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Rees, K; Ebrahim, S; (2002) Secondary prevention of coronary heart disease. Ill defined inclusion criteria resulted in missed trials. BMJ, 324 (7337). p. 611. ISSN 1468-5833 DOI: <https://doi.org/10.1136/bmj.324.7337.611>

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Effectiveness of smoking cessation initiatives

Efforts must take into account smokers' disillusionment with smoking and their delusions about stopping

EDITOR—Our survey of a representative national sample of 893 smokers shows that most are disenchanted with smoking and claim that they would not smoke if they had their time again.¹ Furthermore, most smokers overestimate the likelihood of stopping in the future and greatly underestimate how long it is likely to take.

When asked: "If you had your time again would you start smoking?" 83% of current smokers replied that they would not (79% men, 87% women). Those aged 45 to 64 were most regretful, 90% saying that they would not smoke given their time again. This may reflect the mounting distress of smokers reaching the age at which the main smoking related diseases are becoming noticeable in themselves and among their peers. Given the supposedly carefree and rebellious image attributed to teenagers and young adults, young people were also very disenchanted with smoking: 78% of those aged 16 to 24 declared that they would not smoke given their time again.

We also tested expectations about stopping smoking in the future. We asked:

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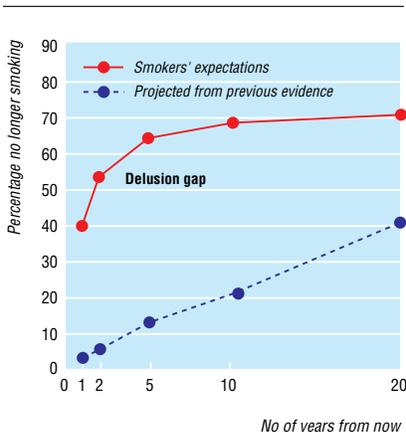
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Smokers' optimism about stopping smoking showing predicted and likely cessation rates

"Looking ahead, do you think you will still be smoking in 1 year's time, or will you have given up?" Those who responded that they would still be smoking were asked the same question looking ahead to 2, 5, 10, and 20 years' time. We checked how realistic smokers' expectations about stopping were by using data from the health survey for England to look at the proportion of ever smokers that had stopped in the equivalent time periods looking back from now.²

The figure shows a delusion gap—a sharp misalignment of expectations about the timing of successfully stopping and the experience of recent history, particularly in the near term—with 53% expecting to stop within two years, but only 6% managing this in recent history. Women were more likely than men to think that they would stop smoking by one year (45% *v* 34%), and younger smokers were more optimistic than older smokers (47% of those aged 16-24 *v* 15% of those aged over 64). Poorer smokers were less likely to think they would have given up by one year (33% among the poorest *v* 47% among the most affluent).

Eighty per cent of smokers under 40 believe that they will have stopped within 20 years; on average they believe they will stop within three years. Recent history shows that only 46% of ever smokers are still smoking at the age of 60.

The widespread disaffection with smoking among smokers combined with their tendency to be deluded about how easy and quick it will be to stop justifies extra urgency in promoting chances to stop. No Smoking

Day on 13 March 2002 is an important opportunity to help smokers take on a realistic view of the difficulties of overcoming nicotine addiction. It will prompt smokers to make a credible attempt at stopping so that they can live the life they would want if they had their time again.

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Smoking cessation services show good return on investment

EDITOR—As Raw et al state, many people around the world have been watching the United Kingdom's smoking cessation services with admiration.¹ We have seen clear evidence from research trials that treatment of tobacco dependence works and is highly cost effective, but the United Kingdom is the first country to try to implement treatment services nationwide. Some might have queried whether sufficient numbers would attend these new services and whether the success rates would be similar to those achieved in research centres. But 127 000 people making an attempt at quitting and 48% achieving short term abstinence is a remarkable achievement in one year.

The resultant cost (< £800 per life year saved) represents excellent value for money by any standards.² In the United States, where much health care is paid for by private health insurance, health insurance companies look for a positive return on their investment in paying for services—that is, they look for cost savings later for dollars invested in treatment now.

Treating tobacco dependence produces a strong return on investment by reducing substantially the high costs of treating myocardial infarctions, cancers, premature births, and chronic respiratory diseases caused by smoking.³ Other positive effects of smoking cessation services include reducing employees' time off work and reducing the number of young people taking up smoking as a result of copying their parents.

Health insurance companies in states such as Utah, California, and Minnesota have realised that treatments with a high return on

investment should receive increased investment. They are thus now expanding coverage (payment) for effective behavioural and pharmacological treatment services similar to those in the United Kingdom.¹

For the NHS to set up such an exemplary service and then allow it to die by not continuing its funding would be truly scandalous. Now that the scheme has been shown to be so successful and has shown such a good return on investment, the minister for health should act to secure increased funding for these services in the long term.

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Commentary: Iloprost for cholesterol emboli syndrome

The following letter was commissioned by the *BMJ* as a commentary to accompany the research pointer published on 2 February. Owing to a failure of our processes we did not publish the commentary alongside the article. We are therefore publishing it now with apologies to Professor Pusey and our readers.

