Letters

Introducing the postoperative care team

Effective postoperative care remains surgeons' role

EDITOR—I am concerned that the suggestions made by David R Goldhill might be taken too seriously rather than remain a point for discussion.[1] Goldhill is right in his assertion that patients can fare badly in their postoperative course because of lack of attention by their attendants and lack of understanding of common postoperative complications. I believe, however, that he has missed the opportunity to lay the blame at the correct door. Surely it is the surgeons' role to carry out preoperative assessment, surgery, and effective postoperative care. If they choose to emphasise their importance in the operating theatre, delegating their other responsibilities to their trainees, the nurse practitioners, and the anaesthetists, why should we encourage this path?

Undoubtedly, anaesthetists are fast becoming the new generalists, with an increasing role outside the operating theatre. These new skills and responsibilities include pain control, intensive care, and transoesophageal echocardiography, to name but a few. Some of my professional colleagues may relish even more diversification and abhor my comments.

Goldhill implies that anaesthetists and postoperative care nurses will lead the postoperative care team; indeed, this is already happening in North America, where they have been sent to run preoperative assessment clinics by surgeons based in the operating room. If the surgical ward round in your hospital has become a quick look through the laboratory results and a glance at the chest radiograph before a rush to the changing rooms then I suggest that waiting for the confidential inquiry into perioperative deaths to point out the obvious is not the solution.

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1 Goldhill DR. Introducing the postoperative care team. *BMJ* 1997;314:389. (8 February.)

Such teams deserve a trial of effectiveness and cost

EDITOR—I share David R Goldhill's concern over the management of postoperative care in many parts of Britain.[1] Goldhill dismisses high dependency units as being too expensive and "unlikely to be available to most postoperative patients, particularly beyond the first few hours of surgery." Precisely this aspect was addressed in the joint report of the Royal Colleges of Anaesthetists and Surgeons of England on graduated patient care; the report concluded that such an organised system, including adequate high dependency unit facilities, would make better use of present resources and was unlikely to be more expensive.[2]

The use of an itinerant, intermittent postoperative care team may be an alternative arrangement at present, necessary because of architectural constraints making it difficult to provide sufficient intensive care and high dependency facilities to deal with the numbers who require this form of treatment. This idea must first have a trial of effectiveness and cost.

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1 Goldhill DR. Introducing the postoperative care team. *BMJ* 1997;314:389. (8 February.)
2 Royal College of Anaesthetists and the Royal College of Surgeons of England. *Report of joint working party on graduated patient care.* London: RCS, 1996.

Royal College of Surgeons has training programme for surgical trainees

EDITOR—It is of note to surgeons, and perhaps a warning, that both main weekly medical journals in Britain should have taken it upon themselves to chide their surgical colleagues about the standards of care pertaining to operations.[1-3] It is to the credit of surgeons that this topic has been highlighted by the report of the national confidential enquiry into perioperative deaths.[4] The solution suggested in David R Goldhill's editorial smacks of the "modern" answer—if there is a problem then manage it with a team.[1] Thus the poor surgical patient will return from theatre to be seen by the pain team, the nutrition team, the stoma team, the lifting and handling expert, and the postoperative care team. Alas, with all these teams the trainee becomes less skilled and less able to manage ill patients, and the continuing care of the patients becomes less good.

All the points that Goldhill makes are those that most surgeons interested in perioperative care take for granted and are surprised to see highlighted in an editorial. Nevertheless, the fact that statements such as "oxygen therapy is effective at preventing hypoxaemia" and "it may be necessary to take on the challenge of postoperative care" can be made in a serious journal suggests that in some institutions there is a serious lack of training in the management of perioperative surgical patients. To overcome any such deficiency the Royal College of Surgeons has designed a training programme for basic surgical trainees to cover all areas commented on in the editorial. The critical care course emphasises the importance of respiratory, cardiac, renal, and nutritional care in the perioperative period. There is also emphasis on communication, which the editorial omits. Without good communication, management by multiple teams leads to chaos. If critical care and good communication skills are taught to surgical trainees then a comprehensive system of continuing postoperative care will decrease morbidity and mortality. Perhaps the team approach can then wither at inception.

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1 Goldhill DR. Introducing the postoperative care team. *BMJ* 1997;314:389. (8 February.)
2 Cunnion RE, Masur H. Physician staffing in intensive care units. *Lancet* 1996;348:1464-5.
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Advice to authors

We receive more letters than we can publish: we can currently accept only about one third. We prefer short letters that relate to articles published within the past four weeks. We also publish some "out of the blue" letters, which usually relate to matters of public policy.

When deciding which letters to publish we favour originality, assertions supported by data or by citation, and a clear prose style. Letters should have fewer than 400 words (please give a word count) and no more than five references (including one to the BMJ article to which they relate); references should be in the Vancouver style. We welcome pictures.

Letters should be typed and signed by each author, and each author's current appointment and address should be stated. We encourage you to declare any conflict of interest. Please enclose a stamped addressed envelope if you would like to know whether your letter has been accepted or rejected.

We may post some letters submitted to us on the world wide web before we decide on publication in the paper version. We will assume that correspondents consent to this unless they specifically say no.

Letters will be edited and may be shortened.

Better to define and enhance role of ward surgical nurses

EDITOR—Few anaesthetists would disagree with David R Goldhill's concerns over postoperative care.[1] However, the implications and the implementation of his proposals send a shudder through the ranks of anaesthetists.

If we consider establishing peripatetic postoperative care teams then two problems must be addressed. Firstly, the ward nursing and medical staff will be deskilled. Secondly, the natural providers of the medical skills for this role are anaesthetists, who may already be helping to manage postsurgical patients on the wards. The anaesthetists are also, however, already running intensive care and high dependency units, and few need reminding that currently this is a shortage specialty.

Acute pain teams have faced similar problems when they have been set up to manage all the pain problems on the wards. Deskilling of staff and overload of the providers have been the consequence. What sounded like a good idea at the outset now needs reconsideration.[2]

The most important factor in the improvement of postoperative care is to define and then enhance the role of ward surgical nurses. Empowering them to take therapeutic measures (for example, recognising hypovolaemia and initiating transfusion, seeking appropriate experienced help early) will avoid so much of the delay and indecision on the part of inexperienced junior doctors that contributes to morbidity. Similarly, they should be routinely managing 98% of the postoperative analgesia, to promote early mobilisation. Ever shorter stays after major surgery require a coordinated approach by the nursing and surgical team to manage the postoperative period aggressively.[3]

Leadership for this surgical system must come from the senior doctors (that is, consultant surgeons and anaesthetists), and they must be full participants in the process. Junior doctors will participate and learn from all. Without an integrated approach by senior doctors and skilled nurses, however, the system will go on as it is, and wandering postoperative care teams will not be able to patch it up.

