Letters

Lessons from the withdrawal of rofecoxib

Observational studies should not be forgotten

EDITOR—In their editorial Dieppe et al describe the lessons from the withdrawal of rofecoxib.[1]

Randomised controlled trials are not the usual way in which serious, uncommon adverse effects of a new drug are discovered. They are usually discovered in observational studies, often case-control studies. Using randomised controlled trials to find adverse effects is dangerous. It would feed on the current vogue from trialists that the only data that count are those from randomised controlled trials. By this standard, we would not have concluded that cigarette smoking causes lung cancer. We should not exclude observational studies but rather continue to do them well, in a timely fashion.

As I understand the rofecoxib story, the cardiovascular effect is strong enough to see in a randomised controlled trial in 2000, although according to some creative interpretation of the data a protective effect of control drug could have explained the result. The current unpublished randomised controlled trial confirms the adverse effect some four years later.

What if, rather than conducting a randomised controlled trial on a few thousand more patients, a large case-control study had been conducted to test the hypothesis of whether or not the increase was from rofecoxib rather than waiting for results of a randomised controlled trial not designed to find the adverse effect? My hunch is that the case-control study would have implicated the drug sooner than waiting for the randomised controlled trial did. At less cost than a randomised controlled trial, the case-control study would probably have been powered to determine an increased risk that would be missed in a randomised controlled trial, simply because one could not afford a large enough sample in the randomised controlled trial.

Observational studies have value. They have played and they probably will continue to play an important part in the protection of the public health.

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Competing interests: GO is a co-inventor on a patent that covers putting folic acid in oral contraceptives and a paid consultant to Johnson and Johnson on this issue.

1 Dieppe PA, Ebrahim S, Martin RM, Jüni P. Lessons from the withdrawal of rofecoxib. *BMJ* 2004;329:867-8. (16 October.)

France has policy for overall assessment of public health impact of new drugs

EDITOR—In their editorial commenting on the withdrawal of rofecoxib, Dieppe et al advocate a series of measures before a drug can be licensed.[1] Several experiences at the directorate general of the Health of France relating to this issue have led to the formulation of a new policy.

Whenever a drug is likely to be used on a large scale, pharmaceutical companies must present a pre-reimbursement assessment and organise a postmarketing study of the public health impact of the drug. This impact assessment goes far beyond single end points, as is the case in trials and classic epidemiological (aetiological) research. It currently works within the framework of a formal agreement signed in May 2003 between the health product economic committee (CEPS) and the association of drug enterprises (LEEM).

Large scale epidemiological evaluations are needed to measure the potential shifts in disease related morbidity and mortality in populations, and risk assessment and the use of other concerned drugs need to be evaluated. Plans should ensure the optimal public health impact of the drug (restricted target populations, selective prescribing, close follow up).

The first example of this new policy was to ask in 2001 for an independent, large scale cohort study of 40 000 patients treated with rofecoxib, celecoxib, or traditional non-steroidal anti-inflammatory drugs. More than 50 such studies have now been agreed for various drugs between pharmaceutical firms and the French authorities. Some of these agreements have included limiting the size of the population for which the drug should be reimbursed and to achieve a stepwise introduction of the drug—a goal similar to that advocated by Dieppe et al. However, the policy has been in operation for only a year and its efficiency will have to be evaluated.

The box lists the elements required for public health impact assessment by the general health inspectorate.

Requirements for public health impact assessment of drugs (2003)

- Description of the treated population and comparison with the target population for the drug. Identification, within the population of treated patients, of the fraction in whom disease is poorly controlled with previous treatments and who should be treated with the new drug alone
- Evaluation of the effect of co-prescriptions, comorbidities, and other risk factors
- Epidemiological assessment of the impact of the new drug on the incidence and prevalence of complications of the treated disease in populations, including morbidity and mortality associated with treated disease and side effects and risks of the drugs
- Measure of the impact of other new comparative drugs in the same population
- Evaluation of the impact of the new drug on the health system (medical and hospital services, use of diagnostic procedures and other treatments, including other drugs)

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Competing interests: LA has been active in or acted as a consultant for studies funded by Roche, United Therapeutics, Negma and AstraZeneca, but none for non-steroidal anti-inflammatory drugs or COX-2 inhibitors.

