Letters

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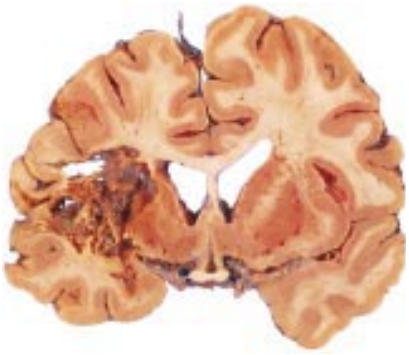
Preventing stroke with ramipril



DR E WALKER/SPL

Results should have been presented in ways that help practising clinicians

EDITOR—I was surprised to see that the *BMJ* published a trial that presented the results in a way that exaggerates the findings.[1] Stroke prevention, the topic under discussion in the paper by Bosch et al, is important for patients, doctors, and funders of care. Hence the results should have been presented in a way that would help practising clinicians—by giving numbers needed to treat (NNT) along with relative risk reductions (RRR). The authors report a relative risk reduction of 32% in all strokes and of 61% in fatal strokes. For all strokes this translates into a number needed to treat of 67 for four and a half years' treatment.

Evidence shows that the way results of clinical trials are presented influences both physicians and funders of health care.[2 3] In the randomised controlled trial by Bucher et al, doctors gave higher ratings for the effectiveness of the drug and were more inclined to prescribe lipid lowering drugs when the results were presented as relative risks.[2] A study from a health authority in the United Kingdom reported that health authority members' willingness to purchase services was influenced by the methods used to present results.[3] Interestingly, both these papers were published in the *BMJ*.

The problem of biased reporting of clinical trials is not a new phenomenon. Pocock et al in their survey of three medical journals in 1987 found that, overall, the reporting of clinical trials seems to be biased towards an exaggeration of treatment differences.[4] What do the CONSORT guidelines say?[5] The following quote may be relevant here: "For both binary and survival time data, expressing the results also as the number needed to treat for benefit (NNTB) or harm (NNTH) can be helpful." The two citations supporting this statement in the CONSORT guidelines are from the *BMJ*.

What can be done to improve the quality of reporting of results of randomised controlled trials? Both reduction in relative risk and reduction in absolute risk should be reported in medical papers because exclusive emphasis on the reduction in relative risk may overstate the effectiveness of a treatment.[2] If general agreement is reached then the next CONSORT guidelines should include a statement that wherever applicable the results of clinical trials should include the numbers needed to treat.

Although the *BMJ* has published many studies on the appropriate way to present results and their impact, in future if it emphasises to the authors of clinical trials the importance of presenting the numbers needed to treat, this will help its readers and avoid criticisms of the authors, reviewers, and editors.

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I thank Kev Hopayian, Leiston, Suffolk, United Kingdom, for bringing this issue to my attention through a posting in the evidence based health discussion list.

1 Bosch J, Yusuf S, Pogue J, Sleight P, Lonn E, Rangoonwala B, et al on behalf of the HOPE Investigators. Use of ramipril in preventing stroke: double blind randomised trial *BMJ* 2002;324:699-702. (23 March.)
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Benefits were considerably overstated

EDITOR—In their editorial commenting on the HOPE study, Schrader and Lüders say that ramipril substantially decreased the risk of stroke and that fatal stroke was reduced by 61% and non-fatal stroke by 24%.[1] The former statement overstates the effect of the drug, and the latter statement is quite simply incorrect.

Although the relative risk reduction in the trial was 61% for fatal stroke and 24% for non-fatal stroke, the absolute risk reduction (ARR), which is the clinically relevant outcome measure, was reduced by only 1.5% and 0.9%, respectively. Since the follow up period of the trial was an average of 4.5 years, these "substantial" results are equivalent to an overall reduction of only 0.33% and 0.2% per year, respectively, in the occurrence of fatal and non-fatal stroke. This shows the pitfalls that can arise when the results of intervention trials are presented only in terms of relative risk reduction but recommendations are made in terms of alleged clinical benefits.[2]

Although patients in the trial were labelled "high risk," participants showed an absolute risk of only 4.9% for any stroke over the follow up period. This confirms the importance of hypertension control as one of the main public health interventions in preventing stroke. In addition, it is already proved that antiplatelet agents are an effective secondary prevention strategy in high risk patients and that anticoagulants effectively prevent stroke in patients with atrial fibrillation.[3 4]

Stroke is an important cause of mortality and disability in the United Kingdom and the search for new cost effective solutions to reducing death and disability must continue. We doubt, however, that the

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results of the HOPE trial warrant Bosch et al's recommendation that patients who are at high risk of stroke should be treated with ramipril irrespective of their blood pressure.[5] Perhaps the authors intuitively accept this when they choose to present the trial outcomes only in terms of relative risk reduction.