In a recent research pointer Elinav et al describe four cases of cholesterol emboli syndrome treated with iloprost.¹ Their main observations were improvement in distal extremity ischaemia in all cases and improvement in renal function in the one patient with acute renal impairment.

Cholesterol emboli syndrome appears to be increasingly common, perhaps because of more frequent vascular intervention for atherosclerotic disease, but it remains underdiagnosed in life.² Patients often have severe distal ischaemic pain, and they can develop life threatening complications such as renal failure and gastrointestinal ischaemia.³ There is no clear evidence that any specific treatment helps this syndrome, although there are reports of improvement after treatment.⁴ Any new approach to treatment is therefore welcome. Iloprost is a stable prostacyclin analogue with several effects of potential benefit in vascular disease, including vasodilatation and inhibition of platelet aggregation. It is effective in severe leg ischaemia⁵ and in Raynaud's phenomenon secondary to connective tissue disease⁶ and is currently suggested in scleroderma renal crisis. It has not previously been used in cholesterol emboli syndrome.

The diagnosis in the cases described in this report seems straightforward as all occurred after one or more recognised pre-

cipitating factors against a background of severe atherosclerotic disease. Cholesterol clefts were shown on biopsy in all four cases, and eosinophilia was noted in three. The authors describe improvement in ischaemic symptoms and in the appearance of distal ischaemic lesions after iloprost infusions. However, as is necessarily the case in this type of report, there was no control group, so we cannot be certain that these patients would not have improved without treatment.

Support for the role of iloprost comes from the patient whose symptoms worsened when the infusion was stopped and improved again once it was restarted. The improvement in renal function in the fourth patient, however, should be interpreted with caution. This patient developed hypotension sufficient to require an intra-aortic balloon pump and could have had a degree of acute tubular necrosis that would have recovered after haemodynamic improvement regardless of the iloprost infusion. Although the short term improvement during continuous administration of iloprost in the first two weeks is comparatively easy to understand, it is harder to understand why weekly infusions of an agent with a short half life should be of benefit in maintenance treatment.

This report describes a new therapeutic approach with a plausible mechanism of action in a severe and otherwise untreatable condition. The question is whether these preliminary findings provide sufficient evidence to justify a randomised controlled trial. I would like to see the authors' observations repeated elsewhere before advocating such a trial.

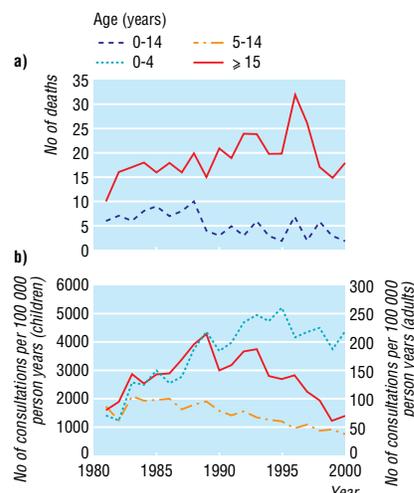
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Deaths from chickenpox

Deaths from chickenpox in adults are decreasing

EDITOR—On the basis of death certificates from the Office for National Statistics from 1995 to 1997, Rawson et al conclude that deaths as a result of chickenpox are increasing in adults in England and Wales.¹ More up to date figures from the Office for National Statistics, however, show that chickenpox mortality is decreasing in adults



Deaths from chickenpox (a) and annual consultation rate for chickenpox (b) in England and Wales, 1981-2000

(from 32 deaths in 1996 to 18 in 2000—see figure (a)). Furthermore, the number of deaths from chickenpox and case fatality rates were significantly higher in 1995-7 (period of the analysis) than at any other period. The claim by Rawson et al that deaths in adults are rising is therefore misleading.

The change in age related varicella mortality is the result of a shift in the age distribution of infection. Over the past two decades there has been an increase in cases in the youngest age group (possibly due to greater attendance of day-care).²⁻⁴ Over the same time period there has been a gradual increase in reported incidence in adults, which peaked in the late 1980s and has been falling since (figure (b)). This is broadly reflected in the gradual decrease in deaths in adults during the past decade. The exception to this trend are 1996 and 1997—exactly the time period when Rawson et al performed their study. What has caused these large shifts in the incidence of varicella in adults is still largely unexplained.

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We would like to thank the Office for National Statistics, and Douglas Fleming and the Birmingham Research Unit, Royal College of General Practitioners, for data.

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Epidemiology of chickenpox in United Kingdom needs further investigation

EDITOR—Rawson et al highlight the potential severity of chickenpox.¹ They say that the age distribution of chickenpox is changeable. But recent data from Scotland, England and Wales, and the United States show that the previous shift towards increased infection in older age groups has not been sustained.²⁻⁴ In recent years the trend has been towards decreased age at infection, with most cases now occurring among the group aged 1-4 years, rather than among children of school age.

Varicella vaccine is recommended for routine administration in the United States and Canada, among other countries, but its suitability for inclusion in the United Kingdom's childhood immunisation programme is still being considered. Further work on the epidemiology of chickenpox in the United Kingdom is therefore now particularly important.

