Cognitive dysfunction after concussion - Authors did not to comment on the single truly significant result

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Letters

Effectiveness and cost effectiveness of compression bandages should be shown

Editor—Fletcher et al report their systematic review of compression treatment for venous leg ulcers.1 As researchers who perform randomised trials of compression bandages, we echo their concern that the quality of research is poor in this area. A particular concern of ours is that, unlike pharmaceuticals, bandages (which are classified as medical devices) do not need to undergo rigorous clinical testing to establish effectiveness before being released on to the market. Moreover, provided that the product conforms to certain specifications, it may be placed on the drug tariff without trials in patients with the condition. While some manufacturers invest time and effort into performing properly controlled trials with adequate sample sizes to show statistical significance, many are dissuaded from doing so. Moreover, multilayer systems, which the paper indicates perform better than single layer bandages, are classified according to the individual bandages that make up the system rather than as a single unit.

Recently the NHS Executive called for proposals to examine methods that provide improved healing in patients with leg ulceration, and we await its decision on the allocation of funds. It is difficult to imagine, however, how appropriate trials can be performed on perhaps 10 different methods of compression, together with another five or so adjunctive treatments, without a coordinated national approach and suitable investment to achieve the quality required.

Using the results of the Stockport and Trafford study,2 we estimate that at least £20m is spent annually on disposables for leg ulcer treatment in Britain, of which about half is spent on bandages. Clearly, evaluation is needed of the effectiveness of these treatments, which seem to offer maintenance treatment rather than healing.

We believe that, until manufacturers have to prove evidence of the effectiveness and cost effectiveness of their products in a similar way to that required of the pharmaceutical industry, they will be content to continue producing “me too” bandages, for which they know there is a market, rather than introducing innovations in bandage technology. The principal treatment for venous ulceration is high compression treatment. Now seems to be the time to evaluate the methods of delivering this treatment, with the aim of avoiding undue waste and unnecessary suffering by the use of ineffective treatments.

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Claim for major advance in treatment of perforated peptic ulcer seems premature

Editor—In their review of recent advances in general surgery Corvera and Kirkwood conclude that recognition of the importance of Helicobacter pylori in perforated peptic ulcer disease has led to a change in management strategy.3 They advocate a move away from acid-reductive surgery to simple closure with subsequent anti-H pylori treatment. While a reduction in the complexity of surgical intervention might be welcome, the published evidence does not support such a central role for medical anti-H pylori treatment in perforated peptic ulcer disease. Successful eradication of H pylori alters the natural course of chronic uncomplicated peptic ulcer disease,2 and recent controlled and uncontrolled studies have shown that clearance of the bacteria reduces the rate of relapsing after an episode of peptic ulcer haemorrhage.3 The situation for perforated ulcers, however, is much less clear cut. Perforated ulcers may represent a specific subgroup of peptic ulcer disease, and anti-H pylori treatment may have a much less important role (if any). Reinfach et al found that perforated duodenal ulcer was not associated with H pylori infection: 47% of patients with perforated ulcer were seropositive for the bacteria, compared with 50% of matched hospital controls;4 this is clearly lower than the 90-100% positivity in chronic uncomplicated duodenal ulceration. Additionally, the small study that Corvera and Kirkwood cite regarding omental patch repair was uncontrolled and contained no data on eradication of H pylori or long term follow up.5 Thus the authors’ suggested management strategy has not been subjected to formal trial, and the epidemiological association between H pylori infection and perforated ulcer remains in doubt.

Clinicians are unlikely to withhold anti-H pylori treatment in infected patients with perforated peptic ulcers. Follow up is advised after attempted eradication of H pylori from bleeding ulcers to ensure that the eradication has been effective, and it also seems necessary after perforation.6 This leaves the problem of what to do about those ulcers that were never infected. While the policy advocated by Corvera and Kirkwood may be both sensible and effective and may well be supported by future studies, it seems premature to base a claim regarding a major advance in the treatment of perforated peptic ulcer on such slim and contradictory evidence.

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Advice to authors

We receive more letters than we can publish; we can currently accept only about one third. We prefer short letters that relate to articles published within the past four weeks. We also publish some “out of the blue” letters, which usually relate to matters of public policy. When deciding which letters to publish we usually relate to matters of public policy. When deciding which letters to publish we favour originality, assertions supported by data or by citation, and a clear prose style. Letters should have fewer than 400 words (please give a word count) and no more than five references (including one to the BMJ article to which they relate); references should be in the Vancouver style. We welcome pictures.

Letters, whether typed or sent by email, should give each author’s current appointment and full address. Letters sent by email should give a telephone and fax number when possible. We encourage you to declare any conflict of interest. Please send a stamped addressed envelope if you would like to know whether your letter has been accepted or rejected.