1 Dieppe PA, Ebrahim S, Martin RM, Jüni P. Lessons from the withdrawal of rofecoxib. *BMJ* 2004;329:867-8. (16 October.)

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Reforming the consultant contract again?

Applied in partnership, the contract works for both consultants and patients

EDITOR—Maynard and Bloor show no benefit of financial incentives, rather their limitations, divisions, and distortions, with regard to the consultant contract.[1] Most recent changes in health have not been driven by such incentives.

Overall, 85% of consultants have signed up to the new contract. Surveys show that a third are already seeing improvements. Monitoring will indicate where adjustments are needed, but there is no planned revision.

The contract gives transparency and ability to manage performance. Trusts that have negotiated job plans, including objectives and supporting resources, in partnership with consultants reflecting the number of programmed activities required will see this to the benefit of patients and consultants. To omit proper job planning misses a fundamental feature of the contract

There is little evidence of support for fee for service payments. Consultants realise the limitations, as shown by the refusal to bow to pressure from the last health secretary.

It is a shame that the widely disputed NERA report is quoted,[2] but the accusation of cartels is of concern. Thorough investigation by the Office of Fair Trading failed to find any evidence. If the authors know of any insurer cartels, implied by their statement, they should make evidence available or rescind their comments.

The final paragraph states any changes should be piloted, an action rarely taken by policy makers. The effects are likely to be minimal compared with the policies of payment by results, practice based commissioning, and patients' "choice." Politicians need to be ready for the entrepreneurs, reconfiguration of services, and probable closures that will inevitably result.

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1 Maynard A, Bloor K Reforming the consultant contract again? *BMJ* 204;329:929-30. (23 October.)
2 Bramley-Harker E, Aslam S. *Fees for medical specialists: how does the UK compare?* London: NERA Economic Consulting, 2003.

Fees for service have been in effect for some time

EDITOR—I am somewhat bemused to read an article by the chair of my NHS trust employer that proposes introducing fees for service—particularly when they have been in effect in his own hospital for several years.[1]

Payments for waiting list initiatives are fees for service that seem to provide no incentive whatsoever, for increasing patient throughput or activity with existing resources. They can even seem to reward inactivity.

The authors speculate on the impact of such personal financial incentives but they do not provide any evidence of benefit. They acknowledge that Germany and France wish to abandon fee for service payments because of their cost. I'm sure our own finance manager would wish the same fate for waiting list initiative payments in York, which have had no effect on reducing the overall waiting list. They also undermine NHS efficiency and create disharmony, with differential pay rates for different employees.

Their editorial also omits the fact that the Department of Health is introducing its own system of fees for service from next April. Payment by results will provide hospitals with a fee for service based on a national tariff. The 2005-6 tariff is based on reference costs from 2003-4. Perhaps the impact of payment by results should be evaluated before suggesting more of the same?

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1 Maynard A, Bloor K Reforming the consultant contract again? *BMJ* 204;329:929-30. (23 October.)

Surgeons' fees for items of service would not work

EDITOR—What would happen to my fees if the patient failed to come in for surgery, or was sent home because of lack of beds, or my theatre list was cancelled because of staff shortages? Who would get the fee if a case was operated on by a trainee "flying solo" while the consultant sat at the back of theatre?

There are too many anomalies to even consider paying consultants in the way outlined by Maynard and Bloor.[1]

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1 Maynard A, Bloor K Reforming the consultant contract again? *BMJ* 204;329:929-30. (23 October.)

Acute trusts should prioritise care of urgently ill patients

EDITOR—It is a paradox that, if NHS consultants eradicate waiting lists, they might experience less success in the private sector.[1] The new contract will not eradicate private practice, and often NHS acute trusts cannot provide bespoke care but crisis management. Patients pay extra for fast, client oriented care with a consultant of their choice.

One major cause of this situation is the need to reduce waiting lists while catering for acutely ill patients who are not on waiting lists and commonly have no choice about treatment. Urgent and elective patients are trying to access the same health resources in acute trusts, and with the current emphasis this may disadvantage urgent cases waiting for treatment.

Acute trusts should make their primary mission the care of urgently ill patients, and financial flows should reflect this mission. Elective work should be done through elective units. Training opportunities may be best in ill patients, not on elective cases that go well. Later, senior trainees could also train in elective units, paired with a trainer.

