mixed, or obstructive.³ The implications of apnoea have become widely known to many parents whose infants have experienced apnoeic or cyanotic episodes or who have previously lost infants with the sudden infant death syndrome.

There is a widely accepted dogma that infants who have had life threatening apnoeic episodes and siblings of victims are at increased risk of death.^{4,5} In these groups it would seem, therefore, logical and justifiable to attempt some form of monitoring, and it is surprising that many paediatricians are reluctant to provide respiratory monitors for use at home. In the United States apnoea programmes have become widely established, but often using expensive, technically complex, and sometimes misused equipment.⁶ Even so, it was interesting to read the interpretation of data from 11 Californian monitoring programmes that excessive deaths of high risk infants might have been prevented.⁶

In the United Kingdom we are fortunate to have had available for the past seven years a fairly inexpensive, uncomplicated monitor, originally described by B M Wright.⁷ Abdominal expansion is detected by means of a small pneumatic capsule, whence pressure pulses are converted electronically into audible and visible signals, and an alarm sounds after a measured period of apnoea. This monitor is widely used in neonatal units and paediatric wards. Over a similar period of time I have personally been informed of 165 cases of the syndrome in Leicestershire and have received 110 requests for respiratory monitors (83 for siblings of victims, 27 for babies with apnoea). All of the parents are made aware that monitors cannot guarantee infant survival, but after instruction in the use of the monitor, guidance on mouth to nose resuscitation, and regular clinic attendance virtually all parents have found monitoring immensely reassuring.

It has helped many parents to sleep, reduced overt anxiety, and improved parent-child interaction. Three infants have been resuscitated after apnoea (all with respiratory infections). Another, with bronchiolitis, was brought to hospital because of monitor alarms. He required mechanical ventilation followed by tracheostomy and is alive and well. Two siblings of victims of the syndrome have died (aged 13 days and 19 months), but neither was connected to the monitor at the time and neither was a typical victim of the syndrome. Some infants with more frequent alarms during intercurrent infections have undergone respiratory and metabolic investigations. Fourteen families have requested second monitors for subsequent siblings; five families declined a second monitor.

This experience leads me to conclude that home respiratory monitoring of previously apnoeic infants and siblings of victims of the sudden infant death syndrome is therapeutically important for parents and useful in alerting us to investigate and treat infants found to have more frequent alarms. Nationally more than 6000 pneumatic capsule devices have been bought, a proportion of which are used in home monitoring. To my knowledge, no infant has died while actually using one of these monitors. This surprising statistic—considering that the infants monitored are those at increased risk—suggests that home monitoring in Britain may be helping to prevent some sudden deaths in infancy.

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SIR,—Though I must accept that there is a lack of scientific proof that apnoea monitors help avert death in the siblings of victims of the sudden infant death syndrome, Professor Hamish Simpson fails to bring out one potential benefit of monitoring that cannot easily be assessed in scientific publications (30 May, p 1367).

As the parent of a victim of the sudden infant death syndrome and the subsequent user (currently for the second time) of an apnoea monitor, I should like to emphasise that we are dealing with a situation that concerns not only the sibling, who is possibly at risk, and his or her medical advisers but also the parents. I am sure that we can all imagine the agony of a parent who has suffered the loss of a child by the sudden infant death syndrome and who subsequently has to care for another child who might be at risk. It is bad enough to have to keep a baby under constant surveillance during normal daylight hours without having to try to observe one continuously during the night.

Professor Simpson thankfully does suggest that a decision whether siblings of victims should be monitored has to be made on the merits of each individual case with medical judgment and close family involvement. For many distressed parents the supply of a monitor will enable them to remain sane. While the incessant tick continues the child is still alive.

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Penetration of antibiotics into the respiratory tract

SIR,—We were interested to read the leading article by Dr Jane Symonds (9 May, p 1181), and we agree that it is important to know the degree of lung tissue damage when choosing antibiotic regimens.

We wish, however, to correct an error in the article's reference to Cole *et al*'s publication showing the efficacy of high dose amoxycillin (3 g 12 hourly) in an open study of patients with severe chronic bronchial suppuration.¹ These patients did not suffer from chronic bronchitis, as stated in the leading article, but from severe bronchiectasis. This is important because the severity and extent of tissue damage in such patients is significantly greater than that in patients with chronic bronchitis. Consequently, the high dose of antimicrobial drug was indicated to attempt to overcome such barriers to drug delivery. This has not yet been conclusively shown to be required for the treatment of chronic bronchitis.

