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Episodic Simulation Reduces Intergroup Bias in Prosocial Intentions and Behavior

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People frequently feel less empathy for and offer less aid to out-groups in need relative to their in-groups. Most attempts aimed at reducing intergroup bias in helping emphasize group-focused cognitions and emotions. However, little is known about how the *sensory properties* of intergroup episodes informs intergroup decisions. Here we investigate whether episodic simulation (i.e., the ability to imagine events in a specific time and place) (a) increases participants' general willingness to help, and (b) decreases the difference in prosocial intentions and behavior toward in-group versus out-group targets. Experiment 1 revealed that imagining a helping episode significantly increased self-reported intention to help in-group and out-group targets, and eliminated the gap between groups relative to a control manipulation. Path modeling analyses indicated that the effect of episodic simulation was mediated by the vividness of the imagined episode and heightened perspective-taking for the target. Experiment 2 replicated these findings and ruled out reduced encoding of group membership as an explanation for the effect. Experiment 3 demonstrated that the effect of episodic simulation on prosocial intentions was distinct from the effects of imagining people (or contact with them). Experiments 4 and 5 replicated previous experiments with helping behaviors (i.e., writing in a letter of support to the victim of a misfortune; monetary donation to the person in need). These results shed light on a previously unexplored channel of group debasing and conflict reduction. We close by considering implications for future research at the intersection of episodic and intergroup processes.

Keywords: episodic simulation, intergroup bias, scene imagery, perspective-taking, prosocial behavior


Supplemental materials: <http://dx.doi.org/10.1037/pspi0000194.supp>

Conflict reduction interventions have become a top priority for policymakers and researchers alike (Cohen & Insko, 2008; Paluck & Green, 2009). While researchers have made major breakthroughs elucidating the precise psychological processes and biases that give rise to and bolster conflict, attempts to harness this

knowledge to reduce bias and conflict have revealed significant challenges to generating successful, sustained intervention effects (e.g., Brewer & Miller, 1984; Bruneau & Saxe, 2012; Hewstone & Brown, 1986; Lai et al., 2014, 2016; Paluck, Green, & Green, 2018; Pettigrew & Tropp, 2006). Indeed, there are instances when such well-intentioned attempts may even backfire, exacerbating intergroup bias, hostility, and inequality (e.g., Dixon, Durrheim, & Tredoux, 2007; Dovidio, Gaertner, & Saguy, 2009; Vorauer & Sasaki, 2009).

So much psychological research in the domain of intergroup bias and conflict interventions emphasizes group related cognitions and emotions directed at out-group members. For example, interventions include highlighting superordinate goals and identities, (e.g., Dovidio & Gaertner, 2000; Hornsey & Hogg, 2000), minimizing attributions of asymmetric motivations (e.g., Kennedy & Pronin, 2008; Waytz, Young, & Ginges, 2014), priming group malleability (e.g., Goldenberg et al., 2017; Halperin, Russell, Trzesniewski, Gross, & Dweck, 2011), training regulation of negative emotions (Halperin, Porat, Tamir, & Gross, 2013), and fostering perspective-taking and empathy across groups (Bruneau & Saxe, 2012; Cikara, Bruneau, & Saxe, 2011). Yet, intergroup interaction consists of more than representing and reacting to people in a vacuum; it

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Experimental materials, data, analysis code, and supplemental materials for all experiments reported in this article can be downloaded from the Open Science Framework at https://osf.io/66gf4/?view_only=d0189dae889d4661894adc4ddc77b87.

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involves the experience of events unfolding through one's mind in a specific time and space (i.e., an episode), within which the relevant parties are embedded.

Does the vividness with which we simulate the episodic details of intergroup interaction affect intergroup behavior? Specifically, does a more detailed imagined scene increase our willingness to engage in prosocial behavior toward out-group members? By imagined scene we mean a space with objects and people integrated into a vivid, coherent sensory experience that unfolds over time as a specific event or episode (Mullally, Intraub, & Maguire, 2012; Maguire & Mullally, 2013; see also Addis & Schacter, 2012; Andrews-Hanna, Reidler, Sepulcre, Poulin, & Buckner, 2010; Rubin & Umanath, 2015). To address these questions we investigated the role of episodic simulation of social interaction with in-group and out-group targets in attenuating intergroup bias and promoting intergroup helping. We aim to leverage episodic simulation, the ability to imagine events in a specific time and place, to increase participants' general willingness to help in-group and out-group targets, and examine whether it would decrease the intergroup bias (i.e., the difference between in-group and out-group targets) in prosocial intentions and behavior.

Episodic Simulation: Past Findings and Relevance to Prosocial Decisions

Episodic simulation is a form of future thinking or prospection that draws on many of the same cognitive processes as episodic memory (cf., Atance & O'Neill, 2001; Schacter, Addis, & Buckner, 2008; Szpunar, Spreng, & Schacter, 2014; see Irish & Piquet, 2013 for relation to semantic memory). Relevant to the current investigation, mounting evidence indicates it can be used to increase prosocial behavior; after imagining a specific event that involves helping a person in need, participants are more willing to help in that situation (Gaesser, DiBiase, & Kensinger, 2017; Gaesser, Dodds, & Schacter, 2017; Gaesser, Horn, & Young, 2015; Gaesser & Schacter, 2014).

These effects are driven by multiple mechanisms. For example, manipulating (i.e., increasing) the familiarity of the location in which the helping episode takes place increased the vividness of scene imagery and subsequently increased willingness to help (Gaesser, Keeler, & Young, 2018). Notably, scene imagery continued to predict willingness to help when individual differences in empathic and prosocial traits were accounted for (Gaesser et al., 2018) and even when imagined episodes elicited negative emotion (Gaesser et al., 2017).

That said, scene imagery does not seem to fully account for the effect of episodic simulation on willingness to help. Episodic simulation also interacts with perspective-taking (Gaesser et al., 2017, 2018), which is the cognitive ability to simulate the mental states (i.e., thoughts and feelings) of others (Davis, 1983; Galinsky, Maddux, Gilin, & White, 2008). Whereas perspective-taking ratings did not consistently account for the effect of scene imagery on willingness to help, they independently contributed to the broader effect of episodic simulation on willingness to help (relative to control conditions; Gaesser et al., 2018). These findings suggest that episodic simulation may recruit and augment considerations of the thoughts and feelings of the person in need when that person is embedded in a specific imagined episode (Gaesser et al., 2017; Gaesser et al., 2018).

Can episodic simulation stimulate prosocial intention and behavior even when the target is an out-group member? People are generally less likely to help an out-group member than an in-group member (Hornstein, 1978; Levine, Prosser, Evans, & Reicher, 2005; Turner, Hogg, Oakes, Reicher, & Wetherell, 1987), but in the studies on episodic simulation the group membership of the person in need was unspecified, remaining ambiguous. Thus, it is an open question whether episodic simulation can be used to facilitate prosocial interactions, eliminating the bias in in-group versus out-group helping, or whether it boosts helping for both while maintaining the relative in-group helping advantage. While we know little about how episodic simulation interacts with intergroup processes, several existing intergroup intervention strategies provide reason to suspect that episodic simulation may be effective in mitigating intergroup bias in prosocial behavior with each intergroup intervention strategy predicting distinct component features of an imagined episode would account for this effect. More important, these intergroup strategies are person-centric; they emphasize that how a person is represented and characterized can affect a prosocial response. Episodic simulation, in contrast, involves binding together details of the location, people, and objects, to form an event that is specific in time and place, not only representing a person but the broader scene in which a person is embedded.

Imagined Contact, Identifiability, and Dilution: Distinct From Episodic Simulation

Imagined Contact

Most pertinent to the current inquiry is research on imagined intergroup contact, an indirect contact strategy for fostering positive intergroup relations. One of the main benefits of imagined contact is that it overcomes limitations of geographic and group segregation where direct contact with out-group members would be unlikely or impossible (Crisp & Turner, 2009). Imagining positive social interactions with out-group members can have broad positive effects on prejudice, explicit and implicit attitudes, intentions, and behavior toward an out-group (see Miles & Crisp, 2014 for meta-analysis). Given that imagining a helping episode involves generating an interaction with another person, perhaps merely imagining any positive interaction with a person in need contributes to the prosocial effect of episodic simulation on intergroup interaction.

While the mechanisms underlying the effects of imagined contact are the focus of ongoing inquiries, there has been a particular emphasis on how imagining positive interactions increases the availability of semantic behavioral scripts that promote how to interact with out-group members (Miles & Crisp, 2014). In contrast, extant research on the effects of episodic simulation on prosocial decisions are associated with related—but distinct—cognitive (i.e., scene imagery and perspective-taking) and neural mechanisms (i.e., medial temporal lobe subsystem, theory of mind network). How an episode is represented and subjectively experienced may also affect how cognitively available it is, but that is distinct from an effect of behavioral scripts that involve semantic knowledge to interact with out-group members in different situations. Although episodic simulation has not been previously considered in the imagined contact literature, there is some evidence

that hints at a role for episodic simulation in an effect of imagined contact. In a study that used an elaborated version of the imagined contact task—thinking specifically about where and when the imagined interaction took place—was found to further enhance the effect of imagined contact on intentions to interact with the out-group (Husnu & Crisp, 2010). Intriguingly, the qualities of spatial and temporal specificity manipulated in this study are defining features of episodic simulation and episodic memory (Schacter et al., 2008; Tulving, 1983, 2002).