An alternative new consultant contract would offer an Australian style working week, with three days a week delivering care to urgent patients in a network of public hospitals. NHS acute trusts could offer eight programmed activities for three days a week, with two programmed activities for training and continuing professional development. Two days a week would then be available for elective care, at 63-75% of BUPA rates, in accredited independent institutions. NHS pensions could be supplemented from this income, by choice.

Any patient would have the right to treatment for urgent or elective conditions through any hospital, free at the point of delivery. Unlimited fee for service risks unnecessary procedures and escalating costs. Time limited fee for service, with time for care of urgent cases and supporting activities and teaching, is a good compromise. In this model, consultants will not be obliged to put the emphasis on patients who have waited a long time when sick patients languish elsewhere awaiting their attention.

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TADS study raises concerns

EDITOR—We have additional concerns to those raised by Lenzer about the adolescents with depression study (TADS).[1 2]

TADS consists of two separate randomised studies: a double blind comparison of fluoxetine (109 subjects) with placebo (112), and an unblinded comparison between cognitive behaviour therapy alone (111) and fluoxetine plus cognitive behaviour therapy (107). The lack of patient blinding and placebo control in the latter group is likely to exaggerate the benefit seen in the fluoxetine plus cognitive behaviour therapy group, who receive more face to face contact and know (as do their doctors) that they are not receiving placebo.

Comparing results across all four groups is therefore misleading. The authors' claim that a cognitive behaviour therapy plus placebo arm would have been both too expensive and too artificial to have clinical relevance is unconvincing.

TADS found no statistical advantage of fluoxetine over placebo on the primary end

Effect of fluoxetine and placebo on various end points

Intervention	Change in children's depression rating scale	Change in adolescent depression scale	Change in suicidal ideation questionnaire	Clinical global impressions improvement of 1 or 2 (%)
Fluoxetine	22.6	16.4	7.4	60.6
Placebo	19.4	14.6	9.2	34.8
Proportion of fluoxetine effect seen in placebo group	0.86	0.89	1.24	N/A

N/A=not applicable, categorical measure.

point, the children's depression rating scale (CDRS-R; P=0.10), but this was not mentioned in the abstract. This and the small or absent advantages of fluoxetine on other end points (table) and in other studies,[3] shows that fluoxetine, like all other antidepressants, is of doubtful clinical importance for children.

Adverse events and suicidal behaviour may be greater than the TADS paper says. Despite small numbers, more subjects leaving the study than reporting adverse effects, and the splitting of adverse events into multiple groups, significantly more psychiatric adverse events occurred in the fluoxetine group than the placebo group (χ^2 test (1 df), P=0.047). Despite small numbers and the exclusion of known suicidal behaviour, TADS found a trend to more suicidal behaviour (six attempts in the fluoxetine groups and one attempt in the non-fluoxetine groups), consistent with other trials of selective serotonin reuptake inhibitors (SSRIs). We are less reassured than the authors by the fact that no attempt was fatal. Suicide is a rare event so that a study the size of TADS should be expected to miss a significantly increased risk.

The data do not support the TADS authors' optimistic conclusions. The balance between benefit and harm of SSRI treatment for depression in childhood and adolescence has yet to be shown to be favourable.

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Competing interests: None declared.

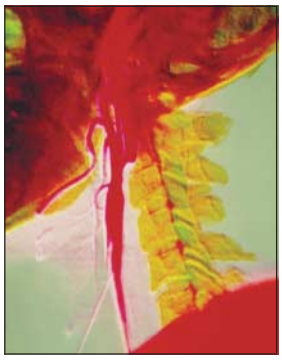
1 Lenzer J. Journalists on Prozac. *BMJ* 2004;329:748. (25 September.)
2 Treatment for Adolescents with Depression Study Team. Fluoxetine, cognitive-behavioral therapy, and their combination for adolescents with depression: treatment for adolescents with depression study (TADS) randomized controlled trial. *JAMA* 2004;292:807-20.
3 Jureidini J, Doecke C, Mansfield P, Haby M, Menkes D, Tonkin A. Efficacy and safety of antidepressants for children and adolescents. *BMJ* 2004;328:879-83.

Surgery for carotid artery stenosis

Patients with critical stenoses should be admitted to stroke prevention units

EDITOR—While shopping in Florida, a man found a booth offering carotid duplex scans for a modest fee. He had a family history of cerebrovascular disease, so he decided to be scanned for peace of mind. Unfortunately, a critical internal carotid stenosis was found.

