Author's response to reviews

Title: Title: Social inhibition as a mediator of neuroticism and depression in the elderly

Authors:

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Author's response to reviews: see over
Re: First manuscript revision, and submission of manuscript - version 2

Dear Professor Melissa Norton, MD

Below are our responses, point-by-point, to the reviewer’s comments on the first draft of our manuscript (dated 4th November 2010) entitled, ‘Social inhibition as a mediator of neuroticism and depression in the elderly’. We have also sent you a revised manuscript (version 2, dated 28th July 2011) for your consideration.

Editorial requests
E1. Please clarify whether the survey data employed for the analysis are publicly available, and if this is not the case, whether appropriate permission and ethical approval were sought for their use.

E1: We thank the editor for raising this important issue. In response to your comments, we have added the following text (with reference to the main results being submitted to the Faculty of Medicine at Chiang Mai University, as it is our funding agency) in the Methods section: ‘This work represents a secondary analysis of the data taken from the previous study: ‘The reliability and validity of the Thai-version of the Inventory of Interpersonal Problems (T-IIP)’ carried out in 2009[19], and was approved by an independent ethics committee at the Faculty of Medicine, Chiang Mai University.’. Please see the comment box ‘ForE1’.

E2. Please include further details on the survey under the Methods section of the revised manuscript, could you clarify whether the main results of this survey have been published, and if so, include the relevant reference. Please also clarify how many participants the original survey involved and how the current subset of participants was chosen.

E2: We have added a reference to the main study as suggested and have re-written the ‘Participants’ part. Please see the comment box ‘For E2’.
Reviewer1: Jennifer Morse

Major concerns:

A1 The last line of the second paragraph and the third paragraph warrant further exposition. That is, the data collected is cross-sectional but the authors are arguing that the concepts of symptoms, interpersonal problems, and personality traits have different durations that imply a sense of timeline supporting their use of a mediation hypothesis and analysis. This makes some sense but should be more thoroughly explained in the introduction. In addition the section of the introduction would be improved if there were additional theoretical arguments or models that support mediation effect that they hypothesize. If there are no published models, the authors might expand the description of previous empirical results in order to argue for their own conceptual model suggesting that social inhibition would mediate the relation between neuroticism/emotional stability and depression.

A1: We thank the reviewer for the suggestions. We have revised the ‘Background’ section as suggested and hope this adds a greater level of understanding.

A2 At the beginning of the statistical analysis the authors say that they will examine the relation between depression and three potential confounders and the links between emotional stability and depressive symptoms – age, education, income, and intelligence. First a minor point, the list includes four variables.

A2: A2-1. We thank the reviewer for pointing out this mistake. We have corrected the error as suggested. Please see comment box ‘For A2-1’.

Next, a more important point, it is not clear why the authors focused on reasoning out of all the primary factors in the 16 PF. Their introduction has not touched on reasoning and so controlling for it seems to come out of the blue. In the results section the authors state that reasoning is not regarded as personality factor but rather a function of intellectual ability and thus was deemed to control variable. This order of presentation is confusing and more justification is needed for designating reasoning, an established scale on the 16 PF, not to be a personality factor. Throughout the remainder of the manuscript, the authors refer to “intelligence” which seems to warrant further justification and empirical support for the use of the reasoning factor as a marker of cognitive ability.
A2-2 Thank you to the reviewer for raising this issue, but we do not consider ‘reasoning’ as a personality factor. In addition, we found that it correlated with depression, so we kept it in the equation. It could be one of the confounding factors (similar to age, education and income) that needs to be controlled. There is evidence to be found that suggests excluding this factor (see Rossier. J. et al. (2004)), since the author in that case did not consider reasoning as a personality factor either. We have added the text ‘Reasoning is not regarded as a personality factor but rather as a function of reasoning/intellectual ability; therefore, it was treated as a variable to be controlled.[27,28]’ with the citation referring to the Statistics analysis part. Please see the comment box ‘For A2-2’.

