08-Jan-2015  
  
Re: "Narcissism and Self-Enhancement: A Review and Meta-Analysis" (MS # PSPB-14-437)  
  
Dear Dr. Grijalva:  
  
I have now received two reviews of your manuscript “Narcissism and Self-Enhancement: A Review and Meta-Analysis” (MS# PSPB-14-437).  Both reviewers are experts in topics relevant to your work, and I am very grateful to have received their feedback on your manuscript.  I also read your manuscript before I read the reviews so that my reading of it would remain unbiased by the reviewers’ comments.  
  
As you will see, both of the reviewers had very favorable reactions to your work.  They both agreed that your topic is important, and that your findings have the potential to make a strong contribution to the literature on narcissism and self-enhancement.  Both reviewers, however, also noted some issues and concerns that led them to conclude that publication would be premature at this point.  Therefore I am offering you a decision of Revise and Resubmit.  
  
The reviewers’ comments are very thorough and I will not reiterate all of them here.  My reading of the reviews suggests that a more favorable decision will require not only some rewriting and reframing, but also some additional analyses and (possibly) inclusion of additional studies in your meta-analysis.  In addition to the issues raised by the two reviewers, I had several thoughts when reading your manuscript and I list these below:  
  
1)      Since you exclude clinical samples from your search, you might frame the introduction to more clearly communicate your interest in non-clinical narcissism as a personality tendency.  For example, highlighting the clinical definition of narcissism from the DSM in the first sentence of your manuscript might not be ideal, if your focus is not on clinical definitions of narcissism.  In line with this, Reviewer 1 suggests getting rid of all references to clinical narcissism, because most readers of PSPB will expect you to focus on non-clinical narcissism.  This seems reasonable to me – if you open the manuscript with the definition of non-clinical narcissism that you used in your literature search, it would not be necessary to bring up clinical narcissism.  
2)      I wonder if the title and initial framing of your study are as precise as possible?  You aren’t interested in “self-enhancement” broadly defined, but instead, in a very specific type of self-enhancement, i.e., self-other discrepancy (what you call “self-insight” self-enhancement).  I think a title that more clearly conveys the type of self-enhancement you’re interested in here would be ideal, because it will give readers a clearer sense of the contents of your manuscript.  
3)      I was a bit surprised that your literature search only yielded 26 usable data sets.  This concern is echoed by Reviewer 2, who asks whether you did a forward search of your articles, and suggests that you expand your search to include the clinical literature.  Even though you are not interested in clinical narcissism, some clinical scholars operationalize narcissism as a personality trait rather than a clinical diagnosis, and their works could be relevant here.  It also occurred to me that you might broaden your database searches beyond keyword searches to searches of “all text,” and look for any articles that include the NPI (or other measures of narcissism that you included).  I understand that meta-analysis can be frustrating because the continual publication of new articles makes your “final” sample somewhat of a moving target.  Nonetheless, it would strengthen your case if you make another attempt to locate all usable data sets.  
4)      On page 8 you write “Regardless of whether or not narcissists are better than average, the potential threat is eliminated…”  What is the “potential threat” you refer to here?  
5)      On page 14, you write that you were primarily interested in social comparison measures, but I think you mean self-insight measures?  
6)      On page 23, you write that narcissists self-enhanced on each of the agentic constructs, without exception, after having just reported that they did not seem to self-enhance on task performance on the RAT.  This could use a bit of rephrasing.  
  
As I mentioned above, both of the reviewers also listed several issues that merit your consideration.  Reviewer 1, for example, suggests a slight theoretical reframing, and recommends several alternative citations that you might include.  (Indeed, some of these suggested citations might contain relevant data sets for inclusion in your meta-analysis.)  Reviewer 2 makes several suggestions regarding additional analyses you might do, and raises some issues concerning the rationale for some of your analyses as well as your interpretations of some of your findings.  I read over all of the reviews carefully, and I believe that every one of them deserves your full consideration in a revision.  Please pay careful attention to all of these comments as you revise your manuscript.  If you do revise and resubmit your manuscript, please include a detailed cover letter that explains exactly how you responded to every one of the reviewers’ comments (or clearly explains why you chose not to address a particular point).  
  