We have proposed a one year period of enhanced active surveillance for severe complications of varicella in children admitted to hospital throughout the United Kingdom and the Republic of Ireland, using the British Paediatric Surveillance Unit's orange card scheme.⁵ The information gained, together with that of Rawson et al, and others, would help to determine the advisability of a universal programme for the United Kingdom, and provide a baseline against which to evaluate its impact should it be adopted.

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Healthcare workers should not be forgotten

EDITOR—We agree with the conclusion of Rawson et al that, although deaths in adults from chickenpox have increased in number and proportion, this does not justify mass immunisation with varicella vaccine.¹ One population, however, that would clearly benefit from vaccination is susceptible health-care workers.

At St George's Hospital in London we identified a total of 25 cases of chickenpox in staff and students from data prospectively collected over the past three years. We were able to determine the country of birth in 22 of these and found that most cases (13/22 (59%)) occurred in people born outside the

United Kingdom. This figure was higher than expected since only 39% of the St George's workforce who have contact with patients are black or from an ethnic minority. Since Rawson et al found that there was a disproportionately higher mortality among such people compared with those born here, it would be interesting to know if occupations, such as those in health care with a higher likelihood of exposure, were over-represented among the cases of fatal varicella.

Live attenuated varicella vaccine has been in use now for over two decades.² Moreover, it has had a licence for use in susceptible individuals in the United States since 1995 and has an excellent safety and efficacy record.³ We believe that the increased mortality from chickenpox in adults of working age of between 1:1000 and 1:5000 shown by Rawson et al may make it indefensible for NHS trusts not to offer varicella vaccine to their susceptible staff for two reasons: personal safety at work and nosocomial chickenpox. If only medical and nursing staff and students had been vaccinated in the last three years at St George's, 85% of chickenpox cases in hospital staff would have been prevented.

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Chickenpox associated morbidity may be long term

EDITOR—Rawson et al analysed deaths from chickenpox during 1995-7.¹ It has previously been recorded that chickenpox in healthy adults has a 25-fold greater risk of complications than in children.² Rawson et al show a significant mortality of chickenpox in England and Wales but do not address the question about associated morbidity, an important issue when addressing the value of immunisation on a population. We recently performed a prospective study on respiratory function in adult patients with chickenpox admitted to a subregional infectious diseases unit in a United Kingdom hospital over a period of 29 months.³

Sixty six adult patients with chickenpox were admitted to hospital during the period, four of whom were immunocompromised. Thirty eight patients fulfilled the study protocol and of these, 50% had radiological evidence of pneumonia (all immunocompetent).³ Three female patients required admission to intensive care unit, two of whom were pregnant. One patient presented with chickenpox encephalitis, and five had superimposed bacterial skin infections.

Severe respiratory disease was associated with the presence of new respiratory

symptoms, close contact with the index case, and a history of current smoking. On follow up at a year post-infection, 37% of patients with radiological pneumonia and 10.6% of those without pneumonia continued to have reduced single breath carbon monoxide transfer factor.

This effect was independent from the effect of smoking and may indicate permanent lung damage. It may therefore be that the morbidity relates not only to the acute infection and admission but also to longer term effects on the lung function, but the exact clinical relevance of our findings is uncertain at present. The study does, however, indicate that chickenpox causes significant morbidity in adults, which may be seen increasingly in the future. Accurate data on morbidity as well as mortality are required to inform the debate on the value of mass vaccination for chickenpox in the United Kingdom.

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Extracorporeal membrane oxygenation has important role

EDITOR—Rawson et al highlighted the potentially devastating effects of varicella infection, particularly the fact that adults in the United Kingdom are dying from it and these deaths are increasing in number.¹ We know the pneumonitis caused by varicella infection can lead to respiratory failure that is often the cause of death in these patients. Antiviral treatment may help in such patients, but only if their severely compromised physiology can be adequately supported until they recover.

Extracorporeal membrane oxygenation has been reported to be used successfully in cases of adult respiratory failure resulting from varicella pneumonia and we would like to bring the results of such intervention to the attention of Rawson et al.²⁻⁵ We have treated 15 adults with this procedure for confirmed varicella pneumonitis in Leicester between August 1992 and December 1999. These 15 patients had a mean age of 36 years (range 24-61), and were significantly hypoxic on referral with a ratio of arterial oxygen tension to fraction of inspired oxygen (PaO₂/FiO₂) of 8.09 kPa. The overall survival rate in these patients was 60%. Of the 11 patients, however, we treated with venovenous extracorporeal membrane oxygenation the survival rate was 75% (compared with zero for the four patients treated with venoarterial extracorporeal membrane oxygenation).

It seems likely, therefore, that this is a treatment that should be considered for fulminant varicella pneumonitis, but the numbers treated so far are too small to be sure of the effectiveness of this invasive treatment. To resolve this uncertainty, currently all such cases in the United Kingdom can be referred for entry into the CESAR (conventional ventilation or extracorporeal membrane oxygenation for severe adult respiratory failure) trial. Suitable patients will be randomised to receive either extracorporeal membrane oxygenation or continued conventional ventilation. Further details about the trial are available from www.cesar-trial.org.