A3 The discussion currently focuses on re-iterating the findings. The authors could significantly strengthen the discussion if they expanded the connections between the previous research and the current findings to expand theoretical models of depression (e.g., expand Alden & Bieling’s work), to articulate their own model, and to reflect on the conceptual issue related to the longevity of symptoms, interpersonal problems, and personality traits.

A3-1: We thank the reviewer for the suggestion and have added the following text to the Discussion part: ‘What can be discerned with regard to this mediating effect? One possible explanation may be provided by the findings of Alden & Bieling [12] in a previous study, which took into account Blatt’s theory of self-criticism [37] and Beck’s theory of autonomy regarding the causes of depression [38]. Clara et al.[39] confirmed that self-criticism is strongly associated with major levels of depression, plus that a number of other personality dimensions may be associated with major depression because of their shared variance with a person’s psychiatric history and current level of emotional distress. Dunkley et al. [40] found that self-criticism can be conceptualized as being hierarchically nested in the higher-order domain of neuroticism, or more specifically that self-criticism accounts for a statistically significant additional amount of variance in the measures of depression beyond that accounted for by neuroticism, when studying a clinical sample.

What is the correlation between social inhibition and self-criticism then? Alden & Bieling [9] found that both self-criticism (as well as autonomy) correlate with and fall into the octant ‘socially inhibited’ within the interpersonal circumplex, which correlates specifically with our results here. Considering this all together, social inhibition can thus be viewed as an interpersonal function of self-criticism, and as a robust predictor of depression that impacts upon the effect emotional stability has on this condition through the self-criticism component. Social inhibition partially represents self-criticism, as argued by Alden & Bieling [12], and theoretically it should be located in the cold-dominant octant, but it is not - it is a less stable characteristic owing to the fact that social inhibition shares co-variances with other variables such as autonomy, plus other factors not included in this study.’ Please see the comment box ‘For A3-1’.

In addition, their stated goal was to use the results to improve prevention or treatments for depression, but they have not commented on any clinical implications of their findings.
A3-2: Regarding implications, we have added the following text to the Discussion section: ‘Even though depressive symptoms tend to occur in those suffering from emotional insecurity, the problem of social inhibition may be another aspect to look for and to study, because, as our results show, the lower the level of social inhibition the less likely individuals are to be affected by neuroticism, or to develop depressive symptoms. Given the fact that depression is related to self-criticism, hostility and submissive behaviours, all of which share the emotional stability and socially inhibited (hostile-submissive) interpersonal problem traits, to move patients from the hostile to the friendly pole, even though it is not as easy as appears, has been proven to be a better outcome despite the fact that personality problems still exist[46-48]’. Please see the comment box ‘For A3-2’.

Minor essential concerns:

A-MEA1. The authors’ use of etc. is unhelpful. In the introduction to writing could be more clear if instead of ending lists with etc., the authors provided either a complete list or an accepted definition. Or in a case where they do not want to list all the possibilities (like listing all the measures that assessed neuroticism), the authors could use that as an opportunity to focus on the measure that they're most interested in.

A-MEA1: Thank you for the suggestion. We have revised this point as advised.

A-MEA2. In the second line of the second paragraph, I believe the authors are first referring to the dependent type of depression but that is not clear. In that same paragraph the phrase “subset of the introversion trait” seems awkward. I believe it is more common to refer to facets of personality traits.

A-MEA2: We thank the reviewer for the suggestion. We have replaced the text “might be viewed as a subset of the introvert” with this text “might be viewed as a facet of the introversion trait”. Please see the comment box ‘For A-MEA2’.

A-MEA3. The clause beginning with i.e. in the third paragraph is confusing. I believe the authors are interested in one mediator not in the possibility of various different mediators but this should be clarified.

A-MEA3: We have replaced that paragraph with this one; ‘The present study aimed to explore to what extent the presence of a socially inhibited interpersonal problem can mediate the effect of emotional stability on depression in the elderly. It is hoped that a greater understanding of this relationship will ultimately help lead to improved preventive and therapeutic interventions for depression patients.’ Please see the comment box ‘For A-MEA3’.

