BRIEF REPORT

Immunization Against Social Fear Learning

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Social fear learning offers an efficient way to transmit information about potential threats; little is known, however, about the learning processes that counteract the social transmission of fear. In three separate experiments, we found that safety information transmitted from another individual (i.e., demonstrator) during preexposure prevented subsequent observational fear learning (Experiments 1–3), and this effect was maintained in a new context involving direct threat confrontation (Experiment 3). This protection from observational fear learning was specific to conditions in which information about both safety and danger was transmitted from the same demonstrator (Experiments 2–3) and was unaffected by increasing the number of the safety demonstrators (Experiment 3). Collectively, these findings demonstrate that observational preexposure can limit social transmission of fear. Future research is needed to better understand the conditions under which such effects generalize across individual demonstrators.

Keywords: social learning, safety, fear conditioning, exposure, latent inhibition

Supplemental materials: http://dx.doi.org/10.1037/xge0000173.supp

Learning about emotional events by observing the actions of other individuals is ubiquitous in human culture. Such social learning has served adaptive purposes throughout human evolution and offers an efficient route to learn which stimuli are dangerous and which are safe. However, despite a growing understanding of the mechanisms underlying social transmission of fear (Olsson & Phelps, 2007), little is known about the processes involved in social transmission of safety. This is surprising given that much of the information conveyed between individuals involves informing about what is safe and harmless. For example, socially transmitted safety information is an important factor in parent modeling, during which children learn appropriate behaviors by observing the actions of their parents (Bandura, 1977), but less is known about how previously learned safety information can influence and determine subsequent social learning. Here, we address how socially transmitted safety information acquired through observation can protect against social transmission of fear in healthy adults.

Research on both human and nonhuman primates (e.g., Hygge & Öhman, 1978; Mineka, Davidson, Cook, & Keir, 1984; Olsson & Phelps, 2004) demonstrates that acquisition of fear through observation shares several features with directly acquired fear (Askew & Field, 2008; Hooker, Verosky, Miyakawa, Knight, & D'Esposito, 2008; Kelly & Forsyth, 2009; Olsson, Nearing, & Phelps, 2006; Olsson & Phelps, 2004). Thus, like directly learned fear, socially transmitted fear can be expressed without conscious awareness (Olsson & Phelps, 2004), develops rapidly after only a few exposures (Mineka et al., 1984), and contributes to the etiology of fear-related anxiety disorders (Rachman, 1977). One implication of the potency of social fear learning is that fears are readily transmitted between individuals living in close proximity (Laland, 2004), and clinical observations suggest that parental modeling contributes to the elevated rates of transmission of anxiety disorders from parents and children (Askew & Field, 2008; Craske, 2003). However, little is known about the learning processes that counteract or reduce the social transmission of fears. One possible route may be via social safety experiences. For instance, although social transmission of fear between individuals living in close proximity occurs readily, a proportion of such fears may be extinguished through observational safety learning experiences, which serve as a particularly efficient form of safety learning (Bandura, Grusec, & Menlove, 1967; Golkar, Selbing, Flygare, Öhman, & Olsson, 2013). Indeed, observational safety procedures are commonly exploited as a part of exposure treatment of fear-related anxiety disorders to optimize safety learning (Seligman & Wuyek, 2005). An alternative explanation is that social transmission of fear is counteracted by safety learning prior to observational fear learning. Indeed, prior exposure to a stimulus presented alone results in a retardation of subsequent conditioning to that stimulus, a phenomenon known as *latent inhibition* (Lubow, 1973). Although the effects of latent inhibition have long been documented using classical conditioning procedures, less is known

about the possibility to prevent the social transmission of fear through prior observational safety exposure. In an attempt to evaluate the influence of prior observational stimulus exposure, Mineka and Cook (1986) reported that rhesus monkeys receiving prior exposure to a nonfearful monkey interacting with a snake (i.e., observational preexposure) were less likely to observationally acquire fear of snakes compared to conditions in which the monkeys received the same amount of direct exposure to snakes only (i.e., direct preexposure) or watched another monkey behaving nonfearfully with a neutral object. This superior effect of prior observational exposure was referred to as "immunization." To date, no study has directly addressed the processes by which socially transferred information determines subsequent learning from another individual in humans.

