

The BDI of the beholder: an eye to experimental design

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We are pleased our paper (MacInnes *et al.*, 2001) prompted Cole and Sumnall (2002) to write to the Journal and welcome the opportunity to reply.

Cole and Sumnall's main point is that the Beck Depression Inventory (BDI) mean score (9.2) for our group of ex-chronic ecstasy users (MacInnes *et al.*, 2001) coincided with that reported for the healthy volunteer control group (9.1) in a study by Tse and Bond (2001) in the same issue. This, they feel creates a dilemma in interpretation. Non-patient samples yield variable results on the BDI (Beck *et al.*, 1988) and can differ according to age, gender and other demographic variables (Enns *et al.*, 2000; Blanchard *et al.*, 2001; Ostler *et al.*, 2001). Indeed, we could cite the example of Reif and co-workers (2001) where control group BDI was only 2.7. Thus cross-study comparisons have very little meaning, the relevant comparison is within a study. Tse and Bond (2001) were able to use the experimental model with random allocation for the design of their comparison of citalopram with placebo, but investigations of users of illicit drugs are restricted to field-studies for ethical reasons.

We matched our controls for age, gender, ethnicity and educational attainment and stand by our interpretation of the significant difference in BDI scores between our control sample (mean BDI 5.2) and ex-chronic ecstasy user group.

Cole and Sumnall (2002) are not correct in assuming we believe the score BDI of 9.2 in our ex-chronic ecstasy users indicates 'mild depression'. The purpose of the BDI is to establish symptom severity, not diagnostic status (Peirson and Herbert, 2000) and, as shown by the title of our paper, we suggest only that our former chronic users of methylenedioxymethamphetamine (MDMA or ecstasy) report 'mild depressive symptoms'. Beck *et al.* (1961) emphasize that their instrument is 'aimed at registering varying degrees of depression along a continuum, it is not designed to distinguish among standard diagnostic categories'. It is for this reason that it is a useful tool with which to examine depressive symptomatology in non-clinical samples. However, we did indeed include the subsidiary remark that Beck and co-workers suggest a score of 9 represents mild depression in our Discussion but the reference for this should have been the Beck Depression Inventory Manual (Beck and Steer, 1987), rather than Beck *et al.* (1961).

Cole and Sumnall (2002) also claim there is no direct evidence to suggest that ecstasy users are at risk of '... developing "a more depressive syndrome" as a direct consequence of their ecstasy use'. We entirely agree. The control-group comparison was only one part of our study and we went on to investigate our ex-user group

in some depth, finding that former chronic ecstasy users fulfil a vulnerability model of depression (Hammen and Cochran, 1981), including a negative attributional style or, as Beck (1991) would state, a negative cognitive shift, i.e. they perceive the outcome of future events as ending in failure or loss. Whether this is cause or consequence of previous use of ecstasy we cannot tell at this stage, but it is from this finding that we predicted a risk of our ex-users developing a 'more depressive syndrome'.

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