A-MEA4. In the results section, first paragraph, the text seems to imply that reasoning was
also correlated with depression because of the use of “also” in the sentence about the relation between intrusiveness and depression. The second sentence in the paragraph is quite long and confusing; breaking it into shorter sentences each of which addresses a correlational finding would be helpful to the reader.

A-MEA4: We thank the reviewer for raising this point. We re-checked our data and re-analyzed the correlation and the regression model, finding that the ‘Intrusive/Needy’ interpersonal style was put into the equation by mistake. We have removed the content re: Intrusive/Needy as a result. However, we have attached the re-analysis for your consideration. Please see this table.

For both reviewers. Intercorrelation matrix

<table>
<thead>
<tr>
<th></th>
<th>1</th>
<th>2</th>
<th>3</th>
<th>4</th>
<th>5</th>
<th>6</th>
<th>7</th>
<th>8</th>
</tr>
</thead>
<tbody>
<tr>
<td>Age</td>
<td>1</td>
<td>-0.358</td>
<td>-0.305</td>
<td>-0.09</td>
<td>0.114</td>
<td>0.004</td>
<td>0.326</td>
<td>0.163</td>
</tr>
<tr>
<td>Education</td>
<td>-0.358</td>
<td>1</td>
<td>0.75</td>
<td>0.217</td>
<td>-0.194</td>
<td>-0.073</td>
<td>-0.286</td>
<td>-0.155</td>
</tr>
<tr>
<td>Income</td>
<td>-0.305</td>
<td>0.75</td>
<td>1</td>
<td>0.277</td>
<td>-0.105</td>
<td>-0.098</td>
<td>-0.265</td>
<td>-0.146</td>
</tr>
<tr>
<td>Reasoning</td>
<td>-0.09</td>
<td>0.217</td>
<td>0.277</td>
<td>1</td>
<td>-0.097</td>
<td>0.036</td>
<td>-0.29</td>
<td>-0.113</td>
</tr>
<tr>
<td>Socially inhibited</td>
<td>0.114</td>
<td>-0.194</td>
<td>-0.105</td>
<td>-0.097</td>
<td>1</td>
<td>-0.178</td>
<td>0.304</td>
<td>-0.095</td>
</tr>
<tr>
<td>Emotional stability</td>
<td>0.004</td>
<td>-0.073</td>
<td>-0.098</td>
<td>0.036</td>
<td>-0.178</td>
<td>1</td>
<td>-0.281</td>
<td>0.765**</td>
</tr>
<tr>
<td>Depressive symptoms</td>
<td>0.326</td>
<td>-0.358</td>
<td>-0.305</td>
<td>-0.09</td>
<td>0.114</td>
<td>0.004</td>
<td>1</td>
<td>0.334**</td>
</tr>
<tr>
<td>Intrusive needy</td>
<td>0.133</td>
<td>-0.017</td>
<td>-0.154</td>
<td>0.039</td>
<td>-0.163</td>
<td>0.054</td>
<td>0.113</td>
<td>1</td>
</tr>
</tbody>
</table>

A-MEA5. In Table 1, the diagonal 1s make the table harder to read and are unnecessary. Also, there is an extra 1 and I wonder if some of the other correlations are also significant.

A-MEA5: We have split the (previous) Table 1 into two tables: Table 1 and Table 2. We have also made a revision to the manuscript regarding the (previous) Table 2, which is now Table 3.

A-MEA6. The abstract needs some revision. The background should state which factors were investigated. The Methods use the Inventory of Interpersonal Problems (not Interpersonal Problem Inventory). The Results can simply state the percentage who were married and had low income – currently the report sounds conditional.
(“of those who”). The mediation finding should be clarified.

A-MEA6: We have rewritten the new abstract as suggested. Please see the Abstract.

Discretionary revisions:
A-DR1. In instrument section, the description of the Inventory of Interpersonal Problems use a slightly different labels than I'm used to seeing in reference to the octants of the IIP. Perhaps this is partially a translation issue but either way a note indicating the more traditional labels (domineering, vindictive, cold, socially avoidant, nonassertive, exploitable, overly nurturant, intrusive) would likely be helpful.