Furthermore, in contrast to the present research, which focuses on increasing prosocial intentions and behavior toward individual in-group and out-group targets, imagined intergroup research has focused on changing explicit and implicit attitudes and behavior only toward out-group targets. To our knowledge, no experiment in the imagined intergroup contact lineage has directly contrasted the effect of imagining interactions with out-group relative to in-group members (see Miles & Crisp, 2014 for meta-analysis). Thus, it is unclear whether imagining helping episodes with in-group members would increase willingness to help in-group members to a similar extent as out-group members, and in doing so preserve the gap between in-group and out-group helping.

Identifiability

Another intervention that may contribute to the prosocial effect of episodic simulation on intergroup interaction is manifested in the *identifiable victim effect*. The identifiable victim effect refers to the tendency of individuals to offer greater aid when a single, identifiable person (“victim”) is portrayed as in need, as compared with a large, vaguely defined group with the same need (Genevsky, Västfjäll, Slovic, & Knutson, 2013; Jenni & Loewenstein, 1997; Kogut & Ritov, 2005; Slovic, 2007; Small, Loewenstein, & Slovic, 2007). Even weak forms of identifiability—specifying which victim will receive help without providing any personalizing information—make people more willing to contribute to charity and compensate others who lost money (Small & Loewenstein, 2003). Linking these results back to effects of episodic simulation, perhaps imagining a specific helping episode facilitates a willingness to help in part by increasing the identifiability of the person in need. However, predicting how this pattern would be affected by a target’s group membership is less clear; identifiability, in the absence of episodic simulation, provides mixed evidence in an intergroup context. In some cases identifiability increases generosity only for in-group members (Kogut & Ritov, 2007), while in others it increases generosity toward an out-group member (Kogut & Ritov, 2011).

Dilution

The *dilution effect* is another related phenomenon that may contribute to the effect of episodic simulation on intergroup behaviors. Stereotypes about out-group members can be reduced by providing individual-level information (Brewer, 1988; Fiske, Lin, & Neuberg, 1999), including nondiagnostic information (e.g., that a person likes her coffee with sugar; Kimmelmeier, 2004; Nisbett, Zukier, & Lemley, 1981). Recent work on the dilution effect in intergroup contexts provides insight into one underlying cognitive mechanism: interference with encoding of targets’ group membership (Bruneau, Cikara, & Saxe, 2015). Specifically, reading indi-

viduating stories about misfortunes of in-group and out-group targets (as opposed to sparse event descriptions: e.g., “Sam missed the flight to his friend’s wedding”) eroded participants’ recognition memory for target’s group membership (but not memory for the events) in a surprise memory task at the end of the experiment. This reduced encoding of targets’ group membership mediated the effect of individualizing descriptions on intergroup empathy bias, such that individuals who had poorer encoding of targets’ group membership exhibited less of an intergroup empathy gap.

Thus, one possibility is that imagining a specific helping episode may facilitate a willingness to help by shifting attention to details about the event (e.g., who does what, where; McLelland, Devitt, Schacter, & Addis, 2015; Szpunar, Addis, McLelland, & Schacter, 2013) and disrupting encoding of group membership. When we attend less to, or forget to what group people belong, we are unable to use that information as an input to decisions about how to allocate our time and resources helping others.

Overview of Current Research and Hypotheses

Taken together, previous research on episodic simulation as a predictor of helping and intergroup interventions leveraging imagined contact, identifiability, and dilution, suggest that episodic simulation could reduce intergroup bias in prosocial intent and behavior—albeit for different reasons. In a series of five experiments we investigate whether episodic simulation can be used to (a) increase participants’ general willingness to help and (b) reduce the difference in prosocial intention and behavior for in-group relative to out-group targets. Furthermore, we test the independent mediating effects of the sensory and mental representations of the individual person in the scenario and sensory representations of the imagined episode (i.e., scene) in which the person is embedded. In Experiment 1 we predict that imagining a helping episode will significantly increase self-reported intention to help in-group and out-group targets (i.e., Democrats and Republicans) and test whether it reduces the gap between groups relative to a control manipulation. Following previous findings, we predict that the positive effect of episodic simulation will be independently mediated by the vividness of the scene imagery and perspective-taking for the target. Experiment 2 replicates this design and further tests whether episodic simulation specifically interferes with encoding individual targets’ group membership. In Experiment 3 we tested whether alternative theoretical accounts from the intergroup literature (i.e., imagined contact, identifiable victim effect) could explain the effect of episodic simulation. The intergroup accounts predict different component features of an imagined episode would effectively mitigate intergroup bias in willingness to help. Critically, these component features are related to—but diverge from—those previously shown to support a prosocial effect of episodic simulation (i.e., scene imagery and theory of mind) when group membership was unspecified, providing a compelling test-case for the unique and shared explanatory power of different theoretical accounts. Finally, Experiments 4 and 5 replicate and extend our previous experiments to test whether episodic simulation can be used to facilitate different forms of helping behavior (in the form of time and money, respectively). Experimental materials, data, analysis code, and online supplemental materials for all experi-

ments reported in this article can be downloaded from the Open Science Framework (OSF) at <https://osf.io/66gf4/>.

Experiment 1: The Effects of Episodic Simulation on Willingness to Help In-Group and Out-Group Members

In Experiment 1, we tested whether episodic simulation would increase participants' willingness to help in-group and out-group members. Drawing on previous episodic simulation manipulations (Gaesser et al., 2015; 2018; Gaesser & Schacter, 2014), we examined the impact of an Episodic Helping task (experimental condition) relative to a No Helping task (control condition, see below for details) on willingness to help, centering on people in need from two real-world, competitive groups—Democrats and Republicans. We hypothesized that consistent with previous findings on intergroup interactions (Hornstein, 1978; Levine et al., 2005; Turner et al., 1987), people would be more willing to help in-group than out-group targets in the absence of episodic simulation instruction.

Because we wanted to be able to determine the direction of the intergroup bias in helping (i.e., are people particularly helpful toward identified in-group members, or just less helpful toward those labeled as out-group members), we included events featuring unaffiliated individuals. We predicted that people would be as willing to help unaffiliated targets as in-group targets because past work has shown that in-group and unaffiliated targets tend to receive similar empathic responses (Cikara, Bruneau, Van Bavel, & Saxe, 2014).

Most important, we hypothesized that by instructing people to vividly imagine themselves helping a person in need, we could reduce or potentially eliminate the gap in willingness to help in-group and out-group targets. We also investigated three potential mechanisms that might account for the relationship between episodic simulation and willingness to help in an intergroup context. The first two were informed by previous work on episodic simulation and helping: scene imagery (i.e., how vividly the scene is represented) and perspective-taking (i.e., the extent to which participants adopt the mental states of the person in need). Informed by studies on identifiability and dilution, we also investigated the role of person imagery (i.e., how vividly participants represented the person embedded in the helping episode).

Method

Participants. We recruited 240 participants via Amazon's Mechanical Turk (MTurk) to complete an experiment titled "Social Decision Making." All participants were aged 18 years or older and provided informed consent to participate in the experiment, which was approved by the Harvard University's Institutional Review Board (Social Decision-Making, 14–3157). The experiment lasted approximately 20 min and participants were paid \$2 for their time.

Because this was the first experiment in this line of research, we did not know the predicted effect size. This precluded us from conducting an a priori power analysis. We initially recruited 120 pilot participants (note that this is a fully within-subjects design) and analyzed the data. The preliminary results revealed the predicted interaction effect, but it was not significant so we ran 120 more participants. The second sampling cohort wave indepen-

dently replicated the results of the first sampling cohort. There was no significant interaction by sampling cohort (see results below) so we collapsed across both samples to increase the experiment's power to detect the predicted Group \times Task interaction.

We excluded 62 participants for one of the following reasons: (a) they were neither Democrat nor Republican ($n = 25$); (b) they failed to pass manipulation checks, answering one of three team-manipulation check questions at the end of the study incorrectly ($n = 19$): "Which team are you on?"; "What other team is playing today?" (Cikara et al., 2014); and "What is the relationship between these teams?"; and/or (c) they failed to comply with the main task instructions on at least one of the six trials ($n = 20$, independent coder interrater reliability = .84, using the same criteria as related work using similar conditions; Gaesser et al., 2017; Gaesser et al., 2015; Gaesser & Schacter, 2014). Please see supplemental material on OSF for a more thorough description of exclusion/inclusion criteria related to task comprehension and compliance. Our final sample included 174 participants ($M_{\text{age}} = 32.52$ years, $SD = 9.91$, age range = 19–77, 54% female; 74% Democrat).

Procedure. This study followed a 3 target group (in-group vs. out-group vs. unaffiliated) \times 2 task (Episodic Helping vs. No Helping) within-subjects factorial design. After providing consent, participants were asked to report their political affiliation as "Democratic Party," "Republican Party," or "Other." The Other option included a textbox where participants could type their affiliation. We then informed participants that they were participating in an ongoing problem-solving tournament that involved two groups, the Democrats and the Republicans, and assigned them to a group that matched their political affiliation. Those who identified themselves as neither Democrats nor Republicans were randomly assigned to the Democrats or Republicans, but were ultimately excluded from the analysis.