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1 Goldhill DR. Introducing the postoperative care team. *BMJ* 1997;314:389. (8 February.)
2 Notcutt WG, Austin J. The acute pain team or the pain management service? *Pain Clinic* 1995;8:167-74.
3 Bardram L, Funch-Jensen P, Jensen P, Crawford ME, Kehlet H. Recovery after laparoscopic colonic surgery with epidural analgesia and early oral nutrition and mobilisation. *Lancet* 1995;345:763-4.

Why people stay healthy

EDITOR—George Davey Smith and Martin Egger explore some theories regarding what makes people stay healthy.[1] The concept of investigating the epidemiology of health as opposed to disease has not interested mainline epidemiologists. It raises many difficult questions not always amenable to quantitative analysis and it moves out into the realms of sociology and psychology, which further complicate matters.

As indicated by the authors, several theories are emerging that strongly promote psychological, economic, and sociological explanations for why people remain healthier in different environments. However, these concepts are far from being new and I was most disappointed that they did not mention Antonovsky's excellent sociologically based work on why people remain healthy despite difficult circumstances.[2]

Antonovsky asked the question: "Why do people stay healthy despite being exposed to seriously adverse circumstances, eg concentration camps?" and developed the sense of coherence theory, which comprises three components: comprehensibility, manageability, and meaningfulness. The ability to make sense of what is going on in our lives, the extent to which we feel able to control this, and a positively dynamic approach to difficult life challenges are the three factors which Antonovsky has identified as maintaining people's health in the face of disaster and even the threat of death.

This work was published in the 1980s and is based on solid research. Surely it is time to stop reinventing wheels and for well developed theories such as that propounded by Antonovsky to be taken on board by epidemiologists who want to unravel the mysteries of disease and ill health.

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1 Davey Smith G, Egger M. Understanding it all—health, meta-theories, and mortality trends. *BMJ* 1996; 313:1584-5. (Commentary on Bunker JP, Stansfeld S, Potter J. Freedom, responsibility, and health. *BMJ* 1996;313:1582-4.) (21-28 December.)
2 Antonovsky A. *Unravelling the mystery of health: how people manage stress and stay well.* San Francisco: Jossey-Bass, 1987.

Exploitative collaborative research must be discouraged

EDITOR—Jair de Jesus Mari and colleagues highlight the advantages of high quality collaborative research conducted in developing countries.[1] They make the case for more collaborative research studies, especially when experience of the disease is likely to be limited in developed countries. Taken at face value, this case is difficult to fault, but I question who its potential beneficiaries are likely to be. There is a general distrust of science from developing countries unless a Western scientist "policed" the studies. Sometimes studies that would not be allowed in developed countries because of ethical problems are undertaken in developing countries, where such stringent conditions may not apply ("The Human Laboratory," BBC2 *Horizon*, 6 Nov 1995). Scientists in developing countries have occasionally had cause to be suspicious of some foreign collaborators because experience has taught them that financial gain and personal advancement rather than altruistic reasons often provide the impetus for collaborative studies with developing countries. There have been instances of foreign research collaborators obtaining knowledge and skills from those with whom they were supposedly collaborating and using these for their own financial gain and to extend their careers by presenting themselves as advocates for the developing countries. A notable example of such a case is that of Dr Aklilu Lemma of Ethiopia, who carried out the original research on the endod berry for the treatment of schistosomiasis, only to see his "collaborators" patent rather than publish.[2] Altruistic collaborative research should certainly be encouraged, but not exploitative collaborative research.

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1 Mari JdeJ, Lozano JM, Duley L. Erasing the global divide in health research. *BMJ* 1997;314:390. (8 February.)
2 Mukerjee M. The berry and the parasite. *Scientific American* 1996;274:18-20.

Persistent fever in pulmonary tuberculosis

Older doctors had more experience of tuberculosis

EDITOR—It is understandable that, with the relatively low level of tuberculosis in Britain, the present generation of consultants is less familiar with the vagaries of the disease than those of us who had experience of it during the massive postwar problem. In the recent Grand Round the patient discussed presented with far advanced disease, clinically and radiologically, as was not uncommon in psychotic patients.[1] His weight is not stated, but the doses given were well below those recommended by the World Health Organisation for a man weighing 33-50 kg. It is notable that his fever subsided when he was later given more normal doses, admittedly intravenously. Possibly he had not swallowed all the doses—a real possibility. Even if psychotic patients are seen to take each dose into their mouth, they sometimes keep them and later spit them out. This could account for the low serum concentrations, though the orange urine suggests absorption at least some of the time.

Nevertheless, in my experience it was not uncommon in such severe cases for the fever to take some weeks to subside. The first paragraph of the article suggests that the fever lasted two months, but figure 1 indicates that it had gone by 34 days and perhaps by 24 days (there is a blank at days 24-34). You should not panic when a response is delayed. If there is no reason in the history to suspect primary resistance (a matter that does not seem to have been explored) and you are giving established treatment, keep your nerve and carry on. In time all will go well. If you start messing about with treatment you may run into trouble. This could have happened here. Drug resistance was suspected and streptomycin

added. This is the classic error. If failure to respond was due to resistance there must have been resistance to all the drugs being given. Adding streptomycin would have amounted to monotherapy. This is how one drug after another can be lost; it is the source of much of the present multidrug resistance in the world.

I am agnostic about the importance of the low blood concentration of rifampicin. I have so often seen malabsorption suggested as an explanation of apparent failure of treatment when in depth clinical assessment showed a failure to give effective standard treatment. Doctors too often think that clinicians may make mistakes but that truth comes out of laboratories. Laboratory workers are also human.

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1 Barakat MT, Scott J, Hughes JMB, Walport M, Calam J, Friedland JS, *et al.* Persistent fever in pulmonary tuberculosis. *BMJ* 1996;313:1543-5. (14 December.)