We have, in fact, investigated further the efficacy of amoxycillin (3 g 12 hourly) in a double blind, placebo controlled study of 36 patients with bronchiectasis and persistent daily purulent sputum expectoration.² Seventeen of the 36 patients had reported no response to conventional oral antibiotic courses. The patients were randomly allocated to receive either oral amoxycillin or placebo for 32 weeks. During treatment 24 hour purulent sputum volume decreased by a mean of more than half in 11 of 17 (65%) patients in the amoxycillin group, compared with only five of

19 (26%) in the control group ($p < 0.025$). (The complete findings of the study are in preparation.)

This study confirms that a higher dose of amoxycillin than conventionally used has a role in management of bronchiectasis associated with daily purulent sputum.

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1 Cole PJ, Roberts DE, Davies SF, Knight RK. A simple oral antimicrobial regimen effective in severe chronic bronchial suppuration associated with culturable *Haemophilus influenzae*. *J Antimicrob Chemother* 1983;11:109-13.

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Infection by airborne Chlamydia trachomatis

SIR,—The reply from Professor M Midulla and colleagues to our letter (23 May, p 1352) merits further comment.

In addition to describing the first case of alleged tetracycline resistance of and airborne infection with *Chlamydia trachomatis* Professor Midulla and colleagues seem to describe the first case of primary infection in the conjunctiva with spread to the genitalia—that is, chlamydial genital infection as a complication of chlamydial conjunctivitis. This is contrary to the accepted transmission routes of *C trachomatis*. Can the authors suggest the actual mode of spread from the conjunctiva of the doctor to his wife's urethra and cervix?

The *C trachomatis* strains from patients in our care who did not seem to respond to conventional treatment with tetracycline were also tested for antibiotic resistance in vitro. In all cases the minimum inhibitory concentration of tetracycline to the original strain and the resistant strain was precisely similar, indicating no acquisition of resistance but reinfection or non-compliance with the treatment regimen advised. We agree with Dr David Taylor Robinson (2 May, p 1161) that Professor Midulla and colleagues should release the strains so that reference centres can confirm by in vitro testing this potentially important finding of tetracycline resistance.

The authors did not perform tests to exclude chlamydial genital infection from the dentist's fiancée, and thus airborne infection cannot be assumed.

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Successful treatment of asymptomatic endometriosis: Does it benefit infertile women?

SIR,—Carefully conducted studies such as that by Dr Eric J Thomas and Professor Ian D Cooke (2 May, p 1117) make a valuable contribution towards the eventual resolution of the conundrum of endometriosis: its role in infertility. While we do not dispute that the presence of minimal endometriosis or treatment to eliminate the ectopic endometrial tissue may have little effect on fertility, we would make the following comments.

Data from this department¹ have confirmed

previous reports^{2,4} that delayed and reduced secretion of progesterone occurs in a large proportion of women with minimal endometriosis. This abnormality persists after suppression of ovarian function for six months (unpublished findings). It may be, therefore, that this functional abnormality is of primary importance in the pathogenesis of infertility in this group of women and that it could in itself influence the development of ectopic endometrial tissue rather than being a secondary or associated factor.

Accordingly, it would seem reasonable to emphasise the need for careful endocrine assessment of these women, including luteal phase progesterone assays, before labelling their infertility as unexplained. Any abnormality of luteal function may then be identified and necessary treatment started. In these circumstances we would suggest that a biphasic basal body temperature, which does not confirm normal luteal function⁵ and is a poor measure for timing ovulation, as the Sheffield workers have shown, is a somewhat inadequate assessment on which to base a diagnosis of unexplained infertility.

Finally, we would question whether the trial was really double blind. Treatment with gestrinone will cause significant menstrual disturbance, if not amenorrhoea, in most patients, whereas placebo will not.⁶ It may therefore be more appropriate to conduct comparative studies of different treatment modalities rather than comparing treatment with placebo.

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General practice consultations

SIR,—Dr K B Thomas's clinical trial of the therapeutic influence of consultation style (9 May, p 1200) has such important implications that it is unfortunate that it is undermined by serious flaws. The most serious is in the design of the study, which contained a possible bias in the responses given to the postal questionnaire. Patients told that they would get better (positive consultations) may have been reluctant to contradict the doctor (the questionnaire was sent from the practice and would thus be associated with it); patients who had negative consultations would be less inhibited to admit that they were still unwell. What was measured was the patients' reluctance to deny cure, not the rate of cure.

Another source of possible bias lies in the comparability of the two groups. We are not told whether the two groups were equally distributed in their choice of usual doctor. Had the negative consultation group contained, by chance, more patients used to seeing a positive doctor then their

lower rates for satisfaction and self reported cure might reflect expectation based on experience rather than some fixed characteristic of all patients.