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Superiority of particular class of antihypertensive agent remains to be shown

EDITOR—Bosch et al claim that the benefits of ramipril in subjects at high risk is unrelated to its antihypertensive effects.[1] The HOPE study showed that the risk of stroke can be reduced by 32% (95% confidence interval 16% to 44%) and of myocardial infarction by 20% (10% to 30%) with ramipril treatment.[1 2] Recent evidence, from the HOPE study itself, makes it likely that this is simply a benefit of further blood pressure reduction.

The claims of protective benefit from angiotensin converting enzyme inhibitors are based on estimates of probable benefit calculated from observed differences in blood pressure between the HOPE study groups. In the study, the mean reduction in blood pressure (taken at the clinic) with treatment was 3/2 mm Hg, which would predict around 13% reduction in risk of stroke and around 6% in that of myocardial infarction, substantially less than that observed.[2 3]

A HOPE substudy was recently published that investigated ambulatory blood pressure in 38 subjects who were treated with ramipril or placebo.[4] In these subjects, no significant differences were found between groups in clinic blood pressure. But 24 h blood pressure was significantly lower in the subjects treated with ramipril (10/4 mm Hg), mainly as a consequence of the substantially lower night time blood pressure (17/8 mm Hg). The authors note that more of the benefits of ramipril in HOPE may be related to reduction of blood pressure (especially during night time) than was explained by the effects on office blood pressure. They also noted that the HOPE protocol is the only large trial in which an antihypertensive agent was given at bedtime, thereby making it unique in regard to variation of blood pressure between day and night.

If similar reductions in blood pressure occurred in all HOPE participants, the calculations of likely benefit would be around 35% for stroke and 15% for myocardial infarction, matching reasonably the actual benefits observed. These estimates might help resolve the HOPE "paradox," whereby the findings of an apparent protective effect of the drug class are not supported by head to head studies.[5] The HOPE study simply shows that in high risk patients, for blood pressure as for cholesterol, the lower the better.

A demonstration of specific organoprotection from a drug or a drug class will require head to head comparisons, with both clinic, and, ideally, ambulatory monitoring, at least in a subset. It still remains to be shown whether any one class of antihypertensive agent provides superior benefit.

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1 Bosch J, Yusuf S, Pogue J, Sleight P, Lonn E, Rangoonwala B, et al. Use of ramipril in preventing stroke: double blind randomised trial. *BMJ* 2002;324:699-702. (23 March.)
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Presentation of data is misleading

EDITOR—Bosch et al show a relative risk reduction of 32% in any stroke and of 61% in fatal strokes, in patients treated with ramipril compared with placebo over 4.5 years.[1] This is a marvellous study result, and the conclusion is appropriate, that patients at high risk of stroke should be treated with ramipril, irrespective of their blood pressure. But analysing the data further and calculating the reduction in absolute risk, the numbers to treat (NNT) and cost, the same data do not seem as impressive. The table shows that the cost of preventing a single stroke with ramipril over 4.5 years is in the range of C$100 000-250 000 (US$64 000-160 000; £42 000-105 000; €65 000-162 500).

The table also shows further analysis of subgroup data given in figure 4 of the long version of the paper (bmj.com/cgi/content/full/324/7339/699). The risk of stroke in ramipril subgroups is not given so my calculations are based on the relative risk reductions shown in the figure.

On the basis of this analysis I believe that ramipril is useful and possibly a cost effective alternative in preventing stroke only in three select groups of patients at high risk.

Patients who are not having aspirin or antiplatelet treatment—Ideally, these patients should receive these agents but if a patient cannot tolerate them or is allergic to these agents then ramipril is a viable alternative.

Patients who are not taking a calcium channel blocker for some other indication—If a patient is already taking a calcium channel blocker (for any indication) then addition of ramipril has not shown any significant effect in prevention of stroke.

