results in disagreement about what is morally acceptable and because there is no consensus on the best theories and methods for determining this. Bloch & Green (2006) address this problem and suggest a solution by proposing the combination of two established ethical approaches – principlism and care ethics. Their attempt is laudable but the result may be lacking.

Perhaps the most important flaw is that care ethics is riddled with problems (Rudnick, 2001) that may not be adequately resolved by combining care ethics and principlism. For instance, care ethics encourages an overly paternalistic approach by practitioners, which is illustrated by the parent–child model of physician–patient interaction, as presented by many care ethics proponents. In addition, care ethics may be philosophically redundant, as it may be reducible to more veteran ethical approaches such as virtue ethics and casuistry (case-based ethics), which are also notoriously problematic.

If care ethics is not satisfactory as part of an ethical framework for psychiatry, what could be a better alternative? A promising and relatively novel approach is dialogical ethics, which may need to be combined with justice or fairness considerations (Rudnick, 2002). This approach accepts moral pluralism but utilises sound procedures and processes of dialogue among all parties involved to address ethical problems satisfactorily. Dialogical ethics may be well suited to highlighting and addressing some of the more special problems of psychiatric ethics. For instance, dialogue with patients may sometimes pose special challenges in psychiatry, as it requires particular communication skills and cognitive abilities that may sometimes be deficient in people with mental illness. This deficiency could be addressed by remediation and accommodation strategies, as well as by substitute decision-making (which would also be required to engage in dialogue to address the given ethical problem). Be that as it may, a reconsideration of the ethical framework of psychiatry is needed.

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I found the philosophical discussions of Bloch & Green (2006) interesting, without necessarily revealing anything new. However, I was deeply concerned by the case used as an illustration. It appeared to reflect a rather paternalistic, single-professional, single-agency approach to child protection. Clinically this perspective can lead to serious mistakes. As named doctor for child protection for the Leicestershire Partnership NHS Trust I train other staff to seek advice from me and from the named nurse. There was no mention by Bloch & Green of statutory duties of care to the child. The fundamental principle of paramountcy was not mentioned. It was identified that with a mother with psychosis there was a significant risk of harm to the young child. Once this is identified, the children's social services department should be notified (Department of Health, 1999), and should take the lead role in carrying out Section 47 child protection enquiries. All agencies have a duty to assist in collating and sharing all relevant information, to update on the situation and assist in monitoring the child and providing additional support. Reder *et al* (1993) give many examples where information is known to one or two individuals in single agencies who fail to share it, resulting in the omission of any child protection plan. If anything seriously untoward were to happen to the baby, a thorough case review would be undertaken by the area child protection committee/local safeguarding board and a doctor could potentially be found negligent for failing to carry out child protection procedures. I wonder whether this highlights the need for many doctors to update their child protection training?

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Job satisfaction of mental health social workers

Evans *et al* (2006) address major issues concerning mental health social workers, who are an important part of the multidisciplinary team. Although a remarkable paper, I would like to raise a few points regarding the methodology.

First, a single-item rating scale was used to measure job satisfaction, which I consider a multidimensional construct. It can be influenced by a variety of factors and should have been measured using scales such as the Job Descriptive Index (JDI; Balzer *et al*, 1997) or the Warr–Cook–Wall scale (Warr *et al*, 1979). The JDI assesses the amount of work in the job, current pay, opportunities for promotion, supervision and co-workers. The Warr–Cook–Wall questionnaire covers overall job satisfaction and satisfaction with nine aspects of work, each rated on a seven-point Likert scale with higher scores representing greater satisfaction.

Second, there is no mention of the reliability or validity of scales used to measure burnout and job satisfaction. In addition, the adjusted response rate is only 49% and the profile of non-responders is not included to clarify responder bias. Moreover, stepwise multiple regression would have been more useful than linear regression to investigate the relationship between several independent variables and a dependent variable.

Notwithstanding these limitations, this paper should be an eye-opener to employers regarding the needs of mental health social workers.

