Temporal effects of ostracism: A meta-analysis of Cyberball studies.

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**Abstract**

We present a meta-analysis of randomized experiments that involved the use of Cyberball to determine whether immediate effects of ostracism decrease over time. In addition we study whether moderation of the ostracism effect by theoretically hypothesized factors (e.g. nature of the group increases over time. To this end, we considered both the first and last outcome measures in 120 experiments (combined N = 11,869) and submitted the data to random- and mixed effects models. Results show that the estimated average ostracism effect is large (d >1), decreases from the first to the last measure across types of measures, and that time passed since being ostracized does not predict the average effect size on the last measure. The analysis of moderation of the ostracism effect by crossed factors showed an average interaction effect of medium size on both the first measure and last measure. Because the confidence intervals for these two average interaction effects overlap, there is no indication for change of the average interaction effect over time. This suggests that ostracism can be moderated immediately after an ostracism sequence, and equally so longer after. Moreover, we found (in an exploratory analysis) that even substantively and methodologically similar studies showed considerable heterogeneity in outcomes (I2 = 92%), posing the question whether the ostracism effect is actually a reflex. We discuss implications of the findings.

*Keywords: Cyberball, meta-analysis, temporal, ostracism*

Since Williams, Cheung and Choi introduced the Cyberball game in 2000, thousands of people have played it. Cyberball is a virtual ball-tossing game, used to manipulate the degree of social inclusion or ostracism in an experiment. Within the ostracism and exclusion fields, this paradigm has been widely used next to other paradigms, such as the future life rejection (see Baumeister, Twenge, & Nuss, 2002). For ostracism however, Cyberball seems to be the major paradigm. More specifically, the literature search that formed the basis of this paper showed that at least 200 published papers involved the use of the Cyberball paradigm, and that over 19,500 participants have played the game. Thus, the Cyberball paradigm has received much traction in experimental work. In the thirteen years since its introduction several major developments have been made in the field of social ostracism.

**Historical background.** Throughout these years social ostracism has culminated in updated theory (i.e., Williams, 2009) and several meta-analyses (Blackhart, Nelson, Knowles, & Baumeister, 2009; Cacioppo, Frum, Asp, Weiss, Lewis, & Cacioppo, 2013; Gerber & Wheeler, 2009). Since the mid-1990s, the interest for this topic is on the rise (Williams & Jarvis, 2006), because of widespread interest in the topic (i.e., everybody gets excluded sometime) and because of its societal relevance (e.g., in the context of school shootings; Leary, Kowalski, Smith, & Phillips, 2003).Through experimental work, it has been repeatedly shown that being ostracized has a detrimental effect on people—on either their psychological functioning (e.g. decreases in mood; e.g. Lustenberger & Jagacinski, 2010) or certain interpersonal behaviors (e.g. increases in aggressive behaviors; e.g. Van Beest, Carter-Sowell, Van Dijk, & Williams, 2012). In the current paper, this detrimental effect of being ostracized will be referred to as the ‘ostracism effect.’ The theory and relevant meta-analyses on this ‘ostracism effect’ from recent years will be shortly reviewed below, after which the goals and hypotheses of the current meta-analysis are described.

Williams (2009) proposed a temporal model of ostracism, in which he suggested three stages in the ostracism effect, namely: (1) a reflexive stage (also called ‘immediate’), (2) a reflective stage (also called ‘delayed’) and (3) a resignation stage. In the reflexive stage, the response to the ostracism sequence occurs like a reflex. This initial response is theorized to be more painful and threatening than later on, due to evolutionary over-sensitivity to cues of ostracism. Such over-sensitivity does not take into account situational specifics, and provides no or little room for coping. The reflective stage, which follows this immediate response, is subject to more rational thought and coping with the threats. The resignation stage occurs after prolonged ostracism, causing prolonged periods of pain and threat. Such prolonged ostracism sequences lead to helplessness, which is a core characteristic of the resignation stage. Williams’ temporal model implies (but does not claim) that as time passes the effects of a singular occurrence of social ostracism will diminish and moderation of such effects by other socially relevant factors (e.g., type of group from which one is excluded) will increase over time.

Previous meta-analyses on the topic of social exclusion have focused on estimating the effect of exclusion1 on different constructs (e.g., belonging, self-esteem, etc.; Gerber & Wheeler, 2009), moderation of effects by types of manipulation (Blackhart et al., 2009) and effects in fMRI studies (Cacioppo, et. al, 2013). Specifically, Blackhart and colleagues’ focused on effect sizes in emotion, mood and self-esteem *and* considered which study characteristics influenced effect sizes (e.g. future-life rejection vs. Cyberball).Whereas these meta-analyses focused on the social ostracism effect within different constructs, in different exclusion paradigms, on specific constructs and on fMRI studies, the current meta-analysis limits the paradigm to that of Cyberball and looks to test more general ideas of social ostracism. Limiting the heterogeneity across study designs included is presumed to decrease variability due to design characteristics. This increases power for moderator analyses. Here we investigate the workings of the reflexive and reflective stages but *not* that of the resignation stage. More specifically, the current meta-analysis considers whether the effects of social ostracism decrease over time; whether they can be moderated, and whether this moderation increases over time.

Two elements are central to this meta-analysis: moderation and time. The operationalization of moderation is straightforward: many experimental designs include a second factor besides the factor related to ostracism, where interaction would indicate moderation. For example a 2 (social status: ostracized vs. included) by 2 (group: in-group vs. out-group) design, where the ostracism effect is expected to be larger for the in-group level than for the out-group level. Such moderation can be numerically seen as an interaction effect. For instance, the difference between simple effects of ostracism for the in- and out-group conditions reflects this interaction (specific calculations are reported in the methods section and formulae in the Appendix). Time was estimated via number of items that were administered after ostracism, which is incorporated in meta-regression analyses as a predictor. These elements make it possible to conduct the analyses needed for our confirmatory hypotheses described later on.

We included only randomized experiments with between-subjects designs that involved the use of the Cyberball paradigm. The main reasons for these inclusion criteria are (1) to assure study quality, (2) to encompass the typical Cyberball experiment, and (3) to remove the need for within-subjects correction of effect sizes. These criteria were also used to ensure broad enough criteria to include as many studies as possible to test our hypotheses, while maintaining study quality.

**Hypotheses.** The current meta-analysis is primarily concerned with modeling the ostracism effect overtime and the degree to which ostracism effects are moderated as time progresses. We will also explore whether specific changes to the Cyberball manipulation affect the average effect sizes and whether the average effect sizes differ between different types of measures. Below, the confirmatory hypotheses and the exploratory hypotheses will be outlined. These hypotheses were registered before the analyses were run on the Open Science Framework.2

**Confirmatory hypotheses.** One of the main ideas behind a temporal model of any construct is that the effect changes over time. We will analyze two changes across time. First, the traditional ostracism effect will be estimated on both the first and last measures as they were used in the primary studies. The main question is: does the effect size of ostracism decrease over time? A formal test of this time-based change is not possible in the current setting (reasons for this are further explained in methods section). Therefore two indirect approaches are taken: 1) the average effect size on the last measure is estimated within a mixed-effects model with time passed since the ostracism manipulation as a predictor, and 2) the confidence intervals of the average effects are compared for overlap. Formally, this first hypothesis can be regarded as

where δ represents the simple effect between the ostracism and inclusion conditions, and T1 and T2 indicate the first and last measure, respectively.

Second, we will inspect the moderation of ostracism over time. From Williams’ (2009) temporal model it follows that the reflexive response becomes less as time progresses, after which coping plays a larger role. This implies that as time progresses, moderation of the ostracism effect would *increase*. Hence, the main question behind this second hypothesis is: does the average moderation of ostracism increase over time? Formally,

where Δ represents the interaction effect and T1 and T2 again represent the first measure and last measure, respectively. The interaction captures the difference between the simple main effects, as will be explained further in the methods section. In sum, the second hypothesis tests whether the moderation effect of ostracism is itself moderated by time.

