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Ordinal Effects of Ostracism: A Meta-Analysis of Cyberball Studies

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Abstract

We examined 120 Cyberball studies (N = 11,869) to determine the effect size of ostracism and conditions under which the effect may be reversed, eliminated or small. Our analyses showed that (1) the average ostracism effect is large ($d > |1.4|$) and (2) generalizes across structural aspects (i.e., number of players, ostracism duration, number of tosses, type of needs scale), sampling aspects (i.e., gender, age), and type of dependent measure (i.e., interpersonal, intrapersonal, fundamental needs). Further, we test Williams’s (2009) proposition that the immediate impact of ostracism is resistant to moderation, but that moderation is more likely to be observed in delayed measures. Our findings suggest that (3) first and last measure are susceptible to moderation, and (4) time passed since being ostracized does not predict effect sizes of the last measure. Thus, support for this proposition is tenuous, and we suggest modifications to the temporal need-threat model of ostracism.

Keywords: Cyberball, meta-analysis, ordinal, ostracism

Ordinal Effects of Ostracism: a Meta-Analysis of Cyberball Studies

Cyberball (Williams, Cheung, & Choi, 2000; Williams & Jarvis, 2006) is a virtual ball-tossing game that is used to manipulate the degree of social inclusion or ostracism in social psychological experiments. In this game the participant supposedly plays with two (or more) other participants, who are in fact part of the computer program. The program varies the degree to which the participant is passed the ball (see Figure 1 for a still from the game). Ostracized players are not passed the ball after two initial tosses and thus obtain fewer ball tosses than the other players. Included players are repeatedly passed the ball and obtain an equal number of ball tosses as the other players. Our literature search showed that at least 200 published papers involved the use of the Cyberball paradigm to study ostracism and that over 19,500 participants have played the game thus far. In this paper we provide a meta-analysis of these studies. Our aim was to gauge the typical effect size of being ostracized in the Cyberball game and to see whether this effect is moderated by cross-cutting variables that were hypothesized to reduce/enhance the psychological impact of ostracism, structural aspects that are inherent in Cyberball (e.g., number of players, number of ball tosses), sampling aspects of the studies (e.g., gender composition), the type of dependent variables used (e.g., intrapersonal measures such as need satisfaction or interpersonal measures such as pro- or antisocial behavior), and (4) the ordinal time point of the variable assessment (i.e., first or last).

Historical background

The first article that used Cyberball was published in 2000 and was specifically introduced to study ostracism – i.e., being excluded and ignored (Williams, Cheung, & Choi, 2000). This focus on ostracism makes it a unique paradigm that sets it apart from other paradigms that have been used to study rejection, such as the future life rejection (see Baumeister, Twenge, & Nuss, 2002), the get-acquainted paradigm (Nezlek, Kowalski, Leary, Blevings, & Holgate, 1997), and the autobiographical memory manipulation (i.e., remember a

ORDINAL EFFECTS OF OSTRACISM

time when you were excluded; Craighead, Kimball, & Rehak, 1979). The difference is that participants in Cyberball are not explicitly informed that they are excluded whereas in the other paradigms participants are provided a reason pertaining to why they are excluded. Cyberball participants simply do not obtain a ball and thus need to infer that they are excluded, whereas in the other paradigms, participants are informed that they are excluded in various ways and thus do not need to infer that they are excluded.

The Cyberball manipulation is a suitable method to study how people react to being ignored and excluded. Humans are social animals and care deeply about whether they are included or ostracized by others. Interestingly, ostracism is not only observed among loved ones, but on all levels of human organization. In fact, research suggests that most people are ignored and excluded at least once a day (Nezlek et al., 1997). The social relevance is further evident in that it not only affects the person who is ostracized, but often also others. As a grim example, research on school shootings has suggested a direct link between ostracism and revenge, which does not only affect the people who were responsible, but also innocent bystanders (Leary, Kowalski, Smith, & Phillips, 2003). The impact of ostracism is also evident in research findings using Cyberball. Through experimental work, it has been repeatedly shown that being ostracized has an effect on people—either on their psychological functioning (e.g., decreases in positive mood; Lustenberger & Jagacinski, 2010) or on certain interpersonal behaviors (e.g., increases in social susceptibility or aggressive behaviors; Carter-Sowell, Chen, & Williams, 2008; Van Beest, Carter-Sowell, Van Dijk, & Williams, 2012). These experiments have highlighted the (mostly negative) impact of ostracism on fundamental needs (e.g., belonging; Baumeister, & Leary, 1995), mood, physiology (e.g., body temperature; IJzerman, Galucci, Pouw, Weißgerber, Van Doesum, & Williams, 2012), and various other constructs, including those measured with behavioral measures (e.g.,

ORDINAL EFFECTS OF OSTRACISM

5

conformity, compliance, aggression). In the current paper, we refer to the general effect of being ostracized compared to being included in Cyberball as the *ostracism effect*.

To capture how people respond to ostracism, Williams (2009) proposed a temporal need-threat model of ostracism. Here he suggested three stages of the ostracism effect, namely: (1) a *reflexive* stage, (2) a *reflective* stage, and (3) a *resignation* stage. In the reflexive stage, the response to the ostracism sequence is immediate and occurs like a reflex. This initial response is theorized to be socially painful, threatening (Baumeister & Leary, 1995), and easily detectable due to evolutionary over-sensitivity to cues of ostracism (Haselton, & Buss, 2000). Such a reflex would not take into account situational specifics and provides little room for coping. The reflex is proposed to affect primarily pain, fundamental needs, and emotional reactions (e.g., increased anger and sadness). The affected fundamental needs are belonging, self-esteem, control, and meaningful existence, typically measured by a need satisfaction scale (Williams, 2009). According to Williams, measures of reflexive responses must occur during, or in the case of self-report measures, immediately following Cyberball (with the wording of the questions referring to how participants felt *during the game*). The *reflective* (or delayed) stage, which follows this immediate response, is subject to more rational thought and coping with the threats. Part of such coping is the necessity for fortification of the threatened fundamental needs. Coping can be measured both in terms of speed of recovery (higher levels of need satisfaction approaching the levels of included participants), and emotional, cognitive, and behavioral choices. The *resignation* stage occurs after prolonged ostracism, causing prolonged periods of pain and more fundamental need threat. If one is not able to fortify the fundamental needs, a prolonged ostracism sequence leads to feelings of helplessness, alienation, depression, and unworthiness. Because the resignation stage is hypothesized to occur only after prolonged and repeated exposure to ostracism (as in months or years), it is not feasible (and even unethical) to study resignation responses in laboratory experiments.

Hence, in this paper we limit ourselves to studying the reflexive and reflective stages. For these stages, Williams asserts that moderation and variation of need satisfaction effects by individual differences and socially relevant factors (e.g., type of group from which one is excluded) will be less likely to occur for reflexive measures than for reflective measures.

Goals of meta-analysis

A limited number of Cyberball experiments have been reviewed in other meta-analyses, but these meta-analyses had a different goal than the current meta-analysis. Previous meta-analyses focused on social rejection and not on ostracism (Blackhart et al., 2009; Gerber & Wheeler, 2009), or focused only on a specific dependent variable (e.g., fMRI, Cacioppo et al. 2013; Rotge et al., 2014). Importantly, none of these early meta-analyses were specifically set up to test Cyberball effects only. Consequently, we do not know how structural variables of Cyberball affect the ostracism effect size. Moreover, none of these meta-analyses considered whether it matters if a specific variable is measured first or last. Thus, it remains unclear whether the ostracism effect size decreases or increases over time and whether immediate measures are more or less moderated by cross-cutting variables. The goal of our meta-analysis is to provide a comprehensive understanding of the Cyberball-induced inclusion versus ostracism effect size. Under what conditions, if any, is the effect size negative, zero, or especially small? Under what conditions is it especially large? To answer these questions we made several important selection decisions (see also the Open Science Framework (OSF) where we registered all hypotheses).¹

We decided to focus on the first and last dependent measure of all the included studies of the meta-analysis. The reason for this selection was that it allowed us to gauge whether effects sizes are affected by the time point at which the effects were measured. Moreover, it served as a proxy to evaluate the hypothesis that immediate measures should be less affected by cross-cutting variables than more delayed measures. Additionally, we considered several

ORDINAL EFFECTS OF OSTRACISM

7

potential moderators of the ostracism effect, where we differentiated between structural- and sampling aspects that are inherent in Cyberball and variables that are explicitly inserted by a researcher to assess moderation.

Prior research has not explicitly tested how structural aspects such as the number of players in Cyberball, the total number of ball tosses, or duration of the game may affect the experience of ostracism. We thus did not have concrete hypotheses regarding these effects and explored whether increasing the number of players in Cyberball, increasing the total number of ball tosses, and increasing the total duration would affect experiences of Cyberball players. Prior research does provide some ideas about how sample characteristics such as gender composition, age distribution, and country of origin of the participants in Cyberball may affect effect sizes. Given that social aspects of an interdependent setting may be less evolutionary relevant for males than for females (e.g., Hawes et al., 2012), and more relevant for younger people than older people (e.g., Pharo, Gross, Richardson, & Hayne, 2011), we tested whether proportion of male participants and mean age would predict ostracism effects. Finally, considering that collectivism might influence the degree to which belonging is important (see Hofstede, 1980), we used a crude categorization of continents (i.e., U.S., other western countries, Asian countries, and remaining countries) to test whether a more collectives orientation would be associated with larger ostracism effects. Importantly, because some of these factors may be related, we decided to use a regression approach in which all factors were entered simultaneously. That is, if one of the predictors is significant, this would mean that it is significant given that it is controlled by the other predictors.