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Authors' reply: We agree that job satisfaction is a multidimensional construct, and we measured several features of

job-related satisfaction in addition to satisfaction with one's job itself. Although the measures suggested by Dr Kader would have been appropriate, some of their items overlapped with the Karasek Job Content Questionnaire (Karasek, 1979), and we were keen to avoid such duplication and overburdening respondents. As we were interested in the relationship between satisfaction with one's job and other indicators of job-related satisfaction such as feelings about pay, operational and policy contexts (which were and remain topical because of proposed changes to the Mental Health Act 1983) and feeling valued, it would have been inappropriate to use a multi-faceted job satisfaction scale as a dependent variable. All of the scales used in the survey are well known and have established reliability and validity.

The adjusted response rate of 49%, although low in comparison with experimental studies, is very reasonable for social surveys of this type. We agree that it would have been helpful to know how non-respondents compared with respondents in terms of demographic and other details, but the methodology meant that was not achievable. Nevertheless, we do know that our sample was very similar, demographically and in terms of tenure, length of experience, approved social worker status, etc., to another recent study of mental health social workers (ADSS Cymru, 2005). Therefore we have no reason to believe that these data are not representative.

Finally, although it might have been interesting to present a stepwise regression model, we opted for an 'enter' model in the interests of brevity. Subsequent analyses have shown that a stepwise approach offers little added value.

Like Dr Kader, we hope that the results of our survey are an eye-opener for employers.

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Psychological factors in bipolar disorder

Jones *et al* (2005) have focused on the important although relatively neglected area of psychosocial aspects/intervention in bipolar affective disorder. Although there are several previous reports on the subject by the same group, this study has a better design and a much larger sample size. However, some central issues remain unresolved.

The authors were unable to find dysfunctional beliefs specific to bipolar disorder. Cognitive therapy as practised in depressive or panic disorders attempts to correct characteristic dysfunctional beliefs (Beck & Rush, 2000). In the absence of a specific pattern of dysfunctional beliefs, devising effective and specific cognitive strategies to treat bipolar disorder may be difficult. This is illustrated by the pilot study of cognitive therapy in bipolar disorders by the same group (Scott *et al*, 2001) in which relatively non-specific strategies such as self-management of symptoms, dealing with non-adherence, anti-relapse techniques, etc. were employed. The lack of precise techniques could also have resulted in the differential efficacy of cognitive therapy, with effects mainly on depressive, rather than manic symptoms.

In the current study Jones *et al* used a 24-item sub-scale version of the Dysfunctional Attitude Scale, whereas in earlier studies (Scott *et al*, 2000; Scott & Pope, 2003) a 40-item scale was used. It is not clear whether the use of different versions of this scale contributed to the ambiguous nature of the dysfunctional beliefs found in bipolar disorder, especially since the two different versions appear to have different sub-scales. Finally, although some potential confounding variables, such as current mental state, were controlled for, others, such as duration of illness, severity, chronicity and possible effects of pharmacophrophylaxis, were not. Cognitive style may vary according to these factors (Scott & Pope, 2003) making it necessary to control for them.

It is possible that these concerns will be addressed by future research. This study paves the way for examination of psychosocial factors in bipolar disorder.

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Authors' reply: We are pleased that Biswas & Chakrabarti highlight the strengths of our study design and large sample size, and consider our work a significant contribution to understanding psychological factors in bipolar disorder. We agree that it is important to consider potential confounders and therefore examined the effects of differences in illness duration and severity. Although there were some differences between our two patient groups on measures of illness severity and a small number of modest correlations between illness severity and cognitive style, covarying for these measures had no effect on our finding that those with bipolar disorder have fragile self-esteem and dysfunctional beliefs similar to those of people with unipolar disorder. We have not been able to examine the possible effects of pharmacoprophylaxis on cognitive style, but agree that this could be a target for future research.

We do not think we would have found differences in cognitive style between participants with bipolar and unipolar disorder if we had used a longer version of the Dysfunctional Attitudes Scale (DAS). The 24-item version used in our study was factor-analytically derived from the longer version and has improved robustness (Power *et al*, 1994). The 'need for achievement' and 'dependency' sub-scales of the 24-item DAS comprise items from the 'perfectionism' and 'need for approval' sub-scales of the 40-item DAS respectively.

We hope that future studies of cognitive style in people with mood disorder will build on the strengths of our study by using