**Exploratory hypotheses.** For exploratory purposes, we will also inspect several compositional elements of the studies. These elements can provide some nuances in how the composition of the study with regards to the Cyberball manipulation, sample composition or type of measure, affect the effect sizes. We will use meta-regressions with study-level indicators to study effects of the composition of the manipulation and the samples.

The measures included in the meta-analysis were only subject to the criterion that they were affected by ostracism, which does not limit the measures to just one type. In other words, we included multiple measures with varying psychometric properties in the primary studies. This choice introduced heterogeneity of effects due to differences in reliability of measures and traits being measured. Such heterogeneity increases the difficulty of assessing the moderating effect of psychometric qualities overall. The preponderance of measures used in Cyberball studies concern ‘fundamental needs’ (i.e., belonging, self-esteem, control and meaningful existence) questionnaires, which were developed within the Cyberball paradigm (see Van Beest & Williams, 2006; Williams et al., 2000; Zadro, Williams, & Richardson, 2004). Next to these fundamental need questionnaires, the measures vary widely and no clear grouping can be made besides intrapersonal measures (i.e., measures relating to the self only) and interpersonal measures (i.e., measures relating to others as well). A more thorough discussion of the psychometric properties of the ‘fundamental needs’ questionnaires will be given in the discussion section. Important to take away is that there are multiple types of measures included in the effect size calculation and subsequently the meta-analysis. This means that the estimated effect size will be an overall estimate and not one for a specific measure, such as belonging. The types of measures are subdivided into fundamental needs, intrapersonal and interpersonal measures and theses subsets are tested separately as a sensitivity analysis of the overall results.

In sum, the hypotheses are subdivided into two main confirmatory and several exploratory questions. The two main questions underlying the confirmatory hypotheses are 1) does the ostracism effect diminish over time? and 2) does the moderation of the ostracism effect due to crossed factors increase over time? The question underlying the exploratory hypotheses is: do the study characteristics affect the estimated average effect? These questions will be answered below with random and mixed-effects meta-analytic models, encompassing 120 studies across the board.

**Method**

**Study inclusion criteria.** Studies were required to use at least an ostracism factor (i.e., included or ostracized) manipulated via the Cyberball paradigm. We included only studies that incorporated a between-subjects design with random assignment. Studies that used other (between-subjects) factors alongside the ostracism factor were included as well. We collapsed effect sizes across irrelevant factors if primary authors expressed no expectation concerning the potential moderating effect of that crossed factor (i.e. non-moderating factors). Whenever primary authors did express an expected moderation by a crossed factor in the primary study, we coded that effect size “as is”. For instance, if authors expected having a dog present while being ostracized would diminish an ostracism effect (Aydin, Krueger, Fischer, et al., 2012), we included dog present/absent as crossed factor. These moderating factors were not required to involve random assignment (i.e., quasi-experimental was allowed). Dichotomized factors were collapsed to an ostracism/inclusion factor due to the many problems dichotomization can cause (e.g., underestimation of effect size, spurious effects; see Hunter & Schmidt, 1990; MacCallum, Zhang, Preacher, & Rucker, 2002). For example, consider a grouping of participants into high- and low neuroticism groups based on a continuous measure of neuroticism (Boyes & French, 2009). In such cases, we used pooled means and standard deviations across these two groups, reducing the design to an ostracism/inclusion design.

Reasons for these inclusion criteria are threefold. First, these criteria were assumed to heighten study quality, which is preferable to subjective quality assessment for individual studies. Second, the inclusion criteria of only between-subject designs rendered computational aspects simpler given the effect size metric used (i.e. standardized mean difference). Third, most Cyberball experiments take place in such a format, making it encompassing criteria for the purposes of this meta-analysis. Within-subject designs were excluded, for several reasons. Firstly, most within-subjects designs regard high-dimensional physiological measurements such as fMRI that are beyond the scope of this meta-analysis (see Cacioppo, et. al, 2013). Secondly, meta-analyses of effects of within-subjects designs require the correlations between measures in primary studies, and we did not expect these to be reliably reported in the papers.

For the dependent measures the criterion was that they were (expected to be) affected by the ostracism manipulation. Manipulation checks (i.e., questions like “I was ignored”, “I was excluded”, or “I received X percent of the throws”) were excluded due to them measuring the manipulation rather than the ostracism effect. We only included studies reported in Dutch or English. In sum, all available English and Dutch manuscripts or papers that used at least a Cyberball manipulation in a randomly assigned between-subjects design were included.

**Literature search.** To have an encompassing meta-analysis of Cyberball studies, seven search strategies were used in the period of November 2012 through April 2013. These search strategies included 1) database searches, 2) a call for data,3) cross-reference with an existing list of Cyberball studies, 4) Google Scholar alerts, 5) citation records, 6) SPSP conference abstracts and 7) personal communication. These will be further explained below.

The databases searched included Web of Knowledge, PubMed, ScienceDirect and Worldcat. The first three cover only published articles, whereas Worldcat also covers dissertations. All these databases were searched with the keywords “cyberball”, “ball-tossing” and “ball AND ostraci\*”. Web of Knowledge was the first database searched. For this database, an additional search term (i.e. “ball AND exclu\*”) was used, but this yielded zero relevant hits on a total of 501 hits. Due to specificity of the searches, this last search term was dropped for the remaining databases. Across all these searches, results included 1927 hits of which 109 were saved for coding. Within Web of Knowledge, citation records of the seminal papers by Williams and colleagues (2000); Williams and Jarvis (2006)were looked through. These papers were cited 332 times (as of 5th of November, 2012), of which 43 papers were saved for coding. This literature search summed up to an initial of 2259 hits, of which 152 were selected to be included in the coding.

The call for data was put on the listservs or forums of Society for Personality and Social Psychology (SPSP), European Association of Social Psychology (EASP), and Social Psychology Network (SPN; all on 3rd of December, 2012). This resulted in nine replies, from which three useful studies were included in the coding procedure.

Kip Williams keeps a list of Cyberball studies on his personal website. This list was used to check for extra articles that did not turn up in the initial searches on the 15th of November.3 The list included 93 papers, of which 9 papers were included to be coded.

The final searches included Google Scholar alerts, SPSP conference abstracts and personal communication. The Google Scholar alerts were used to keep up to date with new literature. These alerts notify a user when new hits for a search term occur, and were used for “cyberball” and “ball-tossing”, which yielded 85 hits of which 25 were saved for coding. SPSP conference abstracts from 2006 through 2013 were searched for Cyberball studies. This led to personal communication with the authors of the conference abstracts, leading to additional studies. Pooled, the personal communication and the conference abstracts yielded 21 hits of which 20 were saved for coding. The seminal paper by Williams and colleagues (2000) was added separately.

In sum, the literature search spanned 2468 hits, resulting in 205 hits that were saved for coding. These papers spanned over 19,000Cyberball players. During coding, papers were assessed to fit the inclusion criteria. These papers were published between 2000 (after the introduction of Cyberball) and April 2013. Several papers appeared to be non-applicable (e.g. within-subjects design, lack of data after multiple requests put to authors), causing them to be excluded from the meta-analysis. This resulted in a final, fully coded sample of 98 papers containing 120 studies, across 11,869 Cyberball participants.4

**Coding procedure.** The first author coded all the studies, of which the second- and third author checked the coding for a subsample of studies. Due to the amount of studies included in coding (i.e. 120), such intercoder agreement was deemed adequate. For reproducibility purposes, an extensive account was kept of the decisions made during the coding, which is publicly available via Open Science Framework on a paper-by-paper basis (see Footnote 2 for the direct link).

