Author’s response to reviews

Title: The relationship between existential well-being and mood-related psychiatric burden in Indian young adults with attachment deficits: A cross-cultural validation study

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Comments to reviewer reports:

John R. Peteet (Reviewer 1): This is a well described study building on previous work, with clear explanations of the findings and their potential implications, as well as a reasoned acknowledgment of the study's limitations. My only concern is that I may not be able to adequately evaluate the statistics.

-&gt; We thank the reviewer for his concise and positive remarks.

Klaus Baumann (Reviewer 2): This is a well written and very interesting paper reporting a replication study in an Indian context with young Hindu adults, replicating a study published in 2011 with young Austrian adults with Catholic upbringing. A such, I strongly recommend its publication, after making use of my following observations and suggestions which hopefully add even more value to this paper.

-&gt; We thank the reviewer for his concise and positive remarks. We have addressed all comments and made several changes to the manuscript (see detailed below).

1. All through your paper, like in 2011, you speak of mood pathology, while the participants are NOT patients in a clinical setting. Rather, as in l. 173, it is more adequate to speak of "psychiatric burden" in the dimensions of the BSI. This should be stated more explicitly, and therefore questions the title of the paper. l. 314 you state that maybe even you have an un-representative high amount of healthy participants (cf. also next point: sampling). Cf. only in l. 314: additional studies in clinical settings are needed, as is also stated in the 2011 paper.

-&gt; We have now revised our wording throughout the paper and in the title so that we no longer refer to “mood pathology” but rather to “mood-related psychiatric burden”.

2. Please report your sampling more transparently: l. 149-150: how did you get the sample of N=541? What was the return rate? l. 188: In the results, you say 443 participants completed all parts. This means that 98 questionnaires were excluded. Any ideas for the difficulties of the participants to complete them?

-&gt; We thank the reviewer for his concise and positive remarks. We have addressed all comments and made several changes to the manuscript (see detailed below).
We have now revised our sample description to include more information on the participants as well as missing data. This now reads as follows:

“The sample consisted of 541 students and post-graduates between 18 and 30 years of age at Karnatak University, Dharwad, India. Participants were recruited randomly; the number of those declining participation was not assessed. All questionnaires were completed in a physical, pencil and paper, format. As some studies indicate that religious orientations differ in their association with mood-related psychiatric burden [37, 38], we focused on the most common religious orientation in India and therefore included only participants with a Hindu upbringing. Consequently, 56 participants with other religious orientations were excluded. After also excluding participants with missing data (n = 42), 443 participants with completed questionnaires and were selected for data analyses. Regarding the missing data, there was no clear pattern indicating that participants had difficulties with specific questionnaires or items.” (p. 7-8)

3. Results
3a: l. 219 you report: "In addition, lower RSWB was also related to an increased amount of mood pathology (r = -.15, p &lt; .01)." This correlation is actually only marginal / negligible and should be stated as such (or omitted). Without this clarification (or omission), readers who are not familiar with these statistical values might be misled. (Actually, not every statistically significant result is also a meaningful result to be reported beyond the tables.)

3b: l. 234 you report: "RWB was unrelated to all dimensions of mood pathology." How do you interpret this result? It should be discussed more (than you do in the discussion part where you only repeat the statement/ result without further reflecting it) in the light of the theoretical model of Kirkpatrick and Granqvist you seem to subscribe to (cf. introduction). To which extent could it question or confirm their affirmations and differentiations of theory (cf. e.g. Granqvist et al. 2010 Religion as Attachment: Normative Processes and Individual Differences. Personality and Social Psychology Review 14/1:49-59. Granqvist 2010 Religion as Attachment: The Godin Award Lecture. Archive for the Psychology of Religion 32/1:5-24. DOI: 10.1163/157361210X487177; Granqvist &amp; Nkara 2017 Nature meets nurture in religious and spiritual development. British Journal of Developmental Psychology 35:142-155. DOI:10.1111/bjdp.121)

Conversely, RWB – including general religiosity, connectedness and transcendent hope – may be more closely related to social contexts and therefore to the correspondence pathway [50]. This consequently may explain why RWB did not contribute to the prediction of the mood-related psychiatric burden independent of attachment parameters.” (p. 15).

4. Discussion
Your discussion is concise and very much to the point, maybe even presupposing concepts (esp. e.g. correspondence and compensation pathway) unfamiliar to readers who are not familiar with Granqvist's work. Why not add one or two sentences to explain them and make your discussion more intelligible to them?

