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# Generalization of rejection and acceptance in social networks☆

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ABSTRACT

Social environments present opportunities for connection and resources, but they also involve the risk of rejection. How do people learn which individuals will reject or accept them upon entering a novel environment? Here, we propose a route to such learning: people use knowledge of relationships in social networks to infer who will be likely to accept or reject them. Previous research shows that people generalize trust from one individual to that individual's friends, yet it remains unclear whether rejection and acceptance experiences generalize in similar ways in social network contexts. We designed a novel experimental paradigm in which participants experienced rejection and acceptance within an artificial group, learned about network connections among group members, and decided which members to approach in a new task. Study 1 found that participants generalized rejection by avoiding individuals socially closer to a rejector and approaching those closer to an accepter, forming a gradient of avoidance and approach based on network distance. Study 2 further demonstrated stronger generalization when networks reflected friendship as opposed to randomly assigned ties, suggesting partner choices depend on explicit inferences about meaningful relationships rather than associative learning alone. Finally, in a longitudinal survey of student groups, Study 3 extended these findings to real-world social networks, revealing similar patterns of generalization in college student organizations. Together, our findings inform the cognitive processes that help humans successfully navigate social environments by adaptively forming new connections.

Humans have a fundamental need to form and maintain meaningful connections with others,commonly known as the need to belong (Baumeister & Leary, 2017). To fulfill this need, people must identify which individuals value them in novel social environments. This task, however, can be challenging and risky. Social environments are highly uncertain (FeldmanHall & Shenhav, 2019) and often involve the risk of social rejection and exclusion. In the short term, rejection can lead to hurt feelings (Eisenberger et al., 2003; MacDonald & Leary, 2005), while in the long-term social exclusion and feelings of loneliness can increase mortality and decrease well-being (Eisenberger, 2013; Snyder-Mackler et al., 2020). How do people detect potential rejectors and accepters when navigating novel social contexts?

One straightforward way to identify rejectors and accepters is through learning based on direct experience (Babür et al., 2024; Cho & Hackel, 2022; Fareri et al., 2012). By interacting with others and discovering whether they accept or reject us, we can update our expectations about them accordingly. However, learning through direct experience isn't always feasible or desirable. Oftentimes, we need to choose social partners by inferring the intentions and attitudes of people with whom we have never interacted. One way to form such indirect inferences is through generalization—the process of deciding how to respond to a new stimulus by applying past feedback from a different stimulus that is perceived as similar or relevant (Shepard, 1987; Fazio et al., 2004). Put differently, if a novel person is in some way "similar" or "connected" to an interaction partner, we might treat them similarly. Here, we propose that people use knowledge of relationships in a social network—particularly information about who is friends with whom—to infer who will likely accept or reject them, approaching those who are closer to accepters and avoiding those closer to rejectors. Specifically, we propose that people generalize the experience of being accepted (or rejected) by one individual to that individual's friends, thus preferring to interact with (or avoid) the friends despite having no direct past experiences.

☆ This paper has been recommended for acceptance by Brett Peters

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## 1. Learning relational value through direct interaction

Social acceptance reveals one's "relational value" in the eyes of others—the degree to which others regard a relationship with them as valuable (Leary, 1999, 2005). When people perceive that they are not valued by a given partner, they tend to seek other sources of connection and avoid or retaliate against those who directly rejected them (Bourgeois & Leary, 2001; DeWall & Richman, 2011; Maner et al., 2007; Twenge et al., 2001). In contrast, when people feel valued, they can anticipate that others will accept them over the long term, fostering approach behavior and closeness. Accordingly, people track cues to relational value over time, using instances of acceptance or rejection to update an internal model estimating how much an interaction partner values them (Babür et al., 2024). In this manner, social interactions serve as direct learning experiences that reveal relational value and guide subsequent social choices.

## 2. Relational value through generalization

When people have not engaged in direct interaction with others, how might they form an estimate of relational value guiding their choices? When learning about others, people can generalize social feedback to novel individuals in multiple ways. For example, people may generalize based on visual similarity, trusting a target more if that target looks similar to someone previously known to be trustworthy (FeldmanHall et al., 2018). Alternatively, people can generalize based on conceptual information, such as a target's group membership. Upon receiving positive feedback from one individual, people readily generalize it to other members of the same social category, which in turn influences their perception and evaluation of the group (Allidina & Cunningham, 2021; Bai et al., 2022; Glaser & Kuchenbrandt, 2017; Hackel et al., 2022; Hein et al., 2016).

These two types of generalization, however, may not always be useful for people entering a novel social group. First, superficial cues such as facial features may reflect arbitrary similarities rather than meaningful underlying differences. Second, because potential partners all belong to the group, group membership cannot distinguish between individual group members in a granular way. Alternatively, people often generalize based on the perceived *diagnosticity* of feedback—the extent to which traits and behaviors of one person are seen as informative of how others will behave (Cone & Ferguson, 2015; Hamilton et al., 2015; Skowronski, 2002). Accordingly, people may try to estimate whether two people in a group are likely to be similar to one another in their tendencies to reject or accept them.

Here, we propose that one such strategy is to treat friendship as a diagnostic cue and generalize based on one's knowledge of the friendships between group members, drawing on a mental map of network structure. People tend to be aware of the structure of their social networks, identifying which individuals are friends (Aslarus et al., 2025; Basyouni & Parkinson, 2022; Schwyck, 2023; Son et al., 2021, 2023). Within such networks, friends often share similar attitudes and behavioral tendencies–a phenomenon known as homophily (McPherson et al., 2001). This fact is not lost on people: perceivers expect homophily in networks and therefore generalize social preferences based on knowledge of friendship ties between network members. For instance, people hold "social priors" that friends tend to behave similarly and use this assumption to decide whom to trust, showing a preference for trusting the friends of trustworthy targets, even when no trustworthiness information is directly available about those friends (Jolly & Chang, 2021; Martinez et al., 2016; Schwyck et al., 2024). Beyond expectations of similarity, people also believe that friends coordinate and share information with one another via gossip (Xia et al., 2025), which can drive ostracism decisions (Feinberg et al., 2014). Accordingly, acceptance or rejection experiences with one group member not only inform how much we are valued by that person but also suggest that their friends may value us to similar extents—whether through similar baseline preferences or information flow via gossip—and thus give us reason to approach or avoid that person's friends. Finally, such generalization is cognitively feasible: individuals can quickly encode information about relationships in a social network (Basyouni & Parkinson, 2022; Jolly et al., 2023) and readily infer this information even when it is not directly observable (Schwyck, 2023; Son et al., 2021, 2023), making generalization based on friendship both feasible and efficient.

Altogether, there is reason to believe that indicators of relational value—including social rejection and acceptance—may be generalized along friendship ties in a network. These ties may be taken as a proxy for similarity, making rejection from one individual a diagnostic cue to how their friends will treat us. In turn, people may approach individuals more closely connected to accepters and avoid individuals more closely connected to rejectors within novel networks. This form of generalization could sometimes be adaptive, allowing rapid inferences and decisions about unfamiliar others; on the other hand, it may be maladaptive in cases of overgeneralization (Fazio et al., 2004; Raes et al., 2023).

## 3. Contribution of the present research

Here, we test whether people generalize rejection and acceptance based on friendship ties within social networks. Although prior work has examined the generalization of trust based on friendship ties, several questions remain unclear. First, while prior work focuses on how people infer others' trustworthiness (e.g., in trust games) and make decisions accordingly, an equally important factor in making social decisions is to infer *whether others value and trust us*, reflecting the *relational value* others hold toward us. These two types of inferences both support social choices yet are distinct (Babür et al., 2024; Cho & Hackel, 2022), and it remains unclear if similar kinds of generalization extend to inferences about who might reject or accept us. Second, past work has demonstrated generalization of trust in *dyadic contexts* in which people make inferences about a target individual's friends. However, in social networks, group members vary in their degrees of separation (i.e. geodesic distance; O'Malley & Marsden, 2008). Individuals who have a closer distance in a network tend to have similar personality, social preferences, and their brains tend to process information in similar ways (Bhargava et al., 2022; Lönnqvist & Itkonen, 2016; Parkinson et al., 2018). Thus, being only "one-step away" from a rejector (i.e. being the rejector's friend) might signal high likelihood of rejection, whereas individuals who are two and three-steps away from a rejector might be perceived as increasingly safer to connect with. In other words, people might show a *gradient* of generalization following social rejection, showing an increasing preference to interact with targets who have a greater network distance from the original rejector. However, so far, little empirical work directly tests whether this gradient of generalization emerges in response to social feedback.

## 4. Overview

We investigated how people generalize the risk of rejection and the promise of acceptance to previously unencountered members of a social network. We hypothesized that participants would avoid targets closely connected to a known rejector, showing stronger avoidance as network distance decreases. Conversely, participants would prefer those who are closer to an accepter, showing greater approach behavior as network distance decreases.

We tested these ideas across three studies (and a pilot study reported in the Supplementary Materials). In Studies 1–2 (lab experiments), participants learned which members of a novel social group tended to reject vs. accept them. Next, participants learned about how the entire group was interconnected and chose which group members to interact with, including novel members they have not interacted with before. By relating these choices to each member's network distance from the known rejector (and accepter), we tested whether a gradient of generalization exists. Importantly, we also examined participants' explicit beliefs about how much the group members were likely to accept them, providing a direct test of perceived acceptance beyond behavioral approach and avoidance alone. In addition, Study 2 manipulated whether social network ties reflected friendship or random-pairing; if participants generalize based on feedback perceived as meaningfully diagnostic of others' behavior, then they should generalize only when ties reflect a meaningful cue to similarity (e.g., friendship) but not otherwise (e.g., random-pairing).

Study 3 used a longitudinal design in real-world student organization networks. We measured participants' social ties and quality of social interaction over time, allowing us to see how participants update their social expectations based on past experiences and their knowledge of network relationships.

Studies 1 and 2 were pre-registered (Study 1: https://aspredicted.org/y43cj.pdf; Study 2: https://aspredicted.org/cw2ad.pdf). All studies, measures, manipulations, data/participant exclusions, and deviations from the pre-registration are reported in the manuscript or its Supplemental Materials. All studies were approved by the Institutional Review Board at the authors' institution.

## 5. Study 1

In Study 1, we asked whether people generalize experiences of acceptance and rejection in social networks based on the social distance between individuals in the group. Participants learned about a novel social group by playing an economic game in which they tried to match with others for a trust-based interaction. Ostensibly, others had read a personal profile written by the participant and decided on this basis whether to trust the participant. Participants initially played this game with either a rejector or an accepter. Afterwards, participants had the opportunity to try to play the game with novel individuals from the group who differed in their distance to the accepter and rejector in the network. We hypothesized that participants would generalize both rejection and acceptance, showing stronger preference for targets who were farther away from the rejector or closer to the accepter in the network.

In a pilot study with a similar design (see Supplemental Materials), participants learned about both an accepter and rejector in one network, after which they preferred novel individuals closer to the accepter and farther from the rejector. By assigning participants to either an acceptance or rejection condition, the present study tested whether participants would separately generalize each type of feedback.

### *5.1. Method*

**Overview**. Study 1 included two sessions conducted one week apart. In Session 1, participants provided self-disclosure responses (ostensibly to be evaluated by others) and completed individual differences questionnaires. In Session 2, participants completed a computer-based task comprising three phases: (1) In the **instrumental learning phase**, participants learned whether an individual tended to accept or reject them, ostensibly based on the profile they had filled out; (2) in the **network learning phase**, participants learned about the friendship relationships among all group members and were tested on their memory for those ties; (3) finally, in the **generalization phase**, participants chose which group members to interact with without feedback. By analyzing choices toward novel Deciders in the generalization phase, we assessed whether participants generalized prior experiences of acceptance or rejection based on network distance.