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

Letters will be edited and may be shortened.
Debate over screening for gestational diabetes

Screening should take place only in context of good quality controlled trials

Editor—Jarrett and Soares et al present arguments respectively against and in favour of screening for gestational diabetes.1 Jarrett has been arguing objectively against gestational diabetes as a useful concept for many years.2 In contrast, many obstetricians have been arguing objectively against gestational diabetes as a useful concept for many years.3 In contrast, many obstetricians have been arguing for gestational diabetes underlines how the scientific field is mirrored in clinical practice. To evaluate the attitudes of Italian gynaecologists towards the monitoring of normal pregnancy, we sent a postal questionnaire to 504 physicians in charge in 57 obstetric and gynaecology centres affiliated to the Association of Italian Gynaecologists and Obstetricians. Although these 57 centres do not formally represent the Italian obstetric departments, the centres that replied were no different from the average Italian obstetric departments as regards geographical distribution. A total of 283 gynaecologists returned the completed questionnaire. Among several questions was one asking: “During a normal pregnancy do you routinely carry out the O’Sullivan test?” This screening test for gestational diabetes is described by Soares et al. Altogether 151 gynaecologists said that they did carry out the test for all pregnant women (55 gave a 50 g oral glucose load and 26 a 100 g glucose load); 60 said that they carried out the test only for women with risk factors, such as obesity and a family history. The remaining 72 gynaecologists did not believe the test to be necessary during a normal pregnancy.

In conclusion, uncertainty in the scientific field is mirrored in clinical practice: half of the gynaecologists who answered our questionnaire believed that there were benefits in screening for gestational diabetes, and half did not. As Jarrett says, “Much confusion surrounds the topic of screening for glucose intolerance—hyperglycaemia in pregnancy.” International and national scientific societies should produce guidelines for screening for gestational diabetes in order to minimise the discrepancies in practice among gynaecologists.

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Evidence from randomised controlled trial is needed

Editor—Soares et al argue in favour of screening for gestational diabetes, using some of the poorest levels of evidence possible—case-control and cross sectional data, as well as early observational studies hypothesising that intrauterine exposure to hyperglycaemia may lead to type 2 diabetes in the child.1 What is lacking is a large randomised single-masked controlled clinical trial to resolve many unanswered questions. Foremost of these questions is whether screening all women, as is the practice in most centres, leads to lower rates of caesarean delivery, use of forceps, and shoulder dystocia. Secondly, can prospectively defined subgroups be identified from such a trial, on the basis of a family history of type 2 diabetes, ethnicity, or body mass index, to better predict who might truly have insulin resistance during pregnancy and develop it again later in life?

Without such a trial, we have wasted our time quoting studies that have little to do with validating the effectiveness of a screening programme that costs a lot of time and money without evidence of benefit. Until such a trial is carried out, universal screening for gestational diabetes should be postponed.

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Health status instruments should satisfy full range of methodological criteria

Editor—Dorman et al are right to draw attention to the higher response rate achieved with the EuroQoL questionnaire compared with the SF-36 instrument among survivors of stroke.1 Published work on health related quality of life overlooks the burden that instruments impose on patients, studies often failing to report response and completion rates. Dorman et al did not mention, however, that the approach selected to measure health related quality of


life should be methodologically sound and satisfy criteria for accuracy, reliability, validity, responsiveness, generalisability, and sensitivity. Although evidence exists for the test-retest reliability and construct validity of the EuroQol questionnaire,2,3 direct comparisions with other health status instruments suggest that further research is required to improve its methodological robustness and performance in different clinical contexts. Brazier et al tested the completion, reliability, and validity of the SF-36 and the EuroQol questionnaire in an elderly female population.4 Although the SF-36 had poorer rates of completion than the EuroQol questionnaire, it showed greater sensitivity to lower degrees of illness. Further work is also needed to evaluate more fully the sensitivity of the EuroQol questionnaire to change over time. The evidence published by Hollingsworth et al suggests that the EuroQol questionnaire may be less responsive than the SF-36 in assessing change in health status.

The selection of instruments to measure the health related quality of life of patients should be determined by the research questions being asked and the specific characteristics of the patients being studied. All instruments considered for selection should satisfy a comprehensive range of methodological criteria and should not be promoted on the basis of simplicity or response frequencies.

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Effect on mortality of switching from cigarettes to pipes or cigars

Study underestimated difference in risk

Entron—In their study of lung cancer and other diseases in male pipe and cigar smokers, Wald and Watt seem to have underestimated the difference in the risk of lung cancer between male current smokers and lifelong non-smokers (a 16-fold difference in their study).1 Non-smokers accounted for seven of 102 lung cancers in the study (figures for most ex-smokers were not supplied). This is very different from the experience of Capewell et al, who studied 3070 Scottish patients with lung cancer.2 Only 0.7% of men with lung cancer were lifelong non-smokers. My experience in Salford (three (0.8%) non-smokers and 217 current smokers among 380 men with lung cancer) is almost identical with that of Capewell et al. On the basis of local data on the prevalence of smoking, I calculate a 62-fold increased risk of lung cancer for Salford smokers compared with non-smokers. Capewell et al’s figures would also imply a risk of cancer of at least 50-fold for current smokers.

