Supplementary Material for

Raising awareness about social exclusion in schools through experiential learning

# Friend & Foe

The current social exclusion program was part of a larger project named *Friend & Foe* (original name in Dutch: *Vriend en Vijand*[[1]](#footnote-1)) developed by *Critical Mass*. This non-profit uses exhibitions and multimedia games to raise awareness and provide insights on social exclusion, prejudice, bullying, social influence, and conflict escalation. For the social exclusion part of this program the second author provided input based on previous work on social exclusion in social psychology (e.g., on how to induce feelings of social exclusion or how to measure the effects of social exclusion and so on). The final decisions in terms of the questions and the procedure were made by the non-profit organization and the overall framework of the program was designed by the organization as well.

# How Did We Clean the Data?

First, we removed participants from sessions with less than three and more than six participants because the game can only be played with three to six players (removed *n* = 1,054, remaining 17,801). Second, upon communication with the non-profit organization, we learned that the data also included the test runs and the data from teachers who sometimes also played the game. To remove people who were not participants of the program (e.g., teachers) and the test runs we applied a conservative age restriction and limited the final data set to participants between and including the ages of 12 and 19. Twelve is the start age of secondary school in the Netherlands, and 19 is an age when most people would have finished secondary school. This left us with only the participants who indicated being between the ages of 12 and 19 (removed *n* = 2,438). Third, we removed the participants who responded with insufficient effort. We relied on a very high or a very low-intra-individual response variability score (IRV: Dunn et al., 2018) as an indication of responding with insufficient effort. Very low IRV scores indicate responding to all or most items with the same value. Very high IRV scores, on the other hand, indicate extreme variability in responses (e.g., giving the lowest possible score to two items and highest possible score to the other two items). We calculated the IRV on the main dependent variable (i.e., need threat measure with four items, see more information in the materials section) because this measure has both negatively and positively worded items and is our main outcome variable in the game dataset. Having a very low IRV score based on this measure would indicate giving the same score to items that are reverse-worded, i.e., no response variability. Whereas, having a very high IRV score would mean indicating the lowest rating (1) on two of the items and the highest rating (7) on the other two, i.e., extreme response variability. To approach the data cleaning conservatively, we removed participants who had both very high and very low IRV scores (*n* = 1,298). We calculated the IRVs with the “careless” package in R (Yentes & Wilhelm, 2021).

# Social Exclusion vs Social Inclusion

In the program, we focused only on the experience of being socially excluded. We did not focus on being socially included. This makes sense from the perspective of a training program carried out by a non-profit organization. Most studies relying on Cyberball to manipulate social exclusion compare the effects of an exclusion condition to an inclusion condition in which participants get an equal number of ball tosses. Such a comparison was out of scope for the current project. Yet, we reasoned that the descriptive statistics of need threat values could benefit from some comparison to values that would be obtained if there was an inclusion condition. To achieve that and to provide more context to otherwise difficult to interpret absolute values, we compared observed descriptive statistics on need threat to previous work also conducted in the Netherlands. We identified two such examples (de Waal-Andrews & van Beest, 2012; Sleegers et al., 2016) that use Cyberball and measure need threat with the 20-item Need Threat Questionnaire (van Beest & Williams, 2006) that our questions were based on. In both studies, like the results presented here, the overall need threat scores were higher than the midpoint of the scale. We could not compare anger scores with these two studies because they do not report the results pertaining to anger separately as we do in this project. Yet work we used when making our analysis plan pertaining to anger shows similar values for levels of anger as we observe here (Rajchert et al., 2017; Svetieva et al., 2016; Zadro et al., 2004). Although we cannot compare the experience of being excluded to that of being included, we reasoned that discussing the descriptive statistics from other studies may help contextualize the absolute values we have and highlight that these scores are mostly in line with what one would observe in studies with similar designs (and studies in a similar context, i.e., the Netherlands, in the case of need threat).