In past work using a similar learning task, acceptance and rejection shaped affect (Cho & Hackel, 2022) and brain activity in regions linked to reward processing and social rejection (Babür et al., 2024), which in turn predicted subjective perceptions of relational value. This task was therefore used to test whether participants generalize these perceptions of relational value based on network ties.

**Participants.** Based on results from a pilot study (see Supplemental Materials) and heuristics, we recruited 300 participants on CloudResearch for Session 1. Of these, 229 returned to Session 2. To ensure data quality, all participants had an approval rate of 95 % or higher. Data from Session 1 and Session 2 were matched using participant IDs; only individuals completing both sessions were included in the final sample. Due to an error in Pavlovia, data were not saved for 14 participants, resulting in 215 complete responses. Following prior work (Cho & Hackel, 2022; Hackel et al., 2022), we administered a pre-registered exclusion rule to remove data from participants who either 1) failed to respond in at least 20 % of the instrumental learning trials, or 2) failed to reach at least 60 % accuracy in the friendship memory test. These criteria resulted in 71 participants being excluded (2 due to missing instrumental learning trials; 71 due to low memory accuracy), leaving 144 participants for analyses (69 women, 74 men, one did not report; *M* age = 39.9, *SD* = 12.1). A sensitivity power analysis for mixed-effects generalized linear models using the *simR* package (Green & MacLeod, 2016) indicated that 144 participants had 80 % power to detect an effect size of $\beta = 0.14$ (Odds Ratio = 1.15) or greater with 5 % false-positive rate.

**Stimuli**. Six group members were represented by face avatars (created on pickaface.net, Fig. S1) and pseudo-names (e.g. John D.; created on random-name-generator.info). The avatars were half male and half female and were assigned names of the corresponding gender. For each participant, the faces were randomly paired with names within the same gender category.

**Procedure**. Participants completed an experiment across two sessions. In Session 1, participants were told that they would be playing a game that involves learning about others. Participants answered six self-disclosure questions about themselves, with an emphasis on trustworthiness (e.g., "When was a time when you were honest, even though you didn't have to be?"), which they were told would then be sent to other participants to read (see Supplemental Materials for a full list of questions). Afterwards, participants completed the UCLA loneliness scale (Russell et al., 1978), the Adult Rejection Sensitivity Questionnaire (ARSQ; Berenson et al., 2009), the Brief Fear of Negative Evaluation (BFNE) Scale (Leary, 1983), and the perspective-taking subscale of the Interpersonal Reactivity Index (IRI; Davis, 1980). As noted in our preregistration, we conducted exploratory analyses testing each measure as a potential moderator of the generalization effects. However, none of them showed consistent effects between Studies 1–2; we therefore report the full results in the Supplemental Materials.

One week later, participants were invited back for Session 2. As a cover story, participants were told that six students enrolled in a course at the authors' institution had read their responses along with the responses of other participants. These students then talked to each other about the responses and decided whom to send points in a trust game (Berg et al., 1995). This instruction allowed for the possibility of information flow between group members, as would occur in real-life networks in which gossip can take place (Xia et al., 2025).

The students were therefore the "Deciders" and the participants were the "Responders". In each round, Responders could decide whether to match with a Decider. If participants chose the Decider and that Decider wanted to match with them in return, they would be able to play the trust game, and if not, they could not play on that round. We told participants that each Decider had made multiple choices involving the Responders, so that participants would need to repeatedly learn about the same Decider in this task; accordingly, a Decider might trust the participant more than some Responders but less than others, leading to different outcomes in different rounds. In reality, the decisions were preprogrammed, which allowed us to manipulate whether participants were accepted or rejected. Crucially, this design manipulated participants' *relational value*—how much the Decider valued and trusted *them*—rather than the Decider's global traits. Because the Decider always trusted *someone* in each round, there was no basis for participants to infer general friendliness or overall propensity to trust; instead, participants could only infer how much they themselves were valued relative to others.

***Instrumental learning phase***. Participants were randomly assigned to one of two between-subject conditions and completed 60 trials in which they learned whether a Decider tended to reject or accept them; only one Decider was seen throughout learning. On each trial, participants chose whether to attempt to match with the Decider; as a nonsocial alternative, participants could instead activate a slot machine and receive a probabilistic payoff. Unbeknownst to participants, in the *acceptance* condition, the Decider, if chosen, had an 80 % probability of matching on each trial, whereas in the *rejection* condition, the Decider had a 20 % probability of matching. Similarly, on each trial, one of two slot machines was shown: a "generous" slot machine, if chosen, had an 80 % probability of paying 30 points to participants and a 20 % probability of paying 0 points, whereas a "stingy" slot machine had a 20 % probability of paying 30 points and an 80 % probability of paying 0 points. Thus, the generous slot machine matched the minimum payoff rate of the Decider in the acceptance condition (i.e. the amount participants would receive if they matched and chose to return half of the points), while the stingy slot machine matched the minimum payoff rate of the Decider in the rejection condition. Participants were therefore incentivized to interact with the human Decider if they anticipated they would be accepted and to choose a slot machine if they anticipated they would be rejected.

On each trial (2 s), the Decider was displayed on one side of the screen, and one of the two slot machines was displayed on the other side (order randomized). Participants could then choose to try to match with the Decider or instead to pick a slot machine by pressing "E" (left) or "I" (right). After 3 s, participants received feedback depending on which side they chose. If they chose the Decider, then they would learn whether the Decider had decided to send points to the participant (i.e. matching) or to send points to a different Responder (i.e. failing to match).

If participants successfully matched with the Decider on a given round, they would play a brief trust game (3 s). In this game, participants received 60 points (tripled from the Decider's initial amount) and had to decide whether to keep all the points or return half. This trust game is a standard economic paradigm used to measure trust and reciprocity: returning half signals cooperation or trustworthiness, while keeping all indicates a preference for personal gain (Berg et al., 1995). If the chosen Decider did not choose participants in return, participants would not be able to play the trust game and instead had to wait for 3 s before the next trial started.

If participants missed a trial, a "NO RESPONSE" warning would display for 0.5 s, followed by a 3 s delay before the next trial. At the end of the study, the number of points participants kept during the trust game was converted to a small bonus compensation, averaged at $0.50.

***Network learning phase***. After instrumental learning, participants learned about friendship ties among all six Deciders. As a cover story, participants were told that, prior to the study, the Deciders had completed a survey indicating who was friends with whom within the course. Two Deciders were supposedly recorded as friends only if both people had listed each other in the survey. Participants' task was to learn who was friends with whom and would be tested for memory accuracy later on.

To help participants learn the friendship relationships, we adapted a paradigm that sequentially presents network ties (Lynn & Bassett, 2020; Tompson et al., 2019; Dziura & Thompson, 2020). In each round, participants saw two avatars on screen, with the following text: "[Name 1] is friends with [Name 2]." Participants then could memorize each friendship tie at their own pace before advancing. Each pair of friends was presented a total of 20 times, with their positions counter-balanced. Participants were told that if two students were never presented on the screen together, then they were not friends. Past work shows that performance on this type of task tracks social-cognitive abilities including perspective-taking (Tompson et al., 2019), indicating that participants treat the presented ties as socially meaningful.

Importantly, unbeknownst to participants, the Deciders' network had a ring structure, such that each Decider was connected to exactly two other Deciders (Fig. 2A). As a result, participants' learning about the Deciders would not be biased by the Deciders' network position (i.e. their number of friends).

After learning, participants completed 32 rounds of a memory test consisting of 16 genuine friend pairs and 16 non-friend pairs who were two degrees apart in the network. In each round, participants saw two Deciders on screen and had to press either "up" ("yes") or "down" ("no") to indicate whether the Deciders were friends. Participants with 60 % accuracy or above were included for further analyses (*M* accuracy before exclusion = 73.5 %, *SD* = 19.0 %, *Range* = 25 % - 100 %). Notably, memory accuracy did not significantly differ between the rejection and acceptance conditions (before exclusion: *M* acceptance = 72.3 %, *M* rejection = 74.2 %, $t(210.38) = -0.71$, $p = .48$; after exclusion: *M* acceptance = 84.4 %, *M* rejection = 84.7 %, $t(133.04) = -0.18$, $p = .86$), indicating that prior experiences of rejection and acceptance did not influence participants' encoding of the network ties.

***Generalization phase***. To assess generalization to novel individuals, participants played additional rounds of the matching game in which we measured participants' choices among all six Deciders (Fig. 1). Participants were told that they would continue to try to match and play trust games with the Deciders but they would no longer receive immediate feedback in each round. Because most Deciders in this phase had never directly accepted or rejected the participant, these trials involving novel Deciders allowed us to measure whether participants generalized avoidance or approach based on each Decider's distance to the original accepter or rejector. This design prevented further learning, allowing us to examine how participants made choices in the absence of feedback about novel individuals.

The generalization phase consisted of three types of trials. First, in human-human trials, participants chose between two Deciders (each combination of Deciders appeared twice, resulting in $A_6^2 = 30$ trials). These trials aimed to test whether participants preferred to match with targets based on their distance to the rejector/accepter *relative to* the alternative target. Second, in human-slot trials ($n = 24$ trials), participants chose between a Decider and a slot machine. In these trials, participants had the opportunity to avoid interacting with any Decider, which allowed us to test whether social rejection and acceptance further affects people's tendency to avoid/approach interactions at all. Finally, in slot-slot trials ($n = 2$ trials), participants chose between the generous and stingy slot machines, in order to confirm they had learned the slot machines' payoff probabilities. These trial types were pseudo-randomly interleaved.

On each trial, participants had 4 s to make a choice by pressing either "E" (left) or "I" (right), followed by an inter-trial interval (ITI) of 1 s. If participants failed to respond within 4 s, they would see a warning of "NO RESPONSE" for 0.5 s. All tasks were programmed in PsychoPy (v2021.2.3) and run via Pavlovia.org (Peirce, 2007).

***Post-task measures***. After the test phase, we measured participants' perception of how much each Decider liked them (7-point Likert scale, 1 (not at all) to 7 (very much)). This measure allowed us to test the extent to which participants generalized their explicit perceptions of relational value after rejection and acceptance, above and beyond their partner choices during the trust game. In addition, we measured how much participants liked the Deciders' group using the same 7-point Likert scale.