He returned to his hotel somewhat perturbed, only to be phoned by a vascular surgeon recommending urgent carotid endarterectomy before he flew home to the United Kingdom. He declined the offer, but underwent successful surgery some months later.


CNRI/SPL

Screening is not without drawbacks. The asymptomatic carotid surgery trial confirms that carefully selected patients benefit from surgery when operated upon by skilled teams.[1] The logic, which Toole finds compelling,[2] is that carotid screening should be considered.

Transcranial Doppler ultrasound can detect microemboli, which allows the efficacy of therapeutic interventions to be rapidly and non-invasively assessed. Controlling the rate of embolisation reduces the risk of an early postoperative stroke.[3] Controlling emboli and symptoms in patients with recurrent or crescendo transient ischaemic attacks by using Doppler directed drug therapy allows these high risk patients to undergo elective carotid surgery safely.[4]

Patients with focal neurological events need assessment within 24-48 hours. Those with critical carotid stenoses, symptoms and emboli should be admitted to a stroke prevention unit (similar to a coronary care unit). It would be jointly managed by vascular surgeons and stroke doctors, with high ratio of staff to patients. Rapid control of microemboli could be achieved, and since microemboli seem to be surrogate markers for future embolic events, some strokes will be prevented.

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1 MRC Asymptomatic Carotid Surgery Trial (ACST) Collaborative Group. Prevention of disabling and fatal strokes by successful carotid endarterectomy in patients without recent neurological symptoms: a randomized controlled trial. *Lancet* 2004;363:1491-502.
2 Toole JF. Surgery for carotid artery stenosis. *BMJ* 2004;329:635-6. (25 September.)
3 Lennard N, Smith J, Abbott R, Evans DH, London NJ, Bell PR, et al. Prevention of postoperative thrombotic stroke after carotid endarterectomy: the role of transcranial Doppler ultrasound. *J Vasc Surg* 1997;26;579-84.
4 Lennard NS, Vijayasekar C, Tiivas, Chan CWM, Higman DJ, Imray CHE. Control of emboli in patients with recurrent or crescendo transient ischaemic attacks using preoperative transcranial Doppler-directed dextran therapy. *Br J Surg* 2003;90:166-70.

Cut-off point is problematic in selecting patients for carotid surgery

EDITOR—Toole's voice is important in the controversial debate on carotid surgery.[1] However, in determining a cut-off point for selecting patients for endarterectomy, the different methods of measurement (local versus distal degree of stenosis) used by European and American surgery trials must be considered.[2]

A cut-off point of 60% stenosis refers to the asymptomatic carotid artery stenosis study (ACAS) and uses the American method of stenosis measurement[3]; that degree of stenosis corresponds to a 75% stenosis according to European criteria.[2] Therefore, to define a cut-off point of 60% stenosis in a European journal is misleading.

I agree with Toole that other indicators for selecting patients for carotid surgery should be considered; however, apart from the degree of stenosis, there are no evidence based criteria that allow medical or surgical treatment to be decided. So the degree of stenosis remains the main criterion; measurement should be performed by means of Doppler and duplex ultrasound evaluation.[4]

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Competing interests: None declared.

1 Toole JF. Surgery for carotid artery stenosis. *BMJ* 2004;329:635-6. (25 September.)
2 Rothwell PM, Gibson RJ, Slattery J, Sellar RJ, Warlow CP. Equivalence of measurements of carotid stenosis. A comparison of three methods on 1001 angiograms. European Carotid Surgery Trialists' Collaborative Group. *Stroke* 1994;25:2435-9.
3 The Executive Committee for the Asymptomatic Carotid Atherosclerosis Study. Endarterectomy for asymptomatic carotid artery stenosis. *JAMA* 1995;273:1421-8.
4 Arning C, Hammer E, Kortmann H, Hahm H, Muller-Jensen A, Lachenmayer L. Quantifying stenosis of the internal carotid artery: which ultrasound criteria are relevant? *Ultraschall Med.* 2003;24:233-8.