# The Evaluation Questionnaire

A subgroup of the participants evaluated the training project that was carried out by *Critical Mass*. This evaluation questionnaire asked participants to evaluate the specific training programs (e.g., social exclusion training or prejudice training) and the overall project in its entirety (e.g., whether the discussion sections were useful in general, or whether they have used their knowledge and so on). In the current manuscript, we reported 12 items that are relevant to the social exclusion training program. Three of these 12 items are specifically about the social exclusion training program (e.g., three questions about gaining insights about social exclusion). The remaining nine items are about the training project in general. That is, participants were asked to evaluate the project in its entirety without referring to any specific training program. This means that these nine items not only tap into the social exclusion training but also training programs on other phenomena (e.g., bullying). We decided to discuss these more general questions here for two reasons. First, the fact that these items are general means that they tap into also the experience in the social exclusion training program. Any questions in the evaluation questionnaire that were solely about another training were left out from this manuscript. Second, albeit general in the current use case, these questions can be used specifically for social exclusion training programs in the future.

# Cross-cutting Factors

A side goal in the current contribution was to test a core assumption of a major model of social exclusion. According to the Temporal Need Threat (TNT) model of ostracism, the initial hurt of social exclusion is robust to cross-cutting variables (Williams, 2009). This is an ongoing field of inquiry and while some studies support this claim in terms of need threat (e.g., Gonsalkorale & Williams, 2007; van Beest et al., 2011; Zadro et al., 2004), some others suggest that the immediate hurt of social exclusion may indeed be prone to moderation (e.g., Bernstein et al., 2010; Eck et al., 2017; Goodwin et al., 2010; van Beest et al., 2011). Previous investigations of cross-cutting factors incorporate factors such as gender (Hawes et al., 2012), age (Abrams et al., 2011; Pharo et al., 2011), size of the group in which the exclusion takes place (Hartgerink et al., 2015; Tobin et al., 2018), or race (Gonsalkorale & Williams, 2007; Goodwin et al., 2010; Mulvey et al., 2016) to name a few. Therefore, we reported our results also in a way that could speak to this debate about the cross-cutting variables by testing whether age, gender, the selected avatar, or group size influences the extent to which participants experience need threat following exclusion.

Relevant to note is that the hypothesis that ostracism is not moderated by cross-cutting variables is often tested in a context where people are both included and ostracized. Findings then show that a cross-cutting variable has less impact on the participants in the ostracism condition than in the inclusion condition. In the current context, we could not make that comparison as the experimental learning is focused on how people deal with exclusion, it was not focused on how people deal with inclusion. Hence, our finding may or may not show that a cross-cutting variable moderated the experience of exclusion. However, it will not show whether this lack of moderation is stronger or weaker in the exclusion condition than the inclusion condition. Moreover, because we evaluate this core assumption within the context of an experiential training program involving 14,065 participants, we decided to test this assumption from the perspective of whether an effect has practical relevance and not whether an effect is statistically significant. In short, we decided to follow the advice of Ferguson (2009) and only consider statistically significant findings relevant if the associated effect size would be equal or greater than a Cohen’s *d* = 0.41 (or *r* = .20).

## 5.1. Results on Cross-Cutting Factors

### 5.1.1. Does gender of the participant influence need threat?

Participant gender affected the reported need threat after the game. Male participants reported lower need threat (*M* = 4.45 *SD* = 1.44) than female participants, (*M* = 4.71, *SD* = 1.25), *t*(13,464), = -11.64, *p* < .001, *d* = -.20, 95% CI [-.23, -.16]. That is, female participants experienced more need threat (e.g., less belonging and control) than their male peers. Participant gender did not change reported anger, *t*(13,700) = 1.80, *p* = .07 *d* = .03, 95% CI [.00, .06]. Although statistically significant, the effect of gender on need threat falls short of what we deem practically significant in the current project (d = .41, Ferguson, 2009).