### 5.2. Results

**Manipulation check**. We first tested whether participants successfully learned about the original Decider by comparing their average likelihood of choosing the original rejector or accepter against novel Deciders in the generalization phase. Participants in the acceptance condition were significantly more likely to choose the original accepter

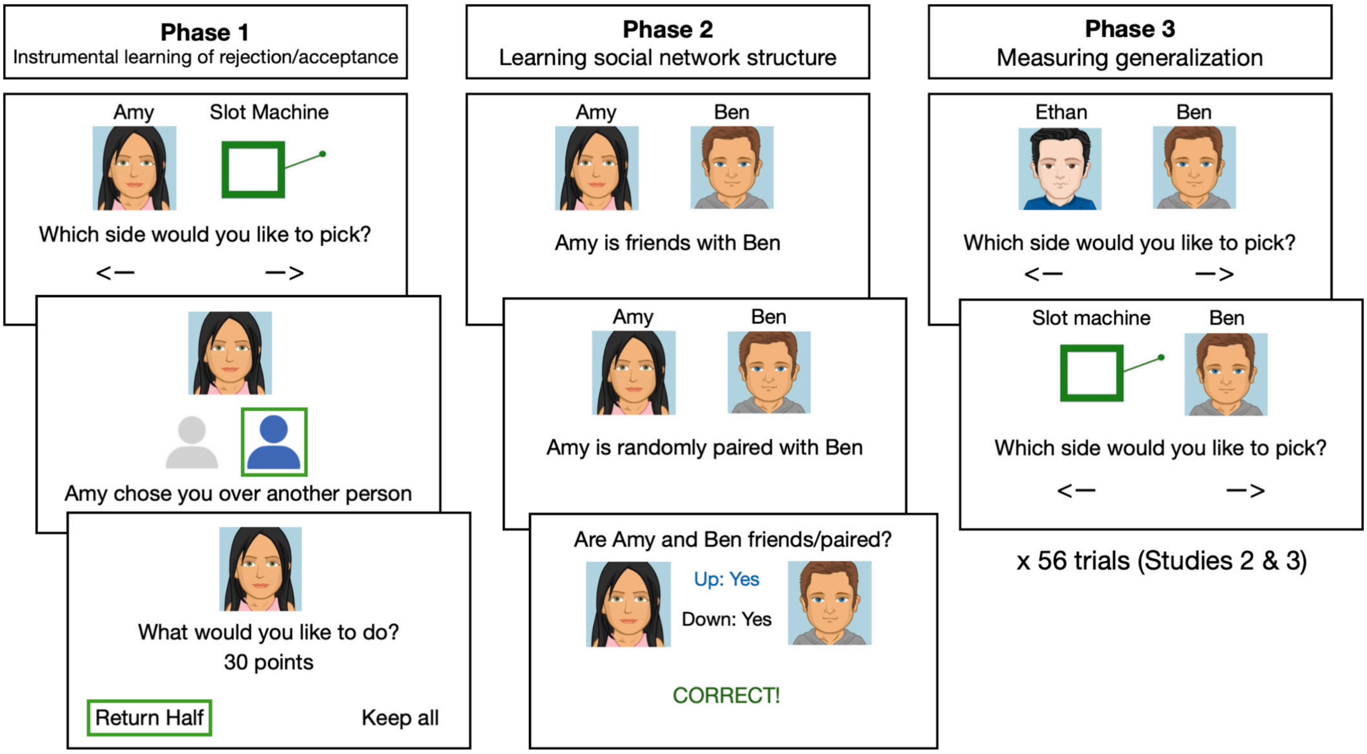


**Fig. 1.** Schematic of Session 2 in Studies 1 and 2. **Instrumental learning phase:** In each round, participants chose to learn about either one human Decider or one of two slot machines. If participants chose a Decider and were matched, they would play a trust game where they chose whether to keep or return points. We manipulated the probability of matching as a manipulation of rejection and acceptance. In Study 1, rejection and acceptance were manipulated *between* subjects, whereas in Study 2, all participants experienced rejection. **Network learning phase:** Participants learned the relationships among all six Deciders and completed a memory accuracy test with feedback. In Study 1, participants learned about the friendship relationships among the Deciders, whereas in Study 2, we manipulated whether participants learned about friendships or random-pairings. **Generalization phase:** Participants continued to choose game partners without feedback. All six Deciders (i.e. both original and novel) were presented. On some trials, participants chose between two Deciders; on some trials, they chose between one Decider and one slot machine; on the rest of trials, they chose between two slot machines.

than chance level (76.5 % > 50 %, $t(63) = 9.43$, $p < .001$, $d = 1.18$) and participants in the rejection condition were significantly less likely to choose the original rejector than chance level (21.5 % < 50 %, $t(79) = -10.32$, $p < .001$, $d = -1.15$).

We then compared participants' likelihood of choosing the two slot machines. Collapsing across both conditions, participants were significantly more likely to choose the generous slot machine (*M* choice = 57.3 %, *SD* = 0.34) than the stingy slot machine (*M* choice = 37.3 %, *SD* = 0.031; $\Delta = 0.20$, $t(143) = 6.79$, $p < .001$, $d = 0.57$). These results suggest that participants successfully learned who tended to reject/accept them and which slot machine tended to offer higher payment.

**Generalization effects**. To test whether participants generalized experiences of rejection and acceptance, we examined their responses on trials that involved novel Deciders, since on these trials, participants could not draw on direct experiences to choose whom to approach or avoid. To do so, we first filtered out all trials involving the original rejector or accepter. Next, we performed two separate analyses. First, we focused on the human-human trials, testing whether participants' choices between two Deciders depended on the Deciders' relative distance to the rejector/accepter. Although this analysis was not pre-registered, these analyses aimed to replicate the findings of our pilot study (Supplemental Materials) and to test whether generalization shaped choices between two potential social partners. Next, following our pre-registration, we focused on the human-slot trials, testing whether the same choice patterns hold when participants had the option of withdrawing from social interactions entirely. Both models were fitted using the *glmer* function in the lme4 library (Bates et al., 2015).

*Choices between two Deciders*. To examine participants' choices between two novel Deciders during the generalization phase, we recoded each target's distance to the original rejector/accepter into a "positivity score", such that a higher positivity score represents a greater distance from the rejector in the rejection condition or a closer distance from the accepter in the acceptance condition. For example, a Decider would have a positivity score of 0 if they were the rejector or were three degrees apart from the accepter; a Decider would have a positivity score of 1 if they were one degree apart from the rejector or two degrees apart from the accepter, and so on. If participants generalize, then they should be more likely to choose Deciders with relatively higher positivity scores. Recoding distance into positivity scores allowed us to compare the gradient of generalization between the rejection and acceptance conditions; for instance, a steeper gradient of generalization for one of the conditions should correspond to an interaction effect between positivity and condition.

We then fitted a mixed-effects logistic regression to model participants' trial-by-trial choices between two novel Deciders during the generalization phase. This model predicted whether participants chose the left-side Decider (1: Yes, 0: No) using 1) the left-side Decider's positivity score *relative to* the right-side Decider, 2) condition (−1: rejection, 1: acceptance), and 3) their interaction. Note that the left-side Decider was arbitrarily chosen; this approach examined whether participants made choices between novel targets based on which target had a greater positivity score. We also included a random intercept for participant and a by-participant random slope for positivity score.

We found a significant effect of positivity on choice ($b = 0.36$, $SE = 0.069$, $z = 5.16$, $p < .001$, 95 % CI = [0.22, 0.49]). Across both conditions, participants were more likely to choose a Decider as that Decider's positivity increased *relative to* the other Decider onscreen. For instance, when choosing between a target who was friends with the rejector (positivity = 1) and a target who was a friend of a friend of the rejection (positivity = 2), participants preferred the latter. There was no significant main effect of condition ($b = -0.062$, $SE = 0.040$, $z = -1.54$, $p = .123$, 95 % CI = [−0.14, 0.017]) or interaction between positivity and condition ($b = 0.054$, $SE = 0.069$, $z = 0.78$, $p = .434$, 95 % CI = [−0.081, 0.19]), suggesting that participants generalized to similar extents across rejection and acceptance (Fig. 3, Table S4). Notably, the generalization gradient remained significant after we controlled for participants' memory accuracy during the network learning phase ($b = 0.36$, $SE = 0.068$, $z = 5.24$, $p < .001$, 95 % CI = [0.22, 0.49]), suggesting that the different patterns of generalization between conditions could not be explained by differences in memory (Table S7).

*Choices between Decider and slot machine*. Next, we tested whether participants generalized rejection and acceptance when they had the option of withdrawing from social interaction entirely, using responses on human-slot trials. We recoded participants' choices in each trial based on whether the human Decider was chosen against the slot machine (1: Yes; 0: No) and fitted a mixed-effects logistic regression predicting choice in each trial using 1) the Decider's positivity score, 2) condition, and 3) their interaction, while controlling for which slot machine was presented (−1: stingy, 1: generous).

Similar to the human-human trials, participants across both conditions were more likely to choose a novel Decider against a slot machine if the Decider had a higher positivity score ($b = 0.28$, $SE = 0.092$, $z = 3.03$, 95 % CI = [0.098, 0.46], $p = .002$), suggesting a generalization gradient. Notably, condition also had a significant main effect on choice, such that participants in the rejection condition were less likely to choose human Deciders against slot machines overall ($b = 0.90$, $SE = 0.22$, $z = 4.14$, $p < .001$, 95 % CI = [0.48, 1.33]) (Fig. 3, Table S4). Participants thus generalized expectations from the original rejector or accepter to the network as a whole, in addition to showing a more fine-grained generalization gradient. These effects remained significant after controlling for memory accuracy ($b$ positivity = 0.27, $SE = 0.091$, $z = 3.00$, $p = .003$, 95 % CI = [0.095, 0.45]; $b$ condition = 0.90, $SE = 0.22$, $z = 4.15$, $p < .001$, 95 % CI = [0.47, 1.32]) (Table S7). In summary, when choosing between humans and slot machines, participants not only showed a gradient of generalization based on the human target's distance from the original rejector and accepter, but also showed generalized avoidance of *all human targets* after rejection compared with acceptance.

**Post-task ratings**. One possible mechanism of generalization is that participants explicitly inferred that they were liked more by Deciders who were farther away from the rejector or closer to the accepter. To test this possibility, we fitted a mixed-effects linear regression, noted as a secondary analysis in the pre-registration, predicting participants' perceived liking by each novel Decider using the Decider's positivity score, condition, and their interaction, with a random intercept for participant and a by-participant random slope for positivity. Positivity had a positive effect on perceived liking ($b = 0.15$, $SE = 0.060$, $t = 2.58$, $p = .011$, 95 % CI = [0.037, 0.27], Table S13). Participants thought they were liked better by a Decider if the Decider was closer to the accepter or farther away from the rejector in the network.

Finally, following the pre-registration, we asked if rejection/acceptance by one Decider influenced participants' liking of the entire group of Deciders. A two-sample $t$-test suggests showed participants liked the group significantly better after acceptance than rejection ($t(141.64) = 4.10$, $p < .001$, $d = 0.33$), again demonstrating generalization to the group as a whole in addition to the more fine-grained generalization gradient.

### 5.3. Discussion

In Study 1, we investigated how people respond to rejection and acceptance within a social network, using a between-subject design to disentangle generalization of rejection and acceptance. When choosing between two novel Deciders, participants who had been rejected preferred Deciders who were farther away from the original rejector, whereas participants who had been accepted preferred Deciders who were closer to the original accepter. In other words, participants generalized both rejection and acceptance based on network ties.

We also let participants sometimes choose between a novel Decider and a slot machine—a design that allowed us to test participants' general tendency to approach or avoid social interaction after rejection and acceptance. We observed a similar gradient of generalization across these trials: participants were more likely to choose a human target (as opposed to slot machine) if the human target was farther away from the original rejector or closer to the original accepter. This finding indicates that the gradient of generalization is robust to these decision-making contexts. Notably, following rejection, participants also showed an overall tendency to avoid choosing human targets, regardless of their distance from the rejector. This finding is consistent with past work showing that the values attached to one member of a group can influence how people behave toward other members of the same group (Hackel et al., 2022) and indicates that rejection and acceptance can be generalized both along social network ties and via common group membership.

