out, there should be continuity of treatment between the community and prison.² There has been considerable pressure to ensure that the delivery of methadone detoxification to opiate users presenting to prison is adequate, and guidelines have been drawn up, with the cooperation of key people involved in drug treatment services, to promote this. Opiate users should be given sufficient methadone during drug withdrawal to reduce the likelihood of them injecting and using contaminated equipment.

Ross and colleagues insist that the existing seven day withdrawal regimen is unethical. Colleagues and I run an inpatient unit where we provide a 10 day withdrawal regimen for all our opiate dependent patients, and we can see no grounds for arguing that reducing this to seven days is unethical or fails to replicate the physiological rates of withdrawal.^{3,4} In my view such withdrawal regimens should be spread evenly throughout the prison system. According to the most recent report of the director of health care for prisoners, a survey of prison treatment establishments found that only a fifth of respondents were providing methadone detoxification for opiate withdrawal. All prison medical officers should provide approved treatments for drug withdrawal; more individualised treatment approaches can evolve in the wake of the widespread provision of a basic minimum of treatment.⁵

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Asthma and having siblings

EDITOR,—We recently completed a study examining risk factors for the development of asthma in 708 children aged 8-13 in New Zealand and found a similar effect for wheezing in the past year to that found by Erika von Mutius and colleagues,¹ with a declining frequency of wheezing with increasing numbers of older siblings.² From a questionnaire given to a parent or guardian we examined reported wheezing in the past year in relation to both older and younger children in the household.

The prevalence of symptoms declined with increasing numbers of old siblings and, to a less extent, with increasing numbers of young siblings (table). We observed a similar trend for symptoms

of hay fever and eczema in the past 12 months, although these trends were not significant.

The hypothesis that this trend might be due to the down regulation of IgE or an altered pattern of release of cytokines because of early exposure to bacterial or viral infections through having older siblings is intriguing, although other maternal and socioeconomic factors might explain the findings. Interestingly, in a study of infants Burr *et al* found the reverse of this trend, with increased reporting of wheeze, nasal discharge, and prolonged colds in the first year of life with increasing number of siblings³; this presumably confirms the increased risk of exposure to viral and bacterial infections in early infancy with increasing numbers of siblings.

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Pneumocystis pneumonia

EDITOR,—Simon P Matusiewicz and colleagues questioned whether reactivation or reinfection was responsible for pneumocystis pneumonia.¹ They used an immunofluorescence assay to examine 220 samples of bronchoalveolar lavage fluid and bronchial washings, but only five samples were found to be positive for *Pneumocystis carinii*. Although the authors quoted work suggesting that immunofluorescence assay was as sensitive as techniques that use the polymerase chain reaction, this is contrary to our experience.

The last 18 samples of bronchoalveolar lavage fluid from 11 patients were examined by a nested polymerase chain reaction and immunofluorescence assay. The results were instructive: the polymerase chain reaction confirmed the results of the immunofluorescence assay in 13; three equivocal results of the immunofluorescence assay were confirmed as positive by the polymerase chain reaction, and the patients responded to treatment; and two samples that yielded negative results in the immunofluorescence assay were positive with the polymerase chain reaction, and again the patients responded to treatment. In patients without pneumocystis pneumonia the concentration of *P carinii* would be low, so it is critical that a sensitive method is used. We believe that it would be informative for the authors to test with the polymerase chain reaction the 215 samples that yielded negative results in the immunofluorescence assay.

All five positive results in the authors' study occurred in patients with small cell lung cancer, and the authors thought that the infection was subclinical. Another fundamental question is how laboratory tests can help clinicians decide on the importance of a positive result. This can be confusing as *P carinii* may be detected when the pneumonia is due to another cause.² We believe

that in these cases serological testing for *P carinii* may be of benefit.^{3,4} We have found that serological testing is particularly useful in patients who do not have AIDS⁵; as these patients have fewer cysts and trophozoites the result is more helpful.

We agree that immunocompromised patients should be monitored prospectively, but we would also perform serological tests. For doctors to deal adequately with pneumocystis pneumonia in immunocompromised patients further studies are required to address fundamental questions on the importance of this infection.