To investigate the learning processes that are likely to prevent social transmission of fear, we performed the following experiments. In Experiment 1, we directly contrasted the effects of observational and direct safety exposure on subsequent observational fear learning. We hypothesized that, if prior exposure to a nonfearful demonstrator is more effective than prior stimulus exposure alone, then the group receiving observational preexposure will show less expression of conditioned fear during the observational fear learning task compared to the group that received prior stimulus exposure only. In two follow-up experiments (Experiments 2–3), we further characterized the boundary conditions of this immunization effect. More specifically, we investigated the generalizability of the effect by addressing if safety information transferred from one individual prevented fear learning from a different individual (Experiment 2) and whether the protective effects of observational preexposure were maintained in a new context (Experiment 3).

Experiment 1

Based on sample sizes in previous research on vicarious learning (Golkar et al., 2013; Olsson et al., 2006), we planned to include 40 participants in Experiment 1. Of the 44 participants who completed Experiment 1, we excluded 4 participants who failed to report the correct contingency between the conditioned stimulus (conditional stimulus [CS]) and the unconditioned stimulus (US), as assessed during a postexperimental interview, and 3 outliers because their skin conductance responses (SCRs) were above or below 3 standard deviations of the mean in the preexposure or acquisition stage. This left 37 participants (27 females; mean age = 23.4 years, SD = 5.63) in the final sample (same demonstrator: n = 20; no demonstrator: n = 17).

The experiment was divided into two stages: preexposure and acquisition (Figure 1, Table 1). During preexposure, the same-demonstrator group watched a video in which a calm male demonstrator watched nonreinforced presentations of two angry male faces (the CSs). The video for the no-demonstrator group was identical but without the demonstrator. In the acquisition video, four presentations of one of the CSs (CS+) were reinforced with a shock given to the demonstrator's wrist. The other CS (CS−) was never reinforced. During both stages, each CS was presented six times with a duration of 6 s. Between each CS trial, a black screen was presented for 12–18 s (supplementary methods).

The conditioned response (CR) was indexed as the differential SCR to the CS+ and CS− (supplementary methods). The mean CRs from Experiments 1 and 2 are displayed in Figure 2. As expected, a 2 (CS+, CS−) × 2 (same demonstrator, no demonstrator) repeated-measures analysis of variance (ANOVA) revealed no significant effects of stimuli (main effect of CS: $F(1, 35)$ = 1.83, p = .19) or differences between groups (CS × Group:
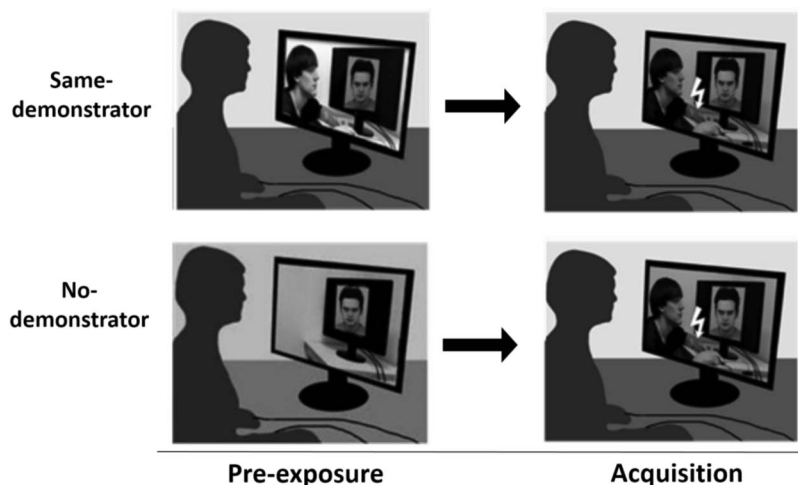
Figure 1. Design of Experiment 1. During the preexposure stage, the video for the same-demonstrator group contained a calm male demonstrator sitting in front of the screen watching unreinforced conditional stimulus (CS) presentations. Apart from the addition of the demonstrator, the video for the no-demonstrator group was identical in terms of content and timing. The observational acquisition video was identical in both groups. In the video, four of six presentations of one of the CSs (CS+) coterminated with a 100-ms shock given to the wrist of the demonstrator who twitched his arm in response to receiving the shock. The six presentations of the other CS (CS−) were never paired with a shock.