In addition to these possibly predictive variables at the study level, we inspected hypothesized cross-cutting factors for the ostracism effect within the studies. Specifically, to inspect whether the ostracism effect could be moderated experimentally, we checked each Cyberball study for a cross-cutting factor, which was expected to moderate the ostracism

ORDINAL EFFECTS OF OSTRACISM

effect. For example, in a 2 (ostracized vs. included) by 2 (in-group vs. out-group) between-subjects design, the ostracism effect could be expected to be larger for the in-group level than for the out-group level. This is only one of many possible moderators. Moderation in these contexts 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).

The last coding decision was that we inspected several types of dependent variables. Overall, the dependent variables included in the meta-analysis were only subject to the criterion that they were expected to be affected by ostracism. In other words, we included multiple types of dependent variables with varying psychometric properties in the primary studies. We considered measures that speak to both how the participant interacts with others (i.e., interpersonal) and how they experience the situation themselves (i.e., intrapersonal). We define interpersonal measures as measures relating to others and intrapersonal measures as measures relating only to the self. Examples of interpersonal measures are donation behavior or aggression. Examples of intrapersonal measures are self-reported anger, self-esteem, control, but also physiological measures such as body temperature or galvanic skin response. Finally, given that most Cyberball studies specifically use *fundamental needs* (i.e., belonging, self-esteem, control and meaningful existence) questionnaires, we also tested these as a separate type of intrapersonal measure (see Van Beest & Williams, 2006; Williams et al., 2000; Zadro, Williams, & Richardson, 2004). This included other measures relating to the fundamental needs, such as the Rosenberg Self-Esteem Scale. Overall, these fundamental needs measures are particularly important for testing Williams’s (2009) prediction concerning moderation of ostracism effects over time. Coding the type of dependent measure allowed us to assess the robustness of the average effect size across different subsets of dependent variables. We also coded whether the first- and last measure were immediate (i.e., variables

ORDINAL EFFECTS OF OSTRACISM

9

relating to *during* the game) or delayed (i.e., variables relating to *after* the game). This ensures model correspondence for the included measures.

Hypotheses

In sum, the hypotheses are subdivided into two primary hypotheses and several secondary hypotheses. The two primary hypotheses were: is there an ordinal decrease of the ostracism effect across time points? (Hypothesis 1), and is there an ordinal difference in the interaction effect across time points (Hypothesis 2)? Secondary hypotheses regarded moderation of the ostracism effect by structural aspects of the studies, sampling aspects of the studies, and different types of dependent measures used. In other words, are the results robust across different subsets that could substantively influence results? These hypotheses will be answered with random and mixed-effects meta-analytic models applied to all 120 studies that we were able to collate.

Method**Study inclusion criteria**

First, we only considered Cyberball experiments that contained a factor that manipulated the number of virtual ball tosses obtained by the participants. For this ostracism factor we only considered the condition in which participants were ostracized by all other participants and the condition in which participants were equally included by all other players. Second, we only considered experiments that incorporated a between-subjects design with random assignment. Within-subject designs were excluded, because most within-subjects designs regard high-dimensional neurophysiological measurements such as fMRI that are beyond the scope of this meta-analysis (see Cacioppo et al., 2013; Rotge et al., 2014). Also, 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.

Third, we also checked whether the experiments contained other factors besides the ostracism factor. These factors were coded as well provided that the primary authors expressed an expectation about the factor. If the authors did not expect that the factor would moderate the effect of ostracism, we collapsed effects sizes across the irrelevant factor. Moreover, continuous variables that were dichotomized into factorial levels were also collapsed 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, when participants were grouped into high- and low neuroticism groups based on a continuous measure of neuroticism (Boyes, & French, 2009), we used pooled means and standard deviations across these two groups, reducing the design to an ostracism/inclusion design.

Fourth, for the dependent measures the criterion was that they were (expected to be) affected by the ostracism manipulation. We considered the measures that immediately followed the manipulation (first measure) and the measure at the end of the study (last measure), while excluding manipulation checks in this assessment.

Reasons for these inclusion criteria are threefold. (1) Most Cyberball experiments take place in such a format, making it an encompassing criterion for the purposes of this meta-analysis. (2) The choice to limit the meta-analysis to between-subject designs rendered computational aspects more feasible based on reported statistics in papers. (3) To eliminate the need for subjective quality assessment of the primary studies, these criteria maximize experimental rigor. Indeed, this focus on specific study designs is presumed to decrease variability due to design characteristics, which increases power for moderator analyses (Hedges & Pigott, 2004).

Literature search

ORDINAL EFFECTS OF OSTRACISM

11

To have a comprehensive meta-analysis of Cyberball studies, we used seven search strategies in the period of November 2012 through April 2013. These search strategies included database searches, a call for data, cross-reference with Kip Williams's online list of Cyberball studies, Google Scholar alerts, citation records, SPSP conference abstracts, and personal communications.

The databases searched included Web of Knowledge, PubMed, ScienceDirect and Worldcat using all sources from the Tilburg University library. The first three cover only published articles, whereas Worldcat also covers books and dissertations as well as the PsycINFO database. 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 additional search term yielded zero relevant hits that were not a result of the other searches and was dropped. . Across all these searches, results included 1927 potentially relevant studies of which a total of 109 were deemed relevant and saved for coding. Within Web of Knowledge, we looked through all citation records of the seminal papers by Williams et al. (2000); Williams and Jarvis (2006). These papers were cited 332 times (as of 5th of November, 2012), of which 43 papers were saved for coding. The entire literature search provided 2259 potentially relevant studies (including possible duplicates across searches), of which 152 were selected to be included in the coding.

The call for data was put on the list servers 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 9 replies, yielding 3 useful studies.

Kip Williams keeps a list of Cyberball studies on his website. This list was used to check for extra articles that did not turn up in the initial searches on November 15th, 2012.² 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 search results for a search term occur and were used for *cyberball* and *ball-tossing*. This yielded 85 search results of which 25 were saved for coding. SPSP conference abstracts from 2006 through 2013 were searched for Cyberball studies. This led to personal communications with the authors of the conference abstracts, leading to additional studies. Pooled, the personal communication and the conference abstracts yielded 21 potentially relevant studies, of which 20 were saved for coding. The seminal paper by Williams et al. (2000) was added separately.

In sum, the literature search spanned 2468 potentially relevant studies, resulting in 205 that were saved for coding. During coding, papers were assessed to fit the inclusion criteria. Of the 205 papers, 107 papers were excluded for a variety of reasons. Several involved the use of a within-subjects design (52 papers). Some papers could not be accessed (5 papers) or could not be included because we did not receive the required data on request (7 papers). Some were excluded for other reasons (43 papers), such as not involving new data (e.g., a dissertation study that was later published). All included papers were published between 2000 (after the introduction of Cyberball) and April 2013. This resulted in a final, fully coded sample of 98 papers containing 120 studies, with mean sample size 98.9 and median sample size 74.³ There were a total of 11,869 Cyberball participants.

Coding procedure

The first author coded all the studies and conducted all the analyses. The third author double-checked a subset of the entire database and the second author double-checked all 52

ORDINAL EFFECTS OF OSTRACISM

13

1 studies that entailed a full two-by-two design. The third author checked and reran the R code
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3 of all analyses. Finally, an extensive account of all coding decisions is publicly available via
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5 Open Science Framework on a paper-by-paper basis (see Footnote 2 for the direct link).
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10 Group means and standard deviations were retrieved for both the first and last relevant
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12 measure in each study for effect size calculation. Relevant measures were defined as
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14 constructs that were expected by primary authors to show an ostracism effect (e.g.,
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16 fundamental needs, mood, pro-social helping behavior, etc.). Coding that was crucial for
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18 testing the primary hypotheses concerned the number of items from the first through last
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20 measure plus any additional time in between (e.g., rest period). This made up the estimation
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22 of time from the first to last measure, where each item was counted as lasting six seconds (the
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24 six-second rule was based on a longstanding practice used to estimate average completion
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26 time in the freshmen testing program of the University of Amsterdam; e.g., Smits, Dolan,
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28 Vorst, Wicherts, & Timmerman, 2011). Any additional time reported in the procedure was
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30 also included. Note that some measures are variable on time and that these were arbitrarily
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32 estimated in a conservative manner to at least take these measures into account at some level
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34 (e.g., grip strength task estimated at one minute).
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38 The type of measure used was coded for in the following general terms: (1)
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40 interpersonal, (2) intrapersonal, and (3) fundamental needs. Interpersonal measures were
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42 defined as measuring constructs that relate to (the self and) others (e.g., *how angry do you feel*
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44 *towards person X?*, donations to charity, etc.). Intrapersonal measures were defined as
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46 measuring constructs that relate only to the self (e.g., *how angry do you feel?*, physiological
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48 measures, etc.). Fundamental needs measures were those that measured self-esteem,
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50 belonging, control, meaningful existence, or were a composite of these. For the analyses of
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52 structural-, sampling-, and measurement aspects of Cyberball, we coded structural
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54 characteristics (i.e., number of players, duration of the game, total number of ball tosses),
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ORDINAL EFFECTS OF OSTRACISM

sample characteristics (i.e., age, gender composition, country of origin), and measure properties (i.e., interpersonal, intrapersonal, fundamental needs), and whether the first- and last measure fit the definition of immediate (i.e., during the game) or delayed (i.e., after the game/now), respectively.

