recent sexual partners. Patients were informed that their confidentiality would be preserved. This is important as many patients would not otherwise give details of their contacts. This is protected by law in the genitourinary medicine clinic. Patients were advised to tell their contacts of their diagnosis and to encourage their attendance at a genitourinary medicine clinic.

Male and female patients found to be positive for chlamydia and who had not received treatment at presentation were recalled, treated, and interviewed by the health adviser. All identified partners were then actively sought by the health adviser, when necessary by a home visit. Those who were traced were offered screening and treatment.

Our experience shows that a programme such as that described by Taylor-Robinson is effective in a genitourinary medicine clinic and suggests that results similar to those in Sweden would be achievable if applied to the community as a whole. We believe that a comprehensive screening programme should include testing of both men and women and that contacts need to be actively pursued by people skilled in preserving confidentiality. The use of a non-invasive first catch urine sample, as we originally described,³ would greatly facilitate widespread screening of sexually active male patients and would identify asymptomatic infections.

Sexually transmitted diseases, including *C trachomatis* infection, are cofactors in the heterosexual transmission of HIV infection.⁵ The need for active intervention in the community cannot therefore be overemphasised.

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Depot medroxyprogesterone acetate and osteoporosis

Monitor serum oestradiol concentration in users

EDITOR,—T Cundy and colleagues again report the theoretical risk of oestrogen deficiency and osteoporosis in long term users of depot medroxyprogesterone acetate.¹ Their previous study² was criticised on methodological grounds,³ and the issue has not yet been resolved. As before, their small study is cross sectional rather than prospective. Once again, we do not know the bone densities of the women before they used depot medroxyprogesterone acetate. In addition, we are not given any details of the oestradiol concentrations during or after use of the contraceptive.

As in the previous study, the bone densities of the women using the contraceptive were not such as to place them at current risk of a fracture. Since 1991, however, we have suggested that serum oestradiol concentrations should be measured in

long term users of depot medroxyprogesterone acetate. For any women found to be oestrogen deficient there is a good alternative to stopping the method: this is to add natural oestrogens, just as if the women were using hormone replacement therapy, with the depot medroxyprogesterone providing the progesterone component. Depot medroxyprogesterone acetate gives a fivefold protective effect against endometrial cancer.⁴ A prospective study of bone density (including measurements of oestradiol concentrations) will start shortly and, we hope, resolve the controversy.

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Smoking may explain findings

EDITOR,—T Cundy and colleagues' paper on bone density in women who stop using depot medroxyprogesterone acetate raises certain questions.¹ There was a considerable difference in the percentage of smokers (22% v 6%) between the women who stopped using the drug and those who have never used it; this could have been responsible for the difference in results between the two groups. Smoking increases the incidence of osteoporosis, and stopping smoking has been recommended as a preventive measure for osteoporosis.²

The hypothesis that medroxyprogesterone acetate reduces bone density contrasts with that of Prior, who has considered the potential role of progesterone in bone formation.³ Prior *et al* have reported the presence of progesterone receptors on osteoblasts in vitro, citing this as evidence that progesterone acts as a bone trophic hormone.⁴

Cundy and colleagues did not find any significant difference in the bone density in the femoral neck, which can be measured accurately; measurements of bone density in the spine are less accurate. Measurement of bone density at the wrist might have yielded more reliable results.

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Palliative care in severe heart failure

EDITOR,—I wish to draw attention to Henry J Dargie and J J V McMurray's comments about the malignant nature of severe heart failure,¹ and the need for discussion about quality rather than quantity of life in this condition. Palliative medicine is the "study and management of patients with active, progressive, far-advanced disease for whom the prognosis is limited and the focus for care is quality of life."² It addresses not the diagnosis but

the need for care in the final stages of life. Patients with cancer are not the only patients with this need.

Palliative medicine has revolutionised the management of pain and other symptoms of cancer, including dyspnoea. End stage heart failure has several features in common with cancer, such as dyspnoea, tiredness, and cachexia. The role of opioids in acute pulmonary oedema is undisputed. Little information exists, however, on the effects of oral or nebulised morphine for dyspnoea in end stage heart failure. Research on this is badly needed and should be possible with cooperation between palliative and other physicians.

