Reviewer’s report

Title: Effect of religiosity/spirituality and sense of coherence on depression within a rural population in Greece: the Spili III project

Version: 2 Date: 27 March 2015

Reviewer: amanda shallcross

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Major Compulsory Revisions

1) This study is framed in terms of addressing whether religiosity/spirituality and sense of coherence (SOC) buffer individuals from experiencing elevated depressive symptoms in the face of stress. However, stress was not measured in this study. Further, an adequate test of the protective or ‘buffering’ effect of religiosity/spirituality and SOC requires longitudinal data, which this study does not present.

2) The terms and constructs in the introduction were not adequately characterized. For example, SOC is described as an “important health promoting resource that induces a positive perceived state of wellbeing.” The definition of this construct requires further explication: e.g., that it refers to a level of confidence within and control over one’s environment and includes key processes/experiences: comprehensibility, manageability, and meaningfulness. These sub-components were not discussed until the methods section. Relatedly, the authors review literature reporting relationships between optimism, hedonic and eudemonic well-being and risk of cardiovascular events without defining such constructs and explaining how they converge with or differ from SOC and religiosity/spirituality. Even more concerning, however, is that there is an extensive body of literature that has examined these constructs (particularly trait optimism) in the context of stress and depression, and yet none of this work was reviewed here.

3a) The authors seem to suggest that examining depression as an outcome within a Greek population would be a novel contribution because “there is much discussion regarding the co-morbidity of depression and mental health disorders with various physical diseases, including diabetes and in a period where Greek people are struggling from the past years of economic crisis.”

I don’t find this to be a compelling argument because there have been studies in proximal countries that have experienced economic strife and found correlations among these variables in the expected direction (Papazisis et al., 2013). Indeed, in the discussion, the authors review literature (that should be included in the introduction, but was not) that has already demonstrated the relationships between the variables in the present study: “Similar findings on the negative association between religiosity and depression were recorded from a meta-analysis of 147 studies [25]. Similar results were indicated in a cross
sectional cohort study among patients with advanced illness, which aimed to examine the association of spiritual well-being with symptoms of anxiety and depression [26 ]."

3b) The authors go on to state: “Therefore, it was of interest to explore in more detail the underlying mechanisms that may contribute further to the hypothesis of the existence of bio-psycho-social protective factors, through the utilization of data from the Spili III study.”

However, this study was not poised to examine ‘underlying mechanisms’ or ‘protective factors’ and causal relationships between these variable cannot be understood with cross-sectional data. This, in combination with the less than convincing evidence for the novelty of this study’s findings, compels me to question the scientific contribution to the literature that this study can provide.

4) In the Discussion, the authors mention that one of the main findings of this research was “that following adjustments, greater spiritual well-being was associated with fewer depressive symptoms.”

Which ‘adjustments’ are the authors referring to: age and gender, family status and religion? Did none of the 147 studies in the meta-analyses adjust for these variables?

5) In the Discussion, the authors state: “The main finding of the current analysis is that highly religious participants as identified by the RFI-SRB scale, presented a lower likelihood of depression presence, as indicated by BDI scale. This is corroborated with another finding that participants with high SOC had a statistically significant negative association with BDI. The second finding may provide a theoretical mechanism that can assist in the interpretation of the first finding. An interesting hypothesis on the pathophysiological mechanisms that could explain these data might be that the existence of specific health assets in this population could exert a buffering effect on neuro-endocrine consequences of stress [1, 2], by reduction of inflammation and inhibition of cortisol hypersecretion [13, 14].”

I don’t understand what the authors mean by ‘the second finding may provide a theoretical mechanism that can assist in the interpretation of the first finding?’ Are they suggesting that SOC might mediate the relationship between RFI-SRB scores and depression? Although a true test of mediation requires longitudinal data, why didn’t the authors examine statistical mediation (e.g., the indirect effect of religiosity/spirituality to depression through SOC?)

Minor Essential Revisions

1) The participants in this study appear to be from the SPILI III cohort. What are the clinical characteristics of this sample? The authors mention that “twenty-five individuals, reporting “good health”, declined our invitation”. Is this a chronically diseased sample? If so, physical health diagnosis variables should be included as they are not separable from mental health outcomes. Minimally, they should be controlled for (even if self-reported using physical health symptom inventory, if available).

2) Did the authors have a priori rationale for testing differences in each of the
variables in the model by gender?

3) Why are there no results for the RFI-SRB scale in model 3 of the regression analyses?

4) The results from R squared don’t seem positioned properly in the table

5) The reference to: “Multiple linear regression analysis of BDI scale in relation to demographic characteristics and RFI-SRB or SOC is tabulated in Table 2”

**Level of interest:** An article of limited interest

**Quality of written English:** Needs some language corrections before being published

**Statistical review:** Yes, and I have assessed the statistics in my report.

**Declaration of competing interests:**

I declare that I have no competing interests.