Although past work on negativity bias gives reason to think people might show stronger generalization of rejection than acceptance (Rozin & Royzman, 2001; Fazio et al., 2004), the slope of the generalization gradient was not significantly different between the two conditions in our data. One possibility is that participants expected some rejection in our paradigm, given the cover story, which may have reduced the negative impact of rejection. It is also possible that the present task promotes a gain frame: given economic incentives, participants might have construed acceptance as reward and rejection as a lack of reward (Babür et al., 2024), thereby reducing the negativity bias in partner choice. Alternatively, people may indeed generalize positive and negative feedback to a comparable degree.

Finally, we found evidence of generalization not only in participants' partner choices but also in their perceived liking by the Deciders. Participants thought they were liked less by targets who were closer to the rejector and those who were farther away from the accepter. This mechanism might explain why participants' partner choices were generalized across network ties: participants might have explicitly reasoned that targets who were closer to a rejector were more likely to reject them and thus should be avoided. We further explored this possibility in Study 2.

## 6. Study 2

Study 1 showed that people use generalization to make social choices following both social rejection and acceptance. After being rejected or accepted by one person in a social network, participants showed generalized avoidance and approach of novel targets based on their network distance to the original rejector and accepter.

However, it remains unclear which cognitive processes underlie these generalization effects. While the findings in Study 1 suggest that people may explicitly infer that targets who are closer in a network have similar tendencies to reject or accept them, the choice findings can also be explained by an associative learning account. Neutral stimuli may acquire positive or negative value when they co-occur with positive or negatively valenced cues (De Houwer et al., 2001; Wimmer & Shohamy, 2012); for instance, after seeing a picture of a tennis ball and a flower repeatedly co-occur, and later learning that the tennis ball is associated with reward, reward value spreads such that people become more likely to choose the flower picture relative to neutral alternatives when allowed to do so. This mechanism can explain how people form attitudes toward other humans without direct experience (FeldmanHall et al., 2017; FeldmanHall & Dunsmoor, 2019; Walther et al., 2005). For example, a neutral target who appears alongside a rejector might be perceived more negatively even if they are unrelated and do not share any traits. In this case, people would show generalized avoidance of a rejector's friends not because they infer shared rejection tendencies or relational value, but because those friends become automatically linked to the rejector through frequent co-occurrence. Alternatively, these choices might reflect more explicit inferences based on perceived diagnosticity: If people expect friends to be similar in their beliefs and attitudes, and/or to share information and coordinate with one another, they may see the rejector/accepter's behavior as diagnostic cues of how their friends will behave and therefore generalize their perception of rejection and acceptance. However, if two people are co-occurring by chance, then the rejection behavior of one target will not be informative of how the other person might behave and people should not show evidence of generalization.

In Study 2, we tested whether associative learning alone can explain the generalization effects observed in Studies 1 by manipulating the nature of the network ties. Specifically, we manipulated whether the network ties were based on friendship or arbitrary pairings. If associative learning alone gives rise to generalization, then we should observe equivalent generalization regardless of whether the network ties are based on friendship or arbitrarily defined. In contrast, if generalization requires the explicit inference that friends have similar tendencies to reject oneself, then we should observe generalization effects when the network ties represent friendship but not when the ties reflect arbitrary pairings.

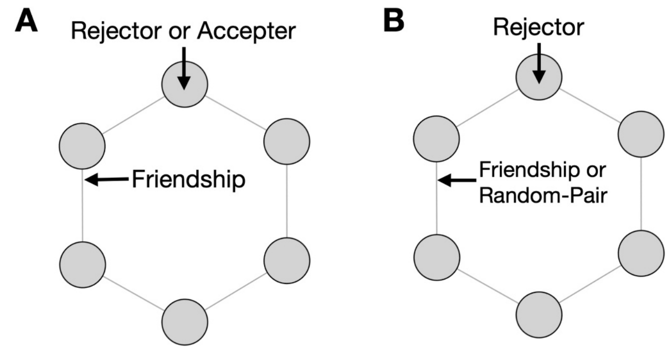


**Fig. 2.** Social network structures in Studies 1–2. **A:** In Study 1, participants interacted with six Deciders arranged in a ring-shaped network, with each Decider directly connected to two others. During instrumental learning, participants learned about one Decider—either a rejector or an accepter. **B:** In Study 2, participants learned about one Decider during instrumental learning who was always a rejector; in addition, we manipulated the nature of the network ties: in the friendship condition, participants were told the connections represented real friendships, whereas in the random-pairing condition, they were told the connections were arbitrarily assigned. Note that these ring-network diagrams are for illustrative purposes and were not shown to participants during the task.

### 6.1. Method

**Participants.** A priori power analysis using the *simR* package suggested that 200 participants would provide 80 % power to detect a small interaction effect between distance and condition ($\beta = 0.15$, Odds Ratio $= 1.16$). Anticipating attrition and exclusion, we recruited 400 participants for Session 1 using CloudResearch. Of these, 264 returned for Session 2. Based on the higher-than-expected exclusion rate in Study 1, we pre-registered a more liberal exclusion rule in Study 2 by removing data from those who either 1) failed to respond in at least 20 % of the instrumental learning trials, or 2) failed to reach at least 50 % (instead of 60 %) accuracy in the network memory test. These criteria led to the exclusion of 58 participants, leaving 206 participants for analyses (111 women, 92 men, three non-binary; $M$ age $= 39.7$, $SD = 11.5$). Sensitivity power analysis suggests 206 participants would provide 80 % power to detect an effect size of $\beta = 0.15$ (Odds Ratio $= 1.16$) or greater for the interaction term in a mixed-effects logistic regression. Informed consent was obtained from all participants in accordance with approval from the IRB of the authors' institution.

**Procedure**. Session 1 followed the same format as the previous studies, with participants completing self-disclosure questions, followed by individual difference measures. However, two changes were made to the questionnaires. First, we removed the rejection sensitivity (ARSQ) and loneliness questionnaires because they did not show any significant moderation effects in Study 1 or the pilot study. Second, we added a 5-item scale measuring participants' belief about homophily (e.g. "If two people are similar, then they are more likely to become friends", "People who are friends tend to like or dislike the same things" (*Cronbach's alpha* $= 0.77$; full list of items and results are reported in the Supplemental Materials). Session 2 also followed a similar design as Study 1, where participants completed instrumental learning, network learning, and generalization tasks, except for the following changes.

***Instrumental learning phase***. All participants were assigned to the rejection condition, in which they learned about a Decider who had a 20 % probability of matching with them in each round of the instrumental learning task. This change allowed us to focus on the instruction manipulation regarding network ties. In addition, to shorten the task, we reduced the number of instrumental learning trials from 60 to 45.

***Network learning phase***. Next, we manipulated the nature of the network ties by randomly assigning participants to a *friendship* or *random-pairing* condition (Fig. 2B). In the friendship condition, participants were told the Deciders had nominated each other as friends, mirroring Study 1. In the random-pairing condition, participants saw instructions stating that the Deciders were arbitrarily paired:

"In this part, we are interested in how well you can memorize information about the group of students. We randomly assigned the students to pairs of two. In this part, you will learn who has been paired with whom. Please note that each student could be paired with more than one other student."

Here, we intentionally left it vague how the pairing was determined in order to ensure that participants saw pairings of faces but could not form clear beliefs that the pairings reflected meaningful relationships. Throughout the rest of the task, we replaced the language about friendship with random-pairing for participants in the random-pairing condition. A comprehension question at the end of the study suggests that most participants understood the nature of the network ties in their condition (see "post-task measures" below). After learning the network ties, participants were tested for their memory following the same procedures as Studies 1 and 2 ($M$ accuracy before exclusion $= 73.0$ %, $SD =$ 19.3 %, $N$ excluded based on memory accuracy $= 50$). As in Study 1, memory accuracy did not significantly differ between the rejection and acceptance conditions (before exclusion: $M$ friendship $= 72.2$ %, $M$ random-pairing $= 73.9$ $t(257.21) = -0.72$, $p = .47$; after exclusion: $M$ friendship $= 79.0$ %, $M$ random-pairing $= 80.7$ %, $t(196.98) = -0.84$, $p = .40$), indicating that prior experiences of rejection and acceptance did not influence participants' encoding of the network ties.

***Generalization phase***. Next, participants made generalization choices without immediate feedback in a total of 56 trials. Half of the trials again involved choices between two Deciders and the other half involved choices between one Decider and one slot machine. These two trial types were intermixed. This phase was identical to that of Study 1.

***Post-task measures***. After generalization, participants rated how likely it was that each Decider had chosen to match with them for the trust game (1: Not at all – 7: Very much). This question, compared with the perceived liking measure in the prior studies, more directly measured participants' explicit inference about their likelihood of being accepted/rejected by each Decider. Participants also completed a comprehension check question indicating whether the network ties were based on 1) friendship, 2) random-pairing, or 3) something else (rate of correct response $= 89.9$ %). Because we did not pre-register task comprehension as an exclusion criterion, for the analyses below, we did not exclude any participant based on their response to this question. However, excluding participants who failed the comprehension question did not alter the direction or significance of our findings (see

