Expecting Exclusion:

Does Bracing for the Worst Buffer the Pain of Social Exclusion?

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Abstract

Evidence from different research areas suggests that expecting negative outcomes can buffer their adverse psychological effects. In the context of social exclusion, however, evidence for buffering effects of expectations on individuals' immediate need threat is mixed and has not been examined in terms of *cognitive bracing*. We present four studies (N = 1159) that test two competing hypotheses (no buffering versus buffering effects) and focus on three explanations that may account for the previous mixed findings. Study 1 provides support for buffering effects. However, Studies 2, 3 and 4 do not replicate these effects. An integrative data analysis across the four studies using equivalence tests suggests no meaningful differences in need threat after exclusion. These results suggest that expectations alone may not suffice to buffer immediate need threat or negative affect after exclusion, and illuminate how prior seemingly contradictory evidence may align well. Conceptual and practical implications are discussed.

Keywords: Social Exclusion, Ostracism, Expectations, Bracing for the Worst, Buffering of Need Threat

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Imagine that in your second week of a new job, you are surprised to learn that your colleagues are planning a joint social activity after work, but nobody has invited you. Now imagine that you have already expected that something like this may happen, since your new colleagues have acted rather reserved towards you since day one. With or without this expectation, you are socially excluded, which may hurt – but intuitively you might think that the exclusion experience may be less painful when you have already expected it. Put differently, one might assume that the negative feelings after being excluded will vary depending on whether one has mentally braced for their occurrence, rather than being taken by surprise.

Empirical support from different fields of research holds that *bracing for the worst,* that is, mentally preparing for negative outcomes, can have beneficial affective consequences (Carroll et al., 2006; Taylor & Shepperd, 1998). However, prior research has uncovered mixed evidence as to whether this tenet holds true in the specific context of social exclusion, too. Some findings suggest that expectations may buffer the immediate threat evoked by social exclusion (e.g., Gerber & Wheeler, 2014; Wirth et al., 2017). In contrast, other research has documented similar responses to exclusion regardless of prior expectations (e.g., Rudert & Greifeneder, 2016).

In the following, we review past literature that speaks against versus in favor of possible buffering effects of expecting social exclusion, and elaborate on potential reasons for these contradicting findings. The present research then investigates whether expectations can buffer the immediate need threat following social exclusion by helping individuals to cognitively brace for the upcoming exclusion.

Exclusion Hurts Regardless of Expectations

The temporal need threat model of ostracism or social exclusion holds that individuals possess an inherent ostracism detection system that immediately reacts to the slightest cues of social exclusion (Williams, 2009). Just as humans experience physical pain to alert them that their well-being is endangered, the ostracism detection system triggers social pain to alert individuals that their status of belonging is endangered (Spoor & Williams, 2007). Amongst other reactions, this social pain is reflected in a threat to individuals' basic needs of belonging, control, self-esteem, and meaningful-existence. Importantly, as this immediate reaction occurs fast and by default, it is often assumed to be non-deliberate. Thus, even though situational and personal factors influence reactions to social exclusion in later stages (Hales, 2016; Timeo et al., 2019), their influence is conceptualized as less pronounced for immediate responses (Spoor & Williams, 2007; Williams, 2009).

Following this reasoning, whether or not individuals expect exclusion should not influence their immediate reaction toward exclusion. Since any form of exclusion threatens one's inclusion status, the ostracism detection system should sound the alarm by triggering social pain. Consistent with this perspective, Rudert and Greifeneder (2016) argue that mere expectations of exclusion are not sufficient to buffer or eliminate the adverse effects of exclusion. They demonstrate that even when individuals are in a highly competitive context and thus may expect to be excluded, exclusion is similarly painful as in a cooperative context where exclusion comes as a surprise. Importantly, when individuals endorse the competitive norm themselves, exclusion is substantially less painful compared to when they do not endorse the competitive norm, suggesting that social norms rather than expectations buffer the adverse effect of exclusion. As a consequence, in many situations, expected exclusion may be less painful than unexpected exclusion, but not due to the expectation per se, but because expected exclusion is also norm-consistent. Moreover, these results suggest that inclusion usually represents the predominant social norm and individuals react strongly to its violation.

Other research indicates that even when individuals are ostensibly excluded by members of a despised political outgroup who are aware of the participant's opposite political affiliation, they report similar levels of need threat as individuals who are excluded by members of their political ingroup (Fayant et al., 2014; Gonsalkorale & Williams, 2007). This suggests that even exclusion that has been somewhat foreseeable due to group membership hurts to the same extent as unexpected exclusion. Moreover, other research suggests that even when individuals may anticipate exclusion because it has already occurred repeatedly in the past, the adverse effects of social exclusion do not diminish with repetition (Büttner et al., in press).

In sum, one perspective holds that as long as exclusion represents a violation of the inclusion norm, it is perceived as threatening and activates the ostracism detection system.

Evidence for Buffering Effects of Expectations

In contrast to the perspective outlined so far, the combination of different strands of research on physical and social pain warrants a different prediction. Pain overlap theory suggests strong similarities between physical and social pain (Eisenberger, 2012; MacDonald & Leary, 2005; Spoor & Williams, 2007); just as touching a flame always provokes physical pain, social exclusion should always provoke social pain, explaining the robust negative reactions to exclusion (Williams, 2007). Given that some research suggests that physical pain varies as a function of expectations (e.g., Carlsson et al., 2006), one could speculate that expectations modulate social pain, too. In particular, physical pain is perceived as less painful when it is predictable compared to unpredictable (Carlsson et al., 2006; Miller, 1981), presumably because anticipating an event evokes specific neural activity that buffers pain reactions (Carlsson et al., 2006). Moreover, certain and uncertain expectations of pain activate different neural patterns, and only certain expectations reduce the intensity of experienced physical pain (Ploghaus et al., 2003). This highlights the importance of predictability and certainty for the pain reducing effect of expectations: Expecting that pain may occur is not sufficient to buffer pain reactions, rather it is necessary to know when and how it will occur (Carlsson et al., 2006; Ploghaus et al., 2003). Together, these findings allow for the prediction that social pain may vary as a function of expectations, too.

Expectations have also been shown to alter affective reactions in domains other than physical pain. For instance, participants who had expected having a disease reported better mood after being diagnosed with the disease than those who had not expected it (Shepperd & McNulty, 2002). Thus, expecting a negative outcome could, at least to some extent, buffer the negative reaction to the negative outcome.

Consistent with this conceptual perspective, some research suggests that expectations may buffer the negative impact of social exclusion. Wirth et al. (2017) asked participants to answer some personal questions which ostensibly were shown to other participants. Participants were then informed whether the alleged other participants liked them or not, allowing to establish expectations about future interactions. After this social cue manipulation, participants' level of need satisfaction was measured for the first time. Importantly, the social cues themselves threatened need satisfaction, even before exclusion was manipulated ($M = 2.90$ after exclusionary cues vs. $M = 3.49$ after inclusionary cues). Participants were then informed whether the other participants would want to collaborate with them (inclusion) or not (exclusion). Individuals' need satisfaction was then measured again. Comparing the first and second need threat measurement, the decrease in need satisfaction (i.e., increase in need *threat*) was smaller for those who expected exclusion compared to those who did not. This may suggest that expectations buffer the adverse effects of exclusion on individuals' reflexive needs.

A study by Gerber & Wheeler (2014) is further consistent with a buffering perspective. In this study, individuals were informed how frequently participants were excluded by their co-players in exclusion manipulations such as Cyberball, an online ball-tossing game (Williams & Jarvis, 2006). Individuals were assigned to one out of four conditions where they either received no information at all, or they received the information that exclusion occurs *nearly always*, *about half the time,* or *rarely*. As results show, exclusion threatened individuals' needs less if it was expected due to prior information on the general frequency of exclusion in Cyberball.

Summing up this second perspective, independent lines of research from different domains suggest that expectations may hold the potential to buffer immediate social pain reactions. Again, some empirical research is in support, which, however, may also be accounted for by alternative explanations.

Potential Explanations for Past Mixed Findings

The reviewed findings on expected exclusion provide a mixed picture, with some pointing to no effects and others pointing to buffering effects of expectations on need threat. We offer three different explanations that may account for this mixed picture.

One potential explanation holds that only some, but not all expectations have the potential to buffer need threat, because buffering effects depend on the *strength* of expectations. As expectations are beliefs about the likelihood of future events, like other beliefs, they can vary in their strength. Individuals have stronger expectations when they are more certain (e.g., because it is very likely), and weaker expectations when they are less certain (e.g., because it is less likely) that a future event will occur (Olson et al., 1996). Accordingly, individuals can have rather strong expectations of being excluded in the future when they believe exclusion is very likely to occur, but they can also have comparably weaker expectations of being excluded when they believe future exclusion is less likely to occur. Analogous to physical pain where buffering effects were observed for predictable, but not unpredictable events (Carlsson et al., 2006), expectations may therefore only buffer social pain when individuals expect it with a high likelihood. From this perspective, only strong expectations, but not weak expectations may be associated with buffering of need threat. These conjectures are consistent with evidence reported by Gerber & Wheeler (2014), where individuals' needs were less threatened the more exclusion was expected. That is, descriptively, participants who moderately versus strongly expected exclusion (i.e., exclusion occurs about half the time versus nearly always) experienced more need threat. However, pairwise comparisons between the conditions were not significant, which might be due to comparably low statistical power.

A second potential explanation for previous inconsistent findings holds that previous studies were not always fit to dissociate expectations from social norms. For instance, Gerber & Wheeler (2014, pp. 26) note that "the expectation manipulation influenced both personal expectations of exclusion and also normative expectations of inclusion. As such, expectations in our context include both personal and normative components". From this perspective, it is conceivable that buffering effects occurred in some studies (but not others) because exclusion was not only expected but also perceived as norm-consistent (see also Rudert & Greifeneder, 2016). One way to investigate this explanation is to test whether buffering effects of expectations can be replicated when removing the confounding influence of social norms.

Finally, a third potential explanation for the heterogeneity in prior findings puts emphasis on differences in baseline levels of need threat prior to the exclusion event. In particular, for buffering effects to show, the expectation manipulation itself must not have threatened needs to such an extent that, at the time of social exclusion, further increase is unlikely. Otherwise, a lack of significant change in need threat may be attributable either to expectations or the inability of threats to become even stronger. One way to make sure that exclusion can threaten needs above and beyond the expectation itself is to allow individuals to recover after the expectation manipulation.

The Present Research

Previous research provides a mixed picture on buffering effects of expectations in the context of exclusion, thus leaving an unsatisfactory state of knowledge. Moreover, several explanations may account for these differences, but have not been systematically addressed. The present research aims at closing this conceptual and empirical gap.

