Editor’s letter.

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BUL-2014-0058  
Ordinal Effects of Ostracism: A Meta-Analysis of Cyberball Studies  
*Psychological Bulletin*  
  
Dear Mr. Hartgerink,  
  
+May 28, 2014  
  
  
RE: Psychological Bulletin 2014-58  
  
Thank you for submitting your manuscript, "Ordinal Effects of Ostracism: A Meta-Analysis of Cyberball Studies" for review to Psychological Bulletin. I now have received three reviews of this manuscript from investigators with substantial expertise in this area of research. Indeed, I was fortunate to obtain reviewers who have expertise in quantitative reviews, especially meta analysis, as well as the particular subject matter at the heart of this review?ostracism.   
  
A copy of each review is included with this letter below. I also have independently read the manuscript and formed my own opinion. I believe you will find the feedback from these reviewers to be particularly helpful and informative, and I thank them for dedicating their valuable time and effort to the review process. I am most grateful to the reviewers for their service to the field.   
  
Reviewers noted strengths of the work described in this manuscript. The meta analysis was competently completed. The manuscript was well-written and provides a succinct, clear statement of the literature. I agree with the reviewers and likewise applaud the authors with placing materials on Open Science Framework. In sum, this paper summarizes the state of knowledge in this area, and it is one that, more broadly, may be of interest to a wider audience, including readers of this journal.   
  
Unfortunately, reviewers noted several weaknesses of this review that limit its  
potential contribution, particularly in its current form. Reviewers found serious shortcomings that make it difficult for the paper to have a significant impact, advance knowledge, and substantially move the broader field of psychological science forward. Based on my own reading, I regret that I must be the bearer of bad news in that I do not think the review and manuscript can be revised accordingly to be suitable for the very high standards of Psychological Bulletin. Even with a substantially revised paper, it does not seem to me that the work can be revised to address the reviewers' concerns and achieve a high level of impact. I want to emphasize that this was a difficult decision as there were several nice aspects to the work, and I know that it can be difficult and very time consuming to conduct this review of the literature. I'm sure this is unwelcome news, yet I hope that the outstanding set of reviews will prove helpful and informative to you in your program of research. Additionally, I hope these comments help to revise the paper and work for possible publication in a different, more specialized outlet. Indeed, the Reviewers and I both believe that the review has merit, albeit in a more selective journal. In particular and what was influential in making this decision for Psychological Bulletin, the Reviewers are highlighting that the review is too selective and focused on Cyberball specifically as one manipulation within a broader, potentially more inclusive, literature. The work could be broadened to be of greater interest and applicability to readers than is currently the case. Presently it may be too targeted toward experimental social psychologists. Additionally there are relevant concerns and questions about the core set of constructs being affected by this manipulation?e.g., is it only ostracism, locus of control, exclusion, rejection? Greater grappling of these deeper issues deserves space in any potential revision you may seek to undertake for submission to a specialty journal. Finally, Reviewer 2 has several specific suggestions that I believe could prove helpful and constructive should you decide to undertake a revision for a different outlet.   
  
As you know, the journal receives many high quality submissions each year, but only a small minority are accepted for publication. Regrettably, I am unable to accept this version of the manuscript for publication in Psychological Bulletin, and I cannot encourage a revised version of this paper.  
  
In summary, I am sorry to conclude that this manuscript is not acceptable for publication in Psychological Bulletin. Although the outcome of this review process is not as you would have preferred, I thank you for considering Psychological Bulletin as a potential outlet for your work. I hope these comments, especially the many constructive remarks by the reviewers, are useful and helpful to you. Thank you for the opportunity to consider this manuscript for Psychological Bulletin, and we hope that you will continue to submit future work to Psychological Bulletin as the preferred outlet for publication.  
  