### 5.1.2. Does age influence need threat?

Participant age was statistically correlated with overall need threat *r* = -.03, *p* < .001, and anger *r* = -.04, *p* < .001. Note that both correlations are not practically relevant (i.e., *r* < .20) and we would thus be hesitant to conclude that older participants in our sample are indeed less negatively affected by exclusion than younger participants.

### 5.1.3. Does avatar choice influence need threat?

Participants were able to select an avatar from a pool of 6 avatars (see Table 2, for descriptive statistics). One-way ANOVA’s with six levels (avatars 1 to 6) showed that avatar selection had a significant effect on need threat, *F*(5, 14,059) = 19.64, *p* < .001, = .007, and anger *F*(5, 14,059) = 5.94, *p* < .001, = .002, Due to the negligible effect sizes, we did not follow-up these effects with post-hoc analyses.

**Table S.1.**

*Selection of available avatars with corresponding descriptive statistics*

|  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- |
| #Avatar | 1 | 2 | 3 | 4 | 5 | 6 |
|  | C:\Users\u1260033\AppData\Local\Microsoft\Windows\Temporary Internet Files\Content.Word\RegularM.PNG | BeanieM | C:\Users\u1260033\AppData\Local\Microsoft\Windows\Temporary Internet Files\Content.Word\MohawkM.PNG | C:\Users\u1260033\AppData\Local\Microsoft\Windows\Temporary Internet Files\Content.Word\RegularF.PNG | Long-hairF | HijabF |
| *n* per avatar | 2,218 | 2,347 | 1,762 | 2,714 | 3,856 | 1,168 |
| Need threat  *M*(*SD*) | 4.45 (1.38) | 4.43  (1.42) | 4.56 (1.44) | 4.72 (1.25) | 4.68 (1.25) | 4.54 (1.50) |
| Anger *M*(*SD*) | 2.45 (1.97) | 2.52 (2.01) | 2.64 (2.12) | 2.51 (1.89) | 2.47 (1.90) | 2.77 (2.20) |

*Note.* #Avatar indicates the avatar number as displayed in the data.

### 5.1.4. Does group size influence need threat and anger?

Participants played the game in groups varying in size between 3 to 6. We investigated whether the size of the group influenced need threat and anger. Group size was statistically correlated with overall need threat *r* = -.027, *p* = .001, and anger, *r* = -.018, *p* = .03. Yet, like other cross-cutting variables we investigated, the size of the effect was again lower than our practical relevance threshold (*r* = .20). Hence, we conclude that group size is not relevant for need threat and anger in this sample.

## 5.2. Discussion of Cross-Cutting Factors

Finally, as a side goal we tested the assumption of Temporal Need-Threat Model of Ostracism (Williams, 2009) that the immediate hurt of social exclusion is impervious to cross-cutting variables. Put differently, this model would assume that age, gender, group size and avatar choice should not moderate experienced need threat and anger. Our analyses provide mixed support for this core assumption. One the one hand, we provide statistical evidence that age, gender and avatar choice did moderate need threat (but not anger). However, the crucial issue here is that we had a very large sample, and that it thus important to account for that and thus rely on whether an effect has practical relevance instead of relying solely on the resulting *p* values. From this perspective, the core assumption of the model is supported as neither age or gender, group size, or avatar choice moderated need threat or anger at the level it becomes practically relevant, a Cohen’s *d* = .41*,* *r* = .20 (Ferguson, 2009).