Patients who are taking lipid lowering agents—Although the risk of stroke was decreased in patients who were treated for hyperlipidaemia with lipid lowering agents, the effect of ramipril was insignificant in both groups. There was, however, a suggestion of a synergistic effect of ramipril in patients who were treated with lipid lowering agents.

Only 28% of these so called high risk patients received lipid lowering agents in the

Absolute risk reductions (ARR) and numbers needed to treat (NNT) with cost analysis to prevent a stroke with ramipril and impact of ramipril in important subgroups of patients

	Ramipril		Placebo				
	No (%) of patients	Stroke risk (%)	No (%) of patients	Stroke risk (%)	ARR (%)	NNT	Cost (C$)*/stroke saved
Outcome							
All strokes	156 (3.4)		226 (4.9)		1.5	66	102 960
Fatal stroke	17 (0.4)		44 (1.0)		0.6	166	258 960
Non-fatal stroke	139 (3.0)		182 (3.9)		0.9	111	173 160
Transient ischaemic attack	190 (4.1)		227 (4.9)		0.8	125	195 000
Stroke and transient ischaemic attack	315 (6.8)		405 (8.7)		1.9	52	81 120
Subgroup (baseline drug use)							
No aspirin (n=2484)		3.05		6.1	3.05	33	51 480
Aspirin (n=6813)		3.52		4.4	0.88	113	176 280
No lipid lowering agent (n=6634)		4.05		5.4	1.35	74	115 440
Lipid lowering agent (n=2658)		1.92		3.5	1.52	65	101 400
No calcium channel blocker (n=4917)		3.19		5.5	2.31	43	67 080
Calcium channel blocker (n=4380)		3.57		4.2	0.63	158	246 480

*Cost is calculated at C$0.95/day=C$346.75/year=C$1560 (US$998; £647; €1013) per patient taking 10 mg ramipril. Calculations exclude the cost of treating side effects and complications related to ramipril (not mentioned in the paper).

late 1990s. On the basis of the evidence given, it is currently not reasonable to recommend widespread use of an angiotensin enzyme inhibitor such as ramipril in patients at high risk of stroke, but it is useful and may be cost effective in a select group of high risk patients as mentioned above.

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1 Bosch J, Yusuf S, Pogue J, Sleight P, Lonn E, Rangoonwala B, et al. Use of ramipril in preventing stroke: double blind randomised trial. *BMJ* 2002;324:699-702. (23 March.)

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Summary of responses

The study reported by Bosch et al with the accompanying editorial by Schrader and Lüders prompted 21 responses on bmj.com and one letter to the editor.[1 2] Twelve responses were from Britain, the rest from Australia, Austria, Canada, France, the Netherlands, the United Arab Emirates, and the United States.

Most of the correspondents criticised the way in which the results had been presented in the study and the editorial. In the words of Brian Mansfield, a general practitioner in Beckington, Somerset: "Am I alone in finding the confusion between lack of evidence for one course of action and positive evidence for another being misinterpreted as the superiority of course B over course A disturbing?"

It was noted that only the relative risk reduction was given in the study. This should have been accompanied by data on absolute risk reduction and number needed to treat and even number needed to harm (as per CONSORT guidelines). Several of the correspondents had done these calculations and concluded that the number needed to treat over 4.5 years and the resulting costs were too high to make ramipril a viable treatment for patients with stroke.

W Hoefnagels, a neurologist from the Netherlands, questions whether the outcomes with ramipril were actually any better than outcomes achieved with aspirin. Trevor Thompson, a clinical lecturer in primary care at Bristol University, further notes that no quality of life data in the two groups were given and the incidence and nature of adverse drug reactions were not mentioned; this is also a criticism made by Yoon Loke, a clinical lecturer in pharmacology in Oxford. Two correspondents note that no measures of all cause and overall mortality were given. Loke draws our attention to the fact that the reported study is a substudy of HOPE and that the adverse data missing from this study are available from the original study.

Several correspondents draw attention to the figures. Figure 3 shows a non-significant effect of ramipril on people with previous stroke; no flow diagrams accompany the article; and the conclusions are not supported by figure 2. Furthermore, table 1 does not add up.