The coding scheme was set up on the basis of substantive and methodological considerations. This resulted in a coding scheme primarily based on group means and standard deviations for effect size calculation. These statistics were retrieved for both the first and last relevant measure in each study. Relevant measures were defined as constructs that were expected by primary authors to show an ostracism effect (e.g. fundamental needs, mood, pro-social helping behavior, etc.). Coding that was crucial for testing the confirmatory hypotheses concerned the amount of items from the first through last measure plus any additional time in between (e.g., rest period). This made up the estimation of time from the first to last measure, where each item was counted as lasting six seconds (the six-second rule was based on a longstanding practice used to estimate average completion time in the freshmen testing program of the University of Amsterdam). Any additional time reported in the procedure was also coded. Note that some measures are variable on time (e.g. persistence tasks) and that these are arbitrarily estimated in a conservative manner to at least take these measures into account at some level.

The type of measure used was coded for in the following general terms: (1) fundamental needs, (2) intrapersonal and (3) interpersonal. Intrapersonal measures were defined as measuring constructs that relate only to the self (e.g., ‘how angry do you feel?’). Interpersonal measures were defined as measuring constructs that relate to (the self and) others (e.g., ‘how angry do you feel towards person X?’). Additional coding for the exploratory analyses was done for sample characteristics (e.g. age, gender composition) and Cyberball characteristics (e.g., amount of players, length of game).

Because relevant measures were defined broadly we included many different kinds of measures. Some measures are expected to show different directions of an ostracism effect. For example, “belongingness need satisfaction” and “feelings of aggression.” Belongingness scores are expected to be lower for ostracized participants, whereas aggression scores are expected to be higher for ostracized participants, when compared included participants. To counteract possible computational problems(i.e. cancellation of effects) being caused by this bidirectionality of effects, we used absolute effect sizes to estimate effect sizes.5

Relevant information that was missing in the papers was requested from the authors via e-mail. In case of non-response, three follow-up e-mails were sent. All this communication was documented and can be found on the OSF page for this project. In case of non-response or non-willingness to send data, studies were either eliminated if the information was crucial (i.e., means and standard deviations of the measures per group), computed if possible (i.e., cell sizes), or assumed if deemed reasonable on the basis of additional information. For instance, when no information was given 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.

Because of the subjectivity involved in such judgments, we did not assess the quality of studies. However, quality of studies was ascertained by setting relatively strict inclusion criteria (i.e. between-subjects; random assignment). However, it is noteworthy that this did not guarantee the quality of all dependent measures.

**Statistical analyses.** For the analyses, we used the “metafor” package (Viechtbauer, 2010) in the R statistical environment (R Core Team, 2013). This section discusses the effect size metric, meta-analytic model and its constituent elements, sensitivity analyses and funnel plot asymmetry tests.

**Effect size metric.** We used Hedges’ g version of the standardized mean differences as the effect size. Hedges’ g corrects for the slightly biased estimate Cohen’s d gives (Hedges, 1981).Standardized simple effects were calculated across the ostracism factor and the interaction effect was calculated by taking the standardized difference between the unstandardized main effects (see the Appendix for the exact formulae used).This was done for both the first and last measurement. In other words, for the first measure, we calculated both the simple main effect of ostracism and an interaction effect; similarly so on the last measure. Therefore, in a 2 (social status: ostracized vs. included) by 2 (moderator: present vs. absent) design with multiple measures, there are two simple effects and two interaction effects included in the datafile. Because we only used between-subject designs, each of such factorial studies delivered two simple effect sizes (one independent sample for each level of the moderating factor) and an interaction effect on both the first and last measure, i.e., six effect sizes in total. Non-factorial studies delivered only main effects for the first and last measure and no interactions.

**Meta-analytic model.** We used random- and mixed-effects models, since heterogeneity in the effect sizes is expected due to the different measures included and additional unknown methodological and substantive factors. The meta-regression element in some of the analyses is the variable time as moderator. Analyses without this study-level predictor reduce to a random-effects model. We used Restricted Maximum Likelihood (REML)to estimate tau-squared (the (residual) variance), as recommended by Viechtbauer (2005). It should be noted that when estimating a mixed- or random effects model, an average effect is estimated—*not* the ‘true’ effect (Viechtbauer, 2005).

**Sensitivity analyses.** To test for robustness of the effects, we incorporated several sensitivity analyses. We flagged possibly problematic outliers on the basis of studentized deleted residuals, Q-Q plots, and Cook’s distance values. Subsequently, we inspected the effect of these outliers on substantial results in sensitivity analyses in which these outliers were excluded. Another sensitivity analysis entailed fitting of the mixed-effects model with tau-squared fit at the upperbound value of the 95% confidence interval.

**Funnel plot asymmetry.** A ‘funnel plot’ plots each study’s effect size against its standard error (Light & Pillemer, 1984). Larger studies have smaller standard errors, and vice versa for smaller studies. Following from a theoretical fluctuation of the population effect size due to sampling variance, a funnel plot should be symmetrical around the estimate effect size. Funnel plot *asymmetry* can indicate many things, including publication bias (Bakker et al, 2012). To this end, we used Egger’s regression test(Egger, Smith, Schneider, & Minder, 1997)for mixed-effects models (Sterne & Egger, 2005). This tests whether the distribution of effect sizes is equal on both sides of the average effect, when accounting for true heterogeneity. Funnel plot asymmetry thus indicates bias in the estimated effect size, and possibly publication bias.

**Results**

The total sample included 98 papers (out of 205 examined), with a total of 120studies including 11,869participantswith mean sample size being 98.9 and median sample size 74.Of the examined 205 papers, 107 papers were excluded for a variety of reasons, mainly because of the use of a within-subjects design (52 papers). Also, some papers could not be accessed (5 papers) or could not be included because we did not receive the required data on time (7 papers). Some had other reasons for exclusion (43 papers), such as not introducing data or a dissertation that had data included in a published paper.

In our reporting of the effect sizes, d indicates a main effect and Δd indicates an interaction effect. Even though Hedges’ g effect sizes were used, the notation of d was maintained, since it is only a minor correction to Cohen’s d. Sensitivity analyses are only reported if they show different effects (all sensitivity analyses can be freely inspected via the files available on OSF).

**Confirmatory hypotheses.** The two confirmatory hypotheses are tested in four meta-analyses. This includes estimation of the average simple effect of ostracism on the first measure, the average simple effect on the last measure and the estimation of the average interaction effect on both the first and last measure. Multiple analyses are needed, because a singular approach would result in a within-subjects design of the meta-analysis, and correlations would be needed to correct for the violation of independent observations. Seeing how these correlations are absent in the coding (due to lack of reporting in papers), the current approach is used.

**Simple ostracism effect (Hypothesis 1).** In a random-effects model on the main effect of ostracism(*k* = 120), heterogeneity was clearly significant, Q(119) = 1285, p < .0001, I2 = 92.7% and estimated τ2 = 0.87, 95% CI [0.68, 1.23].The analysis yields an estimated average effect of d = 1.40, p < .0001, 95% CI [1.23, 2.26].A random-effects version of the Egger’s test (Egger et al., 1997; Sterne & Egger, 2005) showed that there is funnel plot asymmetry, z = 6.87, p < .0001, indicative of bias in the estimated effect. Due to the size of the effect, and hence large power to acquire significant outcomes in primary studies, we do not suspect publication bias to make up this asymmetry. In other words, immediately after being ostracized, the average ostracism effect is estimated at 1.96 standard deviations.

