Does the Prospect of Fulfilling Belonging affect Social Responses to Rejection?

A Conceptual Replication Attempt

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Abstract

Historically, one of the biggest paradoxes in the rejection literature was that rejected people sometimes behaved antisocially and at other times prosocially. Researchers responded to this paradox by demonstrating that the prospect of fulfilling belonging predicted these varied responses. A close inspection of the foundational studies, however, revealed small sample sizes relative to today's standards. Hence, we conducted a conceptual replication of the prospect of fulfilling belonging hypothesis using a high-powered two-day preregistered study. On Day 1, we manipulated the prospect of fulfilling belonging through a profile exchange with a confederate. On Day 2, we manipulated rejection using a recall paradigm and then measured prosocial and antisocial responses towards the confederate. Rejected participants who expected the confederate to fulfill belonging showed similar levels of prosocial and antisocial responses compared with those who did not. These results suggest that the prospect of fulfilling belonging hypothesis needs further refinement.

Keywords: prosocial behavior; antisocial behavior; need to belong; ostracism; relational devaluation

Does the Prospect of Fulfilling Belonging affect Social Responses to Rejection?:

A Conceptual Replication Attempt

How do people react to an acute episode of interpersonal rejection? Existing evidence suggests that rejected people respond in both *prosocial* and *antisocial* ways, having positive and negative impacts to belonging respectively (e.g., Bernstein, Young, Brown, Sacco, & Claypool, 2008; DeWall, Twenge, Bushman, Im, & Williams, 2010; Twenge, Baumeister, Tice, & Stucke, 2001; Williams, Cheung, & Choi, 2000). Sometimes, rejected people try to replenish belonging through prosocial responses, such as cooperating in group tasks and conforming to others (Williams et al., 2000; Williams & Sommer, 1997). At other times, rejected people show antisocial responses that would damage belonging, such as blasting louder noise at another person and derogating others (Twenge et al., 2001; Warburton, Williams, & Cairns, 2006). For years, these findings represented a paradox in the rejection literature because prosocial and antisocial responses are opposing behaviors with contradictory effects on belonging.

The Prospect of Fulfilling Belonging Hypothesis

Relatively recent theoretical and empirical advances proposed *the prospect of fulfilling belonging hypothesis* to explain why rejection could lead to both antisocial and prosocial behavior (DeWall & Richman, 2011; Maner, DeWall, Baumeister, & Schaller, 2007). This hypothesis suggests that, after a person is rejected, if they expect their subsequent interaction partner to fulfill their belonging, they will behave prosocially towards that person. On the other hand, if they don't expect their partner to fulfill their belonging, they will behave antisocially. A series of studies supported this hypothesis—for example, rejected participants allocated more money to a new study partner than to the rejection perpetrator, presumably because a new study partner offered a greater likelihood of fulfilling belonging than the rejection perpetrator (Maner

et al., 2007, Study 5). These findings were supported across 5 studies, providing empirical

evidence for the prospect of fulfilling belonging hypothesis, which became an influential

cornerstone in the rejection literature (cited 862 times as of January 2018). Other research has

also supported the prospect of fulfilling belonging hypothesis (e.g., Romero-Canyas et al., 2010;

Twenge et al., 2001), and subsequent theoretical work relied on this hypothesis as a foundation

for explaining antisocial and prosocial behavior to belonging threats (e.g., DeWall & Richman,

2011; Richman & Leary, 2009; Shilling & Brown, 2015).

Need for a Replication

A close inspection of the foundational studies supporting the prospect of fulfilling

belonging hypothesis reveals small sample sizes for today's standards (i.e., <20 observations per

cell; Simmons, Nelson, & Simonsohn, 2011). For example, a 5-group design study had 47

participants (9–10 per cell), and another had 49 participants in a 2×2 between-subjects design

(12–13 per cell; Maner et al., 2007, Study 5; Twenge et al., 2001, Experiment 1). Small-sample

studies tend to be underpowered, difficult to replicate, and prone to false positives (Type I error;

Fraley & Vazire, 2014; Kühberger, Fritz, & Scherndl, 2014). Although it is possible that the true

effect sizes of prior work might have been large enough to be captured by small sample sizes,

often the effect sizes of small-sample studies are inflated (Simonsohn, 2015). In fact, many

small-sample studies in this literature have relatively large effect sizes (e.g., ds of 1.29 and 1.59),

suggesting this possibility could be true (e.g., Maner et al., 2007, Study 5; Twenge et al., 2001,

Experiment 1). Furthermore, psychological theories should be conceptually replicated to avoid

conceptual Type I errors—acceptance of false theories (Crandall & Sherman, 2016), particularly

when those theories are influential to the field. For these reasons, combined with the importance

of this work within the rejection literature, we conducted a conceptual replication using a sample size that meets today's standards.

Current Study

The goal of the current study was to conduct a conceptual replication testing the prospect of fulfilling belonging hypothesis (DeWall & Richman, 2011; Maner et al., 2007). Specifically, we tested whether a higher vs. lower prospect of fulfilling belonging predicted prosocial vs. antisocial responses to rejection. To accomplish this goal, we designed a two-day study that manipulated the constructs implied by the original empirical and theoretical work. In this study, we treat rejection, ostracism, and social exclusion as related constructs that thwart belonging, and use the term *social rejection* to denote the unifying construct (see Leary, Twenge, & Quinlivan, 2006; Williams, 2009 for discussions about the similarities and differences between these overlapping constructs).