Peter David Burrill, a specialist in pharmaceutical public health, comments that the two different dosages of ramipril should have been examined with respect to their efficacy. Several authors find that the fact that the blood pressure lowering effect of the drug might have been responsible for the benefits had not been taken into consideration. Claudia Stöllberger and colleagues from Austria believe that the prevalence of atrial fibrillation and antithrombotic treatment should have been made known as atherothrombosis is not the only cause of stroke. John Attia and his team from Australia question whether the conflicting results of the HOPE and PROGRESS trials may be a function of the ethnic group of participants.

Three correspondents mention the importance of authors declaring their competing interests, and pessimism about modern medicine being dominated by pharmaceutical companies, as evidenced by the prodrug bias of the study.

Birte Twisselmann *technical editor, BMJ*

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New trial should clarify lithium use in bipolar disorder

Editor—We agree with Dinan's statement in his editorial that there is considerable evidence that lithium is an effective maintenance treatment in bipolar disorder.[1] Our recent Cochrane review found that lithium reduced the relative risk of relapse in bipolar disorder by 42% (95% confidence interval 30% to 52%).[2] We also accept that the widespread switch away from lithium—especially in the United States—is based on marketing and opinion rather than compelling evidence. The absence of good evidence for valproate does not mean, of course, that lithium is more efficacious or more acceptable than valproate. It is also possible that the combination of lithium plus valproate is more effective than either drug alone. We are therefore less confident in accepting the unequivocal recommendation that lithium should remain the first line treatment. There is genuine clinical uncertainty about this issue—and such wide international variations in clinical practice—that an overwhelming case can be made for a clinical trial comparing lithium and valproate.[3 4]

We are currently conducting a large randomised trial in the United Kingdom comparing valproate monotherapy, lithium monotherapy, and combination therapy with valproate plus lithium.[5] This large collaborative trial (bipolar affective disorder lithium anticonvulsant evaluation, BALANCE) funded by the Stanley Foundation, a mental health charity in the United States, and the trial drugs have been generously donated by Sanofi-Synthelabo. BALANCE has been designed and conducted entirely independently of the pharmaceutical industry. Clinicians and patients are welcome to participate in this pivotal clinical trial. Further information is available at www.psychiatry.ox.ac.uk/balance.

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Burden of illness and suicide in elderly people

ROBERT DISCALFANI/PHOTONICA

Physical disease and depression are prevalent in elderly Finnish suicide victims

Editor—Waern et al report that among elderly people (those aged 65 years or more) serious physical illness was associated with increased risk of suicide in men but not in women.[1] In addition, mental illnesses, particularly mood disorders, were strongly associated with suicide. Thus, the authors called for further investigations into depres-

Prevalences of physical illness, comorbid mental disorders, and comorbid depression among elderly people who committed suicide during 1998-2000 in northern Finland

	Physical illness (% (No))		Any comorbid mental disorder (% (No))				Comorbid depression (% (No))			
	Men (n=115)	Women (n=43)	Men (n=37)	P value*	Women (n=29)	P value*	Men (n=18)	P value*	Women (n=22)	P value*
Heart and vascular	57 (65)	54 (23)	40 (26)	0.031	74 (17)	0.259	22 (14)	0.040	57 (13)	0.327
Respiratory	20 (23)	12 (5)	30 (7)	0.527	80 (4)	0.469	13 (3)	0.493	40 (2)	0.477
Gastrointestinal	46 (53)	35 (15)	40 (21)	0.084	67 (10)	0.598	19 (10)	0.267	53 (8)	0.545
Genitourinary	18 (21)	33 (14)	38 (8)	0.345	64 (9)	0.510	29 (6)	0.076	64 (9)	0.192
Musculoskeletal	33 (38)	49 (21)	42 (16)	0.083	71 (15)	0.414	18 (7)	0.375	52 (11)	0.559
Endocrine/metabolic	9 (10)	14 (6)	40 (4)	0.408	83 (5)	0.351	30 (3)	0.190	67 (4)	0.355
Neurological	24 (28)	19 (8)	57 (16)	0.002	50 (4)	0.224	32 (9)	0.009	50 (4)	0.624
Malignancy	20 (23)	14 (6)	30 (7)	0.527	100 (6)	0.078	17 (4)	0.507	83 (5)	0.103

*Fisher's exact test (two tailed) for comparing distribution of any comorbid mental disorder or depression between subjects with and without physical disease.

sion in the context of physical disease in elderly people.