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Secondary prevention of coronary heart disease

Ill defined inclusion criteria resulted in missed trials

EDITOR—The review by McAlister et al of secondary prevention programmes in coronary heart disease does not adhere to some of the major principals of good practice when conducting systematic reviews of the medical literature.¹⁻³ These include a clearly defined research question, strict inclusion criteria so that the review can be replicated, an exhaustive search of the medical literature to find all relevant studies, and findings that can be interpreted easily by the reader and relate to clinical practice.

McAlister et al, at first glance, have chosen a huge area of the medical literature to review, encompassing both pharmacological and non-pharmacological interventions for the secondary prevention of coronary heart disease. On closer inspection, they say that it is their intention to review the literature concerned with disease management programmes for coronary heart disease. The

definition of disease management programmes used is broad and is quoted as that proposed by Hunter et al as a combination of patient education, provider use of practice guidelines, appropriate consultation, and supplies of drugs and ancillary services; from the same source, Hunter et al also say that the spectrum of disease management extends from health promotion and disease prevention, through diagnosis, treatment and rehabilitation to long term care.⁴ This highlights the need to be absolutely explicit about the inclusion and exclusion criteria applied to a review in this area.

The trials included in the review are a mix of nursing and multidisciplinary team interventions, and also comprehensive cardiac rehabilitation programmes. Single modality rehabilitation programmes were excluded. Many trials of comprehensive cardiac rehabilitation—which should be considered admissible under the umbrella of disease management programmes—are missing from studies included in the review. A recent Cochrane review of exercise based rehabilitation for coronary heart disease, which was not picked up with the search strategy used by McAlister et al, cites at least 17 trials concerned with comprehensive cardiac rehabilitation that could have also been included in the review by McAlister et al.⁵ Including these trials in the current review results in a pooled odds ratio for all cause mortality of 0.87 (95% confidence interval 0.76 to 1.0).

A precise definition of disease management programmes is problematic, but using the authors' own description, it seems that deficiencies in searching and application of inclusion criteria have resulted in a review that that is difficult to interpret, is not replicable, and is potentially misleading.

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Secondary prevention programmes may reduce overall mortality in high risk patients

EDITOR—In a meta-analysis of randomised trials, McAlister et al showed that secondary prevention programmes reduce admission to hospital and improve quality of life or functional status in patients with coronary heart disease, but their effects on patients' survival remained uncertain.¹ In primary prevention through lowering blood pres-

sure, the absolute risk reduction is positively related to the overall risk profile of the patient.^{2,3} This implies that in combining patients at high and low risk the meta-analysis may have missed an effect in the latter group of patients. We reassessed the risk reduction in overall mortality by dividing the trials equally into two groups according to the proportion of death events in the control group, an indicator of how likely a patient may die in the absence of the prevention programmes.

Heterogeneity in the risk difference and the odds ratio on a logarithmic scale was not significant ($P > 0.20$) in the 10 trials that provided data on mortality. The mortality in the control group of the trials varied from 1.9% to 19.1%. Although the trial by Cupple et al showed a significant difference in mortality between the intervention and control group, neither the combined overall odds ratio nor the risk difference was significant. A risk of 7% divided the trials into two groups with an equal number. The combined risk difference, according to the fixed effect method, was 0.3% (95% confidence interval -0.7% to 1.3%) in the five low risk trials and -2.7% (-4.6% to -0.8%) in the five high risk trials.⁴ The two confidence intervals did not overlap, and the difference between the patients at high and low patients was significant ($P = 0.01$). In addition, the combined odds ratio was also significantly lower in the high risk trials than that in the low risk trials ($P = 0.05$).

These results suggest that secondary prevention programmes may reduce the overall mortality in patients at high risk but not in those at low risk. This reanalysis provides an example in which genuine clinical heterogeneity may still exist even when the overall heterogeneity is insignificant, heightening the importance of exploring the sources of heterogeneity in meta-analysis.⁵

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Improved outcomes need to be defined

EDITOR—The paper by McAlister et al on secondary prevention programmes in coronary heart disease was highlighted in Editor's choice and This week in the *BMJ* with the teaser that it showed that disease management programmes in coronary heart disease prevention can improve outcomes.¹

What outcomes does the paper actually measure? Reinfarction rate? No. Overall mortality? No. McAlister et al point out that the study periods were too brief. Rate of admission to hospital? Debatable. Only two of the six reporting studies showed benefit and not to level of significance. Quality of life? Some. Again, significance not shown. Processes of care? This is an interesting one. McAlister et al suggest that this means the recording of risk factors and the prescribing of drugs.

The bottom line is the claim that improved outcomes means that the intervention groups are prescribed more drugs and that that this is a good thing, claiming that the overall benefits of the drugs are already proved. Is this what improved outcomes really are? The consumption of a larger number of drugs to treat lipids, platelets, and blood pressure readings—where are the patients in all this? What is their experience? What about adverse reactions and side effects of the drugs? What were the improved outcomes of those who took cerivastatin and developed rhabdomyolysis?