To make the problem-solving challenge competitive, we told participants that a competition was taking place between groups and that the winning group would be awarded a bonus of \$1 per group member. Participants answered questions about their identification with each political party, respectively, on unmarked slider scales ranging from 0 (*strongly disagree*) to 100 (*strongly agree*): "I [value/like/feel connected to] the [Democratic party/Republican party]." Responses for all three measures were averaged for each group to provide in-group and out-group identification measures.

We then provided participants with the following cover-story:

Scientific evidence suggests that the more people know about other players' personal experiences, the better people perform in these particular problem-solving challenges. We're going to give you the opportunity to get to know the other players, by letting you read some of their recent experiences, especially in situations where they might be in need of help.

We explained the inclusion of unaffiliated individuals as follows:

You will know whether it's a Democrat or Republican group member's story by the background logo. Some participants do not fit the profile of either a Democrat or Republican; nevertheless we have included their experiences in the study so they have the opportunity to earn money for participating (adapted from Cikara et al., 2014).

We explained that on each trial, participants would either engage in an “Imagine Helping” (i.e., the Episodic Helping) task or a “Revise Writing” (i.e., the No Helping control) task. In the Episodic Helping task, participants were supposed to imagine a positive interaction specific in time and place in which they helped the person in need, generate as much detail as possible when writing the description of what they imagined. In the No Helping task, we told participants to identify word choices and writing style that would improve the professionalism of the story. Subjects were provided an example scenario “Steven has fallen off his Harley and he is laying on the side of the road.” They were then shown examples of what one might imagine for each task:

Episodic Helping,

I imagined myself driving down the road and seeing a biker crash. It was sunny outside and mildly warm. I pulled over my car a few yards in front of the biker and got out to make sure he was ok. His leg seemed to hurt a lot and he could not walk. I called an ambulance and waited with him until it arrived.

No Helping,

you may point out how changing a particular term (e.g., motorcycle instead of Harley) or the structure of the piece (e.g., incorrect grammar) would make it more professional. Another example is that you might rewrite the sentence in your own words and explain how you have made the sentence more professional (e.g., by adding description and detail, you may improve the story’s clarity).

Comparing these tasks allowed us to assess whether imagining an episode of helping facilitates prosocial intentions beyond a baseline reaction to learning about another person’s plight and thinking about the scenario more generally (Gaesser & Schacter, 2014). Please see OSF link for complete materials and instructions.

Participants read six scenarios depicting people in need (taken from Gaesser et al., 2015; Gaesser & Schacter, 2014; see also Rameson, Morelli, & Lieberman, 2012 for similar scenarios used to elicit responses that predict helping behavior outside the lab). Each scenario portrayed an everyday event that involved a person in need of help (e.g., “Zach is locked out of his apartment”). All targets were male to control for target-gender effects. The assignment of scenarios to target names, target groups, and tasks were counterbalanced across participants. Finally, the six scenarios were presented to each participant in a randomized order to control for order effects. We indicated whether the scenario was happening to a Democrat or Republican group member by including a background logo (blue donkey for Democrats and red elephant for Republicans) and a statement naming the group in capital letters (i.e., “Jared is on the DEMOCRATS”) above the scenario. Scenarios about “unaffiliated” targets appeared without the logo and statement. After being presented with each scenario, participants either imagined a helping episode (Episodic Helping task) or they thought about how they would improve the writing of the scenario (No Helping task). For both tasks, participants typed descriptions of their responses in two to three sentences.

Measures. Each of the written prompts was followed on the next page by a series of self-report ratings assessing their willingness to help, 1 (*not at all willing to help*) to 7 (*very willing to help*) their representation of the imagined episodes (adapted from Gaesser & Schacter, 2014), their representation of the person in need, and their engagement of perspective-taking.

To assess *scene imagery*, participants were asked to rate their imagined episodes for scene coherence, “The imagined scene of helping in your mind was . . .” 1 (*not at all coherent or clear*) to 7 (*coherent and clear*), and scene vividness, “Did you experience the event of helping the person in your mind?” on a scale of 1 (*not at all*) to 7 (*vividly, as if currently there*). A reliability analysis showed that the two measures of scene imagery (i.e., scene coherence, scene vividness) were highly correlated ($r = .92$, controlling for random effect of participant); thus, these items were averaged to form a scene imagery index consistent with previous work (Gaesser et al., 2017; Gaesser et al., 2018). To assess how the person in need was represented, participants reported their subjective *person imagery*, “Did you visualize the person in your mind?” on a scale from 1 (*not at all*) to 7 (*vividly, as if currently there*). Finally, participants also rated the extent to which they adopted the mental states of the person in need (*perspective-taking*), “Did you consider the person’s thoughts and feelings?” on a scale from 1 (*not at all*) to 7 (*strongly considered*) and generated an affective forecast, “How would helping in this situation make you feel?” on a scale of 1 (*very negative*) to 7 (*very positive*). The affective forecasting measure was exploratory so we do not discuss it further.

As a manipulation check we asked participants on each trial, “Did you consider improving the writing of the story?” on a scale of 1 (*not at all*) to 7 (*strongly considered*). More important, whereas previous work using similar ratings (e.g., Gaesser et al., 2018; Gaesser & Schacter, 2014) only included ratings that were intended to be task-specific (e.g., collecting scene imagery ratings only in the Episodic Helping condition), here we collected *all* ratings across both tasks; thereby, allowing us to assess the extent to which participants spontaneously generated a helping scene in the control condition or focused on improving the story of need in the imagine condition.

After completing six trials of the main tasks and ratings, participants reported their gender, age, and ethnicity. Following that, we informed participants that they were not actually participating in a problem-solving challenge and provided them with a full debrief. We paid all participants a \$1 bonus on top of the base pay.

Analysis. Multilevel models: We analyzed the data using multilevel linear mixed-effects models, implemented with the ‘lmer’ function from the package ‘lmerTest’ (Kuznetsova, Brockhoff, & Bojesen Christensen, 2014) in R to account for the fact that all responses were nested within participants. Failing to account for dependency among trials can contribute to underestimated *SEs* and overestimated predictor significance (Cohen, Cohen, West, & Aiken, 2003).

We mean-centered all predictors before fitting the model and implemented the unweighted effect coding to center the categorical variables (West, Aiken, & Krull, 1996). Because there were three target groups, we had two effect-coded group predictors, treating in-group as the reference group (this applies to all of the following analyses in Experiment 1): one predictor compared the difference in willingness to help for in-group relative to out-group targets (in-group = 1/2, out-group = -1/2, unaffiliated = 0), and the other compared the same difference for in-group relative to unaffiliated target (in-group = 1/2, out-group = 0, unaffiliated = -1/2).

Because the interaction term in the original model comprised a categorical variable with more than two levels, we tested the overall interaction of Task \times Target Group via model comparison.

We began with the full model, which included the main effects of task, the main effects of target group, and the interaction terms. To remove parameters that failed to account for a significant proportion of variance in our main dependent variable, willingness to help, we compared the full model with two reduced models using the ‘anova’ function in R: one without the main effects of target group, and the other without the interaction terms. A series of likelihood ratio tests and model diagnostics revealed that the model without the interaction terms provided the best fit for the data. The final model included scenario and participant as random effects, and the effect-coded repeated-measures factors—target group and task—as fixed effects. We estimated the model using the restricted-estimate maximum-likelihood (REML) approach and Satterthwaite approximation for calculating p values.

We also evaluated the amount of variance explained (R^2) by the final model using the ‘r.squaredGLMM’ function from the ‘MuMIn’ package in R (Barton & Barton, 2018). However, most definitions of R^2 for mixed-effects have theoretical (e.g., decreased or negative R^2 values in larger models) and/or technical (e.g., implementation) problems; to address these issues, we calculated both marginal and conditional R^2 , which are considered less susceptible to the problems that afflict alternative measures of R^2 (Nakagawa & Schielzeth, 2013). The marginal R^2 is only concerned with variance explained by fixed variables while the conditional R^2 is concerned with variance explained by both fixed and random variables.

Accounting for two waves of data collection: When we included sampling cohort (first = 0, second = 1) as another fixed effect in the model, there was no significant main effect and/or interaction by sample cohort (all $ps > .101$). Therefore, we collapsed across the two samples for the main analysis. To account for the increased Type I error that would have been introduced in the process, we reported both raw and adjusted p values using sequential analysis, implemented with the graphical user interface in the package ‘GroupSeq’ 1.3.5 (Lakens, 2014).

Mediation analyses: To investigate to what degree scene imagery, person imagery, and perspective-taking mediated the within-subjects relationship between task (No Helping vs. Episodic Helping) and willingness to help, we conducted multilevel mediation analyses using Rockwood and Haye’s (2017) MLmed SPSS macro (see Zhang, Zyphur, & Preacher, 2009 and Preacher, Zyphur, & Zhang, 2010 for detailed statistical analyses). MLmed accounts for the multilevel nature of the data by centering Level 1 predictor variables, creating novel variables that contains their group means, and estimating all of the parameters in the model simultaneously (Rockwood, 2017; see detailed statistical method in Bauer, Preacher, & Gil, 2006). We estimated the indirect effect by a Monte-Carlo simulation generating 95% confidence intervals (CIs) using 10,000 resamples. (The analyses indicate a significant statistical effect at $\alpha < .05$ when the confidence interval does not include the value zero.)