Table 1

Summary of the Experimental Manipulations Across Experiments 1–3

Experiment/ group	Preexposure demonstrator	Acquisition demonstrator	Test (only Experiment 3)
Experiment 1			
Same	A	A	
No	No	A	
Experiment 2			
Same	A	A	
Different	B	A	
Experiment 3			
Same	A	A	No
Different	B	A	No
Multiple	B + C	A	No

Note. We assessed the effect of observational preexposure in the same-demonstrator group on fear acquisition (Experiments 1–3) and test (Experiment 3) compared to a no-demonstrator group (Experiment1), a different-demonstrator group (Experiments 2–3), or a multiple-demonstrators group (Experiment 3). Same = same demonstrator; No = no demonstrator; Different = different demonstrator. A, B, and C denote the identity of the demonstrator.

$F(1, 35) = .24$, $p = .63$) during the initial preexposure stage. The predicted group differences emerged during the observational acquisition stage (Stimulus × Group: $F(1, 35) = 5.22$, $p = .028$, $\eta^2 = .12$). Planned follow-up t tests confirmed that the same-demonstrator group acquired significantly less CS+/CS− differentiation than the no-demonstrator group, $t(35) = 2.29$, $p = .028$, 95% CI [−0.37, −0.02]. Thus, only the no-demonstrator group that received CS preexposure in the absence of the demonstrator showed successful fear acquisition, as indexed by a significantly

higher response to the CS+ compared to the CS−, $t(16) = 2.75$, $p = .014$, 95% CI [0.036, 0.276], whereas this effect was not significant in the same-demonstrator group that received preexposure with the demonstrator, $t(19) = .63$, $p = .533$, 95% CI [−0.17, 0.09].

Experiment 2

Experiment 1 showed that compared to direct CS preexposure, observational CS preexposure prevented subsequent observational fear acquisition. To address the generalizability of this effect, Experiment 2 used the same procedure as in Experiment 1, with the following changes. First, we replaced the CSs (an image of a snake served as the CS+, and an image of a spider served as the CS−) and the demonstrator. Second, to address whether observational CS preexposure generalized across different individual demonstrators, we compared an observational preexposure group presented to the same male demonstrator during both preexposure and fear acquisition (as in Experiment 1) with a group that was exposed to a novel, male demonstrator during fear acquisition. Which demonstrator served during preexposure and which served during observational fear acquisition was counterbalanced across participants.