We have now added more information on the concept of correspondence and compensation pathway. This now reads as follows:
“As stated above, the relationship to a higher power can fulfil the criteria of an attachment bond and can consequently be assumed to have similar psychological advantages [27]. This association between attachment and spirituality can be explained by two hypotheses [27]: On the one hand, Bowlby’s [47] correspondence hypothesis states that mental models can generalize across various attachment relationships and therefore may also extend to the relationships with a higher power. On the other hand, Ainsworth’s [48] compensation hypothesis suggests that an attachment to a higher power may be developed as a surrogate for secure human attachment figures. Therefore, correspondence and compensation hypotheses can be seen as two pathways to as well as two ways of being religious [49]. In addition, the compensation pathway implies that secure attachment can be learned even later in life and without support from another human individual [49].” (p. 13)

5. Limitations and Conclusion
The limitations are put very well and the conclusions are both plausible as well as positively humble, deserving attention and reception in the scientific community.

One element could be added. You state the limitation of the presumed connection between attachment and spirituality:

l. 309-311: Regarding attachment, our assessment of the current attachment style - although largely based on past experiences [48] - might not be as strong a predictor of spirituality. This could be part of the problem of self-reports in questionnaires, esp. as to Religion/ Spirituality. Cf. Granqvist 2010, 20, agreeing to Wulff 2006. (Again, the above remarks on the sampling get salient).

-&gt; We have now extended our statement regarding self-report measures to include measures of religion/spirituality. This now reads as follows:
“Furthermore, the inherent problem of self-report measures (e.g., defensive responding) [50] may have influenced the assessment, especially of attachment and RSWB.” (p. 15)

Hopefully you can make constructive use of these observations either integrating or discussing them. Looking forward to seeing your research paper published soon - kind regards

Ralph L. Piedmont, PhD (Reviewer 3): The purpose of this study was to examine the relationships between attachment style and spirituality in a sample of Indian youths. The goal was to determine whether obtained patterns of associations among these variables found in Western samples (e.g., Austrian) would be replicated in this Asian group. In comparing the Indian sample with Austrian data, some differences were obtained between the two groups. In examining the role of spirituality and attachment style in predicting mental health factors, it was found that the spiritual variables did not make a significant contribution. Overall, a straight-forward study, however, there are some issues to be addressed.

-&gt; We thank the reviewer for his extensive remarks. We have addressed all comments and made several changes to the manuscript (see detailed below).

1) It was noted on p. 9, when comparing the two cultural samples that there were differences in age and gender, so these variables were used as covariates in making the comparison. However, when only examining the Indian data, on p. 10 it was stated that in order to maintain comparability with the Austrian sample, gender was used as a covariate, but not age. Why was this not done? This seems inconsistent.

-&gt; We apologize for the potentially misleading description. Regarding the hierarchical regression
analyses we did not intend to ensure comparability with the Austrian sample but rather with the hierarchical regression analyses performed in the Austrian sample. We therefore chose to include the same predictors in the same order as we had done in the hierarchical regression performed with the Austrian sample. We have now adapted the description as follows to avoid confusion:

“In the hierarchical regression analyses, sex was entered as a control variable at Step 1 to ensure comparability with the analyses performed in our previous study [11].” (p. 10)

2) Because there were multiple comparisons being done, a .01 alpha level was selected to determine significance. While this is insufficient a control for this study, the authors are inconsistent in using this cut-off. A number of their findings, especially those relating to the spirituality component, were found to reach only the .05 level of significance, which according to these authors should be interpreted as being NON-significant. However, the authors attempt to have it both ways by noting that these findings were “trend significant.” I am not familiar with this term, but it is inappropriate to interpret/present these results as being significant when they failed to meet the a priori criterion cut-off for significance. As such, it is clear from these data, that the religious/spiritual constructs had NO significant predictive effects in this study. That needs to be made very clear. Further, given the number of correlated comparisons that are being made in this study, I do not understand why a overall multivariate omnibus test was not conducted. This would have been the preferred analytic approach rather than the weaker, less specific alpha adjustment that was done. A multivariate test would be more powerful in controlling for experiment-wise Type I error rates.