We entered the task (effect-coded: No Helping = $-.5$, Episodic Helping = $.5$) as an independent variable, willingness to help as dependent variables, and scene imagery, perspective-taking, and person imagery as mediators to construct the present mediational model. We omitted the between-subjects effect of task from the model because every participant completed both tasks. Because there was no between-subjects variability in task, there was no between-subjects indirect effect in the model.

Beginning with the simplest model, we conducted likelihood ratio tests until we found the model that yielded the best fit for the

data. In the end, we estimated all nonredundant parameters for the final model, including random effects, fixed effects, random intercepts (all intercepts are specified as random), and random slopes from task to mediators, which were specified to vary randomly and covary (all other slopes remained fixed). The final model also included the between-subjects effect of scene imagery and perspective-taking on willingness to help, but omitted the effect of person imagery. The number of estimated parameters includes 13 fixed effects, four Level 1 variances, and 14 Level 2 variances for a total of 31 parameters. The deviance of this model was 14,069.59.

Although the MLmed macro does not provide the total effect in the output, we separately computed the total effects of task on willingness to help using the same framework as the macro to maximize consistency. To calculate the total effect of task on willingness to help, we separately constructed another MLmed model, that included the task as a predictor, willingness to help as an ostensible mediator (that was the actual dependent variable), and a filler variable as an ostensible response. The direct effect from task to willingness to help in this model was statistically equivalent for the total effect (Rockwood & Hayes, 2017) and, thus, we included the output in the corresponding results and figure as the total effect in the main multilevel mediation model (this applies to all of the following mediation analyses).

Results

In-group and out-group identification scores. We created composite in-group and out-group identification scores by averaging the liking/valuing/feeling connected items for each political party (in-group items $\alpha = .94$; out-group items $\alpha = .89$). Consistent with previous research, participants reported that they were more highly identified with their in-group ($M = 76.50$, $SD = 18.23$) than the out-group ($M = 20.35$, $SD = 20.07$, $t(173) = 24.12$, $p < .001$, $d = 1.83$).

Manipulation check. The No Helping task ($M = 2.18$, $SD = 1.18$) elicited significantly higher ratings of thoughts about revising the story compared with the Episodic Helping task ($M = -2.18$, $SD = 1.69$, $b = -4.38$, $t(869.9) = -51.59$, raw $p < .001$, adjusted $p < .001$, marginal $R^2 = .69$, conditional $R^2 = .73$).

Willingness to help. The final multilevel model predicting willingness to help revealed the predicted main effect of task. Episodic Helping led to greater willingness to help ($M = .97$, $SD = 1.36$) than No Helping ($M = -.97$, $SD = 1.98$, $b = 1.95$, $t(866.6) = 23.00$, raw $p < .001$, adjusted $p < .001$, marginal $R^2 = .25$, conditional $R^2 = .52$). We also observed the predicted main effects of target group. As expected, willingness to help was marginally greater for in-group targets ($M = .09$, $SD = 1.93$) than out-group targets ($M = -.18$, $SD = 2.02$, $b = .13$, $t(865.4) = 2.60$, raw $p = .009$, adjusted $p = .145$); however, in-group was no different from the unaffiliated targets ($M = .09$, $SD = 1.91$, $b = .001$, $t(865.2) = .02$, raw $p = .984$, adjusted $p = .982$, see Figure 1).

Although likelihood ratio tests and model diagnostics revealed that the model with the interaction terms did not necessarily improve fit to the data, we conducted planned contrasts comparing in-group versus out-group to test our a priori prediction (Tybout et al., 2001). We created an orthogonalized contrast matrix (see online supplemental materials on OSF) including the two contrasts of interest (in-group vs. out-group in Episodic Helping; in-group

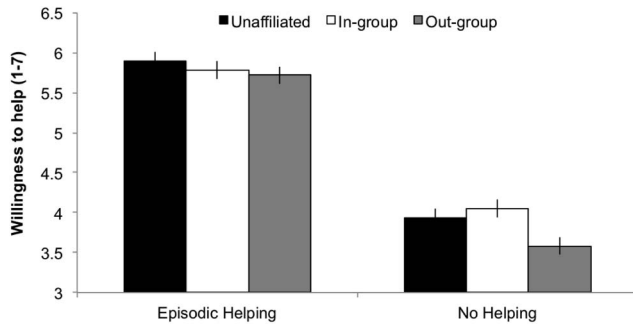


Figure 1. Mean value for willingness to help (1 = not at all to 7 = very willing) in Experiment 1. Error bars denote $\pm SEM$.

vs. out-group in No Helping) and three filler contrasts. As predicted, the in-group versus out-group difference was significant in the No Helping task ($b = .23$, $t(860.2) = 3.07$, raw $p = .002$, adjusted $p = .036$), but eliminated in the Episodic Helping task ($b = .04$, $t(860.4) = .60$, raw $p = .546$, adjusted $p = .971$).

Representation and perspective-taking. See Table 1 in for a summary of the descriptive statistics for these measures. The multilevel models (conducted separately for each outcome) revealed that the Episodic Helping task effectively increased scene imagery ($b = 3.94$, $t(870.2) = 51.05$, raw $p < .001$, adjusted $p < .001$, marginal $R^2 = .66$, conditional $R^2 = .74$), person imagery ($b = 2.47$, $t(868.9) = 28.09$, raw $p < .001$, adjusted $p < .001$, marginal $R^2 = .35$, conditional $R^2 = .55$), and perspective-taking ($b = 2.70$, $t(868.4) = 29.56$, raw $p < .001$, adjusted $p < .001$, marginal $R^2 = .37$, conditional $R^2 = .57$) relative to No Helping control task. Compared with the main effect of task, the models showed a smaller, but significant main effect of in-group relative to out-group on person imagery ($b = .14$, $t(868.8) = 2.67$, raw $p = .008$, adjusted $p = .008$), which was greater for the in-group targets than out-group targets. No other main effects were significant.

Mediators driving willingness to help. As shown in Figure 2, the within-indirect effects of task on willingness to help via

scene imagery ($b = 1.30$, $SE = .20$, 95% CI [.90, 1.69], $p < .001$), person imagery ($b = .34$, $SE = .11$, 95% CI [.13, .55], $p = .002$), and perspective-taking ($b = .37$, $SE = .11$, 95% CI [.15, .60], $p = .001$) were significantly positive, replicating the previous mediational results (when group membership of the target was unspecified; Gaesser et al., 2018). Together, the inclusion of three mediators reduced the total positive effect of task on willingness to help ($b = 1.94$, $SE = .09$, $p < .001$) to a nonsignificant residual effect ($b = -.07$, $SE = .20$, $p = .725$) at the within-subjects level of analysis, indicating the full mediation.

The covariances between the random indirect effects of task on willingness to help via three mediators were positively correlated ($r_s = .74$). These results suggest that participants with a higher than average indirect effect of task on willingness to help through scene imagery tended to also have a higher than average indirect effect through person imagery as well as perspective-taking, consistent with previous work (Gaesser et al., 2018).

The MLmed tested for differences between specific indirect effects at the within-subjects level. The model revealed that the indirect effect through scene imagery was significantly stronger than the indirect effect through perspective-taking (difference = $-.93$, 95% CI [-1.42 , $-.41$]) and the indirect effect through person imagery (difference = $-.96$, 95% CI [-1.44 , $-.48$]). The indirect effects through person imagery and perspective-taking were not significantly different from one another (difference = $-.04$, 95% CI [$-.40$, $.33$]).