I would like to receive the revision within 90 days from receipt of this letter (or 180 days with data collection).  If it is not possible for you to submit your revision by the due date, you will need to request an extension.  The revision should be submitted via SageTrack, once the revised manuscript is prepared.  Log into [http://mc.manuscriptcentral.com/pspb](https://webmail.illinois.edu/owa/redir.aspx?C=CJtVNCkGEEyAdyb20RW0f8Sx0YtNA9IIEJiO61doE1JZcj5cXaCEzbGTYaA857O5G5dahF52RRU.&URL=http%3a%2f%2fmc.manuscriptcentral.com%2fpspb) and enter your Author Center, where you will find your manuscript title listed under "Manuscripts with Decisions."  Under "Actions," you can click on "Create a Revision."  Your manuscript number has been appended to denote a revision.  Your original files are available to you when you upload your final manuscript.  Please delete any redundant files before completing the submission.  Please include in your cover letter a description of all of the revisions you have made.  If you have any questions during this process, please contact the editorial office at [Kristen.Marchetti@sagepub.com](https://webmail.illinois.edu/owa/redir.aspx?C=CJtVNCkGEEyAdyb20RW0f8Sx0YtNA9IIEJiO61doE1JZcj5cXaCEzbGTYaA857O5G5dahF52RRU.&URL=mailto%3aKristen.Marchetti%40sagepub.com).  
  
Of course, I cannot guarantee a favorable decision on a revised manuscript.  Given the overall enthusiasm for this topic, however, I am hopeful that a revision will be met with a more favorable response.  In the meantime, please let me know if you have any questions about the contents of this letter, my decision, or the reviews themselves.  I look forward to seeing a revision of your manuscript.  Thank you for considering PSPB as an outlet for your work.  
  
Sincerely,  
  
Dr. Jennifer Bosson  
Associate Editor  
Personality and Social Psychology Bulletin  
  
  
Reviewer(s)' Comments to Author:  
  
Reviewer: 1  
  
Comments to the Author  
This submission has a lot of merit. It addresses an important and timely issue, offers some interesting theorizing, and reports a competently conducted meta-analysis. I had comments that will hopefully be of some relevance to the authors (and the action editor).  
  
GENERAL ISSUES  
  
In the opening statement and in several spots all over the manuscript (and a couple of footnotes), the authors refer to pathological narcissism. I suggest removing all references to it. The readership of PSPB is interested in normal or everyday narcissism, not in pathological one. By now (well, since the 90s), the distinction between narcissism varying on a continuum (i.e., narcissism as a personality trait) and pathological narcissism has been well-established, and it is acceptable to just mention narcissism as a trait and taking form there. Also, removal of pathological narcissism will streamline the manuscript. (By the way, it should be “clinical”, not pathological, narcissism.)  
  
On p. 13, top, I wondered: If the self-criterion residual method is superior (to difference scores), why wouldn’t the author focus on it and delegate reporting of difference scores to a footnote? Then, reading on and reaching pp. 21-22, I realize that the authors have a theoretical reason for comparing effect sizes for the two methods. This theoretical rationale ought to be carried forward.  
  
p. 23, middle. The authors report “the narcissism-self-enhancement relationship for other agentic constructs …” Should p < (rather than p >) for all traits (intelligence, leadership, extraversion, attractiveness, and openness)?  
  
  
MINOR ISSUES  
  
p. 3. In the second paragraph, the authors present the term ego-involving contexts without defining it. In the next sentence, they make a definitional attempt, but it’s roundabout. I suggest defining this term in a more direct manner.  
  
p. 4, first half: “b” is unclear. It only becomes clear with the last sentence of p. 11.  
  