Sincerely,  
  
  
Ben Hankin, Ph.D.  
Associate Editor

Reviewer #1

This is a clearly written paper looking at studies of one manipulation of ostracism (Cyberball) as they relate to predictions made by Williams in his chapter in *Advances in Experimental Social Psychology* (2009). Cyberball had been in use for some years before that time, so the predictions were Williams’ view of the area as it stood at that time. Previous meta-analyses of the ostracism/rejection literature have appeared, but this is the first one dealing with just Cyberball. One could argue that restricting the analysis to just Cyberball is too restricting if the intent is to understand ostracism/rejection in general. On the other hand, if we could establish clear results for this one manipulation, we would be in much better position to examine differences with other manipulations. I think that the attempt is worthwhile. I am also happy that all the material is available through the Open Science Framework.

Unfortunately, the meta-analytic results do not leave us much wiser than we were. Cyberball ostracism has a strong effect and is not appreciably moderated by anything that has been measured. Those facts are well known and occupy only a small part of the current paper. The major hypothesis tested was that the immediate impact of ostracism is resistant to moderation, but that moderation is more likely to be observed in delayed measures. The immediate response is a reflexive pain-like response, but the delayed effect is more reflective and allows coping mechanisms to have their effect. The meta-analysis did not support this hypothesis. The authors propose a revision of the model that recognizes that there can be factors that moderate immediate reflexive responses, but they are most concerned with the length of the period between reflexive and reflective responses; they suggest that research be done that directly tests different time periods. Because of the sample sizes needed, they suggest the research be done by an interuniversity consortium.

