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Letter to the Editor

Response to the Letter to the Editor, "Depression not related to lower religious involvement in bipolar disorders?"

To the Editor:

We would like to thank the authors of the letter, "Depression not related to lower religious involvement in bipolar disorders?" (1). They have identified further study analyses that could shed more light on the issue of bipolar affective episodes and religious involvement. The authors of the letter point out that, in contrast to our findings, prior studies have found that a significant proportion of individuals suffering from bipolar disorder psychoses (15–22%) have religious delusions or 'enlightenment' (2). They hypothesize that religious delusions in mania are associated with, or predispose individuals to, the use of religious activities as a coping strategy in depressed and mixed states of bipolar disorder. Also, they discuss one methodologic limitation, the lack of a control group, that reduced our ability to detect a relationship between depressive and/or manic states and the Duke Religion Index (DRI) subscale scores (3). To address this methodologic concern, we performed a multinomial logistic regression analysis to assess the authors' hypothesized relationships while controlling for the covariates in our initial analyses (i.e., gender, race, age, income, anxiety, alcohol use, and disability). Mutinomial logistic regression was chosen because it allows comparison across the different bipolar episodes at once with euthymia as the reference group. DRI subscale scores were again entered into the model separately due to multicollinearity. These analyses revealed a positive and significant relationship between private religious activities and depression [odds ratio (OR) 1.229, 95% confidence interval (CI): 1.004–1.504, $p = 0.0458$] as well as mixed states (OR 1.419, 95% CI: 1.155–1.744, $p < 0.001$) compared to euthymia. We did not find a significant relationship between mania compared to euthymia on the DRI subscales of religious attendance, private religious activities such as prayer/meditation, or intrinsic religiosity. This suggests, as the authors propose, that prayer/meditation may be a signature form of coping in both depressed and mixed states of bipolar disorder.

With respect to our present findings, two limitations inherent to the nature of our dataset must be taken into consideration. First, patients were excluded if they presented delusions or hallucinations at the time of study entry (i.e., these symptoms precluded informed consent). This limitation may explain our negative finding between DRI subscale scores and mania. Including individuals with delusions would help to discern whether prayer/meditation is potentially a signature form of coping in bipolar disorder in general or just with specific mood states. Second, the cross-sectional nature of the data only allows us to determine associations but not to identify potential directions of association or causation. Future longitudinal controlled studies are needed to test the authors' hypothesis of a link between religious delusions in manic states and use of religious behaviours in depressed states. Taken together, the analyses from our paper and this response to the letter provide a broader snapshot of the religious behaviours and activities of bipolar disorder patients. We hope these analyses spur future research that will further expand our understanding of the impact of religious attitudes and behaviours on the health and well-being of individuals who suffer from bipolar disorder.

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