In the spirit of research that reports buffering effects of expectations on negative affect as a result of cognitive bracing (e.g., Shepperd & McNulty, 2002), we define expectations as beliefs about the likelihood of future events (Olson et al., 1996). Following this definition, buffering effects should occur as a result of the belief that exclusion will occur, since this belief allows for cognitive bracing. The present research tests whether expected exclusion changes need threat compared to unexpected exclusion. To that aim, four studies confront participants with the likelihood of future exclusion. In all studies, a recovery period after the expectation but prior to the exclusion manipulation is implemented to restore needs to a level where further threat is possible. With this recovery period in place, buffering effects that occur as a result of cognitive bracing for the exclusion event can be distinguished from a situation where the expectation manipulation itself has already threatened needs to such an extent that further increase in need threat appears unlikely.

Study 1 was designed to test if a strong expectation can buffer need threat following exclusion. Studies 2 and 3 aimed at replicating Study 1's results and test whether buffering effects on need threat following exclusion occur as a function of the strength of expectations. Finally, Study 4 tests whether the prior findings on expectations generalize to paradigms other than Cyberball and other dependent measures. Moreover, Study 4 was designed to rule out the possibility that the recovery period implemented in all four studies would distract participants from the likelihood of future exclusion, and further allows to conceptually differentiate expectations from norms.

All studies have been approved by the institutional review board of the authors' university and conform to recognized ethical standards. Code and data for all analyses can be found on the OSF: https://osf.io/q6ry5/. All analyses were conducted using RStudio (Version 1.2.5042).

For all studies, we determined a smallest effect size of interest which served as a basis for power calculations and allows to perform equivalence test in case of non-significant null hypothesis tests (Lakens, 2017, 2018). This smallest effect size of interest was set to Cohen's $d = 0.4$ for Studies 1 to 3, and Cohen's $d = 0.3$ for Study 4 (thus affording an even more conservative test). These effect sizes are smaller than typical effect sizes of expectation effects (e.g., Shepperd & McNulty, 2002) and the difference between expected and unexpected exclusion in the study by Gerber and Wheeler ($d = 0.74$, 2014).

To put this choice of smallest effect size in further perspective, it is important to note the generally large main effect of exclusion (meta-analytic $d > 1.4$, Hartgerink et al., 2015). Compared to this large main effect, potential buffering effects of expectations that are smaller than $d = 0.4$ or even $d = 0.3$ appear negligible, because there would still remain a substantial main effect of exclusion.

We stress that significant equivalence tests do not allow for the conclusion that no differences between expected and unexpected exclusion exist. However, equivalence testing allows to determine whether effects at least as large as the smallest effect size of interest can be rejected (Lakens et al., 2018). Throughout this paper, whenever we speak of the absence of buffering effects of expectations, we refer to the absence of effects larger than the smallest effect size of interest.

Study 1

Study 1 tests whether or not buffering effects of exclusion occur. In contrast to prior research (e.g. Rudert & Greifeneder, 2016; Wirth et al., 2017), we induce expectations directly by informing individuals about the likelihood that they will be excluded in the later Cyberball Game. Also, we introduce a temporal delay between the information about the future exclusion and the actual exclusion experience to allow for recovery from potential affective reactions to the exclusion information.

Since effect sizes are difficult to predict given that prior research yielded mixed results, we opted for a sequential testing approach (Lakens, 2014; Lakens et al., 2021). This approach allows to analyze data before the a priori determined sample size is collected and to stop data collection earlier if a significant effect is observed. The alpha-level is adjusted at each look to control for risk of Type 1 errors (see Lakens et al., 2021). As elaborated, we determined a smallest effect size of interest ($d = 0.4$) based on statistical effects we deem to be *non-substantial* and planned to perform equivalence tests (Lakens, 2017), allowing us to determine whether we can reject effects larger than the a-priori specified effect size.

Hypotheses

Previous findings provide support for two competing hypotheses:

H1.0: Individuals who expected to be excluded *do not* report less reflexive need threat following an exclusion experience than individuals who did not expect to be excluded (effect size smaller than Cohen's $d = 0.4$).

H1.1: Individuals who expected to be excluded report less reflexive need threat following an exclusion experience than individuals who did not expect to be excluded (effect size equal to or larger than Cohen's $d = 0.4$).

Methods

The study procedure was preregistered on the OSF: https://osf.io/r57pv

Sequential Sampling Procedure

Power analysis for sequential designs using the R-package rpact (Wassmer & Pahlke, 2022) indicates that for a two-sided *t*-test with an alpha level of .05, a desired statistical power of .90, and three equally spaced looks using a Pocock type alpha spending function, a total of 305 participants is needed.

We planned to perform two interim analyses with equally spaced looks at information rates of 33.3 % ($N = 102$) and 66.7% ($N = 204$). Due to the unpredictable nature of data collection, such as an unknown number of participants that need to be excluded from analysis due to the preregistered exclusion criteria, it is not possible to perform the interim analyses at precisely these information rates. We thus planned to adjust the two-sided local significance levels based on the actual number of observations at each look. In case of a p-value lower than the two-sided local significance level at an interim analysis, data collection is terminated. In case of a non-significant *t*-test at the last look, we pre-registered to conduct an equivalence test. This allows to test whether effects at least as large as $d = 0.4$ (Smallest Effect Size of Interest) and at least as small as $d = -0.4$ can be rejected.

As a result of the pre-registered Sequential Sampling Procedure, data collection was terminated after the first look ($N = 102$).

Participants and Design

Participants (> 18 years old UK residents fluent in English) were recruited via Prolific Academic. They were asked to participate only if they had never played Cyberball before.

In accordance with the outlined sequential sampling procedure, data from 102 participants (33.3% of the planned sample size) was collected before the first look. Participants' data was excluded from analysis if they indicated an insufficient level of seriousness (< 6 on a 9-point scale), self-excluded from data analysis, indicated having played Cyberball before or indicated having been familiar with Cyberball prior to the study. Applying these criteria, 93 participants (77.77% female, 1 non-binary, 1 did not indicate their gender, $M_{age} = 32.90$, $SD = 10.80$) remained. The study design had two between-subjects conditions: *expected exclusion* vs. *unexpected exclusion*.

Materials and Procedure

Participants were told that they would take part in a study on social interactions and mental visualization. After providing informed consent, participants were randomly assigned to one of two conditions. In both conditions, participants were told that they later will play Cyberball (Williams et al., 2000), an interactive ball tossing game with two computer-generated players and 30 ball throws in total.

Manipulation of Expectations. In the *unexpected exclusion* condition, participants did not receive any further information. Prior research strongly suggests that without further information, participants should expect to be included and to receive a fair share of throws (Rudert & Greifeneder, 2016).

In the *expected exclusion* condition, participants additionally received the information that they would be excluded from the game with a very high probability: *"At the beginning of the game, you will receive two ball throws from your co-players. However, after these two throws at the beginning of the game, there is a very high probability (more than 95%) that your co-players will exclude you from the game so that you won't receive any more ball throws."*

All participants were told that in a fair game with three players, every player would receive 10 ball throws and were then asked to indicate the number of ball throws they expected to receive in the later game.

Filler Task. After the manipulation, participants completed a filler task to allow participants in the expected exclusion condition to emotionally recover from the information that they will most likely be excluded in the later game. As detailed in the introduction, we implemented the filler task so that participants across conditions do not differ in need threat *prior* to playing Cyberball. In the filler task, participants were asked to practice their mental visualization by choosing from 20 pairs of photos the one photo they liked better (photos were neutral, i.e., 3.5 to 4.5 on a scale from 1 = very negative to 7 = very positive, and evoked low arousal, i.e., > 2.5 on a scale from 1 = very low arousal to 7 = very high arousal; see Kurdi et al., 2017).

Baseline Need Threat. Following the filler task, participants indicated their current level of need threat and mood (*baseline* measure) to assess whether there are substantial differences between the conditions prior to Cyberball. Participants answered four items on a 9-point semantic differential scale assessing the four fundamental needs belonging (*rejected* to *accepted),* self-esteem (*devalued to valued*), control (*powerless* to *powerful*), and meaningful existence (*invisible* to *recognized;* Rudert & Greifeneder, 2016). Given high internal consistency ($\alpha = .86$), we averaged the four items into one composite score. The scale was recoded so that higher values reflect lower need satisfaction and thus stronger need threat. One additional item assessed participants' mood (*good* to *bad).*

Cyberball. After the baseline measure of need threat, all participants played Cyberball (Williams et al., 2000). In the game, the participant is depicted as one of three figures who are tossing a ball to each other. In both conditions, participants only received the ball twice at the beginning of the game and then were excluded from the ball tossing.

Reflexive Need Threat. After Cyberball, participants were again asked to indicate their levels of need threat (Cronbach's $\alpha = .89$) and their mood, this time with respect to how they felt during the game (*reflexive* measure). Since mood and need threat were highly correlated ($r = -.81$ at baseline and $r = -.56$ after Cyberball, $p < .001$) and mood followed a similar pattern as need threat, analyses of mood are reported online (see OSF).

All participants were asked how actively they had participated in the game and how many throws they had actually received. Additionally, only participants in the expected exclusion condition were asked to what extent they had believed that they would actually be excluded by their co-players.

End of survey. After providing demographic information, participants were asked if they had responded seriously throughout the study, if there was a reason not to use their data, and if they had ever played Cyberball before. Finally, participants where thanked and compensated.

Results

Based on the *p*-values of the *t*-test for reflexive need threat, data collection was stopped after the first look. Following the pre-registered Sequential Testing procedure, the alpha-level was adjusted based on the sample size at the first look ($N = 93$) using a Pocock type alpha spending function. Therefore, for all statistical inferences we applied $\alpha < .021$ as threshold for significance. In line with recommendations for Sequential Testing (Lakens et al., 2021), we report unadjusted and adjusted effect sizes.

Manipulation Checks

To check whether expectations about being excluded differed between the conditions, we calculated a two-sided Welch *t*-test with the number of expected throws as the dependent variable: As intended, participants in the expected exclusion condition expected to receive fewer ball throws than participants in the unexpected condition, $t(72.93) = -4.84$, $p < .001$, $d = -0.98$, $d_{adjusted} = -0.97$, 95%CI =[−1.40; −0.54].[1]

Expecting versus not expecting exclusion did not change how exclusion was perceived: There was no significant difference in participants' perception of how actively they participated in the game, $t(89.48) = 0.64$, $p = .53$, $d = 0.13$, 95%CI = [−0.28; 0.54], nor in the perceived number of received throws, $t(55.14) = -0.47$, $p = .64$, , $d = -0.10$, 95%CI = [−0.51; 0.31].