-> We agree with the reviewer’s reasoning that the use of a .01 alpha level and the reporting of results that only reach the .05 level of significance is inconsistent. To avoid the notion of “trend significant” effects and in light of the explorative nature of the study as well as the intendent comparability of the statistical analysis with our previous study, we have now set the alpha level to .05 and adjusted all descriptions accordingly. While we agree with the reviewer, that other more stringent analyses could have been performed, we feel justified in applying a .05 alpha level as our focus is not on the significance of single results but rather on the overall pattern of results. We are furthermore aware that significant results are not per se “relevant” and have therefore also included effect sizes for the group comparisons. Furthermore, we have added the reviewer’s suggestions regarding statistical analyses to our limitations. This now reads as follows:

“Regarding the statistical analyses, alpha was set to $p < .05$ due to the explorative nature of the study. While the results have therefore to be interpreted with some caution, a more stringent alpha or different (e.g., multivariate) analyses were not applied as this may have promoted a premature exclusion of potentially relevant variables in future studies.” (p. 15)

3) Further elaborating on #2 above, the authors inappropriately interpret their findings with the religious/spiritual (R/S) constructs. As noted above, the regression results clearly demonstrated that these constructs did NOT reach their set level of significance. Undeterred, they continue to make substantive interpretations of these non-significant findings. This is misleading and inappropriate. What is clear from these findings is that R/S constructs have nothing to do with negative affect. This finding replicates other research which also fails to find a relationship between R/S constructs and negative affect (e.g., Piedmont et al., [2007]. The relations among spirituality and religiosity and Axis II functioning in two college samples. Research in the Social Scientific Study of Religion, 18, 53- 73).

-> As stated above we have now adapted our alpha level and the results section. In addition, we have adapted the relevant section in the discussion to better reflect the explorative nature of our results. While the correlation analyses clearly show connections between R/S and attachment security as well as mood-related psychiatric burden, the hierarchical regression analyses suggest that R/S may only
have a small effect on mood-related psychiatric burden independent of attachment security.

The adapted section now reads as follows:

“When the relation between lower RSWB and more insecure attachment (low AX / low AV) supports these similarities (i.e., correspondence pathway), EWB appears to offer some protection against mood-related psychiatric burden independent of attachment security. Importantly, as we only found a small independent effect of EWB on mood-related psychiatric burden, future research will have to further explore whether and under what conditions EWB can influence mood-related psychiatric burden independent of attachment security. In general, this independent effect of EWB tentatively supports the idea that the relationship to a higher power might compensate for an insecure attachment to other people (i.e., compensation pathway).” (p.13-14)

4) What is lacking in this study is any rationale as to why R/S should be related to negative affect. What value is there in examining this relationship? Using only correlationally-based techniques will not tell us anything about the underlying causal nature of any potential relationships. Are depressed people less religious because the negative affect sours all aspects of one’s life, or is it that disturbances in one’s relationship to the transcendent can have an impact on the psychological tenor of our inner lives? This is an important question to be tested, but one that needs to be outlined and developed. As it stands, much of the Discussion section goes way beyond the data that is being presented (e.g., attachment related coping). It is not clear how the current data address these larger conceptual issues, which were not well developed in the Introduction. In fact, much of the discussion centers on issues not tested in this study. Claims are made that are inappropriate. For example, in the Conclusion section, the authors noted, “Higher existential well-being appears to have a compensating effect on the relation between insecure attachment and impaired mental health.” Where was this tested? Unfortunately, no interaction effect for these two variables was tested in the regression models. Thus, the manuscript needs to be significantly revised so that the claims of this study can be tempered to fit with the very modest findings.

-&gt; As stated in the introduction, Religious/Spiritual Well-Being (RSWB) is thought to be negatively related to mood-related psychiatric burden as “the relationship to God or a higher power frequently meets the parameters of attachment and may consequently confer very similar positive psychological influences [27].” (p. 5). The connection between attachment and mood-related psychiatric burden is further explained in the section “Attachment related coping strategies”. We have now also extended our description of the link between R/S, attachment and negative effect in the introduction as well as in the discussion. Furthermore, we already included the reviewer’s concern regarding causal relationships between R/S and mood-related psychiatric burden in the limitation section. Lastly, we have now adapted our conclusions to better reflect our results regarding EWB.