Deciding when a patient with heart failure has reached a stage when palliative care is appropriate is not easy. Doctors treating patients with heart disease are often obsessed with resuscitation even when this is inappropriate, and dying patients may be subjected to intensive treatment because no one has said stop. If the quality of life of patients with terminal heart disease is to be improved we need to lose more of our preconceptions.

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Catatonic stupor after "ecstasy"

EDITOR,—D L Maxwell and colleagues report two cases of catatonic stupor in patients who had taken 3,4-methylenedioxymethamphetamine ("ecstasy").¹ I doubt whether the patients exhibited sufficient features to justify the diagnosis of catatonic stupor.

Catatonia is a behavioural neurological syndrome characterised by abnormal motor behaviour and periods of extreme hyperactivity and hypoactivity.² Mutism and stupor are common, but alone they are not pathognomonic. Several organic conditions associated with catatonia have been reported, but the catatonias were often not diagnosed according to strict criteria.³

To aid research and communication a consensus definition of diagnostic criteria for the catatonic syndrome is desirable. According to diagnostic guidelines in the *International Classification of Diseases*, tenth revision, organic catatonic stupor is a state (of assumed organic aetiology) of diminution or absence of spontaneous movement with partial or complete mutism, negativism, and rigid posturing; the presence of other catatonic phenomena such as stereotypies or waxy flexibility increases confidence in the diagnosis.⁴ Interestingly, specific operational criteria for catatonia, as a separate category, have also been suggested for the *Diagnostic and Statistical Manual of Mental Disorders*, fourth edition.⁵

The first patient reported on by the authors was drowsy and could be roused but was uncommunicative. She responded appropriately to pain but not to command. She intermittently opened her eyes but did not respond to or recognise anyone. This is a picture of organic (neurological) stupor, which may be associated with the absence of verbal response—mutism, when the subject aroused is incapable of normal alertness and interaction. There were no catatonic features other than stupor and mutism. The second patient became restless and uncommunicative after taking 3,4-methylenedioxymethamphetamine. She was apparently conscious. She did not speak. At times she sat up and moved about spontaneously with a "wild eyed" look. She was not stuporous in the neurological or psychiatric sense. In psychiatric practice stupor is regarded as a syndrome whose central feature is a reduction in or absence of relational

functions (that is, action and speech). There were no catatonic features other than mutism. The diagnosis of catatonic stupor therefore seems unjustified in both cases.

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Funding family planning

EDITOR,—Brian Harradine calls for an end to "population control programmes" funded by, among others, the International Planned Parenthood Federation "because of their coercive nature and the negative effect they have on women's health."¹ The aim of the International Planned Parenthood Federation is to provide women and their families with information and services that will allow them to live healthier and longer lives. Through its worldwide network of autonomous national family planning associations the federation responds to family planning needs and reproductive health needs expressed by local people, particularly women.

Currently more than 120 million women in the developing world are not using contraception although they say that they want to avoid pregnancy.² As many as 500 000 women die every year from causes related to pregnancy.³ An African woman has a 1 in 22 likelihood of dying from a cause related to pregnancy.⁴ These figures are well known to those working with reproductive health issues. On paper, however, they do not necessarily reflect the suffering and ill health that each of those women may encounter in her desire to practise the right to regulate her own fertility.

The International Planned Parenthood Federation agrees with Harradine's hope that family planning programmes will continue to concentrate on the individual needs of users. It enjoys a close relationship with the Australian International Development Assistance Bureau and hopes for the bureau's continuing support in its work.

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Cystoscopy in bladder cancer

EDITOR,—R R Hall and colleagues¹ support the Medical Research Council's proposals for changes in cystoscopic follow up of patients with bladder cancer and adjuvant intravesical chemotherapy.² We wish to add some words of caution. One of us (DAT) was a coauthor of the Medical Research Council's paper and recognised that the population with superficial bladder cancer on which the conclusions were drawn was biased in favour of

Effect of using Medical Research Council's criteria on number of cystoscopies performed in patients with bladder cancer

Risk group	No (%) of patients	No of cystoscopies (% positive)			
		Actual	According to MRC protocol	Yielding positive result	Rate of positive cystoscopies*
Low	68 (42)	430 (14)	279 (22)	62	1 every 3.0 years
Intermediate	57 (36)	372 (33)	450 (28)	124	1 every 1.7 years
High	34 (22)	275 (55)	372 (41)	152	1 every 0.9 years
Total	159	1077 (31)	1101 (31)	338	

*Excluding recurrence at first check cystoscopy.

disease with a good prognosis, based on tumour recurrence rates of 40% compared with a generally accepted rate of 70% in other series.³ This led us to examine what would have happened if this approach had been adopted in our own department with an unselected group of patients (annual meeting of British Association of Urological Surgeons, 1993).