The study consisted of two visits (Day 1 and Day 2) separated by 1–7 days (*M*=6 days; see Figure 1 for a flowchart). The purpose of Day 1 was to establish background information about an ostensible study partner (i.e., a confederate), allowing participants to learn whether their study partner was likely to fulfill their belonging. To accomplish this goal, we manipulated the prospect of fulfilling belonging using a combination of two factors: participants' desires to affiliate with the confederate (participant desire), and the confederate's desire to affiliate with the participant (confederate desire). Together, both high participant and confederate desires signal that the confederate is likely to fulfill the participant's belonging. In other words, a person is only likely to feel connected to another person if they desire to be connected with that person, and they believe that desire is reciprocated. For a detailed discussion about both manipulations and how they relate to prior empirical and theoretical work, see Supplemental Materials, p.2.
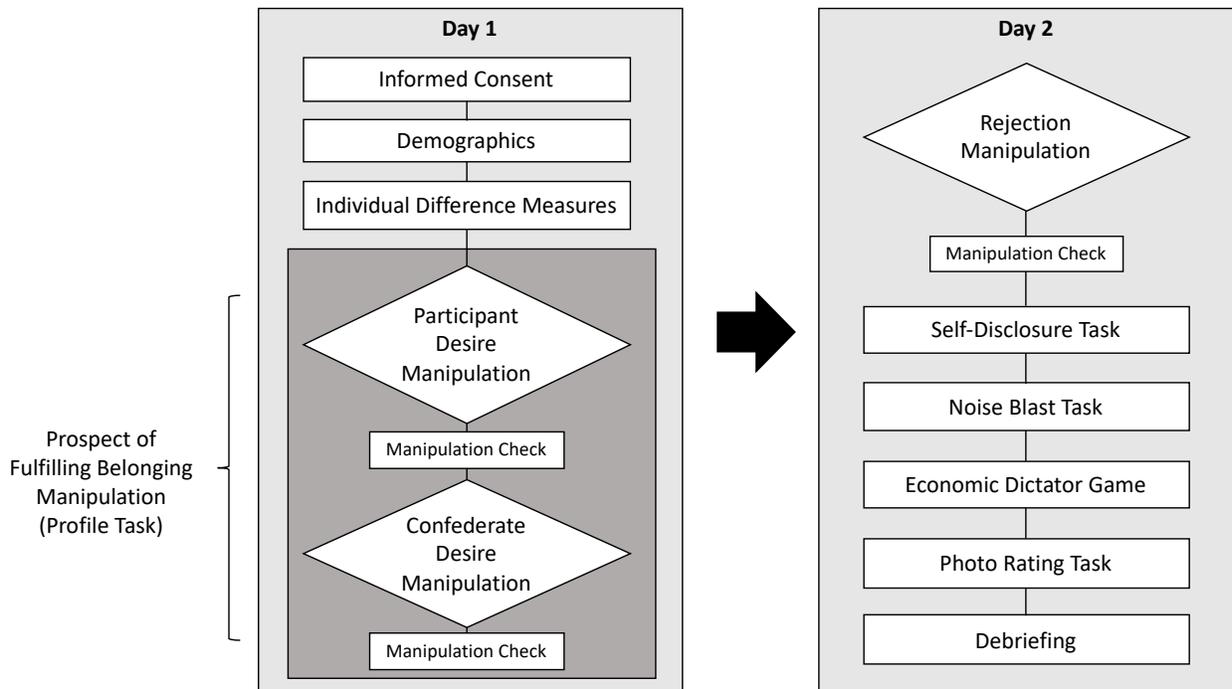
Figure 1. Flowchart showing the design of the current study. Rectangles represent key

procedures and measured variables. Diamonds represent manipulations. The profile task (the

dark grey rectangle) manipulated two key factors comprising the prospect of fulfilling belonging:

participant desires and confederate desires.

The purpose of Day 2 was to reject participants and measure subsequent social behavior towards the confederate, who was purposefully not involved in the Day 2 rejection. Rather, participants were asked to remember a past time in their life when they were rejected. This design allowed us to tightly control participants' perceived prospect of fulfilling belonging with the confederate via the background information about the confederate presented on Day 1 (manipulating confederate desire and participant desire), and separate this construct from the rejection experience on Day 2. Doing so also simulated a real-world experience of having pre-existing knowledge about a person (that we established on Day 1) that guides behaviors towards this person after being rejected by someone else (which we accomplished on Day 2). We then assessed prosocial vs. antisocial responses toward the confederate using a range of outcomes theoretically relevant to the prospect of fulfilling belonging: prosocial behavior that had the potential to fulfill belonging (a self-disclosure task), antisocial behavior that had the potential to thwart belonging (a noise blast task), and a combination of prosocial and antisocial behavior that ranged in its implications for belonging (an economic dictator game). In addition, we measured volunteer behavior to examine whether the effects of rejection extended to multiple forms of prosocial behavior, or only overtly affiliative prosocial behavior.

The current study examined two competing hypotheses:

- Hypothesis 1: Based on prior research, we expected an interaction between the prospect of fulfilling belonging (higher vs. lower participant desire; higher vs. lower confederate desire), and the rejection manipulation (rejection vs. control) predicting prosocial and antisocial behaviors. Specifically, participants should behave more prosocially (and less antisocially) towards a person they perceived as more likely to fulfill belonging (i.e., higher participant desire and higher confederate desire) relative to those less likely to fulfill belonging (i.e.,

lower participant desire and/or lower confederate desire). This inclination should be stronger among those in the rejection condition relative to those in the control condition.

- Hypothesis 2: Based on the possibility that the prospect of fulfilling belonging hypothesis would not replicate, we expected a null effect for the prospect of fulfilling belonging (higher vs. lower participant desire; higher vs. lower confederate desire) by rejection manipulation (rejection vs. control) interaction predicting prosocial and antisocial behaviors.

We also explored moderators previously suggested as affecting responses to social rejection. These moderators included gender, attachment anxiety, attachment avoidance, fear of negative evaluation, rejection sensitivity, and self-esteem (e.g., Ayduk, Gyurak, & Luerssen, 2008; DeWall et al., 2012; Ford & Collins, 2010; Maner et al., 2007; Williams & Sommer, 1997). The methods and results of these exploratory analyses are discussed in Supplemental Materials, pp.14–24.

Method

We pre-registered the hypotheses, procedures, and materials on Open Science Framework before data collection began. The preregistered materials (https://osf.io/xpr6b/), dataset (https://osf.io/myj4w/), and analysis codes (https://osf.io/ynptc/) are accessible online.

Sample Size Rationale and Study Design

The power analysis was done using the *pwr R* package (Champely et al., 2017). We wanted to ensure that null findings (Hypothesis 2) would be as informative as possible. Accordingly, we chose a small effect size per Cohen's guidelines ($f^2=.02$; Cohen, 1992). We set power to .90, higher than the conventional standard (.80) and chose an alpha of .05 by convention. These decisions minimized the Type II error rate, allowing us to be as confident as

possible in null findings. With these parameters, the required sample size was 533 to detect the participant desire × confederate desire × rejection 3-way interaction.

