

Letters

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Motivational interviewing for smokers

Soria and colleagues used trained physicians to give motivational interviewing (MI). Physicians recruited and randomised using sealed envelopes. The distribution of patients favours MI with 114 people in the MI group and 86 in the brief advice group. The *P*-value for the χ^2 for goodness of fit to the binomial distribution is 0.048. Patients in the MI group were more likely to intend to stop smoking or consider it than those in the brief advice group measured by stage of change (*P* = 0.036). Neither of these values prove that randomisation was subverted, but sealed envelopes are notorious for this. Were checks made?

Five people in the MI group received bupropion, but none in the advice group. Bupropion doubles the likelihood of cessation.¹ The authors used logistic regression to potentially adjust for confounders if significant, but this leads to important confounding. Epidemiologists recommend adjusting for a range of potential confounders regardless of their statistical significance.²

The outcome assessment makes interpretation difficult. The outcome is point prevalence abstinence for an undefined period measured by the physician giving treatment. MI patients had up to three sessions with the outcome assessor to motivate cessation, while those in the brief advice arm had one. Might those in the MI arm have felt pressure to declare abstinence when it was not fully achieved? Most smokers have an exhaled carbon monoxide in the non-smoker range by overnight abstinence.³ Additionally, most who are

point prevalent abstinent do not achieve lifetime abstinence, which is the outcome linked to health benefits. If the authors have the data, it would be preferable (and in line with recommendations)⁴ to report sustained abstinence for 6 months between 6 and 12 months, as around 30–40% of these patients will achieve lifetime abstinence.

Soria *et al* compare the odds ratio for MI in this study (6.25) to the odds ratio from the meta-analysis in the Cochrane review (1.56) of individual behavioural support for smoking cessation.⁵ These are not comparable interventions. MI, like brief advice, primarily motivates patients to attempt to stop smoking. Behavioural support assists people who have already stopped smoking. In the UK, we have a network of smoking cessation services, but the rates of advice to stop smoking given by GPs are low and more than 90% of quit attempts do not use this support.⁶ The test for MI is whether teaching GPs these skills could change this.

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Ever been HAD?

If I may be allowed to respond to Dougal Jeffries' letter in the November Journal,¹ I would like to come back with the following:

I must apologise to Dougal Jeffries if I wrongly assumed that his column was suggesting that the use of depression severity measures like the Hospital Anxiety and Depression Scale will encourage antidepressant prescribing.¹ On re-reading his original column, I note that he suggests that the QOF is 'a Trojan horse' aimed at 'subverting general practice to the ends of ... the pharmaceutical industry, and their academic comrades-in-arms'.² I took this to imply that he thought the use of measures like the HAD scale would encourage prescribing. Clearly, I have perceived a subtlety in his column which he didn't intend.

Dougal is not surprised that use of the HAD scale leads to more selective prescribing of antidepressants, but seems to dismiss this as rather unimportant. He challenges me to respond to the deeper thrust of his opinion piece. I share his concern that the routine use of depression scoring scales may detract from the human interaction between doctor and patient, but I think he is wrong in suggesting that it would be almost impossible to gather evidence for and against this proposition. Together with colleagues in Liverpool, we are just about to start qualitative interviews with doctors and patients on their views of the new depression measures. It wasn't really possible to gather such evidence until after the depression measures had been introduced into the contract, since practices who had decided to use the measures voluntarily would not be representative of all GPs.

I would take issue with his suggestion that the QOF constitutes centrally-dictated micromanagement of primary care. In the case of depression, it is difficult to follow NICE guidance without a measure of severity, since guidance differs between mild and moderate levels of depression. However, the subsequent management following assessment of severity will vary widely. I would be very surprised if the management of depression depended only on the severity score. I do not have such a pessimistic view of my colleagues' approach to management. Clearly, the score on the scale has to be considered against the wider clinical background in any one case. Just as we would not decide on the management of a patient on the basis of their peak flow measurement alone, without taking into account past history, concurrent clinical features, and the individual patient's personality and approach to the instrument, nor should we decide on the management of a patient on the basis of their depression score alone. I don't believe my GP colleagues do this, and indeed the audit data I presented in my last letter shows that they do not slavishly follow scores when deciding on treatment and referral. It is for that reason that the QOF rewards them for using the measure, rather than for offering treatment exactly in line with the scores.

There is evidence that we are poor at judging severity without using structured assessments, and I have given him some evidence that using the HAD leads to better targeting of treatment.³ I do not believe that individual GPs should be allowed to follow their own approach to management, where there is evidence that complying with the QOF will improve care. The other side of professional independence is a willingness to change practice over time in the light of emerging evidence. The QOF payments facilitate practitioners to make such changes to their practice.

The proof of the pudding for me will be what happens to antidepressant prescribing rates. In the meantime, we are conducting further research into the effects of the depression severity measures, both quantitative and qualitative.

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Medicalising domestic violence

Fiona Duxbury's thoughtful response to my critique of the burgeoning medical preoccupation with domestic violence is a welcome departure from the familiar resort of promoters of this trend to evasion and caricature of opposing arguments.^{1,2} However, I believe that her advocacy of medical interventions in this area is based on a series of dubious assumptions and that such interventions may do more harm than good.

Dr Duxbury assumes that post-traumatic stress disorder (PTSD) is a coherent disease entity, linking First World War soldiers with 'shell-shock' to contemporary victims of 'intimate partner abuse'. But this is ahistorical: as Professor Simon Wessely has recently insisted, 'shell shock is not just another name for PTSD'.³ It is well known that the official acceptance of the diagnosis of PTSD was the result of a campaign in the U by veterans of the Vietnam War. As psychiatrist Derek Summerfield has observed, this label 'legitimised their "victimhood", gave them moral exculpation, and guaranteed them a disability pension'.⁴ Summerfield notes that a diagnosis originally associated with extreme experiences 'has come to be associated with a growing list of relatively commonplace events', including accidents, muggings, difficult labours, verbal sexual harassment, receiving bad news. Paramedics attending road accidents, police and firefighters on duty at disasters, even doctors treating bomb casualties have all been diagnosed with PTSD. For

Summerfield, PTSD 'has become the means by which people seek victim status — and its associated moral high ground — in pursuit of recognition and compensation'.

Dr Duxbury assumes that naming a patient's experience as PTSD has a liberating effect, although she cites no evidence in support of this extravagant claim. The fact that many welcome this diagnostic label merely confirms the ascendancy of the culture of victimhood in contemporary society. Dr Duxbury further assumes that providing social, medical or psychological 'support' is beneficial. But for many, diagnosis is likely to lead on to prolonged courses of medication or psychotherapy, both interventions of doubtful efficacy and carrying significant risks of adverse effects. 'Social' support is likely to take the form of intervention by the police, the courts and child protection agencies, which in my — considerable — experience, is often damaging to the patient and her wider family relationships.

It is true that I present medical and social approaches to problems of family life as alternatives. Medicalising domestic violence invites professional intrusion into personal and family life in ways that are corrosive of the individual autonomy and civil liberties of all family members, female as well as male. But it is the development of women's autonomy — in both the public and the private realms — that has been crucial to the progress towards more egalitarian and less abusive relations between the sexes that has taken place over the past half century.

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