We all want to achieve improved outcomes, but it would be more helpful if the *BMJ* were to be more specific in defining its terms. I know from this article that the described interventions lead to the prescribing of more drugs. I do not know whether these programmes are to be recommended.

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1 McAlister FA, Lawson FME, Teo KK, Armstrong PW. Randomised trials of secondary prevention programmes in coronary heart disease: systematic review. *BMJ* 2001;323:957-62. (27 October.)

Advances in virtual reality are wide ranging

EDITOR—McCloy and Stone have reviewed virtual reality in surgery.¹ Providing an updated and clear picture of a field that changes and advances continuously is hard, and we would like to point out some other things that virtual reality offers to surgery. Applications are likely to determine

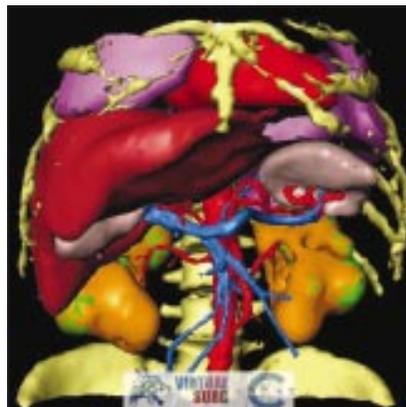


Fig 1 Automated 3D liver reconstruction



Fig 2 Augmented reality: anatomical structures seen through virtual transparency

advances not only in surgical training but also in operative planning, operative strategy, and techniques, as well as in telesurgery.

Systems that allow the assessment of psychomotor skills hold great interest for surgical training. We believe, though, that training in live tissues is still preferable, as it ensures interaction with organs and vessels, which is the core of surgical performance. Only by the future development of systems with full anatomical simulation of organs and operations will the need for training on animals be avoided.

Current applications of virtual reality in preoperative diagnostics include gastroscopy, bronchoscopy, and colonoscopy. Some authors have suggested that virtual colonoscopy may be better than barium enema for detecting polyps in the colon.² Virtual colonoscopy has the additional advantage of allowing navigation in the bowel lumen and views of the mucosa from any angle, as well as the possibility of passing through stenosis and even crossing the colon wall into adjacent structures.² These advantages and the ease of interpretation because of the wealth of imaging information might render virtual colonoscopy especially suitable in screening programmes for colorectal cancer.

The development of systems for 3D reconstruction of liver anatomy and hepatic lesions improves the localisation of tumours and operation planning.³ At the European Institute of Telesurgery we have developed fully automated software that, from computed tomographic scans and magnetic resonance images, quickly provides an accurate 3D reconstruction of anatomical and pathological structures of the liver as well as invisible functional information such as portal vein labelling and anatomical segment delineation (fig 1). Use of this method in over 30 patients has shown that automated delineation of anatomical structures is more sensitive and more specific than manual delineation by a radiologist.

Our group is also developing an augmented reality system allowing the 3D reconstruction of anatomical structures to be superimposed on the patient. For instance, 3D reconstruction of the vessels can appear on the visible surface of the liver through a virtual transparency (fig 2). A further step will then be to combine augmented reality and robotic systems to allow automation of surgical operations.

Although costs are a limitation at present, we think that the advances in virtual reality and the possibility of obtaining experts' help from a remote site through the use of a robot⁴ represent the best means to improve surgical care worldwide.

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News article on report about drug researcher was biased

EDITOR—I wish to draw attention to an error in an article by Spurgeon.¹ The drug company Apotex was sponsoring research into a new drug, deferiprone, and terminated a contract with one of the researchers because of several matters, but not as a result of her publishing her claims about the investigational drug.

The researcher declared her intention to publish her claims only after the company had terminated her contract. Had Spurgeon contacted other parties involved in this controversy he would have realised that the information on which he reported was inaccurate. Instead, he seems to have relied on a report commissioned by the Canadian Association of University Teachers, a union of teachers that was supporting the researcher.

Even if the union's motives had been to establish the truth I question how the committee, selected by the union, could have investigated a complex matter by obtaining direct input from only one of the four major parties concerned. Though it may have had access to some documents, particularly those provided by the researcher in question, it did not have many of those belonging to the other parties.

Because of the lack of a balanced input the report seems merely to represent the researcher's pleadings. Apotex agreed to provide input if the panel would investigate specific matters pertaining to the case, and on which it was based, but such assurances were not provided. This led Apotex to conclude that the assessment might not be truly independent—a fact borne out by the report.

Although the report acknowledges that three of the four parties did not participate in the inquiry, Spurgeon's article failed to make this bias clear. The pro-researcher position of the report is evident throughout. For example, it failed to note that the researcher did not acknowledge in her publication that hers was a dissenting view among scientists who were studying the drug and that subsequently she and her supporters attempted to prevent publication of other scientists' views of the drug.

The researcher's claims regarding deferiprone were evaluated by the European Agency for the Evaluation of Medicinal Products in a special hearing, in addition to the standard assessment of safety and efficacy that is part of the regulatory process for the marketing authorisation of a new drug. After this evaluation the agency recommended the approval of deferiprone, a decision that was then authorised by the European Commission. This approval was particularly important because deferiprone was the first orally active iron chelator approved in Europe. Patients who were unable to tolerate the only other treatment (an injectable drug) now have an alternative.