There are several possible explanations for the high proportion of non-smokers with lung cancer in Wald and Watt’s study. Firstly, this was an elite group of subjects (business and professional men), of whom only one a fifth smoked; one would therefore expect proportionally more cancers among non-smokers. In a previous study of professional British male subjects (the British doctors study), however, only seven (1.6%) of 441 deaths from lung cancer occurred in non-smokers.3 Secondly, the number of lung cancers in Wald and Watt’s study was small (102 cases), so the finding of seven cancers in non-smokers (compared with an expected finding of one or two cancers based on the above studies) may have occurred by chance. Thirdly, some long term ex-smokers in Wald and Watt’s study may have described themselves as non-smokers (Capewell et al found that at least a quarter of “non-smokers” were really ex-smokers).

Whatever the explanation, Wald and Watt seem likely to have overestimated the risk of lung cancer in lifelong non-smokers compared with that in larger British studies. It is therefore important to emphasise to smokers in the general population that their risk of lung cancer is far more than 16 times that of a non-smoker and that this risk can be reduced greatly by stopping smoking. It is also likely that pipe and cigar smokers (and long term ex-cigarette smokers) are at greater relative risk of lung cancer compared with non-smokers as suggested in the authors’ study.

B Ronan O’Driscoll
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1 Wald NJ, Watt HC. Prospective study of effect of switching from cigarettes to pipes or cigars on mortality from three smoking related diseases. BMJ 1997;314:1860-3. (28 June.)
3 Doll R, Peto R. Mortality in relation to smoking: 20 years’ experience of Capewell et al, who studied 3070 Scottish patients with lung cancer.2 Only 0.7% of men with lung cancer were lifelong non-smokers. My experience in Salford (three (0.8%) non-smokers and 217 current smokers among 380 men with lung cancer) is almost identical with that of Capewell et al. On the basis of local data on the prevalence of smoking, I calculate a 62-fold increased risk of lung cancer for Salford smokers compared with non-smokers. Capewell et al’s figures would also imply a risk of cancer of at least 50-fold for current smokers.

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“Switchers” will have had higher cumulative exposure to tobacco

Entron—Wald and Watt reported increased mortality related to smoking among cigar and pipe smokers who previously smoked cigarettes (switchers) compared with cigar and pipe smokers who had not previously smoked cigarettes (non-switchers).1 We have some concerns about the validity and interpretation of the findings, and would like to add to the authors’ key messages. The validity of their findings is weakened by incomplete data on exposure. There were no data on duration of smoking before entry to the study, and exposure status was categorized on the basis of a single assessment of current smoking behaviour at the time of entry, with no reassessment during follow up. This could introduce bias if, for example, switchers were subsequently more or less likely to give up smoking entirely than non-switchers. There is also a paucity of data on possible confounding factors other than
blood pressure and blood cholesterol concentrations at entry. Even if the principal finding is accepted as valid, we question the explanation offered for it. We believe that a higher cumulative exposure to tobacco (a known predictor of mortality, particularly from lung cancer) among switchers is more likely to account for the observed differences in mortality than any minor variations in inhaling. This is for two reasons. Firstly, switchers are by definition former cigarette smokers, who, as the paper shows, have a higher consumption of tobacco than cigar and pipe smokers. Secondly, switchers are likely to have had a longer duration of exposure to tobacco since they had all given up smoking cigarettes at least 20 years before the health examination, and there were no reported criteria for duration of smoking among non-switchers.

The study confirms previous findings that mortality is higher among cigar and pipe smokers than non-smokers. Therefore, we believe that healthcare workers should advise cigar and pipe smokers to give up completely and, if the findings from this study are confirmed, could justifiably concentrate their efforts on cigar and pipe smokers who formerly smoked cigarettes as a particularly high risk group.

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1 Wald NJ, Watt HC. Prospective study of effect of switching from cigarettes to pipes or cigars on mortality from three smoking related diseases. BMJ 1987;1:1809-3. (28 June.)
3 American study supported conclusions

**Editor—**Wald and Watt presented results of a prospective study indicating that cigarette smokers decrease their chance of death from ischaemic heart disease, lung cancer, and chronic obstructive lung disease by switching to cigars or pipes. The study was limited by small numbers of deaths, particularly from lung cancer, on which changing smoking habits would be expected to have the greatest impact; an inability to evaluate cigar and pipe smoking separately; and the use of disease mortality rather than incidence. The findings prompted us to re-examine data from a large case-control study of lung cancer carried out at seven locations in Europe.