Author's reply

EDITOR—I am pleased that my editorial has evoked responses about the looming epidemic of stroke, often the result of carotid artery disease. We hope that all risk factors will be reduced by careful attention to good health habits including diet, smoking, blood pressure control, etc, and in selected cases, platelet anti-aggregants and statins.[1] For

patients who, despite control of risk factors, go on to develop severe, carotid bifurcation atherosclerosis, simple methods now exist to identify preclinical disease by using ultrasound and for delineation of transient ischaemic attack with a short questionnaire.[2] Auscultation for bruits is practical depending on the auscultatory technique and ambient noise. Identifying cases is of little benefit unless the opportunity to intervene exists in the healthcare system.

It would be foolhardy to make blanket or case specific recommendations for medical and surgical management. Moreover, screening has nothing to do with the treatment that might be provided, which should most often be reduction of risk factors. It must never be considered that the reason for screening is to identify people who might be subjected to an interventional procedure such as stent, balloon angioplasty, or endarterectomy. It is for this reason that I urge that non-procedure oriented physicians be firmly in charge of the screening and the recommendations that are made.

I am among those who suspect that the condition of the carotid artery is a marker for atherosclerosis in other organs, particularly the heart. If the easily accessible carotid artery could be used as the indicator for the other arteries, including the coronaries, abdominals, and cerebral circulation, this would be a big step forward. It may be premature to call for mass screening, but it is highly appropriate for individual doctors to use the technology now at hand for identification of cases and early intervention with long term follow up designed to reduce risk.

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Competing interests: None declared.

1 Toole JF, Sane D, Bettermann K. Stroke prevention: optimizing the response to a common threat. *JAMA* 2004;292:1885-7.
2 Karanjia PN, Nelson JJ, Lefkowitz DS, Dick AR, Toole JF, Chambless LE, et al. Validation of the ACAS TIA/stroke algorithm. *Neurology* 1997;48:346-51.

Transparency and trust

Figure for ghost written articles was misquoted

Editor—Editor's choice in the issue of 23 October on transparency and trust seems to perpetuate a misleading press citation of my testimony to a House of Commons Select Committee last month.[1] The original statement, supported by the transcript, was that 50% of the articles dealing with therapeutics were ghost written, not 50% of all articles.[2 3]

I, like most readers, almost instinctively shrink from a claim that anything like 50% of the articles, even those on therapeutics alone, are ghost written in journals such as the *BMJ*, *New England Journal of Medicine*, *JAMA*, and the *Lancet*. But equally instinctively, most readers if asked to estimate how many of the key articles on their drugs, and this means articles in major journals, pharmaceutical companies are likely to have had a determining role in writing, would probably come up with figures close to 100%. If the question is in what proportion of articles on therapeutics in major journals do the apparent academics hold the raw data and are able to share that data if needed, the answer in many estimates will not be much greater than 0%.

Abbasi usefully brings out a point made in the select committee's meeting, that the key problem with ghost writing is not the medical writing itself but the issue of transparency. When there is reason to believe that the articles that result from the ghost writing process do not offer a fair representation of the underlying data there is a problem. Otherwise ghost writing poses much less of a problem.

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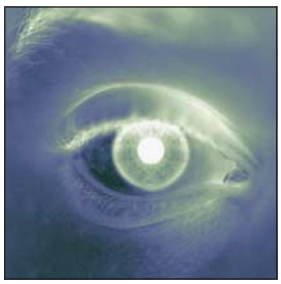
1 Abbasi K. Editor's choice. Transparency and trust. *BMJ* 2004;329:0-g. (23 October.)
2 Healy D, Cattell D. The interface between authorship, industry and science in the domain of therapeutics. *Br J Psychiatry* 2003;182:22-7.
3 Elliott C. Pharma goes to the laundry. *Hastings Center Report* 2004;34(5):18-23.

In defence of medical writers

Editor—If ghost writing is defined as what happens when the identity of a writer is concealed, then Abbasi's statement, "We know that ghost writing happens, and the identity and the motivations of the ghost writer are not revealed" is self evidently true, albeit not very informative.[1] However, many people understand medical ghost writing to mean that a professional medical writer, whose name does not appear on the author list, wrote the paper. When this happens, the identity of the writer is sometimes not revealed, but it often is, usually in the acknowledgments section. It is therefore misleading to state that the identity of the ghost writer is not revealed as though this were a universal truth.