As a consequence of defining relevant dependent measures broadly, we included different kinds of measures that are expected to show different directions of an ostracism effect. For example, when compared to included participants, belongingness scores are expected to be lower for ostracized participants, whereas retaliation scores are expected to be higher for ostracized participants. To counteract computational problems (i.e., cancellation of effects) being caused by this bidirectionality of ostracism effects, we coded the direction of the ostracism effect for each specific measure, such that negative effect sizes depict negative psychological effects. Moreover, in two-by-two designs in which the ostracism effect was crossed with another factor (i.e., a moderator), we coded for expected direction of that moderator. For example, in Table 1, we show hypothetical data for the four study designs that are possible when crossing direction of the effect and direction of the moderation. The relevant effect sizes should be corrected to attain comparable effect sizes across studies. Effect sizes for the simple ostracism effect (column wise) were corrected only for the type of measure. For instance, for panels (a) (involving, e.g., need threat) and (c) (involving, e.g., need satisfaction), the corrections entailed a multiplication with -1 or +1, respectively. Simple moderator effects (row wise comparisons) are interesting for understanding the effect of the moderator under either ostracism or inclusion. These simple moderator effects were corrected for both the type of measure *and* the expected moderation (i.e., exacerbation, -1, or minimization, +1). For example in panel (c), the 5 and 8 on the right are used to compute the *standard ostracism effect* (as in Williams et al., 2000), whereas the 3 and 8 in the left column represent an ostracism effect that is thought to be exacerbated. For example, in a given

ORDINAL EFFECTS OF OSTRACISM

15

ostracism study with a two-by-two design, adolescents are expected to show stronger ostracism effects, compared to young adults (Pharo, et al., 2011). The 5 and 8 would subsequently represent the scores for the young adults, whereas the 3 and 8 would represent the scores for the young adolescents. In panel (d) we depict a study in which the *moderated* column is thought to lead to a minimal ostracism effect, as could be expected when Cyberball is played with members of a despised out-group (Gonsalkorale & Williams, 2007). The margins (greyed out) denote the simple effects, which are after correction comparable across all panels (a) through (d), indicating that this correction did what we intended it to.

Relevant information that was missing in the papers was requested from the authors via e-mail. In case of non-response, we sent three follow-up e-mails. 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 (descriptions of all cases are described in the log file on the OSF).

Statistical analyses

For the analyses, we used the *metafor* package (Viechtbauer, 2010) in the R statistical environment (R Core Team, 2013).

Effect size metric. We used Hedges's *g* version of the standardized mean differences as the effect size. Hedges's *g* corrects for the slightly biased estimate given by Cohen's *d* (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 dependent variable in each experiment. For example, in a 2 (ostracized vs. included) by 2 (moderator present vs. moderator absent) design with multiple measures, we calculated two simple ostracism effects (Hypothesis 1) and two interaction effects (Hypothesis 2). Non-factorial studies delivered only simple effects for the first and last measure and no interactions.

Meta-analytic model. We used random- and mixed-effects models, because heterogeneity in the effect sizes is expected due to both the inclusion of different measures and additional unknown methodological and substantive factors. The meta-regression element in some of the analyses is the variable time as predictor of the ostracism effect. Analyses without this study-level predictor reduce to a random-effects model. We used Restricted Maximum Likelihood (REML) to estimate tau-squared (i.e., the residual variance), as recommended by Viechtbauer (2005). Note that when estimating a mixed- or random effects model, one does not estimate a single *true* effect, but rather the mean and variance of underlying effects (Viechtbauer, 2005).

Statistical sensitivity analyses. To test for robustness of the effects, we incorporated several statistical 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 statistical sensitivity analyses in which these outliers were excluded. Another statistical sensitivity analysis entailed fitting of the mixed-effects model with tau-squared fit at the upper bound value of the 95% confidence interval.

Funnel plot asymmetry. A funnel plot depicts 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 estimated mean

ORDINAL EFFECTS OF OSTRACISM

17

effect size. If there are no methodological or substantive reasons to expect a link between effect sizes and standard errors, funnel plot *asymmetry* can indicate publication bias (e.g., Bakker et al., 2012). To test funnel plot asymmetry, 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 mean effect size, and possibly publication bias.

Results

In our reporting of the effect sizes, d indicates a main effect and Δd indicates an interaction effect. Even though we used Hedges's g , we maintained the notation of d , because g is only a minor correction to Cohen's d . Statistical sensitivity analyses are only reported if they showed different effects (all statistical sensitivity analyses can be found on OSF).

Primary hypotheses

The two primary hypotheses are tested in four meta-analyses, of which the study level effects are reported in Table 2. The table includes effect sizes used in the 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.

Simple ostracism effect (Hypothesis 1). In a random-effects model on the main effect of ostracism ($k = 120$), residual heterogeneity was significant, $Q(119) = 1395, p < .001, I^2 = 92.99\%$ and estimated at $\tau^2 = 0.90, 95\% \text{ CI } [0.70, 1.24]$. The heterogeneity measure τ^2 includes both the estimated proportion of explained variance at the study level and unexplained variance in the distribution of underlying effect sizes (i.e., τ_{res}^2). The analysis yielded an estimated average effect of $d = -1.36, p < .001, 95\% \text{ CI } [-1.54, -1.18]$. A random-effects version of the Egger's test (Sterne & Egger, 2005) indicated funnel plot asymmetry, $Z = -6.14, p < .001$. Due to the size of the average effect, and hence large power to acquire

ORDINAL EFFECTS OF OSTRACISM

significant outcomes in primary studies, we do not suspect publication bias to explain this asymmetry. In other words, immediately after being ostracized, the average ostracism effect is estimated at -1.36 standard deviation units, which entails a large effect (Cohen, 1988).

Next, we fitted a mixed-effects regression model for the ostracism effect on the last measure ($k = 95$), including estimated time in seconds since completing the Cyberball game as predictor. Residual heterogeneity was significant, $Q_E(93) = 803, p < .001$ and estimated at $\tau_{res}^2 = 0.38, 95\% \text{ CI } [0.27, 0.54]$. The intercept was estimated at $d_{intercept} = -0.76, p < .001, 95\% \text{ CI } [-0.91, -0.61]$. Moreover, the estimated time in seconds between exclusion in Cyberball and the moment at which the last measure was taken failed to moderate the average effect, $b = 0.0001, p = .187, 95\% \text{ CI } [-0.0001, 0.0003]$. However, 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 (i.e., including both the time and SE as predictor) showed no funnel plot asymmetry, $Z = -0.72, p = .474$. In short, long after ostracism has occurred ($M_{time} = 291.2$ seconds), ostracized participants on average scored around -0.73 standard deviation units lower when compared with included participants, an effect that does not appear to be moderated further by time passed since the ostracism occurrence.

Thus, results show a clear effect of ostracism on both the first and last measures, of which the latter is *not* predicted by our operationalization of time. The ostracism effect over time can also be inspected via confidence intervals. Comparing the 95% confidence intervals for the average ostracism effect on the first measure (i.e., $[-1.54, -1.18]$) and on the last measure (i.e., $[-0.86, -0.59]$) showed no 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), the mean difference is sizeable and CIs confirms our prediction that the average ostracism effect is smaller for the

ORDINAL EFFECTS OF OSTRACISM

19

last measure.. In fact, given the expected positive correlation between effects for first and last measures, the comparison of CIs is likely to be conservative (Schenker & Gentleman, 2001). Additionally, we noted that estimated residual heterogeneity was larger on the first- than on the last measure. We conclude that the average ostracism effects decreases from the first- to last measures, and that study-level effects are more similar on the last measure.

Moderation of ostracism (Hypothesis 2). To test moderation of the ostracism effect, we selected the factorial experiments that manipulated ostracism and another independent variable in between-subjects designs. A random-effects model on the interaction effect (Δd) on the first measure ($k = 52$) showed heterogeneity in underlying effects, $Q(51) = 103.24, p < .001, I^2 = 50.60\%$ and an estimated $\tau^2 = 0.19$, 95% CI [0.07, 0.41]. The average interaction effect equaled $\Delta d = -0.46, p < .001$, 95% CI [-0.64, -0.28], indicating a change in the ostracism effect due to the moderator level and vice versa (i.e., moderation of the ostracism effect). There was indication of funnel plot asymmetry in this analysis, $Z = -2.43, p = .015$. Thus, the data indicate that, across the board, the ostracism effect *can* be moderated on the first measure following the ostracism sequence, but it is possible that publication bias may have affected the interaction estimates.

On the last measure ($k = 46$), the mixed-effects model (with estimated time as predictor) for the interaction effect again showed residual heterogeneity, $Q_E(44) = 100.82, p < .001$ and estimated $\tau_{res}^2 = 0.21$, 95% CI [0.10, 0.55]. The intercept of the interaction effect was estimated at $\Delta d_{intercept} = -0.20, p = .052$, 95% CI [-0.402, 0.002] and no significant moderation of time was found, $b = 0.0002, p = .159$, 95% CI [-0.0001, 0.0004]. The regression test with the time and SE as predictors showed no funnel plot asymmetry, $Z = -0.68, p = .495$. These results indicate that moderation of the average ostracism effect is *not* found at a later time point in the included studies, and time itself does not moderate the computed interaction effects. However, statistical sensitivity analyses showed that this interaction *was* significant

when we removed three outliers based on studentized residuals, $\Delta d_{intercept} = -0.32, p = .029$, 95% CI [-0.60, -0.03], whereas the regression coefficient time continued to be non-significant, $b = 0.0002, p = .207$, 95% CI [-0.0001, 0.0006]. On the last measure, this indicates that the non-significant interaction effect is sensitive to outliers in the data.