Several factors were not considered

Editor—The Grand Round on persistent fever in pulmonary tuberculosis deserves comment for several reasons.[1]

Firstly, the patient's weight is not stated. The dose of rifampicin given (360 mg) was probably too low and the dose of isoniazid (150 mg) certainly too low.

Secondly, the possibility of poor bioavailability from the combination product does not seem to have been considered.

Thirdly, the white cell count was uncharacteristically high for tuberculosis at times of fever. Both variables were reduced by intravenous cortisone. This suggests some form of host/organism/drug hypersensitivity reaction.

Fourthly, routine liver function testing was shown to be unnecessary.

Fifthly, the cardinal error of adding a single drug to a failing regimen was committed. Had the patient been resistant to two of the initial drugs, he would have been resistant to the third by the time the fourth (streptomycin) was added, so that he was potentially rendered resistant to the four drugs.

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1 Barakat MT, Scott J, Hughes JMB, Walport M, Calam J, Friedland JS, *et al.* Persistent fever in pulmonary tuberculosis. *BMJ* 1996;313:1543-5. (14 December.)

Public scare has not deterred Finnish teenagers from using oral contraceptives

Editor—The increased risk of venous thromboembolism related to the use of third generation oral contraceptives has been widely discussed in the media since October 1995, about two months before the original research reports were published.[1 2] A few months after the negative publicity an increase of 10-11% in the number of induced abortions was reported from the United Kingdom.[3 4]

Table 1 Contraceptive methods used at most recent intercourse by teenagers aged 15-16 in 1995 and 1996

	Girls		Boys	
	1995 (n=463)	1996 (n=500)	1995 (n=329)	1996 (n=408)
No method	65 (14)	84 (17)	55 (17)	66 (16)
Condom	273 (59)	280 (56)	220 (67)	276 (68)
Oral contraceptives	97 (21)	119 (24)	37 (11)	41 (10)
Double contraception*	26 (6)	17 (3)	14 (4)	22 (5)
Other	2 (0.4)	0	3 (1)	3 (1)

*Oral contraceptives and condom.
For girls χ^2=5.06, df=3, P=0.167; for boys χ^2=0.77, df=3, P=0.85 for comparison of distributions of contraception in 1995 and 1996. Category other was excluded from analysis.

In Finland the rate of teenage abortions has been successfully reduced over the past 10 years, the abortion rate being 9 per 1000 girls aged 15-19 in 1994.[5] To a great extent this can be explained by a sharp increase in the use of oral contraceptives during the 1980s. In 1993, 17% of 16 year olds and 38% of 18 year olds used them.[5] As the pill scare was expected to decrease teenagers' use of oral contraceptives and increase abortions, we compared data from school health surveys carried out six months before and after the negative publicity. In addition, we compared the abortion rates in the first four months of the past three years.

All teenagers aged 15-16 in the ninth grade of comprehensive schools in the city of Turku and in the province of central Finland completed a structured questionnaire, with similar questions on sexual behaviour, in April-May 1995 and 1996. The number of respondents was 2995 (1528 girls, 1467 boys; response rate 83%) in 1995 and 3294 (1690 girls, 1604 boys; response rate 89%) in 1996.

The proportion of teenagers who had had sexual intercourse at least once was the same in both years: in 1995 it was 32% of girls (475/1499) and 24% of boys (335/1408) and in 1996, 30% of girls (502/1671) and 27% of boys (422/1563). In 1996 only 10% (163/1668) of girls and 7% (109/1478) of boys were worried about adverse health effects of oral contraceptives, and there was no difference according to sexual experience or earlier or current use of contraceptives. The distribution of contraceptive methods used at the most recent intercourse did not change significantly (table 1). The proportion of teenagers using oral contraception was 21% in 1995 and 24% in 1996 (table 1). About one fifth of sexually experienced girls had sometimes used emergency contraception in both surveys (19% (92/475) in 1995 and 23% (116/502) in 1996). The number of induced abortions did not increase during the first four months of 1996 (431) compared with the two earlier years (430 in 1994, 452 in 1995), the abortion rates for the four months being 2.7, 2.8, and 2.7 per 1000 girls aged 15-19 years, respectively.

Finnish reactions to the pill scare were generally moderate. In the main, the mass media handled the news in a realistic and responsible way, and health authorities did not hurry to change official guidelines. This moderate line may be a reason why Finnish adolescents seem to have survived the pill scare without having more abortions.

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1 WHO Collaborative Study of Cardiovascular Disease and Steroid Hormone Contraception. Venous thromboembolic disease and combined oral contraceptives: results of international multicentre case-control study. *Lancet* 1995;346:1575-82.
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Sight tests to detect glaucoma

Reliability of screening procedures and effectiveness of treatment need to be assessed

Editor—Though Richard Wormald and colleagues refrain from advocating a nationwide screening programme for glaucoma,[1] any increase in case finding should be exposed to the same scrutiny as are screening programmes and should fulfil most of the accepted criteria for such programmes.

Firstly, can glaucoma be easily detected in its early stages? Anyone working in eye clinics flooded with referrals of patients with false positive results will have their doubts. In reality, patients are referred on the basis of raised pressure or visual field loss or apparent cupping of the optic disc rather than a combination of these three variables. As a result, the specificity of screening for glaucoma is too low for an increase in case finding to be advocated.

Secondly, is blindness due to glaucoma really preventable? There is room for doubt. A recent meta-analysis of trials of medical treatment of glaucoma did not show any protective effect on visual fields.[2] Furthermore, medical treatment of glaucoma has the potential to do harm.[3] In common with most ophthalmologists I believe that bigger and better trials would probably show a protective effect, but the quality of evidence is not yet sufficient to justify expansion of the current screening arrangements.

Before we look again at sight tests we should look again at the reliability of our screening procedures and the effectiveness of our treatment. Only then should we push for an expansion of screening into high risk groups.

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1 Wormald R, Fraser F, Bunce C. Time to look again at sight tests. *BMJ* 1997;314:245. (25 January.)
2 Rosetti L, Marchetti I, Orzalesi N, Scorpiglione N, Torri V, Liberati A. Randomized clinical trials on medical treatment of glaucoma: are they an appropriate guide to clinical practice? *Arch Ophthalmol* 1993;111:96-103.
3 Diggory P, Franks W. Medical treatment of glaucoma—a reappraisal of the risks. *Br J Ophthalmol* 1996;80:85-9.