Participants

We recruited undergraduate students who were 18+ years old and self-identified as White. We targeted White participants to eliminate potential confounds arising from cross-race interactions (e.g., Mendes, Major, McCoy, & Blascovich, 2008). We continued recruitment until our final analytical sample reached the target sample size. Overall, 674 and 605 participants completed Day 1 and Day 2 respectively. We excluded 59 participants from the analyses because our debriefing coding procedure determined that they were suspicious (see Supplemental Materials, p.3, for a detailed description). Due to a recruitment error, eight non-White participants were in our dataset and were thus excluded. The final analytical sample consisted of 538 participants (349 women; 188 men; 1 transgender).

Procedure and Manipulations

Day 1. Participants were told that they were attending 2 visits with another participant matched based on availability. Participants also learned that they would complete an in-person interaction with this study partner (i.e., the confederate) on Day 2. In reality, this was a cover story, and no study partner existed. After completing background questionnaires, participants had their photo taken for a bogus profile exchange.

Next, participants completed a personal profile with questions about demographics (e.g., age) and personal preferences (e.g., favorite color). Participants then learned that they and the confederate would exchange profiles with each other. This profile exchange manipulated participant desire, operationalized as perceived compatibility with the confederate (higher vs. lower), since people desire affiliation with similar others (Montoya, Horton, & Kirchner, 2008).

In the higher participant desire condition, the confederate's profile was filled with answers

similar to participants' answers (e.g., if the participant liked dogs, they saw that their partner

liked dogs too). In addition, a pie chart on screen indicated that their profiles matched 80%. In

the lower participant desire condition, the confederate's profile was filled with answers different

from participants (e.g., if the participant liked dogs, they see the other person liked cats). A pie

chart on screen indicated that their profile matched 20%. Regardless of condition, participants

always saw a profile and picture of a same gender and same race person either named Jan

(female) or John (male).

After the profile exchange, participants completed the participant desire manipulation

check. Then, participants saw feedback from the confederate on how much the confederate

wanted to get to know them, manipulating confederate desire. In the lower confederate desire

condition, the confederate indicated an ambivalent desire to meet the participant (i.e., answering

"undecided" to a question about getting to know the participant). In the higher confederate desire

condition, the confederate indicated a higher desire to meet the participant (i.e., answering

"definitely" to a question about getting to know the participant). Next, participants completed the

confederate desire manipulation check. These manipulations provided participants with clear

information about whether the confederate was likely to fulfill belonging on Day 2 (for

additional information about these manipulations, see Supplemental Materials, p.2).

Day 2. On Day 2, participants saw the confederate's name and photo again, and then

learned that they would complete "getting-to-know-you" activities with the confederate before

the actual face-to-face interaction. Then, participants were introduced to the social rejection task.

The rejection and control manipulations were adopted verbatim from an autobiographical

recall paradigm previously published (Twenge & Campbell, 2003, Study 1). In the rejection

condition, participants wrote about a time when they felt rejected by a person or group around their own age (but not a romantic rejection, as per prior work). In the control condition, participants wrote about their experience yesterday morning. All participants spent 5 minutes writing the essay, and answered manipulation check questions.

Next, participants completed the outcome measures as ostensible getting-to-know-you activities with the confederate. The measures were presented in a fixed order: the self-disclosure task, followed by the noise blast task, the economic dictator game, and the photo rating task (see Supplemental Materials, pp.47, for correlations among measures and a rationale for the task order). Finally, participants completed a debriefing questionnaire probing for suspicion.

Measures: Day 1

We used two related sets of items for the participant desire manipulation check. First, we assessed perceived compatibility using three items, based on research demonstrating that people desire affiliation with compatible others (e.g., Montoya, Horton, & Kirchner, 2008; e.g., "My partner and I are a 'good match'"; α=.83). Second, we directly assessed participant desire using three items (e.g., "I am interested in getting to know my partner"; α=.95). We conducted a separate manipulation check for confederate desire using three items (e.g., "My partner seems like she/he is interested in getting to know me"; α=.97).[1]

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[1] We did not ask participants directly whether their study partner had a higher vs. lower prospect of fulfilling belonging as a manipulation check since the phrase "satisfying belonging" is uncommon in everyday language. Future studies may use a more direct manipulation check.

Measures: Day 2

Manipulation Check for Rejection Manipulation. We modified the belonging items from the need-threat scale (Williams, 2009). Participants answered four questions assessing degree of belonging need threat (e.g., "How rejected do you feel?"; α=.84).

Self-Disclosure Task. We chose a self-disclosure task as a prosocial outcome because self-disclosing increases perceived belonging with others (Aron, Melinat, Aron, Vallone, & Bator, 1997; Ko & Kuo, 2009). The measure included 20 questions previously ranked according to degree of self-disclosure (Aron et al., 1997). Participants chose five questions that would be used during the ostensible face-to-face interaction with the confederate at the end of Day 2. To calculate the self-disclosure score, we first used previously published information to rank the 20 items (e.g., 1=least self-disclosing item, 20=most self-disclosing item; Aron et al., 1997). We calculated the sum of the five items participants selected and used this index for the planned analysis (see Supplemental Materials, p.10, for an alternative composite).

Noise Blast Task. To measure antisocial responses, we adopted a bogus competitive reaction time task used previously (e.g., Twenge et al., 2001). Participants learned that they would complete a computer game with the confederate. During this game, whoever pressed a button quicker after seeing a square on the screen would blast white noise at their opponent. Before the first and only trial, participants determined the level and duration of the noise to be blasted at the confederate (in preparation for potentially being the quickest to press the button). The game was rigged, so that participants always tied. No actual noise was blasted, ostensibly because of the tie. Since participants committed to a noise intensity and duration that they believed would be blasted at the confederate, those choices represented antisocial behavior. The

intensity and duration of the noise blast were standardized and summed into an index for antisocial responding (Twenge et al., 2001).