Discussion

Consistent with previous work on episodic simulation (Gaesser et al., 2015, 2018; Gaesser & Schacter, 2014), participants were more willing to help others in need after imagining a helping episode (the Episodic Helping task) compared with thinking about how to revise the story (the No Helping task). Second, Experiment 1 confirmed our hypotheses that people are on average more willing to help an in-group target relative to an out-group target, while they are equally willing to help in-group and unaffiliated targets. Third, we hypothesized that the intergroup gap in willingness to help might be diminished in the

Table 1

Descriptive Statistics for Representational and Perspective-Taking Items by Task, Target Group, and Task \times Target Group in Experiment 1

Condition	Scene imagery		Revise writing		Person imagery		Perspective-taking	
	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>
Task								
Episodic Helping (EH)	1.97	1.06	-2.18	1.69	1.24	1.37	1.35	1.51
No Helping (NH)	-1.97	1.69	2.18	1.18	-1.24	2.01	-1.35	2.03
Target group								
In-group	.04	2.41	-.02	2.62	.10	2.09	.09	2.23
Out-group	-.03	2.42	.01	2.62	-.19	2.16	-.13	2.24
Unaffiliated	-.01	2.44	.01	2.63	.09	2.10	.04	2.25
Task \times Target Group								
EH \times In-Group	2.00	1.08	-2.19	1.66	1.32	1.30	1.38	1.56
EH \times Out-Group	1.93	1.12	-2.17	1.73	1.08	1.51	1.23	1.57
EH \times Unaffiliated	1.98	.99	-2.19	1.69	1.31	1.28	1.44	1.36
NH \times In-Group	-1.91	-1.91	2.15	1.28	-1.13	2.01	-1.20	2.04
NH \times Out-Group	-2.00	-2.00	2.19	1.13	-1.46	1.95	-1.48	1.98
NH \times Unaffiliated	-2.00	-2.00	2.21	1.13	-1.12	2.06	-1.36	2.06

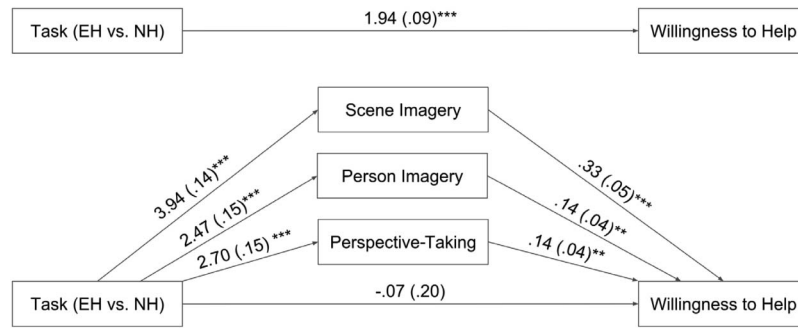


Figure 2. Multilevel mediation model on the relationship between task (Episodic Helping: EH vs. No Helping: NH) and willingness to help mediated by scene imagery, person imagery, and perspective-taking for the within-subjects level of analysis in Experiment 1. The model presents unstandardized regression coefficients and SEs in parentheses. ** $p < .01$. *** $p < .001$.

Episodic Helping task relative to the No Helping task. Consistent with this hypothesis, the episodic simulation eliminated the difference in willingness to help for in-group versus out-group targets.

To better understand the how episodic simulation exerts its effects, we conducted a multilevel mediation analysis accounting for within-subjects variability. Scene imagery, person imagery, and perspective-taking independently drove the effect of episodic simulation on willingness to help at the within-subjects level. Additionally, the indirect effect through scene imagery was significantly stronger than the indirect effects of perspective-taking and person imagery. These results suggest that scene imagery may be the most robust component driving the effect of episodic simulation on helping.

Experiment 2: Replicating Experiment 1 and Testing the Role of Group Membership Encoding

Overall, the results from Experiment 1 indicated that episodic simulation eliminated the effect of target group membership on participants' willingness to help. One possible mechanism through which episodic simulation could minimize the gap in willingness to help in-group versus out-group is by "individuating" targets. In other words, imagining a helping episode may facilitate encoding event details, undermining encoding of targets' group membership.

Previous work on intergroup empathy (feeling more empathy for in-group than out-group members in situations of need) has shown that narrative descriptions of a target's experiences reduces empathy bias by decreasing the salience of that target's group membership and by focusing attention on the target as an individual human being (Bruneau et al., 2015). Given these findings, we hypothesized that episodic simulation of helping a person in need could shift attention and encoding away from the target's group membership and, in return, reduce the impact of group membership on their willingness to help. To test this hypothesis, participants in Experiment 2 performed one of two surprise forced-choice memory tests at the end of the experiment, recalling either each target's group membership or the specific event in which each target was involved (Taylor & Falcone, 1982; Taylor, Fiske, Etcoff, & Ruderman, 1978). We

predicted that episodic simulation would degrade "group memory," while leaving "event memory" intact or improved.

Method

Participants. We conducted an a priori power analysis of the effect size ($d = .25$) corresponding to the central contrast of interest in Experiment 1 (i.e., the difference in willingness to help for in-group vs. out-group targets in the No Helping task) using G*Power (Faul, Erdfelder, Lang, & Buchner, 2007) with $\alpha = .05$. The analysis indicated that running 289 participants in each task would allow for the detection of the predicted effect (power $> .95$). Because we excluded 28% of the collected participants from the previous study, we collected 800 participants on MTurk to retain approximately 578 participants after exclusions.

Participant recruitment and inclusion/exclusion criteria followed the same procedure in Experiment 1. 205 participants were excluded for the following reasons: (a) they were neither a Democrat nor a Republican ($n = 104$); (b) they answered one of three team-manipulation check questions at the end of the study incorrectly ($n = 70$); and/or (c) they failed to comply with the main task instructions on at least two of the 10 trials ($n = 31$, independent coder interrater reliability = .91, see online supplemental material on OSF for inclusion/exclusion criteria). Our final sample included 581 participants ($M_{\text{age}} = 33.95$ years, $SD = 10.35$, age range = 18–64; 44% female; 69% Democrat).

Procedure. Participants were randomly assigned to one of four conditions in a 2 task (between-subjects: Episodic Helping vs. No Helping) \times 2 memory test (between-subjects: Group Memory vs. Event Memory) \times 2 target group (within-subjects: in-group vs. out-group) mixed factorial design.

The experimental design was identical to Experiment 1, with a few modifications: (a) we removed unaffiliated targets because they were not significantly different from the in-group on willingness to help in Experiment 1; (b) we reduced the number of self-report ratings to the three factors that showed evidence of full or partial mediation (i.e., scene imagery, person imagery, and perspective-taking) in Experiment 1; (c) we reverse-coded person imagery with 1 (*vividly, as if currently there*) and 7 (*not at all*) to

check for response bias;¹ and (d) we increased the number of scenarios depicting people in need to 10 scenarios (Gaesser et al., 2015; Gaesser & Schacter, 2014; see also Rameson et al., 2012; see experiment materials on OSF).

At the end of Experiment 2, we presented participants with either a surprise Group Memory or Event Memory test, adapted from a previous study (Bruneau et al., 2015). In each Group Memory question, participants saw each of the 10 event/target pairings, and reported to which group the target belonged. Specifically, we presented “Democrat” and “Republican” as the two alternative choices. In the Event Memory question, participants saw each of the 10 group/target pairings and reported with which event the target was associated. For each item we presented two choices: one was the correct answer and the other was the correct answer for a different target. Note that this was a fully between-subjects factor; we assigned each participant to either the Group Memory or the Event Memory test.

Analysis. The main multilevel analysis was identical to that in Experiment 1. We examined the fit of multilevel models by conducting a series of likelihood ratio tests. The final model included scenario and participant as random effects, target group as a random slope, and the effect-coded repeated-measures factors—target group (in-group = 1, out-group = -1) and task (Episodic Helping = 1, No Helping = -1)—as fixed effects.

Results

In-group and out-group identification scores. As in Experiment 1, we created composite in-group and out-group identification scores by averaging the liking/valuing/feeling connected items for each political party (In-group $\alpha = .94$; Out-group $\alpha = .88$). Consistent with Experiment 1, participants reported that they identified more with their in-group ($M = 73.09$, $SD = 22.68$) than out-group members ($M = 21.01$, $SD = 18.90$; $t(580) = 39.69$, $p < .001$, $d = 1.64$).

Willingness to help. The final multilevel model predicting willingness to help revealed the main effects of task and target group, replicating the results of Experiment 1. The Episodic Helping task led to a greater willingness to help ($M = .50$, $SD = 1.44$) than the No Helping task ($M = -.48$, $SD = 1.77$, $b = .49$, $t(579.0) = 10.06$, $p < .001$, marginal $R^2 = .09$, conditional $R^2 = .58$), and willingness to help was significantly greater for in-group ($M = .05$, $SD = 1.67$) than out-group targets ($M = -.05$, $SD = 1.71$, $b = .11$, $t(686.5) = 6.00$, $p < .001$, see Figure 3). The

predicted Task \times Target Group interaction was not significant ($b = -.03$, $t(579.1) = -1.47$, $p = .141$).

Although the omnibus Task \times Target Group interaction effect was not significant, we turned to our planned comparison analyses between in/out-group and tasks to test whether the predicted effect replicated the results of Experiment 1. We created an orthogonalized contrast matrix (see online supplemental materials on OSF) including the two contrasts of interest (in-group vs. out-group in Episodic Helping; in-group vs. out-group in No Helping) and one filler contrast, which was a main effect of task (Episodic Helping vs. No Helping). In contrast with the results of Experiment 1, both in-group versus out-group differences in the No Helping task ($b = .26$, $t(632.3) = 5.40$, $p < .001$) and in the Episodic Helping task ($b = .16$, $t(631.6) = 3.29$, $p = .001$) were significant.

Representational and perspective-taking items. See Table 2 for descriptive statistics. Replicating the results of Experiment 1, the multilevel models revealed a main effect of task on scene imagery ($b = .96$, $t(579.0) = 17.91$, $p < .001$, marginal $R^2 = .27$, conditional $R^2 = .75$), person imagery ($b = .28$, $t(579.0) = 4.27$, $p < .001$, marginal $R^2 = .02$, conditional $R^2 = .64$), and perspective-taking ($b = .83$, $t(579.0) = 14.47$, $p < .001$, marginal $R^2 = .20$, conditional $R^2 = .72$), all of which were higher in the Episodic Helping relative to No Helping task. The models also revealed a main effect of target group on scene imagery ($b = .09$, $t(700.9) = 6.38$, $p < .001$) and perspective-taking ($b = .09$, $t(700.0) = 5.88$, $p < .001$), all of which were greater for the in-group than the out-group targets. No other main effect or interaction was significant.