Entitlement to free sight tests should be reviewed

EDITOR—I share Richard Wormald and colleagues' concern about the failure to detect early glaucoma in elderly people, and I agree that one of the measures to be considered should be a review of the present exemptions from charges for sight tests.[1] These regulations discriminate against certain groups of patients—namely, those with conditions that require frequent and costly changes in their prescription and those who are at increased risk of developing disorders that could be successfully screened for by an optician.

In my view, patients with thyroid eye disease should be entitled to free sight tests. In this condition the extraocular muscles are affected by an inflammatory process that may persist for some years and result in changes in the shape and size of these muscles, as well as changes in their movements.[2] Appreciable swelling of the retro-orbital tissues causes proptosis and influences the shape of the globe, with subsequent changes in the refractive error. The muscles are not always affected simultaneously, hence the need for frequent prescriptions. Changes in eye movements may cause diplopia and the need for prisms, which, if incorporated in the prescription, increase the cost dramatically. Analysis of one patient's records from 1988 to 1994, during which time she developed thyroid eye disease, showed that her glasses were changed six times at a cost of £347.90. In that time she developed 1.25 dioptres of astigmatism in the right eye and 1.5 dioptres in the left eye and the axis varied 40° in the right eye and 30° in the left eye. Finally, the astigmatism disappeared in the left eye and halved in the right.

The assessment of corrected acuity in these patients is of great value. While not all patients need to be monitored in an eye department, it is essential that they are referred urgently if their vision decreases, as this may be the first indication of sight threatening compression of the optic nerve.

Finally, there is a recognised association between hypothyroidism and primary open angle glaucoma. Smith *et al* showed that 23.4% of a group of patients with known primary open angle glaucoma had hypothyroidism.[3] Cartwright *et al* also found that 30% of patients with normal tension glaucoma had immune related disease.[4] Although we do not know how many patients with thyroid disease will develop glaucoma, screening should be considered.

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1 Wormald R, Fraser S, Bunce C. Time to look again at sight tests. *BMJ* 1997;314:245. (25 January.)
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Systemic lupus erythematosus complicated by antiphospholipid antibody syndrome

Young women should be referred to an obstetrician or gynaecologist

EDITOR—I read the Grand Round about a patient with systemic lupus erythematosus and the antiphospholipid antibody syndrome with concern.[1] The absence of an obstetrician and gynaecologist from the discussion group led to inaccuracies and omissions in the management advised.

In women—with or without systemic lupus erythematosus—the presence of antiphospholipid antibodies is the most sensitive indicator of late fetal death[2] and necessitates treatment with low dose aspirin and either subcutaneous heparin at an anticoagulant dose or steroids. Despite the morbidity associated with this regimen, previous fetal loss should not be a prerequisite for its prescription. One study reported a successful outcome of pregnancy in six untreated women with the antiphospholipid antibody syndrome; it used Doppler studies for fetal monitoring.[3] While obstetricians agree that careful fetal monitoring is the essence of successful management in most high risk pregnancies, the relative contributions of surveillance versus treatment in the antiphospholipid antibody syndrome have yet to be studied. No amount of scanning, however, will prevent fetal or neonatal death before 24 weeks' gestation.

The group also failed to mention the importance of future adequate contraception and the role of prenatal counselling. Systemic lupus erythematosus is recognised to have an adverse effect on the outcome of pregnancy, and vice versa, not uncommonly resulting in fetal death and severe maternal morbidity. Timely referral to a gynaecologist not only may prevent unwanted conception but would warn those women who are anxious to conceive of the potential complications of pregnancy. Indeed, disease activity at conception is a major factor in determining the outcome of the pregnancy. It is imperative that colleagues involved in the management of young women with long term medical or surgical disease consider this avenue of referral in all cases.

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1 Cockwell P, Savage COS, Owen JJT, Thompson RA, Gordon C, Adu D, *et al.* Systemic lupus erythematosus. *BMJ* 1997;314:292-5. (25 January.)
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3 Trudinger BH, Stewart GJ, Cook CM, Connelly A, Exner T. Monitoring lupus anticoagulant positive pregnancies with umbilical artery flow velocity waveforms. *Obstet Gynaecol* 1988;72:215-8.

Pulse treatment with cyclophosphamide would have been more appropriate

EDITOR—The Grand Round about a woman with systemic lupus erythematosus merits further comment.[1] When the patient presented at the age of 25 with a malar rash and polyarthritis, tests confirmed the presence of systemic lupus erythematosus, with a positive antinuclear antibody titre and anti-double stranded DNA titre and hypocomplementaemia. Most importantly, she had haematuria and appreciable proteinuria (3.4 g/24 h) suggesting lupus nephritis, but renal biopsy was not performed. The normal creatinine clearance and normal ultrasound scan of the kidneys do not exclude lupus nephritis, and her subsequent treatment with only a non-steroidal anti-inflammatory drug must be questioned. She had to wait a further seven months before a renal biopsy confirmed lupus nephritis (World Health Organisation class III), warranting immunosuppressive treatment. A diagnostic delay of seven months is unacceptable because prompt and vigorous treatment of classes III and IV lupus nephritis is crucial to minimise the risk of permanent renal damage.

Daily cyclophosphamide is not the best choice in a 25 year old woman. Pulse treatment with cyclophosphamide and high dose steroids is the most effective treatment in lupus nephritis.[2] Furthermore, pulse cyclophosphamide rather than continuous daily treatment probably reduces the risk of a premature menopause and bladder toxicity, both of which are important considerations in young women.[3]

Although we accept that this woman may have had secondary antiphospholipid antibody syndrome, venous thrombosis is also a recognised complication of the nephrotic syndrome. The degree of proteinuria and the presence or absence of hypoalbuminaemia at the time she had a percutaneous renal biopsy are not stated. An isolated measurement of the IgM anticardiolipin antibody also has less diagnostic predictive

value than measurement of the IgG anticardiolipin antibody and lupus anticoagulant status, both of which were negative.[4] In this setting we question the decision to maintain the patient on lifelong warfarin, especially with the increased haemorrhagic risk of maintaining the international normalised ratio in the range 3.0-4.5 in this syndrome.[5]

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1 Cockwell P, Savage COS, Owen JJT, Thompson RA, Gordon C, Adu D, *et al.* Systemic lupus erythematosus. *BMJ* 1997;314:292-5. (25 January.)
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Authors' reply

EDITOR—There is no evidence to support treatment of patients with antiphospholipid antibodies who do not have a history of fetal loss, thrombosis, or other features of the antiphospholipid syndrome.[1] Two prospective studies have addressed the treatment of antiphospholipid antibodies in pregnancy for women with two or more fetal losses. Both indicate that low dose aspirin and prophylactic heparin is the treatment of choice.[2 3] There is no good evidence for using steroids in pregnant patients with the antiphospholipid syndrome. Indeed, the inappropriate use of steroids in pregnancy in this disorder may further worsen outcome.[4]

We agree with Robert Llewelyn about the importance of adequate contraception and prenatal counselling in patients with systemic lupus erythematosus with or without an antiphospholipid syndrome. We provide routine contraceptive advice and, with our obstetric colleagues, hold a joint prenatal clinic for all patients with systemic lupus erythematosus.