Reflexive Need Threat

To test whether need threat after exclusion in the Cyberball game differed between the expected and the unexpected exclusion condition, we calculated a two-sided Welch *t*-test. This test indicated that participants who expected to be excluded reported lower levels of need threat than participants who did not expect to be excluded, $t(83.24) = 2.64$, $p = .009$, $d = 0.54$.

[1] The game consisted of 30 ball throws, therefore, each player can receive a maximum of 15 throws. Excluding participants who indicated expecting more than 15 throws ($n = 3$) did not change significance levels of any analysis.

$d_{adjusted}$ = 0.53, 95%CI = [0.059; 1.141]. The data supported H1, thus, we rejected the H0 that no difference between the conditions exists and stopped data collection. Means and standard deviations of all variables per condition are depicted in Table 1.

Exploratory Mixed ANOVA

We exploratorily calculated a mixed ANOVA (*rstatix* version 0.5.0; Kassambara, 2020) with expectation condition as a between-subjects variable and the timepoint of measuring need threat (baseline vs. reflexive measure) as a within-subjects variable (see Figure 1 for the distribution of need threat as a function of timepoints and expectation condition).

This analysis revealed a significant interaction between timepoint and expectation, $F(1,91) = 8.24$, $p = .005$, $\eta_p^2 = .08$, 95%CI = [.008; .203]. Decomposing the interaction into simple main effects showed lower levels of need threat in participants who expected (vs. did not expect) exclusion *after* Cyberball (reflexive measure), $F(1,91) = 6.68$, $p = .022$, $\eta_p^2 = .07$, 95%CI = [0.003; .18], but not *before* Cyberball (baseline), $F(1,91) = 1.66$, $p = .402$, $\eta_p^2 = .02$, 95%CI = [0; .10]. Importantly, there was a significant decline in need threat over time in both conditions, which was descriptively smaller in the expected exclusion condition, $F(1,48) = 150.00$, $p < .001$, $\eta_p^2 = .76$, 95%CI = [.62; .83], compared to the unexpected exclusion condition, $F(1,43) = 397.00$, $p < .001$, $\eta_p^2 = .90$, 95%CI = [.84; .93]. Thus, in both conditions, exclusion substantially threatened individuals' needs, but to a larger extent when exclusion was unexpected compared to excepted.

Discussion

Consistent with Hypothesis 1, Study 1 suggests that expecting exclusion with a very high likelihood buffers need threat after exclusion, as participants who were informed that they will be excluded with a more than 95% likelihood reported less need threat after Cyberball compared to those who were unexpectedly excluded. However, expectations did not eliminate need threat completely, since participants who expected to be excluded also experienced a strong increase in need threat after exclusion in Cyberball.

These results provide a first piece of evidence that strong expectations may buffer need threat after exclusion. However, as sequential sampling approaches bear the risk of obtaining inflated effect sizes (Lakens, 2021) and significance testing bears the risk of obtaining false-positives, we aimed at replicating the effect with a standard approach (non-sequential testing) in order to strengthen the evidence for buffering effects of expectations. Moreover, Study 1 only reveals insights into possible buffering effects of strong expectations, but not into whether the strength of expectations determines buffering effects. Study 2 was designed to close this gap.

Study 2

In Study 2, we additionally examined if the occurrence of buffering effects depends on whether individuals have weak or strong expectations about the occurrence of exclusion. To that purpose, we manipulated the strength of individuals' expectations of being excluded in Cyberball by varying the probability of future exclusion (50% vs. 95%). While participants in the 95% condition should be relatively certain that exclusion will occur, the 50% condition entails maximums uncertainty, as both outcomes (inclusion and exclusion) are equally likely. We compared the two conditions of expected exclusion (95% vs. 50%) with two conditions where either exclusion or inclusion occurred without any prior information.

Hypotheses

H2.1) Replicating the effects of Study 1, participants who receive no information about future exclusion will report higher reflexive need threat compared to participants who were informed that there is a 95% probability of being excluded.

H2.2) Participants who are informed that there is a 50% probability of being excluded will report higher reflexive need threat compared to participants who are informed that there is a 95% probability of being excluded.

H2.3) Participants who are excluded will report higher reflexive need threat compared to participants who are included, irrespective of the type of information that excluded individuals receive prior to the exclusion event.

Method

The pre-registration of this study is available on the OSF: https://osf.io/8aqr9

Determination of Sample Size

A power analysis was conducted using the R-package Superpower (Lakens & Caldwell, 2021) for a one-way ANOVA design with four conditions and post-hoc pairwise comparisons. To factor in corrections for multiple testing, the alpha level for the power analysis was set to .01. We aimed for statistical power of at least .90 for detecting an effect of $d = 0.4$, representing our smallest effect size of interest. Given the above-specified parameters, this power is reached with a sample size of $n = 84$ per condition (total $N = 336$). We planned to continue data collection until a sample size of 336 valid observations is reached (thus excluding observations that do not fulfill the pre-registered inclusion criteria); however, measures taken to prevent the spread of COVID-19 in the end of 2021 and beginning of 2022 forced us to stop data collection before the pre-registered sample size was reached.

Participants and Design

A total of 309 students from the University of Basel participated in exchange for a small snack or course credit for psychology students. They were randomly assigned to one of four conditions of a one factorial design: No information and inclusion, no information and exclusion, 50% likelihood of exclusion and exclusion, 95% likelihood of exclusion and exclusion. Following pre-registration, participants were excluded from the analyses if they indicated insufficient levels of seriousness ($n = 11$), that they had trouble understanding the study ($n = 3$), or another reason not to use their data ($n = 8$). Also, we excluded participants' data if they had indicated that they had played Cyberball before (n = 21) or that they were familiar with Cyberball before this study (n = 24). The remaining sample consisted of 258 participants (62.79% female, 4 non-binary, 1 not indicated, M_{age} = 23.39, SD = 6.76).

Materials and Procedure

The procedure was very similar to Study 1, but we introduced two additional experimental conditions, as outlined above. In all conditions, participants received the information that the later Cyberball game would consist of 30 ball throws in total. Depending on condition, information on the number of to be expected throws in Cyberball as well as follow-up questions on this information varied as follows:

No information. In the inclusion and in the unexpected exclusion condition, participants received no additional information about the number of throws they could expect in Cyberball.

95% likelihood of exclusion. Analogous to the *expected exclusion* condition in Study 1, participants in the 95% likelihood of exclusion condition received the information that they will be excluded from the game *with a very high probability (95%)*.

50% likelihood of exclusion. Participants in the 50% likelihood of exclusion condition received the information that they will be excluded from the game *with a moderate probability (50%)*.

Only participants in the 50% likelihood of exclusion and 95% likelihood of exclusion conditions were then asked to indicate their subjective estimate of the likelihood of being excluded ("*How likely do you personally believe it is that you will be excluded in the Cyberball Game?*" 0-100%). This measure was administered to check if participants in the 95% likelihood of exclusion condition indeed indicate a higher subjective likelihood of exclusion than participants in the 50% likelihood of exclusion condition. To be able to examine whether the likelihood of future exclusion also influences participants' feelings of certainty, participants were further asked to quantify how certain they were about this likelihood estimate ("*How certain are you that this personal estimate is correct?*") and about what might happen during Cyberball in general ("*How uncertain/certain are you about what will happen during the Cyberball Game?*") on a scale from 1 = *very uncertain* to 9 = *very certain*.

All participants then worked on the same filler task and answered four *baseline* need threat items and one item on mood as in Study 1. Next, participants were either excluded from Cyberball (see Study 1) or included, meaning that they received an equal share of ball throws from their co-players.

After the Cyberball game, participants were asked to indicate their levels of *reflexive* need threat and their mood during the game as well as how actively they had participated in the Cyberball game and how many throws they had actually received. The one item of mood was again highly correlated with need threat ($r = -.68$ at baseline and $r = -.76$ after Cyberball, $p < .001$) and is thus reported online (see OSF).

Participants then provided demographic information, were asked if they had responded seriously throughout the study, if there was a reason not to use their data, and if they had played Cyberball before. Finally, participants where thanked and compensated.

Results

If not indicated otherwise, an alpha-level of .05 is applied for all statistical inferences.

Manipulation Checks

To check whether the subjective likelihood estimate of exclusion in the upcoming Cyberball Game differs between the 50% likelihood of exclusion and 95% likelihood of exclusion conditions, we calculated a two-sided Welch *t*-test with the subjective likelihood estimate as the dependent variable. Participants in the 50% likelihood of exclusion condition expected to be excluded with an average probability of 48.01% ($SD = 16.89$), whereas participants in the 95% likelihood of exclusion condition expected to be excluded with an average probability of 66.64% (*SD* = 27.88), $t(107.89) = -4.59$, $p < .001$, $d = -0.80$, 95%CI = [−1.16; −0.44].

There was no significant difference between the 50% and the 95% likelihood of exclusion condition regarding participants' certainty about the correctness of their likelihood estimate, $t(126.78) = 0.83$, $p = .407$, $d = 0.15$, 95%CI = [−0.20; 0.50], and their certainty about what will happen during the Cyberball Game, $t(126.95) = -1.69$, $p = .093$, $d = -0.30$, 95%CI = [−0.65; 0.05].

We calculated a one-way ANOVA and follow-up pairwise comparisons using Tukey HSD tests to check whether the four groups differ in their perception of active participation in Cyberball and perceived ball throws. There was no significant difference in active participation and perceived throws between any of the three exclusion conditions (smallest $p = .086$), but, as intended, included participants reported to have participated more actively and to have received more ball throws in the game compared to participants in any of the exclusion conditions (all $p < .001$; see Table 2 for all means and standard deviations).

Need Threat

We calculated a mixed ANOVA with condition as a between-subjects variable and the timepoint of measuring need threat (before vs. after Cyberball) as a within-subjects variable. This analysis revealed a significant timepoint × condition interaction, $F(3,254) = 98.92$, $p < .001$, $\eta_p^2 = .54$, 95%CI = [.46; .60], which was qualified by a significant increase in need threat for participants in all exclusion conditions ($\eta_p^2 > .83$, $p < .001$), but not the inclusion condition, $F(1,63) = 0.16$, $p = .69$, $\eta_p^2 < .01$, 95%CI = [.00; .02]. Repeating the mixed ANOVA with only the exclusion conditions showed only a main effect of timepoint, $F(1,191) = 1122.83$, $p < .001$, $\eta_p^2 = .86$, 95%CI = [.82; .88], but no significant timepoint × condition interaction $F(1,191) = 0.18$, $p = .84$, $\eta_p^2 < .01$, 95%CI = [.00; .03]. Thus, contradicting the findings from Study 1, there is no evidence for a larger incline in need threat when exclusion occurs unexpectedly compared to expectedly.