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Dysphagia

Causes of high dysphagia should be assessed by ENT surgeons

EDITOR—There are serious omissions in the section on high dysphagia in Owen's article on dysphagia.¹ It seems ill considered to discuss the causes of upper oesophageal dysphagia without proper consideration of the possibility of head and neck malignancy. We acknowledge that the possibility of oesophageal malignancy is mentioned in the accompanying box, but no mention is made of head and neck cancer in the main text.

The incidence of head and neck cancer is increasing.² Most patients with such cancer present with a combination of symptoms, of which cervical, or high, dysphagia is cardinal. It is imperative that patients presenting with high dysphagia are seen by a doctor experienced in assessing the upper aerodigestive tract, such as an ear, nose, and throat surgeon.

The suggestion that radiology is more rewarding than endoscopy in the investigation of high dysphagia is misleading.

Radiology—in particular, contrast studies—is of little value in the investigation of head and neck malignancy.³ As specialists, we visualise the pharynx and upper oesophagus with mirrors and a flexible laryngoscope and use rigid endoscopy under anaesthesia, which is the gold standard for investigation of this difficult area.

In summary, patients presenting with high dysphagia with no obvious neurological cause should be referred promptly to an ear, nose, and throat surgeon.

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With endoscopic pouch stapling, pouch excision is no longer necessary

EDITOR—I take issue with Owen's statement that for patients with a pouch, excision is the treatment of choice in most cases.¹ In 1992 Newbegin and Van Hirsch described the endoscopic stapling technique, and this has been widely adopted by ear, nose, and throat surgeons. A key message from the Royal College of Surgeons' report into perioperative deaths in 1996-7 was that the endoscopic stapling technique should become the method of choice and should be performed by surgeons with a special interest in pouch surgery.² The reduced morbidity and mortality associated with the technique led to this conclusion.

A review of the surgical management of pharyngeal pouch in England and Wales in 1997 showed that most general surgeons were cross referring patients to an ear, nose, and throat surgeon with a special interest in this type of surgery.³ Favourable long term results have now been reported (PR Counter, DL Baldwin, annual meeting of British Association of Otorhinolaryngologists-Head and Neck Surgeons, London, September 2001). A few centres have also achieved good results by using an endoscopic technique and a laser to divide the cricopharyngeal bar.⁴

I would say that the good results and minimal morbidity associated with endoscopic pouch stapling mean that there is no longer a place for pouch excision.

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Psychiatrists' perspective is insufficient to root out racism

EDITOR—Minnis et al in their vignette based report claim that racial stereotyping that occurs at the first psychiatric interview is insufficient to account for the inequalities in diagnosis of schizophrenia between black and white men in the United Kingdom.¹ Their findings are not surprising. What people say and what they do are often two quite separate things. Respondents may have consciously or subconsciously over-compensated for their prejudices in the current climate of sensitivity to racial issues and because previous studies in psychiatry have used similar investigative formats.²

The clinical evaluation that a psychiatrist performs is not simply a list of objective facts. It is a subjective account of an interaction between two people. This interaction is fashioned by the perceptions of the evaluator and the importance he or she chooses to give to certain information. The relative importance of different parts of the history depends on the culture of the speciality of psychiatry and the individual psychiatrist's beliefs, understanding, value judgments, and prejudices. This reliance on preconceived beliefs is likely to be especially important at the first meeting with a patient. It is difficult for the assessor to acknowledge these beliefs, let alone for them to be elicited in a vignette study.

Studies have shown that, although initial pathways to psychiatric care are similar for different, British, ethnic minority groups, the subsequent care given to patients of African and Caribbean origin is more coercive and their service related outcome is poorer.^{3,4} There is something about the interaction of these groups of patients with psychiatric services that leads to their detriment.

Patients of African and African-Caribbean origin in the United Kingdom often have a negative experience of psychiatry. Unpublished work in our department has shown that this is more likely to be attributed to racism by the individual. A way forward may be to assess the service-users' perceptions of discrimination and use these to improve services. Attention to this may improve the therapeutic alliance and outcome. Racism is, however, a complex, multi-faceted process and likely to affect the outcome of therapeutic interactions at the interpersonal, institutional, and community level.⁵

The study by Minnis et al is a welcome start, but racism needs to be examined on several different levels and from a number of different perspectives if we are to produce a truly equitable service.

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Management of *Helicobacter pylori* infection

Treatment of ulcers can be improved and over-reliance on proton pump inhibitors reduced

EDITOR—Harris and Misiewicz in their review of managing *Helicobacter pylori* infection take a balanced view of the contentious issues surrounding treatment in patients without ulcers.¹ But several inconsistencies are apparent in their approach to patients with ulcers, for whom solid evidence for treatment is available.