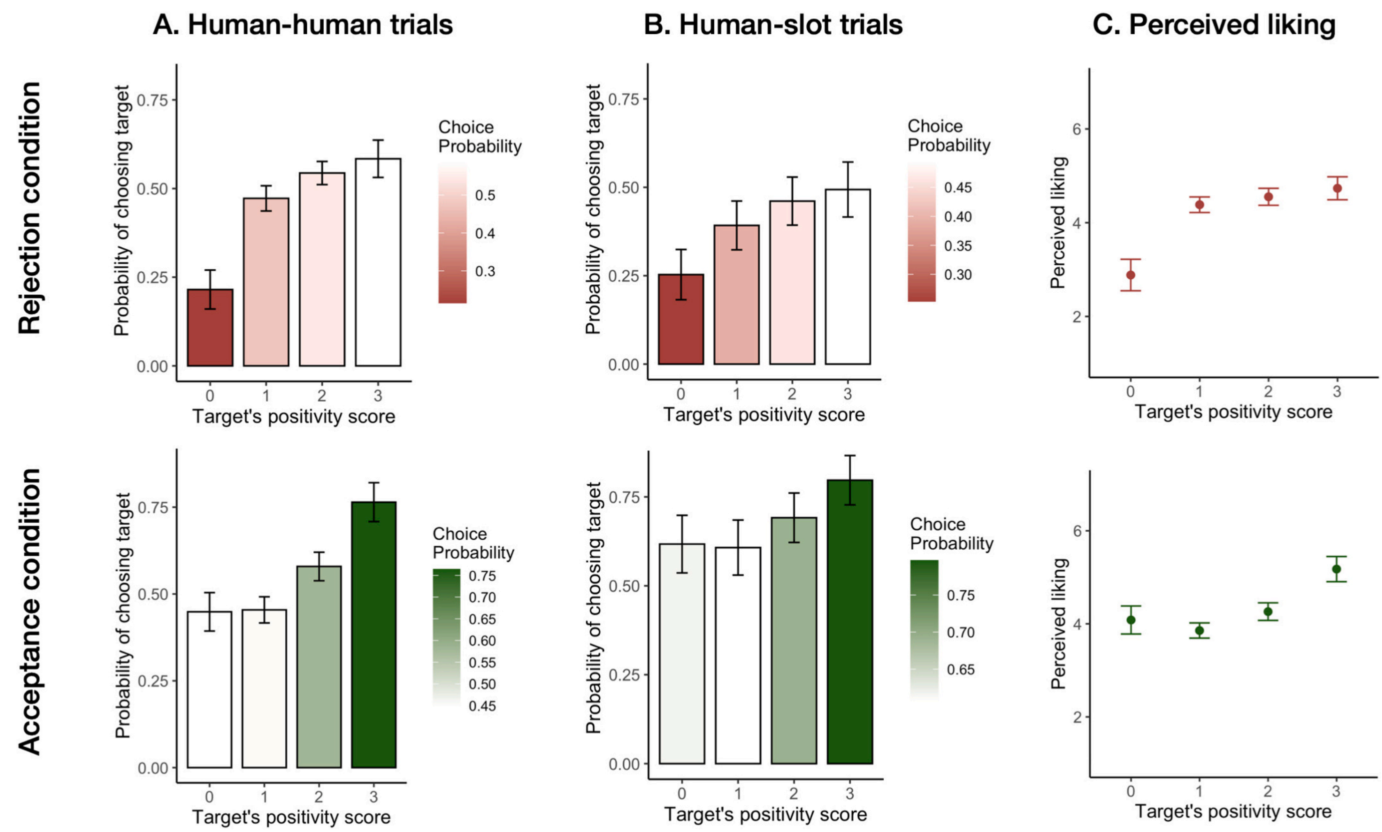


**Fig. 3.** Visualization of the gradient of generalization in Study 1 across partner choice and perceived liking. **A.** When choosing between two novel human Deciders, participants in both conditions were more likely to choose the Decider with higher positivity score, suggesting generalization for both rejection and acceptance. **B.** When choosing between a novel Decider and a slot machine, participants showed a similar gradient of generalization based on the Decider's positivity score. Notably, participants in the rejection condition also showed a general tendency to withdraw from social interactions, choosing all novel Deciders less frequently compared with participants in the acceptance condition. **C.** Averaged across both conditions, participants also generalized their explicit perceptions—perceiving more liking by novel Deciders with a higher positivity score. The error bars represent 95 % CI.

Supplemental Materials, Table S10). Finally, participants answered demographic questions and were debriefed.

### 6.2. Results

**Manipulation check**. We first checked whether participants successfully learned the difference among the Deciders and between the two slot machines. We calculated participants' average likelihood of choosing the rejector during the human-human trials and the probability of choosing each slot machine during the human-slot trials. A one-sample $t$-test suggests that participants were less likely to choose the rejector than chance level ($M$ choice = 21.1 % < 50 %, $t(205) = -18.67$, $p < .001$, $d = -1.30$) and a paired-samples t-test suggests that participants were more likely to choose the generous slot machine ($M = 74.0$ %, $SD = 29$ %) than the stingy slot machine (M = 47.8 %, $SD = 34.7$ %, $t(205) = 10.01$, $p < .001$, $d = 0.70$), indicating that participants learned the differential reward outcomes for the rejector and the slot machines.

**Generalization effects**. We asked whether participants generalized rejection based on network ties and whether the gradient of generalization depended on the nature of those ties (i.e. friendship vs. random-pairing). As in Study 1, we tested these effects separately using the human-human trials and the human-slot trials. The human-slot trials were noted in the pre-registration and the human-human trials were not; however, we include both analyses to replicate analyses from prior studies and because they reveal different aspects of generalization.

*Choices between two Deciders*. To test whether the patterns of generalization on the human-human trials differed between the friendship and random-pairing conditions, we first filtered out all trials involving the original rejector and then fitted a mixed-effects logistic regression model predicting trial-by-trial choices of the left-side Decider (1: Yes, 0: No) using 1) the left-side Decider's distance to the rejector relative to the right-side Decider, 2) condition (−1: Random-pairing, 1: Friendship) and 3) their interaction. We also added a random intercept for participant and a by-participant random slope for relative distance. Consistent with Study 1, we found an overall gradient of generalization: participants were more likely to choose a Decider as their distance to the rejector increased relative to the other Decider ($b = 0.17$, $SE = 0.060$, $z = 2.84$, 95 % CI = [0.053, 0.29], $p = .004$, Table S8). Thus, participants generalized rejection to novel targets who were closely connected to a rejector in the network.

If generalization is due to associative spread of value alone, then participants should show equivalent generalization across both conditions; in both conditions, they saw targets with network ties presented together with identical frequency, and the only difference between conditions was the instruction about what these ties meant. In contrast, if generalization depends on explicit beliefs about the diagnosticity of friendship, then participants should generalize only when they believe that network ties reflect friendship. Supporting the latter account, we found a significant interaction between distance and condition, such that the gradient of generalization was stronger in the friendship condition than the random-pairing condition ($b = 0.13$, $SE = 0.60$, $z = 2.10$, $p = .036$, 95 % CI = [0.008, 0.24], Table S8). Analyses of simple effects suggest that this interaction was driven by a significant effect of distance on choice in the friendship condition ($b = 0.30$, $SE = 0.09$, $z = 3.23$, $p = .001$, 95 % CI = [0.12, 0.49]), and a non-significant effect of distance in the random-pairing condition ($b = 0.04$, $SE = 0.07$, $z = 0.56$, $p = .572$, 95 % CI = [−0.10, 0.19]). These findings indicate that participants generalized rejection based on friendship ties but did not generalize

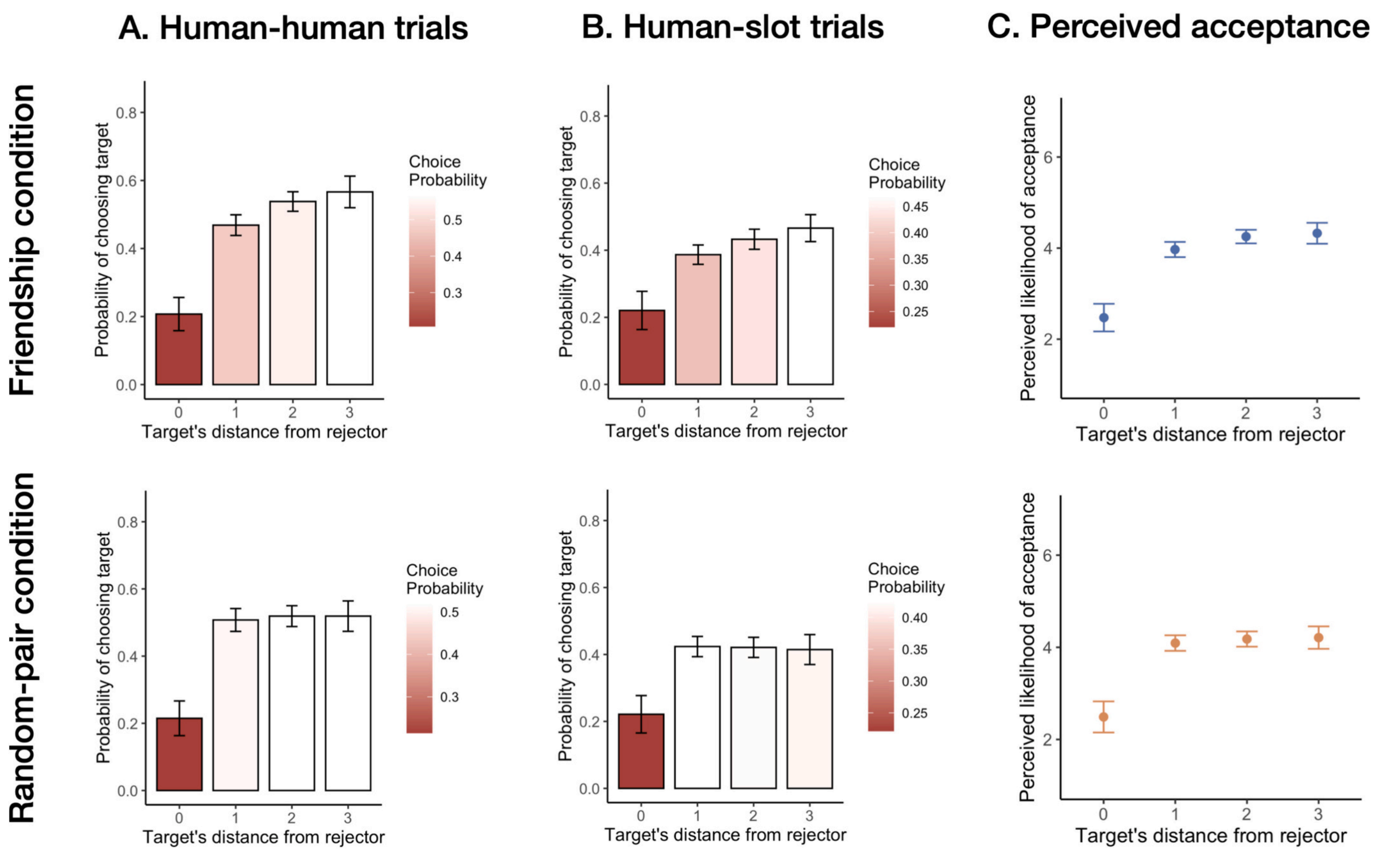


**Fig. 4.** Visualization of the gradient of generalization in Study 2 across partner choice and perceived acceptance. **A.** When choosing between two human Deciders, participants in the friendship, but not random-pairing, condition were more likely to choose the target who was relatively farther from the rejector, suggesting that generalization depends on knowledge of friendship. **B.** When choosing between a human Decider and a slot machine, participants showed a similar trend, preferring human targets farther from the rejector in the friendship, but not the random-pairing condition. **C.** Averaged across both conditions, participants also generalized their explicit perception of acceptance, perceiving a higher likelihood of acceptance by novel Deciders who were farther away from the rejector. All error bars represent 95 % CI.

based on random-pairing. Notably, these effects remained significant after we controlled for participants' memory accuracy during the network learning phase ($b$ = 0.17, $p$ = .004; $b$ = 0.13, $p$ = .027, respectively), suggesting that the different patterns of generalization between conditions were not due to differences in memory (Table S9).

*Choices between Decider and slot machine*. Next, we examined whether generalization happened for choices on the human-slot trials. We fitted a mixed-effects regression model predicting whether participants' chose a novel human Decider (1: yes, 0: no) using the Decider's distance from the rejector, condition, and their interaction, while controlling for which slot machine was present (1: generous, −1: stingy). Across conditions, the Decider's distance from the rejector did not significantly predict their probability of being chosen ($b$ = 0.11, SE = 0.065, $z$ = 1.68, $p$ = .094), 95 % CI = [−0.018, 0.24]), indicating that there was no significant gradient of generalization averaged across the friendship and random-pairing conditions. However, the interaction between distance and condition was significant, such that the gradient of generalization was stronger in the friendship condition than the random-pairing condition ($b$ = 0.17, SE = 0.063, z = 2.73, $p$ = .006, 95 % CI = [0.048, 0.30], Fig. 4B and Table S8). Specifically, participants chose Deciders based on distance in the friendship condition ($b$ = 0.28, $SE$ = 0.096, $z$ = 2.96, $p$ = .003), 95 % CI = [0.096, 0.47]), but not in the random-pairing condition ($b$ = −0.046, $SE$ = 0.084, $z$ = −0.54, $p$ = .588), 95 % CI = [−0.21, 0.22]). The two-way interaction between distance and condition remained significant after we controlled for memory accuracy ($b$ = 0.18, $p$ = .005) (Table S9). Thus, across both human-human and human-slot trials, participants generalized rejection based on friendship ties but not randomly paired ties.