Finland has one of the world's highest death rates from suicide.[2] The national Finnish hospital discharge register makes it possible to investigate reliably all hospital admissions for any physical diseases and mental disorders of each person living in Finland.[3] We explored comorbid depression in the main categories of physical diseases as they appear in ICD-8 and ICD-9 in people aged 65 years and over who committed suicide.[4]

We used data on all suicides (1296 males, 289 females) committed during 1988-2000 in northern Finland in the province of Oulu. Details of the database and study protocols have been reported earlier.[5] The lifetime diagnoses of the suicide victims, based on psychiatric and somatic admissions and relevant codes from the International Classification of Diseases, were extracted from the hospital discharge register until the end of 1999. Depression was defined to be present if any of the following ICD codes was found in the register: ICD-8, 2960, 2980, 3004; ICD-9, 2961, 2968, 3004; ICD-10, F32-F34.1.

The table shows that heart and vascular diseases and gastrointestinal, musculoskeletal, and neurological disorders were the most common physical diseases among male suicide victims, the prevalence varying from 24% to 56%. In comparison with disease free subjects in each physical disease category, male suicide victims with heart and vascular or neurological diseases had a significantly higher prevalence of any comorbid mental disorder as well as comorbid depression. Among women no association between any comorbid mental disorder or depression and physical diseases reached significance, although in most physical disease categories (except respiratory diseases) over half of the female suicide victims were found to have the given disease. However, this was probably because of the small number of female suicide victims in our data, which easily leads to type II error in statistical analyses.

In conclusion, our results are in line with the findings of Waern et al on neurological disorders, but they also highlight the importance of detecting comorbid depression among geriatric patients with heart and vascular diseases to prevent suicide among elderly people. The physical diseases of suicide victims in our data were extracted from the reliable national hospital discharge register, which means that only the information on diseases serious enough for hospital treatment were used in statistical analyses. Thus, the memory bias linked with personal interviews was avoided in our study.

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Death, where's thy sting?

Editor—After reading the paper by Waern et al on the burden of illness and suicide in elderly people[1] I was reminded of a remark by Sigmund Freud (1856-1939), who suffered greatly from cancer of the palate.

In 1926, speaking to "the American Viereck" (probably the journalist George Sylvester Viereck), he said, "It may be that the gods are merciful when they make our lives more unpleasant as we grow old. In the end, death seems less intolerable than the many burdens we have to bear."[2]

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Negotiators explain flexible training under the new deal

Editor—We led the team that negotiated the new pay deal for junior doctors and read Davies and Eaton's criticisms.[1] They seem to believe we obtained too much money for flexible trainees. Many doctors who were previously paid less than their childminder to combine a career in medicine and a family might disagree.

Band 3 (non-compliant posts under the new deal) is intended to be a penal rate for trusts. Both sides involved in negotiating the deal believed that as flexible trainee posts are supernumerary nothing should prevent them from being compliant with the new deal.

The threats to flexible training are a result not of the new pay rate, but of government failure to fund flexible training centrally. When we negotiated the deal we pointed out that the existing funding arrangements for flexible training would not work, and the Department of Health agreed.

We proposed funding flexible trainees from a central fund, with enough money for an increase in the 1000 or so existing part timers. Trusts and deaneries then no longer have a disincentive to employ flexible trainees. The Department of Health has now agreed that this is the solution but has delayed its implementation.

We also disagree with the authors' assertion that flexible trainees will always cost more per hour than full timers. If we compare the December 2002 rates for the various bands, a flexible trainee in band 1a can be actually working up to just under 40 hours per week. For this they will be paid 1.25 times the basic salary. A full time trainee in band 1a can be working up to 48 hours per week, at 1.5 times the basic salary. The proportional payment is exactly the same (40/48=1.25/1.5), so flexible trainees could not be paid any less than this. Similar comparisons exist for other bands. Thus the claim that flexible trainees are more expensive than full timers is not true.

If flexible trainees are working fewer than maximum hours for their band they will be more expensive per hour, but the same is true of full timers. European law requires that flexible trainees work at least half the hours of full time trainees, and in practice nearly all flexible trainees work three or four days a week plus on call or the equivalent.

If we are truly to make the NHS family friendly and allow doctors to work part time, we need to ensure that flexible trainees are adequately paid and trained, and have adequate opportunity to obtain a part time job. The first has been achieved; to achieve the rest we need better organisation and funding, rather than renegotiating the pay deal.