To see whether the interaction effects changed from the first to the last measure, we again compared confidence intervals. On the first measure, the 95% CI was [-0.64, -0.28] whereas for the last measure, the 95% CI was [-0.32, 0.05]. Considering the overlap of these CIs, one needs to be careful to interpret this as a reduction in the moderation across the measures examined. It is clear, however, that the average effect size of the interaction does not increase from first to last measure.

Secondary Hypotheses

In addition to the simple effects over all studies, we analyzed subsets of studies that differ in type of dependent measure to study robustness of the effects. We also inspected whether sample composition, scale composition, and Cyberball specifics could predict the estimated effect size. Finally, we selected a homogeneous subset of studies to come to grips with the relatively large heterogeneity of simple main effects found for the primary hypotheses.

Measures. To inspect the robustness of the estimates of the first and last measure, we studied simple effects across several subsets of measures. These subsets encompassed interpersonal measures (i.e., measures that relate to others or the self in the context of others), intrapersonal measures (i.e., measures that relate only to the self), fundamental needs (single- and composite needs), and 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; shown in Figure 2 as *model*). We ran the analyses for the different measures for the two time points separately (i.e., first and last measure).

ORDINAL EFFECTS OF OSTRACISM

21

The different panels in Figure 2 show the results for the different simple effects per subset and overall; Table 3 summarizes the estimated interaction effects. A comparison of the results within each panel shows whether the overall results are robust and representative of all subsets, or whether there are nuances per type of measure. The main differences are notable in panels (1), (2) and (5). The first and second panels indicate that the effect of ostracism is weaker for interpersonal measures, compared to all intrapersonal measures (including fundamental needs). This indicates that in a similar factorial design, interpersonal measures show weaker effects than intrapersonal measures. Panel 5 indicates that the moderation of interpersonal measures is stronger compared to the other subsets. This suggests that interpersonal measures are more subject to moderation, whereas the effects of ostracism on interpersonal measures are smaller initially. Additionally, for the specific subset of fundamental needs, we noted that the point estimated interactions (Table 3) follow the pattern predicted by the need-threat model (Williams, 2009): i.e., first measures are moderated less strongly than the last measures.⁴

Composition. We ran mixed-effects models on the ostracism effect (as in Hypothesis 1) inspecting for composition effects, on both the first and the last measure. The predictors in the mixed effects model were (1) country (US, other Western country, Asian, other), (2) proportion of males in the study, (3) mean age of the sample, (4) number of players in the game, (5) length of the game (≤ 5 min, 5-10 min or > 10 min), (6) the number of throws in the game and (7) type of needs scale referenced (by assigning unique values for every unique reference).

On the first measure, this model ($k = 45$) showed clear residual heterogeneity after controlling for these structural- and sampling aspects of the studies, $Q_E(32) = 449.52$, $p < .001$, estimated $\tau_{\text{res}}^2 = 0.90$, 95% CI [0.54, 1.59], but no overall moderation, $Q_M(11) = 10.75$, $p = .465$. The different types of need scales (e.g., Van Beest & Williams, 2006; Williams,

2009; Zadro et al., 2004) did not significantly moderate effect sizes, showing psychometric convergence among the three scales. Inspecting the predictors individually also showed no indication for moderation ($ps > .137$; see Table 4).

On the last measure ($k = 41$), no overall moderation was found, $Q_M(12) = 6.00, p = .873$, but the number of players in the game did significantly predict the effects, $b = 1.55, p = .047$, 95% CI [0.2; 3.07], which would be interpreted as four players eliciting smaller ostracism effects, when compared to three players. The significance of this individual predictor should be interpreted carefully, as the omnibus moderation test showed no systematic decrease in heterogeneity. Overall, we found no strong evidence for moderation due to study or sample composition.⁵

Homogeneity? The analysis of the simple ostracism effect on the first measure showed that differences of underlying effects made up 93% of the variability in study outcomes. We performed an additional secondary analysis in a more homogenous subset of studies to better understand this heterogeneity. This subset only included typical Cyberball studies that involved three players in the game, 30 throws, and lasted less than five minutes. In addition, the homogeneous subset of typical Cyberball studies only involved measures of immediate fundamental needs (single or composite). Performing a meta-analysis on this homogeneous subset of 19 studies showed an I^2 value of 83%, indicating that 83% of the total variability can be attributed to heterogeneity in the effect sizes. We noted that the mean simple ostracism effect in these 19 studies was relatively strong and estimated at $d = -2.05$, 95% CI [-2.44, -1.65]. In other words, given that the heterogeneity remains large even in a homogeneous subset, suggests that the heterogeneity found in the overall analyses does not appear to be an artifact from the inclusion of different measures and the use of alternative Cyberball setups.

Discussion

ORDINAL EFFECTS OF OSTRACISM

23

In this meta-analysis of Cyberball studies we estimated the average ostracism effect of the first and last dependent variable used in 120 Cyberball experiments. The primary hypotheses were (a) that the ostracism effect size would decrease from first to last measure and (b) that first measures would be less affected by cross-cutting variables than last measures. The secondary hypotheses tested whether the above generalizes across structural variables of the game, sample characteristics, or type of dependent variable used.

The results confirmed the first primary hypothesis that the ostracism effect decreased from the first ($d = -1.36$) to the last measure ($d = -.76$). Results showed that this difference was not predicted by our estimation of duration between first and last measure. The analyses also showed that the heterogeneity of this effect was larger on the first measure ($\tau^2 = 0.90$) than on the last measure ($\tau^2 = 0.38$) as depicted in Figure 3.

The results did not directly support the second primary hypothesis that last measures were more strongly moderated than first measures. That is, results showed that cross-cutting variables moderated both the first and last measure. Moreover, average estimated interaction effect sizes actually decreased in size from first ($\Delta d = -.46$) to last ($\Delta d = -.19$), although confidence intervals of these estimates overlap. To help interpret these interactions, we reported simple effect size estimates in Figure 2. These results show that for both first and last measures, the *overall* ostracism effect operates similarly at both levels of the cross-cutting moderator variable. Moreover, both (simple) ostracism effects are relatively large. In addition, when we compared the mean effects of the moderator variable *within* the two possible levels of ostracism factor in the primary studies (i.e., ostracized or include), results indicate (cf. Figure 2) a mean relatively weak *positive* effect within the ostracism level and a mean relatively weak *negative* effect within the inclusion level. To further explain the implication of the findings it may be fruitful to consider an example in which participants are ostracized or included by either an outgroup or an ingroup. In such a setting, our findings would thus

suggest that the relative effect of ostracism compared to inclusion (i.e., the ostracism effect), is similar for both outgroup *and* ingroup conditions. Moreover, if one compares the effect of group status (outgroup vs. ingroup), one would predict that those ostracized by outgroup members would slightly benefit whereas those included by ingroup members would slightly be harmed. Taken together, these contrasts support the robustness of the ostracism effect.⁶

Structural Aspects of Cyberball and Different Dependent Variables

For the secondary hypotheses we considered several structural elements of the Cyberball game and aspects of the sample and analyzed whether these reliably predicted the found effect sizes.

Does gender of participants matter? Previous research provided evidence for a difference in the ostracism effect across genders (e.g., Hawes et al., 2012). Our results indicated that, contrary to this, proportions of males and females did not significantly predict the mean effect size. In our coded studies, the mean proportion of males was approximately 39% (observed range: 0-100%).

Does age of participants matter? Whereas previous research has indicated increased sensitivity to ostracism in younger age groups (Pharo, Gross, Richardson, & Hayne, 2011), we failed to find moderation of ostracism effects by mean age of the study samples. Coded studies had a mean sample age ranging from 10 through 32.5 years, with an average of approximately 20.5 years. This indicates that most of the research with Cyberball has been done on young adults, with relatively few or no studies investigating children, middle-aged participants, or senior citizens. More research could focus on specific (individual-level) age moderation of ostracism.

Does culture or country matter? We found no indication that culture predicted the average effect size. In our coded studies, approximately 52% were from the United States, 45% from other Western countries (e.g., Australia, the Netherlands, Germany) and 3% from

ORDINAL EFFECTS OF OSTRACISM

25

Asian countries. Our analyses used the United States as reference category. We note that the low prevalence of Asian countries might cause a lack of power, and that we cannot definitively state there is no difference between Western and Asian responses to ostracism. We can state that there is no systematic difference in the ostracism response for Western countries and the United States.

Does number of players matter? In the studies included in this meta-analysis, approximately 89% of the studies used the three-player version of Cyberball and 11% used the four-player version of Cyberball. Average ostracism effects differed between these subsets, with smaller predicted effects in the four-player setting, but we are hesitant to interpret this due to a nonsignificant omnibus test for the predictive model (see ‘Composition’ in the results section). Preferably, this moderator of the ostracism effect in Cyberball should be subject to further work in which the number of players is experimentally varied.

Does number of throws or length of the study matter? We also considered the length of Cyberball in two ways. We coded the number of ball tosses and estimated the length of the study. Of the coded studies, 60% used 30 throws, 11% used 40 throws, 8% used 20 throws, 4% used 60 throws, and 2% for both 15 and 24 throws. Other categories ranging from 10 through 200 make up the remaining percentages, each making up 1%. Only 2 out of 120 studies were estimated to last longer than 5 minutes. Our results indicated the mean ostracism effect was *not* reliably predicted to be different across different lengths of the study or the different number of total throws in the omnibus test. The single meta-regression on ball tosses suggested it may predict the effect size of the first measure. As above, we are hesitant to interpret this, but do note that increasing ball tosses may be more associated with a diffused ostracism effect than with an increased ostracism effect.