Of the 45 participants who completed the experiment, we excluded 3 outliers because their SCRs were above or below 3 standard deviations of the mean during the preexposure or acquisition stages, as well as 1 participant who erroneously reported receiving shocks during the experiment. This left 41 participants (15 female, mean age = 25.5 years, $SD = 6.72$) in the final sample (same demonstrator: $n = 21$; different demonstrator: $n = 20$).
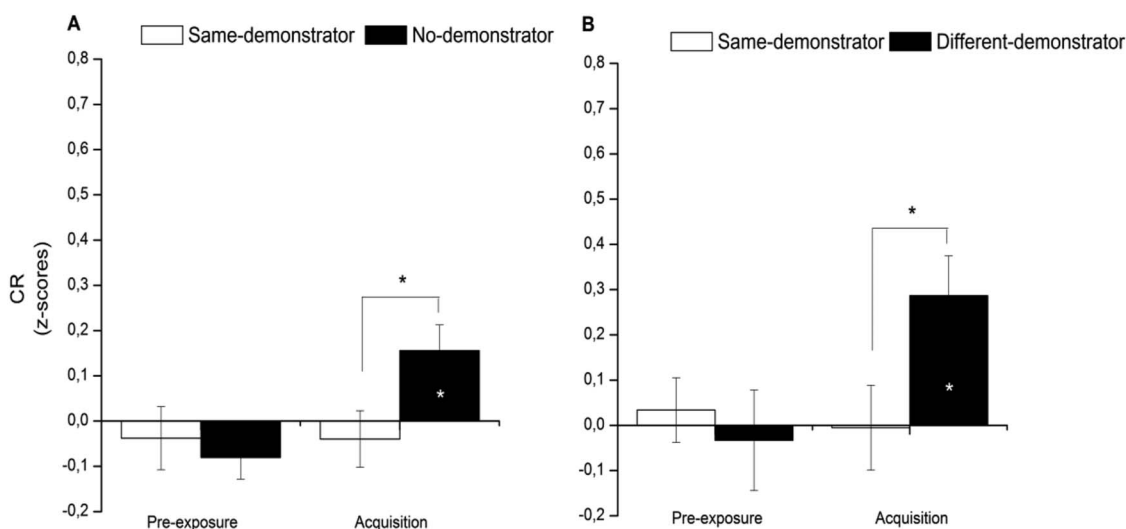

Figure 2. Observational preexposure effectively prevented fear learning. (A) In Experiment 1, observational conditional stimulus (CS) preexposure prevented subsequent observational fear acquisition in the same-demonstrator group compared to the no-demonstrator group receiving direct CS exposure only. (B) In Experiment 2, we replicated this effect in a group exposed to the same individual demonstrator during both preexposure and fear acquisition (same-demonstrator group), but the effect of observational CS preexposure did not protect against observational fear acquisition from a different individual (different-demonstrator group). Error bars indicate standard error of the mean. * Significant differences. CR = conditioned response (CS+ > CS − skin conductance response difference).

There were no significant effects of stimuli (main effect of CS: $F(1, 39) = .01$, $p = .99$) or CS × Group interaction ($F(1, 39) = .25$, $p = .62$) during the preexposure stage. Group differences emerged during the observational acquisition stage (CS × Group: $F(1, 39) = 5.17$, $p = .029$, $\eta^2 = .11$), and planned comparisons confirmed that CS+/CS– differentiation was less pronounced in the same-demonstrator group compared to the different-demonstrator group, $t(39) = 2.27$, $p = .029$, 95% CI [−0.55, −0.03]. Successful fear acquisition was only evident in the different-demonstrator group, $t(20) = 3.26$, $p = .01$, 95% CI [0.05, 0.44], and was not significant in same-demonstrator group, $t(19) = .06$, $p = .95$, 95% CI [−0.20, 0.19], replicating the immunization effect from Experiment 1.

Experiment 3

Experiment 2 demonstrated that the immunization effect was reproducible using a new set of stimuli and demonstrators but restricted to conditions in which information about safety and danger was transmitted from the same demonstrator. If the protective effect of immunization, once established, is maintained, this effect should critically be demonstrated in a new context. Therefore, the design of Experiment 3 was identical to that of Experiment 2, with the addition of a test stage during which participants were reexposed to six nonreinforced presentations of each of the two CSs in the absence of a demonstrator. Moreover, we also included a group that was exposed to multiple demonstrators during the preexposure stage. This group was included to address whether the addition of a preexposure demonstrator would enhance safety learning, similar to related work in which safety exposure involving multiple demonstrators has been shown to enhance the effects of model-based exposure treatments (Bandura & Menlove, 1968). To this end, participants in the multiple-demonstrator group were exposed to two different demonstrators during preexposure; the first demonstrator watched the first half of the unreinforced CS presentations (three presentations/CS), and the second demonstrator watched the remaining three presentations/CS. The order with which the demonstrators appeared was counterbalanced across participants, and both the multiple- and different-demonstrator groups were exposed to a novel demonstrator during the observational fear-learning stage.