**Post-task ratings.** To further understand generalization, we examined whether participants formed explicit belief that Deciders who were closer to the rejector were more likely to reject them. We fitted a mixed-effects linear regression model, noted as a secondary analysis in the pre-registration, predicting participants' perceived likelihood of acceptance by each novel Decider using 1) the Decider's distance from the rejector, 2) condition, and 3) their interaction, with a random intercept for participant and a by-participant random slope for distance. As expected, distance positively predicted perceived acceptance ($b$ = 0.13, $SE$ = 0.053, $t$ = 2.42, $p$ = .016, 95 % CI = [0.025, 0.23], Table S13). However, the interaction between condition and distance did not have a significant effect on ratings ($b$ = 0.065, $SE$ = 0.053, $t$ = 1.23, $p$ = .22, 95 % CI = [−0.039, 0.17], Table S13). Thus, while participants inferred that Deciders closer to the rejector in the network were more likely to reject them, the strength of this effect did not significantly depend on the type of network ties.

To assess whether this generalized perception of acceptance shaped decision-making, we conducted a mediation analysis, noted as a secondary analysis in the pre-registration, testing whether perceived likelihood of acceptance mediated the effect of distance on participants' choice during the generalization phase. Results suggest that participants thought they were more likely to be rejected by Deciders closer to the rejector, which in turn made them more likely to avoid those Deciders during generalization. Full details of the mediation analysis are described in the Supplemental Materials (Table S14).

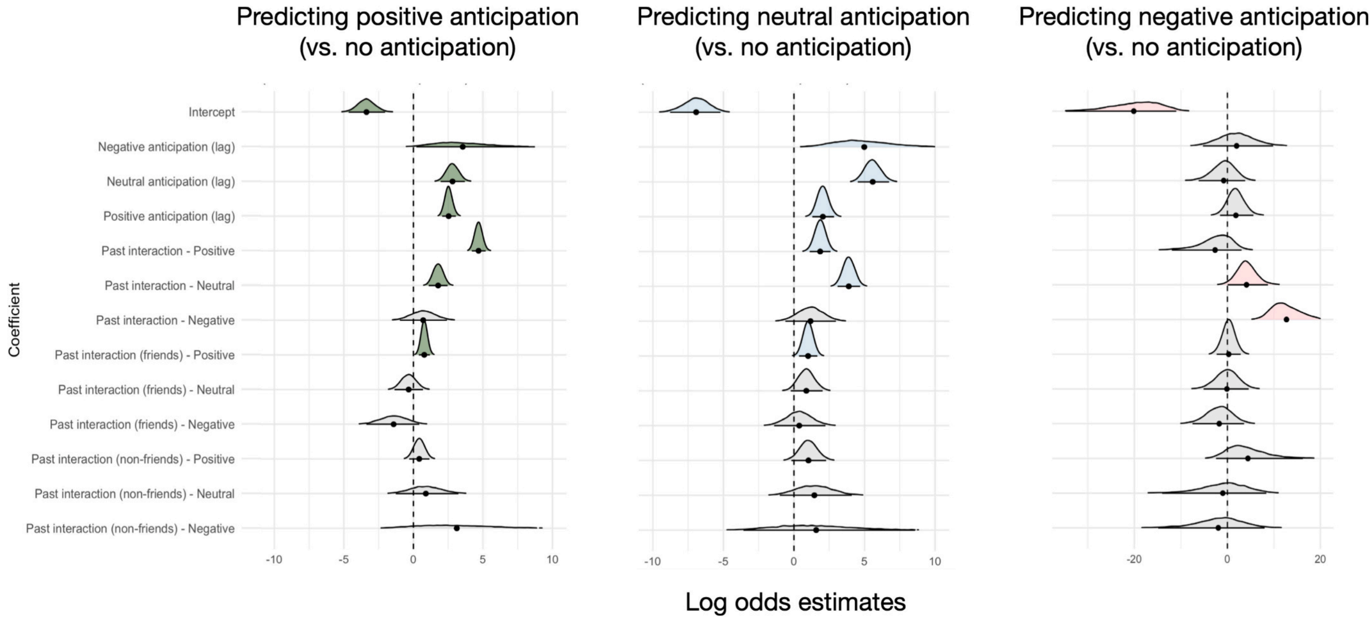


**Fig. 5.** Bayesian multinomial logistic regression predicting anticipated interaction quality. Figure shows posterior distributions (in log-odds) for the coefficients in the Bayesian multinomial logistic regression model. The left panel represents coefficients predicting positive anticipated interactions (relative to no anticipation); the middle panel represents coefficients for variables predicting neutral anticipation, and the right panel represents coefficients for variables predicting negative anticipation. Lagged variables reflect measurements from the previous timepoints. Past interaction variables reflect interactions reported in the interval since the previous timepoint. The shaded curves depict the estimated distribution for each predictor, with black dots marking the posterior means and horizontal lines indicating 95 % credible intervals. To aid interpretation, posterior distributions with 0 falling inside the 95 % credible intervals were colored in grey, indicating lack of strong evidence for an effect.

### *6.3. Discussion*

Study 2 further examined the mechanisms underlying the generalization of rejection. Across both human-human and human-slot trials, participants showed a gradient of avoidance based on social network distance, and this gradient was steeper when the Deciders were connected by friendship than when they were arbitrarily paired. This finding suggests that inferences based on the knowledge of friendship ties contributed to generalization of rejection. Moreover, no gradient of generalization was found in the random-pairing condition, suggesting that associative learning alone did not give rise to generalization.

One alternative explanation is that participants might have encoded the network structure differently between the two conditions. For example, during network learning, the label "friend" could have increased participants' attention to the associations, thereby boosting encoding and leading to stronger generalization. In contrast to this notion, memory accuracy for the network structure did not significantly differ between conditions, and the effect of condition on generalization remained significant when controlling for memory accuracy. These findings indicate that the stronger generalization of rejection in the friendship condition did not simply result from a better memory for the network structure, but reflected the inferences participants made based on the friendship ties.

Notably, replicating Study 1, participants not only generalized social choices based on network ties but also generalized explicit inferences: averaged across both conditions, participants believed they were more likely to be accepted by targets who were farther away from the rejector, showing a gradient of generalization in their explicit beliefs. However, the slope of this generalization effect did not significantly differ between the friendship and random-pairing conditions and could be further tested in future work.

## 7. Study 3

Collectively, Studies 1–2 demonstrate that people used their knowledge of social network structure to make generalized inferences about rejection and acceptance and chose social partners based on those inferences. In Study 3 we aimed to test generalization in real-world contexts, which involve more complex network structures and relationship dynamics.

Notably, in Studies 1–2, we used a behavioral definition of relational value feedback, defining it based on the rejection and acceptance feedback participants received in the trust game. In the real-world, however, relational value can be manifested in many ways other than explicit rejection and acceptance (e.g., how another person treats us, how frequently another person interacts with us, etc.). Therefore, we measured participants' broader *interaction quality* with the group members, along with the quantity of interaction, rather than focusing on explicit rejection and acceptance alone.

To test whether participants' social preferences generalized along social network ties, we conducted an eight-week longitudinal study with eight student organizations. During each wave of the experiment, we measured participants' relationships in the network, their social interaction experiences over the last two weeks, and their anticipated interaction quality each group member over the next two weeks. This allowed us to test whether social network ties and past experiences together shape social preferences, such that having a positive interaction with one individual leads people to anticipate positive interactions with that individual's friends.

### *7.1. Method*

**Participants.** We used data from a larger study designed to provide insight into group membership and well-being among young adults. The study involved members of student organizations completing four longitudinal surveys. Each follow-up survey took place two weeks after the previous one and participants had two weeks to complete each survey. The study therefore took eight weeks for each student organization. In the baseline (i.e. first) survey, participants completed 1) social network nominations, 2) measures of past and anticipated interaction quality, 3) group-related questions (e.g. collective identification), 3) trait questionnaires (e.g. loneliness), and 4) demographic questions. In each follow-up survey, questions 1, 2, and 3 were asked again to assess how participants' relationships and perceptions changed over time. Here we only report measures that are relevant to the current research questions. A full list of measures can be found in the Supplemental Materials.

We aimed to recruit 100 participants from multiple student organizations. The sample size was determined based on a mix of factors, including available funding, recruitment timeline, and the pace of enrollment based on the number of interested and eligible groups. The sample size was comparable to past research with a similar design (Häusser et al., 2023; Mojzisch et al., 2021). By the end of the school year, we reached out to 200 individuals from eight student organizations; of those, 120 signed up for the study and 97 completed at least one survey (79 women, 10 men, four non-binary, four did not report; *M* age = 20.00, SD = 1.79). In addition, 64 participants completed all four waves (baseline $n = 95$, second survey $n = 85$, third survey $n = 77$, fourth survey $n = 72$). Thus, missing data arose both from individuals who did not enroll ($n = 80$) in the study and from gradual attrition across waves ($n = 33$). Among enrolled participants, attrition was not systematically related to measures of centrality in the close friends network (although it related to centrality in networks related to group processes; see Supplemental Materials and Table S19).

The student organizations included performing arts groups ($n = 2$), sororities or fraternities ($n = 2$), sports clubs ($n = 1$), and academic groups ($n = 3$), with group size ranging from 9 to 42 and participation rate ranging from 26 % to 83 %. We completed sensitivity power analysis based on a frequentist version of the Bayesian model we report below. Simulation indicated that, with 97 participants, we had 80 % power to detect an effect of $\beta = 0.09$ (standardized beta) or greater with a 5 % false-positive rate.

**Network nominations**. In order to characterize interpersonal ties and test potential gradients of generalization, we collected network nominations from each group member at each wave of the study. Prior to the baseline survey, we collected the roster of each group and created a unique survey for each group by inserting the group members' names under each network nomination question. To measure friendship ties, we asked participants "which group members are you closest to?" Participants saw all group members' names at once and were asked to select as many or as few names as they'd like.

**Past interaction quality**. To characterize positive and negative social experiences, we measured participants' past interaction quality with each other by asking them to rate how their interaction went with each group member over the past two weeks on a 5-point Likert scale (1 (very negative), 2 (slightly negative), 3 (neutral), 4 (slightly positive), 5 (very positive), or Not Applicable ("I did not interact with this person").

**Anticipated interaction quality**. To measure expectations about future interactions, we asked participants how they thought their interaction would go with each group member in the next two weeks, using the same scale.

**Perceived acceptance by the group**. We also included a measure asking participants how much they felt accepted by their group as a whole in each wave of the study. This question was administered on a 7-point Likert scale (1 (not at all), 5 (moderately), 7 (very much)).

### 7.2. Results

**Data transformation**. If individuals generalize positive and negative social experiences across their network, then their anticipated interactions with a target should depend not only on past interaction with that target but also on past interactions with the target's *friends*. Furthermore, if this generalization follows a gradient, then anticipated interactions should be influenced to a lesser extent by past interactions with the target's *non-friends*.

To test this hypothesis, we transformed the data as follows: for each participant (i.e. the ego), we identified all other individuals in the network who also completed the survey (i.e. the alters). For each alter, we determined that alter's friends and non-friends based on the alter's nominations in response to the closeness question. Because participants nominated close friends in each of the four waves of the study, each participant's friends and non-friends could differ slightly in each wave, reflecting the dynamic structure of the network. Next, for each participant-alter pair, we computed the participant's average past interaction quality with 1) the alter, 2) the alter's close friends and 3) the alter's non-friends.