Kmietowicz's news article also misleads by saying that distinguished authors put their names to papers without ever seeing the raw data.[2] This may be true but is hardly the whole story. What exactly are you supposed to do with thousands upon thousands of laboratory results, for example? Data from clinical studies can be interpreted only once they have been processed into summary tables and graphs: a job that is more appropriately done by a statistician than a clinician. In my experience of writing papers on behalf of investigators, the named authors always have access to the summary tables and graphs, which is far more important than access to the raw data.

I agree, however, that high ethical standards must be maintained when professional medical writers draft papers on behalf of named authors, and that transparency is an essential part of this. One set of recently published guidelines seeks to ensure good practice in this context,[3] and the European Medical Writers Association is currently preparing guidelines that will further define the ethical responsibilities of professional medical writers.

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1 Abbasi K. Editor's choice. Transparency and trust. *BMJ* 2004;329:0-g. (23 October.)
2 Kmietowicz Z. Consumer organisations criticise influence of drug companies. *BMJ* 2004;329:937. (23 October.)
3 Wager E, Field EA, Grossman L. Good publication practice for pharmaceutical companies. *Curr Med Res Opin* 2003;19:149–54.

Clear definition of ghost writing would be helpful

Editor—The requirement that all authors have the idea, do all the work, get the data, analyse the data, and write the paper may be perfectly applicable to fundamental research, perhaps, or small clinical trials. In large studies it is not applicable: we are doing a 40 000 patient study of non-steroidal anti-inflammatory drugs and COX-2 inhibitors, requested by the regulatory authorities, financed by pharmaceutical companies, driven by an independent scientific committee. Fifty people, including half a dozen statisticians, work in this study, which will generate about a hundred million bits of data. Papers will be written by medical writers under the surveillance and final approval of the scientific committee. Is this ghost writing?

May I hire a professional writer to write papers students did not or could not write, and I don't have the time to? Should these data lie ignored? Should that writer, who was not involved in the initial conception or in data collection or its analysis be an author? If not, is it ghost writing?

There is an infinity of variations between the lone searcher who does everything, and the key opinion leader who does nothing but sign.

Abbasi's simple statement that 50% of all publications are ghostwritten is misleading and derogatory,[1] indicating a misunderstanding of the complexities of modern studies. It could too easily be picked up by politicians (who we all know write their speeches themselves) and others for some easy doctor bashing. There may be some

cases, of course. A difficult topic, no simple answers.

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Competing interests: NM must have hidden interests, certainly, although at the moment he can't recall any relevant ones.

1 Abbasi K. Editor's choice. Transparency and trust. *BMJ* 2004;329:0-g. (23 October.)

Every prescription is a clinical trial

EDITOR—Senn questions whether individual response to treatment is a valid assumption.[1] Patients have never responded consistently to treatment, and, additionally, every time a prescription is written (except for identical twins) what effectively begins is a clinical trial with n = 1.

Evidence based medicine or evidence based clinical practice is the judicious application of best current knowledge to the condition and values of each patient.[2] It should therefore allow for individualised treatment, which may entail a drug different from the "best" identified after systematic review. Can the gold standard randomised controlled trial really deliver the desired certainty when identifying which patients will respond to the treatment is impossible?

Trials organised by pharmaceutical companies are designed to show the superiority of a company's product over a competitor's to ensure optimum market share, with little thought for the individual patient receiving the drug. Promotion follows to ensure product recognition at the point of "sale." Examination of published study results soon shows that some subjects do much better with the drug that is statistically inferior.

Consider intravenous regional sympathetic block. Systematic review combined with a double blind evaluation has not supported it as an evidence based treatment,[3] yet individual patients are reported as deriving notable benefit, which, in this typical case was 18 months' pain relief after two treatments.[4] As it remains, however, a useful and valued component of the planned staged approach to the management of chronic regional pain syndrome type 1, many pain clinicians will continue to include it or an equivalent intervention in their armamentarium.[5]

An additional problem that regularly occurs is the removal from the market of valuable drugs (although often for limited indications) which are effective in, and appropriate for, many individual patients.

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Competing interests: None declared.