To investigate differences between conditions on reflexive need threat directly after Cyberball, we calculated a one-way ANOVA and follow-up Tukey HSD tests. Participants in the inclusion condition reported significantly lower levels of need threat compared to participants in all exclusion conditions (all $p < .001$). However, in contradiction to Hypotheses 2.1 and 2.2, there were no significant differences between the unexpected exclusion and the 95% likelihood of exclusion, $p = .961$, $d = -0.08$, 95%CI = [–0.43; 0.26], the 95% likelihood of exclusion and the 50% likelihood of exclusion condition, $p = .805$, $d = 0.18$, 95%CI = [–0.17; 0.52], and the unexpected exclusion and the 50% likelihood of exclusion condition, $p = .511$, $d = -0.25$, 95%CI = [–0.59; 0.10].

To test whether potential differences between the three exclusion conditions are smaller than a pre-defined smallest effect size of interest, we performed equivalence tests using the R-package TOSTER (Lakens, 2018). We pre-registered to consider only effects larger than $d = \pm 0.4$ as meaningful. The equivalence test was significant for the comparison between unexpected exclusion and the 95% likelihood of exclusion condition, $t(123.39) = 1.80$, $p = .037$, 90%CI = [–0.47; 0.26], suggesting that if differences between the conditions were to exist, they would be of a non-meaningful effect size. In contrast, the equivalence test was not significant for the two comparisons between the 95% likelihood of exclusion and the 50% likelihood of exclusion condition, $t(127) = 1.26$, $p = .106$, 90%CI = [–0.52; 0.12], and between the unexpected exclusion and the 50% likelihood of exclusion condition, $t(120.59) = 0.87$, $p = .192$, 90%CI = [–0.66; 0.06]. Thus, no strong inferences can be drawn about the presence or absence of meaningful effects for these two comparisons. For the distribution of need threat between conditions, see Figure 2.

Exploratory Analysis of the Subjective Likelihood of Being Excluded

As reported previously, manipulation checks show that individuals in the 95% likelihood condition on average underestimated the likelihood of exclusion to be only 66%, suggesting that the expectation manipulation was not entirely successful. Conceivably, participants' subjective estimate of the likelihood of being excluded may thus be a better predictor of reactions to exclusion than the experimental condition. For that reason, we exploratorily calculated the correlation between the subjective estimate of the likelihood of being excluded and need threat following Cyberball in the 50% and the 95% likelihood of exclusion condition. However, this correlation was not significant, $r = .05$, $p = .596$, 95%CI = [−.13, .22].

Discussion

Study 2 did not replicate the expectation findings from Study 1, as there was no difference between participants that expected to be excluded with a very high likelihood (95%) compared to those who did not expect to be excluded at all. An equivalence test further suggests that the presence of meaningful effects for this comparison can be rejected.

Several reasons may explain the discrepancy in the findings between Studies 1 and 2. First, there were slight differences in the materials, in particular regarding the control questions we used after the expectation manipulation (i.e., asking about the number of expected ball throws in Study 1 versus the subjective likelihood of exclusion in Study 2). Second, Studies 1 and 2 relied on different samples: Whereas Study 1 was based on an online sample recruited via Prolific in the UK, Study 2 was administered as a lab study including students from a European university. This resulted in differences in mode of data collection, study language, and demographics. Conceivably, some or all of these differences together may have impacted the success of the expectation manipulation and, consequently, buffering effects on need threat. Arguably, another reason for the failed replication is the possibility that the findings on buffering effects of expectations on need threat were based on a false positive in Study 1 or over-estimated effect sizes (see Lakens, 2021).

To shed light on the mixed evidence in Study 1 versus Study 2, we conducted a third study as a close replication of Study 1, but without sequential sampling.

Study 3

Study 3 sought to examine the same hypotheses as Study 2 and involved the same four experimental conditions, but adopted the materials from Study 1 and recruited a larger sample size. This approach allowed conducting a close replication of Study 1 while still being able to test whether the strength of expectations influences buffering effects on exclusion.

Method

The pre-registration is available on the OSF:

https://osf.io/jg79t

Determination of Sample Size

Based on the same power analysis as in Study 2, we planned to collect 336 valid observations.

Participants and Design

A total of 393 participants completed the study via Prolific Academic. Participants were randomly assigned to the same four conditions as in Study 2: Inclusion, unexpected exclusion, 50% likelihood of exclusion, and 95% likelihood of exclusion. In accordance with pre-registration, participants' data were removed if they had indicated that there was a reason not to use their data ($n = 9$); that they had trouble understanding the study ($n = 3$); or that they had played Cyberball before or were familiar with the game ($n = 36$). The remaining sample consisted of 337 participants (66.17% female, 6 non-binary, $M_{age} = 38.18$, $SD = 13.56$).

Materials and Procedure

Study 3 adopted the materials and procedure from Study 1. For instance, rather than asking participants about their subjective estimate of the likelihood of being excluded as in

Study 2, participants in all conditions were asked to indicate the number of ball throws they expected to receive in the later Cyberball game.

Results

If not indicated otherwise, an alpha-level of .05 is applied for all statistical inferences. All descriptive statistics for Study 3 are depicted in Table 3.

Manipulation Checks

To check whether expectations about being excluded differed between conditions, we calculated a one-way ANOVA and follow-up Tukey HSD tests with the number of expected throws as the dependent variable. This analysis showed that participants' in the 95% likelihood of exclusion condition expected fewer ball throws compared to participants in the inclusion condition, $p < .001$, $d = 1.31$, 95%CI = [0.98; 1.64], fewer than participants in the unexpected exclusion condition, $p < .001$, $d = 1.25$, 95%CI =[0.92; 1.58] and fewer than participants in the 50% likelihood of exclusion condition, $p < .001$, $d = 0.85$, 95%CI = [0.54; 1.16]. Participants in the 50% likelihood of exclusion condition expected fewer ball throws than participants in the unexpected exclusion condition, $p = .005$, $d = 0.47$, 95%CI = [0.16; 0.77]. There was no significant difference between the inclusion and the unexpected exclusion condition, $p = .271$, $d = 0.30$, 95%CI = [–0.01; 0.61], and no significant difference between the inclusion and the 50% likelihood of exclusion condition, $p = .418$, $d = 0.29$, 95%CI = [–0.02; 0.59].

We further tested whether the four groups differ in their perception of active participation in Cyberball and received ball throws. There was no significant difference in active participation or perceived throws between any of the three exclusion conditions (smallest p =.086), but included participants reported to have participated more actively and to have received more ball throws in the game compared to participants in any of the exclusion conditions (all $p < .001$).

Need Threat

A mixed ANOVA with condition as a between-subjects variable and the timepoint of measuring need threat (before vs. after Cyberball) as a within-subjects variable showed a significant timepoint × condition interaction, $F(3,333) = 100.44$, $p < .001$, $\eta_p^2 = .48$, 95%CI = [.40; .53]. Follow-up simple main effects showed that there was a significant increase in need threat for participants in all exclusion conditions ($\eta_p^2 > .77$, $p < .001$), but not in the inclusion condition, $F(1,82) = 1.38$, $p = .24$, $\eta_p^2 = .02$, 95%CI = [.00; .03]. Repeating the mixed ANOVA with only the exclusion conditions showed only a main effect of timepoint, $F(1,251) = 998.75$, $p < .001$, $\eta_p^2 = .80$, 95%CI = [.76; .83], but no significant timepoint × condition interaction $F(1,251) = 0.20$, $p = .82$, $\eta_p^2 < .01$, 95%CI = [.00; .02]. Thus, in line with Study 2, there is no evidence for a larger incline in need threat when exclusion occurs unexpectedly compared to expectedly.

To investigate differences between conditions on reflexive need threat directly after Cyberball, we calculated a one-way ANOVA and follow-up Tukey HSD tests. Participants in the inclusion condition reported significantly lower levels of need threat compared to participants in all exclusion conditions (all $p < .001$). However, in contradiction to Hypotheses 2.1 and 2.2, and analogous to Study 2, there was no significant difference between any of the three exclusion conditions (smallest $p = .511$).

To test whether potential differences between the three exclusion conditions are smaller than our pre-defined smallest effect size of interest ($d = \pm0.4$), we performed equivalence tests using the R-package TOSTER (Lakens, 2018). The equivalence test was significant for the comparison between unexpected exclusion and the 95% likelihood of exclusion condition, $t(162.86) = -2.34$, $p = .010$, 90%CI = [−0.30; 0.41], for the comparison between the 50% likelihood of exclusion, and the 95% likelihood of exclusion condition, $t(168.87) = 2.61$, $p = .005$, 90%CI = [−0.32; 0.32], and for comparison between the unexpected exclusion and the 50% likelihood of exclusion condition, $t(158.42) = -2.32$, $p = .011$, 90%CI = [−0.29; 0.41]. Thus, the presence of meaningful effects ($d = \pm0.4$) can be rejected for the comparison between all three exclusion conditions. For the distribution of need threat between conditions, see Figure 3.

Exploratory Analysis of Expected Throws

To test whether there is an association between the number of expected throws and need threat in the exclusion conditions, we calculated the correlation between both measures across all three exclusion conditions. This correlation was not significant, $r = .05$, $p = .47$, 95% CI [−.08, .17], suggesting no association between the number of expected throws and need threat.

Discussion

Study 3 replicates findings from Study 2. Contradicting Study 1, there was no difference in need threat between participants that expected to be excluded with a very high likelihood (95%) compared to those who did not expect to be excluded at all. Participants' need threat in both groups also did not differ from participants that expected to be excluded with a likelihood of 50%. Equivalence tests indicate statistical equivalence between all three conditions, suggesting that there are no meaningful buffering effects of expectations on need threat.

Study 4

Both Studies 2 and 3 provide evidence against buffering effects of expectations. However, inferences are limited in several regards. First, we only administered one specific measure of need threat and a one-item mood measure, and cannot rule out that the absence of meaningful buffering effects is due to idiosyncrasies of these measures. Second, we used only the Cyberball paradigm, and it is unclear to what extent the absence of expectation effect generalizes to other exclusion experiences. Even though Cyberball is a common and well-established paradigm in exclusion research (Hartgerink et al., 2015), it may have idiosyncrasies of importance when studying expectation effects. Third, it cannot be ruled out that the filler task between the expectation and the exclusion manipulation cognitively distracted participants from the prospect of an upcoming exclusion experience. While we implemented the filler task so that participants across conditions do not differ in need threat *prior* to playing Cyberball, one could argue that working on the filler task made participants forget about the expectation manipulation, thus reducing its impact.