Does type of dependent variable matter? Secondary analyses also showed that the majority of the results were robust across subsets of dependent measures and the overall set of

dependent measures (see Figure 2). Exceptions were interpersonal measures showing relatively weaker ostracism effects on the first measure when compared to the other subsets. This suggests that psychological effects of ostracism are large, but that this effect might be smaller for interpersonal behaviors. On top of this, interpersonal measures also show more moderation, suggesting that interpersonal behaviors caused by ostracism are more easily moderated by cross-cutting factors. Additionally, we estimated interactions for the measure subsets interpersonal (i.e., measures relating to others), intrapersonal (measures relating to the self), fundamental needs, model (i.e., first measure is reflexive and last measure is reflective) and an overlap of the latter two subsets. For all but two, these subsets showed that measures taken at the first time point were moderated more strongly than the measures taken last. Finally, the analyses including only fundamental needs showed that moderation was larger at the last time point, when compared to the first time point. This result is crucial, as Williams (2009) specifically predicted this pattern for fundamental needs. Our overall test of ordinal effects of moderation that included many different types of measures failed to show the predicted pattern. Hence, we can reliably state there are interactions on both time points, but cannot make any general conclusions as to how they relate, as results are crucially dependent on the measures taken.

Williams’s Model of Ostracism: Supported or Not?

Regarding the test of Williams’s (2009) model, there are several important observations and limitations. First, Williams proposed fundamental need threat as a result of even a brief episode of ostracism. This was supported by the meta-analysis. The model asserts that negative emotional reactions (i.e., sadness and anger) are also induced by ostracism, and this proposition was contested by Blackhart et al.’s (2009) meta-analysis in which they argued for affective numbness. We did not explicitly test this in the present analysis. That moderation is predicted to occur in the reflective stage, when the context and meaning of the ostracism

ORDINAL EFFECTS OF OSTRACISM

27

event can be appraised, yielded some support in the present meta-analysis. The final stage of Williams's model—resignation—is outside the aims of the present meta-analysis, because it requires long-term exposure to ostracism. Thus, most propositions set forth in Williams's model that were tested within this meta-analysis, were supported.

The proposition that appears to lack support from this meta-analysis is that reflexive reactions to ostracism are more resistant to moderation than reflective reactions. Across the board, our results indicate there is more moderation of ostracism effects on the first time point than on the last time point. However, there are two limitations to this conclusion. Firstly, Williams specifically refers to physiological, online, or immediate retrospective reports to assess reflexive reactions. In many instances in this meta-analysis, the first reaction is not isomorphic with reflexive measures. Anything taken after the game, or assessed by wording indicating present state (rather than the participants' state during the game), is not assumed to be reflexive, nor predicted to be resistant to moderation. Secondly, Williams's proposition is restricted to fundamental needs only. Indeed, our specific analyses involving only studies that employed measures of immediate and delayed fundamental need satisfaction corroborated the model prediction that there is more moderation on the last time point, than on the first time point (but see Footnote 4).

Because of this quantitative difference in moderation across measures, we encourage direct testing of this time difference in moderation as predicted by Williams (2009), just as the study by Bernstein and Claypool (2012) was a direct, experimental test of a finding by Gerber and Wheeler (2009). However, the mean size of the interaction effect in our meta-analysis was quite small, raising power issues for future studies. Using our estimated interaction effects to determine sample size under a power of .8, a sample size of 2186 would be necessary to have sufficient power on both time points.⁷ Note that the mean sample size in full factorial designs in our meta-analysis is 110, showing that the mean power in these studies is .08 to detect an

interaction at the last time point (notably, power for the standard ostracism effect is highly sufficient in the included studies, due to the large effect). A large Mechanical Turk study is feasible and could provide the sample needed. Additional ways of increasing power are by reducing error on the measurements by using validated psychometric scales.

Changes to the need-threat model of ostracism. As a result of our findings, we suggest that the temporal need-threat model of ostracism should be modified. Firstly, it should be recognized that there is potential for moderation in the reflexive stage, where immediate measures of impact tap into participants’ reactions during the game. If factors can reduce physical pain and distress, like for instance acetaminophen (DeWall et al, 2010)⁸ or transcranial magnetic stimulation (Riva et al., 2012), or if certain populations are less likely to feel pain (e.g., those higher in schizotypal personality disorder; Wirth et al, 2010; see Lautenbacher & Krieg, 1994), then we would also expect moderation of immediate measures of distress. Secondly, our results may suggest important issues related to the timing of measuring ostracism effects by way of the ordinal differences. Specifically, time passed after the ostracism episode occurred is likely to affect the extent immediate distress measures will be subject to moderation. For example, if researchers wait long enough before administering the immediate need satisfaction measures (e.g. “playing the game made me feel insecure”), it becomes more likely that all participants will have recovered from the negative impact of ostracism, thus resulting in a homogeneous (and highly satisfied) between-group result. Thus, differences in recovery from ostracism based upon social-situational factors and/or personality differences, if any, occur somewhere between initial pain and final recovery. It is difficult to predict exactly when that time period is. Zadro et al. (2006) report delayed recovery by those high in social anxiety 45-minutes later. Other studies show full recovery within 5-10 minutes. Future research needs to examine the time course more carefully, to determine if and when moderation occurs in delayed measures.

Limitations

Within the current meta-analysis there are several limitations. 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 possibly yield low response rates (Wicherts, Borsboom, Kats, & Molenaar, 2006), lowering the sample size of the meta-analysis overall.⁹ This lack of direct testing was thus chosen as a way of retaining sample size within the meta-analysis.

Second, we focused only on the first and last measure of the primary studies and consequently did not use other measures of the primary studies. 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 a sufficiently large set of studies and comprehensiveness. Another reason for only including the first and last measures was that every additional 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 a statistical correction due to correlations between measures, to facilitate repeated-measures analyses to minimize Type I error rates. In other words, our decision to limit our analysis to first and last prevents the problem of multiple testing and nonresponse to data requests, which would lead to a smaller set of useful studies and hence less powerful analyses.

Third, random (non-systematic) heterogeneity in the effect sizes poses a problem for the power of finding 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 subset of typical Cyberball studies still showed substantial variability in the effect sizes: $I^2 = 83\%$. This indicates that the effects are quite variable to begin with, and makes it unlikely that the effects are misrepresented.

Additionally, the specific null-effect of our estimation of time as a predictor of the ostracism effect could be due to one of three reasons. First, the (random) heterogeneity in the effect sizes may have been too large to find moderation by time. This cannot be counteracted in the current dataset and remains a limitation. Second, imprecise reporting of the measures in the papers led to inaccurate time estimations. To counteract this imprecise reporting of 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). Third, the difference in the effect sizes between time points was not due to time, but due to differences in dependent measures administered at the different time points. This alternative explanation can be addressed by creating a difference index in which the difference in dependent measures at the first and second time point are inspected by creating a difference index (i.e., coded value on first measure minus coded value on last measure) and regressing the index on the observed effect sizes in a meta-regression. Doing this for the standard ostracism effect on the last measure, showed no significant predictive effect of this difference ($b = -0.03, p = .531$), indicating that the difference in estimated effects is not driven by difference in measures on the first and last time point. 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). Inspecting whether the types of measures used across all studies are different, and

ORDINAL EFFECTS OF OSTRACISM

31

not the difference within a study, shows that these are similarly distributed across time points (maximum discrepancy of 4.9 percentage points). Substantive differences in proportions of measures across time points are minimal, and form an unlikely driving force for our findings. In sum, we conclude that the findings are not an artifact of selecting the first and last measures.

Fourth, the current meta-analysis only examined between-subjects designs. Possibly ostracism effects in between- and within-subjects designs differ, which is 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 et al. (2013) and Rotge et al. (2014) 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. Additionally, the fact that we only included between-subjects designs did not allow for inspection of the moderating effects of personality, as these are continuous measures that should be modeled linearly instead of in a factorial design, meta-analysis

Fifth, this paper only summarized the results of the measures included in the studies. However obvious this might be, it should be pointed out, because the validity of the conclusions are reliant on the validity of the measures. Most prominently represented in the current meta-analysis are the fundamental need measures, which have no proper psychometric validation up-to-date, notwithstanding their wide use. Other kinds of included measures possibly also lack proper validation, and one has been openly criticized (e.g., the Hot Sauce aggression paradigm; Ritter & Eslea, 2005). We note that results in this paper are conditional on that these measures are valid.

Conclusion

Our meta-analysis of 120 Cyberball studies extends the temporal need-threat model of ostracism. We observed that the average effect size approaches 1.5 standard deviations and that this average effect size is not affected by the composition of the sample used (i.e., age, gender, country of origin) nor by structural aspects of the game (i.e., number of ball tosses, duration, players). We also observed that findings are relatively robust across the typical dependent variables that are used in Cyberball and that the overall effect size decreases from first to last measure. Importantly, we also observed that first measures can be moderated by cross-cutting variables and that only fundamental needs measures show stronger moderation for the last measures as opposed to the first measure taken in the studies. The moderation analyses by cross-cutting variables also revealed that the interaction effects sizes are considerably smaller than the direct inclusion vs. ostracism effect size. This revealed that the typical Cyberball study has enough power to detect main effects, but should substantially increase sample size to study theoretically relevant interactions. Intriguingly, we also observed that effect sizes were rather heterogeneous even when we limited our analysis to a very homogenous subset of studies. This indicates that there are potentially relevant moderators that have yet not been discovered. We invite fellow researchers to reanalyze our data (osf.io/ht25n) and test new hypotheses, and to further expand our knowledge of ostracism with Cyberball.