Reviewer #2  
This manuscript presents a comprehensive review of ostracism research using the Cyberball paradigm and focuses on the basic self-reported effects (thereby disregarding other effects, e.g., underlying physiological reactions - in the brain, for instance). The sheer number of studies included (and the aggregated number of participants) is truly impressive. The broad search strategy adopted and the general way in which the procedures are described reveals that the authors are skillful meta-analysts. I also applaud the authors' openness about their research, as they placed extensive supplementary materials on a publicly accessible website (Open Science Framework). This is a major move towards greater scientific integrity and greater replicability of earlier findings. The use of meta-analytic techniques (which aggregates findings across multiple studies and thousands of participants) seems particularly useful in research using the Cyberball paradigm, as most of the studies included in the meta-analysis were conducted on small numbers of participants. In short, as an evaluative and integrative review of the extant literature on a specific topic, the manuscript meets the overall criteria for a Psychological Bulletin article.   
However, there are a number of issues that have to be addressed in somewhat greater detail or with somewhat greater consistency. These issues, which fall into three categories: general, specific and stylistic, will now be discussed in greater detail.  
General Issues  
Accessibility for the broader readership (or diverse audience). I am not an experimental social psychologist myself, but I do research on social relationships and loneliness or perceived social isolation (which is related to ostracism, I guess) and I have a fairly good grasp of traditional meta-analytic techniques in non-experimental contexts (i.e., standardized mean differences to estimate gender differences and meta-analytic techniques that use correlations as basic input). With this specific background, l struggled with two issues throughout this manuscript: (a) as a non-expert on experimental methods, I found it difficult to understand what the authors' basic questions entailed (e.g., I did not really understand Table 1) and (b) as an experienced reader of "classical" meta-analytic reviews, I felt a need for more specific information on the many decisions that the authors had to take during their extensive meta-analytic effort.  
Regarding the first issue, I want to point out that I found the rudimentary graphical representation of the two main hypotheses, as provided on the Open Science Framework, very useful. I would also extend Figure 1 to include representations of social exclusion (Panel A) and social inclusion (Panel B), again for the non-initiated reader. I may add that I have read the Gerber and Wheeler (2009) meta-analysis and quickly understood what those authors wanted to accomplish. So, I encourage the authors of the present manuscript to rewrite the Introduction and the Method sections in such a way that all the readers who can handle the level of difficulty of the Gerber and Wheeler review can quickly grasp what the current meta-analysis attempts to do.  
Regarding the second issue, I would encourage the authors to provide more information on key decisions they made throughout their work. I do understand that a word limit applies here, but some or the current formulations are really vague.  
Implications for and interpretations of the literature. I think that the opening phrases of the Discussion section are the strongest part of the manuscript, for a general audience, and that the authors give a good overview of the limitations of the work they conducted (though I will later add other limitations to that list). When reading other sections, I had the impression that the authors got sidetracked by all sorts of subsidiary issues that may be of some relevance to experimental social psychologists, but not to other types of readers.  
I guess what I am trying to say here is that the authors have a good review, but that they should try to sell it better to a non-specialized audience.  
In my discussion of specific issues, I will mainly concentrate on those sections that I found difficult to understand and on the implications of the main findings when placed against the background of a somewhat longer list of limitations of the work performed.  
Specific Issues  
Unique selling position. On p. 5, the authors have to state clearly how their meta-analysis is uniquely different from other published meta-analyses. The title of the manuscript clearly tries to capture this uniqueness using two key terms, that is, Cyberball and ordinal. However, the term "ordinal" has to be explained in greater detail. I was under the impression that "temporal" also entered into the picture somehow (and what I came up with for the title was: "Moderation and Temporal Effects of Ostracism: A Meta-Analysis of Cyberball Studies"). Upon closer inspection, I arrived at three key distinctions here: (a) Cyberball only (Cacioppo et al. and present manuscript) versus other measures included as well (Gerber and Wheeler and Blackhart et al.), (b) self-report measures (most reviews) versus fMRI studies (Cacioppo et al.), and (c) "classical" analyses (all other reviews) versus "ordinal"' analyses (the present study). The authors devote two paragraphs to this issue (i.e., final paragraph of p. 5 and first full paragraph of p. 6), but I would keep the second paragraph only and work the references from the first paragraph into the second one. The first paragraph is somewhat distracting in its attention to the details of each review.  
Explain acronyms. The acronym SPSP has to be explained. My guess is that it means "Society for Personality and Social Psychology", but many readers may not come up with that educated guess.  
Inclusion decision. The authors repeatedly use the expression "of the x hits, y studies were saved for coding". I guess they want to say that "of the x potentially relevant studies, only y studies met the inclusion criteria (i.e., used Cyberball, had a between subjects design and contained some codable information on at least one variable that can be expected to be affected by ostracism") and were subsequently saved and coded". In the current wording, the reason for initial inclusion is not specified and the inclusion criteria are only mentioned when going through the papers that were saved for coding. Please be as precise as possible here.  