Next, we fitted a mixed-effects regression model for the ostracism effect on the last measure (*k =* 95), including estimated time in seconds as predictor. Residual heterogeneity was clearly significant, QE (93) = 704, p < .0001 and estimated τ2 = 0.30, 95% CI [0.21, 0.43]. The intercept was estimated to be: dintercept= 0.81, p < .0001, 95% CI [0.68, 0.95]. Estimated time in seconds did not moderate the average effect, b= -0.0001, p = .115, 95% CI [-0.0003, ~0.0000]. We have to take into consideration the low power of the moderation analyses due to the large (residual) heterogeneity in effect sizes (Hedges & Pigott, 2004). A regression test for mixed-effects model with moderator showed no funnel plot asymmetry, z = 1.09, p = .276. In short, long after ostracism has occurred (Mtime = 291.18 seconds), ostracized participants on average score around 0.81 standard deviations differently from included participants, an effect that does not appear to be moderated further by time passed since the ostracism occurrence.

It thus seems that there is an effect of ostracism on both the first and last measures, of which the latter is *not* moderated by time in our analysis. The ostracism effect over time is alternatively inspected via confidence intervals. Comparing the 95% confidence intervals for the average ostracism effect on the first measure (i.e., [1.23, 2.26]) and on the last measure (i.e., [0.68, 0.95]) shows that these do not overlap. Although the difference in average effect sizes between first and last measure cannot be formally tested (because of a lack of information on the correlation between measures in the primary studies), a difference is present and CIs suggest that the average ostracism effect is smaller for the last measure. Seeing that estimated time did not appear to moderate the ostracism effect on the last measure in our meta-regression analysis, this suggests that other factors besides time passed since the ostracism may account for the smaller effects on the last measure.

**Moderation of ostracism (Hypothesis 2).**A random-effects model on the interaction effect (Δd) on the first measure (*k* = 52) shows no heterogeneity, Q(51) = 61.44, p = .150, I2 = 21.3% and estimated τ2 = 0.05, 95% CI [0, 0.157].An average interaction effect was found, Δd = 0.59, p < .0001, 95% CI [0.45, 0.73], indicating a change in the ostracism effect due to the moderator level and vice versa (i.e., moderation of the ostracism effect). There was an indication of funnel plot asymmetry, z = 3.91, p < .001. In other words, the data indicate that the ostracism effect *can* be moderated on the first measure following the ostracism sequence.

On the last measure (*k* = 46), the mixed-effects model (with estimated time as predictor) for the interaction effect showed no residual heterogeneity, QE(44) = 40.8917, p = .606 and estimated τ2 = 0.004, 95% CI [0, .075]. The intercept of the interaction effect was estimated at Δdintercept­ = 0.48, p < .001, 95% CI [0.35, 0.61] and no moderation of time was found, b = ~0.0000, p = .838, 95% CI [-0.0002, 0.0002]. The regression test showed funnel plot asymmetry, z = 3.59, p = .0003. These results indicate that moderation of the average ostracism effect is also found as time progressed, and time itself does not moderate this interaction.

To see whether the interaction decreases from the first to the last measure, we again compared confidence intervals. On the first measure, the 95% CI was [0.45,0.73] whereas for the last measure, the 95% CI was [0.35,0.61]. Given the clear overlap of these CIs, there is no indication for decrease in the moderation across the measures examined.

In sum, our analyses of the confirmatory hypotheses indicate that the simple main effect of ostracism is very large (d = 1.40) at first, reducing to d = .78on the last measure (when using the mean time to estimate the effect from the meta-regression). For the interaction effect, the data indicate there is an interaction effect on the first (Δd = 0.59) and the last measure (Δdintercept = 0.48), which are similar in size. In other words, the current data indicate that ostracism has a large effect at first, which reduces later on in the studies (but not necessarily because of elapsed time) and this ostracism effect can be moderated equally across the first and last measure.

**Exploratory hypotheses.** The exploratory analyses concern simple effects of both ostracism and the moderators in the studies, in subsets of studies that differ in terms of measure type, sample composition, scale composition, Cyberball specifics. We also inspect variability in a homogeneous subset of studies to come to grips with the relatively large heterogeneity of simple main effects found in the confirmatory analyses.

**Simple effects.** The confirmatory analyses showed that the effect of ostracism is different for the different levels of moderators used in factorial experiments. Running random effects models for the simple effects across all measures shows the nuances of the average interaction effect, because one can compute 4 different effect sizes in the 2-by-2 factorial designs. We also inspected the simple effects per type of measure to see whether there were differences for the subsets (1) fundamental needs, (2) intrapersonal measures, and (3) interpersonal measures. In addition, we coded whether the immediate or delayed measures were in line with the conceptual idea of immediate (i.e. related to during the game) and delayed (i.e. related to after the game).The simple effects overall and the subsets are represented in Figure 1 with dotplots. For space considerations and the exploratory nature of these analyses, test statistics are omitted and only represented in the dotplot figure; effect size estimates are given in-text.

The interpretation of main effects is compromised by the presence of an interaction. After all, the interaction between X and Y indicates that the main effect of X is different for the different levels of Y. As described, our analyses involved 2-by-2 factorial designs in which we can compute different effect sizes. In the main analysis, we considered the two simple main effects together as they are based on independent samples. However, in light of the interactions, it is useful to consider specifically all four effect sizes that can be computed in such designs. These are: (1) the simple ostracism/inclusion main effect with the expected moderator present (e.g., dog present during Cyberball) or (2) expected moderator absent (e.g., dog absent during Cyberball; Aydin et al., 2012), (3) the simple main effect of the moderator (present vs. absent) in the ostracism level or (4) the inclusion level.

Inspection of the overall simple effects shows that the ostracism effect is present on both the moderator present (d = 1.44, 95% CI [1.11, 1.76]) and the moderator absent (d = 1.40, 95% CI [1.23, 1.58]) levels on the first measure. The figure also shows that the moderator factors have an average effect on both the ostracism (d = 0.57, 95% CI [0.42, 0.72]) and inclusion (d = 0.48, 95% CI [0.35, 0.60]) levels. On the last measure, the ostracism effect decreases (moderator present: d = 0.80, 95% CI [0.59, 1.00]; moderator absent level: d = 0.77, 95% CI [0.65, 0.90]), whereas the effect of moderation is similar on the last measure (ostracism level: d = 0.40, 95% CI [0.31, 0.50]; inclusion level: d = 0.35, 95% CI [0.26, 0.44]). This thus indicates that the simple ostracism effects decrease over time, but the simple moderator effects stay similar in size.

**Measures.** To inspect how robust the overall estimate is, we ran similar simple effects models across several subsets of measures. These measures encompassed (1) fundamental needs (single- and composite measures), (2) intrapersonal measures (i.e. measures that relate only to the self), (3) interpersonal (i.e. measures that relate to others or the self in the context of others) measures and (4) measures that were coded by the first two authors as fitting the description of being immediate or delayed (i.e. questions related to during- or after the game, respectively). The analyses for the different measures are conducted for the two time points separately (i.e. first and last measure).

The second, third, fourth and fifth panels in Figure 1 show the results per subset. Comparing these dotplots shows that the trend across the subsets is similar, except that the ostracism effect is stronger in the subset of fundamental needs and weaker in the interpersonal behavior measures. Also, the effect of the moderator on the inclusion level is not significant for the fundamental needs subset. These differences are only minor, but should be taken into consideration when estimating power for different kinds of measures. However, substantively, only one result is different and only in case of the fundamental needs. Hence, conclusions only change in one case and we therefore conclude that the findings of the overall estimates can be considered robust across the subsets.