p. 5, first few lines. The choice of citations is somewhat unusual.  
\*I am not sure the three cites (Campbell, Reeder, Sedikides, & Elliot, 2000; Gramzow & Willard, 2006; John & Robins, 1994) are the most relevant to the point of a “general tendency for people to have inflated vies of themselves.” Shouldn’t the authors refer to reviews/books rather than primary sources (which are a bit old)? Alternative citations are: Alicke & Sedikides, 2009 (European Review of Social Psychology), Alicke & Sedikides (2011; Handbook of Self-Enhancement and Self-Protection), Sedikides & Gregg, 2008 (Perspectives on Psychological Science).  
\*For the point that people “endorse self-serving attributions”, I’d recommend replacing the two cites (of which the Blaine & Crocker is particularly peculiar) with:  
Mezulis, Abramson, Hyde, & Hankin, 2004 (Psychological Bulletin) and Campbell & Sedikides (2009, Review of General Psychology).  
\*for the BTAE, I suggest replacing the Krueger & Mueller cite with: Alicke, M. D., & Govorun, O. (2005). The better-than-average effect. In M. D. Alicke, D. A. Dunning, & J. I. Krueger (Eds.), The self in social judgment (pp. 85–106). Philadelphia, PA: Psychology Press.  
  
p. 5, last par. The author will do well to replace most, if no all, of the primary sources a couple of recent reviews. See the Rhodewalt chapter in Handbook of Self and Identity (2013) and the Morf et al. chapter in Handbook of Self-Enhancement and Self-Protection (2011).  
  
p. 11, middle. When the authors claim that “narcissists make positive first impressions that deteriorate as people get to know them better”, they could also consider citing Back, Schmukle, and Egloff (2010, JPSP).  
  
p. 24, first part. Interesting that it is important for narcissists to be likable. Reminds one of the quintessential narcissism, Bill Clinton, and his drive (for lack of better word) to be likable.  
  
  
  
  
Reviewer: 2  
  
Comments to the Author  
This is an excellent paper meta-analyzing the association between narcissism and self-enhancement, and testing several moderators.  I am not an expert on meta-analysis so cannot evaluate the statistics, but I have several suggestions for the authors to consider.  In my view, a slightly improved version of this paper should be published in PSPB.  
  
1.  I wonder if the authors could find some more samples to include.  Did the authors do a forward search of all articles they found (i.e., looked for more recent papers that cited those papers)?  There are some references that the authors are missing that make me wonder if their literature search may have missed some results.  In addition, I think it would be worth looking through the clinical psychology literature (excluding clinical samples, but some of that literature is based on non-clinical samples) and including other measures of narcissism that are more often used by clinicians (though be careful to avoid measures that also tap into 'vulnerable narcissism' which, in my view, is not narcissism and will muddy the picture).  I can't think of the names of the scales that clinical psychologists use off the top of my head, but a quick search should turn them up.  
  
2.  I think the authors need to address the question of publication bias.  If possible, I would like to see a p-curve analysis, and some estimate of the effect size corrected for publication bias.  My guess is there will not be much publication bias for the communal constructs, but there could be some for the agentic constructs, because those are likely the focal constructs in most published studies.  
  
3.  I am not an expert on meta-analysis, but I thought that, before examining potential moderators, it is typical to estimate the amount of heterogeneity in effect sizes.  I think it would be useful to present such an analysis.  Then, it would also be useful to know how much of that variance is accounted for by each moderator, and how much heterogeneity there is within the subgroups for each moderator (e.g., within the agentic effect sizes and the communal effect sizes, etc.) for the categorical moderators.  
  
4.  For research question 1, you need a better criterion to determine whether the effect sizes for studies using residuals are more valid than effect sizes for studies using difference scores.  Just because the residual studies have larger effects doesn't mean they are more valid.  A better test might be whether there is less heterogeneity in the studies using residuals than difference scores.  If you could show that there was less heterogeneity in results based on residuals (controlling for other moderators that might be confounds), that would be more convincing. Another approach would be to just describe the difference between the two kinds of studies without arguing that the larger effects for residuals is evidence that they are more valid. (Also, difference scores have some of the same undesirable properties as difference scores, but fewer of them, it seems. Still, they are not perfect and I think it would be important to point this out when discussing them on pp. 27-28).  
5. For the last analysis reported on page 19, it seems to me that it would be better to leave the constructs in the 'neither' category out of the analysis altogether.  With them in there (coded as '0'), it is possible that they are partly responsible for the effect.  i also think the analysis just before that one is unnecessary (showing that there is no difference between the 'neither' constructs and all other constructs) - if it is important, the rationale for this analysis needs to be made more explicit.  
6.  Was there enough detail provided in the studies to use a more fine-grained measure of acquaintance in the moderator analyses?  If a continuous variable is not possible, perhaps the studies/effect sizes can be grouped into more than two categories?  I do think the current analysis (very low acquaintance vs. everyone else) is very interesting and if it's the best information that's available, then the authors should still retain this analysis, but if it would be possible to also compare medium vs. long acquaintance, for example, that would be useful too.  
  