There were 6919 male incident cases of lung cancer and 13 458 controls, including 573 cases and 1036 controls who smoked cigarettes and cigars or pipes and 13 846 cases and 56 controls who switched from cigarettes to cigars or pipes. Previous analyses concluded that cigarette smokers who switched from non-filter to filter cigarettes or reduced the number of cigarettes smoked per day lowered their risk of lung cancer.1,5

Relative risks of lung cancer were lower for former than current smokers (table 1). In addition, relative risks for cigarette and cigar or pipe smokers were lower than those for cigarette-only smokers but higher than those for cigar-only, pipe-only, and cigar and pipe smokers. Those who switched from cigarettes to cigars or pipes had risks similar to those of cigar-only and pipe-only smokers.

For cigarette and cigar or pipe smokers, relative risks for former smokers declined only if subjects stopped smoking cigarettes (table 2). The relative risk was 10.9 for current cigarette and cigar smokers, increased to 12.4 for former cigarette smokers who continued to smoke cigarettes, and fell to 5.9 for former cigarette smokers who continued to smoke cigars. The relative risk for subjects who stopped smoking cigarettes and cigars was 4.6. A similar pattern occurred for cigarette and pipe smokers. Relative risks were 11.6 for current cigarette and pipe smokers, 11.4 for former pipe smokers who continued smoking cigarettes, 7.6 for former cigarette smokers who continued smoking pipes, and 3.5 for subjects who stopped smoking cigarettes and pipes.

Our analysis showed that cigarette smokers who switch to cigars or pipes reduce their risk of lung cancer, thus supporting the conclusion of Wald and Watt. We also found that mixed smokers who stop smoking cigarettes but continue smoking cigars or pipes also lower their risk of lung cancer, although they continue to incur a risk five times higher than that of non-smokers.

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1 Wald NJ, Watt HC. Prospective study of effect of switching from cigarettes to pipes or cigars on mortality from three smoking related diseases. BMJ 1987;1:1809-3. (28 June.)
3 Lubin JH, Richter BS, Brot LJ. Lung cancer risk with cigar and pipe use. JNCI 1984;75:577-82.

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Authors' reply

**Editor—**The risk of lung cancer among current cigarette smokers compared with lifelong non-smokers in our paper (a 16-fold increase) is virtually the same as that found in the prospective study of British

| Table 1 | Number of cases and controls in study by status as current or former smoker,* and relative risk of lung cancer†; data are on men only |
|-----------------------------|-----------------------------|-----------------------------|-----------------------------|-----------------------------|
| **Smoking status** | **Current smoker** | **Former smoker** | **Relative risk (95% Cl)** |
| cigarettes only | 5131 | 6681 | 11.2 (9.6 to 13.1) | 4.52 (3.8 to 5.3) |
| cigars only | 28 | 98 | 3.69 (2.4 to 5.8) | 2.40 (1.2 to 5.0) |
| pipe only | 35 | 165 | 2.91 (2.0 to 4.3) | 1.54 (0.6 to 4.5) |
| cigarettes and cigars¶ | 138 | 230 | 7.92 (6.1 to 9.6) | 4.12 (2.7 to 6.3) |
| Switch from cigarettes to cigars¶ | 8 | 29 | 3.95 (1.8 to 8.8) | 2.42 (0.3 to 20.9) |
| cigarettes and pipes¶ | 262 | 367 | 9.88 (7.9 to 12.3) | 3.37 (2.3 to 4.9) |
| Switch from cigarettes to pipes¶** | 5 | 15 | 4.57 (1.6 to 12.8) | 2.00 (0.2 to 16.4) |
| cigars and pipes | 17 | 46 | 4.46 (2.5 to 7.9) | 3.83 (1.5 to 10.1) |
| cigarettes, cigars, and pipes | 76 | 111 | 5.12 (6.6 to 12.7) | 4.42 (2.7 to 7.3) |

*Current smokers included those who stopped smoking within five years of date of occurrence of cancer (cases) or interview (controls).
†Reference category was lifelong non-smokers (190 cases and 2617 controls). Relative risks were adjusted for age and study location.
‡Relative risks significantly different for current and former smokers, P<0.01.
§Subjects who consumed cigarettes and pipes or cigarettes and pipes concurrently.
¶Mean of 18 years (median 15 years) from cessation of cigarette use to age at diagnosis (case or interview) (control).
**Mean of 24 years (median 21 years) from cessation of cigarette use to age at diagnosis (case or interview) (control).