Dr Thomas makes unsubstantiated conclusions about patient satisfaction as the reported differences were not significant, and even if they had been significant the conclusions would be irrelevant to clinical practice as the negative consultation chosen in this study does not reflect real shared consultation styles. Telling a patient "I cannot say exactly what the matter is but I am certain that it is not serious and what is more it will get better on its own" is a more accurate example of an honest consultation. It does not deserve to be castigated as negative.

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AUTHOR'S REPLY.—Dr Hopayian suggests that some patients treated positively felt obliged to say that they were well when in fact they were not. If this was so I would have expected an increase in the rates of return of those patients to see a doctor and a decrease in the rates of return to see me rather than another doctor compared with rates in the patients treated negatively. These changes did not, however, occur.

The point of my investigation was that what is said by the doctor in the consultation may influence the satisfaction and the recovery of the patient. Dr Hopayian's suggestion that patients treated negatively who are used to seeing positive doctors have lower rates of satisfaction and recovery from illness surely supports the premise of the investigation. In any case, I showed that patients who chose to see me and who therefore presumably had some experience of my consulting style did very little better, and not significantly better, than those who did not choose to see me. I cannot help wondering how an anxious patient would view Dr Hopayian's statement that her illness is not serious and that it will get better on its own when she has just heard him admit that he cannot say exactly what is the matter.

Finally, I did not "castigate as negative" the shared consultation. I stated that "it would be wrong to assume that this new approach is the same as my negative consultation." On the other hand, it is surely important that the shared consultation should be examined to see what effect, if any, it has on patient satisfaction and recovery from illness. Perhaps I could refer Dr Hopayian to the leading article by Professor Bracken on the importance of controlled trials and quote his remark that we choose to ignore, and even to slay, the bearer of bad news when controlled trials tell us what we do not want to hear about many of our diagnostic and therapeutic manoeuvres.¹

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Regional secure units: arriving but under threat

SIR,—Not all of those who "rushed into building" a secure unit had problems of building design or poor siting, as suggested by Dr Peter Richard Snowden (23 May, p 1310).

In Trent we are satisfied with the building, have been committed to a forensic service, and have had

no problems in forming links with the courts, probation service, prisons, or district psychiatric services. We did, however, have considerable problems in recruiting nurses and have been forced to operate the unit without adequate medical staff cover.

The regional health authority and the Department of Health and Social Security agreed that the service would be established without detriment to the existing psychiatric services and would require a non-consultant establishment both for training and to provide 24 hour, on the spot availability of medical staff. An establishment of two senior registrars and four registrars was agreed, but only one senior registrar post has been established. This is justified in terms of the manpower freeze and the subsequent *Achieving a Balance*.

It is not possible to provide an appropriate therapeutic milieu in a totally closed unit without good medical leadership and availability. We cannot alleviate the current shortage of forensic psychiatrists without more and better training posts. There is surely a strong case to depart from the currently inflexible manpower guidelines to ensure that secure units are properly staffed. Without a proper balance of staff secure units could become dangerous and damaging mini-asylums.

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Refusal to treat AIDS and HIV positive patients

SIR,—Dr Raanan Gillon (23 May, p 1332) seems to have failed to address two important issues. Firstly, it is not a question of whether the patient should be treated so much as how much treatment the patient should receive, and, secondly, there is the question of whether the doctor's attitude should be different if he does not have a monopoly of a particular treatment.

Mr Guy (14 February, p 445) has not refused to treat patients with the human immunodeficiency virus when their condition is life threatening, but where should we draw the line? Most doctors would, I believe, treat symptoms that cause discomfort, but even here the issue is not clear cut. What would the General Medical Council's attitude be if a doctor refused to do a haemorrhoidectomy on a patient who was a promiscuous homosexual with the acquired immune deficiency syndrome and wanted treatment only because of uncomfortable anal intercourse? It seems to me that in many cases doctors may and do refuse patients all that they want because of the doctors' own moral stance. For example, would a doctor be acting morally if he treated a torturer who had hurt his arm torturing somebody and who intended to use the arm to go on with the torturing?

The example of a bigoted gay venereologist assumes that the syphilitic surgeon had no one else to go to. If this were so the circumstances would be more extreme and thus the onus would be on the venereologist to examine his views more carefully. Usually there are other doctors available, and it is well within a doctor's rights to refuse to collude with an activity that he considers to be immoral. For example, if a woman smokes cigarettes during pregnancy the doctor may believe that to continue to supervise her antenatal care could be construed as colluding with unnecessary damage to the fetus, which has no choice in the matter. Other doctors may feel differently, and provided there is another doctor who will take her on it seems perfectly in