A fourth study was designed to overcome these limitations, implementing three important adaptations: First, we applied more extensive measures of need threat and negative affect. Second, in addition to Cyberball, we implemented a new exclusion paradigm, henceforth referred to as *team selection paradigm* (for a similar approach, see Bourgeois & Leary, 2001). In the team selection paradigm, participants are told that a set of team leaders (computer-generated co-players) select their preferred team members based on individual player profiles created in the beginning of the study. To induce feelings of exclusion, participants then experience the selection process; while all other players in the game are subsequently chosen by one of the team leaders, the participant is the only one to remain unselected. The process implemented in the team selection paradigm mimics a real-life exclusion situation many individuals are familiar with (e.g., selection processes in sports class) and allows to test for expectation effects in a more realistic setting. Third, in the expectation conditions, the filler task included a constant reminder about the likelihood of exclusion to make sure that participants do not forget about the upcoming exclusion event, thereby upholding the cognitive expectation of exclusion. We still included a filler task to allow for affective recovery from the potential threat of the information on the upcoming exclusion.

Finally, because expectations and social norms are often confounded (Gerber & Wheeler, 2014), Study 4 exploratorily assessed perceived norm-consistency of the social exclusion experience.

Hypotheses

Based on previous findings in Studies 2 and 3, we predicted that:

H4.1: Participants who expect (versus do not expect) exclusion do not differ (effect size smaller than Cohen's $d = 0.3$) in their average levels of reflexive need threat after exclusion, irrespective of the exclusion paradigm.

H4.2: Participants who expect (versus do not expect) exclusion do not differ (effect size smaller than Cohen's $d = 0.3$) in their average levels of negative affect after exclusion, irrespective of the exclusion paradigm.

Method

The pre-registration is available on the OSF: https://osf.io/x63ud

Determination of Sample Size

To be able to detect even smaller potential expectation effects as in the previous studies, we lowered our smallest effect size of interest (referred to meaningful) to $d = 0.3$. A power analysis for reflexive need threat suggested a total of 469 participants to detect main and interaction effects of the between-subject factors expectation and paradigm of $d = 0.3$ ($f = .15$) size with a power of .90 at alpha = .05.

Participants and Design

A total of 507 participants completed the study via Prolific Academic. Participants' data were removed if they indicated that there was a reason not to use their data (*n* = 1); that they had not participated seriously (*n* = 2); or that they had trouble understanding the study (*n* = 2). Moreover, participants' data was removed if they indicated that they had played Cyberball before (Cyberball condition only; *n* = 30), or that they would do the task together with a group rather than alone (Team Selection Paradigm condition only, *n* = 1). The remaining sample consisted of 471 participants (45.65% female, 2 non-binary, 2 not specified, M_{age} = 40.16, *SD* = 13.28). Participants were randomly assigned to one of four conditions in a 2 (paradigm: Cyberball versus team selection paradigm) x 2 (expectation: expectation vs. no-expectation) between-subjects design.

Materials and Procedure

Participants in all conditions experienced an exclusion episode, either in Cyberball or in the team selection paradigm.

Team Selection Paradigm. In the beginning of the study, participants learnt that they would later take part in a group task (i.e., a word creativity game) together with computer-generated players programmed to mimic human behavior. Participants were then asked to answer a few trivial questions (e.g., "Imagine you could freely pick the color of your house. Which color would it be?") and choose a player name which would build the basis for their player profile. They were further informed that a few randomly selected players would later be selected to be group leaders and to choose their preferred teammates based on the player profiles. Importantly, it was highlighted that each group leader could select between three to five players for their team and that in the ideal case, all players would be included in a team (i.e., to mimic the default inclusion norm in Cyberball).

In the *unexpected exclusion condition*, after creating the player profile, participants directly proceeded with the identical filler task as in Studies 1-3, again introduced as a mental visualization task.

In the *expected exclusion condition*, after creating the player profile, participants were informed that the study's algorithm suggested a very high likelihood (95%) that the other players would not include them into their team and that they would have to do the group task alone. They were then asked how likely they personally thought it was that they would not be part of a team (subjective likelihood of exclusion; from 0% to 100% likelihood) in the later task. Next, participants proceeded to the photo-selection filler task. In contrast to the previous studies and the no expectation condition, the filler task included a constant reminder of the upcoming exclusion. That is, participants were constantly presented with the information "*95% percent likelihood that you will not be part of a team*."

After the filler task, baseline need threat levels and negative affect were assessed. In addition to the 4-item need threat scale used in the prior studies, a 20-item need threat scale (Jamieson et al., 2010) with five items per need and an eight item negative affect scale (as used in Wirth et al., 2017) were administered, too. Answers to both scales were given on a 9-point Likert scale from *1 = not at all* to *9 = extremely* with respect to current feelings (e.g., "*I feel like an outsider*" for belongingness needs; "*I feel angry*" for negative affect). Internal consistency for the 4-item ($\alpha = .93$), the 20-item need satisfaction ($\alpha = .96$), and the negative affect ($\alpha = .92$) scale was high so that we calculated a composite score across all items for each scale.

Next, in both the expectation and the no expectation condition, participants experienced an exclusion episode in the team selection paradigm, which occurred as follows (see also supplemental materials): Participants were informed that five other players were selected as group leaders and could choose their preferred teammates amongst a total of 21 other players, including the participant. Every time a team leader chose a player, the player's name disappeared from the list of remaining players and appeared in the team leader's group. In the end of the selection process, only the participant's player name remained and was not selected by a team leader, even though not all spots in the team had been filled. The participant was informed that nobody selected them for their team and that they had to do the following task alone rather than with a group.

After the exclusion episode, reflexive need threat levels (α = .97) and negative affect (α = .94) were assessed with the same items as for the baseline measurement. To check whether participants had paid attention to the outcome of the team selection paradigm, they were asked to indicated with whom they will be doing the subsequent task. To uphold the cover story, participants then worked alone on the task that allegedly would have been a group task had they been selected for a team. Finally, participants were asked to rate the perceived norm consistency of the experienced exclusion episode by means of one item assessing injunctive norms (*"The group leaders behaved how people should behave")* and one item assessing descriptive norms ("*The group leaders behaved how people usually behave"*; *1 = do not at all agree* to *7 = completely agree*). Participants then answered the same final questions as in the previous studies before being debriefed and compensated.

Cyberball. The procedure in the Cyberball conditions was largely identical to Study 3. However, as in the expected exclusion team selection paradigm condition, in Study 4 the filler task in the expected exclusion Cyberball condition included a constant reminder about the upcoming exclusion (i.e., "*95% percent likelihood that you will not be part of a team*"). All other measures were identical to those in the team selection paradigm condition, but were framed with regard to the other players instead of the group leaders.

Results

If not indicated otherwise, an alpha-level of .05 is applied for all statistical inferences. All descriptive statistics for Study 4 are depicted in Table 4.

Need Threat

The 4-item and 20-item need satisfaction composite scores were correlated to r = .42 at baseline and r = .68 at the reflexive stage. Since no differences regarding the interaction effect of interest (paradigm × expectation) between the 4-item and the 20-item need satisfaction scale occurred, we here only report the results for the 20-item need satisfaction scale (see supplemental materials for the 4-item scale).

We calculated a three-way mixed ANOVA for need threat with paradigm and expectation as between-subjects and timepoint of measurement (baseline versus reflexive stage) as within-subjects factor. This analysis revealed a significant main effect for timepoint, $F(1,467) = 275.35$, $p < .001$, $\eta_p^2 = .37$, 95% CI = [.30, .43], and a significant two-way interaction between paradigm and timepoint, $F(1,467) = 21.19$, $p < .001$, $\eta_p^2 = .04$, 95% CI = [.01, .08]. Decomposing this two-way interaction into simple main effects revealed a significant effect of paradigm at the reflexive, $F(1,467) = 7.78$, $p = .005$, $\eta_p^2 = .02$, 95% CI = [.001, .05], but not at the baseline measure, $F(1, 467) = 1.26$, $p = .261$, $\eta_p^2 < .01$, 95% CI = [.00, .02]. Specifically, participants in the Cyberball condition experienced more need threat following exclusion compared to participants in the team selection paradigm (see Table 4).

All other effects in the model were not significant ($p > .269$). Most importantly, there was neither a significant main effect of expectation, $F(1, 467) = 0.24$, $p = .623$, $\eta_p^2 < .01$, 95% CI = [.00, .01], nor a significant two-way interaction between expectation and timepoint of measurement, $F(1,467) = 0.86$, $p = .355$, $\eta_p^2 < .01$, 95% CI = [.00, .02].

As a result of the non-significant effect of expectation and adhering to the pre-registration, we performed an equivalence test based on a smallest effect size of interest of $d \pm 0.3$ for reflexive need threat between the no expectation and the expectation condition across paradigms. This equivalence test was significant, $t(467.52) = 2.51$, $p = .006$, 90%CI = [−0.42;

0.16], indicating that the presence of meaningful differences ($d = \pm 0.3$) between the expectation and the no expectation condition can be rejected. Figure 4 depicts participants' need threat levels in all four conditions as a function of timepoint.

Negative Affect

The 8-item negative affect measure was strongly correlated with need threat at both timepoints ($r = -0.82$ at baseline; $r = -0.85$ at reflexive stage, $p < .001$). The same mixed ANOVA as for need threat was repeated with the negative affect score as a dependent variable. As for need threat, there was a significant main effect of timepoint, $F(1,467) = 224.08$ $p < .001$, $\eta_p^2 = .32$, 95% CI = [.26, .38], and a significant two-way interaction between paradigm and timepoint, $F(1,467) = 12.73$, $p < .001$, $\eta_p^2 = .03$, 95% CI = [.01, .06], as participants in the Cyberball condition experienced more negative affect following exclusion (reflexive stage) compared to participants in the team selection paradigm.

There were again no other significant effects, most importantly no significant main effect of expectation, $F(1,467) = 1.63$, $p = .203$, $\eta_p^2 < .01$, 95% CI = [.00, .02], and no significant two-way interaction between expectation and timepoint of measurement, $F(1,467) = 0.44$, $p = .506$, $\eta_p^2 < .01$, 95% CI = [.00, .01]. An equivalence test, $t(468.05) = -2.05$, $p = .021$, 90%CI = [–0.08; 0.52], indicated that the presence of meaningful differences ($d = \pm 0.3$) between the expectation and the no expectation condition for negative affect can be rejected.