Group and event memory by task. We analyzed participants' recall using a 2 task (between-subjects: Episodic Helping vs. No Helping) \times 2 memory test (between-subjects: Group Memory vs. Event Memory) analysis of variance (ANOVA), implemented with the ‘ezANOVA’ function from the ‘ez’ package in R (Lawrence & Lawrence, 2016). The results indicated a main effect of memory test ($F(1, 582) = 227.06$, $p < .001$, $\eta^2 = .28$), such that participants performed better on the event memory test ($M = 8.59/10$, $SD = 1.40$) relative to the group memory test ($M = 6.55/10$, $SD = 1.87$), and a slightly smaller main effect of task ($F(1, 582) = 6.09$, $p = .014$, $\eta^2 = .01$), which indicated participants' had better memory in the Episodic Helping task ($M = 7.75/10$, $SD = 1.87$) relative to the No Helping task ($M = 7.39/10$, $SD = 1.99$). These main effects were qualified by an Task \times Memory Test interaction ($F(1, 582) = 7.83$, $p = .005$, $\eta^2 = .01$).

To unpack the source of the interaction, we ran follow-up simple effects analyses within each memory condition. In the event memory test, there was no significant difference in memory for targets' event by task (Episodic Helping: $M = 8.57/10$, $SD = 1.40$, No Helping: $M = 8.61$, $SD = 1.41$; $t(291.67) = .26$, $p = .792$, $d = .03$). Contrary to our prediction, however, in the group memory test participants who engaged in the Episodic Helping task showed *better* memory for targets' group membership ($M = 6.91/10$, $SD = 1.93$) than participants who completed the No Helping task ($M = 6.20/10$, $SD = 1.75$; $t(286.36) = -3.31$, $p = .001$, $d = .39$).

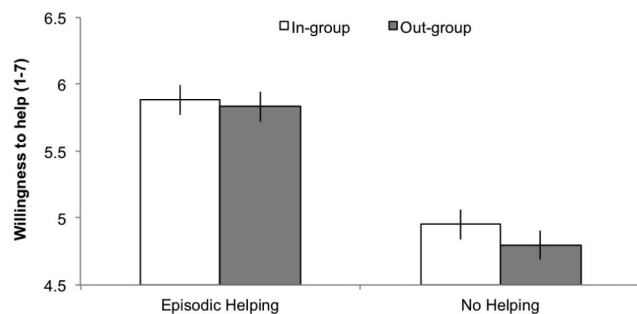


Figure 3. Mean value for willingness to help (1 = not at all to 7 = very willing) in Experiment 2. Error bars denote $\pm SEM$.

¹ We reverse-coded person imagery in this study to look for response bias.

Table 2

Descriptive Statistics for Representational and Perspective-Taking Items by Task, Target Group, and Task × Target Group in Experiment 2

Condition	Scene imagery		Person imagery		Perspective-taking	
	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>
Task						
Episodic Helping (EH)	.97	1.06	.29	2.07	.84	1.29
No Helping (NH)	-.94	1.94	-.28	1.86	-.81	1.95
Target group						
In-group	.07	1.82	.02	2.01	.07	1.83
Out-group	-.07	1.86	-.02	1.97	-.07	1.86
Task × Target Group						
EH × In-Group	1.03	1.05	.29	2.10	.91	1.23
EH × Out-Group	.92	1.08	.29	2.04	.77	1.34
NH × In-Group	-.85	1.93	-.24	1.87	-.75	1.95
NH × Out-Group	-1.04	1.94	-.31	1.84	-.88	1.94

Mediators driving willingness to help. To investigate the between-subjects relationship between task (No Helping vs. Episodic Helping) and willingness to help, we conducted the mediation analyses using the same mediators and procedure in Experiment 1. We omitted the within-subjects effect of task from the mediational model because each participant only completed one task and, therefore, no within-subjects indirect effect was estimated. All nonredundant parameters for the model included random effects, fixed effects, random intercepts (all intercepts are specified as random), and random slopes from each mediator to task (all other slopes remained fixed). Both random intercept and random slopes were specified to vary randomly and covary. The model also included the within-subjects effect of each mediator on willingness to help. The number of estimated parameters includes 14 fixed effects, four Level 1 variances, and 13 Level 2 variances for a total of 31 parameters. In the present mediational model, we entered the task (effect-coded: No Helping = $-.5$, Episodic Helping = $.5$) as independent variables, willingness to help as dependent variables, and scene imagery, perspective-taking and person imagery as potential mediators. The deviance of this model was 73,281.49.

As shown in Figure 4, between-indirect effects of task on willingness to help via scene imagery ($b = .75$, $SE = .10$, 95% CI

[.56, .96], $p < .001$) and perspective-taking ($b = .34$, $SE = .08$, 95% CI [.19, .50], $p < .001$) were significantly positive; however, the indirect coefficient for person imagery was not significant ($b < .001$, $SE = .01$, 95% CI [-.03, .03], $p = .979$). Together, the inclusion of three mediators reduced the total positive effect of task on willingness to help ($b = .98$, $SE = .10$, $p < .001$) to a nonsignificant residual effect ($b = -.11$, $SE = .09$, $p = .230$) at the between-subjects level of analysis, indicating full mediation.

The MLmed tested for differences between specific indirect effects at the between-subjects level: the indirect effect through scene imagery was significantly stronger than the indirect effects through perspective-taking (difference = $-.41$, 95% CI [-.73, -.10]) and person imagery (difference = $-.75$, 95% CI [-.96, -.56]). Moreover, the indirect effect through perspective-taking was significantly stronger than the indirect effect through person imagery (difference = $-.34$, 95% CI [-.51, -.18]).

Discussion

Experiment 2 replicated the main effects and directionally replicated the planned comparison results between in/out-group and tasks from Experiment 1: episodic simulation of helping events increased one's willingness to help as well as reduced the difference in willingness to help for in-group versus out-group targets (though the difference between groups remained significant in the helping task). In Experiment 2, we also hypothesized that, based on the dilution effect, imagining a helping episode may simultaneously degrade "group memory," while leaving "event memory" intact. Contrary to our hypothesis, the results revealed that episodic simulation actually improved memory for targets' group membership indicating that dilution could not account for our pattern of results in Experiments 1 and 2. The multilevel mediation analyses also replicated the findings of Experiment 1: (a) the relationship between tasks and willingness to help was fully mediated by scene imagery and perspective-taking (but not by person imagery), and (b) scene imagery was the most consistent and robust attribute driving the effect of episodic simulation, followed in order by perspective-taking and person imagery.

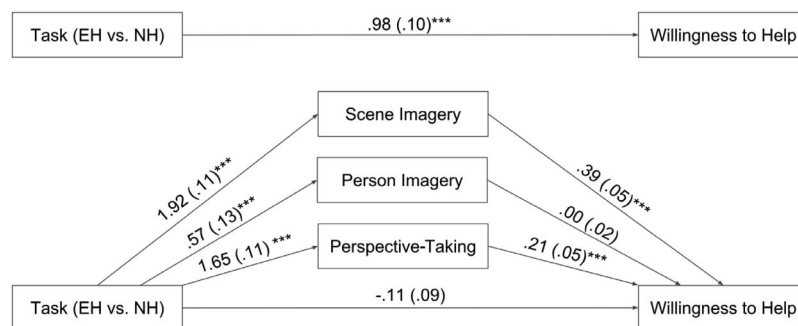


Figure 4. Multilevel mediation model on the relationship between task (Episodic Helping: EH vs. No Helping: NH) and willingness to help mediated by scene imagery and perspective-taking for the between-subjects level of analysis in Experiment 2 (person imagery was not a significant mediator). The model presents unstandardized regression coefficients and *SEs* in parentheses. *** $p < .001$.

Experiment 3: Testing the Role of Imagined Contact and Person

In Experiment 3, we sought to replicate the main results of Experiments 1 and 2, while comparing episodic simulation against competing theoretical accounts. To explore the relative efficacy of episodic simulation, we created two new tasks—Imagined Contact and Imagined Person—that were adapted from the imagined contact (Crisp & Turner, 2009; Husnu & Crisp, 2010) and the identifiable victim literatures (Genevsky et al., 2013; Jenni & Loewenstein, 1997; Kogut & Ritov, 2005; Slovic, 2007; Small et al., 2007), respectively. These tasks allowed us to isolate component elements of an imagined helping episode that alternative theoretical accounts predict could drive the prosocial effect of episodic simulation.

Research on imagined contact has shown that imagining positive social interactions generally reduces prejudice and attitudes toward out-group member (Miles & Crisp, 2014). Research on identifiable victim effect has shown that a single, identifiable person in need elicits more aid compared with a large, vaguely defined group with the same need (Slovic, 2007; Small et al., 2007). Imagining an episode that involves helping a person in need by necessity includes generating an interaction with another person (imagined contact) as well as an individual in need (identifiable victim) embedded within the episode. Thus, either one or both of these components could be contributing to the prosocial effect of episodic simulation.