Importantly, when anticipating future interactions, participants could indicate that they expected no interaction or, if they did expect an interaction, they could indicate how negative or positive they thought that interaction would be. We therefore used an analytic strategy that could account for both types of responses (no interaction or a continuous interaction score). Our rationale was that "No interaction" responses can meaningfully reflect negative expectations after a bad interaction or a lack of opportunity to interact with a target; as a result, these responses could not be excluded or analyzed separately, which would lead a considerable amount of data to be missing not at random. Instead, we aimed to keep all responses, allowing us to test whether positive interactions led subjects to anticipate more positive interactions (relative to no interactions). We therefore transformed responses in a manner that would permit us to analyze all responses as categorical variables. Specifically, we recoded past interaction quality scores into four categories reflecting whether the interaction was positive (raw score above 3), neutral (raw score was equal to 3), negative (raw score was below 3), or no interaction. These categories were represented with three binary dummy variables for positive, neutral and negative interactions, respectively, with a 1 if the trial belonged to that category and a 0 otherwise. If a participant selected "no interaction", all three dummy variables for that interaction were set to 0, indicating no reported interaction. This coding scheme allowed us to capture both the *valence* of interactions and the *presence or absence* of interaction in the same analysis. Similarly, to address "no interaction" values in anticipated interaction, we recoded the raw responses into four equivalent categories: *positive anticipation* (scoring higher than 3), *neutral anticipation* (scoring equal to 3), *negative anticipation* (scoring lower than 3), or *no anticipation* (when "no interaction" was selected).

**Generalization of interaction quality**. To examine how past interaction with a target and their friends shapes anticipated interaction quality with the target, we fitted a Bayesian multinomial logistic regression model using the brms package in R (Bürkner, 2017). A Bayesian model was used because the predictors were highly unbalanced (where interactions were predominantly positive, with relatively few neutral and negative interactions), in which case a Bayesian approach would produce more stable estimates than frequentist approaches (Pérez-Millan et al., 2022). A multinomial model was used given the categorical nature of the outcome variable. The model predicted anticipated interaction quality (negative, neutral, positive, or no anticipation, with no anticipation being the reference category) using nine dummy variables: three representing participants' quality of past interaction with each alter, three representing past interaction with each alter's friends, and three representing past interaction with each alter's non-friends. Moreover, we controlled for anticipated interaction quality in the previous wave (negative, neutral, positive, or no anticipation). This allowed us to assess how people *change* their anticipation based on recent experiences. To account for the hierarchical structure of the data, this model also included random intercepts for group, participant, alter, and dyadic relationships in each participant-alter pair. We did not have strong prior expectation of the frequency of each type of interaction or the amount of variation within subjects and groups; therefore, following recommendations of prior work (Gelman, 2006), we used weakly informative priors (e.g. $b \sim t(4, 0, 5)$ for fixed-effect coefficients and intercepts, $\sigma \sim Half\text{-}t(4, 0, 2.5)$ for random intercepts; see Supplemental Materials for full model specification). Our model ran four Markov Chain Monte Carlo (MCMC) chains with 2000 iterations each, discarding

1000 warm-up samples. Model convergence was confirmed and the full details can be found in the Supplemental Materials (no divergent transitions found, Rhat =1.00 for all parameters, bulk/tail ESS > 2000, good mixture of the MCMC chains post-warmup, Fig. S4, S5, Table S15). As a robustness check, we also re-estimated the model with alternative weakly informative priors (e.g. $b \sim Normal(0,1)$, $\sigma \sim Half\text{-}Normal[0,1]$), and the results yielded the same inferences (see Supplemental Materials, Table S16).

If participants engage in direct learning from social experience (Cho & Hackel, 2022), then a positive experience with a group member should lead participants to expect more favorable future interactions with that individual whereas a negative experience should lead to less favorable expectations. Indeed, participants who reported positive interactions with a target showed strong evidence of anticipating positive interactions with the same target in the future relative to no anticipation ($b$ = 4.69, 95 % credible interval = [4.20, 5.19], P($b$ > 0) = 1.00); they were also more likely to anticipate neutral interactions over no anticipation, though to a lesser extent ($b$ = 1.86, 95 % credible interval = [1.11, 2.59], P($b$ > 0) = 1.00). In contrast, participants who reported negative interactions with a target had a high posterior probability of anticipating negative interaction with the target relative to no anticipation ($b$ = 12.70, 95 % credible interval = [7.48, 20.23], P (b > 0) = 1.00). These results suggest that participants learned from direct social experience (Fig. 5, Table S15).

More importantly, however, if participants generalized these experiences along friendship ties, they should also expect more positive interactions with a given individual if they had more positive interactions with that individual's *friends* in the past two weeks. Consistent with this prediction, experiencing positive interactions with a target's friends predicted greater likelihood of anticipating positive interaction with the target, relative to no interaction ($b$ = 0.78, 95 % credible interval = [0.39, 1.19], P($b$ > 0) = 1.00). However, there was no strong evidence that experiencing neutral or negative interaction with a target's friends predicted positive anticipation ($b$ neutral = −0.34, 95 % credible interval = [−1.36, 0.69], P($b$ > 0) = 0.25; $b$ negative = −1.42, 95 % credible interval = [−3.38, 0.43], P(b > 0) = 0.07). There also was no strong evidence that interactions with a target's *non-friends* would predict positive anticipation for the target ($b$ positive = 0.42, 95 % credible interval = [−0.31, 1.15], P(b > 0) = 0.87; $b$ neutral = 0.89, 95 % credible interval = [−1.27, 3.24], P(b > 0) = 0.78; $b$ negative = 3.12, 95 % credible interval = [−1.68, 10.24], P(b > 0) = 0.86). Together, these results indicate that participants generalized their positive social experiences along friendship ties to form expectations about future interactions, a pattern consistent with the gradient of generalization observed in Studies 1–3 (Fig. 5, Table S15).

Our model also explored the predictors of *neutral* anticipation. Participants were more likely to anticipate *neutral* future interaction with a target if they had previously experienced neutral interactions with the target ($b$ = 3.88, 95 % credible interval = [3.09, 4.69], P(b > 0) = 1.00), positive interactions with the target ($b$ = 1.86, 95 % credible interval = [1.11, 2.59], P(b > 0) = 1.00) or positive interactions with the target's friends ($b$ = 1.00, 95 % credible interval = [0.36, 1.66], P(b > 0) = 1.00). In other words, neutral anticipations, like positive ones, were shaped not only by direct experiences with a target, but also by positive and neutral interactions with the target's friends. (Fig. 5, Table S15).

**Anticipated interactions as a predictor of perceived group acceptance.** As an exploratory analysis, we tested whether participants' anticipated interactions related to their overall feelings of acceptance by the group. Past work suggests both the quantity and quality of social interactions relate to feelings of acceptance (Enting et al., 2024; Sun et al., 2020); thus, at each time point, we computed two metrics for each participant: the *quality* of anticipated interactions (i.e. the mean valence of anticipated interactions for all group members) and the *quantity* of anticipated interactions (i.e. the proportion of anticipated interaction out of all possible interactions within one's group). We then decomposed each predictor into a between-subject component (the subject's mean across timepoints, mean-centered at the group level) and a within-subject component (the subject's scores across timepoints, mean-centered at the within-subject level). The between-subject component reflected each participant's anticipations relative to *other* participants, whereas the within-subject component reflected the fluctuations of anticipation across the four waves, capturing longitudinal change. We then fitted a linear mixed-effects regression predicting perceived acceptance in each wave using these predictors. We also included wave number as a fixed factor to control for any overall time trend. Random intercepts were specified for participants nested within groups to account for repeated measures and group clustering.

Both quality and quantity of interactions positively predicted perceived group acceptance, across not only between-subject differences (quality: $b = 0.89$, $SE = 0.23$, $t = 3.95$, $p < .001$, 95 % CI = [0.45, 1.34]; quantity: $b = 1.43$, $SE = 0.42$, $t = 3.40$, $p = .001$, 95 % CI = [0.61, 2.26], Table S17) but also within-subject changes across waves (quality: $b = 0.56$, $SE = 0.12$, $t = 4.58$, $p < .001$, 95 % CI = [0.32, 0.80]; quantity: $b = 0.60$, $SE = 0.24$, $t = 2.52$, $p = .012$, 95 % CI = [0.13, 1.06], Table S17). In other words, when a participant anticipated more interactions and higher-quality interactions with group members, they also perceived more overall acceptance from the group.

### 7.3. Discussion

Study 3 aimed to extend the findings of Studies 1–2 to real-world contexts. Across multiple waves, members of student organizations indicated whether they had positive, neutral or negative experiences with other group members. Consistent with Studies 1–2, we found that positive interactions with a target in the past not only led participants to anticipate positive interactions with that target in the future but also with that target's friends. This finding suggests that generalization mechanisms not only influence who we connect with in computer-mediated interactions or novel groups but also shape social connections in familiar real-world settings. Notably, Study 3 focused on overall experiences of positive and negative interactions, which may include broader forms of negative interaction (e.g., exclusion, ostracism, or conflict), demonstrating robustness to different operational definitions. These findings indicate that these broader forms of social expectations can be generalized in a similar way as more controlled experiences of rejection and acceptance.

Our analyses also revealed some unexpected findings. First, *neutral* interactions with a target led participants to expect not only more neutral interactions with the target in the future, but also more positive *and* negative interactions (relative to no interaction). One explanation is that neutral interactions make a target more salient as a potential interaction partner (Bayer et al., 2020), increasing general expectations of future engagement across different valence categories. Second, *positive* interactions with a target's friends led participants to anticipate *neutral* interactions with the target relative to no anticipation. This could suggest that participants who had positive experiences with a target became more open to future interactions with the target's friends, even if they did not expect those interactions to be especially positive.

Interestingly, while participants generalized past experiences to form positive and neutral expectations about the future, we did not find evidence that generalization influenced participants' anticipation of *negative* interactions. One possible reason is that there were too few cases of genuinely negative anticipation (16 out of 3015 nominations, or 0.5 %) to detect such an effect. Alternatively, participants may have avoided expressing negative expectations due to social desirability or perceived group norms. It is also possible that negative expectations were genuinely less influenced by indirect social learning and were more dependent on direct negative experiences.

Study 3 also has several limitations. As noted above, the interaction quality measure was designed to allow a broader range of real-world kinds of rejection; at the same time, this broad framing makes it less precise and may reflect additional constructs such as general liking or expectations of positivity in group settings. This ambiguity allows additional explanations for the observed generalization effects. For example, participants may have anticipated better interaction quality with the friends of an accepter because they expected the accepter to be present during that interaction. Notably, participants' expectations of interaction quality closely tracked their perceived acceptance by the group as a whole in each wave of the study, supporting the notion that anticipated interaction ratings related to perceptions of relational value. Nonetheless, future work can aim to replicate these findings with more targeted measures that directly assess relational value in isolation from related constructs.

Another limitation of Study 3 is the imperfect enrollment rate and attrition across the four waves. As a result, the observed patterns of generalization may not be representative of how non-participating group members would behave. While this is a common issue for longitudinal studies of real-world social networks (Borgatti & Molina, 2005; de la Haye et al., 2017), future work could further optimize survey design and recruitment strategies to obtain more representative samples (Agneessens & Labianca, 2021; Birkett et al., 2021).