This patient underwent an urgent renal biopsy on referral to this centre, and we agree with Elizabeth M McDermott and colleagues about the usefulness of an early renal biopsy in the management of suspected lupus nephritis. There is no evidence that prednisolone plus intravenous pulse cyclophosphamide is superior to oral cyclophosphamide; the only controlled study showed no significant difference in renal survival.[5] We believe that two to three months of daily oral cyclophosphamide at a dose of 1.5-2 mg/kg followed by daily oral azathioprine causes less gonadal toxicity and is as efficacious as intermittent pulse cyclophosphamide for two years. We are planning a multicentre randomised controlled study to compare the efficacy and toxicity of these regimens.

Despite having proteinuria the patient had a normal serum albumin concentration. We recognise that venous thrombosis is a complication of the nephrotic syndrome. The duration of anticoagulation treatment is determined by the underlying disease and risk of recurrent thromboembolism. In a patient with a prothrombotic tendency (probable antiphospholipid syndrome) with a potentially fatal ileofemoral thrombosis (her third), five months after warfarin treatment was stopped, we believe that most doctors would accept the risk-benefit ratio of long term anticoagulation.

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The tobacco industry and scientific publications

Challenges on grounds of self evident potential bias are not unfair

EDITOR—Peter N Lee complains about the concern, expressed by George Davey Smith and Andrew N Phillips, that Lee's vested interest in tobacco industry revenue to P N Lee Statistics and Computing Ltd might influence his interpretation of epidemiological evidence.[1 2] But what is unfair about challenges on the grounds of self evident potential bias? *BMJ* journals now require a clear statement from authors on conflict of interest.

Nevertheless Lee has been given the privilege of reply, but he asserts only that he is widely consulted on many issues. Granted, but may we now see an audited statement on the proportion of P N Lee Ltd's gross income from the tobacco industry during the past five years?

Lee is the author of *Environmental Tobacco Smoke and Mortality*.[3] In his conclusion to the preface of this book he states that "There is no convincing evidence that exposure to ETS [environmental tobacco smoke] results in an increased risk of death from cancer, heart disease or any other disease in non-smokers." Would Lee now clarify in what way the tobacco industry supported the publication of his monograph and how much he received?

The problem for Lee and others who depend on revenue from the tobacco industry for a large proportion of their consultancy income is that the industry is clearly determined to corrupt the medical and scientific literature on tobacco and health through funding academics, conferences, publications, and delegates' attendance at events supported by the industry in attractive venues. New initiatives include the establishment of academic posts in prestigious institutions world wide, and especially in regions that are now prime targets for market expansion. The industry's apparently limitless largesse is particularly noticeable in the Asia Pacific, where it is now trying to recruit health professionals as its advocates.

P N Lee Ltd and others that take the industry's commissions will have to find more novel reasons why we should not regard them as its servants and treat their outputs with circumspection.

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1 Lee PN. Many claims about passive smoking are inadequately justified. *BMJ* 1997;314:371. (1 February.)
2 Davey Smith G, Phillips AN. Passive smoking and health: should we believe Philip Morris's "experts"? *BMJ* 1996;313:929-33. (12 October.)
3 Lee PN. *Environmental tobacco smoke and mortality.* Basle: Karger, 1992.

Findings of scientists who were and were not funded by tobacco industry were strikingly different

EDITOR—It is hard to decide which part of Peter N Lee's letter is the most objectionable, but it is worth commenting on three points for the sake of truth.[1] Firstly, Lee whines that George Davey Smith and Andrew N Phillips mention that he receives tobacco industry funding. He implies that they insinuate that this financial support distorts his scientific veracity. But what is wrong with noting the truth about the source of his funding? I suspect that the real problem is that Lee's longtime association with the industry, which for decades has done everything it can to obfuscate the truth, may have had its effect—perhaps subconsciously—on him.

Secondly, to support one of his arguments he cites as prime evidence a report funded by Philip Morris USA.[2] He fails to mention this financial link, incestuous as it is in the context of his letter, despite his presumed search for truth. Furthermore, he does not note how the authors of that report, LeVois and Layard, obtained the data they used. For this information we need to turn to scientists who do not receive tobacco industry funding.[3] They say, "Several years ago the tobacco industry's lawyers obtained the American Cancer Society's CPS [cancer prevention study] data set, ostensibly to help in preparation of the defence of a wrongful death suit against a tobacco company. The industry's lawyers subsequently provided this data set to two consultants, LeVois and Layard, who conducted an analysis of these data, which concluded that passive smoking did not affect the rise of heart disease."

Lee also fails to mention that another group of scientists, not funded by the tobacco industry, examined the same huge data set as the one examined by LeVois and Layard and came up with conclusions strikingly at odds with LeVois and Layard's.[4] These other scientists write that their own results "are consistent with prior reports that never-smokers currently exposed to [environmental tobacco smoke] have about 20% higher [coronary heart disease] death rates."

Thirdly, Lee cites a particular study about smoking and the sudden infant death syndrome, hinting that its "adjustment for numerous risk factors" somehow undermines the evidence linking the syndrome with household smoking habits. The authors of this study themselves conclude, however, "Passive tobacco smoking is causally related to [the sudden infant death syndrome]."[5]

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1 Lee PN. Many claims about passive smoking are inadequately justified. *BMJ* 1997;314:371. (1 February.)
2 LeVois ME, Layard MW. Publication bias in the environmental tobacco smoke/coronary heart disease epidemiologic literature. *Regul Toxicol Pharmacol* 1995;21:184-91.
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5 Mitchell EA, Ford RKP, Stewart AW, Taylor BJ, Becroft DMO, Thompson JMD, *et al.* Smoking and sudden infant death syndrome. *Pediatrics* 1993;91:893-6.