| Table 2 | Number of cases and controls for cigarette and cigar smokers or cigarette and pipe smokers by status as current or former smoker* and relative risk of lung cancer† |
|-----------------------------|-----------------------------|-----------------------------|-----------------------------|-----------------------------|
| **Cigarettes and pipes§** | **Current cigarette smoker** | **Former cigarette smoker** | **Relative risk (95% Cl)** |
| Current cigarette smoker | 93 | 138 | 10.9 (7.8 to 15.4) | 5.91 (2.9 to 8.7) |
| Former cigarette smoker | 24 | 34 | 12.4 (7.0 to 22.0) | 4.59 (2.9 to 7.5) |
| Current pipe smoker | 146 | 195 | 11.6 (8.7 to 15.4) | 7.63 (4.8 to 12.6) |
| Former pipe smoker | 88 | 120 | 11.4 (8.2 to 15.9) | 3.52 (2.4 to 5.2) |

*Current smokers included subjects who stopped smoking within five years of date of diagnosis (cases) or interview (controls).
†Reference category was lifelong non-smokers (190 cases and 2617 controls). Relative risks were adjusted for age and study location.
physicians (a 15-fold increase).1 This confirms that our estimate of risk is reasonably accurate. The risk of death from lung cancer in lifelong non-smokers was 7.8 per 100 000 per year (95% confidence interval 3.7 to 16.5) in our study, which was of men aged 35-64 at entry who were followed up for an average of 14 years and 4 months.

Jarvis expresses concern that the men who switched from smoking cigarettes to smoking pipes and cigars (switchers) may have had lower former cigarette consumption than those who continued to smoke cigarettes, in which case there would not necessarily be a reduction in risk because it would be lower anyway. This is possible, although our data suggest that, if so, it had only a small effect. In men aged 15-24 the mean cigarette consumption in switchers and continuing cigarette smokers was the same, and in men aged 25-34 it was on average three cigarettes a day lower among switchers. This indicates that most of the difference in risk between switchers and continuing cigarette smokers is likely to be a reduction in risk as a result of switching.

We agree with Edwards and Jakubovic that obtaining repeated measures of smoking habit would improve the precision of smoking data, but it is remarkable that a single assessment of smoking was so predictive of mortality many years later. If there were any error, it is more likely that it would have masked effects, not “created” them. We believe that confounding is a material issue only with respect to heart disease, and we adjusted for blood pressure and serum cholesterol concentration, which are two factors that are strongly related to ischaemic heart disease. It is unlikely that other factors would introduce significant confounding.

We acknowledge that the amount of tobacco smoked per day may be more important than the extent of inhaling in determining risk of smoking related death, but there is evidence that both are involved. Finally, we were pleased to see the corroborative results of Lubin and Fraumeni.

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Fact that no SHO post was given five years’ approval is worrying

Editor—In his letter about the approval of senior house office posts, Lloyd (the secretary of the Royal College of Physicians) explains that limited approval is given to posts that are found to be inadequate.1 In attempting to defend the college he highlights a fundamental facet of the problem. He states that “each quarter the director of training submits to the coun-

BUPA and the tobacco industry

Chairperson of BUPA in Republic of Ireland is also chairperson of tobacco producer

Editor—I was delighted to see that Wald and Watt, from the BUPA Epidemiological Research Group, were able to state conclusively that there were no conflicts of interest evident in their study on smoking related diseases.2 In the same issue of the BMJ the BUPA Foundation advertised generous research awards for communication, epidemiology, health care, and research. Could this be the same BUPA that has recently established a division in the Republic of Ireland and has had the foresight to appoint one of our most successful businesswomen, Dr Margaret Downes, as its chairperson?

That she is also the chairperson of one of the biggest tobacco producers in the Republic of Ireland (Gallaher) seems to be irrelevant to BUPA. One cannot readily criticise Dr Downes for accepting the offer to chair BUPA Ireland—after all, being chairperson of BUPA must offer some degree of respectability, and the tobacco industry is in short supply of that. Furthermore, it is probably the best each-way bet one could hope for (especially if you work for the tobacco industry)—profits when the smokers are smoking and profits when the smokers are dying.

Surely someone in BUPA can see the potential conflicts of interest in this appointment. If they have difficulty in doing so, perhaps some of the recipients of their largesse in the United Kingdom would point out the hard realities to them and do us all a favour.

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*Fenton Howell is a member of the European Medical Association Smoking or Health and a member of the board of ASH Ireland.

1 Wald NJ, Watt HC. Prospective study of effect of switching from cigarettes to pipes or cigars on mortality from smoking related diseases. BMJ 1997;314:1896-3. (29 June.)

Reply from BUPA Ireland

Editor—Dr Margaret Downes is a highly respected non-executive chairman of BUPA Ireland, whose skills and integrity are widely recognised. As with her many other directorships, she is not involved in the day to day operations or decision making of this company. Her primary responsibilities relate to board and corporate governance matters, for which she is eminently qualified.