The imagine contact and identifiable victim effect are person-centric, emphasizing the role of how a person is represented and characterized can affect a prosocial response. In contrast, episodic simulation involves not just representing a person but the broader scene a person is embedded in, binding together details of the location, people, and objects, to form an event that is specific in time and place. Moreover, the cognitive (i.e., scene imagery and perspective-taking) and neural (i.e., medial temporal lobe subsystem, theory of mind network) mechanisms shown to underlie the prosocial effect of episodic simulation have not been previously considered in research on imagined intergroup contact and identifiable victim effect.

We hypothesized that, to the extent that episodic helping is unique, the episodic helping manipulation would lead to greater willingness to help compared with alternative manipulations—an effect that would be supported by increases in scene imagery and perspective-taking in particular. Thus, we added two conditions to the original experimental paradigm: the Imagine Contact condition (i.e., imagine a positive interaction with the person in need) and the Person Only condition (e.g., imagine and think about only the person sans episode), and conducted analyses to examine whether the mechanisms that underlie the effect of episodic simulation on willingness to help were distinct from the effects of imagining people (or contact with them) on prosocial intention. Moreover, by comparing the Imagine Contact and Person Only conditions with the baseline No Helping control condition we can quantify any prosocial gains attributable to these component features of imagined events.

Method

Participants. Based on the same sample collection rationale in Experiment 2, we recruited participants on MTurk until we had approximately 400 participants per condition (before exclusions). The participants' recruitment and inclusion/exclusion criteria followed the same procedures in Experiments 1 and 2. We collected a total of 1,714 participants, a subset of which we excluded for the following reasons: (a) they were neither a Democrat nor a Republican ($n = 166$); (b) they failed to comply with the main task instructions at least two of the 10 trials ($n = 147$, independent coder interrater reliability $>.82$, see online supplemental materials on OSF for inclusion/exclusion criteria); (c) they answered one of the team-manipulation check questions at the end of the study incorrectly ($n = 284$); (d) they participated in the study more than once ($n = 1$); and/or (e) the survey failed to record the outcome measure ($n = 1$). Our final sample included 1,115 participants ($M_{\text{age}} = 36.01$ years, $SD = 11.41$, age range = 18–82; 63% female; 64% Democrat).

Procedure. Participants were randomly assigned to one of four conditions in a 4 task (between-subjects: Episodic Helping vs. No Helping vs. Imagined Contact vs. Imagined Person) \times 2 target group (within-subjects: in-group vs. out-group) mixed factorial design. The experimental design was identical to that in Experiment 2, with a few modifications: (a) we removed the memory tests; (b) we made all participants write at least 30 words during the tasks to increase consistency across tasks and participants; and (c) we presented a combination of a scenario of need and the task at the same time to help reduce participant confusion.

In the Imagined Contact task, participants were instructed to imagine themselves positively interacting with the person in the scenario for the first time in an unrelated event that occurred before the described misfortune, imagining what the person looks like, the conversation that follows and the ways that they categorize and related to the person. In the Imagined Person task, participants were instructed to imagine the person in the scenario as if they were looking at them in a photo with a blank background, imagining only the person from the scenario and not including any additional objects, associated background contexts, or events.

Subjects were provided an example scenario "Steven has fallen off his Harley and he is laying on the side of the road." They were then shown examples of what one might imagine for each task:

Imagine Contact,

I imagined meeting Steven at a park. I pulled over my car a few yards in front of Steven parking his bike. He is wearing a gray jacket, a helmet, and ripped jeans with boots. His face looks older, and his hair is long and white. I saw that he had an Eagles sticker on his bike, so we started talking about football. We agreed we looked forward to the next season starting.

Imagine Person,

"I imagined a biker with nothing in the background, just Steven floating in space. He is wearing a gray jacket, a helmet, and ripped jeans with boots. His face looks older, and his hair is long and white." The Episodic Helping and No Helping tasks are identical to those in Experiments 1 and 2.

Analysis. The main analysis was identical to that in Experiments 1 and 2. We ensured that the letters participants wrote to the

target were similar in terms of valence across conditions by conducting content analysis of texts in the letter writing task (see online supplemental materials on OSF for full analysis and results). To take into account the unequal number of participants in conditions, we calculated the weighted effect w for each condition by dividing the number of observations in an experimental condition (e.g., Episodic Helping) by the number of observations in a control condition (e.g., No Helping; see detailed explanations about weighted effect coding in te Grotenhuis et al., 2017; West et al., 1996).

We had three weighted effect-coded task predictors, which treated the No Helping condition as the reference task (this applies to all of the following analyses in Experiment 3): the first predictor compared the difference in willingness to help in the Episodic Helping relative to the No Helping task (Episodic Helping = $1/2$, No Helping = $-w/2$, Imagined Contact = 0, Imagined Person = 0), the second predictor compared the difference for the Imagined Contact relative to the No Helping task (Episodic Helping = 0, No Helping = $-w/2$, Imagined Contact = $1/2$, Imagined Person = 0), and the last predictor compared the difference for the Imagined Person relative to the No Helping task (Episodic Helping = 0, No Helping = $-w/2$, Imagined Contact = 0, Imagined Person = $1/2$). (See below for simple effects comparisons across active tasks.)

Because the original model included an interaction term with a categorical variable, which had more than two levels, we assessed the overall Task \times Target interaction. We constructed the full model by including the main effects of task, the main effects of target group, and the interaction terms. Following the same multimodel comparison and diagnosis method in Experiment 1, we contrasted the full model with two reduced models—one without the main effects of task and the other without the interaction terms—and confirmed that the model without the interaction terms provided the best fit for the data. The final model included scenario and participant as random effects, target group as a random slope, and the effect-coded repeated-measures factors—target group and task—as fixed effects.

Results

In-group and out-group identification scores. As in Experiments 1 and 2, we created composite in-group and out-group identification scores by averaging the liking/valuing/feeling connected items for each political party (In-group $\alpha = .94$; Out-group $\alpha = .88$). Consistent with Experiments 1 and 2, participants reported that they identified more with their in-group ($M = 76.53$, $SD = 18.37$) than out-group members ($M = 22.17$, $SD = 20.00$; $t(1,114) = 63.73$, $p < .001$, $d = 1.91$).

Willingness to help. The final model predicting willingness to help revealed main effects of task and target group, replicating the results of Experiments 1 and 2. The Episodic Helping task led to a greater willingness to help ($M = .67$, $SD = 1.40$) than the No Helping task ($M = -.32$, $SD = 1.78$, $b = .48$, $t(1,115) = 10.77$, $p < .001$, marginal $R^2 = .07$, conditional $R^2 = .45$), and willingness to help was significantly greater for in-group ($M = .18$, $SD = 1.69$) than out-group targets ($M = -.18$, $SD = 1.85$, $b = .36$, $t(1,116) = 11.26$, $p < .001$, see Figure 5). The Imagined Contact ($M = .09$, $SD = 1.83$) also led to significantly greater willingness to help than the No Helping task ($b = .17$, $t(1,114) = 3.92$, $p < .001$) while the Imagined Person task ($M = -.41$, $SD = 1.89$) was

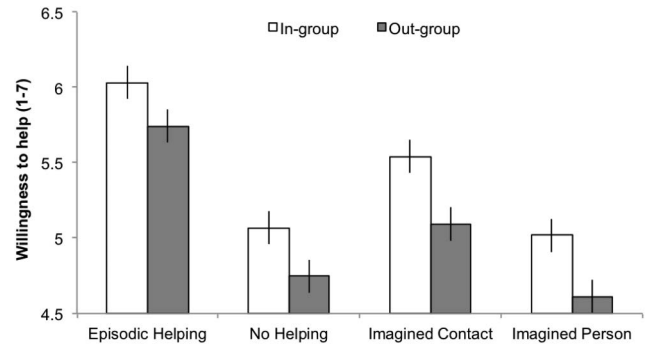


Figure 5. Mean value for willingness to help (1 = not at all to 7 = very willing) in Experiment 3. Error bars denote \pm SEM.

not significantly different from the No Helping task ($b = -.06$, $t(1,113) = -1.33$, $p = .184$).

Although the model did not include the interaction terms, we turned to our planned comparison analyses between in/out-group and each task predictor because we had specific predictions about Task \times Target Group interaction. As in Experiment 2, all of the in-group versus out-group differences across tasks were significant: No Helping task ($b = .32$, $t(207.21) = 5.27$, $p < .001$), Imagined Contact task ($b = .45$, $t(327.92) = 6.36$, $p < .001$), Imagined Person task ($b = .41$, $t(247.03) = 6.36$, $p < .001$), and Episodic Helping task ($b = .29$, $t(237.75) = 4.55$, $p < .001$). Note, however, that the difference is smallest in the Episodic Helping condition.

Because our multilevel model revealed that both the Episodic Helping and the Imagined Contact tasks led to greater willingness to help than the No Helping task, we included the Episodic Helping versus the Imagined Contact contrast in our contrast matrix to test our hypothesis that episodic simulation leads to greater willingness to help than alternative accounts. As predicted, willingness to help was greater in the Episodic Helping than the Imagined Contact task ($b = .57$, $t(1,111) = 5.70$, $p < .001$).