Coding the lapse of time. On p. 13, the authors are very vague when they state that "some measures are variable and were arbitrarily estimated in a conservative manner to at least take them into account at some level". Please specify in greater detail what was done in such cases. I was also wondering whether the 'six second' rule, which is used in Amsterdam, is also used at other universities.  
Ad hoc coding in case of author non-response. The following sentence on p. 15 is unclear: "We considered the Cyberball manipulation characteristics to be similar to previous studies in the same paper or in earlier papers referred to in the paper". Please explain what was done exactly.   
Absence of "structural" effects. On pp. 24-26, I would suggest that the authors somehow point out to the non-initiated reader that the results of moderator analyses in meta-analytic reviews can never be extended beyond the range of moderator values examined and that the actual distribution across these values should also be taken into account. Some of the moderator variables show limited variability, which would provide a simple explanation for the overall absence of moderator (or "structural") effects. The range for the percentage of males should be mentioned in the manuscript.  
Limitations. The authors have nicely pointed out some of the main limitations of their study on pp. 28-31 (e.g., the fact that the interaction test was indirect - through the comparison of confidence intervals - and that the time estimates may have led to inaccurate reporting). I would definitely add the two limitations I referred to earlier on in my commentary (i.e., the vagueness in coding the lapse of time and the seemingly ad hoc coding in case of author non-response). I would also emphasize somewhat more strongly that the original Williams need measure, and the many variations thereof, that are used in the Cyberball literature have untested psychometric properties. In the Gerber and Wheeler meta-analysis, the effects of social rejection were fairly large for the original Williams measure and for its variations, but close to zero for well-established measures. (The fact that other author-developed measures, as used in the Hot Sauce paradigm, are equally poor is not a strong argument).   
Interpretation of the findings and recommendations for future research. I struggled to find the "take home" message of this meta-analysis. The fact that the Williams temporal model (i.e., reflexive - reflective) is not supported by the findings is certainly interesting and could lead to a modification of Williams's theory. However, the authors introduced so many caveats and were so cautious that I lost track of their narrative. One important remark that they make on p. 28 is that the temporal features of the model are not clearly defined (and that, as a consequence, we do not know how long it typically takes to recover from rejection in the Cyberball paradigm). Under these circumstances, "testing" the model must remain, by necessity, an exploratory effort.  
The recommendation on p. 27 that an interuniversity consortium be formed to test the presumed interaction on the required sample size of N = 2,186 looked a bit odd to me. I do not think this is feasible and the interaction effect seems so small that it has no practical relevance. I would recommend, for instance , that future research would use well-established need measures with strong psychometric characteristics, such as the Need to Belong Scale (Leary, Kelly, Cottrell, & Schreindorfer, 2013) rather than the Williams measures and would rely on well-developed motivation theories, such as Self-Determination Theory (SDT; Ryan and Deci) with its set of three needs - competence, relatedness, and autonomy - and its associated measures.  
Tables. In Table 2, I wondered how the authors arrived at participant numbers such as 44.67 or 159.33, as the number of participants is typically expressed using integer numbers (such as 44, 45, 159, or 160). Perhaps the authors could explain their approach in a footnote to that table.  
Stylistic Issues  
Style. Generally speaking, the manuscript has been written in good English. However, the authors use "since" where the APA states that "because" should be used and "while" where the manual prescribes "whereas". The "moderator factor" (on p. 23) should read the "moderator variable". There appear to be numerous intrusions of the authors' native language, which I suspect to be either German or Dutch, into their English phrases. One example is their overextensive use of the term "composition". Using the phrase "the composition of the sample" is fine, but "the composition of the manipulation" should read "the nature of the manipulation". The authors also use phrases such as "the amount of participants" or "the amount of studies" where "number" is the appropriate word in those phrases. This seems a bit odd. As German and Dutch belong to the same linguistic family as English, I would expect that the difference between countable and non-countable entities applies across all of these languages. Finally, there are occasional errors such as "in accordance Williams's ? theory" which should read "in accordance with Williams's ?". I therefore suggest that the authors have their manuscript checked by a native speaker of English.  
References. The overall quality of the references is adequate, but the Sterne and Egger (2005) reference has multiple issues (overextensive use of capitalization and the page numbers are missing). Please stick to the APA rules carefully for all of the references.  
Supplementary materials. Many of the references in the list of included studies do not comply with the APA rules. The "Anonymous (in preparation)" reference is excessively vague. Please check all of these references carefully.   
Reference  
Leary, M. R., Kelly, K. M., Cottrell, C. A., & Schreindorfer, L. S. (2013). Construct validity of the Need to Belong Scale: Mapping the nomological network. Journal of Personality Assesment, 95, 610-624.