Modified Economic Dictator Game. We simultaneously measured a range of prosocial and antisocial responses through a modified version of the economic dictator game, a widely used measure of punitive and affiliative responses (Bolton, Katok, & Zwick, 1998). Participants learned that they would be randomly assigned as either the decider or receiver in an ostensible monetary lottery game. The instructions stated that (a) the decider was in charge of dividing the lottery tickets between themselves and the receiver, and (b) the receiver would know that the decider was in charge. In reality, all participants became the decider and divided 9 tickets. The amount of tickets allocated toward the confederate vs. the self served as an index of prosocial vs. antisocial responding.

Photo Rating Task. We measured prosocial behavior that wasn't overtly affiliative by assessing participants' willingness to volunteer to rate photos for an unrelated study. Participants indicated the number of photos that they wished to rate, ranging from 0 (no volunteering) to 20 photos (maximal volunteering).

Analysis Strategy

The data analysis strategy was preregistered on OSF. We used Welch's *t*-tests for the manipulation checks, assuming heterogeneity of variance (Delacre, Lakens, & Leys, 2017). To test the competing hypotheses, we performed a 2 (Participant Desire: higher vs. lower) × 2 (Confederate Desire: higher vs. lower) × 2 (Rejection: rejection vs. control) between-subjects ANOVA for each dependent variable: the self-disclosure score, the noise blast index, the number of tickets allocated to the confederate, and the number of photos volunteered for rating.

Results

Manipulation Checks

The manipulation check analyses suggested that the manipulations were effective. People in the higher (vs. lower) participant desire condition saw the confederate as more compatible, $t(500.36)=-21.91$, $p<.001$, $d=-1.89$, 95%CI[-0.47,-0.12], and expressed more desire to get to know the confederate, $t(534.67)=-4.90$, $p<.001$, $d=-0.42$, 95%CI[-0.59, -0.25]. In addition, participants in the higher (vs. lower) confederate desire condition indicated that the confederate wanted to get to know them more, $t(536.00)=-40.61$, $p<.001$, $d=-3.50$, 95%CI[-3.77, -3.23]. Finally, participants in the rejection condition felt more rejected than those in the control condition, $t(513.33)=3.42$, $p<.001$, $d=-0.30$, 95%CI[-0.47,-0.12]. Degrees of freedom in these tests varied because we used Welch's t-tests.

Planned Analyses

Consistent with the competing Hypothesis 2, the 3-way participant desire × confederate desire × rejection interaction was non-significant for the following outcomes: self-disclosure scores, the noise blast index, and the number of tickets allocated, $F(1,530)=0.13$, $p=.723$, $\eta^2_p<.001$; $F(1,530)=0.26$, $p=.611$, $\eta^2_p<.001$; and $F(1,530)=1.21$, $p=.271$, $\eta^2_p=.002$ respectively. As described earlier, we operationally defined the prospect of fulfilling belonging in two ways (participant desire and confederate desire). Accordingly, to verify our null results, we examined the 2-way interaction between rejection and each of the two manipulations separately. The 2-way participant desire × rejection interaction did not predict self-disclosure scores, the noise blast index, or the number of tickets allocated, $F(1,530)=1.69$, $p=.195$, $\eta^2_p=.003$; $F(1,530)=0.16$, $p=.688$, $\eta^2_p<.001$; and $F(1,530)=0.09$, $p=.767$, $\eta^2_p<.001$ respectively. Similarly, the 2-way confederate desire × rejection interaction did not predict self-disclosure scores, the noise blast

index, or the number of tickets allocated, $F(1,530) = 0.64$, $p = .423$, $\eta^2_p = .001$; $F(1,530) = 1.70$, $p = .193$, $\eta^2_p = .003$; and $F(1,530) = 0.96$, $p = .327$, $\eta^2_p = .002$ respectively. Table 1 shows the descriptive statistics, and Table 2 summarizes the planned analyses. Figures 2, 3, and 4 show error bar plots across conditions. These results suggest that responses to rejection do not differ across targets with a higher vs. lower prospect of fulfilling belonging.

Table 1.

Descriptive Statistics by Condition

Participant Desire	Confederate Desire	Rejection	n	Self-Disclosure (sum)		Noise Blast Index		Tickets Allocated		Photos Volunteered to Rate	
				M	*SD*	*M*	*SD*	*M*	*SD*	*M*	*SD*
Lower	Lower	Control	59	51.68	(11.69)	-0.26	(1.50)	4.27	(1.05)	9.54	(6.11)
		Rejection	67	48.01	(11.24)	0.19	(1.88)	4.25	(0.96)	8.76	(6.80)
	Higher	Control	77	49.61	(14.19)	0.14	(2.11)	4.61	(1.07)	9.74	(6.40)
		Rejection	65	48.40	(12.09)	0.03	(1.84)	4.62	(1.38)	7.92	(5.68)
Higher	Lower	Control	71	47.75	(11.46)	-0.03	(1.73)	4.13	(0.96)	8.00	(6.28)
		Rejection	66	47.59	(12.42)	0.14	(1.49)	4.26	(1.01)	10.76	(6.92)
	Higher	Control	61	48.80	(10.85)	-0.09	(1.80)	4.67	(1.12)	10.16	(6.87)
		Rejection	72	49.60	(13.29)	-0.17	(1.64)	4.42	(1.00)	8.61	(6.22)

Table 2.

Summary of the Planned Analyses.

Outcome	Type	Predictor	F	p	η_p^2
Self-Disclosure	Main effect	Participant Desire	0.87	.351	.002
	Main effect	Confederate Desire	0.11	.745	< .001
	Main effect	Rejection	1.00	.319	.002
	2-way interaction	Participant Desire × Confederate Desire	1.25	.264	.002
	2-way interaction	Participant Desire × Rejection	1.69	.195	.003
	2-way interaction	Confederate Desire × Rejection	0.64	.423	.001
	3-way interaction	Participant Desire × Confederate Desire × Rejection	0.13	.723	< .001
Noise Blast Index	Main effect	Participant Desire	0.18	.674	< .001
	Main effect	Confederate Desire	0.04	.834	< .001
	Main effect	Rejection	0.47	.495	.001
	2-way interaction	Participant Desire × Confederate Desire	1.03	.311	.002
	2-way interaction	Participant Desire × Rejection	0.16	.688	< .001
	2-way interaction	Confederate Desire × Rejection	1.70	.193	.003
	3-way interaction	Participant Desire × Confederate Desire × Rejection	0.26	.611	< .001
Tickets Allocated	Main effect	Participant Desire	0.56	.455	.001
	Main effect	Confederate Desire	14.34	< .001	.026
	Main effect	Rejection	0.14	.712	<.001
	2-way interaction	Participant Desire × Confederate Desire	< 0.01	.992	<.001
	2-way interaction	Participant Desire × Rejection	0.09	.763	<.001
	2-way interaction	Confederate Desire × Rejection	0.96	.327	.002
	3-way interaction	Participant Desire × Confederate Desire × Rejection	1.21	.271	.002
	Main effect	Participant Desire	0.50	.481	.001
	Main effect	Confederate Desire	0.08	.779	< .001