Exploratory Analyses of Subjective Likelihood and Norm Consistency

We exploratorily calculated separate linear regressions with the injunctive norm, the descriptive norm, and the subjective likelihood of exclusion as a predictor variable of reflexive need threat while controlling for the experimental factors expectation and paradigm (both effect-coded; -1,+1). This analysis revealed a negative association between need threat and the injunctive norm, $\beta = -0.33$, $t(467) = -6.01$, $p < .001$, indicating that the more participants perceived the other players in Cyberball or the group leaders in the team selection paradigm to behave as they should behave, the less they experienced need threat. Neither the association between need threat and the descriptive norm, $\beta = 0.02$, $t(467) = 0.36$, $p = .720$, nor between need threat and the subjective likelihood of exclusion, $\beta = 0.05$, $t(229) = 0.78$, $p = .437$, were significant.

Discussion

Study 4 further supports the evidence from Studies 2 and 3 suggesting no buffering effects on need threat following exclusion regardless of prior expectations. Most importantly, Study 4 suggests that the absence of buffering effects is not specific to Cyberball or the use of specific need threat measures, and that the absence cannot be explained by assuming that participants forgot about the subsequent exclusion during the filler task.

Moreover, the bigger sample size in Study 4 allows for inferences about an even smaller smallest effect size of interest compared to the previous studies, suggesting that effects larger than $d = \pm 0.3$ can be rejected.

In addition, Study 4 replicates prior research by indicating that not expectations, but injunctive social norms buffer need threat following exclusion (Rudert & Greifeneder, 2016). This finding is of particular interest concerning the integration of mixed prior evidence, suggesting that prior buffering effects may go back to the circumstance that expected exclusion may have been perceived as more norm consistent than unexpected exclusion.

Integrative Data Analysis Across Studies

In order to understand the overall evidence in the present four studies, we performed an integrative data analyses across the pooled data from Studies 1 to 4 (see Curran & Hussong, 2009). Since only the unexpected exclusion and the 95% likelihood of exclusion condition were present in all studies, we constrained our analyses to these conditions, resulting in an overall sample size of $N = 865$. We first calculated separate linear regressions testing for interaction with each of the following variables: Modus of data collection (lab versus online), exclusion paradigm (Cyberball versus team selection paradigm), and need satisfaction measure (4-item versus 20-item scale). Since there was neither a main effect of expectation, nor an interaction between expectation and any of the factors (all $p > .210$), we conducted an equivalence test between the two expectation conditions across all four studies. The equivalence test was significant, suggesting that need threat levels in the unexpected exclusion ($M = 6.47$, $SD = 1.99$) and the 95% likelihood of exclusion condition ($M = 6.39$, $SD = 1.96$) are equivalent to the extent that effect sizes larger than $\pm\, d = 0.3$ can be rejected, $t(862.58) = 3.82$, $p < .001$, 90%CI = [−0.34; 0.18].

General Discussion

Expectations can buffer negative responses to adverse outcomes in different domains (e.g., Shepperd & McNulty, 2002). In contrast, evidence on buffering effects of expectations on need threat after exclusion is mixed and scarce (Wesselmann et al., 2017). Four studies examined whether expectations of exclusion can buffer the immediate threat of exclusion to individuals' basic needs. Study 1 was in line with the assumption of buffering effects of expectations, suggesting that participants who expect compared to not expect to be excluded experience less need threat. However, Studies 2, 3 and 4 could not replicate this effect, as they suggest no difference in need threat between participants who do not compared to those who expect to be excluded with a high (95%) or a moderate likelihood (50%). Moreover, the absence of buffering effects does not only apply to measures of need threat, but also to mood and negative affect. An integrative data analysis applying an equivalence test across the data from all four studies suggests that buffering effects of expectations larger than $d = \pm 0.3$ can be rejected.

From a conceptual perspective, the absence of buffering effects of expectations aligns with the idea that immediate need threat occurs as a default reaction to any cues that threaten individuals' status of inclusion; as a result, they should be relatively robust and difficult to buffer (Williams, 2009). The present findings thus provide no support for the perspective that, similar to physical pain, expectations may buffer the intensity of experienced social pain by social exclusion, and thus point to an interesting conceptual dissociation between social and physical pain. In the following, we elaborate on these conceptual conclusions and discuss potential limitations and practical implications.

Is the Overall Picture not as Inconsistent After all?

Findings on buffering effects of expectations within this as well as in prior work have been inconsistent. With regard to the evidence accrued in this paper, the most likely explanation for the inconsistent results between Study 1 versus Studies 2, 3 and 4 may be false positive or overestimated effects in Study 1. As the 95% likelihood of exclusion condition and the unexpected exclusion condition in Study 3 were a direct replication of Study 1, the inconsistent results cannot be attributed to differences in procedure between Studies 1 and 3.

As for the results across earlier studies, three explanations were introduced that have the potential to reconcile what appears heterogenous. In what follows, we discuss each of these explanations. Given that prior studies varied in many respects, none of these explanations can account for all differences; rather, taken together, the explanations provide a coherent picture.

No Evidence for Moderation by the Strength of Expectations

The first explanation held that differences in expectation strength may explain prior contradicting findings on buffering of expectations. In particular, it appeared plausible that buffering may have occurred in prior studies where individuals were rather certain and thus had a strong expectation that exclusion will occur (Gerber & Wheeler, 2014), but not in studies where individuals were uncertain and thus had a weaker expectation that exclusion will occur (e.g., Rudert & Greifeneder, 2016). Yet, in the present studies, there were neither buffering effects for weak expectations (50% likelihood of exclusion), nor for strong expectations (95% likelihood of exclusion).

Several reasons may explain why even strong expectations about future exclusion do not buffer need threat after exclusion. First, even our manipulation of strong expectations may not have been strong enough, as a 95% likelihood to be excluded still involves some degree of uncertainty (i.e., 5% chance of being included if inclusion and exclusion are perceived as a binary outcome). According to Tversky & Kahneman (1992), the psychological impact of probabilities does not follow a linear function, and the difference between certain outcomes and anything less than certain is particularly large. Conceivably, buffering effects of expectations may only occur for participants with a maximally certain expectation (likelihood = 100%) that exclusion will occur. However, only very few participants (e.g., two participants in Study 2) in our studies had such strong expectations.

Second, research on buffering effects of physical pain emphasizes the importance of predictability of the intensity and onset of pain (Carlsson et al., 2006; Ploghaus et al., 2003). As our participants indicated to have never played Cyberball before, there might have been uncertainty involved about when exactly exclusion will occur and what it will feel like. Possibly, even if participants were completely certain that they will be excluded, the information participants received did not allow for predictions about intensity and onset and might thus not have been sufficient to buffer social pain.

Third, our data shows that participants in the 95% likelihood of exclusion condition, on average, strongly underestimated the likelihood of actually being excluded in Cyberball (i.e., 66%). Perhaps this is because individuals tend to be unrealistically optimistic about future outcomes, generally expecting that they are less likely to experience negative and more likely to experience positive events than their fellow humans (Shepperd et al., 2015; Weinstein, 1980). To account for the variance in individuals' subjective beliefs, we tested the association between participants' actual expectations and reflexive need threat. Notably, there was neither an association between the subjective likelihood estimate of exclusion and need threat (Studies 2 and 4), nor between the number of expected ball throws and need threat (Study 3), suggesting that buffering effects do not occur as a function of the subjective likelihood of exclusion.

In summary, our results suggest that the way we manipulated strong expectations did not buffer need threat after exclusion to a meaningful extent. This absence of buffering effects has to be treated with caution, as we cannot rule out that other manipulations could create situations in which expectations are more conducive to showing buffering effects. It is less clear, however, how frequent such situations are in real-life, because the prospect on future events always entails a certain degree of uncertainty and individuals tend to be unrealistically optimistic about future outcomes. We thus conclude that albeit interesting from a theoretical perspective, situations where individuals have expectations that are equivalent to being certain that one will be excluded may be too rare to be of practical interest.

While Norms may Buffer Need Threat, Expectations do not

The second explanation related to a frequent confound of expectations and social norms in prior studies (as discussed by Gerber & Wheeler, 2014) as well as in many real-life settings. Replicating prior evidence (Rudert & Greifeneder, 2016), Study 4 suggests that different to expectations, perceived norm-consistency can buffer need threat following exclusion. It thus seems plausible that buffering effects of expectation occurred in prior studies whenever expected exclusion was perceived as norm-consistent. Given that expectations and norms often align in real life, this finding may explain why expecting exclusion intuitively appears to be a powerful buffer. To illustrate, recall the example introduced in the very beginning, where your colleagues' behavior may or may not have created the expectation that you will be excluded from a joint social activity. The present findings suggest that regardless of your prior expectation, not being invited hurts. However, the picture is likely different if expectations are consistent with social norms. Imagine that you are not just a colleague, but the new boss. In this case, the expectation of being excluded aligns with existing social norms relating to hierarchy, and this social norm is likely to buffer need threat (Rudert & Greifeneder, 2016).

For Cognitive Bracing to Show, Need Threat Must be Able to Increase Further

The third explanation held that buffering effects across studies depend on whether baseline levels of need threat had room for further increase in response to the exclusion experience. For this reason, we consistently implemented filler tasks, which resulted in similarly low baseline levels of need threat prior to the exclusion event across all conditions. This procedure is different from, for instance, the study conducted by Wirth et al. (2017), which did not include a filler task and in which the expectation manipulation (i.e., the social cues participants received) already threatened needs to such an extent that further increase on the applied measurement scale appears unlikely. Notably, the findings of Wirth are important because they show that when individuals already are in a state of strong need threat, there is no further increase after new episodes of exclusion. This process, however, is psychologically different to *cognitive bracing* (Mellers et al., 1997; Shepperd & McNulty, 2002) as investigated here.

Limitations and Future Research

The present research took great care to overcome situations that may have led to heterogeneous evidence in the past. Nevertheless, limitations apply and need short discussion.

Very Small Effects Could Still Exist

First, while the present findings suggest that no meaningful differences ($d > 0.3$) between expected and unexpected exclusion exist, we stress that it cannot be ruled out that expectation effects smaller than this effect size exist. However, given the generally large effects of social exclusion ($d > 1.4$; Hartgerink et al., 2015), expectation effect sizes smaller than $d = 0.3$ would mean that a considerable exclusion effect remains, thus making expectations not the first choice when considering interventions in real-life.

No Inferences About Recovery Possible

Second, it should be noted that we only addressed immediate reactions to exclusion, and thus cannot make inferences about whether recovery from exclusion differs as a function of prior expectations. Future research may address the question of how expectations influence recovery, particularly focusing on the interplay with attributions of exclusion. Prior research suggests that excluded individuals recover more slowly when they attribute exclusion to a permanent group membership (e.g., gender) compared to a temporary group membership (e.g., color assigned in a game; Wirth & Williams, 2009) or when they attribute exclusion to racial prejudice (Goodwin et al., 2010). It is unclear, however, how expectations would interact with these attributions. Possibly, expecting that one will be excluded due to others' discriminatory behavior (e.g., racial prejudice) might help to recover more quickly. Alternatively, expecting that one will be excluded due to discrimination might heighten individuals' sensitivity towards exclusion. As a consequence, individuals may be more likely to attribute innocuous exclusion (e.g., normative exclusion) to malicious motives (e.g., racism), impeding recovery from exclusion (Bernstein et al., 2010; Goodwin et al., 2010).