**Composition.** We ran a mixed-effects model on the ostracism effect of the first measure (as in Hypothesis 1) for the composition effects, because we expected that the effects would be largest in this setting and the highest number of studies could be included in this analysis. The predictors in the mixed effects model were (1) number of players in the game, (2) length of the game, (3) number of throws in the game, (4) proportion of males in the study, and (5) mean age of the participants and (6) type of needs scale referenced. This model (*k =* 43) showed clear residual heterogeneity, QE(36) = 348, p < .0001, estimated τ2 = 0.62, 95% CI [0.37, 1.09], but no overall moderation, QM(6) = 4.37, p = .627. Inspecting the predictors individually also showed no indication for moderation (*p*s> .22). These analyses are again troubled by the large heterogeneity in the effect sizes, necessitating the moderators to have very a very large influence on the effect (i.e., half a standard deviation at least) in order to be detectable. However, even the overall moderator test showed no such effect. In sum, no compositional moderators were found, but this might be due to the large (unmodelled) heterogeneity in the effect sizes.

We performed an additional exploratory analysis to establish a more homogenous subset of studies. As was seen in the analysis of the ostracism main effect on the first measure, heterogeneity made up 92.7% of the variability. A subset of typical Cyberball studies was selected to further understand heterogeneity in outcomes. This type of study was defined as having 3 players in the game, 30 throws, lasting less than five minutes and measuring fundamental needs (single or composite). Performing a meta-analysis on these ‘typical Cyberball studies’ (*k =* 37) showed an I2 value of 92%, indicating there is much variability in the effect sizes even in this substantively homogenous subset of studies. In other words, the heterogeneity found in the overall analyses does not appear to have arisen from the inclusion of different measures and the use of alternative Cyberball setups.

In sum, the exploratory tests indicate several things. First, there appear to be no composition effects of the study on the average effect size. Second, the overall estimated effects of ostracism are robust for measure effects, except for the simple main effect of the moderator factor on the inclusion level, where the fundamental needs measures shows no effect on the last measure. Third, there is much heterogeneity in the effect sizes, even when taking a very heterogeneous setup of the study and its measures. Fourth, inspecting the simple effects for the moderator factor per ostracism level shows that moderation occurs on both the ostracism- and inclusion levels.

**Discussion**

This paper began with a deduction of several confirmatory hypotheses from theory and presenting several interesting avenues for exploratory hypotheses. After having presented the meta-analytic results concerning these hypotheses, we will elaborate on the implications for theory and practice. Thereafter, we discuss several limitations of the current meta-analysis and provide suggestions for future research.

For the confirmatory hypotheses, two questions were central: 1) “does the effect size of ostracism decrease over time?” and 2) “does the average moderation of ostracism increase over time?” The results from the analyses show that there is indication for a decrease in the effect from the first to the last measure, but that this is not moderated by the estimated time. In other words, the aggregated studies do show a decrease in the ostracism effect, but this cannot be simply ascribed to time passed since being ostracized. The interaction effect was present on both the first and the last measure. In other words, the results indicate that the ostracism effect can be moderated by other factors equally across time.

We also ran exploratory analyses to study whether the make-up of a study influenced the effect sizes. The analyses showed that this was not the case for mean age of the participants, proportion of male participants, and the setup of the Cyberball manipulation. Exploratory analyses also showed that the effects of ostracism were equal across subsets of measures and the overall set of measures, except that interpersonal measures showed relatively weaker ostracism effects, while fundamental needs measures (single and composite) showed somewhat stronger ostracism effects on the first measure. Another exploratory analysis showed that there is much heterogeneity in the effect sizes found immediately after being ostracized, even when taking a subset of the studies that is highly homogeneous (i.e. the typical Cyberball study that measures fundamental needs). This seems to go against the idea of the reflexive response to ostracism, as proposed by Williams (2009). A reflex usually implies a similar response to a similar stimulus, which the data suggest is not the case for the ostracism effect within the Cyberball paradigm, at least as measured in the current set of studies. The exploratory hypotheses indicate that the ostracism effect has some nuances that have not been suggested before.

It thus seems that the data provide some new insights to the workings of ostracism. Congruent is that the effect of ostracism seems to decrease as the studies progress (first alternative hypothesis: the ostracism effect is smaller on the last measure, when compared to the first). Time as a predictor seems to not play a role, even in the subsets of measures. This lack of moderation is possibly due to one of three reasons: (1) the heterogeneity in the effect sizes is too large to find moderation by time, (2) imprecise reporting of the measures in the papers led to faulty time estimation, or (3) the difference in the effect size is not due to time but differences between the type of measures administered at the different time points. For the imprecise reporting of the measures, authors could be contacted, but this also poses new problems (i.e., nonresponse or authors might not be willing to admit that measures were left out in the paper; LeBel et al., 2013). The difference in measures can be inspected by creating a difference index between the types of measures and regressing that on the effect sizes. Doing this for the main effect on the last measure, shows no significant effect of this difference (b = -0.07, p = .32). In short, there are some limitations of the analyses with time as a moderator, but these limitations are either hard to address (i.e. imprecise reporting or heterogeneity), or the data indicates the opposite (i.e. difference in measures).

Our second hypothesis concerned direct moderation of ostracism in primary studies (i.e., by the interaction, not the predictor). The data in this paper suggests that there is similar moderation of the ostracism effect on both the first and last measures in the primary studies. This goes against the second alternative hypothesis derived from Williams (2009), who postulated that as time since the ostracism occurrence increased, coping would increase after being numbed, thereby facilitating an increase in moderation. In contrast, the current data suggest that the ostracism effect can be moderated immediately *and* later on.

In sum, the results of the current meta-analysis extend the theory by Williams (2009), by indicating that the effect of ostracism decreases and also indicating that the ostracism effect can be moderated, regardless of the time point at which this happens.

**Limitations.** Within the current meta-analysis there are several limitations. Four limitations are discussed below.

First, our test of differences between the first and last measure was indirect. In its current setting, the meta-analysis makes comparisons between the first and last measures based upon the confidence intervals of these estimates. This is an indirect and informal test of whether the effects differ. A direct test would provide more conclusive evidence on whether or not the effect is equal across the first and last measurements. However, such a direct test requires correlations between the measurements per study, per cell, which are (usually) not reported in papers. This would thus require a direct request for data from each paper, which would likely yield low response rates (Wicherts, Borsboom, Kats, & Molenaar, 2006), lowering the sample size of the meta-analysis overall.6 This lack of direct testing was thus chosen as a way of retaining sample size within the meta-analysis.

Second, not all measures were included and tested in a repeated-measures meta-analysis. Initially, a pre-test was run including all measures, but this showed that many papers did not include all statistics required for all measures. Requesting all of this information from the authors yielded a limitation that was similar to the first: a trade-off between retaining sample size and comprehensiveness. Another reason why only the first and last measures were included was that every measure would require two separate meta-analyses to test both the main- and interaction effect (increasing Type I error rates) if a similar analytical model was used. If all measures were included, it would increase the importance of including an effect size correction due to correlations between measures, to facilitate repeated-measures analyses to minimize Type I error rates. In other words, the failure to include all measures was to prevent the problem of multiple testing and nonresponse to data requests, which would lead to a smaller sample size.

Third, the heterogeneity in the effect sizes poses a problem for the power of finding any moderator effects (Hedges & Pigott, 2004). This could pose the problem that several of the non-effects found are actually there, but not detected (Type II errors). However, the substantively chosen subset of studies still showed variability in the effect sizes: I2 = 92%. This indicates that the effects are very variable to begin with, and makes it unlikely that the effects are misrepresented.

Fourth, the current meta-analysis only examined between-subjects designs. Possibly there is a difference for the ostracism effect in between- and within-subjects designs, something that we have not directly investigated. Also, the within-subjects designs often used fMRI data or other physiological data such as EEG (27 out of 49 at least), which pose an interesting avenue for further research in a meta-analytic domain of neurophysiological measures to add to the work of Cacioppo and colleagues (2013) within the physiological framework. These references can easily be retrieved from the database of examined papers, as is available on the OSF page of this paper.