Photos	Main effect	Rejection	0.39	.531	.001
Volunteered	2-way interaction	Participant Desire × Confederate Desire	0.09	.767	< .001
to Rate	2-way interaction	Participant Desire × Rejection	2.93	.087	.005
	2-way interaction	Confederate Desire × Rejection	5.79	.016	.011
	3-way interaction	Participant Desire × Confederate Desire × Rejection	2.17	.141	.004

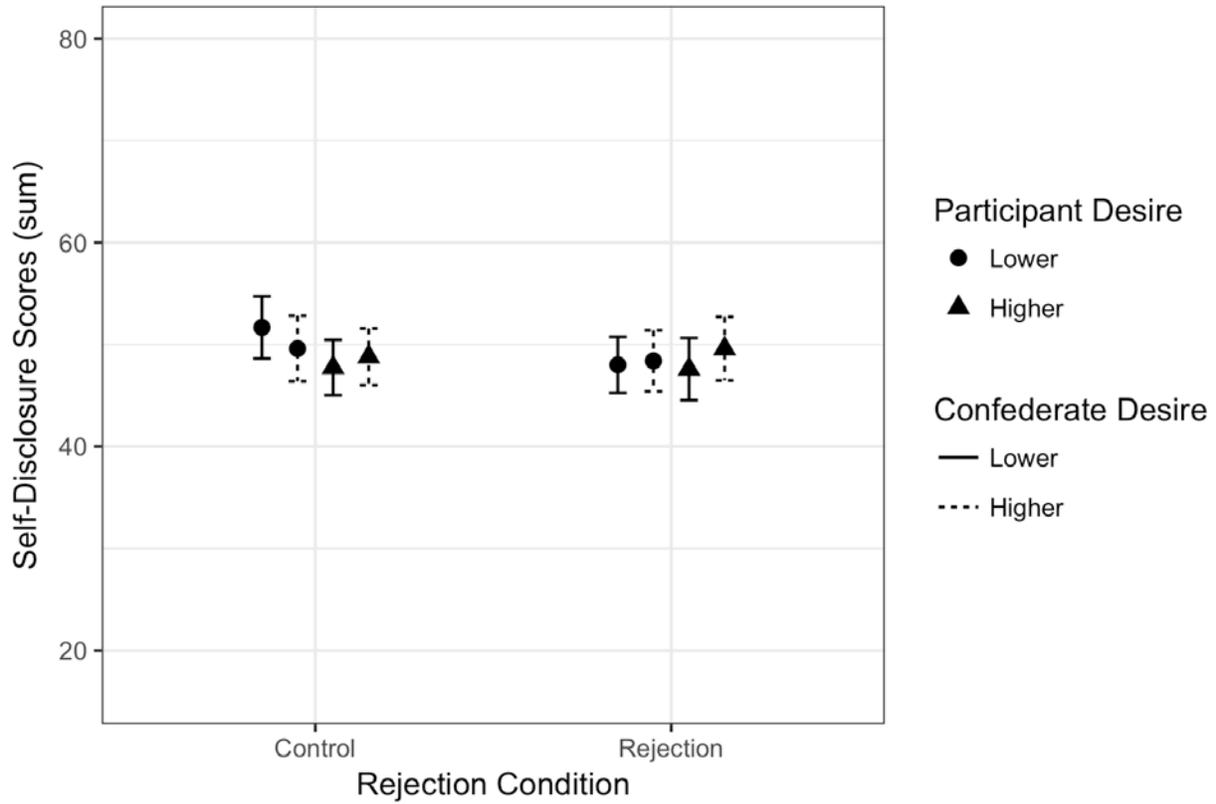*Note.* Results were based on Type III sums of squares.

Figure 2. Error bar plot for the summed self-disclosure scores across conditions. The error bars represent 95% confidence limits around the sample mean.
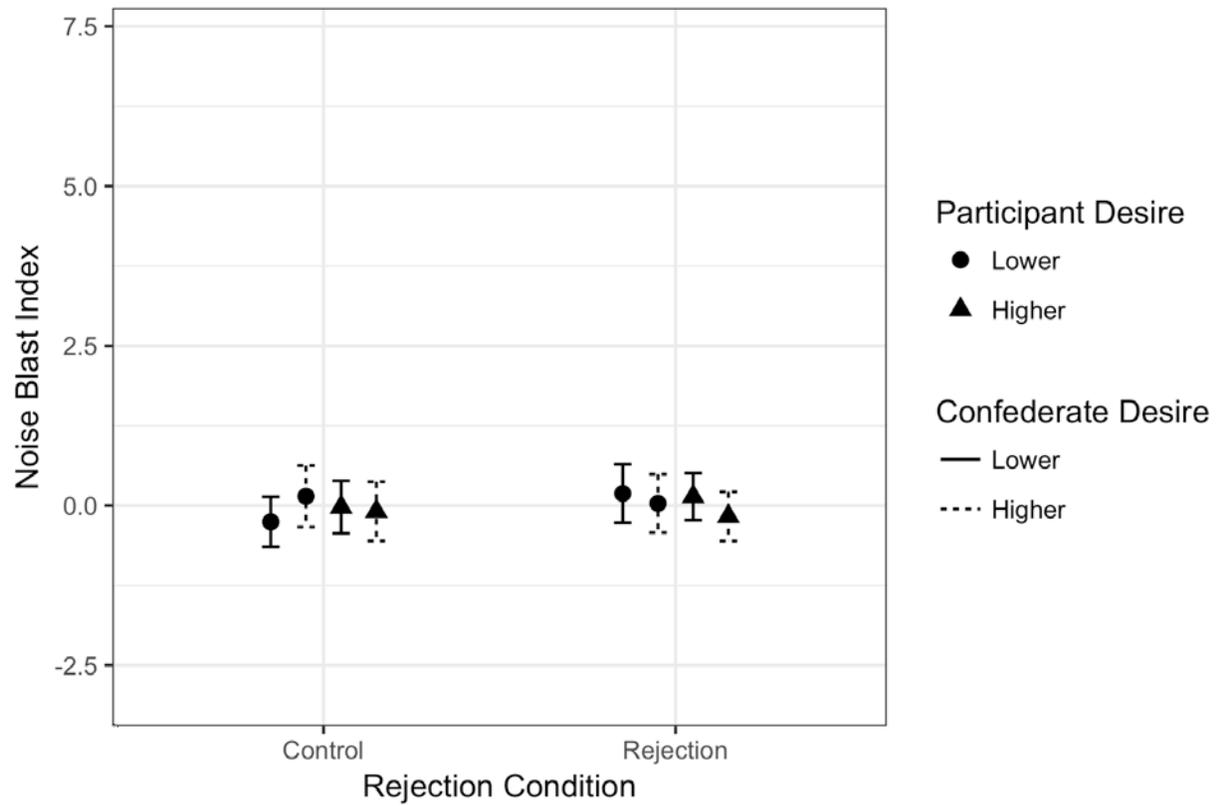
Figure 3. Error bar plot for the noise blast index scores across conditions. The error bars