This reads as follows:

“Studies have shown that Religious/Spiritual Well-Being (RSWB) is connected to lower levels of anxiety, depression, neuroticism and suicidal ideation while simultaneously being connected to more adequate coping and more sense of coherence [35].” (p. 6)

“As stated above, the relationship to a higher power can fulfill the criteria of an attachment bond and can consequently be assumed to have similar psychological advantages [27]. This association between attachment and spirituality can be explained by two hypotheses [27]: On the one hand, Bowlby’s [47] correspondence hypothesis states that mental models can generalize across various attachment relationships and therefore may also extend to the relationships with a higher power. On the other hand, Ainsworth’s [48] compensation hypothesis suggests that an attachment to a higher power may be developed as a surrogate for secure human attachment figures. Therefore, correspondence and compensation hypotheses can be seen as two pathways to as well as two ways of being religious [49]. In addition, the compensation pathway implies that secure attachment can be learned even later in life.
and without support from another human individual [49].” (p. 13)

“Lastly, studies with a longitudinal design are needed to explore all possible interactions between attachment, spirituality and mood-related psychiatric burden. For example, as insecurely attached individuals are more prone to undergo major fluctuations [28] in their spiritual beliefs that correspond with distressing life events [29], mood-related psychiatric burden might cause and/or result from lower RSWB.” (p. 15)

“Especially existential well-being appears be relevant for the connection between attachment and mental health.” (p. 16)

5) Another issue concerns the measure of R/S functioning. This scale seems to be the creation of the authors and little is really known about the properties of this scale. Some evidence of the construct validity of this measure ought to be given in the Method section. I do know that other such measures of Existential and Religious Well-Being represent two very different constructs. Combining the two sub scales into a larger measure is really not appropriate. Nonetheless, the reader is not informed on what exactly the Religious scale captures and if it can really be considered a measure of R/S characteristics. Also, some discussion about how the authors understand R/S as a psychological construct would be helpful. Given that there is no consensual definition of these constructs in the field, how do they understand these constructs? It appears that they see them to be universal qualities that transcend religious beliefs and theologies. However, not everyone would agree with this approach. Nonetheless, the authors should develop their perspectives on the psychological nature of their R/S constructs and how they believe these variables operate in the larger mental life of individuals.

-> We have now extended the information on the MI-RSWB in the Introduction, the Discussion and the Methods section. This now reads as follows:

“In this study we follow on the conceptualization of spirituality as the ‘ability to experience and integrate meaning and purpose in existence through a connectedness with self, others or a power greater than oneself’ (p. 117) [32]. Therein, the quality of the attachment to a higher power can be linked to the level of spiritual well-being [33]. In line with earlier work [34], this conceptualization integrates an immanent (i.e., bio-psycho-social) and a transcendent (i.e., spiritual) field of perception [35]. The measure used to assess this Religious/Spiritual Well-Being (RSWB) includes ‘Hope Immanent’, ‘Forgiveness’ and ‘Experiences of Sense and Meaning’ as components of an immanent Existential Well-Being (EWB), while ‘Hope Transcendent’, ‘General Religiosity’ and ‘Connectedness’ are components of a transcendent Religious Well-Being (RWB) [35]. Therein, General Religiosity can be understood as a person’s faith in connection to institutions or specific religious communities and traditions, Connectedness refers to a more deinstitutionalized form of religious belief [35]. In total, the scale therefore follows the idea of a bio-psycho-socio-spiritual model of health and disease [35]. While RWB is mainly linked to a relationship with God, EWB does not refer to a specific higher power but is linked to life satisfaction as well as a confidence in the meaningfulness of life [36].” (p. 6)

“To date, the MI-RSWB has already been extensively applied in studies of different clinical as well as healthy populations [35].” (p. 8)

“Conversely, RWB – including general religiosity, connectedness and transcendent hope – may be more closely related to social contexts and therefore to the correspondence pathway [50]. This consequently may explain why RWB did not contribute to the prediction of the mood-related psychiatric burden independent of attachment parameters.” (p. 14)

6) The role of R/S constructs is not really well presented. While the data are clear that they have no unique relationship to negative affect, the authors discuss their findings as if they do. So, what do we take away from this aspect of the study concerning R/S constructs? Also, the manuscript refers to “mood pathology” but no evidence of this is ever given. Again, a very imprecise usage is being applied
here. While the BSI-18 does capture negative affect, it is not clear what percentage of the samples examined (both Austrian and Indian) actually have a mood disorder. Rather, it is more accurate to state how the predictors relate to aspects of negative emotionality. This sample seems to be a nonclinical one, so to use the term “mood pathology” is inaccurate and inappropriate.

As described above we have now considerably extended our description of the R/S constructs in several sections of the paper. Furthermore, we now no longer refer to “mood pathology” but have substituted this term for “mood-related psychiatric burden” throughout the manuscript in accordance with the suggestion made by Reviewer 2.

Overall, I think there are some serious methodological weaknesses in this report as well as significant statistical conclusion invalidity (i.e., the authors misinterpret their findings).