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Discrimination against people with HIV infection and AIDS

EDITOR,—Renée Danziger's paper on discrimination against people with HIV infection and AIDS in Poland, in particular by health staff,¹ stimulated us to update our study on this in Italy. Between May 1991 and May this year, by means of a coded questionnaire distributed to all consenting patients attending the division of medical oncology and AIDS of the Centro di Riferimento Oncologico, Aviano, Italy, we interviewed 212 patients about whether they had been discriminated against by health care workers. One hundred and sixty two patients were men (the median age 30 (range 20-65)).

Two hundred and nine of those interviewed were HIV positive; 140 were injecting drug users, which accords with Italian epidemiological findings with regard to HIV infection. Seventy six reported 88 episodes of discrimination by health care workers (dentists and doctors in almost all cases) in public and private Italian institutions. In three cases nurses were involved. Thirteen patients reported more than one episode of discrimination. Dentists were the category of health care workers most commonly mentioned, with 44 episodes of discrimination, while no infectious disease specialist was mentioned. The nature of the reported episodes differed, but in 60 cases the patients had been denied the health care service requested.

One hundred and seventy six patients spontaneously reported to health care workers that they were HIV positive while 36 did not; 14 did not do so because they had previously suffered discrimination while 10 had never reported their seropositive status and had never suffered discrimination, and 12 patients did not give a reason for not reporting their status. Twenty two continued to report their status despite having been discriminated against previously. Sometimes those who did not report their HIV status reported instead that they were suffering from hepatitis, to allow health care workers to take more precautions.

This study shows that there is a high rate of discrimination by Italian health staff against

Prevalence of reported wheezing in past year among 708 children aged 8-13 related to number of older and younger children in household

No of siblings	% (No) with wheeze		Odds ratio (95% confidence interval)*	
	With older siblings	With younger siblings	With older siblings	With younger siblings
0	24.8 (96)	25.9 (67)	1	1
1	18.8 (45)	19.7 (46)	0.7 (0.5 to 1.1)	0.7 (0.5 to 1.1)
2	13.6 (9)	14.4 (20)	0.5 (0.2 to 1.0)	0.5 (0.3 to 0.9)
≥3	6.7 (1)	24.6 (15)	0.2 (0 to 1.6)	0.9 (0.5 to 1.9)

*Unadjusted odds ratios.

P value for trend: older siblings, $P < 0.01$; younger siblings, $P = 0.09$.

patients with HIV infection. In our previous evaluation three years ago in 86 patients with HIV infection we found a rate of discrimination of 40%.² In Italy legislation protects people with HIV infection, but seemingly health care workers are often discriminatory.

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Acanthamoeba keratitis

EDITOR,—Having read D V Seal's editorial on acanthamoeba keratitis,¹ we would like to report a case of *Acanthamoeba culbertsoni* keratitis in a 28 year old man who wore soft contact lenses and cleaned them daily with kitchen water. This patient was referred to our institute from Kerala, south India, for a corneal ulcer of the left eye that was refractory to all antibacterial and antifungal agents. The ulcer was paracentral and ring shaped with raised margins and a severe inflammatory response. Diagnoses of tubercular, fungal, and herpes virus aetiologies were considered, but no bacterial or fungal agent was found or cultured. Instead the wet mounts of corneal scrapings and button showed numerous acanthamoeba cysts measuring 17-19 µm with wrinkled endocysts. Cysts had six or seven ostiols with moderate scallops. The patient did not respond to any medical treatment, and vision could be restored only by keratoplasty followed by local iatracanazole and neomycin.

The acanthamoeba culture (figure) grew luxuriantly and has been maintained for the past three years on non-nutrient agar plates seeded with enteric bacteria. The parasite was identified as *A. culbertsoni* on the basis of morphology and growth at 40°C,^{2,3} which was later confirmed at the National Institutes of Health, Bethesda, USA, by immunological and animal pathogenicity experiments.⁴ As a follow up to this case, we isolated *Acanthamoeba* spp from chlorinated drinking water of a residential campus.