## 8. General discussion

In complex social environments, people often need to quickly and accurately identify potential social partners they can connect with or individuals who might reject them. One way to do so is by tracking the relational value others ascribe to them—an ability that helps individuals fulfill the need to belong and maintain social connections important to well-being. Across three studies (two lab experiments and one longitudinal study), we examined how individuals generalize their experiences of rejection and acceptance and make inferences about their relational value in social networks.

Using a novel paradigm, Study 1 found that after initial rejection and acceptance, participants readily generalized their experiences to novel members of the same group, avoiding targets closely connected to the rejector while approaching those closely connected to the accepter. Importantly, Study 2 revealed that this gradient of generalization emerged only when network connections reflected meaningful friendships rather than arbitrary pairings, suggesting that generalization of rejection depended on inferences about relationships above and beyond associative learning alone. Moreover, participants used the network structure to explicitly infer whether others liked them (Study 1) or were likely to accept them (Study 2), extending these findings beyond approach or avoidance behaviors alone. This finding indicates that participants indeed inferred relational value from acceptance and rejection, rather than solely forming their own likes and dislikes of others based on friendship ties.

In real-world social interactions, people sometimes make choices between two individuals (e.g., choosing whom to approach when help is needed) and sometimes make choices about whether to engage in interaction at all (e.g., deciding whether to get lunch alone or to invite a colleague). In Studies 1–2, similar patterns of generalization held across these two types of choices, both when participants chose between two human targets and when they decided whether or not to interact with one human target. Once participants inferred likely acceptance or rejection through generalization, they used this information both to make relative choices between partners and absolute choices about solitude versus interaction.

Study 3 built on these findings by examining how generalization unfolds in naturalistic social networks. Using a longitudinal design with student organizations, we found that positive interactions with one group member led participants to anticipate more positive interactions with that individual (relative to no interaction), supporting prior work on direct learning of social acceptance in more controlled settings (Babür et al., 2024; Cho & Hackel, 2022). Importantly, participants also anticipated more positive interactions with that individual's close friends (relative to no interaction)—but not with non-friends in the network. These positive anticipations in turn predicted participants' perceived acceptance by their group. Thus, even outside controlled lab environments, individuals use network ties to make inferences about social interactions in a generalization gradient.

### 8.1. *Contribution to prior research*

Our findings contribute to prior work in several ways. First, our work builds on the idea that social interaction requires two types of inferences: how much we *value others* (e.g., trust or respect them) and how much other people *value us* (i.e. place "relational value" on us; Leary, 1999, 2005; Cho & Hackel, 2022; Babür et al., 2024). Whereas prior research shows that perceivers generalize perceptions of trustworthiness from one person to another (FeldmanHall et al., 2018; Martinez et al., 2016; Schwyck et al., 2024), we show that individuals can similarly generalize inferences about how much they are trusted by others, experiencing generalized acceptance or rejection toward unfamiliar group members. In this manner, generalization happens not only for the perceived traits of others, like trustworthiness, but also perceptions of relational value including rejection and acceptance. Notably, in the design of Studies 1–2, participants had no basis for inferring global traits; instead, they had to infer how others value them relative to other available partners, suggesting that relational value generalization cannot be reduced to trait generalization.

Second, Studies 1–3 extend past work by showing that people not only generalize social preferences from a target to their friends, but that they also use their knowledge of relationships in the *entire social network* to form a *gradient* of avoidance and approach, highlighting a nuanced form of generalization in group contexts. Whereas past work demonstrates gradients in how behaviors diffuse through social networks (Christakis & Fowler, 2007; Fowler & Christakis, 2008), the present findings demonstrate that perceivers expect these gradients and use these expectations to guide decisions about whom to approach.

Third, our work speaks to the mechanisms underlying the generalization of rejection and acceptance. In Studies 1–2, participants not only generalized social choices but also generalized *explicit inferences*: they thought they were less liked and had a lower probability of being accepted by group members who were closer to the rejector or farther away from the accepter. Moreover, by comparing generalization across friendship ties versus random pairings, Study 2 found that mere association alone could not explain the generalization of partner choice we observed here.

These findings are consistent with the idea that people generalize when information is perceived as diagnostic, or meaningfully indicative of how others will behave (Cone & Ferguson, 2015; Hamilton et al., 2015; Skowronski, 2002). In the present work, participants might have regarded the behaviors of the rejector and accepter as informative about how their *friends*, but not *randomly-paired group members*, would behave and therefore only generalized in the friendship condition. At the same time, the diagnosticity of the rejector/accepter's feedback might also depend on other factors. For instance, if one member of the group randomly rejects others or does so only due to unusual circumstances like a personal stressor, this feedback would not be diagnostic of what others are likely to do. Similarly, if an observer witnesses a group member reject a potential partner for reasons clearly specific to that partner, this information also would not be meaningfully informative of how the observer will themselves be treated. Future work can manipulate other forms of diagnosticity to test how these inferences shape generalization.

Finally, our work combines the benefits of highly-controlled lab experiments and naturalistic data to examine generalization. While the social learning paradigm used in Studies 1–2 helps identify precise mechanisms of generalization, Study 3 complemented these results by showing that generalization of social preferences also occurs in real-world social networks, which involve more complex structures and relationship dynamics. Together, these studies help establish the robustness of the generalization mechanism and highlight its real-world impact.

### 8.2. Limitations and future directions

In the present work, we focused on how people learn about their relational value through generalization of direct learning (i.e. reaching out to others and receive feedback). However, other learning mechanisms, such as observational learning, may also shape social preferences in social networks (Lindström et al., 2019). For example, observing a group member being rejected might lead individuals to avoid interacting with the rejector and their friends, even in the absence of any direct experience. This might be especially true if observing one's own friends experiencing rejection, which might lead individuals to infer that they themselves are likely to be rejected in the future. Future work can separately model these processes and try to tease them apart.

One limitation of the current work is that it leaves open the precise inferences that led participants to generalize their perceptions of relational value. The present findings suggest that people generalize rejection and acceptance in part because they explicitly infer that friends will have similar tendencies to reject or accept them. Yet, this inference could depend, in turn, on a few lay beliefs. First, people may generalize because they infer that friends tend to have similar social preferences and therefore come to similar conclusions about whom to trust (*homophily in social evaluation*), even without coordinating with each other. Second, because our cover story allowed the possibility that Deciders communicated before making their choices, participants might also assume that the Deciders would emulate their close friends or be influenced by gossip (*social influence*). Indeed, people tend to be aware that gossip can drive ostracism (Feinberg et al., 2014) and they may generalize based on this expectation. Relatedly, the current work does not address whether generalization depends on relatively more heuristic inferences about relationships or explicit reasoning processes. Given that either type of process can lead to generalization (Gigerenzer & Gaissmaier, 2011; Tenenbaum & Griffiths, 2001), and given that both processes would depend on inferences about relationships above and beyond association alone, the present research leaves open these possible pathways. However, the same paradigm could be used to further tease apart mechanisms of generalization—for instance, dissociating homophily beliefs and social influence beliefs by manipulating participants' expectations of homophily and belief in how social information is transmitted in the networks.

**Generalization in various social network contexts**. While the present studies documented similar generalization patterns across artificial and real-world social networks, future work can continue to examine how network structure modulates the generalization effects. First, structural features of social networks (e.g. size, density, and clustering) might influence the way people update their mental models following social rejection and acceptance. For example, as network size increases, it might become increasingly challenging to keep track of the specific relationship ties in a network; in such situations, people might rely less on a detailed "gradient" of generalization and instead generalize rejection and acceptance based on inferred, rather than actual, network ties (Aslarus et al., 2025; Son et al., 2021; Son et al., 2023). Similarly, forming a strict gradient of avoidance or approach might be less beneficial in high-density groups, where most members are only one or two degrees apart and network distance provides less distinguishing information. Future work could test how social network structure influences cognitive load and mental representation for networks, and how these factors subsequently influence generalization.

Second, generalization may also be modulated by the network position of the rejector and the accepter. In our lab experiments, all Deciders have the same number of ties. However, members of real-world social networks differ in network positions such as centrality. To the extent people generalize rejection across friendship ties, rejection by a central member (i.e. with many friends) could propagate farther and bias attitudes toward the entire group, whereas rejection by a peripheral member may carry less weight.

Third, people may form different expectations about rejection and acceptance across different types of networks, which in turn leads to different generalization strategies. For example, when people expect rejection to be rare, they may see any single instance of rejection as especially informative (Hamilton et al., 2015) and thus show stronger generalized avoidance from the rejector to their friends. Altogether, future research should vary network structure experimentally or compare naturally occurring networks that differ on these dimensions to identify boundary conditions of the generalization gradient.

**Adaptiveness and consequences on well-being**. Finally, an important direction for future research is to examine how different generalization strategies vary in *adaptiveness* and their impact on well-being. Although relying on friendship ties may lead to more accurate predictions than using more superficial cues like appearance, this assumption has yet to be empirically tested. In addition, given the well-documented importance of social connections for physical and psychological well-being (Baumeister & Leary, 2017; Diener & Seligman, 2002; Holt-Lunstad et al., 2010; Shah et al., 2024; Snyder-Mackler et al., 2020), selecting the appropriate generalization strategies may not only shape who we connect with but also have downstream consequences for health and well-being. For instance, generalization could be maladaptive in cases of overgeneralization, in which people could assume novel individuals will reject them, avoid social interactions with those individuals, and fail to learn about potential partners who might accept them (Fazio et al., 2004; Watson & Nesdale, 2012). Overgeneralized feelings of rejection might in turn heighten social anxiety and depression (Kirchner et al., 2025; Rapee & Heimberg, 1997) or exacerbate loneliness, leading lonely people to drift to more peripheral positions over time (Cacioppo et al., 2009). Finally, generalization may not operate uniformly across individuals: traits such as rejection sensitivity, social anxiety, attachment style, need to belong, or strength of identification with the group could moderate how much people generalize rejection across network ties. Although our supplemental analyses of several moderators showed inconsistent results, these factors should be further tested in future research. Together, future studies should more systematically explore how different generalization strategies interact with network and individual-level factors to predict outcomes such as popularity, loneliness, and health.

## 9. Conclusions

Across three studies, we demonstrate that people use knowledge of network ties to generalize rejection and acceptance, extending inferences about who might value them from one individual to the broader group. These findings emerged in both controlled laboratory settings and real-world social networks, showing the flexibility and relevance of network-based learning. By showing how social rejection and acceptance propagate across varying degrees of separation, our work opens new avenues for exploring how learning mechanisms interact with social network dynamics to shape person perception, relationship formation, and well-being.

## Open practices

Studies 1 and 2 are pre-registered (Study 1: https://aspredicted.org/y43cj.pdf; Study 2: https://aspredicted.org/cw2ad.pdf). All data, analysis scripts, and study materials are available at https://osf.io/r52jv/?view_only=8ca54786a2b94185a41b83faf8aafae4.

## CRediT authorship contribution statement

**Yi Zhang:** Writing – review & editing, Writing – original draft, Visualization, Software, Project administration, Methodology, Investigation, Formal analysis, Data curation, Conceptualization. **Leor M.**

**Hackel:** Writing – review & editing, Validation, Supervision, Resources, Project administration, Methodology, Funding acquisition, Conceptualization.

## Declaration of generative AI and AI-assisted technologies in the writing process

During the preparation of this work the authors used ChatGPT in order to improve readability and language and to help create data analysis scripts. After using this tool, the authors reviewed and edited the content as needed and take full responsibility for the content of the publication.

## Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

## Appendix A. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.jesp.2025.104834.

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