethoprim may confer advantages and tolerance appears to be better than with nitrofurantoin. In contrast, cephalosporins are not suitable for long-term prophylactic use. Unfortunately, the authors give no indication of the sensitivity of *E. coli* to cephalexin in their series, although they recommend its use.

In bacterial upper respiratory tract infections, trimethoprim has recently been shown to be as effective as amoxycillin in a prospective, randomized double-blind trial in general practice.[3] It penetrates sputum well and is an effective alternative to the penicillin group.

Trimethoprim is usually ineffective against *Pseudomonas aeruginosa* and *Neisseria gonorrheae,* neither is it recommended for use in pregnancy. With these provisos it remains a safe and effective antibiotic in the general practitioner's armamentarium.

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References

1. Maskell R. Chronic prostatitis. *Br Med J* 1987; **295:** 611.
2. White RHR. Management of urinary tract infections. *Arch Dis Child* 1987; **62:** 421-427.
3. McGuinness BW, Cooper J, Kent PL. Trimethoprim versus amoxycillin for upper respiratory tract infection. *Practitioner* 1986; **230:** 905-908.

Sir,

Dr Ditchburn and colleagues are to be congratulated on their assiduous retrospective six year study of bacteriology in rural general practice (March *Journal,* p.110). My surprise on reading this article — and I suspect that it also surprised the authors — was the relatively high percentage resistance of urinary tract pathogens to trimethoprim.

One reason for this may be that they analysed all positive urine cultures together whether the samples were obtained from patients with acute or chronic problems. The latter tend to have repeated cultures from which may be grown colonies of bacteria of unusual genera and resistance to antibiotics. In this study, culture rates of *E. coli* were perhaps less than one would expect while those of *K. pneumoniae* were perhaps higher. This again suggests an unusual predominance of chronic infection.

If the authors could separate from their figures those results derived from acute urinary tract infection and so prove their point, I should feel happier in discarding trimethoprim as my first choice in the treatment of acute urinary infection. Nitrofurantoin, which the authors suggest as their drug of choice, is more expensive, associated with more side effects and is contraindicated in the presence of renal failure.

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Sir,

We are grateful to Dr Miller for his comments on our paper. We had considered the possibility that an unusually high number of chronic infections might have been responsible for our finding of a high frequency of resistance to trimethoprim in urinary pathogens. However, this does not appear to be a significant factor. Of the 325 urinary pathogens isolated, 54 came from patients with recurrent infections caused by structural or functional abnormalities of the urinary tract. These indeed had an atypical flora — only 31% grew *E. coli* and 26% *K. pneumoniae.* Among the remaining 271 'normal' cases 68% grew *E. coli* and only 8% grew *K. pneumoniae.* The sensitivity to trimethoprim in these cases was still only 72%. This is because of the relatively high resistance to trimethoprim of all the urinary pathogens including *E. coli.* Trimethoprim resistance was present in 21% of *E. coli* strains, 45% of *K. pneumoniae* strains and 43% of *Streptococcus faecalis* strains. Thus, although Dr Miller may be right in ascribing our relatively low frequency of *E. coli* in urinary infections to abnormal cases, these do not explain most of the trimethoprim resistance found.

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Medicine in South Africa

Sir,

I read with interest Dr Donald's editorial (March *Journal,* p.97) on international aspects of general practice. The author states with some pride that 'within three