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39

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Footnotes

1. The direct link: <https://osf.io/ht25n/>
2. It has been updated since, but the list that was used can be found on the Open Science Framework, see Footnote 1.
3. Oaten, Williams, Jones and Zadro (2008) was applicable, but was excluded due to being an outlier with respect to effect size ($ds > 15$). See also 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.”
4. Because fundamental needs showed effects in the theorized direction, we explored this further by overlapping the subset of fundamental need measures with the model definition of immediate and delayed (i.e., whether the measures related to feelings during or after the Cyberball game). Estimated interactions for this selection were $\Delta d = -0.37$, 95% CI [-0.60, -0.14] ($k = 29$) and $\Delta d = -0.13$, 95% CI [-0.53, 0.27] ($k = 8$) for the first and last measure, respectively. So in this particular subset of studies that use immediate or delayed fundamental needs measures, results are not in line with Williams’s (2009) prediction. The reported fundamental need selection can be specified even further to only include studies that explicitly focus on composite need satisfaction as typically defined by Kip Williams. Such a selection again provides support for the hypothesis that immediate fundamental need satisfaction is less moderated, $\Delta d = -0.18$, 95% CI [-0.47, -0.11] ($k = 15$), than delayed need satisfaction, $\Delta d = -0.93$, 95% CI [-1.67, -0.19] ($k = 3$). Note, however, that such a selection is based on 3 studies for delayed measures.
5. We also conducted individual meta-regressions for each of the structural- and sampling variables. These individual analyses yield similar results as the overall

ORDINAL EFFECTS OF OSTRACISM

41

analyses. We again observed that four players are less hurt by ostracism than three players ($b = .84$, $SE = .28$, $p = .003$) on the last measure. What is new is that we also observed that number of ball tosses affected the effect size ($b = .02$, $SE = .01$, $p = .046$) on the first measure. This showed that increasing the number of ball tosses decreases the negative impact of ostracism. Taken together this suggests that the impact of ostracism is diffused when it is the result of more players and more ball tosses compared to fewer players and fewer balls tosses.

6. It is important that the simple effects in Figure 2 are averaged over studies, thus potentially subject to Simpson's paradox.
7. We used G*Power 3.1.7 to calculate this between-subjects interaction effect (F -test, fixed effects, .8 power); with $k = 4$ and the smaller interaction (last time point; numerator $df = k - 1$). The effect size Δd was transformed in to f by means of $\sqrt{[d^2/(2k)]}$, resulting in $f = .0707$.
8. DeWall et al. (2007) was not included in the meta-analysis, because we were not able to retrieve all information.
9. Note that out of the 72 data requests, we received timely replies of 52 (i.e., ~72%). However, these requests were only for specific information and not for raw datasets, as was the case in Wicherts et al. (2006).

ORDINAL EFFECTS OF OSTRACISM

42

Table 1

Hypothetical data example of coding correction

(a) Negative moderator, negative measure					(b) Positive moderator, negative measure						
		Moderated	Not-moderated/control	Raw	Correct			Moderated	Not-moderated/control	Raw	Correct
Ostracism factor	Ostracism	13	11	2	2	Ostracism factor	Ostracism	9	11	-2	2
	Inclusion	8	8	0	0		Inclusion	8	8	0	0
	Raw	5	3				Raw	1	3		
	Correct	-5	-3				Correct	-1	-3		
(c) Negative moderator, positive measure					(d) Positive moderator, positive measure						
		Moderated	Not-moderated/control	Raw	Correct			Moderated	Not-moderated/control	Raw	Correct
Ostracism factor	Ostracism	3	5	-2	2	Ostracism factor	Ostracism	7	5	2	2
	Inclusion	8	8	0	0		Inclusion	8	8	0	0
	Raw	-5	-3				Raw	-1	-3		
	Correct	-5	-3				Correct	-1	-3		

ORDINAL EFFECTS OF OSTRACISM

43

Note. Raw denotes the simple effect in the hypothetical data before correction whereas correct denotes the simple effect after correction. Column wise effects are multiplied by the type of measure only, whereas column wise effects are multiplied by both the type of moderator and type of measure.

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ORDINAL EFFECTS OF OSTRACISM

Table 2

Effect sizes per study for the primary hypotheses

First author	Year	N	d T1	(SE)	d T2	(SE)	Δd T1	(SE)	Δd T2	(SE)
Alvares	2010	74	-1.21	0.12	-0.10	0.10	-0.15	0.24	1.12	0.23
Ambrosini	2013	40	-1.69	0.13	-0.97	0.11	-	-	-	-
Aydin	2012	68	-0.95	0.13	-0.40	0.12	-1.19	0.24	0.72	0.23
Banki	2012	89	-1.87	0.07	-0.35	0.05	-	-	-	-
Bastian	2010	72	-2.75	0.11	-1.42	0.07	-	-	-	-
Bernstein	2012	24	-0.41	0.16	-	-	-	-	-	-
Bernstein	2012	25.50	-1.04	0.17	-	-	-	-	-	-
Bernstein	2010	73	-1.63	0.16	-1.63	0.16	-0.86	0.37	-1.11	0.40
Bernstein	2010	138	-2.67	0.10	-1.96	0.08	-0.53	0.22	-0.51	0.17
Bernstein	2012	67	-2.00	0.17	-0.99	0.13	-1.07	0.45	-0.80	0.30
Bernstein	2012	27	-1.39	0.17	-	-	-	-	-	-
Boyes	2009	89	-0.43	0.05	-0.80	0.05	-	-	-	-
Boyes	2009	87	-0.20	0.05	-0.84	0.05	-	-	-	-
Brochu	-	35	-2.51	0.20	-0.48	0.11	-	-	-	-
Brown	2009	52	-0.64	0.08	-	-	-	-	-	-
Carter	2008	143	-0.28	0.06	0.20	0.06	0.34	0.11	0.17	0.11
Carter-Sowell	2008	65	-2.86	0.12	-1.48	0.08	-	-	-	-
Carter-Sowell	2010	74	-1.60	0.14	-1.49	0.13	-1.23	0.33	-1.15	0.34
Carter-Sowell	2010	70.67	-2.09	0.17	-0.56	0.11	-0.65	0.39	-0.63	0.24
Chen	2012	60	-1.04	0.14	-	-	-1.35	0.27	-	-
Chen	2012	83	-1.32	0.11	-	-	-1.32	0.21	-	-
Chernyak	2010	76	-1.52	0.10	0.15	0.08	-	-	-	-
Chow	2008	75	-1.20	0.06	-1.31	0.06	-	-	-	-
Chrisp	2012	77	-0.70	0.06	-0.15	0.05	-	-	-	-

ORDINAL EFFECTS OF OSTRACISM

45

First author	Year	<i>N</i>	<i>d</i> T1	(<i>SE</i>)	<i>d</i> T2	(<i>SE</i>)	Δd T1	(<i>SE</i>)	Δd T2	(<i>SE</i>)
Coyne	2011	40	-0.56	0.10	-	-	-	-	-	-
De Waal-Andrews	2012	136	-3.55	0.16	-2.55	0.11	-1.29	0.24	-0.87	0.18
De Waal-Andrews	2012	112	-4.21	0.22	-2.17	0.11	-1.56	0.31	-1.20	0.18
DeBono	-	57	-1.07	0.15	-0.05	0.13	-1.55	0.29	-0.48	0.27
DeBono	-	81	-1.07	0.11	-0.10	0.09	-0.33	0.21	0.24	0.19
DeBono	-	83	-0.13	0.09	-	-	-0.75	0.19	-	-
Dietrich	2010	75	1.43	0.07	-	-	-	-	-	-
Duclos	2012	59	-0.63	0.07	-	-	-	-	-	-
Eisenberger	2006	48	-0.15	0.08	-1.24	0.10	-	-	-	-
Fayant	-	60	-2.04	0.20	-1.12	0.15	0.22	0.38	-0.44	0.28
Floor	2007	88	-1.92	0.13	-0.73	0.09	-0.21	0.28	-0.59	0.19
Gallardo-Pujol	2012	57	-1.18	0.16	-0.52	0.15	-1.17	0.31	0.11	0.29
Gan	2012	72	-0.54	0.03	-0.07	0.03	-0.62	0.06	0.02	0.06
Garczynski	2013	83	-1.51	0.19	0.39	0.15	-1.29	0.33	-0.01	0.29
Geniole	2011	74	0.19	0.06	-0.11	0.06	-	-	-	-
Gerber	-	38	-2.09	0.16	-	-	-	-	-	-
Gerber	-	89	-3.38	0.21	-	-	-	-	-	-
Gonsalkorale	2007	97	-1.31	0.14	0.26	0.12	0.49	0.30	1.31	0.25
Goodwin	2010	300	-1.81	0.04	-0.94	0.03	0.20	0.08	-0.43	0.07
Goodwin	2010	314	0.13	0.02	-0.09	0.02	0.35	0.06	-0.10	0.06
Greitemeyer	2012	56	-0.48	0.07	-0.23	0.07	-	-	-	-
Gruijters	-	113	-0.26	0.06	-1.07	0.07	-	-	-	-
Hackenbracht	2013	51	-1.92	0.11	-0.18	0.08	-	-	-	-
Hawes	2012	55	-2.16	0.23	0.69	0.15	0.00	0.38	-1.05	0.28
Hellmann	-	76	-1.21	0.12	0.19	0.10	-1.40	0.22	0.74	0.21
Hess	2010	162	-2.34	0.04	-0.87	0.03	-	-	-	-
Hess	2011	38	-0.64	0.11	-	-	-	-	-	-
Horn	-	68	-0.77	0.12	-0.99	0.13	-0.99	0.23	1.49	0.24
IJzerman	2012	86	-1.67	0.12	-	-	-1.07	0.22	-	-