The table shows that our patients were overall at higher risk of recurrence than those in the Medical Research Council's study, in which 60% were classified as at low risk, 30% at intermediate risk, and 10% at high risk. Possibly as a consequence of this, our audit of prospectively recorded data on 159 patients with superficial bladder cancer treated in our department suggested that the adoption of such a policy, far from reducing the number of check cystoscopies, would entail a 2% increase in the frequency of cystoscopy overall. This, however, would be the result of increasing the frequency of cystoscopy in the intermediate and high risk groups, for whom the benefits would be greater; thus our resources would be better used.

We strongly agree that, if such a policy was generally implemented, high grade tumours that are already invading the submucosa (G3T1) should be excluded and treated aggressively since all four patients from this study population who progressed to muscle invasive bladder cancer had a tumour of this type at diagnosis. One of them would have been placed in the group at low risk of recurrence if the Medical Research Council's criteria had been strictly applied.

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Murder in the NHS

EDITOR,—In his editorial on the murder by nurse Beverley Allitt of four children in hospital W J Appleyard states that one of the two consultant paediatricians at the hospital, Dr Nanayakkara, "sought the opinion of a paediatric pathologist but this request was persistently denied."¹ Readers might infer that my department was at fault, and I strongly refute this. Appleyard may not be aware that it was the coroner for Grantham who decided not to call in a specialist paediatric pathologist to perform the postmortem examinations on the children. Instead he instructed the local pathologists (who were employed in a locum capacity) to carry out the examinations. Dr Nanayakkara was concerned about the pathologist's diagnosis in the initial case and discussed the case over the telephone

with our paediatric pathologist, who readily agreed to comment on any tissues or tissue sections preserved from the postmortem examination. He was unable to give a firm opinion on the limited material made available at that time.

The chain of events initiated by Beverley Allitt was extraordinary, and, as is evident from Appleyard's editorial, many lessons are being learnt from it. Perhaps what needs to be emphasised is that it is the coroner's responsibility to ascertain the cause of death, often using information obtained by the pathologist. In the case of children the coroner should employ a pathologist with a record of skill in paediatric work, particularly when the circumstances of death are ill defined.

Finally, making the correct diagnosis in the cases in Grantham would have been extremely difficult for anyone since the interpretation of biochemical variables after death is unreliable. Not only biochemical tests but careful evaluation of the clinical records were ultimately required to reach the correct diagnoses.

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- 1 Appleyard WJ. Murder in the NHS. *BMJ* 1994;308:287-8. (29 January.)

Treatment of heroin users

EDITOR,—It is unhelpful for John Marks to continue to force an artificial polarisation of the debate on prescribing.¹ In distancing ourselves from the suggestion that we are "pandering to American prohibitionist fundamentalism" we do not wish to be forced to embrace a similarly extreme opposite position of, for example, providing insufficiently controlled supplies of the "drug of choice" or "state-rationed drugs"^{2,3} or becoming "a dealer by appointment to Her Majesty's Government."⁴ We wish to be allowed to continue to explore the more fertile intermediate terrain.

In our original letter we expressed concern that excessive reliance on individual clinical judgment may jeopardise British doctors' privileged autonomy in prescribing—especially if the manner in which this freedom is used or exploited seems excessive.⁵ There is little in Marks's response that reduces our concern. The choices of drug, form, and dose each stretch the bounds of clinical freedom to their limits. In all three of these aspects, the prescribing that we described previously is extraordinary: neither heroin nor cocaine can be prescribed to drug users in any country in the world other than Britain. Likewise, the prescribing of injectable drugs to users has long been an exclusively British behaviour (with the minor exceptions that we outlined previously).⁵ We disagree fundamentally with Marks's statement on dose conversion between methadone and heroin: considering a prescribed daily dose of 900 mg of diamorphine to be about equal to 80 mg of methadone is so far off the mark to beggar belief since the more usual conversions are around parity.

Marks's "defence" of the right to offer treatment to drug users temporarily resident in