Harris and Misiewicz advocate only two attempts at eradication in patients with duodenal and gastric ulcers; failure of the second attempt is followed by maintenance treatment with antisecretory drugs. Although the 90% success rate for one course of eradication treatment is ideal, combined data from randomised controlled trials suggest that eradication rates of 73-87% are more usual.² In everyday practice, rates of 64% or lower may be expected, depending on the regimen and interest of the clinician.³

After two courses of treatment, potentially one in eight patients may still be infected, and failure to persevere with eradication denies these patients a treatment that alters the natural history of the disease by preventing recurrence and haemorrhage of ulcers. Continuous antisecretory treatment is less convenient, less effective, and more costly; strategies must therefore be constructed to improve overall eradication rates.

In the light of this need to optimise success, Harris and Misiewicz's bias towards treatments based on using proton pump inhibitors and reluctance to endorse ranitidine bismuth citrate is surprising. Ranitidine based triple treatments are at least equivalent, and in some cases significantly superior, to regimens based on proton pump inhibitors as initial treatment.² Bismuth based regimens (either ranitidine bismuth citrate or colloidal bismuth) seem superior to others after an initial failure.^{3,4} In this situation, triple therapy with ranitidine bismuth citrate, tetracycline, and metronidazole produced significantly better eradication rates than the quadruple therapy with proton pump inhibitors and bismuth advocated by Harris and Misiewicz.⁵ Treatments based on ranitidine bismuth citrate are effective and equivalent in cost to those using proton pump inhibitors, but Harris and Misiewicz did not recommend their wider use.

Few data are available concerning third line treatments. After using regimens containing clarithromycin and nitroimidazole, there is no logical combination. Although a combination regimen using proton pump inhibitors, rifabutin, and amoxicillin seems promising in this situation, treatment directed by endoscopy, culture, and sensitivity testing seems better than empirical choice.³ More than 98% of patients requiring *H pylori* eradication can be successfully treated using a three step algorithm, removing the need for continued drug treatment.³ Strategies for *H pylori* eradication should not be based merely on first line eradication rates but include further steps to maximise success in those who will definitely benefit from treatment.

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Competing interests: IB has received research funding from AstraZeneca and financial support and sponsorship for educational activities from AstraZeneca, Janssen-Cilag, and Wyeth. IB has investments in a number of unit trusts which have holdings in several pharmaceutical companies including AstraZeneca and GlaxoSmithKline.

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Eradication treatment can be tailored in patients undergoing endoscopy

EDITOR—In their review of the management of *Helicobacter pylori* infection Harris and Misiewicz suggest that patients likely to have metronidazole resistant *H pylori* infection should be treated with non-nitroimidazole containing eradication regimens.¹ But prediction of resistance to antimicrobials in *H pylori* infection relies on the availability of resistance data in the given geographic area of practice. Few hospitals carry out sensitivity testing of *H pylori*, therefore local data are seldom available. In addition, although Harris and Misiewicz point out that metronidazole resistance is commoner in women and patients from developing countries, translation of this knowledge into prescribing practice is difficult in treating individual patients.

Our study of 1064 consecutive patients in Sheffield found to be *H pylori* culture positive at endoscopy showed metronidazole resistance rates to be 45% for women compared with 37% for men (odds ratio 1.48; 95% confidence interval 1.15 to 1.91). For patients >60 years resistance was 34% compared with 44% for patients <60 years (odds ratio 0.62; 0.48 to 0.8).² A pragmatic approach to *H. pylori* eradication may be to exclude nitroimidazoles from regimens.

This would not be advised, as metronidazole is a cheap and effective antibiotic when used in regimens to treat metronidazole sensitive strains. Also, increased use of clarithromycin, as a replacement for metronidazole, is likely to result in more disruption and induction of resistance in host microflora and thereby reduce the efficacy of macrolides in the treatment of other infectious conditions.³

Culture and sensitivity testing of *H pylori* is well established and requires few special facilities. As Harris and Misiewicz discuss management of *H pylori* after endoscopy, it seems appropriate to recommend taking an additional biopsy at the time of procedure for microbiological culture. The patients could be treated with proton pump inhibitors if indicated, while sensitivity results are obtained (about one week), and then prescribed a specific, effective eradication regimen. In this era of increasing resistance to antimicrobials optimisation of treatment is of paramount importance in clinical practice.

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Dental plaque is a potential reservoir of *Helicobacter pylori*

EDITOR—In their review of the management of *Helicobacter pylori* infection Harris and Misiewicz do not mention the potential reservoir of *H pylori* in dental plaque on teeth.¹ Because this is a biofilm, no antibiotic will penetrate it, and if it carries the organism, it must be removed mechanically with oral hygiene, scaling, and root planing—exactly as for periodontal diseases caused by plaque micro-organisms.²

The effect of removal and control of dental plaque on the plaque reservoir of *H pylori* was shown clearly in a short study.³ Triple therapy alone (omeprazole, clarithromycin, and metronidazole) was wholly ineffective, but scaling followed by chlorhexidine mouthrinse eradicated *H pylori* in 80-90% of patients. Everyone dealing with bacterial diseases should remember that if biofilms are involved, antibiotics alone are unlikely to be sufficient treatment. Similarly, the mouth rinse is useless without the scaling.