Should practitioners not care about these cross-cutting variables? We recommend otherwise. There is previous work that relies on a broader operationalization of both social exclusion (e.g., peer rejection) and reactions to social exclusion (e.g., distress or delayed responses) that shows moderation effects of variables such as age and gender (Beeri & Lev-Wiesel, 2012; Reijntjes et al., 2006). In the current paper we acknowledge these inconsistencies in the literature with regards to how people feel about social exclusion depending on certain characteristics. At the same time, we do not see this inconsistency in prior findings as a problem. After all, the training program does not only assess the immediate hurt, but it also includes a moment to actively discuss what happened to participants. This discussion is thus more attuned to reflection and prior research has provided consistent findings that upon reflection key cross-cutting variables to impact how people cope with the initial hurt of ostracism (e.g., Hartgerink et al., 2015; Ren et al., 2013; Yaakobi et al., 2021; Zadro et al., 2006). In other words, even though our analysis supports the Temporal Model of Ostracism (Williams, 2009) that the initial hurt is not substantially altered by cross-cutting variables, it is quite likely that they might when people are reflecting upon the experience. We thus recommend that, within the context of this program, the feeling of exclusion induced by the ball-tossing game should only be used as a steppingstone for future discussion. We suggest that when building upon these feelings of exclusion practitioners or researchers can think about the different ways in which this program can be adapted to the specific needs and characteristics of their target population. More specifically, we recommend relying on the large body of work investigating how people experience social exclusion or other relevant experiences (e.g., Smart Richman & Leary, 2009; Wesselmann et al., 2016) based on their identity or group membership to enrich the program and make it more useful for their special use case (e.g., Beeri & Lev-Wiesel, 2012; DeSouza et al., 2017, 2019; Goodwin et al., 2010; Hawes et al., 2012; Killen & Stangor, 2001; London et al., 2012; Lopez & DuBois, 2005; Mendoza-Denton et al., 2002).

We would like to draw the reader’s attention to the results pertaining to the gender factor to illustrate a point. In the current work, the effect of gender on need threat (*d* = -.20) fell below our threshold (*d* = .41) of practical relevance. Yet scholars have urged other scholars to not use these thresholds very strictly, and instead asked them to consider the context and the methodology of any project while making decisions surrounding practical relevance and significance (e.g., Ferguson, 2009; Funder & Ozer, 2019). This is relevant given that if one were to decide on a lower threshold for practical relevance (e.g., Funder & Ozer, 2019), they would have concluded that gender had a significant (and practically relevant) effect on post-game measures. Such inconsistencies are to occur given that the relationship between practical and statistical significance have long been an ongoing debate (e.g., Ferguson, 2009; Funder & Ozer, 2019). What we aimed to do here was to be transparent about our decision. We hope that this will enable future researchers and practitioners to evaluate the evidential value of our claims and make an informed judgment about our results and interpretations.

The non-profit organization designed the game with graphics and avatars with the idea that this would increase the involvement of participants within the game. As rightly pointed out by one of the reviewers of the current contribution, however, this could also inadvertently make some participants feel excluded. Even though there is a certain range of avatars included in the game, the number is still relatively small. If a participant felt like none of the avatars represented them well enough, this may have had a negative impact on their involvement with the game, rather than increasing it. In fact, previous work shows that player-avatar identification is positively related to game enjoyment (Trepte & Reinecke, 2010) and can positively increase how the game can relate to a person's beliefs in areas similar to the content of the game (Song & Fox, 2016). This issue pertaining to participants feeling left out due to avatar selection can potentially be resolved in multiple ways. First, future users or developers of paradigms as the one we report on here can include a larger array of avatars or offer more avatar customization features. This can allow participants to feel more identification with their avatars and help practitioners achieve the goals of the programs easier. Alternatively, researchers or practitioners can use programs that do not rely on avatar choice either by using a more minimal version of the ball-tossing game (Williams & Jarvis, 2006), or relying on paradigms in which participants tap into their own experiences (such as recalling past instances of social exclusion, Chen et al., 2008), or role-play certain exclusion scenarios (Zadro et al., 2005).