ORDINAL EFFECTS OF OSTRACISM

First author	Year	<i>N</i>	<i>d</i> T1	(<i>SE</i>)	<i>d</i> T2	(<i>SE</i>)	Δd T1	(<i>SE</i>)	Δd T2	(<i>SE</i>)
Jamieson	2010	33	-1.56	0.15	-1.06	0.13	-	-	-	-
Jamieson	2010	68	-1.94	0.09	-1.47	0.07	-	-	-	-
Johnson	2010	104	-0.73	0.04	-0.79	0.04	-	-	-	-
Kassner	-	85	-1.72	0.13	-1.02	0.11	-0.87	0.31	-0.30	0.21
Kassner	2012	49	-2.11	0.12	-1.78	0.11	-	-	-	-
Kerr	2008	250	-1.66	0.02	-0.05	0.02	-	-	-	-
Kesting	2013	76	-0.28	0.05	-0.79	0.06	-	-	-	-
Knowles	2010	62	-0.38	0.12	-	-	-0.99	0.25	-	-
Knowles	2012	60	-0.60	0.07	-	-	-	-	-	-
Krijnen	2008	144	-4.74	0.11	-0.18	0.03	-	-	-	-
Krill	2008	119	-2.11	0.05	-0.57	0.03	-	-	-	-
Lakin	2008	36	-1.53	0.14	-0.51	0.11	-	-	-	-
Lau	2009	56	-2.50	0.23	-1.09	0.15	-0.06	0.58	1.36	0.46
Lustenberger	2010	71	-0.83	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	-0.74	0.06	-	-	-	-	-	-
Nordgren	2011	74	-0.80	0.06	-	-	-	-	-	-
Nordgren	2011	46	-2.24	0.14	-	-	-	-	-	-
Nordgren	2011	44.67	-0.55	0.09	-0.75	0.09	-	-	-	-
Nordgren	2011	58.67	-0.65	0.07	-	-	-	-	-	-
Oberleitner	2012	88	-2.36	0.08	0.42	0.05	-	-	-	-
O'Brien	2012	125	-0.58	0.03	-0.69	0.03	-	-	-	-
Peterson	2011	40	-0.89	0.11	-0.91	0.11	-	-	-	-
Pharo	2011	74	-1.33	0.13	-0.58	0.11	-1.01	0.30	-0.84	0.23
Plaisier	2012	149	-0.36	0.05	0.23	0.05	-0.40	0.11	-0.56	0.11
Ramirez	2009	121	-2.26	0.05	-1.02	0.04	-	-	-	-
Ren	2012	53	-2.18	0.12	-0.17	0.07	-	-	-	-

ORDINAL EFFECTS OF OSTRACISM

47

First author	Year	<i>N</i>	<i>d</i> T1	(<i>SE</i>)	<i>d</i> T2	(<i>SE</i>)	Δd T1	(<i>SE</i>)	Δd T2	(<i>SE</i>)
Renneberg	2011	60	-1.46	0.16	-1.30	0.15	0.47	0.29	0.51	0.29
Riva	2011	100	-2.10	0.13	-1.09	0.09	-	-	-	-
Ruggieri	-	91	-0.39	0.04	-0.57	0.05	-	-	-	-
Ruggieri	-	74	-0.06	0.13	-0.23	0.13	-0.31	0.24	-0.68	0.23
Sacco	2011	51	-2.40	0.13	-1.45	0.10	-	-	-	-
Sacco	2011	21	-2.28	0.29	-1.46	0.22	-	-	-	-
Sacco	2011	38	-1.74	0.14	-1.04	0.11	-	-	-	-
Salvy	2010	59	-1.45	0.08	-1.43	0.08	-	-	-	-
Salvy	2009	103	-1.48	0.05	-1.31	0.05	-	-	-	-
Schaafsma	2012	720	-1.42	0.02	-0.49	0.02	0.09	0.03	0.33	0.03
Segovia	2012	56	0.14	0.13	-	-	-1.89	0.32	-	-
Staebler	2011	68	-0.79	0.12	-0.05	0.12	0.50	0.23	0.42	0.23
Stillman	2009	121	-0.74	0.15	-1.13	0.16	0.57	0.22	-1.19	0.24
Stock	2011	155	-2.00	0.04	-0.13	0.03	-	-	-	-
Van Beest	2011	87	-0.94	0.10	-0.58	0.09	-0.40	0.24	-0.44	0.19
Van Beest	2011	183	-2.64	0.13	-0.50	0.07	-0.76	0.22	-0.11	0.13
Van Beest	2006	135	-1.29	0.07	-0.65	0.06	-0.10	0.14	-0.13	0.12
Van Beest	2006	111.33	-2.11	0.11	0.09	0.07	-0.09	0.22	-0.19	0.14
Van Beest	2012	125	-2.68	0.11	-1.24	0.07	0.06	0.35	-0.23	0.15
Van Beest	2012	85	-3.10	0.20	0.05	0.09	-0.28	0.44	0.07	0.18
Van Beest	2013	49	-3.97	0.24	-1.32	0.10	-	-	-	-
Van Beest	2013	91	-3.17	0.20	-0.48	0.09	0.75	0.56	0.53	0.18
Van Dijk	-	51	-1.50	0.10	-0.04	0.08	-	-	-	-
Webb	-	170	-0.91	0.05	-0.38	0.05	0.03	0.10	0.04	0.09
Weik	2010	65	0.16	0.12	-0.22	0.12	-0.43	0.24	0.66	0.24
Wesselmann	2009	82	-0.71	0.10	-2.03	0.14	-1.30	0.24	-0.20	0.28
Wesselmann	2012	91	-1.46	0.06	-	-	-	-	-	-
Williams	2002	390	-0.39	0.01	-2.35	0.02	-	-	-	-
Williams	2000	732	-0.79	0.01	-1.44	0.01	-	-	-	-

ORDINAL EFFECTS OF OSTRACISM

First author	Year	<i>N</i>	<i>d</i> T1	(<i>SE</i>)	<i>d</i> T2	(<i>SE</i>)	Δd T1	(<i>SE</i>)	Δd T2	(<i>SE</i>)
Williams	2000	111	-0.26	0.06	-1.01	0.07	-0.20	0.15	-0.98	0.15
Wirth	2009	159.33	-2.29	0.08	-0.76	0.05	0.05	0.17	0.46	0.11
Wirth	2010	76	-0.96	0.06	-1.64	0.07	-	-	-	-
Zadro	2004	62	-1.63	0.16	-0.19	0.12	-0.11	0.32	-1.12	0.28
Zadro	2004	77	-1.75	0.14	-0.33	0.10	-0.29	0.28	-0.70	0.21
Zadro	2006	56	-3.70	0.19	-0.87	0.08	-	-	-	-
Zhong	2008	52	-0.72	0.15	-	-	-	-	-	-
Zoller	2010	57	-0.24	0.07	-0.09	0.07	-	-	-	-
Zwolinski	2012	56	-2.01	0.11	-0.28	0.07	-	-	-	-

Note. *d* T1 refers to ostracism effect on first measure; *d* T2 refers to ostracism effect on last measure; Δd represent interactions. Non-integer *N*s arise from division of full sample *N* for included conditions, appropriate due to random assignment.

ORDINAL EFFECTS OF OSTRACISM

49

Table 3

Interaction effect per subset

							95% CI	95% CI
		<i>k</i>	Estimate	(SE)	Z-value	<i>p</i> -value	Lowerbound	Upperbound
Overall	T1	52	-0.46	0.09	-5.08	< .001	-0.64	-0.28
	T2	46	-0.19	0.11	-1.82	.069	-0.40	0.02
Fundamental	T1	30	-0.39	0.12	-3.42	< .001	-0.62	-0.17
	T2	17	-0.77	0.25	-3.05	.002	-1.27	-0.28
Intrapersonal	T1	42	-0.31	0.09	-3.38	< .001	-0.49	-0.13
	T2	39	-0.21	0.11	-1.87	.062	-0.44	0.01
Interpersonal	T1	10	-1.03	0.18	-5.69	<.0001	-1.38	-0.67
	T1 _{listwise}	6	-0.36	0.22	-1.63	.104	-0.79	0.07
	T2	6	0.63	0.62	1.02	.309	-0.58	1.84
Model	T1	36	-0.29	0.10	-2.99	.003	-0.48	-0.10
	T2	23	0.01	0.17	0.08	.938	-0.31	0.34

Note. Overall estimates are based on all data, where the rest form subsets. Model indicates that the first measure was indeed reflexive and the last measure reflective. Listwise deletion for equal *ks* across time points within a subset yielded highly similar results, except for interpersonal measures, which is depicted in the row labeled T1_{listwise}.