A-DR1: We thank the reviewer for the advice. We have utilized the Horowitz L.M. et al. (2000) version of IIP-64. The terms used in the study were thus those used in the manual (Domineering/Controlling, Vindictive/Self-centered, Cold/Distant, Socially Inhibited, Non-assertive, Overly Accommodating, Self-sacrificing and Intrusive/Needy).

Minor issues not for publication:

A-MI1. The description of the SCL 90 would be more complete if the authors could include the Cronbach's alpha for the Thai version. Also I believe there is a typo at the end of this description and that the authors are referring to known group technique.

A-MI1: We have added the Cronbach’s alpha and corrected the typo as suggested. Please see the comment boxes ‘For A-MI1’

A-MI2. I believe that the Cattell measure is actually called the 16 Personality Factor Questionnaire, abbreviated 16 PF. And a small point, the statistics for test-retest reliability and internal consistency should probably have their statistical symbols.

A-MI2: We have already corrected these mistakes. Please see the Instruments section ‘For A-MI2’.

A-MI3. At the end of the second paragraph regarding statistical analyses, I believe the word whether is missing – “whether it reduces” and that “indexing” is not required.

A-MI3: We have made the changes as suggested. Thank you. Please see the comment box ‘For A-MI3’.

A-MI4. In the results section, first paragraph, a) is missing.

A-MI4: Thank you. We have added a) as suggested.
Reviewer: Julie Wetherell

B1. Please provide more information about the sample. Specifically, was this "national survey" demographically representative of Thailand? How were prospective respondents identified? How many were approached, and how? How many refused?

B1: We thank both the reviewer and the editor for raising this issue. The survey used a stratified sampling technique with four age groups to recruit participants from the five geographic regions (north, northeast-upper and lower, central and south), and in order to represent the Thai norm. The sample was calculated from the size of the residential community. Proportional sampling was performed in each province, in accordance with the population according to the Thailand 2009 census. A total sample size of 452 people was used for the study, with 126 participants age 60 and older. The sample was purposively approached according to random selection criteria. Please see the comment boxes ‘For E1, and For E2’.

B2. I am skeptical about the ability of a self-report personality scale to adequately measure intelligence. Please either provide citations to support a strong relationship between the 16PF and standard intelligence or neuropsychological tests, or else rephrase to describe the measure more accurately as "self-reported intellectual interests" or "openness to experience" or whatever the 16PF questions actually assess.

B2: We have added a citation with regard to recent findings made by Nermin Djapo et al. (2011) on the correlation between intelligence tests and the ‘reasoning’ scale of the 16-PF. Their findings show that reasoning is the strongest variable in terms of a correlation with fluid and crystallized intelligence (p < 0.001). Please see the comment box ‘For A2-2 and B2’

B3. Although the inclusion of social inhibition does reduce the strength of the association between low emotional stability and depressive symptoms, the magnitude of the change (-0.29 to -0.25 in the abstract and figure, -0.29 to -0.26 in the text of the Results and in the table - therefore the abstract and figure need to be changed) seems small. Is this difference statistically significant? Do Baron and Kenny or other methodologists provide guidance on how much change in regression coefficients is required in order to conclude that mediation is present?

B3: Thank you to the reviewer for pointing out this mistake. The correct statement should be: -0.29 to -0.26, and have made the appropriate revision to the abstract. Regarding the regression coefficients, please see the comment box ‘For B3’

Is this difference statistically significant?
- As suggested by Kenny, in this case there was a partial mediation effect from Social inhibition which was significant: B = .022 (95% CI .005, .039), p = 0.013. In terms of the effect size, this
could be regarded as medium in significance \( (d = 0.37) \) (Cohen J. Statistical Power Analysis for the Behavioral Sciences. Hillsdale N.J.: Lawrence Erlbaum Associates; 1988.)