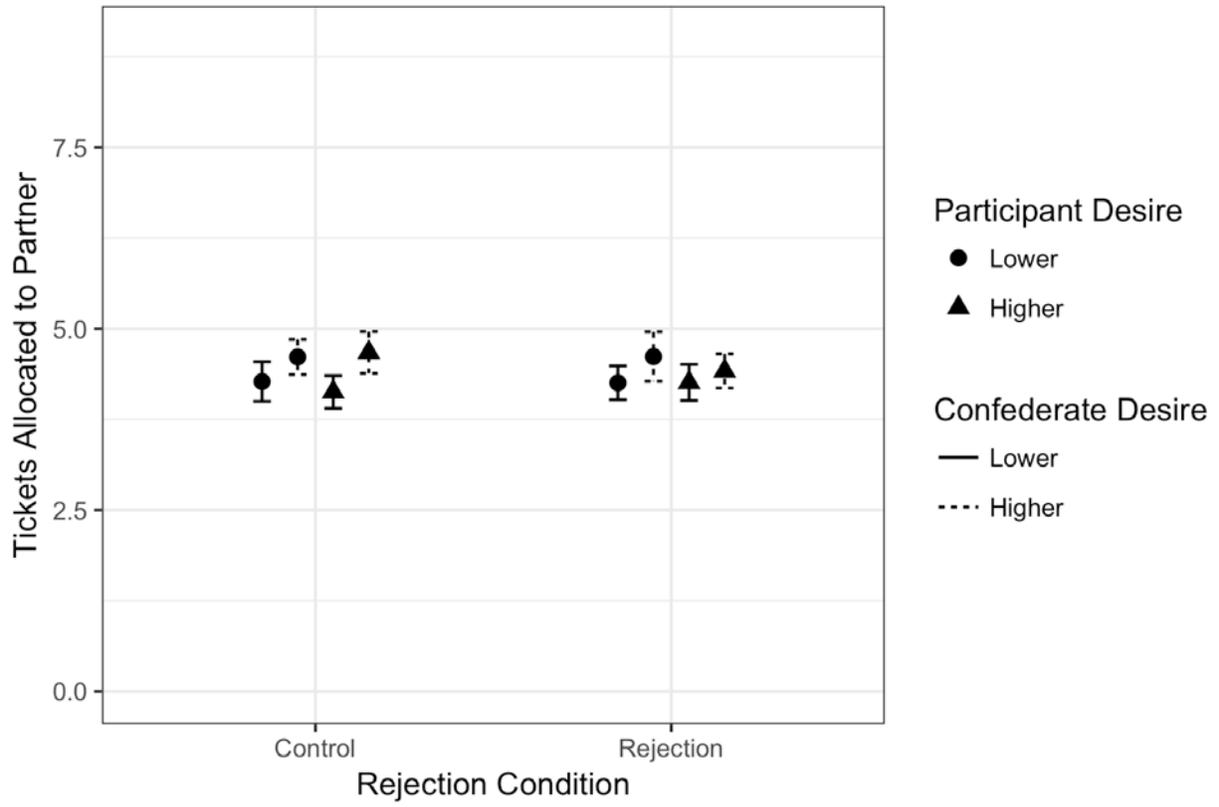represent 95% confidence limits around the sample mean.

Figure 4. Error bar plot for the number of tickets allocated to the confederate across conditions.

The error bars represent 95% confidence limits around the sample mean.