More intensive care unit beds are needed

Editor—Peter G M Wallace and Paul G Lawler highlight the need for regional intensive care unit transfer teams.[1] As members of a regional transfer team based at the Western Infirmary in Glasgow we write to confirm the value of such teams and the need for more beds in intensive care units. The Western Infirmary's transfer team, which was established in 1975, has transferred 3595 critically ill patients, with only one death in transit. We provide a transfer service to over 32 hospitals in west Scotland, serving a population of about 2.5 million. The service is funded by the purchasing boards in the west of Scotland on a pro rata basis. Reviews of our team in the past have shown the importance of skilled staff, suitable monitoring and equipment, and stabilising patients before transfer.[2 3]

It has become increasingly common for us to have to transfer patients because an intensive care unit bed is not available at the referring centre. Such transfers due to lack of beds are termed bedspace transfers, whereas transfers for specialist intervention or an upgrade in care are termed upgrades. We have examined our workload over 1993-6; data on the total number of transfers, number of bedspace transfers, and time for each were collected from the database.

The total number of transfers increased from 343 in 1993 to 459 in 1996. The percentage of bedspace transfers increased from 12.8% (95% confidence interval 9.27% to 16.33%) in 1993 to 32.7% (28.41% to 36.99%) in 1996. There was no significant difference between the time taken for a bedspace transfer or an upgrade (2.30 h *v* 2.46 h, $P > 0.2$). This reflects the time needed for resuscitation and stabilisation regardless of the indication for moving the patient.

The increased workload experienced by our team is likely to be reflected nationally due to the lack of locally available intensive care unit beds. The national bed bureau will speed up the process of allocating beds. Nevertheless, a considerable number of intensive care unit patients who are transferred will continue to be accompanied by inexperienced, ill equipped staff.[4] The setting up of regional dedicated transfer teams is urgently required to reduce the inherent risks associated with the transport of critically ill patients.[5] This should not, however, deflect attention from the need, as our figures show, for more intensive care unit beds. Until this happens, the number of transfers of critically ill patients due to the shortage of locally available beds will continue to rise.

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1 Wallace PGM, Lawler PG. Regional intensive care transfer teams are needed. *BMJ* 1997;314:369. (1 February.)
2 Runcie CJ, Reeve W, Reidy J, Wallace PGM. Secondary transport of the critically ill adult. *Clin Intensive Care* 1991;2:217-25.
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Submucosal haemorrhage—or ruptured nodule in a multinodular goitre?

Ultrasound scan suggested recent haemorrhage in a left upper pole thyroid nodule

Editor—A woman known to have a multinodular goitre and primary hyperparathyroidism presented with a two day history of sudden onset of pain and swelling in the left side of the neck, sore throat, and dysphagia. Two days later she developed bruising over the front of the neck and upper chest (fig 1). Ultrasound examination confirmed the presence of a multinodular goitre and strongly suggested recent haemorrhage in a left upper pole thyroid nodule that had presumably ruptured. Her symptoms and signs gradually subsided over the next few days. She presented shortly after a photograph of a patient with similar symptoms had been reported on by R M Walsh and J T Little in the Minerva section of the *BMJ*; the presumed diagnosis was spontaneous submucosal haemorrhage in the pharynx.[1]

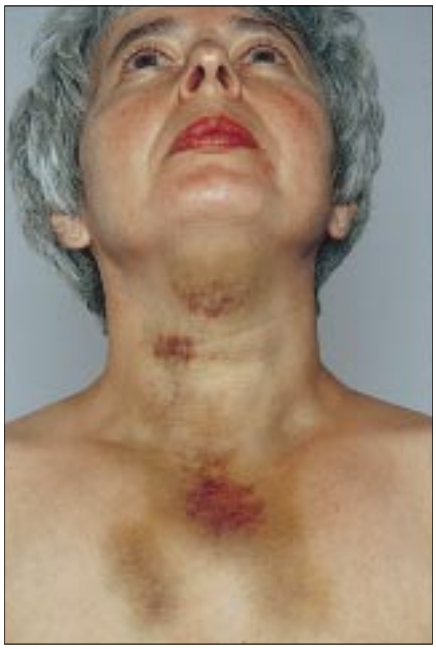

Fig 1 Patient in case (published with patient's permission)

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1 Walsh RM, Little JT. Minerva. *BMJ* 1996;312:1682.

⁂This letter was sent to the authors of the original report in Minerva for reply.

Author's reply

Editor—The subcutaneous bruising with which my patient presented is unlikely to have been due to a ruptured nodule in a multinodular goitre as he did not have a palpable goitre and the subcutaneous bruising extended deeply into the walls of the larynx, hypopharynx, oral pharynx, and indeed into the posterior wall of the nasal pharynx. Blood is unlikely to track this high from the region of the thyroid gland.

However, the aetiology of this patient's pharyngeal and subsequent subcutaneous bruising is still not known, despite thorough ear, nose, and throat investigations and medical tests. The patient remains completely well. I would be grateful for any further suggestions on diagnosis.

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Journals and the internet

Medical journals will continue to be important in prioritising important data

Editor—Over the past three years I have tried to keep track of the developments on the internet that relate to medicine. Although a computer enthusiast, I still find

that few examples of organiser software can match the convenience of a well structured Filofax. The perceived divide between the information superhighway and paper biomedical journals seems to be an artificial one—just as few couples decide on "television *or* radio" or "television *or* film" or "film *or* book." The media, which are seemingly in great competition when a new medium is launched, usually settle with time into a redesigned corner of the market and thereafter develop alongside each other.

The internet is a marvellous phenomenon. There is no other way of conducting a discussion forum among tens or hundreds or thousands across the globe. There is no other means of having such a vast amount of information at one's fingertips for retrieval. At the same time, data gluttony is not the answer. The information overload (and data in printed form are much more responsible for this than the internet is) can be handled only if the important few data are prioritised, sorted, and concentrated on. Medical journals do a great job with this, as Richard Smith says.[1] There is little doubt that they will continue to do so—far beyond the time when obituaries have been published about all of that pre-computer generation, for whom a chip was something you ate with fish.

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1 LaPorte RE, Hibbitts B, Smith R, Horton R, Lundberg GD, Davidoff F. Rights, wrongs, and journals in the age of cyberspace. *BMJ* 1996;313:1609-12. (21-28 December.)