No Insights Into Potential Moderators

Third, the present research does not reveal any insights into potential moderators of expectation effects and does not answer why and when expectations do not buffer reactions to social exclusion. While we tested but did not find evidence for the assumption that the strength of expectations determines the occurrence of buffering effects, other factors may moderate the occurrence of buffering effects. Following the rationale of the temporal need threat model (Williams, 2009), expectations may be effective buffers whenever they allow individuals to construe upcoming exclusion in a less threatening way (see Rudert & Greifeneder, 2016). Investigating under which circumstances expectations change the construal of exclusion situations may thus be a promising avenue for further investigations.

Further Evidence for the Dissociation Between Physical and Social Pain Needed

Fourth, the present findings suggest an interesting dissociation between physical and social pain with regard to the role of expectations. In particular, while predictable compared to unpredictable physical pain has been shown to be less aversive (Carlsson et al., 2006), no comparable effect of expectations were observed in the context of social pain. This dissociation has the potential to extend frameworks such as pain overlap theory (Eisenberger, 2012), stressing the commonalities between the two kinds of pain. We hasten to add, however, that this dissociation may also hinge on specific features of the present studies. In particular, consistent with most ostracism research, we conceptualized social pain in terms of a threat to individuals' basic needs. Arguably, a measure assessing threat to psychological needs may not accurately capture social pain. Excluded individuals may, for instance, feel devalued by their co-players to a similar extent, but they may still differ in how painful they perceive this feeling. Thus, the present results may foremost show that expectations do not buffer *need threat*, but they may be silent about the potential to buffer the pain associated with this threat. However, need threat and hurt feelings are usually highly correlated (Rudert et al., 2017; Rudert & Greifeneder, 2016) and need threat correlates with activation of brain areas that are associated with physical pain (Eisenberger et al., 2003). From this perspective, one could argue that the assessment of pain instead of need threat would not have yielded different results.

Ecological Validity Versus Methodological Rigor

Finally, many prior studies on expectations in the context of exclusion are characterized by a high ecological validity, since the way expectations were manipulated closely mirrors the development of expectation in real-life (i.e., based on prior social interactions or social norms; Gerber & Wheeler, 2014; Wirth et al., 2017). In contrast, one could argue that the manipulation of expectations applied in the present Studies 1 to 3 was more detached from real-life scenarios (usually, individuals do not develop expectations about future exclusion by being informed about likely future outcomes). We agree and note that future research may fruitfully explore other pathways to balance ecological validity and methodological rigor. Going some way in this direction, Study 4 (a) manipulated expectations about the upcoming exclusion experience via individual player profiles, and (b) excluded participants in a team selection process that mimics a real-life situation many individuals may be familiar with.

Practical Implications

The present studies suggest that even if individuals are explicitly told that there is a high likelihood of being excluded (95%), this knowledge does not reduce the immediate negative impact of social exclusion. This has important implications for exclusion research as well as our understanding of everyday exclusion experiences.

First, for exclusion researchers, the absence of buffering effects of expectation implies that participants do not need to be completely naïve, but may, for instance, be familiar with exclusion paradigms or the study procedure. This is particularly important given that many studies rely on student samples or professional participants (e.g., MTurk), who frequently participate in studies and may be familiar with specific studies' content. Based on our observations in the present research, one may thus conclude that exclusion researchers do not need to be overly concerned about participants' prior expectations.

Second, whereas bracing for the worst might be an adaptive strategy to reduce the negative impact of adverse outcomes in other domains (e.g., Mellers et al., 1997), it seems not effective in the context of exclusion. Put differently, even knowing that being excluded is the most likely outcome does not reduce its adverse immediate psychological effects. That might also be the reason why individuals who experience chronic social exclusion most often do not just get used to it and develop effective strategies to buffer the pain of social exclusion, but rather end up in resignation and often suffer from severe mental-health problems (Lev-Wiesel et al., 2006, Rudert et al., 2021).

Nowadays, exclusion from a group is generally not life-threatening. The current findings illustrate why adhering to a norm of inclusion still seems indispensable: Social exclusion is no less threatening when individuals are mentally prepared for it. The pervasive threat of exclusion, even for those instances that occur expectedly, is all the more important when thinking about people who are frequently faced with exclusion due to discrimination. That is, one might assume that some people develop expectations about future exclusion based on, for instance, prior experiences with exclusion due to sexism, racism, and other forms of discrimination (e.g., Madera & Hebl, 2013). Our research suggests that regardless of these expectations, every instance of exclusion hurts anew, thus once more emphasizing the importance of inclusiveness.

Conclusion

Our research provides no evidence that expectations can buffer the immediate adverse effect of social exclusion to a meaningful extent, as expected exclusion is similarly painful as unexpected exclusion. Thus, the very moment you realize that your colleagues did not invite you to a joint social activity may hurt to the same extent, regardless of whether you have seen it coming or not. This finding affords new conceptual insights and closes a gap in prior research, as to date, heterogeneous evidence about the possibility of bracing for the worst in the context of social exclusion had been reported.

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Table 1

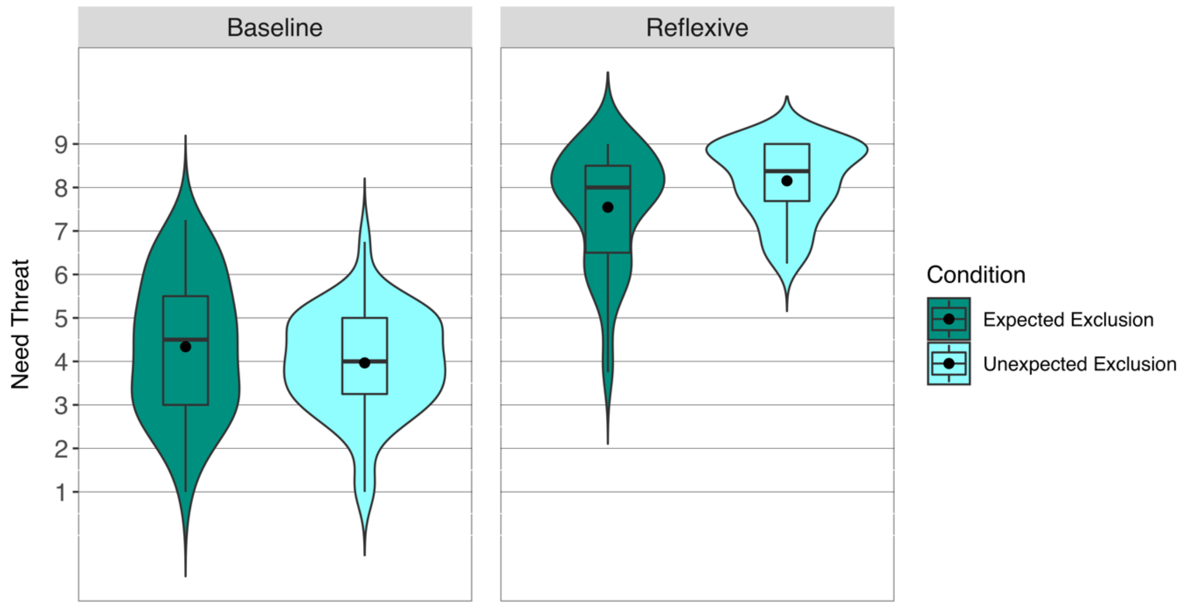
Means and Standard Deviations (in Parentheses) of all Variables Assessed Before and After Cyberball in Study 1.

	Condition	
Before Cyberball	Expected Exclusion (n = 49)	Unexpected Exclusion (n = 44)
Baseline Need Threat (α = .86)	4.34 (1.57)	3.97 (1.15)
Mood	6.26 (1.73)	6.86 (1.72)
Expected Throws	5.74 (5.98)	10.43 (3.05)
After Cyberball		
Reflexive Need Threat (α = .89)	7.55 (1.33)	8.15 (0.86)
Mood	3.51 (1.95)	3.25 (1.94)
Perceived Throws	2.02 (0.25)	2.07 (0.62)
Perceived Participation	2.88 (2.04)	2.64 (1.60)
"To what extent did you believe that you will actually be excluded by your co-players?"	6.45 (2.38)	–

Note. Expected and perceived throws are indicated as absolute numbers. All other measures ranged from 1 to 9; higher scores indicate higher levels of need threat/better mood/more participation/stronger beliefs.

Figure 1

Distribution of Need Threat in Participants that Expected Versus did not Expect Exclusion Across Time in Study 1



Note. The width of the violin plots represents the distribution of data points per condition. Black dots in the violin plot indicate the arithmetic mean, horizontal lines indicate the median. $N = 93$.

Table 2

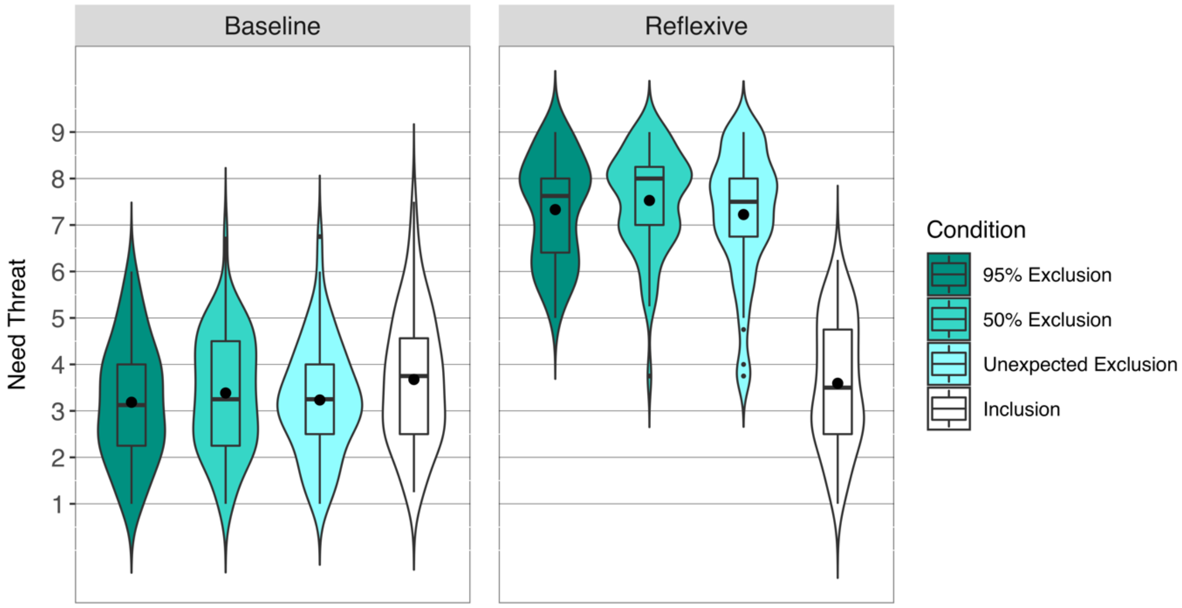
Means and Standard Deviations (in Parentheses) of all Variables Assessed Before and after Cyberball in Study 2.