A final note is that this paper only summarized the results on the measures included in the studies. However obvious this might be, it should be pointed out, because the use of some of the measures might be criticized. There are three types of scales that are typically used to measure fundamental needs: that by Williams (cf. 2009), that by Zadro and colleagues (2004) and that by Van Beest and Williams (2006). However, these scales can be grouped into two categories: asking state questions in general terms (e.g. ‘I feel excluded’; Williams, 2009) and those asking state questions in more direct terms with regards to the game (e.g. ‘I felt excluded during the game’; Van Beest, & Williams, 2006; Zadro et al., 2004). We did not find that the type of scale moderated effect sizes. However, this might be due to the heterogeneity in the effect sizes, improper reporting of the scale used (some authors assume all scales to be similar) or other reasons. Also, there might truly be no difference. Either way, this is a crucial aspect in the ostracism field. This aspect is in need of more sensitive testing, to see whether the effects are dependent on the type of scale included. Also, no proper psychometric validation has been performed on these scales, even though they are widely used.7 This indicates some possible problems with the fundamental needs scales, which make up many measures in Cyberball studies and thus should be investigated in future work.

Despite these limitations, the meta-analysis provides some interesting findings with regards to the workings of ostracism, and subsequently also poses interesting new questions. The findings show that the ostracism effect is huge, with an effect size approaching two standard deviation units. Additionally, and possibly more interesting, how can it be explained that ostracism can be moderated equally across the first and last measure within the studies? And finally, the heterogeneity in the effect sizes is high even when considering a homogeneous subset of studies. This poses the question whether the ostracism effect is actually as reflexive as has been proposed previously, or whether it taps on different cognitive processes. These findings extend the need-threat model (Williams, 2009), which has played a major role in ostracism research until now. We invite fellow researchers to think and test ideas that might provide some explanation for these findings.

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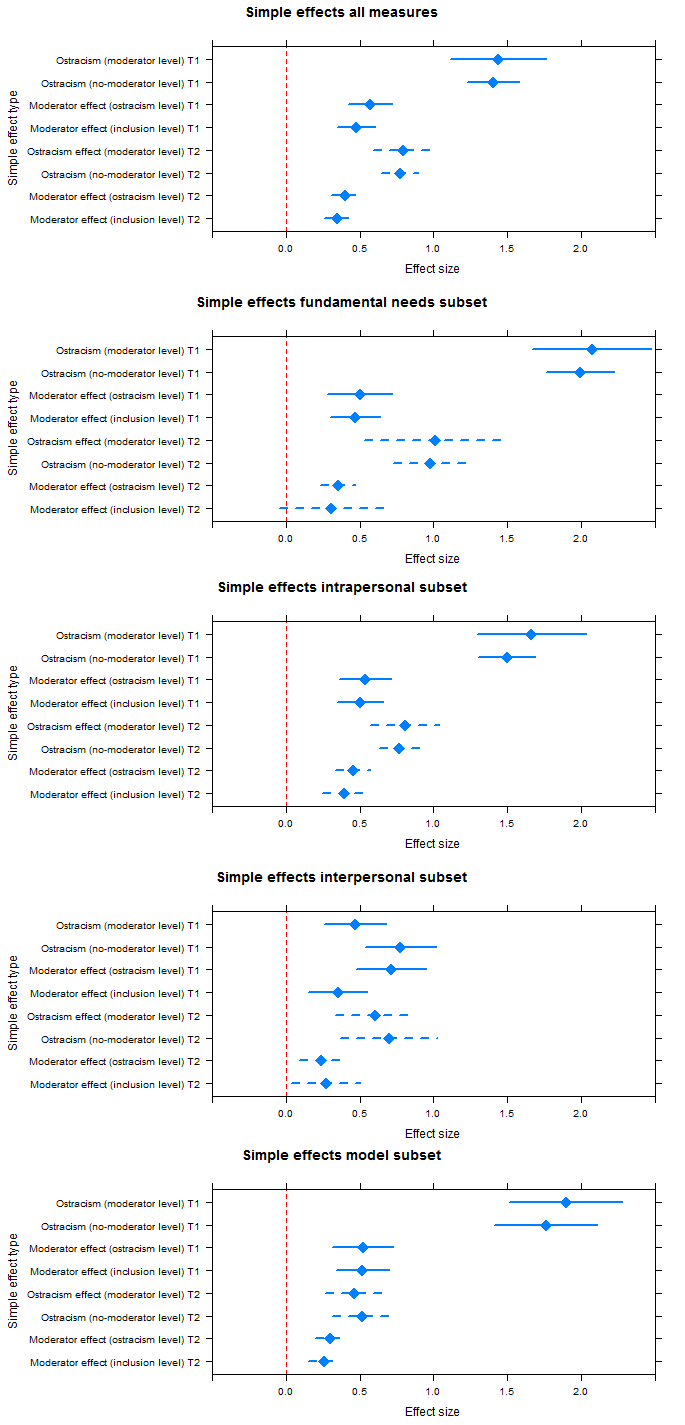
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**Footnotes**

1. Please note that exclusion encompasses both social rejection and social ostracism.
2. The direct link: <http://openscienceframework.org/project/HT25n/>
3. It has been updated since, but the list that was used can be found on the Open Science Framework page.
4. It should be noted that Oaten, Williams, Jones and Zadro (2008)was applicable, but was excluded due to being an outlier with respect to effect size (ds > 15). This in similar vein as in Gerber and Wheeler (2009; p. 473): “*One study (Oaten, Williams, Jones, & Zadro, 2007) had need effect sizes that were clear outliers (effect sizes were 5–7 standard deviations above the means)*[…and…]*were excluded from the analyses.*”
5. We initially coded for these expected effects to correct for this bidirectionality, but it failed to solve the problem (i.e., cancellation of effects still occurred). We therefore decided to use absolute effect sizes and give up any directionality of the effect size estimates.
6. It should be noted, that out of the 72 data requests, we received timely replies of 52 (i.e. ~72%). However, these request were only for specific information and not for raw datasets, as was the case in Wicherts and colleagues (2006).
7. Some might say that *because* they are widely used, they do not require psychometric validation, that is, they would have been selected out of research if they were improper. This however is no argument for a lack of psychometric validation, because there are no arguments to oppose psychometric validation of any scale, that is there are no arguments for using unvalidated scales over validated scales.