Of the 70 participants who completed the experiment, we excluded 4 participants who failed to report the correct CS-US contingency and 2 participants due to technical problems. This left 64 participants (40 female, mean age = 25.2 years, $SD = 5.00$) in the final sample (same demonstrator: $n = 23$; different demonstrator: $n = 22$; multiple demonstrators: $n = 19$). A 2 × 3 repeated-measures ANOVA revealed no significant effects during the preexposure stage (main effect of CS: $F(1, 61) = .59$, $p = .45$; CS × Group: $F(2, 61) = .36$, $p = .68$). Although the group differences were not significant during the observational acquisition stage (CS × Group: $F(2, 61) = 1.90$, $p = .18$), planned comparisons revealed that fear acquisition was significant in the different-demonstrator group, $t(21) = 2.38$, $p = .03$, 95% CI [0.04, 0.60]; marginally significant in the multiple-demonstrator group, $t(18) = 1.18$, $p = .087$, 95% CI [−0.03, 0.46]; and, again replicating Experiments 1–2, not significant in same-demonstrator group, $t(22) = .87$, $p = .40$, 95% CI [−0.12, 0.30]. Importantly, we observed a significant CS × Group interaction during the direct

test stage ($F(2, 61) = 5.00$, $p = .01$, $\eta^2 = .11$). This interaction was driven by less CS+/CS– differentiation in the same-demonstrator group compared to the different-demonstrator group, $t(43) = 2.17$, $p = .036$, 95% CI [−0.857, 0.30], and the multiple demonstrator group, $t(40) = 3.22$, $p = .003$, 95% CI [−1.045, 0.239], whereas the different-demonstrator and the multiple-demonstrator groups did not differ, $t(39) = .089$, $p = .377$. Thus, immunization in the same-demonstrator group generalized to the direct test situation in which the demonstrator was absent, $t(22) = .28$, $p = .78$, 95% CI [−0.23, 0.31], whereas CS+/CS– differentiation was maintained in the different-demonstrator group, $t(21) = 3.02$, $p = .01$, 95% CI [0.15, 0.81], and multiple-demonstrator group, $t(18) = 4.45$, $p < .001$, 95% CI [0.81, 0.99] (see Figure 3).

Finally, to provide an estimate of the immunization effect obtained across Experiments 1–3, we followed a previous recommendation (e.g., Braver, Thoemmes, & Rosenthal, 2014) and conducted an internal meta-analysis (Borenstein, Hedges, Higgins, & Rothstein, 2009). We computed the effect size and its 95% confidence interval for the individual studies by taking the mean acquisition differences (CS+/CS– difference in same-demonstrator vs. no/different-demonstrator groups) and dividing them by the pooled standardized difference (Cohen's d). A positive effect size indicates less CS+/CS– differentiation in the same-demonstrator group (i.e., favors the immunization effect). The averaged standardized mean difference for the immunization effect was $d = 0.611$, 95% CI [−0.248, 0.973], $Z = 3.302$, $p = .001$ (Supplementary Figure 1). Post hoc power computations showed that the meta-analysis achieved a power of 92% to detect an effect of this size, supporting that preexposure to the same demonstrator retards observational fear learning.