Reviewer #3

This is a relatively compact paper that reports a meta-analysis of studies on ostracism that use the Cyberball method. It finds a large effect, not moderated by age, gender, duration of procedure, and various other aspects of the game and procedure.

I have a mixed reaction to this. I do think the paper deserves to be published. It adds a few useful bits of information to the literature. The meta-analysis seems to have been done competently. The paper is well written. On the negative side, the findings are not all that dramatic, especially given that there are already several published meta-analyses of work on ostracism. We already know, I think, that Cyberball (and other procedures) produce significant effects. So I’m not sure this paper is quite strong enough for Psychological Bulletin. But it certainly deserves a place in the literature.

Also, do I understand correctly that all different kinds of dependent variables are lumped together to produce an overall average effect size? This is a bit confusing. Are we to infer that cyberball has a large effect on every possible variable? Surely there must be some things that are not affected by this manipulation.

One issue that could be addressed in a revision is whether the Cyberball is specifically a manipulation of exclusion and rejection, as opposed to affecting other things as well. The authors at times describe it as specifically affecting exclusion, thus as a manipulation of being socially ignored and rejected. At other times they cite Williams’s point that ostracism affects multiple other needs, such as control. If I understand the game, it seems like it could be considered a manipulation of control, akin to learned helplessness. In one condition, the participant catches the ball and throws it to others, thus successfully exerting control. In the other condition, the person is deprived of any opportunity to catch and throw, despite constantly wanting such an opportunity and presumably anticipating it at each throw between the other players. Isn’t it plausible that part of the success of this manipulation, including its large effect sizes, is due to the deprivation of control?

Addressing this question would be of considerable importance to the field. Some authors seem to treat and use Cyberball manipulation as something that is quite specific to social exclusion. Others, including Williams himself, have discussed it in broader terms, including its effect on control (not to mention self-esteem). Given the popularity of this manipulation, it would be a valuable service to the field to determine what it manipulates.

Comments Kip

my first response is, other than reviewer 2, these are surprisingly substandard reviews for any journal, let alone *Psych Bulletin*. If our graduate students wrote such reviews in our journal review class, they would receive Fs for these other two reviews.

I'll read Rev 2 in more detail soon to give suggestions.

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Okay, having read all 3 reviews and editor's comments, I think that the major and difficult issues deal with the importance/usefulness of the meta-analysis. To that end, readers don't care about the need satisfaction measures (by themselves) or the temporal predictions. What they probably care about is (a) discussing factors that people wonder if they moderate the effects (gender, ethnicity, culture, age, 3- vs. 4-person games, number of throws, % of excluded throws! personality/individual differences, ingroup-outgroup), and (b) what are the reliable DV effects and what aren't reliable, broken down perhaps by meaningful categories: Intrapsychic self-reports (of which the need satisfaction and mood measures are but one type), pro- and anti-social self-reports, pro- and anti-social intentions, pro- and anti-social behaviors. We can perhaps exclude EEG and fMRI studies from consideration because of Cacioppo's work, although that would interesting to include, too.   
That's what I Hinkle would make a compelling Cyberball meta-analysis.

Comments Ilja

Hi Chris, Jelte and Kip

My reading of the reviews is that they are quite positive about the quality of our work but that they wonder whether it advances the field far enough to be published in psychological bulletin. The editor also mentions that it was a close call.

One reviewer gives a summary of our paper. I think the summary is correct and it is thus good to know that the main part of our paper got through: (a) big effects, (b) no moderation by structural aspects of the game age, duration, etc. and (c) no support  for less moderation at immediate measures than delayed measures.

The other reviewer muses about whether cyberball is a manipulation of control. He/she advises us to test this.  I am not sure whether we can. This would require that we look at the specific needs and report separate effect sizes for each fundamental need. The key prediction would then be that control needs have a bigger effect size than the other needs. First, I doubt that this is the case. After all, the intercorrelations between these meausures are high and therefore typically lumped together in analyses. Second that would require considerable work as I do not believe we have this data readily available. This would thus – I think – basically be a new meta analysis.

The third reviewer is most informative.  I think he/she offers many suggestions that are relatively easily addressed. We can update all parts where more information is needed.  I think we should  focus more on the unique selling points that sets it apart from prior meta analysis. What do we now know more than we did from these others?

As for new journals. I think PSPR is a likely candidate (i.e, blackhart, nelson, Knowles, baumeister) or Perspectives on Psychological Science (i.e., gerber and wheeler). If, “not novel”  is again problematic we can also aim for PlosOne.