Acanthamoeba is being recognised as a major aetiological agent of keratitis in the United States and elsewhere,^{1,2,4} including India.⁵ This, however, is the first case of *A. culbertsoni* causing keratitis in a contact lens wearer from India. All nine cases reported previously occurred in people who did not wear contact lenses, secondary to physical trauma.^{1,5} Our finding of acanthamoeba in chlorinated water emphasises the resistance of this

organism to chlorination¹; this poses a health threat to contact lens wearers who cannot afford commercial cleaning solutions and use drinking water.

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Limitations of methods of describing populations

Inaccuracies in data must not be forgotten

EDITOR,—F Azeem Majeed and colleagues use standard techniques to transpose descriptions of populations defined one way (as census data by ward) to those of populations defined in another way (practice list by ward).¹ In their discussion they recognise some of the limitations of both the data and the technique. Their observation that they "were not able to validate any of the other census variables for the practices and so do not know how accurately they were measured" by this method deserves emphasis. The only variable apparently accessed by both methods was the percentage of the population aged under 5. The concordance of the two measures of what is in theory the same thing (if the method works) was tested by correlation—a rather odd test, surely—and described as "significant but weak."

The shortage of accurate practice data, and the planners' need to know, is driving many authorities to perform these transformations. Unfortunately, published measures of their accuracy at practice level are scarce, for the same reason. I am concerned that the intensity of our wish to know results may tempt us to make a passing acknowledgment of the unmeasured error and then to take the results as accurate.

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Authors' reply

EDITOR,—Our paper had two main objectives: firstly, to show how routine data could be used to produce a range of patient and practice variables for each general practice in one family health services authority and, secondly, to show how these variables could be used to explain variations in one aspect of a practice's performance (cervical cytology). Although these methods may lead to more flexible ways of planning general practice services and of measuring practices' performance, we agree with Peter Fitton that further research is required to validate them. This is especially important as modern information technology puts such methods within the reach of most family health services authorities and may lead to their

widespread use by people who are not fully familiar with their limitations.

We discussed the limitations of our methods in our paper and suggested some ways in which they might be overcome (for example, by use of census data for enumeration districts). Detailed validation of our methods requires substantial additional research and was beyond the scope of our paper. We compared the estimated percentage of each practice's population under the age of 5 obtained from census data with the percentage obtained from the family health service authority's age-sex register. Because family health services authorities may be more interested in the relative rankings of practices than in the absolute values of any of the patient variables we used correlation to compare the two measures as this allows the linear agreement between the two measurements to be examined. If the absolute agreement is to be compared, however, a more appropriate method would be to use the limits of agreement approach described by Bland and Altman.¹ We aim to address some of the issues raised in this letter in our subsequent work.

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Low serum cholesterol and depression

EDITOR,—S Lori Brown and colleagues conclude that "after several factors relating to health had been controlled for, no significant association between low cholesterol concentration and severe depressive symptoms was found."¹ We remain sceptical about this finding.

Firstly, brain serotonin-1A and serotonin-2A receptors and their functional interactions have been implicated in the pathogenesis of depression.² Preclinical evidence suggests that extreme lowering of plasma cholesterol concentration with fairly high doses of cholesterol lowering agents such as gemfibrozil and cholestyramine alters the functional state of these receptors and their interactions. Another factor that may need to be considered is the duration of the low cholesterol state.

Secondly, in our continuing pilot study in schizophrenic patients we have found considerably lower plasma total cholesterol and low density lipoprotein concentrations in non-responders (patients receiving clozapine) than in responders (patients responding to typical antipsychotic drugs). However, although total cholesterol concentrations were in the range seen in normal controls, low density lipoprotein concentrations were still considerably higher than those in normal controls. These results suggest that the abnormalities of cholesterol metabolism may play a part in generating a refractory state with regard to serotonin receptors in schizophrenia.³ This implies that simultaneous determinations of both high density and low density lipoproteins should also be performed, which was not done in Brown and colleagues' study.

Thirdly, the relation between peripheral and central cholesterol concentrations needs to be addressed. The study's findings seem to support the opinion that peripheral concentrations do not directly indicate central concentrations, although the authors do not discuss this. As all body cholesterol, including that in the brain, is in continual metabolic flux, however, there seems no logical reason why peripheral cholesterol

"*Acanthamoeba culbertsoni*" grown from cornea of a wearer of soft contact lenses who developed keratitis