1 Senn S. Individual response to treatment: is it a valid assumption? *BMJ* 2004;329:966-8. (23 October.)
2 Muir Gray JA. Evidence based policy making. *BMJ* 2004;329:988-9. (30 October.)
3 Jadad AR, Carroll D, Glynn CJ, McQuay HJ. Intravenous regional sympathetic blockade for pain relief in reflex sympathetic dystrophy: a systematic review and a randomised, double-blind crossover study. *J Pain Symptom Manage* 1995;10:13-20.
4 Vijayan R, Low KH. Pain relief with intravenous regional guanethidine in post-traumatic reflex sympathetic dystrophy? A case report. *Med J Malaysia* 1993;48:236-9.
5 Lake APJ. Intravenous regional sympathetic block: past, present and future? *Pain Res Manage* 2004;9:35-7.

Doctors should help patients with their sex lives

EDITOR—In his personal view Barrett shared the sadly far too common scenario of a patient's sexual needs being overlooked and unfairly judged.[1] Human rights include sexual rights, and the public is currently being led to have high expectations of sexual intercourse. Yet staff in primary and secondary care are not adequately trained in issues surrounding sexual intercourse, particularly interpersonal relationships, awareness, and respect of sexual difference. They also lack the confidence to communicate comfortably on sensitive topics. Unsurprisingly they either avoid discussion of the issue or do not handle it well.

Healthcare practitioners should be able to deliver sex information ranging from advising on sexually transmitted infections or sexual dysfunctions to helping people for whom sexual contact is difficult or who may have missed out on sex education. This does not mean that they have to book their patients a prostitute or conduct a sex education class, but they should know where to refer them.

Training of health professionals needs to be improved to include awareness of sex related issues beyond management of sexual disease. Staff cannot be blamed if they are neither trained for nor supported in this task. Also, prostitutes cannot be relied on when patients need help with sexual liaisons because they may not be adequately skilled or supported to help vulnerable people. Sex surrogates may be far more appropriate and should be supported by the medical profession.

Health Care Without Shame may help those who want to improve their practice.[2] The Outsiders Trust (www.outsiders.org.uk) and a site for surrogate partners (www.icasa.co.uk) are there to help patients.

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Competing interests: PMB runs training courses for general practitioners in improving their skills to communicate with patients about sexual intercourse.

1 Barrett J. Personal services or dangerous liaisons: should we help patients hire prostitutes? *BMJ* 2004;329:985. (23 October.)
2 Moser C. *Health care without shame: a handbook for the sexually diverse and their caregivers.* San Francisco: Greenery Press, 1999.

Health conventions in smoke free places have positive economic impact

EDITOR—Policies have the potential to affect the health of populations positively. After non-smoking policies had been adopted and taxation increased, the New York City Department of Health announced on 14 May 2004, that smoking rates were down 11% from 2002 to 2003—the biggest one year drop ever recorded.[1]

Scollo et al, in their review of the quality of studies on the economic effects of smoke free policies on the hospitality industry, concluded that non-smoking policies had no impact or a positive impact on sales and employment.[2] Policy makers can act to protect citizens from the toxins in secondhand smoke and be confident in rejecting industry claims of adverse economic impacts. However, the tobacco industry will expend whatever effort is necessary to thwart policies that would adversely affect consumption of cigarettes and, therefore, profit.[3] Coalitions, supported by the tobacco industry, continue to release information claiming negative economic effects.

But what if there was a simple way to show an economic gain by adopting smoke free policies? Health professional associations hold conferences and conventions that have a positive effect on those municipalities where they are held. These economic benefits should reward those that have prioritised health and become smoke free, such as Ireland. In May 2003 the Canadian Public Health Association adopted a resolution to hold its conferences in jurisdictions with smoking restrictions, to urge others to adopt similar resolutions, and to communicate this resolution to potential convention sites (see bmj.com).

How can your professional association support public health policy?

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1 New York City Department of Health and Mental Hygiene Online. *New York City's smoking rate declines rapidly from 2002 to 2003, the most significant one-year drop ever recorded.* 12 May 2004. http://www.nyc.gov/html/doh/html/public/press04/press04.html (accessed 24 Nov 2004).
2 Scollo M, Lal A, Hyland A, Glantz G. Review of the quality of studies on the economic effects of smoke-free policies on the hospitality industry. *Tobacco Control* 2003; 12:13-20.
3 Muggli ME, Hurt RE, Repace J. The tobacco industry's political efforts to derail the EPA report on ETS. *Am J Prevent Med* 2004;26:167-77.

P+ *Resolution of the Canadian Public Health Association is on bmj.com*