Discussion

Both experimental (Golkar et al., 2013; Olsson & Phelps, 2007) and clinical research (Rachman, 1977) highlight that social leaning serves a central role in the development and treatment of fear-related anxiety disorders. What is unknown is whether there are practical ways to directly immunize against the acquisition of anxiety disorders, such as specific phobias. Here, we demonstrated that social transmission of fear could be prevented by prior social safety exposure. Participants who observed another individual (demonstrator) behaving nonfearfully in the presence of the CSs did not acquire significant levels of conditioned fear toward the CS+ during the observational conditioning stage, and this effect was maintained during direct threat confrontation in a novel test context. Importantly, observational preexposure prevented observational fear learning when information about both safety and danger was transmitted from the same individual demonstrator. The specificity of this effect suggests that the contextual similarity between the preexposure and the acquisition stage is critical to establish the immunization effect. This contextual dependency of observational preexposure is predicted from other forms of nonreinforced exposure, including latent inhibition and extinction. For example, latent inhibition is typically disrupted by a change of context between preexposure and acquisition (Gray et al., 2001; Westbrook, Jones, Bailey, & Harris, 2000), but when preexposure and acquisition occurred in the same context, latent inhibition was insensitive to contextual changes that first occurred at test (San-
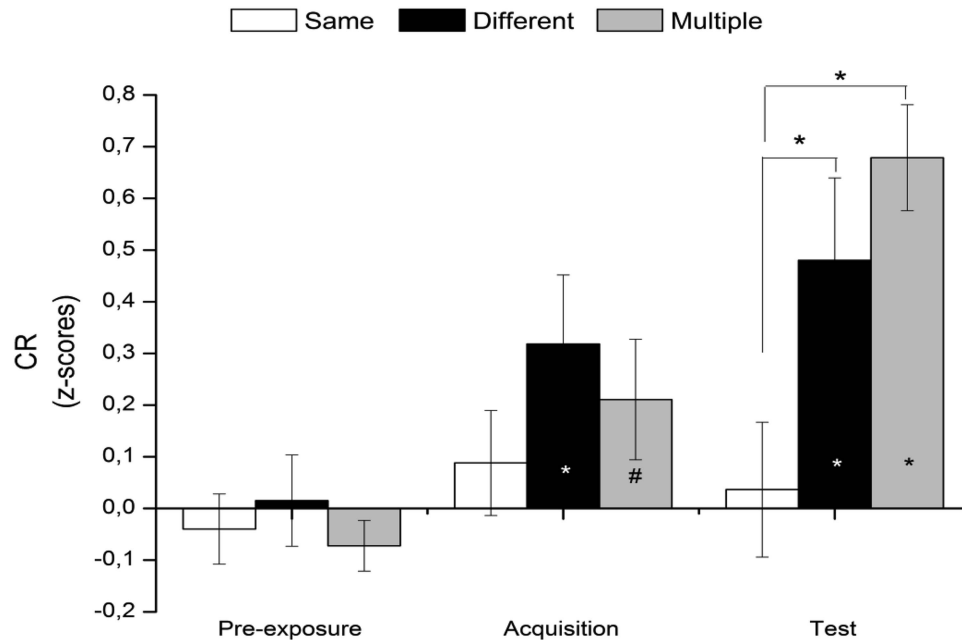
Figure 3. Observational preexposure prevented fear learning in the same-demonstrator group, and this protective effect generalized to a direct test context. Changing the identity of the demonstrator between preexposure and acquisition stages did not prevent observational fear learning in the different-demonstrator group and in the group exposed to multiple preexposure demonstrators. Error bars indicate standard error of the mean. * $p < .05$. # $p < .09$. CR = conditioned response (CS+ > CS – skin conductance response difference); Same = same demonstrator; Different = different demonstrator; Multiple = multiple demonstrators.

juan, Alonso, & Nelson, 2006; Westbrook et al., 2000). Similarly, the loss of CS responding that accompanies extinction learning is highly context specific as revealed by postextinction phenomena such as renewal, during which extinguished CRs reappear due to a change of context (Bouton, 2002). However, human extinction studies have failed to demonstrate renewal in a novel test context when the preceding fear and safety learning contexts were held constant (i.e., AAB design; Vansteenwegen et al., 2005; Vervliet, Vansteenwegen, & Eelen, 2004). Both these phenomena parallel the findings of Experiment 3, during which CRs were maintained at a low level in the same-demonstrator group despite the context shift introduced during the test stage.