We have added the following text to the Results section: ‘Nevertheless, the effect sizes for the main link in this mediational model were moderate \( (d = 0.37) \)’ Please see the comment box ‘For B3-2’.

Minor Essential Revisions

B-ME1. The direction of the relationships should be identified in the writing. For example (from the abstract): "Emotional stability yielded the highest standardized regression coefficient" with depressive symptoms is strictly speaking accurate but leaves the impression that high levels of emotional stability are associated with high levels of depression. Better to rephrase as "low levels of emotional stability were most strongly associated with depressive symptoms" or similar phrasing.

B-ME1-1. Thank you for your comments. We have replaced the phrase as suggested. Please see the comment box ‘For B-ME1’.

Similarly, it should be made clear that low levels of intellectual curiosity or whatever the authors decide to rename what they are currently calling intellectual ability/reasoning are associated with depressive symptoms.

B-ME1-2: We have added the following text to the Results section: ‘Age and reasoning, but not income, have an influence on depression; the higher the age, the less reasoning is used and the more depressed the person is likely to be \( (R^2 = 0.243, p < 0.01; R^2 = -0.212, p < 0.01, \text{ respectively}) \)’. Please see the comment box ‘For B-ME1-2’.

B-ME2. In the Background section, "IIP" needs to be spelled out the first time it appears.
B-ME2: Thank you. We have used the full name as suggested. Please see the comment box ‘For B-ME2’

B-ME3. The authors indicate that the SCL-90 was developed for people aged 15-67 years of age, yet they are using it in a sample ranging in age from 60-93. Please add citations to validation studies in older, ideally Thai, adults.

B-ME3: There is only one Thai version of SCL-90 available, but we reported a good internal consistency for the population used in our study. Please see the comment box ‘For A-MI1’. Since there have been two studies on this subject (Hassel R. et al.: Z Gerontol Geriatr (2001) and Pallesen S. et al.: Scand J Psychol (2002)) that have utilized SCL-90-R on the elderly, we applied the Thai version on elderly Thai people.

B-ME4. Because the sample is not clinically depressed, references to SCL-90 scores
should be to "depressive symptoms" rather than to depression.

B-ME4: We have replaced the term depression with ‘depressive symptoms’ in suitable places throughout the manuscript.

5. Please number the variables in the left column of Table 1 in order to make it easier to read.

B-ME5: We have split the (previous) Table 1 into two tables: Table 1 and Table 2. We have also made a revision in the manuscript text regarding the (previous) Table 2, which is now Table 3.

6. Please include page numbers in any subsequent revisions to facilitate review.
B-ME6. Thank you for your suggestion. We have added page numbers in this revision.

Discretionary Revisions

B-DR1. I was curious to read that social inhibition is not considered very stable. It seems as though it would be related to the construct of behavioral inhibition, which is highly stable - it can be measured in infants and predicts anxiety disorders later in life. Consider clarifying this.

B-DR1: Thank you to the reviewer for raising this point; however, socially inhibited interpersonal problem is not considered a personality trait.

B-DR2. In the Results, the authors state that there is a "lack of a clear theoretical rationale indicatig that the interpersonal style of intrusive needy confers risk [of] depression". Really?

B-DR2: Thank you for the comment. The text was a mistake, and we have omitted it. Please see the additional document submitted to the reviewers.

DR3. It seems to me that one of the factors that would distinguish this article from others is that it is a sample of older Thais, and yet cultural issues are not mentioned in the text. Do we know that geriatric depression is a similar phenomenon in Thailand as elsewhere (for example, similar prevalence rates, similar symptoms as in other countries)?
B-DR3: We do not think cultural factors affect the symptoms as they were measured by SCL-90.

We hope that the above revisions satisfy the reviewers and the editorial team - all the authors read the final manuscript prior to this submission. We thank you for your patience and your positive attitude in considering our paper. Please feel free to get back to us if needed - we look forward to your responses.
Regards,

Nahathai Wongpakaran, MD, FRCPsychT
Tinakon Wongpakara, MD, FRCPsychT
Robert van Reekum, MD, FRCPC