For the number of photos volunteered to rate (a general prosocial outcome), the 3-way participant desire × confederate desire × rejection interaction was not significant, $F(1,530)=2.17$, $p=.141$, $\eta_p^2 <.004$. Contrary to other outcomes, there was a significant 2-way confederate desire × rejection interaction, $F(1,530)=5.79$, $p=.016$, $\eta_p^2=.011$. Follow-up analyses suggested that among rejected participants, those in the higher confederate desire condition ($M=8.27$, $SE=0.55$) showed similar levels of volunteering compared with those in the lower confederate desire condition ($M=9.76$, $SE=0.56$; $p=.057$). Likewise, among participants in the control condition, those in the higher confederate desire condition also showed similar levels of volunteering ($M=9.95$, $SE=0.55$) compared with those in the lower confederate desire condition ($M=8.77$, $SE=0.57$; $p =$ .135). The 2-way participant desire × rejection interaction did not significantly predict volunteering, $F(1,530)=2.93$, $p=.087$, $\eta^2_p=.005$. Figure 5 shows the error bars across conditions.
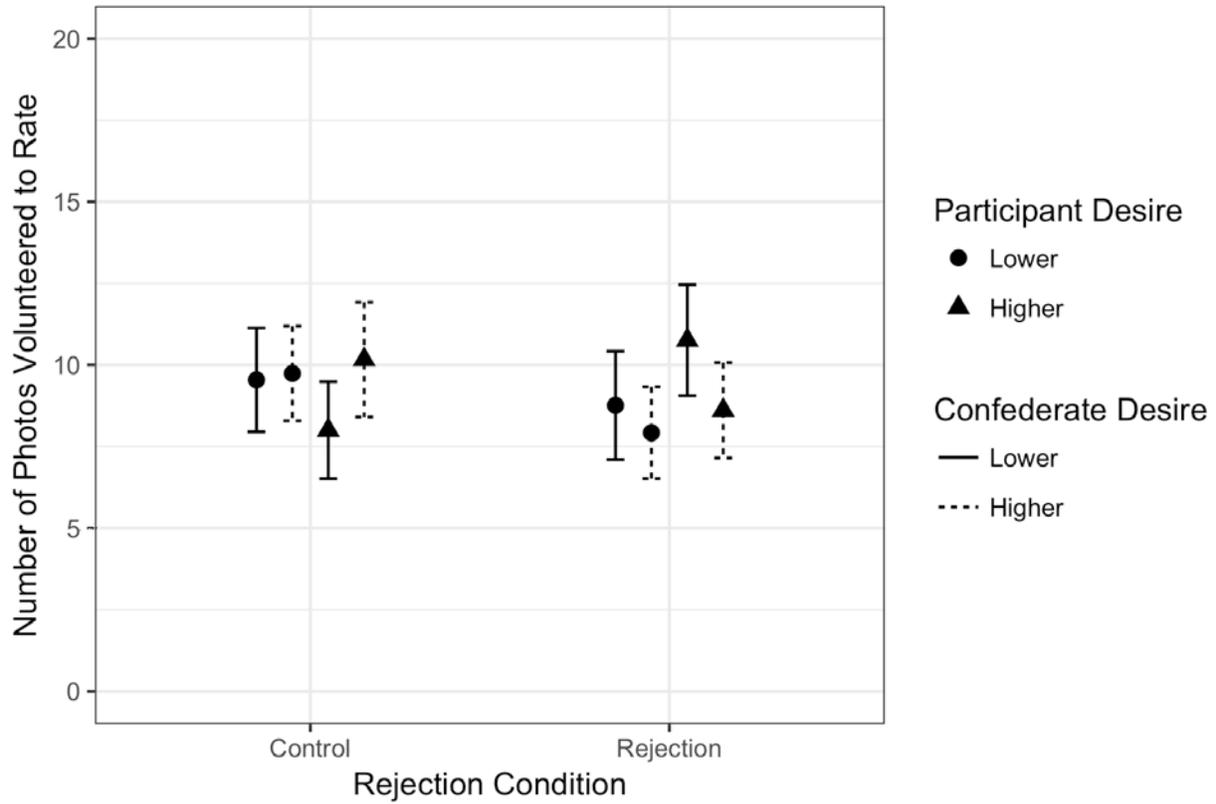
Figure 5. Error bar plot for the number of photos volunteered to rate across conditions. The error

bars represent 95% confidence limits around the sample mean.

To formally explore the null results for the 3-way interaction terms, we conducted

equivalence tests (Lakens, 2017; Schuirmann, 1987). These tests allowed us to examine whether

the obtained effects were small enough to be considered as zero for practical purposes.

Following published recommendations (Lakens, 2017; Simonsohn, 2015), we estimated the

effect sizes from previous studies assuming 33% power (Maner et al., 2007). Since there were

several studies, we averaged these estimated effect sizes to derive the *smallest effect size of*

interest (SESOI; $f^2=.21$; see Supplemental Materials, p.8, for detailed descriptions). Then, we

calculated the confidence intervals for the 3-way interaction terms and tested whether the

obtained effects were significantly smaller than the SESOI. We first calculated eta-squared

confidence intervals using the *MBESS R* package (Kelley, 2018) and then converted them into *f*-

squares using the following formula: $f^2 = \eta_p^2 / (1 - \eta_p^2)$ (Cohen, 1977). Results are summarized in

Table 3. All upper 90% and 95% confidence limits in the current study did not include the

SESOI obtained from prior work. These results suggest that the obtained effects are practically

insignificant (see Supplemental Materials, p.8, for an alternative estimated effect size method

that reached the same conclusion).

Table 3.

Obtained Effect Sizes and Confidence Limits used in the Equivalence Tests

Outcome	f^2	90%CI Upper Limit	95%CI Upper Limit
Self-Disclosure Scores (sum)	.00024	.00667	.00931
Noise Blast Index	.00049	.00836	.01109
Tickets Allocated to Partner	.00229	.01422	.01742
Number of Photos Volunteered to Rate	.00410	.01843	.02194

Note. The effect sizes were based on the 3-way interaction effect (Participant Desire ×
Confederate Desire × Rejection). We calculated the confidence intervals in eta-squares using the
MBESS R package (Kelley, 2018) and converted them into f-squares using the following
formula: $f^2 = \eta_p^2 / (1 - \eta_p^2)$ (Cohen, 1977). All lower 90% and 95% confidence limits were zero.
All obtained effect sizes were significantly smaller than the calculated *smallest effect size of
interest* (SESOI; $f^2 = .21$).

Discussion

The prospect of fulfilling belonging hypothesis suggests that rejected people behave prosocially towards a person who is likely to fulfill belonging and antisocially towards a person who is not (Maner et al., 2007). Our data did not support this hypothesis in its current form—rejected people showed comparable levels of antisocial and prosocial responses regardless of the prospect of fulfilling belonging with their subsequent interaction partner. Specifically, neither manipulation of the prospect of fulfilling belonging (desire to affiliate with the target and perceived affiliation desire from the target) determined prosocial and antisocial responses to rejection. Results were consistent across multiple outcomes: self-disclosure (assessing prosocial behavior), noise blast (assessing antisocial behavior), and ticket allocation (assessing both prosocial and antisocial behavior).