Previous work suggests that increasing contextual diversity during postacquisition exposure provides a larger number of contextual cues to be associated with safety learning, thereby promoting generalization of this learning to other contexts (Gunther, Denniston, & Miller, 1998; Vansteenwegen et al., 2007; but see Bouton, García-Gutiérrez, Zilski, & Moody, 2006; Neumann, Lipp, & Cory, 2007). In humans, postacquisition safety exposure conducted with multiple demonstrators (in combination with increased diversity of the CSs) enhanced the effects of safety exposure compared to a single-demonstrator procedure (Bandura & Menlove, 1968), suggesting that safety information acquired from multiple demonstrators may similarly enhance the generalizability of preacquisition safety learning. In the present study, merely increasing the number of safety demonstrators during preexposure did not immunize against observational fear learning from a novel demonstrator, lending support to the fact that the efficacy of the immunization procedure is governed by contextual cues, including

the identity of the demonstrator and the CS. It is unclear from the present work whether adding more than one safety demonstrator could have facilitated generalization across demonstrators or if combining multiple models with a larger number of exposure trials could enhance the protective effects of observational preexposure.

An intriguing possibility is that the generalization of safety learning between different individual demonstrators depends on other factors such as familiarity and social group membership. Indeed, humans have an evolved capacity to quickly categorize other individuals into social groups to distinguish between those who belong to one's own group (in-group) and those outside of one's group (out-group; Allport, 1954; Kurzban, Tooby, & Cosmides, 2001). Recently, both observational fear and safety learning were demonstrated to be superior when learning occurred from a demonstrator belonging to a racial in-group compared to an out-group (Golkar, Castro, & Olsson, 2015). Similar effects have been reported in rodents, in which the strength of observational fear learning is enhanced by relatedness (Jeon et al., 2010; Kavaliers, Colwell, & Choleris, 2005), familiarity, and social status (Kavaliers et al., 2005). Future studies should address whether the immunization effect reflects a social effect that similarly is influenced and determined by factors such as familiarity and social group membership. For example, such effects might help to understand if safety information acquired from familiar members more easily protects against fear learning from other, unfamiliar individuals in more ecologically valid settings when information flows between related and familiar individuals, such as children, parents, and peers.

In sum, we found that social safety learning provided stronger protection against subsequent social fear learning than did directly transmitted safety information and that the protective effect of prior safety learning was maintained during direct threat confrontation in a novel context. These findings may help develop practical strategies to prevent the onset of fear-related anxiety disorders in particularly exposed and vulnerable groups of individuals. To more fully understand the processes that govern when and from whom such strategies may provide useful, future research should address whether social information provides a safety cue beyond what is provided by context only and, if so, how social factors such as familiarity and group membership influence the generalizability of socially transmitted safety information across individuals.

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The primary mission of *Psychological Bulletin* is to contribute a cohesive, authoritative, theory-based, and complete synthesis of scientific evidence in the field of psychology.

The editorial team is currently interested in the contribution of meta-analysis and systematic reviews to answering questions about the replication and reproducibility of psychological findings. This call is designed to encourage authors to consider the degree to which multi-study primary research papers, systematic replications efforts (e.g., Open Science Framework and Many Labs Replications), and meta-analysis can contribute to substantial psychological knowledge.

The editors of *Psychological Bulletin* announce interest in publishing Replication and Reproducibility: Questions Asked and Answered via Research Synthesis. Papers may be published as a special issue or as a series of special sections. Manuscripts will be evaluated and may be published as they come in but must be **submitted by June 30, 2017** for consideration for the special sections/issue.

We invite authors to submit single papers that address a clearly defined question related to replication and reproducibility of psychological findings using systematic-review or meta-analytic perspectives. Purely methodological papers, such as tests or criticisms of particular approaches, or commentaries on replication and reproducibility, are not appropriate for *Psychological Bulletin*. Primary-level research reports are also outside of the interest domain of *Psychological Bulletin*. Examples of possible manuscripts include but are not limited to meta-analyzing replication efforts, comparing evidence from different replication methods, synthesizing models of replication determinants, meta-analyzing predictors of questionable research practices and retraction, and comparing methods to gauge publication bias.

For guidance in proposing a submission, interested authors may contact the Editor, Dolores Albarracín (dalbarra@illinois.edu), or Associate Editor, Blair T. Johnson (blair.t.johnson@uconn.edu). Alternatively, authors are free to submit unsolicited manuscripts and should indicate that the submission targets this call in the cover letter.