Correspondence
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Ethical framework in clinical psychiatry

Bloch & Green (2006) provide an excellent overview of the ethical issues that are encountered in clinical psychiatry and the different available frameworks for understanding and resolving them. What is striking, however, is that this discourse is almost entirely without reference to God or religion.

Such pragmatic atheism is, of course, not at unusual these days. It is a reflection of the impact of the Enlightenment upon our understanding of the way in which public discourse on such matters should be conducted. Indeed, there are even avowedly religious writers (such as Bishop Richard Holloway) who consider that it is unhelpful to bring God into such debates (Holloway, 2000). However, it is still remarkable that an entire article of this kind fails even to mention the matter.

It is remarkable, for example, that the important historical influence of Judaeo-Christian ethical thinking upon the culture in which we live is apparently entirely ignored. It is equally remarkable that the religious pluralism of contemporary Western culture is not addressed.

The omission is also remarkable, however, because it avoids discussion of the possibility of a point of reference for both rule-based and character-based ethics which might actually transcend that of the human parties involved. Again, I recognise that there are those who will deny that such a point of reference exists – but surely the discussion about whether or not it exists, its potential impact and the plurality of views about its existence is rather important.

Declaration of interest

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Bloch & Green are to be congratulated for their lucid and helpful paper, which deserves to be read widely. The progression from Kant to the ‘ethics of care’ seems similar to that from the Old Testament rules/laws/commandments to the New Testament injunction of Christ that people love one another, consonant in turn with the recommendations of other faith traditions about developing wisdom and compassion together.

Bloch & Green’s paper resonates with my sense that, as professional caregivers, we do well to acknowledge our own journeys towards personal, moral and spiritual maturity, as described by James Fowler (1981), who draws on both Erik Erikson and Lawrence Kohlberg. Dilemmas such as that described in the vignette offer people opportunities to grow wiser. Grieving losses occasioned by our limitations on the way, we may develop an incremental degree of emotional equanimity, enabling more detached observation and closer engagement.

By staying calm in difficult situations, we foster the trust of others, which is paramount in encouraging people at least to share – and sometimes with relief to relinquish – decision-making and control. Authority comes not only from a professional role and medico-legal powers, but crucially also through a competent, composed and thereby reassuring personal demeanour.

The subjectivity involved should not require an apology. On the contrary, it is essential in allowing us properly and privately to reflect later on our part in what has occurred. This aspect deserves greater emphasis in medical and psychiatric education; for is it not at the heart of why we choose our profession? We want to be good people as well as good doctors, and passing exams is only the half of it.

I disagree, therefore, with the authors’ comment, ‘Nothing extraordinary is required of [the doctor]’. Consistently selfless devotion to the well-being of others is, sadly, well outside the ordinary these days; but it is exactly what we might choose to ask of ourselves if we are to get the fullest satisfaction from our professional lives. An ethical framework such as Bloch & Green have generously provided is welcome, but they are surely telling us that protocols alone will simply not be enough.

Declaration of interest

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Medical ethics is in crisis and psychiatry is not exempt. This is so because the pluralism of contemporary democratic society
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 satisfac-
tion 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 in-
terested in the relationship between satisfac-
tion 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 vari-
able. 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 experi-
mental 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 respon-
dents 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, de-
monographically 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 in-
teresting 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.

**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 dys-
functional 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 em-
ployed. 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 Dysfunc-
tional 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 dif-
f erent 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 pharma-
cophrolyxiasis, 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.

**Authors’ reply:** We are pleased that Bis-
wass & Chakrabarti highlight the strengths
of our study design and large sample size,
and consider our work a significant con-
tribution to understanding psychological
factors in bipolar disorder. We agree that
it is important to consider potential con-
founders and therefore examined the effects
of differences in illness duration and se-
verity. Although there were some differences
between our two patient groups on mea-
sures 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
What is pathological lying?
The article by Yang et al (2005) is provocative, thoughtful and intriguing and provided much food for thought. Participants were divided into three groups: liars, normal controls and antisocial controls. Half of those in the liar group were malingerers and the others displayed conning/manipulative behaviour on the Psychopathy Checklist – Revised (PCL–R), deceitfulness criteria for DSM–IV antisocial personality disorders or pathological lying as defined in the PCL–R. Yang et al referred to pathological liars specifically in the title of their paper but we are concerned that the definition of liars was so broad and wondered whether the article would not have been better entitled ‘Prefrontal white matter in liars’. The authors included individuals with different lying characteristics in a group of pathological liars and this is problematic.

Our recent review (Dike et al, 2005) showed that the term ‘pathological lying’ has been used differently in the literature from how it was used by Yang et al. Pathological lying is distinct from malinger or the other forms of lying exhibited by those included by Yang et al in the liar group. We defined pathological lying as ‘falsification entirely disproportionate to any discernible end in view, may be extensive and very complicated, and may manifest over a period of years or even a lifetime’. Pathological lying is a repetitive pattern of lying for which an external reason (such as financial gain) often appears absent, and the psychological basis is often unclear. This definition has not been accepted by the psychiatric community but summarises the elements of pathological lying. Interestingly, we found that pathological lying can also be found among successful individuals without a history of criminal behaviour.

We commend Yang et al for investigating the neurobiological basis of lying. Whether the prefrontal white matter changes indicate a causal relationship with lying or just an association is unknown. However, pathological lying per se was not specifically investigated, as suggested.

Authors’ reply: We wholeheartedly agree with Dike et al that the definition of ‘pathological liar’ is vague and confusing. Although pathological lying has been defined in several different ways, no specific psychological test is available. Hence we applied a symptom-based approach and defined individuals as ‘liars’ if they fulfilled: (a) criteria for pathological lying on the Psychopathy Checklist – Revised (PCL–R), (b) criteria for conning/manipulative behaviour on the PCL–R, (c) the deceitfulness criterion for DSM–IV, or (d) criteria for malinger as reported in a self-report crime interview.

We maintain that our study did investigate at least one form of pathological lying. In a new analysis, we found that 42% of our liars had psychopathy, antisocial personality disorders or borderline personality disorder. These liars likely correspond to those Healy & Healy (1926) refer to as ‘secondary pathological liars’ – people whose lying is a complication of disorders such as those above. The other 58% of our group, who did not meet this comorbid requirement, probably correspond to the ‘primary pathological liars’ described by Healy & Healy – people who habitually lie but do not demonstrate symptoms of a clearly defined psychiatric disorder. This new analysis also revealed that liars with or without psychiatric disorders showed significantly increased prefrontal white matter volume compared with antisocial controls ($P=0.003$, $P=0.01$, two-tailed respectively) and normal controls ($P=0.005$, $P=0.014$ respectively). Although our study is a preliminary attempt to reveal brain abnormalities in people who lie, cheat and deceive we hope that it will stimulate interest in this important but understudied phenomenon.
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