Use of the internet for on line peer review must be explored further

EDITOR—Debate about the role of the internet in medical publishing continues,[1] and some form of democratisation of the peer review process may soon arrive. The impact of an internet based open review of articles accepted for publication is already being explored (http://www.library.usyd.edu.au/MJA/mja), but on line peer review could go further, abandoning both directed submission and selective review.

(1) Articles would not be submitted to any particular journal but placed on the internet in a review forum web site.

(2) Rather than there being reviewers selected by editorial staff, any interested party could comment and suggest modifications. Relevant email comments would be posted alongside any response from the authors.

(3) Authors could modify their work in response to comment at any stage.

(4) Journal editors would be able to offer publication at any stage.

(5) Authors could either accept the first offer of publication or wait for subsequent offers from their preferred journals.

(6) A time limit would be set (for example, six weeks), at the end of which the authors would have to accept an offer to publish or withdraw the paper. Papers not attracting any offers of publication would sink at this stage—although useful data could be archived for future systematic reviews.

Anyone could have early access to new data in their field and the chance to offer constructive criticism. Comments could be either open or anonymous.[2] Papers might attract a mixture of both, the onus being on authors to confront or ignore criticism regardless of its provenance. The maximum duration of review would be six weeks, but well developed papers might be published much sooner, with journal editors essentially competing to publish high impact papers quickly.

For an open on line review forum to work, one or more of the leading hard copy journals would have to break ranks over the notion that placing a paper on the internet in this way constitutes prior publication,[3] rendering the paper ineligible for further consideration. Anxiety about this among journal editors may revolve around the possibility of a shift in the focus of their readership towards the review forum and away from the journal itself. In reality, a healthy symbiosis should develop, with readers using the review forum to sustain an up to the minute, interactive view of their narrow fields of interest but relying on definitive publication in hard copy or electronic journals for the broader context.

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1 LaPorte RE, Hibbitts B, Smith R, Horton R, Lundberg GD, Davidoff F. Rights, wrongs, and journals in the age of cyberspace. *BMJ* 1996;313:1609-12. (21-28 December.)
2 Fabiato A. Anonymity of reviewers. *Cardiovasc Res* 1994;28:1134-9.
3 Kassirer JP, Angell M. The internet and the journal. *N Engl J Med* 1995;332:1709-10.

Copyright must be reconsidered

EDITOR—The points raised by Ronald R LaPorte and Bernard Hibbitts about the relation between the scientific community and journal publishers are particularly valuable as the present, paper based copyright laws are adapted to electronic documents.[1] Technology has the potential to ensure a better spread of medical information to developing countries, but it could make distribution worse if copyright practices become more restrictive.

Many important medical libraries in sub-Saharan Africa have been unable to subscribe to any journals for over 10 years.[2] For these and many others in the developing world, licensing or subscription arrangements for electronic versions of journals offer no improvement on the present situation. For them the most cost effective way of obtaining their information requirements is on an article by article basis; this is improved by the speed with which electronic documents can be delivered.

Current trends in the handling of electronic copyright (especially in Britain) are not reassuring in this respect. In Belgium and Germany, documents requested by individual people for their personal study and research may be scanned by the holding library and sent over the network or internet to the requester. This is also possible in the United States, with some limitations. In Britain most electronic copying is prohibited; photocopied articles for developing countries have, therefore, to be sent by mail, which is often very slow—two to three weeks is not uncommon.

The increasing number of suppliers of electronic documents now entering the market automatically charge the copyright fee demanded by the publisher unless subscription or licensing arrangements are in place. Such practices bypass the "fair dealing" exemption to copyright, which allows an individual person one copy of one article from any one issue of a journal for his or her personal study or research. Ignoring this exemption makes research and study more expensive—a very great disadvantage, if not an absolute deterrent, to those in developing countries.

It is difficult to escape the conclusion that some publishers charge a copyright fee on the basis of what the market in the developed world will stand rather than added value. Transfer of copyright to a publisher needs to be matched contractually by the publisher's responsibilities and obligation to the scientific community. Allowing large areas of the world's population to be unduly disadvantaged through the powers of copyright is one of the issues that need to be addressed.

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1 LaPorte RE, Hibbitts B, Smith R, Horton R, Lundberg GD, Davidoff F. Rights, wrongs, and journals in the age of cyberspace. *BMJ* 1996;313:1609-12. (21-28 December.)
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Regulations on registration of a fetus papyraceus need to be revised

EDITOR—I read with interest the disparate views about the registration of a fetus papyraceus expressed by P O D Pharoah and R W I Cooke[1] and Malcolm Griffiths[2] in reply to my previous letter.[3] I was unaware of the association of fetus papyraceus with cerebral palsy in surviving twins cited by Pharoah and Cooke and am converted to their view that cases of fetus papyraceus should be notified and recorded centrally.

Such notification need not, however, entail the registration of these fetuses as stillbirths, with all that this entails, as the law currently insists. The requirement that Pharoah and Cooke identify could be addressed by retaining the present ruling that all births after 24 weeks be notified to the Office for National Statistics with relevant circumstantial details but agreeing that, in accordance with the World Health Organisation's advice, only fetuses weighing $\geqslant$500 g that are born dead be classified as stillbirths for statistical purposes. Such a measure would also overcome the reluctance of clinicians such as myself to notify a fetus papyraceus: in Pharoah and Cooke's study, clinicians failed to do this in six of 18 cases.

Griffiths's experience was similar to mine and confirms the inconsistency with which the present laws in this regard are upheld. I too had given instructions that the birth be regarded as a singleton, but in my case the parents mentioned the second fetus to the local registrar, who insisted on the death being registered as a stillbirth. It surely cannot be acceptable that the registration of such cases be subject to the whim of such officials or that obstetricians be placed—as Griffiths advocates—in the invidious position of countermanding legal requirements, however "technical" these may be considered.

What is clearly apparent from this correspondence is that current regulations are seriously flawed and in urgent need of revision.

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1 Pharoah POD, Cooke RWI. Registering a fetus papyraceus. *BMJ* 1997;314:441-2. (8 February.)
2 Griffiths M. Registering a fetus papyraceus. *BMJ* 1997;314:442. (8 February.)
3 Heys RF. Selective abortion. *BMJ* 1996;313:1004. (19 October.)