# The Original Items Used in The Questionnaires

## 6.1. Reflection questions / Immediate need satisfaction questions

1. Ik had tijdens het spel het gevoel dat ik erbij hoorde (*I had the feeling that I was belonging during the game*,).
2. Ik voelde mij onzichtbaar tijdens het spel (During the game, I felt invisible).
3. Ik voelde me goed tijdens het spel (During the game, I felt good).
4. Ik had het gevoel dat ik de baas was over het spel (I had the feeling that I was the boss of the game).
5. "Ik voelde me boos tijdens het spel (During the game I felt angry).

## 6.2. Discussion statements following the exclusion game

Students use these possible reactions and discuss what they could do in case they witness exclusion. Items are presented in Dutch (in their original form) and the English translations are presented within parentheses.

1. Er niet aan proberen te denken (You could try not to think about it).
2. Met andere mensen omgaan (You could simply start hanging out with other people).
3. Van je af bijten (You could stick up for yourself).
4. Hulp vragen aan leerkracht/iemand in je omgeving (You could ask for help from a teacher/someone around you).
5. Erover praten met vrienden en/of ouders (You could talk about it with friends and/or parents).
6. Vragen waarom je wordt buitengesloten (You could ask why you were left out).

## 6.3. Evaluation Questionnaire Items

Items from Table 1 in Dutch (the original version), the English versions are between parentheses).

1. Ik ben aan het denken gezet over buitensluiting (I got to think about social exclusion).
2. Ik heb inzicht gekregen in het effect van buitensluiting (I have gained insights into the effect of being socially excluded).
3. Ik heb inzicht gekregen in mijn eigen rol in situaties voor mensen worden buitengesloten (I have gained insight into my own role in situations where people get socially excluded).
4. De nabespreking les was leuk (The discussion session was nice).
5. De nabespreking les was nuttig (The discussion session was useful).
6. De nabespreking les was onzinnig (The discussion session was nonsensical).
7. De nabespreking les was interessant (The discussion session was interesting).
8. In de nabespreking les hebben we onze ervaringen in de containers gedeeld (In the discussion session we shared our experiences in the containers).
9. In de nabespreking les hebben we besproken hoe de sfeer in onze klaas is (In the discussion session we discussed the environment in our class).
10. In de nabespreking les hebben we (nieuwe) afspraken gemaakt over hoe we met elkaar omgaan (In the discussion session we made (new) agreements about how we treat each other).
11. In de nabespreking les hebben we verder nagedacht over vooroordelen (In the discussion session we further reflected on prejudice).
12. De begeleider stelde vragen waardoor ik aan het denken gezet (The facilitator made me think).

The original versions (in Dutch) of items in Figure 4. The items appear in the same order as they do so in the figure.

1. Na de expeditie [Hoe vaak heb je het er met klasgenoten over gehad?] (After the expedition [How often did you talk about it with classmates?])
2. Na de expeditie [Hoe vaak heb je het er thuis over gehad (met bijv. je ouders, broer of zus)?] (After the expedition [How often did you talk about it at home (e.g., with your parents, siblings)?)
3. Na de expeditie [Hoe vaak heb je het er met vrienden buiten school (bijv. van de sportclub) over gehad?] (​​After the expedition [How often did you talk about it with friends outside of school (e.g., from the sports club)?)

Questions about applying the knowledge to real life.

1. ​​Heb je sinds het bezoek aan de containers een vervelende situatie (zoals buitensluiting, pesten, etc.) gezien? (1 = *Nee*, 2 = *Ja*) (Have you seen an unpleasant situation (such as social exclusion, bullying, etc.) since the visit to the containers)
2. Heb je toen iets anders gedaan dan je vóór Expeditie VRIEND&VIJAND zou hebben gedaan? (1 = *Nee*, 2 = *Ja*) (Did you do anything different then than you would have done before the expedition FRIEND&VIJAND?)

\*Note: The non-profit organization called the training sessions an “expedition (expeditie in Dutch).” In the translations here, we remained close to the original choice of words and also used “expedition.” Yet, in the manuscript we always refer to it as a training program.

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