ORDINAL EFFECTS OF OSTRACISM 50

Table 4

Meta regression coefficients for composition effects (first measure; $k = 45$)

	Estimate	(SE)	Z-value	p-value	95% CI Lowerbound	95% CI Upperbound
Intercept	-2.14	3.27	-1.89	0.058	-4.35	0.07
<i>Structural</i>						
Nr. of players	-0.22	1.05	-0.21	0.837	-2.28	1.85
Nr. of throws	0.03	0.02	1.49	0.137	-0.01	0.07
Ostracism <5 min	-	-	-	-	-	-
Ostracism 5-10 min	0.75	0.81	0.92	0.358	-0.84	2.34
Need scale =	-	-	-	-	-	-
Williams (2000)						
Need scale = Zadro	-0.36	0.41	-0.88	0.381	-1.16	0.45
et al. (2004)						
Need scale = Van						
Beest & Williams	0.07	0.54	0.13	0.894	-0.98	1.12
(2006)						
Need scale =	-0.03	0.62	-0.04	0.965	-1.25	1.19
Williams Zadro						
Need scale =						
Gonsalkorale &	0.68	0.82	0.82	0.414	-0.94	2.30
Williams (2007)						
<i>Sampling</i>						
Country = US	-	-	-	-	-	-
Country = Western	-0.42	0.36	-1.15	0.249	-1.13	0.29
Country = Asian	-0.30	1.13	-0.26	0.793	-2.51	1.92
Proportion male	1.54	1.09	1.42	0.156	-0.59	3.68
Mean age	-0.05	0.05	-0.97	0.332	-0.16	0.05

Note. This can be interpreted as a standard regression formula. Empty rows represent reference categories.

ORDINAL EFFECTS OF OSTRACISM

51

Table 5

Meta-regression coefficients for composition effects (last measure; k = 41)

	Estimate	(SE)	Z-value	p-value	95% CI	
					Lowerbound	Upperbound
Intercept	-1.12	0.92	-1.21	0.227	-2.95	-0.70
<i>Structural</i>						
Nr. of players	1.55	0.78	1.98	0.047	0.02	3.07
Nr. of throws	0.01	0.02	0.59	0.556	-0.02	0.04
Ostracism <5 min	-	-	-	-	-	-
Ostracism 5-10 min	0.38	0.62	0.61	0.539	-0.83	1.59
Need scale = Williams (2000)	-	-	-	-	-	-
Need scale = Zadro et al. (2004)	-0.14	0.32	-0.44	0.658	-0.77	0.49
Need scale = Van Beest & Williams (2006)	-0.21	0.41	-0.51	0.613	-1.02	0.60
Need scale = Williams Zadro	-0.12	0.53	-0.22	0.826	-1.16	0.92
Need scale = Gonsalkorale & Williams (2007)	-0.07	0.65	-0.10	0.916	-1.33	1.20
<i>Sampling</i>						
Country = US	-	-	-	-	-	-
Country = Western	0.26	0.30	0.87	0.387	-0.33	0.86
Country = Asian	0.85	0.84	1.01	0.313	-0.80	2.49
Proportion male	0.29	0.83	0.35	0.730	-1.34	1.91
Mean age	-0.01	0.04	-0.25	0.806	-0.10	0.08

Note. This can be interpreted as a standard regression formula. Empty rows represent reference categories.

ORDINAL EFFECTS OF OSTRACISM

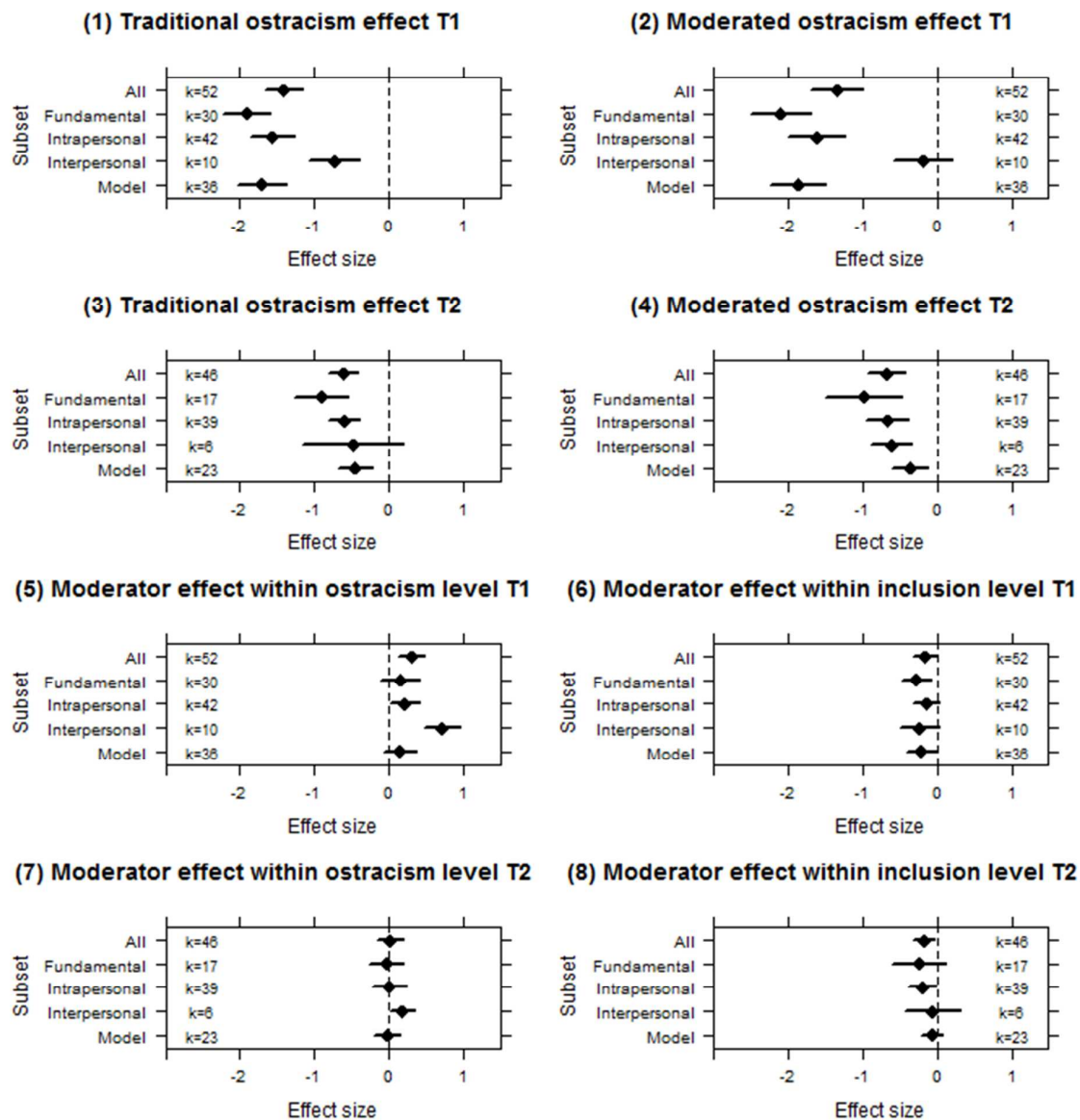
Figure 1. A screenshot of the Cyberball game.



ORDINAL EFFECTS OF OSTRACISM

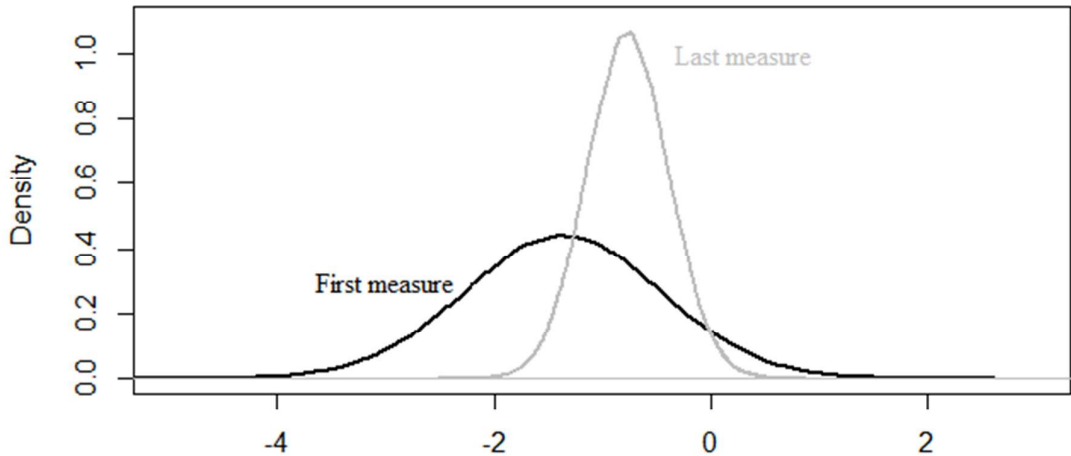
53

Figure 2. Dotplots of the average estimated simple effects with 95% confidence intervals, where T1 represents first measure, and T2 represents last measure. These effects are across the same subset. Traditional ostracism effect refers to the between-subjects effect of being ostracized with *no* moderator present, whereas moderated ostracism effect refers to being ostracized *with* a moderator present. Vice versa, moderator effect within ostracism/inclusion level refers to the between-subjects effect of the moderator factor, within the ostracized/inclusion conditions. All = all measures; Fundamental = only fundamental need measures; Intrapersonal = all intrapersonal measures; interpersonal = all interpersonal measures; model = first is immediate and last is delayed. For lists of studies in each subset, see Supplementary Materials.



ORDINAL EFFECTS OF OSTRACISM

Figure 3. Simulated effects under the model estimates for the standard ostracism effect, showing higher estimated heterogeneity on the first measure than last measure.



Appendix

All formulae reported below originate from the chapter by Michael Borenstein (2009).

Hedges' g was calculated as

$$g = d \left(1 - \frac{3}{4df_w - 9} \right)$$

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

$$\Delta d = \frac{(\bar{X}_{11} - \bar{X}_{12}) - (\bar{X}_{21} - \bar{X}_{22})}{s_p}$$

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

$$s_g = \left(1 - \frac{3}{4df_w - 9} \right)^2 s_d$$

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