It is public policy in Ireland, under the 1994 Health Insurance Act, to operate a community rated system which does not discriminate between subscribers to health insurance because of their lifestyle choices. BUPA Ireland is bound by the public policy requirements.

The fact that Dr Downes is the chair of Gallaher Ireland and of BUPA Ireland is public information, and no attempt has been made to conceal it. She was appointed by BUPA because of her expertise in financial services, health care, and business management: she was the best person for the job.

Martin O’Rourke Managing director BUPA Ireland, 12 Fitzwilliam Square, Dublin 2, Republic of Ireland

Community based programmes can help to manage tuberculosis more effectively

Editor—In many low income countries the cure rates achieved by tuberculosis field programmes (for sputum smear positive cases) exceed the 85% target set by the World Health Organisation.3 However, the decision to favour outpatient rather than hospital treatment of tuberculosis in many such countries has been influenced by several factors not mentioned in Squire and Wilkinson’s editorial4 that were only alluded to in the two accompanying papers.3,4

Programmes to control tuberculosis are often the responsibility of the divisions for public health, primary healthcare, or control of communicable diseases within the countries’ health ministries. Hospitals, particularly at secondary or tertiary referral level (provincial, regional, and university hospitals), usually fall under another division, or even another ministry. Those working in the control programme often have neither the authority nor the status to promote national policy guidelines in these hospitals. This may result in misdiagnosis, idiosyncratic drug regimens, and inadequate documentation and reporting for patients managed by hospitals. In contrast, community centres, clinics, and dispen-
Cognitive dysfunction after concussion

Authors did not to comment on the single truly significant result

Editor—Our finding of an increased rate of cognitive dysfunction among subjects tested within one week of sustaining concussion was unsurprising given the numerous studies pointing to the same conclusion.1 The marginal lack of significance of the binomial test (P = 0.06) is due to a lack of statistical power when only eight subjects are studied. That the lower limit of the 95% confidence interval for the risk ratio should nevertheless lie above unity (1.23) is certainly anomalous, but such discrepancies can arise given the different calculations involved.

Interpretation of significant cognitive dysfunction over 200 days after concussion needed to be deferred until the results for those injured after being tested were examined. It then seemed that there was an increased rate of cognitive dysfunction among subjects whether they were tested before or after sustaining concussion. This pointed to cognitive dysfunction being a risk factor for concussion. That the risk factor had manifested itself more strongly in those subjects who were injured after being tested could have been due to their being relatively older at injury than those subjects injured before being tested (four fifths of whom were injured more than six months before testing). We found a lower rate of cognitive dysfunction among those injured at age ≤ 18 than those injured at age ≥ 19. Strachan et al suggest that this argument would be strengthened if both groups were subdivided according to whether they sustained concussion before or after being tested. The table shows the relevant data.

The table provides only partial support for our argument in that the age effect appears only among those injured after testing. There is, however, substantial confounding between age at injury and whether testing took place before or after the injury. Furthermore, dichotomising age involves a reduction of information. In a stepwise logistic regression we found age at injury to be significantly related to the test score (dysfunctional/normal) (P = 0.017), and thereafter there was no significant contribution of injury before or after testing (P = 0.45). In default of alternative hypotheses, we therefore continue to believe that the poorer performance in cognitive tests of those young men who were tested before they sustained concussion may well be explained by factors related to their relatively greater age at injury.

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Determining prognosis after acute myocardial infarction in the thrombolytic era

Rescue angioplasty after failed thrombolysis may put patients at risk

Editor—Beller brings to readers’ attention the fact that routine invasive procedures after acute myocardial infarction offer no significant benefit over that offered by the routine practice of risk stratification with non-invasive methods.1 We are concerned, however, with the blanket statement that high risk patients should have early angioplasty or rescue angioplasty after failed thrombolysis. This technique should be used with caution.

A meta-analysis by Ellis et al indicated a mortality of 10.6% after the procedure, either

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from the disease process or as a direct complication of the procedure.\textsuperscript{7} Furthermore, this procedure fails in 20\% of cases and those failed cases have a mortality of 40\%. Vigorous clinical assessment is therefore necessary before a patient is classified as being at high risk. Inadequate optimisation of supportive treatment often leads to signs such as hypotension and sinus tachycardia, which in turn predispose to further chest pain, interpreted as postinfarction angina even in the absence of electrocardiographic changes. Chest crepitations related to aging are often confused with those associated with pulmonary oedema. One prime example is inferior myocardial infarction with right ventricular extension. This is due to an occlusion of a dominant right coronary artery, which carries a relatively good prognosis. Suboptimal fluid replacement and the indiscriminate use of inotropic agents without prior careful assessment of left ventricular function with echocardiography and guidance by Swan-Ganz catheterisation lead to patients being classified as at high risk without having prior or incidental left coronary artery disease.