Biofilm, like dental plaque, is a ready source for reinfection. They are complex communities of many bacterial species with

powerful defences against chemical and pharmacological threats, but some organisms may not gain a foothold in them because of bacterial antagonisms.⁴ This means that not all patients with *H pylori* infection will necessarily have the organism in their dental plaque, which may have misled some investigators in the past. Reinfection from plaque will also be subject to variable host defences and therefore not occur consistently in all cases in which conventional *H pylori* treatment is used.

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Clinical databases can complement controlled trials

EDITOR—We were interested to read the article by Padkin et al on the serendipitous use of the database of the Intensive Care National Audit and Research Centre (ICN-ARC) to complement the results of clinical trials.¹ There is at least one other such high quality database in Britain.

Since 1995 the audit group of the Scottish Intensive Care Society, with national funding from the Clinical Resource and Audit Group, has collected a similar dataset. We receive data from 100% of Scottish adult intensive care beds (in 26 units) and so can be certain that our findings are generalisable. Because of our defined geographical area we can provide a denominator for incidence figures.

We support the use of the data in the manner described by Padkin et al. We had also queried our data to obtain an approximation of the incidence of sepsis in Scotland, with similar results. We have found this type of information useful not only at a national level but also in giving a very approximate indication of the effects on local budgets.

We are aware of the limitations of such an analysis. Data collected during the first 24 hours of intensive care and structured for another purpose can only estimate the number of patients with sepsis and, as Padkin et al say, cannot identify the proportion who would meet the full entry criteria for the PROWESS trial. Because of this we believe that prospective data collection is still necessary. One of the strengths of our organisation is our ability to collect additional information for short periods to address specific questions.

Such an exercise is due to start soon to collect data on the incidence and outcomes of sepsis. We have used a similar approach in the past to identify the incidence of combined respiratory and renal failure and

acute respiratory distress syndrome.^{2,3} We believe that undue reliability cannot be given to information gleaned from data gathered for another purpose and that such data, although useful, should be used as a starting point for more detailed studies.

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Hospital revises its own data in government league table

EDITOR—The government has placed great emphasis on hospital league tables, although potential and real flaws have been identified. We therefore thought it was appropriate to review our hospital's process, assessing the accuracy of the mortality data in general surgery and the quality of patient care.

The data submitted included several patients who had undergone procedures that were specifically excluded. In the non-emergency group this would have reduced the number of patient deaths from 32 to 24 and in the emergency group from 121 to 100. In addition, the government guidelines can be ambiguous in defining minor operations. It seems unreasonable to include diagnostic or palliative procedures such as paracentesis. Such exclusions would have further reduced the deaths from 24 to 20 in the non-emergency group, and from 100 to 89 in the emergency group. Overall reductions would have been 37.5% and 26.5% in the respective groups. We have been unable to calculate the potential change in our league table positions as the government's demographic correction factors are not readily obtainable.

An independent review of the case records of all deaths was undertaken by a panel of clinicians. Areas of concern were identified in 28 out of 9380 patients who underwent a procedure. The criticism in most of these cases was a failure by junior staff to recognise significant physiological deterioration; as a result senior staff was not contacted. The benefits of using an early warning scoring system have been shown to identify these cases, and increased emphasis has been made to ensure that this method of assessment is used regularly in all patients.¹ This in turn should ensure appropriate involvement of senior staff.

Most patients have little choice but to attend their local hospital. If it seems that any hospital is underperforming, it will produce an illogical fear in the public eye. Fears regarding standards of local health care may be exacerbated by inaccurate government statistics. Small numbers, dubious statistics, and classification errors lead to

potentially large differences in the position in league tables.

We recommend that data for submission should be assessed by an appropriately clinically qualified individual and the published list of exclusion procedures should be clarified and regularly revised.

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Death rates of surgeons should not be published

EDITOR—Surgical organisations seem generally to have accepted the proposal to publish the death rates of individual surgeons. As a physician of sorts, although soon to be deregistered, I stand amazed. Although surgeons have rightly questioned the validity of rates that depend predominately on the nature of the problem and on the previous health of the patient, rather than on individual surgical skill, that is not the main objection to the proposal. Consider its possible effects. Unless surgeons are to be cloned, a range of competence will be found in any group of surgeons, which may, or may not, be reflected in death rates, because of the confounding factors.

But even if the rates were a valid indicator of competence, what would result from their publication? Either nothing (which would be sensible), or a trend by referring doctors or choosing patients to pick the surgeons at the upper end of the list. If this second reaction were widespread, it would produce one group of surgeons who were overstretched, and also tempted to opt for "safer" patients and procedures in order to preserve their safety status, and another group of surgeons who were underemployed, and increasingly out of practice. The raw consumerist reaction to that (often exemplified in the press) might be, "Well, sack them."

A gloomy picture, but I hope an unlikely one. From extensive experience, I have faith in the good sense of doctors and patients to discern that this proposal is a spin reaction to consumerism gone mad. I am optimistic enough to hope that the information generated by what will be a costly exercise will be largely ignored. But the best option is not to do it.

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Rapid responses

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