| Table 1 – Effect sizes per study for the confirmatory hypotheses | | | | | | | | | | |
| --- | --- | --- | --- | --- | --- | --- | --- | --- | --- | --- |
| First\_author | Year | N | d T1 | (SE) | d T2 | (SE) | Δd T1 | (SE) | Δd T2 | (SE) |
| Alvares | 2010 | 74 | -2.06 | 0.08 | -1.32 | 0.07 | -0.15 | 0.24 | 1.12 | 0.23 |
| Ambrosini | 2013 | 40 | -1.64 | 0.13 | -0.94 | 0.11 | - | - | - | - |
| Anonymous | - | 49 | -3.88 | 0.24 | -1.29 | 0.10 | - | - | - | - |
| Anonymous | - | 91 | -8.01 | 0.40 | -1.43 | 0.06 | 0.75 | 0.56 | 0.53 | 0.18 |
| Aydin | 2012 | 68 | -0.81 | 0.06 | -0.31 | 0.06 | -1.19 | 0.24 | 0.72 | 0.23 |
| Banki | 2012 | 89 | -1.84 | 0.07 | -0.35 | 0.05 | - | - | - | - |
| Bastian | 2010 | 72 | -2.71 | 0.11 | -1.40 | 0.07 | - | - | - | - |
| Bernstein | 2012 | 24 | -0.39 | 0.17 | - | - | - | - | - | - |
| Bernstein | 2012 | 25.50 | -0.99 | 0.18 | - | - | - | - | - | - |
| Bernstein | 2010 | 73 | -4.65 | 0.20 | -4.90 | 0.22 | -0.86 | 0.37 | -1.11 | 0.40 |
| Bernstein | 2010 | 138 | -5.46 | 0.14 | -4.06 | 0.09 | -0.53 | 0.22 | -0.51 | 0.17 |
| Bernstein | 2012 | 67 | -5.26 | 0.27 | -2.89 | 0.12 | -1.07 | 0.45 | -0.80 | 0.30 |
| Bernstein | 2012 | 27 | -1.32 | 0.18 | - | - | - | - | - | - |
| Boyes | 2009 | 89 | -0.43 | 0.05 | -0.79 | 0.05 | - | - | - | - |
| Boyes | 2009 | 87 | -0.19 | 0.05 | -0.83 | 0.05 | - | - | - | - |
| Brochu | - | 35 | -2.43 | 0.20 | -0.46 | 0.12 | - | - | - | - |
| Brown | 2009 | 52 | -0.63 | 0.08 | - | - | - | - | - | - |
| Carter | 2008 | 143 | -0.09 | 0.03 | 0.24 | 0.03 | 0.34 | 0.11 | 0.17 | 0.11 |
| Carter-Sowell | 2008 | 65 | -2.82 | 0.12 | -1.46 | 0.08 | - | - | - | - |
| Carter-Sowell | 2010 | 74 | -4.22 | 0.17 | -4.25 | 0.18 | -1.23 | 0.33 | -1.15 | 0.34 |
| Carter-Sowell | 2010 | 70.67 | -4.93 | 0.23 | -1.73 | 0.08 | -0.65 | 0.39 | -0.63 | 0.24 |
| Chen | 2012 | 60 | -0.97 | 0.07 | - | - | -1.35 | 0.27 | - | - |
| Chen | 2012 | 83 | -1.08 | 0.06 | - | - | -1.32 | 0.21 | - | - |
| Chernyak | 2010 | 76 | -1.49 | 0.10 | 0.15 | 0.08 | - | - | - | - |
| Chow | 2008 | 75 | -1.18 | 0.06 | -1.30 | 0.06 | - | - | - | - |
| Chrisp | 2012 | 77 | -0.69 | 0.06 | -0.14 | 0.05 | - | - | - | - |
| Coyne | 2011 | 40 | -0.54 | 0.10 | - | - | - | - | - | - |
| De Waal-Andrews | 2012 | 136 | -4.84 | 0.12 | -3.92 | 0.09 | -1.29 | 0.24 | -0.87 | 0.18 |
| De Waal-Andrews | 2012 | 112 | -4.92 | 0.14 | -2.63 | 0.07 | -1.56 | 0.31 | -1.20 | 0.18 |
| DeBono | - | 57 | -1.08 | 0.08 | 0.38 | 0.07 | -1.55 | 0.29 | -0.48 | 0.27 |
| DeBono | - | 81 | -1.41 | 0.06 | 0.09 | 0.05 | -0.33 | 0.21 | 0.24 | 0.19 |
| DeBono | - | 83 | -0.81 | 0.05 | - | - | -0.75 | 0.19 | - | - |
| Dietrich | 2010 | 75 | 1.41 | 0.07 | - | - | - | - | - | - |
| Duclos | 2012 | 59 | -0.62 | 0.07 | - | - | - | - | - | - |
| Eisenberger | 2006 | 48 | -0.15 | 0.08 | -1.21 | 0.10 | - | - | - | - |
| Fayant | - | 60 | -4.00 | 0.20 | -1.36 | 0.08 | 0.22 | 0.38 | -0.44 | 0.28 |
| Floor | 2007 | 88 | -4.13 | 0.14 | -0.66 | 0.05 | -0.21 | 0.28 | -0.59 | 0.19 |
| Gallardo-Pujol | 2012 | 57 | -1.58 | 0.10 | -0.88 | 0.08 | -1.17 | 0.31 | 0.11 | 0.29 |
| Gan | 2012 | 72 | -0.59 | 0.02 | -0.15 | 0.02 | -0.62 | 0.06 | 0.02 | 0.06 |
| Garczynski | 2013 | 83 | -2.31 | 0.12 | 1.01 | 0.08 | -1.29 | 0.33 | -0.01 | 0.29 |
| Geniole | 2011 | 74 | 0.19 | 0.06 | -0.11 | 0.06 | - | - | - | - |
| Gerber | - | 38 | -2.03 | 0.16 | - | - | - | - | - | - |
| Gerber | - | 89 | -3.30 | 0.21 | - | - | - | - | - | - |
| Gonsalkorale | 2007 | 97 | -3.03 | 0.13 | -0.85 | 0.07 | 0.49 | 0.30 | 1.31 | 0.25 |
| Goodwin | 2010 | 300 | -3.78 | 0.04 | -1.43 | 0.02 | 0.20 | 0.08 | -0.43 | 0.07 |
| Goodwin | 2010 | 314 | -0.12 | 0.01 | -0.04 | 0.01 | 0.35 | 0.06 | -0.10 | 0.06 |
| Greitemeyer | 2012 | 56 | -0.47 | 0.07 | -0.23 | 0.07 | - | - | - | - |
| Gruijters | - | 113 | -0.25 | 0.06 | -1.05 | 0.07 | - | - | - | - |
| Hackenbracht | 2013 | 51 | -1.88 | 0.11 | -0.17 | 0.08 | - | - | - | - |
| Hawes | 2012 | 55 | -2.77 | 0.14 | 0.51 | 0.08 | 0.00 | 0.38 | -1.05 | 0.28 |
| Hellmann | - | 76 | -0.61 | 0.06 | -0.32 | 0.05 | -1.40 | 0.22 | 0.74 | 0.21 |
| Hess | 2010 | 162 | -2.33 | 0.04 | -0.86 | 0.03 | - | - | - | - |
| Hess | 2011 | 38 | -0.62 | 0.11 | - | - | - | - | - | - |
| Horn | - | 68 | -0.65 | 0.06 | -0.35 | 0.06 | -0.99 | 0.23 | 1.49 | 0.24 |
| Ijzerman | 2012 | 86 | -2.41 | 0.08 | - | - | -1.07 | 0.22 | - | - |
| Jamieson | 2010 | 33 | -1.50 | 0.16 | -1.02 | 0.14 | - | - | - | - |
| Jamieson | 2010 | 68 | -1.91 | 0.09 | -1.44 | 0.07 | - | - | - | - |
| Johnson | 2010 | 104 | -0.73 | 0.04 | -0.78 | 0.04 | - | - | - | - |
| Kassner | - | 85 | -4.60 | 0.17 | -2.36 | 0.08 | -0.87 | 0.31 | -0.30 | 0.21 |
| Kassner | 2012 | 49 | -2.07 | 0.13 | -1.74 | 0.11 | - | - | - | - |
| Kerr | 2008 | 250 | -1.65 | 0.02 | -0.05 | 0.02 | - | - | - | - |
| Kesting | 2013 | 76 | -0.27 | 0.05 | -0.78 | 0.06 | - | - | - | - |
| Knowles | 2010 | 62 | 0.21 | 0.07 | - | - | -0.99 | 0.25 | - | - |
| Knowles | 2012 | 60 | -0.59 | 0.07 | - | - | - | - | - | - |