The fact that rescue angioplasty for right coronary artery occlusion is associated with excessive complications\textsuperscript{8} should lead doctors to question whether this form of intervention is putting a patient’s life at risk, turning a relatively benign course into a fatal one.

\textbf{Letters} \textsuperscript{2}

\textbf{Author’s reply}

\textbf{Editor—}Lim and Shiels make a valid point regarding the increased risk of rescue angioplasty after presumed failed thrombolysis, but I never addressed the issue of early angioplasty in my editorial. The thrust of my discussion regarding risk stratification related to the identification of clinical variables and variables determined with non-invasive tests that could be used to select those patients after infarction who are most likely to benefit from coronary angiography and coronary revascularisation.

With respect to clinical variables, I mentioned that the combination of rules in over a third of the lung field, hypotension, and sinus tachycardia on admission was an important observation that indicated a high risk status, since these haemodynamic alterations reflect a large area of myocardium at jeopardy with ischaemia or necrosis, or both; they can also point to underlying multisystem disease or a large infarct, or both. I agree that each one of these haemodynamic changes in isolation is not highly specific for a high risk designation. Certainly, crackles at the lung bases alone without evidence of other signs of left ventricular pump failure can indicate atelectasis or pulmonary disease. Hypotension in isolation, without sinus tachycardia and pulmonary rates, can be due to volume depletion or right ventricular infarction and not be secondary to extensive left ventricular dysfunction.

The main message of my editorial was that a routine invasive strategy for risk assessment before discharge is not superior to a watchful waiting, non-invasive strategy in which patients undergo angiography for high risk clinical findings or for spontaneous or inducible ischaemia within or remote from the infarct zone. Recent data reported from the VA non-Q wave infarction strategies in-hospital trial, in which 920 patients with non-Q wave infarction were randomised to an initial invasive strategy or an initial conservative strategy, support this approach.\textsuperscript{3} At one year after discharge there was no difference in cardiac death or recurrent infarction between the two groups. Also, new data from Yusuf et al showed no difference in outcome for patients with infarction admitted to hospitals with cardiac catheterisation facilities (catheterisation rate 66\%) compared with those admitted to hospitals with no catheterisation facilities on site (catheterisation rate 34\%).\textsuperscript{4}

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\textsuperscript{1} Beller GA. Determining prognosis after acute myocardial infarction in the thrombolytic era. BMJ 1997;315:761-2. (27 September).


\textbf{The caring doctor is an oxymoron}

\textbf{General practice will develop best if “caring” is replaced by professionalism}

\textbf{Editor—}Mackenzie’s hypothesis that the term “the caring doctor” is an oxymoron struck a chord with many doctors I speak to. I have long thought that general practice will develop best if we replace the sham of caring with better professionalism. This does not stop us practising good medicine in a compassionate and considerate manner. We spend a lot of time teaching consulting skills to registrars. Good consulting is not the same as “niceness,” and the term “the caring professions” is patronising and arrogant.

There is still a place for the registered list of patients, but in a computerised world it is a tool for targeting and delivering good medicine—for example, in secondary prevention. The modern world is demanding of us. If general practice is to remain vibrant and attract high quality recruits we have to develop practice beyond the personal attributes of individual doctors. We need to think imaginatively, to continue the drive for better organisation, to use information technology to the full, to recognise the strengths of other members of the team, to delegate
and give up some of our traditional perceived duties to those who often do them better. Surely routine visiting is a thing of the past. Practitioners as team leaders need to have the time and energy to step back and look critically at what the practice does as a whole and not to be afraid to initiate change. Too much of what we do still depends on tradition, rather than planning. This opens the doors to external influences, as we have seen in the past few years to our disadvantage.

Clinical audit should no longer be a threat to most of us, and perhaps we need to open the debate about clinical outcomes in practice. This debate should include the limitations that patients should expect of our services, and their responsibilities towards their own health. It should be aimed at reducing the dependency on doctors that seems to be ingrained in some quarters—for example, for numerous prescriptions for minor illness.

I suspect that patients in general would support attempts to improve services at the expense of personal doctoring, which is probably more important to some doctors than to most patients. Inevitably this may touch on the question of standards, but if the debate is open and the standards explicit then most of us have nothing to fear.

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Frequent callers to cooperatives provoke same feelings there

Enrrow—I strongly disagree with Mackenzie’s contention that patients receive better care from doctors who work in a cooperative than from their own general practitioner.1 In my experience, doctors who work in a cooperative provide safety-first, firefighting medicine to deal with acute problems. The decision rests between admitting the patient to hospital or managing him or her at home until the next day. The lack of notes and an accurate, reliable medical history is certainly not better—and can be a major hindrance. Most cooperatives rapidly get to know their frequent callers, who provoke the same feelings out of hours as they do in their own practice. Familiarity can breed contempt, but being aware of this risk can prevent missed diagnoses. Familiarity can also provide huge benefits for somatisers in rescuing them from repeated investigations by doctors unfamiliar with their history. If all patients were seen by doctors working in cooperatives, fat files would be even fatter and surgeries would resemble hospital outpatient clinics, where patients see a different senior house officer at every visit.