Risk of lung cancer needs to be studied in younger patients who keep pet birds

EDITOR—The prevalence of pet birds and of lung cancer differs between the Netherlands and Sweden. Mortality from lung cancer is much higher in the Netherlands than in Sweden—even higher than that in the United States. Compared with Sweden, the Netherlands has a higher percentage of people who breed birds and a higher concentration of the international bird trade. Breeding birds and keeping birds in family homes result in higher amounts of dust in the indoor air, poorer hygiene, and a greater risk of having infected birds. More young families than old families keep and breed household birds, and breeding is primarily a sport of adult men, not of elderly people. Cecilia Modigh and colleagues suggested[1 2] that the positive results of the earlier European studies[3-5] could be due to the confounding influence of the higher prevalence of ownership of pet birds among the lower socioeconomic classes, who have higher rates of lung cancer. This does not apply to our study in the Netherlands, which adjusted for social class.[4]

There was an important difference in the patients selected for analysis between our study in the Netherlands and the studies in Sweden and the United States. Modigh and colleagues analysed patients of all ages and have not published an analysis of patients aged 65 and under. Our patients were aged 65 and under. During the 10 years of the general practice survey[3] I observed that the percentage of people who kept birds seemed not to be increased among patients with lung cancer aged over 65 in my own and in neighbouring general practices. Elderly people often have a medical contraindication to keeping pets, having previously had lung disease. Moreover, we thought that the influence of variables other than smoking would be easier to see in younger patients, who have not had so much time to accumulate the effects of smoking over large numbers of pack years. Our study among new patients with lung cancer in The Hague[4] was therefore designed to analyse only patients aged 65 and under. Among newly diagnosed patients half are older than 65. To make our results comparable I would therefore ask Modigh and colleagues to analyse the patients in their study aged 65 and under. Because of the size of the Swedish and Missouri studies it should also be possible to analyse the patients aged 60 and under.

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1 Modigh C, Axelsson G, Alavanja M, Andersson L, Rylander R. Pet birds and risk of lung cancer in Sweden: a case-control study. *BMJ* 1996;313:1236-8. (16 November.)
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5 Kohlmeier L, Arminger A, Bartolomeycik S, Bellach B, Rehm J, Thamm M. Pet birds as an independent risk factor for lung cancer: case-control study. *BMJ* 1992;305:986-9.

CS gas is not a chemical means of restraining a person

EDITOR—Peter Trigwell's report about police officers' use of CS gas in an attempt to restrain a mentally ill person concerns me.[1] This action was obviously premeditated because Trigwell was instructed to be prepared to move out of the way "so that we can spray him with the CS gas." CS gas is not a restraining agent but a harassing one, producing a severe irritation on exposed body surfaces—in particular, the external eye, skin, and the mucous membranes of the respiratory tract. People so exposed become highly motivated to escape from the environment contaminated with the agent. CS gas produces disabling symptoms at atmospheric concentrations as low as 0.73 µmol/l,[2] yet a 5% solution of CS gas was sprayed directly into the patient's face. It is my understanding that the action described is contrary to the Association of Chief Police Officers' guidelines on the use of CS sprays and that the safety of these sprays was inferred from the reports of the Himsworth committee after the use of CS gas to control rioting in Ulster in 1969.[3 4] This obviously is a different scenario from that reported by Trigwell, with a population being exposed to low doses of CS gas in open spaces rather than high concentrations of a chemical warfare agent being sprayed directly into a person's face. The patient reported on by Trigwell had probably already received some pharmacological sedatives, which would impair his natural defence mechanisms, such as blinking and coughing; thus he would have been more likely to sustain sequelae of the lacrimatory agent, which may be as severe as a fatal respiratory arrest.[5] The use of CS gas in the circumstances described should be deplored. The gas is not a chemical means of restraining or subduing a person but an effective and safe agent for controlling riots.

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1 Trigwell P. CS gas has been used as chemical restraint in mentally ill person. *BMJ* 1997;314:444. (8 February.)
2 Ballantyne B, Swanston DW. The irritant potential of dilute solutions of ortho-chlorobenzylidene malononitrile (CS) on the eye and tongue. *Acta Pharmacol Toxicol* 1973;32:266-77.
3 Himsworth H. *Report of the enquiry into the medical and toxicological aspects of CS (orthochlorobenzylidene malononitrile). Part 1.* London: HMSO, 1969. (Cmnd 4173.)
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5 Chapman AJ, White C. Death resulting from lacrimatory agents. *J Forensic Sci* 1978;23:527-30.

Talk works—if the patient is willing

EDITOR—That cognitive behaviour therapy works well in certain psychiatric disorders and that there is evidence from good quality research to support this is an important fact forcibly made in Gavin Andrews's editorial.[1] However, to state that "the effective [cognitive behavioural techniques] are as good as drugs" is oversimplistic. Cognitive behaviour therapy (as all psychotherapies) cannot be "prescribed" but depends on the patient being both willing and able to engage actively in the therapeutic process, usually over a period of several weeks. For a number of reasons this is often simply not practicable. In a recent study investigating the use of cognitive therapy in acute psychosis, for example, only 40 of 69 patients satisfying the inclusion criteria out of a sample of 117 inpatients with acute non-affective psychosis could be randomised to the treatment groups.[2] It should also be noted that cognitive therapy in this study was used as an adjunct to pharmacotherapy in acute psychosis, not as an alternative. Similar limitations affect the use of cognitive behavioural approaches in other psychiatric illnesses,[3 4] and this needs to be explicitly stated in any discussion of the use and efficacy of this therapy. Failure to do so gives a distorted impression of an easy and universal applicability of these techniques that is at odds with the clinical realities which not infrequently limit their use.

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1 Andrews G. Talk that works: the rise of cognitive behaviour therapy. *BMJ* 1996;313:1501-2. (14 December.)
2 Drury V, Birchwood M, Cochrane R, MacMillan F. Cognitive therapy and recovery from acute psychosis: a controlled trial. I. Impact on psychotic symptoms. *Br J Psychiatry* 1996;169:593-601.
3 Salkovskis PM, Westbrook D. Behaviour therapy and obsessional ruminations: can failure be turned into success? *Behav Res Ther* 1989;27:149-60.
4 Rush AJ, Shaw BF. Failures in treating depression by cognitive behavioural therapy. In: Foa EB, Emmelkamp PMG, eds. *Failures in behaviour therapy.* New York: Wiley, 1983:217-28.