| Krijnen | 2008 | 144 | -4.70 | 0.10 | -0.17 | 0.03 | - | - | - | - |
| Krill | 2008 | 119 | -2.09 | 0.05 | -0.56 | 0.03 | - | - | - | - |
| Lakin | 2008 | 36 | -1.48 | 0.14 | -0.49 | 0.11 | - | - | - | - |
| Lau | 2009 | 56 | -5.63 | 0.35 | -3.80 | 0.20 | -0.06 | 0.58 | 1.36 | 0.46 |
| Lustenberger | 2010 | 71 | -0.81 | 0.06 | 0.04 | 0.06 | - | - | - | - |
| Lustenberger | 2010 | 156 | -0.70 | 0.03 | - | - | - | - | - | - |
| MacDonald | 2008 | 63 | -0.15 | 0.06 | - | - | - | - | - | - |
| McDonald | 2012 | 270 | -0.06 | 0.02 | -2.40 | 0.03 | - | - | - | - |
| Nordgren | 2011 | 71 | -0.73 | 0.06 | - | - | - | - | - | - |
| Nordgren | 2011 | 74 | -0.79 | 0.06 | - | - | - | - | - | - |
| Nordgren | 2011 | 46 | -2.18 | 0.14 | - | - | - | - | - | - |
| Nordgren | 2011 | 44.67 | -0.54 | 0.09 | -0.73 | 0.10 | - | - | - | - |
| Nordgren | 2011 | 58.67 | -0.64 | 0.07 | - | - | - | - | - | - |
| Oberleitner | 2012 | 88 | -2.33 | 0.08 | 0.42 | 0.05 | - | - | - | - |
| OBrien | 2012 | 125 | -0.57 | 0.03 | -0.69 | 0.03 | - | - | - | - |
| Peterson | 2011 | 40 | -0.87 | 0.11 | -0.88 | 0.11 | - | - | - | - |
| Pharo | 2011 | 74 | -0.87 | 0.06 | -0.99 | 0.06 | -1.00 | 0.22 | -0.84 | 0.22 |
| Plaisier | 2012 | 149 | -0.87 | 0.03 | -0.20 | 0.03 | -0.40 | 0.11 | -0.56 | 0.11 |
| Ramirez | 2009 | 121 | -2.24 | 0.05 | -1.01 | 0.04 | - | - | - | - |
| Ren | 2012 | 53 | -2.13 | 0.12 | -0.17 | 0.08 | - | - | - | - |
| Renneberg | 2011 | 60 | -8.09 | 0.61 | -1.89 | 0.10 | 1.83 | 0.81 | 0.51 | 0.29 |
| Riva | 2011 | 100 | -2.05 | 0.13 | -1.06 | 0.10 | - | - | - | - |
| Ruggieri | - | 91 | -0.38 | 0.04 | -0.56 | 0.05 | - | - | - | - |
| Ruggieri | - | 74 | -0.49 | 0.06 | 0.25 | 0.05 | -0.31 | 0.24 | -0.68 | 0.23 |
| Sacco | 2011 | 51 | -2.35 | 0.13 | -1.42 | 0.10 | - | - | - | - |
| Sacco | 2011 | 21 | -2.15 | 0.29 | -1.38 | 0.23 | - | - | - | - |
| Sacco | 2011 | 38 | -1.69 | 0.14 | -1.01 | 0.12 | - | - | - | - |
| Salvy | 2010 | 59 | -1.43 | 0.09 | -1.41 | 0.08 | - | - | - | - |
| Salvy | 2009 | 103 | -1.46 | 0.05 | -1.30 | 0.05 | - | - | - | - |
| Schaafsma | 2012 | 720 | -2.55 | 0.01 | -1.32 | 0.01 | 0.09 | 0.03 | 0.33 | 0.03 |
| Segovia | 2012 | 56 | -1.61 | 0.09 | - | - | -1.89 | 0.32 | - | - |
| Staebler | 2011 | 68 | -0.59 | 0.06 | -0.53 | 0.06 | 0.50 | 0.23 | 0.42 | 0.23 |
| Stillman | 2009 | 121 | -0.82 | 0.05 | -1.31 | 0.06 | 0.57 | 0.22 | -1.19 | 0.24 |
| Stock | 2011 | 155 | -1.98 | 0.04 | -0.13 | 0.03 | - | - | - | - |
| Van Beest | 2011 | 87 | -2.16 | 0.07 | -1.15 | 0.05 | -0.40 | 0.24 | -0.44 | 0.19 |
| Van Beest | 2011 | 183 | -4.75 | 0.13 | -1.12 | 0.04 | -0.76 | 0.22 | -0.11 | 0.13 |
| Van Beest | 2006 | 135 | -2.43 | 0.05 | -1.18 | 0.03 | 0.10 | 0.14 | 0.13 | 0.12 |
| Van Beest | 2006 | 111.33 | -4.30 | 0.12 | 0.01 | 0.04 | 0.09 | 0.22 | 0.19 | 0.14 |
| Van Beest | 2012 | 125 | -6.53 | 0.20 | -2.66 | 0.06 | 0.06 | 0.35 | -0.23 | 0.15 |
| Van Beest | 2012 | 85 | -6.56 | 0.30 | 0.04 | 0.05 | -0.28 | 0.44 | 0.07 | 0.18 |
| Van Dijk | - | 51 | -1.47 | 0.10 | -0.04 | 0.08 | - | - | - | - |
| Webb | - | 170 | -1.78 | 0.03 | -0.74 | 0.03 | 0.03 | 0.10 | 0.04 | 0.09 |
| Weik | 2010 | 65 | -0.04 | 0.06 | 0.09 | 0.06 | -0.43 | 0.24 | 0.66 | 0.24 |
| Wesselmann | 2009 | 82 | -2.77 | 0.10 | -4.01 | 0.15 | -1.30 | 0.24 | -0.20 | 0.28 |
| Wesselmann | 2012 | 91 | -1.44 | 0.06 | - | - | - | - | - | - |
| Williams | 2002 | 390 | -0.39 | 0.01 | -2.34 | 0.02 | - | - | - | - |
| Williams | 2000 | 732 | -0.79 | 0.01 | -1.43 | 0.01 | - | - | - | - |
| Williams | 2000 | 111 | -0.71 | 0.04 | -0.91 | 0.04 | -0.20 | 0.15 | -0.98 | 0.15 |
| Wirth | 2009 | 159.33 | -4.86 | 0.10 | -2.02 | 0.04 | 0.05 | 0.17 | 0.46 | 0.11 |
| Wirth | 2010 | 76 | -0.95 | 0.06 | -1.62 | 0.07 | - | - | - | - |
| Zadro | 2004 | 62 | -3.05 | 0.14 | -1.49 | 0.08 | -0.11 | 0.32 | -1.12 | 0.28 |
| Zadro | 2004 | 77 | -3.47 | 0.13 | -1.35 | 0.06 | -0.29 | 0.28 | -0.70 | 0.21 |
| Zadro | 2006 | 56 | -3.63 | 0.19 | -0.86 | 0.08 | - | - | - | - |
| Zhong | 2008 | 52 | -0.68 | 0.16 | - | - | - | - | - | - |
| Zoller | 2010 | 57 | -0.24 | 0.07 | -0.08 | 0.07 | - | - | - | - |
| Zwolinski | 2012 | 56 | -1.97 | 0.11 | -0.28 | 0.07 | - | - | - | - |



*Figure 1.*A dotplot of the average estimated simple effects with 95% confidence intervals.

Appendix

All formulae reported below originate from the chapter by Michael Borenstein (2009). Hedges’ g was calculated as

where d is the standardized main effect. For the standardized interaction effect d was calculated as

where the first term in the nominator is the ostracism effect and the second term is the ostracism effect in the moderator conditions. This Δd corresponds to the partial eta-squared of the interaction. Sampling variance of g was calculated by multiplying the sampling variance of d by the squared correction factor, that is

where the sampling variance of the interaction was calculated as the sum of the sampling variances ofboth the simple main effects.

**Supplementary materials**

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