To me the oxymoron is “depersonalised general practice.” Let’s improve our consultation skills and not lose the benefits of long term continuity of care.

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1 Mackenzie GM. The caring doctor is an oxymoron. BMJ 1997;315:687-8. (13 September.)

GPss will soon become extinct

Enrrow—Mackenzie’s personal view was undoubtedly intended to be provocative.1 I was provoked, not only to consult my dictionary on that curious word “oxymoron” but also to challenge Mackenzie’s profoundly negative view of personal care in general practice.

There is a growing danger that the best feature of Britain’s NHS (the family doctor who visits patients in their homes) will soon become extinct, driven by the wall by the primary care physician with his or her burgeoning team and an efficient computer system. What is wrong with being both clinically and administratively efficient and preserving that special bond between ill people and the doctor who knows them best?

—Bring back the individual commitment, the romance, and the poetry into general practice.

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1 Mackenzie GM. The caring doctor is an oxymoron. BMJ 1997;315:687-8. (13 September.)

Out of hours emergencies and continuing problems need different approaches

Enrrow—Mackenzie’s argument in his article entitled “The caring doctor is an oxymoron” is based on the central assumption that, with improving records and information technology, most doctors should have enough information to negate the usefulness of patients seeing doctors who think that they know the patients well.1 I disagree.

It is my experience that in general practice, as in the whole of medicine, the state of medical records and information technology is still far removed from such an ideal. Consultation records are often partial, illegible, or completely absent. Current and past problem summaries are frequently incomplete, and attempting to obtain the information from the patient is time consuming and often fruitless. Therefore continuity of care in general practice continues to be valuable because the familiar doctor’s memory can compensate for many of these deficiencies.

Mackenzie does not mention the time that new doctors take to read even the most perfect of medical records to update themselves fully on a patient’s progress to date. This eats into valuable consultation time. If the patient sees a familiar doctor then this time is saved.

Finally, I take issue with Mackenzie’s argument about the superiority of care provided by doctors working in cooperatives over that provided by the patient’s own general practitioner. Even if this were true in general (which I doubt) it is only partially relevant as it relates to out of hours emergency consultations. By their nature, out of hours emergencies are amenable to care by any general practitioner with only an outline of the patient’s medical history. The more complex continuing problems, with their mixture of physical, psychological, and social aspects, which are the meat of general practice during working hours, require a different approach.

I believe that continuity is essential for efficient care—quite apart from any sentimental attachment to the idea of the “cradle to grave” doctor.

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1 Mackenzie GM. The caring doctor is an oxymoron. BMJ 1997;315:687-8. (13 September.)

GPss could refer patients to clinical hypnot otherists

Enrrow—Mackenzie believes that the term “a caring doctor” is an oxymoron, maintaining that, at least in general practice, “overperson alising care can result in poor delivery of appropriate medicine.”I believe that by removing the personal touch he is missing out on a valuable, but time consuming, therapeutic tool. He dislikes the “caring doctor” approach because of its effect on general practitioners’ morale, saying that what wears general practitioners down is the “constant assault from the same people, which dulls the intellect and forces general practitioners to operate in a completely different way from how their training taught them.” Of course, being nice takes time—a precious commodity. Being nice, however, is a valuable therapeutic tool in itself whose value is often overlooked. Doctors who believe that their abilities should be assessed solely on their performance as clinicians are not necessarily providing all the help that some of their patients need.

Mackenzie’s underlying implication is that good clinicians do not expect to see their patients either for very long or for many repeat visits. So what does that make doctors who cannot seem to get rid of some patients? Are they bad clinicians? Of course not. The patients are benefiting in some way from the visit itself, otherwise they wouldn’t come. But the benefit does not come out of a bottle, cannot easily be quantified, and does not show up in an audit in a positive way.

Once a general practitioner has tried all the medical options, perhaps another form of help is more appropriate for those patients who will not go away. Patients now recognise that doctors do not have all the answers, and it has become acceptable for doctors to receive help themselves. If doctors feel uncomfortable being nice and spending extra time with particular patients then they can now refer them to another source of help, a clinical hypnot otherist: one who has the time to be nice to patients and has been professionally trained to encourage them to take responsibility for themselves and deal with their own problems. This will certainly uncloud the surgery systems, freeing the doctors to see those patients they can help most effectively, reducing their stress, improving their morale, and, dare I say it, even helping the patient too.

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1 Mackenzie GM. The caring doctor is an oxymoron. BMJ 1997;315:687-8. (13 September.)