	Condition			
Before Cyberball	Inclusion (n = 64)	Unexpected Exclusion (n = 65)	50% Exclusion (n = 63)	95% Exclusion (n = 66)
Baseline Need Threat (α = .85)	3.68 (1.43)	3.23 (1.21)	3.38 (1.25)	3.19 (1.27)
Baseline Mood	6.67 (1.61)	7.35 (1.46)	7.30 (1.45)	7.38 (1.60)
Subjective Likelihood of Exclusion	–	–	48.10 (16.89)	66.64 (27.88)
Certainty about Correctness of Likelihood Estimate	–	–	5.92 (2.14)	5.61 (2.15)
Certainty General	–	–	4.35 (2.20)	5.02 (2.26)
After Cyberball				
Reflexive Need Threat (α = .94)	3.59 (1.36)	7.23 (1.37)	7.53 (1.07)	7.33 (1.12)
Reflexive Mood	6.91 (1.35)	3.82 (2.24)	3.39 (1.74)	4.20 (1.92)
Perceived Throws	9.98 (3.04)	2.06 (0.61)	2.22 (0.68)	2.03 (0.35)
Perceived Participation	6.61 (1.45)	2.42 (1.03)	3.06 (1.81)	3.08 (1.93)

Note. Perceived throws are indicated as absolute numbers. All other measures ranged from 1 to 9; higher scores indicate higher levels of need threat/better mood/more participation/higher certainty.

Figure 2

Distribution of Need Threat in all Conditions Across Time in Study 2



Note. The width of the violin plots represents the distribution of data points per condition. Black dots in the violin plot indicate the arithmetic mean, horizontal lines indicate the median. $N = 258$.

Table 3

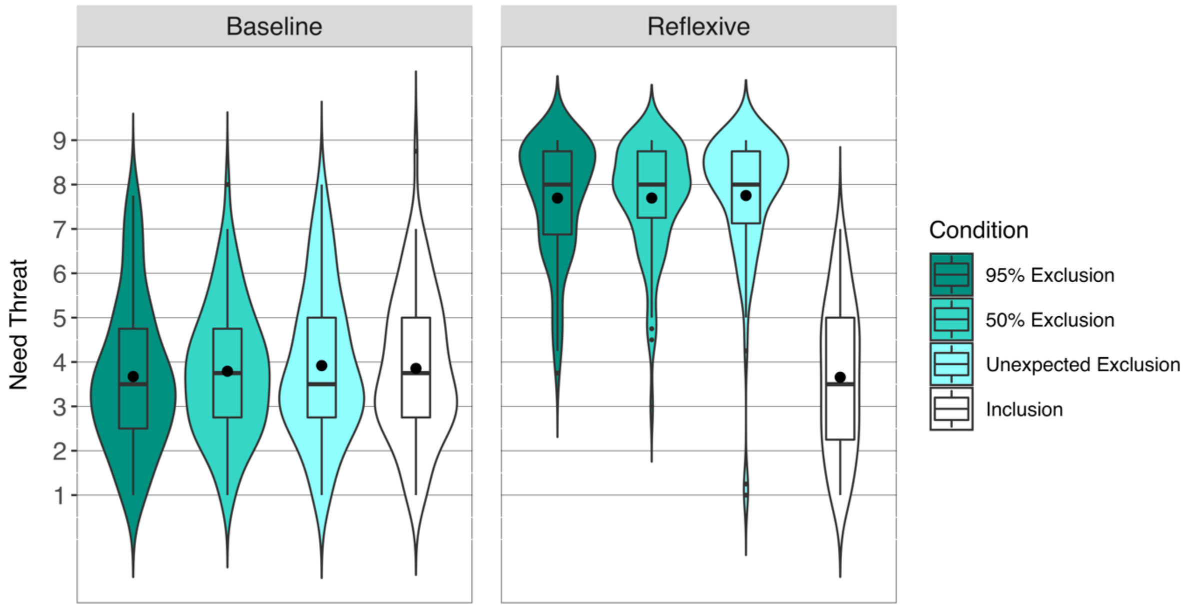
Means and Standard Deviations (in Parentheses) for all Variables Assessed Before and after Cyberball in Study 3.

	Condition			
Before Cyberball	Inclusion (n = 83)	Unexpected Exclusion (n = 83)	50% Exclusion (n = 84)	95% Exclusion (n = 87)
Baseline Need Threat (α = .85)	3.86 (1.61)	3.92 (1.67)	3.79 (1.46)	3.67 (1.67)
Baseline Mood	6.57 (1.93)	6.90 (1.81)	6.90 (1.46)	6.68 (2.08)
Expected Throws	10.18 (2.39)	11.34 (4.88)	9.20 (4.23)	5.48 (4.50)
After Cyberball				
Reflexive Need Threat (α = .94)	3.66 (1.65)	7.75 (1.49)	7.70 (1.22)	7.70 (1.30)
Reflexive Mood	6.64 (1.75)	3.34 (2.04)	3.70 (1.94)	3.46 (1.89)
Perceived Throws	9.11 (1.68)	2.04 (0.65)	2.04 (0.39)	2.08 (0.41)
Perceived Participation	7.61 (1.32)	3.18 (2.38)	2.92 (1.96)	3.06 (2.00)

Note. Expected and perceived throws are indicated as absolute numbers. All other measures ranged from 1 to 9; higher scores indicate higher levels of need threat/better mood/more participation/stronger beliefs.

Figure 3

Distribution of Need Threat in all Conditions Across Time in Study 3



Note. The width of the violin plots represents the distribution of data points per condition. Black dots in the violin plot indicate the arithmetic mean, horizontal lines indicate the median. *N* = 337.

Table 4

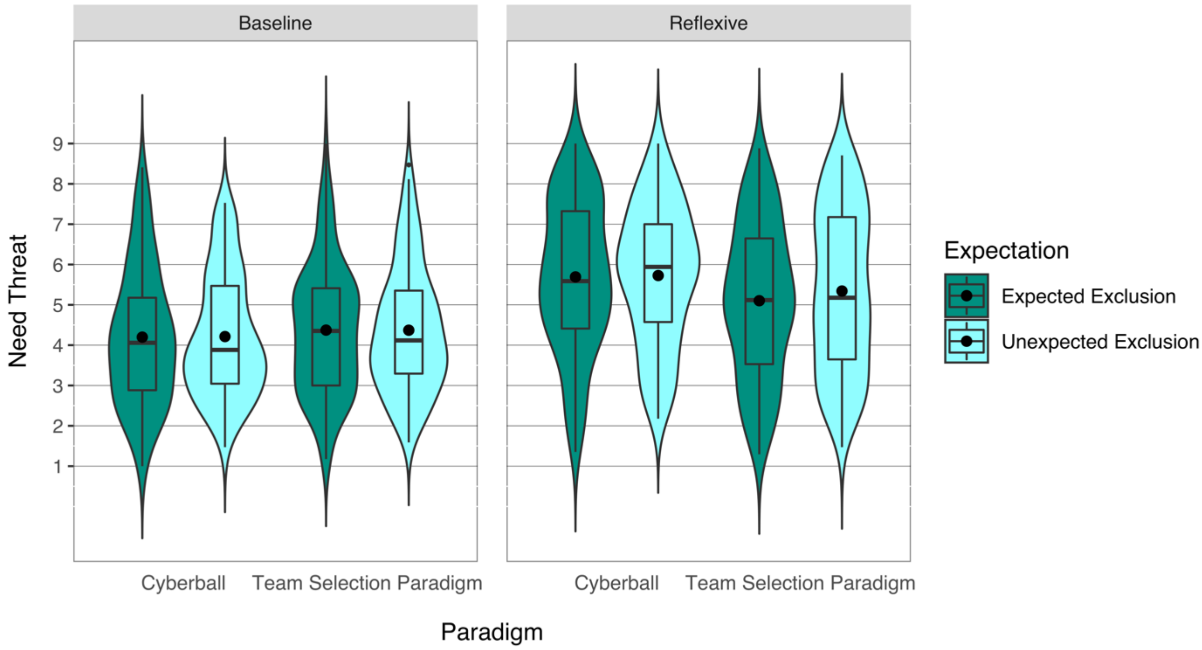
Means and Standard Deviations (in Parentheses) of Variables Assessed Before and After Exclusion in Study 4.

	Condition			
	Cyberball		Team Selection Paradigm	
Before Cyberball	Expected Exclusion (n = 111)	Unexpected Exclusion (n = 104)	Expected Exclusion (n = 121)	Unexpected Exclusion (n = 135)
Need Threat (20-item scale)	4.20 (1.70)	4.21 (1.51)	4.37 (1.61)	4.37 (1.55)
Need Threat (4-item scale)	5.08 (2.30)	5.19 (2.26)	5.20 (2.04)	4.89 (2.06)
Negative Affect	6.86 (1.71)	6.77 (1.51)	6.79 (1.45)	6.61 (1.53)
Expected Likelihood of Exclusion	79.89 (26.08)	–	69.78 (27.02)	–
After Cyberball				
Need Threat (20-item scale)	5.69(1.87)	5.73 (1.72)	5.10 (1.91)	5.34 (1.72)
Need Threat (4-item scale)	6.23 (2.17)	6.35 (1.98)	5.91 (2.16)	6.25 (2.19)
Negative Affect	5.53 (2.11)	5.35 (1.94)	6.0 (1.86)	5.72 (1.97)
Injunctive Norm	1.42 (0.69)	1.48 (0.76)	3.15 (1.56)	3.49 (1.66)
Descriptive Norm	2.52 (1.39)	2.42 (1.27)	4.06 (1.69)	4.36 (1.45)

Note. All scales ranged from 1 to 9; higher scores indicate higher levels of need threat and negative mood. The expected likelihood of exclusion ranged from 0%-100%.

Figure 4

Distribution of Need Threat in all Conditions Across Time in Study 4



Note. The width of the violin plots represents the distribution of data points per condition. Black dots in the violin plot indicate the arithmetic mean, horizontal lines indicate the median. $N = 471$.