rare in clinical psychiatric practice. Therefore the next step was to design an effectiveness trial to see whether mental health nurses, without prior experience of CBT could be trained over a short period and then supervised to effectively and safely deliver brief CBT to large numbers of people with schizophrenia in the community. As this involved raters being masked to group allocation, this was therefore not an 'open-label' trial.

In relation to the effect size, it is certainly true that when an antipsychotic is compared with a placebo in drug-naive patients a much larger effect is demonstrable. The patients recruited to this trial were, however, almost entirely stabilised on antipsychotics and had already achieved such improvement from them. The effect size with any psychological treatment added to antipsychotics is always likely to be less than that initially achieved by the medication. We acknowledge that the effect size on symptoms at follow-up is modest but the impact on relapse is significant, clinically and in terms of resource savings, for such a brief intervention.

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Contingency management for substance misuse

Petry (2006) provides a welcome review of contingency management in substance misuse settings and expresses surprise that it has not been employed more widely in Europe, particularly given the greater acceptance of 'harm minimisation' here than in the USA, where contingency management has been championed. This is broadly true but some UK drug services are experimenting with interventions informed by reinforcement principles.

The injectable opiate clinic at the Chelsea and Westminster Hospital in London has for some years used reinforcement principles to target illicit opiate and crack cocaine use. Urine samples are regularly tested and the results used alongside clinical judgement to determine the proportion of a client's total daily opiate dose which may be administered intravenously as opposed to orally. In this way, access to injectable rather than oral opiate preparations is the 'reward' for positive behaviour. Staff increase or decrease the injectable proportion of the client's prescription depending on the client's stability.

As a first step towards developing an intervention study (Medical Research Council, 2000) we completed qualitative interviews with staff and clients to assess attitudes towards the further development of reinforcement methods. Staff and clients both cautiously supported reinforcement principles, and staff perceived clients to be more stable and less likely to use illicit substances under the present reinforcement scheme. Nevertheless, challenges were also highlighted. Most staff had reservations about developing voucher-based contingency management, citing possible increased workloads and a potential for damage to staff–client relationships. Despite a strong commitment to harm minimisation strategies at the clinic, some staff also had ethical objections to the development of voucher-based contingency management.

Our study was small and more research is required to explore the feasibility of voucher- or prize-based contingency management. However, as Petry emphasises, contingency management strategies have a good evidence base in a complex and challenging client group where positive outcomes are elusive. It is surely time to evaluate whether contingency management has a place in UK drug treatment services. Our work suggests that debate about the theoretical basis of contingency management and its ethical implications is needed to win support for experimentation among hard-pressed drug treatment workers in the UK.

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Author's reply: McQuaid *et al* report that clinicians working in their injectable opiate clinic were cautiously supportive of the use of injectable opiates for reinforcement but more hesitant about the use of voucher- and prize-based contingency management procedures.

These perceptions mimic those of clinicians in the USA. Upon initial exposure to contingency management interventions, many clinicians express concerns ranging from hesitation to outright opposition. However, after observing the beneficial effects in practice great shifts in attitude occur. Some who were initially the greatest critics become the strongest supporters of contingency management once they see its benefits with particularly difficult clients.

As in the London programme, critics often evoke 'ethics' to dismiss contingency management. This denunciation is paradoxical, as reinforcement principles upon which contingency management interventions are based are operative in every facet of life. Furthermore, one must wonder about the ethics of withholding an efficacious intervention. It was not long ago that opiate substitution treatment, now considered one of the most effective prevention interventions for HIV transmission, was itself labelled unethical.

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Depression and anxiety after myocardial infarction

Dickens *et al* (2006) stress the importance of detection and treatment of anxiety and depression for quality of life after myocardial infarction and point to the mediating role of energy and fatigue.

We agree that depression following myocardial infarction predicts long-term quality of life and we recently showed that this effect persists after controlling for cardiac condition and quality of life at 3 months post-myocardial infarction (de Jonge *et al*, 2006). However, it is unclear whether and how detection and treatment of depression can counter these effects. In the SADHART study Glassman *et al* (2002) found that the effects of sertraline were modest and appeared to be restricted to depression with an onset before the infarction, but Dickens *et al* found that depression and anxiety which were present before myocardial infarction did not predict quality of life. In the ENRICHD trial (Berkman *et al*, 2003), cognitive–behavioural therapy had modest effects on depressive symptoms at 6 months post-infarction in patients with depression and social isolation, but these effects diminished over time. In the EXIT trial (Appels *et al*, 2005), where the focus of treatment was explicitly on vital exhaustion, only some intervention effects were observed and these were modified by the presence of a previous cardiac history.

We agree with Dickens *et al* that there is a need for improved detection and treatment of depression and anxiety following myocardial infarction but several questions need to be addressed. These include 'can the effects of depression and anxiety be linked to specific subgroups of emotional disorders based on symptoms and/or onset?; 'can interventions that were developed in general psychiatry be applied to depression post-myocardial infarction or should they be adapted?'; and 'how can psychiatric interventions be integrated into regular cardiac aftercare?'

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Authors' reply: We agree that although observational studies have shown that depression is associated with subsequent impairment in health-related quality of life in coronary heart disease, intervention studies have failed to provide convincing proof that treating depression improves this outcome. Previous intervention studies have not addressed this question satisfactorily because the SADHART study (Glassman *et al*, 2002) was not sufficiently powered to demonstrate the efficacy of antidepressants in coronary heart disease and the ENRICHD study (Berkman *et al*, 2003) did not anticipate very high rates of spontaneous remission of depression or unplanned prescription of antidepressants in the control group. The results of these trials, however, together with our own results are valuable for planning future treatment trials.

We also agree that there are many unanswered questions relating to the nature of the association between depression and negative outcomes in coronary disease. As mentioned by de Jonge & Ormel, the timing of the onset of depression (Dickens *et al*, 2004*a*), the specific aspects of depression or anxiety that are associated with poor outcome and the possibility of vulnerable sub-populations of patients (such as those without social support) (Dickens *et al*, 2004*b*) require further investigation. Furthermore, whether the association between depression and negative outcomes in coronary disease is the result of residual confounding by severity of heart disease (Dickens *et al*, 2005) remains unsolved. Further research is required to address these questions, although it is likely that most will only be convincingly resolved through intervention studies.

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High female suicide rates: ecological fallacy or sad reality?

Yip & Liu (2006) present a demographic perspective of female suicide in China, the only country in which the suicide rate is higher among women than men. However, this reversed gender representation also exists in certain communities in other countries. In the Indian subcontinent suicide rates are higher in men than in women but the difference is lower than in most countries: the male:female suicide ratio in India is 1.3:1 (Cheng & Lee, 2000). Suicide among immigrants from the Indian subcontinent to Britain was higher among young married women than men (Soni Raleigh *et al*, 1990). Tadros & Salib (2006) also reported that significantly more Asian women than Asian men killed themselves in Birmingham and Solihull, a clearly reversed gender ratio compared with suicide in the White population and in other ethnic groups in Birmingham and the UK as a whole.

Suicide terrorism is not an egoistic suicide but none the less is a form of fatal self-harm in the legal and human sense and has a distinct underlying political, individual and social logic. The support of and acceptance by the attackers' own communities ensure an endless supply of volunteers who seek 'voluntary violent death' in a bizarre act of so-called martyrdom, in order to promote what they firmly believe to be a just cause. Women carried out 15% (64) of such attacks over the past 25 years (Pope, 2005). Chechen women carried out 60% of all suicide bombings in Russia