There are multiple possible explanations for the current null results. One possibility is that the prospect of fulfilling belonging hypothesis may be inaccurate in one of the following ways. First, it is possible that the prospect of fulfilling belonging doesn't determine prosocial and antisocial responses following rejection at all. This possibility suggests that existing published work confirming the prospect of fulfilling belonging hypothesis, which relied on small sample sizes, was the result of a Type I error. Second, and perhaps more plausibly, the hypothesis may need further theoretical refinement. Contextual differences, called *hidden moderators,* are one plausible explanation for a failed replication (Van Bavel, Mende-Siedlecki, Brady, & Reinero, 2016). One such hidden moderator may be the type of relationship between the rejected person and the potential affiliation target. For example, real-life close relationships often provide a clear prospect of fulfilling belonging compared with superficial relationships simulated in the laboratory. It is possible that responses to rejection may have differed if the prospect of fulfilling

belonging was more extreme, like when the interaction target is a close relationship partner. This speculation suggests that the prospect of fulfilling belonging hypothesis may operate when such prospects are at extremes. Yet, existing empirical evidence supporting this hypothesis is largely based on strangers in laboratory settings. The current results highlight the importance of revisiting the prospect of fulfilling belonging hypothesis to identify its boundary conditions.

Another possibility is that the null results are due to methodological artifacts of our study design. In other words, it is possible that features of the current study produced null results that are not reflective of the broader theory. Although we cannot rule out this possibility completely, it is unlikely for the following reasons. First, all three manipulations (participant desire, confederate desire, and rejection) succeeded in altering their corresponding constructs, as shown in the manipulation checks. These data demonstrate that we manipulated the relevant constructs from the prospect of fulfilling belonging hypothesis—thus, we should have obtained results consistent with this hypothesis. Second, we carefully tested the prospect of fulfilling belonging hypothesis in multiple ways. We strongly manipulated the prospect of fulfilling belonging by including two explicit manipulations (participant desire and confederate desire). We also assessed multiple prosocial and antisocial behaviors that conceptually mapped onto the prospect of fulfilling belonging. Third, our design choices safeguarded against possibilities of a Type II error. We conducted a well-powered study using power=.90, which is above the conventional standard of .80. In addition, we assumed a small effect (Cohen, 1992) that was much smaller than those reported by the original studies, to avoid overestimating the true effect size and under-sampling. Finally, we pre-registered the study plan to minimize researcher degrees of freedom. Given these careful design choices, the current null effects are unlikely to be methodological artifacts.

There was suggestive evidence that among rejected participants, those who saw a confederate expressing a higher desire to affiliate were less interested in volunteering compared with those who saw a confederate expressing a lower desire to affiliate. These results were unexpected in the context of the prospect of fulfilling belonging hypothesis. Further research is warranted to investigate this unexpected finding.

The effect size for the rejection manipulation was small ($d=.30$; Cohen, 1992). Small effect sizes often still have important implications (Prentice & Miller, 1992; Rosenthal, 1990). In addition, based on the prospect of fulfilling belonging hypothesis, even a smaller effect should impact behavior—threatening belonging should cause prosocial and antisocial responses when the prospect of fulfilling belonging is higher and lower, respectively. The current study successfully threatened belonging, and thus there should have been corresponding changes in responses. With that being said, it is possible that a stronger rejection manipulation (or a comparison with an inclusion condition) would produce results consistent with the prospect of fulfilling belonging hypothesis. If future empirical data support this possibility, it would suggest an important revision to the theory.

Since we presented our outcomes in a fixed order, we were unable to examine order effects (see Supplemental Materials, p.7, for correlations among the outcomes). However, given the null effect of the manipulations on the first outcome (the self-disclosure task), the current null results are unlikely to be a byproduct of the order among measures. If the prospect of fulfilling belonging hypothesis was accurate in its current form, there should have been an effect on the first outcome at a minimum. Future studies should examine whether the prospect of fulfilling belonging hypothesis applies to specific outcome measures more than others.

Following previously published recommendations (Simons, Shoda, & Lindsay, 2017), the current section reviews possible constraints on the generality of the results. First, the rejection and control manipulations used in the current study were copied verbatim from an existing study (Twenge & Campbell, 2003, Study 1). The instructions for the rejection condition asked participants to recall non-romantic, non-group-based rejection. It is possible that other types of manipulations might produce different results, limiting the generality of this study. One possible way to differentiate among rejection manipulations is to combine theories about expectancy violation and the prospect of fulfilling hypothesis, as suggested recently (Gerber & Wheeler, 2013; Wesselmann, Wirth, & Bernstein, 2017; Wirth, Bernstein, Wesselmann, & LeRoy, 2015). Some participants in the current study may have experienced expectancy violation, influencing their behavior. Incorporating these two theoretical perspectives may shed additional light in the current results, the prospect of fulfilling belonging hypothesis, or both. However, the overall interpretation of the current null results remains the same. The current study successfully threatened belonging—thus, we should have observed changes in behavior consistent with the prospect of fulfilling belonging hypothesis.

Second, we speculate that the results may not be generalizable to intergroup situations. Our participants were all White, and they believed that their partner was also White. Past research suggested that social rejection in intergroup contexts (e.g., a Black person rejected by a White person) produced different responses compared with within-group contexts (e.g., Mendes et al., 2008). Future research should examine the prospect of fulfilling belonging hypothesis in intergroup contexts. Third, the current results may not generalize outside of the laboratory. The current study manipulated the prospect of fulfilling belonging and social rejection in a highly-controlled laboratory setting. As described earlier, people may experience a very high prospect

of fulfilling belonging from their loved ones. Similarly, people should be highly affected by real-world rejection experiences. Both of these real-life experiences might more strongly impact prosocial and antisocial behavior than laboratory manipulations. Other than the specific constraints described above, we do not believe that the results depend on other characteristics of the participants, materials, or context.

Conclusions

Our results failed to support the prospect of fulfilling belonging hypothesis—rejected people showed comparable levels of prosocial and antisocial responses regardless of the prospect of belonging with their subsequent interaction partner. We operationalized the prospect of fulfilling belonging by separately manipulating participants' desire to affiliate ("how much I want to be with the study partner") and perceived desire to be affiliated with ("how much my study partner will accept me"). Neither of the two constructs nor the combination of the two affected responses to rejection. The current null findings provide a strong case against the prospect of fulfilling belonging hypothesis in its current form. This hypothesis is a theoretical cornerstone of the rejection literature (DeWall & Richman, 2011; Richman & Leary, 2009; Shilling & Brown, 2015). Overall, our results suggest that this cornerstone needs further revision—an important next step to advance the rejection and relationships literatures.

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