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Interpreting exercise treadmill tests needs scoring system

Editor—In their article on exercise tolerance testing in the ABC of electrocardiography Hill and Timmis emphasised that ST segment depression (horizontal or downsloping) is the most reliable indicator of exercise induced ischaemia.[1] The overall accuracy of the exercise induced ST segment depression is only about 65%.[2 3] This has led to intense research over the past 60 years to develop multivariate models and improve the predictive value of the exercise treadmill test.

One of the most useful and widely cited scoring systems is the Duke treadmill score,[4] which was incorporated in the American College of Cardiology-American Heart Association's guidelines for exercise testing.[5] In the score three independent variables (exercise time, ST segment deviation, and angina index) were taken into account to interpret the result of the test. The following equation will serve as a clinically useful guide to interpret an exercise treadmill test to predict prognosis and plan further management for patients with suspected coronary artery disease.

Duke treadmill score = maximum exercise time in minutes – 5×ST segment deviation in mm – 4×angina index (where 0 = no angina, 1 = non-limiting angina, 2 = exercise limiting angina).

A Duke treatment score ⩾5 indicates low risk for cardiovascular events (predicted 4 year survival was 99%). This population does not need further investigation with coronary angiography. A score <–10 indicates high risk for cardiovascular events (predicted 4 year survival was 79%). These patients require further investigation with coronary angiography. A score between 4 and –10 indicates intermediate risk. Such patients may require further investigation with myocardial perfusion scanning or coronary angiography, or both, depending on the pretest probability.

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Authors respond to criticisms of booked inpatient admissions and hospital capacity study

Editor—This letter is in response to two letters from the NHS Modernisation Agency criticising our work on booked admissions and capacity.[1] The letter by Castille et al rests on the claim that we concluded that the greater the variability in cycle time the less efficient the system[2]: either more resources are required or longer queues will develop. This is a misrepresentation of our work since our paper is not concerned with the length of queues, and we did not make any use of the terms cycle time or efficiency. It is not clear what these terms mean in the context of our work. According to Castille et al, we also assume that variability is inevitable because it has occurred in the past. Although we never made such a claim, this actually seems quite plausible in the absence of evidence that introducing booking systems will cause variability of length of stay to reduce.

The letter by Rogers et al at least discusses what we had written.[3] Although its rhetoric is largely based on assertion rather than proof, interesting issues are raised. For example, the letter asserts that it is possible to predict length of stay on the basis of features such as age and comorbidity. No references are given to support this claim, but we accept that, to a certain extent, such forecasting may be feasible.

Important research questions follow from this. In particular, how precise can such forecasting be, how would one use it in practice, and would it have a major impact on capacity needs? These issues are paramount. Without solid research evidence, which should include mathematical modelling, it is hopeful to assume that such forecasting would remove the capacity problems caused by variability in length of stay.

As it happens, for the intensive care unit example that we used in our paper,[1] a reasonably good forecasting algorithm was available. If we had predicted ahead of time that a patient would spend at most 24 hours in intensive care, we would have been correct 83% of the time. Unfortunately, this relatively high degree of precision does not help.

Our paper has clearly annoyed the NHS Modernisation Agency, which was not our intention; we are, however, pleased to see that the issue of variability of length of stay seems to have been accepted as important.

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Lesbian parenting may make a difference

Editor—In her editorial on adoption by lesbian couples Golombok gave a favourable interpretation of the research findings that is not universally agreed.[1] Even the American Academy of Pediatrics in its technical report about adoption by same sex co-parents sounded a note of caution about the research, saying: "The small and non-representative samples studied and the relatively young age of most of the children suggests some reserve."[2] Also, Stacey and Biblartz criticised the way "researchers frequently down-play findings indicating differences regarding the children's gender and sexual preferences and behaviour."[3]

Golombok quotes her own studies to support her editorial, so I will comment on her main longitudinal study.[4] Lesbian and single heterosexual mothers were recruited in 1976-7. The families were followed up in 1992-3, when the children were young adults. Golombok et al found that 14 of the 25 young adults reared in lesbian homes had considered having a lesbian or gay relationship, compared with only three of the 21 of the young adults reared by single heterosexual mothers (Fp = 0·003). In addition six of the 25 of those from lesbian homes had been involved in a sexual relationship with one or more people of the same sex, whereas none of those from heterosexual homes had had a same sex relationship (Fp = 0·022). Thus they did find significant differences of outcome.