7.  It would be useful if the authors would give confidence intervals and exact p-values for all results, including non-significant results.  
  
8.  It is very interesting that the type of criterion (observer-report vs. objective measure) did not moderate the magnitude of the effect.  I think the authors should discuss this more.  
  
9.  In the competing moderators model, why include moderators that were not significant on their own?  
  
10. Excluding the remote associates task from the task performance analyses seems very post-hoc.  Please explain the rationale for considering it a different type of task, and please also explicitly say that there is greater chance of false positive since the decision to split the task performance results was based on examining the raw data (effect sizes) after you found a non-significant effect.  (For example, the overall effect size for conscientiousness was very similar to the overall effect size for task performance, but you didn't go and look at the individual studies to try to figure out if there are a subset of studies that did find large effects for conscientiousness, because this result was consistent with your hypothesis.  Related to this, I would encourage you not to rely too heavily on statistical significance, and consider than an effect size of .13 or .14 might actually be meaningful.)  
  
11.  When listing all agentic constructs (p. 23), you report all p-values as greater than .05, but then state in the next sentence that all effects were significant.  reporting exact p-values and confidence intervals will help eliminate this kind of error.  
  
12. In the discussion, I don't think you can conclude that narcissists' self-reports of communal constructs are likely to be accurate.  Just because narcissism is not correlated with self-enhancement for agreeableness and conscientiousness doesn't mean that narcissists are accurate.  For example, it could be that all self-reports of agreeableness are inaccurate.  In other words, there are other ways to be inaccurate besides self-enhancing.  A more correct statement would be that narcissists are not more likey to be inaccurate on these constructs than non-narcissists. (See also p. 26 where you again refer to narcissists' self-reports on communal traits as 'accurate'.)  
  
Smaller points:  
  
In the introduction, Kurt & Paulhus (2008) is very relevant.  It might also meet the inclusion criteria for your meta-analysis.  
In the introduction (especially pages 9-10) and the discussion, Carlson (2013; Self and Identity) is very relevant and directly addresses the question of whether narcissists value communal traits.  
I think there is a lot more literature on the concept of narcissism as 'unmitigated agency' than the authors discuss.  
p. 11 - Naumann et al. (2008) is relevant to accuracy of thin slice judgments of narcissism.  
p. 12 - When describing hypothesis 3, change 'when based on ratings from close others' to 'when the criterion measure is based on ratings from close others  
p. 17 - 'between the study moderator variables' should be 'among'  
p. 17 - 'Many of the relationships in Table 1' should be 'Table 2'  
p. 17 - 'percentage of total variance between effect sizes' should be 'percentage of total variance in effect sizes'  
p. 17 - I'm a little confused about the ICC analyses - isn't MLM necessary if the proportion of variance within samples is high enough (not between samples)?  either way, whether it's 27% or 63%, I think MLM is warranted, but I think justifying the need for MLM by saying that there was 'sufficient between samples variance' is confusing. (Also, be consistent about 'between samples' vs. 'between studies')  
p. 21 - the sentence beginning "The number of effect sizes decreased but..." is confusing.  
p. 22 - 'the competing moderators' model' should be 'the competing moderators model'  
p. 22 - 'the extent to which narcissists' self-enhanced' should be 'the extent to which narcissists self-enhanced'  
p. 28 - 'If round-robin data is available' should be 'If round-robin data are available'  
  
This is a signed review.  
-Simine Vazire