Also two of the young adults reared by lesbians identified themselves as lesbian but

none of the young adults from heterosexual homes did. This difference was not significant but with the small samples and the low incidence of lesbianism in the general population, it does seem to indicate a trend.

Of course, some may not be concerned if young people choose to have lesbian or homosexual relationships. But surely commentators should be willing to admit that what evidence there is does show significant differences in outcomes in this and in other areas.

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Increasing response rates to postal questionnaires

Changing layout of questionnaires increases response rates

Editor—The systematic review by Edwards et al is helpful to those of us who routinely design and use questionnaires in our research.[1] One area not covered by the review, which is important, is the quality of the response.

Response rates are clearly important, but the quality of the responses is also important in that returned questionnaires with some questions either missing or incorrectly filled in will have the same effect as a poor response. In a pilot study of questionnaires for a trial among people with venous ulcers one of us (CPI) noticed that many items on the SF12 were either missed or incorrectly completed. The questions that were missed were those within the stem and leaf format of the SF12; questions in a self contained format were not missed.

We therefore changed the layout of the questionnaire by altering all the stem and leaf questions to self contained ones. We then tested the revised version in a randomised trial among 1500 women aged 70 or over.[2] The rates of questionnaires returned did not differ, but there was a large difference in the proportion of missing or incorrectly completed questions.

Thus 26.6% of women missed or incorrectly completed at least one item in the standard version of the SF12 compared with only 8.5% for the revised version (difference=18.1%, 95% confidence interval 11.1% to 25.1%). The quality of the revised version was tested in a factor analysis, which showed that it had similar internal reliability to the standard version. In short, therefore, attention should be given to factors that improve the quality of responses as well as to response rates.

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Effect of incentives on response rates must be considered

Editor—Edwards et al report on interventions to improve response rates to questionnaires.[1] We agree that a high response rate is important to avoid bias, and we report here evidence that shows the effect of monetary incentives on this source of bias.

We undertook a randomised controlled trial to evaluate the effect of direct payments on questionnaire response rates as part of a study of menopause services in the north west of England.[2] Questionnaires were sent to a random sample of 1000 women aged 40-65, with a payment of £5 for each respondent. Use of the payment incentive increased absolute response rates by 12%. Of note is that the payment group had lower ever use of hormone replacement therapy than the non-payment group (difference = 8.5%, 95% confidence interval of difference 0% to 16.9%, P=0.056), although mean age and level of educational qualifications did not differ between the groups.

More non-users of hormone replacement therapy had responded to payment, which suggested that payment had a larger impact among women for whom the questionnaire had a lower interest (that is, non-users of hormone replacement therapy). The estimate of the prevalence of use of hormone replacement therapy would have been biased without the incentive, in that women who had never used hormone replacement therapy were less likely to reply without it.

Edwards et al have shown the powerful effect of monetary incentives on response rates to questionnaires, and we have shown that such incentives may preferentially increase response rates among recipients with least interest in the subject of the survey, thereby reducing bias. Researchers may need to consider whether the extra cost of incentives is worth while and whether increasing response rates might lead to a more representative sample.

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Slang in clinical practice

Editor—The article on medical slang by Fox et al is fascinating.[1] The use of slang in clinical practice (and not just medical practice) is quite widespread.

Firstly, slang can express the clinician's view of the patient's personality and behaviour. Examples of these include

- Dysphorics
- Black holes
- Medical care abusers
- Bothersome
- Manipulative
- Problem
- Trouble
- Butterflies

Secondly, slang can have pejorative undertones. Some labels used by clinicians have a distinctive pejorative nature, for example:

- Trolls
- Turkeys
- Rubbish
- Odd
- Patients from hell

Thirdly, acronyms and adaptive humour are used. A large part of the slang used by clinicans is adaptive. Much of this type of slang is witnessed in accident and emergency departments, for example:

- Gomer (get out of my emergency room)
- Tatt (talks all the time)
- Tatt (tired all the time)
- Rabbit (rabbits on)
- Teeth (tried everything try homeopathy)
- Sig (stroppy ignorant girl)
- Ttfo (tell them to f*&%k off)
- Pafo (pissed and fell over)
- Grolies (*Guardian* reader of limited intelligence in ethnic skirt)
- Oap (overanxious patient)

All of these terms have been recorded within the medical literature and in my unpublished thesis.[2]

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