Representational and perspective-taking items. See Table 3 for descriptive statistics. Replicating Experiments 1 and 2, the multilevel models comparing the Episodic Simulation task against No Helping revealed a main effect of task on scene imagery ($b = .80$, $t(1,115) = 14.53$, $p < .001$, marginal $R^2 = .16$, conditional $R^2 = .63$), person imagery ($b = .57$, $t(1,115) = 12.37$, $p < .001$, marginal $R^2 = .15$, conditional $R^2 = .73$), and perspective-taking ($b = .69$, $t(1,115) = 14.51$, $p < .001$, marginal $R^2 = .12$, conditional $R^2 = .65$), all of which were higher in the Episodic Helping relative to No Helping task. The models also revealed a main effect of target group on scene imagery ($b = .18$, $t(1,116) = 7.48$, $p < .001$), person imagery ($b = .07$, $t(1,115) = 4.03$, $p < .001$), and perspective-taking ($b = .16$, $t(1,115) = 6.94$, $p < .001$), all of which were greater for the in-group than the out-group targets.

The Imagined Contact task led to greater scene imagery ($b = .15$, $t(1,115) = 2.77$, $p = .006$), person imagery ($b = .49$, $t(1,115) = 10.91$, $p < .001$), and perspective-taking ($b = .36$, $t(1,115) = 7.71$, $p < .001$) compared with the No Helping task. The Imagined Person elicited significantly greater person imagery ($b = .67$, $t(1,115) = 14.61$, $p < .001$) and perspective-taking ($b =$

Table 3

Descriptive Statistics for Representational and Perspective-Taking Items by Task, Target Group, and Task × Target Group in Experiment 3

Condition	Scene imagery		Person imagery		Perspective-taking	
	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>
Task						
Episodic Helping (EH)	1.23	1.11	.29	1.31	.61	1.19
No Helping (NH)	-.39	1.83	-.93	1.77	-.84	1.88
Imagined Contact (IC)	.01	2.00	.30	1.26	.09	1.53
Imagined Person (IP)	-.83	2.15	.50	1.05	.24	1.46
Target group						
In-group	.09	1.94	.03	1.51	.08	1.61
Out-group	-.09	2.00	-.03	1.51	-.08	1.67
Task × Target Group						
EH × In-Group	1.29	1.06	.33	1.32	.67	1.16
EH × Out-Group	1.17	1.17	.24	1.31	.54	1.21
NH × In-Group	-.32	1.84	-.89	1.78	-.78	1.89
NH × Out-Group	-.47	1.83	-.96	1.77	-.90	1.86
IC × In-Group	.11	1.96	.33	1.26	.21	1.46
IC × Out-Group	-.09	2.04	.27	1.26	-.03	1.59
IP × In-Group	-.69	2.13	.52	1.04	.32	1.39
IP × Out-Group	-.97	2.17	.48	1.05	.17	1.53

.50, $t(1,115) = 10.55$, $p < .001$), however, scene imagery was significantly weaker in the Imagine Person than the No Helping task ($b = -.24$, $t(1,115) = -4.31$, $p < .001$).

Mediators driving willingness to help. To assess the between-subjects relationship between task and willingness to help mediated by the three potential mediators, we conducted mediation analyses following the same procedure in Experiments 1 and 2. We removed the within-subjects effect of task from the model and, thus, no within-subjects indirect effect was estimated.

Because MLmed is not compatible with processing a multicategorical independent variable (such as two or more experimental conditions relative to a control condition), we included the three weighted effect-coded task predictors by treating one predictor as the independent variable and the other two as Level 2 covariates (see Hayes & Preacher, 2014 for the detailed method).

In the present Episodic Helping model, we entered the Episodic Helping versus No Helping task (weighted effect-coded: Episodic Helping = 1/2, No Helping = -w/2, Imagined Contact = 0, Imagined Person = 0) as the independent variable, willingness to help as the dependent variable, scene imagery, perspective-taking, and person imagery as potential mediators, and other two task predictors (Imagined Contact vs. No Helping and Imagined Person vs. No Helping) as covariates. The number and contents of other nonredundant parameters for the model were identical to those in Experiment 2. The deviance of this model was 210080.2.

We further examined the same between-subjects relationship between task and willingness to help by comparing the Imagined Contact and Imagined Person tasks against the No Helping task, respectively. To do this, we refitted the model iterating through each task predictor as an independent variable and treating the remaining two as covariates.

As shown in Figure 6, between-indirect effects of task on willingness to help via scene imagery ($b = .93$, $SE = .08$, 95% CI [.77, .1.10], $p < .001$) and perspective-taking ($b = .53$, $SE = .07$, 95% CI [.40, .69], $p < .001$) were significantly positive; however, the indirect coefficient for person imagery was not significant ($b = -.04$, $SE = .04$, 95% CI [-.11, -.04], $p = .32$). Together, the inclusion of three mediators reduced the total positive effect of task on willingness to help ($b = 1.56$, $SE = .14$, $p < .001$) to a nonsignificant residual effect ($b = .12$, $SE = .12$, $p = .32$) at the between-subjects level of analysis, indicating full mediation. Removing the two alternative tasks from these analyses as covariates did not alter the results (see supplemental materials on OSF for the Episodic Helping model without covariates).

The MLmed tested for differences between specific indirect effects at the between-subjects level. The model revealed that the indirect effect through scene imagery was significantly larger than the indirect effects through perspective-taking (difference = -.40, 95% CI [-.61, -.17]) and person imagery (difference = -.97, 95% CI [-1.16, -.79]). Moreover, the indirect effect through perspective-taking was significantly larger than the indirect effect through person imagery (difference = -.57, 95% CI [-.78, -.39]).

The Imagined Contact model revealed similar patterns of mediation effects to those in the Episodic Helping model, except that the indirect

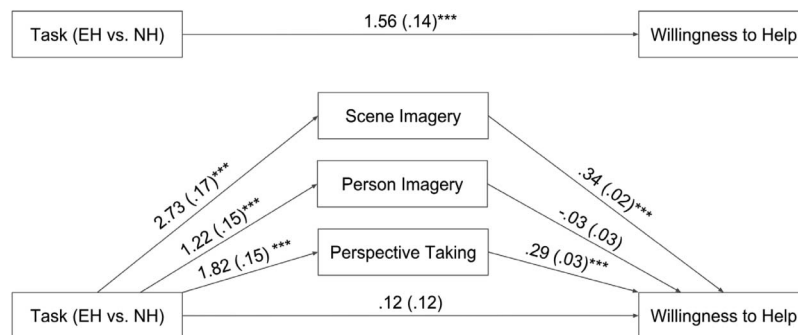


Figure 6. Multilevel mediation model on the relationship between task (Episodic Helping: EH vs. No Helping: NH) and willingness to help mediated by scene imagery and perspective-taking for the between-subjects level of analysis in Experiment 3 (person imagery was not a significant mediator). Two alternative tasks were included in these analyses as covariates. The model presents unstandardized regression coefficients and SEs in parentheses. *** $p < .001$.

effect via scene imagery ($b = .10$, $SE = .07$, 95% CI $[-.05, .24]$, $p = .19$) was no longer significant, leaving perspective-taking ($b = .23$, $SE = .06$, 95% CI $[.12, .35]$, $p < .001$) as the only mediator between task and willingness to help. In the Imagined Person model, the meditational pattern for perspective-taking was similar to that in the Episodic Helping model, but the indirect effect via scene imagery ($b = -.43$, $SE = .06$, 95% CI $[-.56, -.31]$, $p < .001$) was significantly negative (see online supplemental materials on OSF for full results and each model without covariates).

Discussion

Experiment 3 replicated the main effects and directionally replicated the planned comparison results between in/out-group and tasks from Experiments 1 and 2: episodic simulation of helping events increased one's willingness to help as well as reduced the difference in willingness to help for in-group versus out-group targets (though this difference remained significant in the Episodic Helping task). The multilevel mediation analyses also replicated the findings of Experiments 1 and 2: (a) the relationship between tasks and willingness to help was fully mediated by scene imagery and perspective-taking (but not by person imagery), and (b) scene imagery was the most robust attribute driving the effect of episodic simulation, followed in order by perspective-taking and person imagery.

Critically, Experiment 3 demonstrated that episodic simulation led to a greater willingness to help compared with manipulations that isolated alternative explanatory elements of an imagined helping episode. These results provide clear-cut evidence for a distinct effect of episodic simulation as supported by increases in scene imagery and perspective-taking in particular.

Experiment 4: The Effects of Episodic Simulation on Writing to Person in Need

In Experiment 4, we sought to replicate Experiments 1 through 3 with actual helping behavior. Instead of answering a question about one's willingness to help, participants engaged in the letter writing task in which they were asked to write to a person who had experienced a misfortune to offer some support (modeled after Masten, Morelli, & Eisenberger, 2011). We predicted that episodic simulation would increase the amount of effort participants' would expend to comfort the person in need, as measured by time spent writing and the number of words they wrote, while diminishing the differences in these indices for in-group and out-group targets (relative to the control condition).

Method

Participants. Based on the previous experiments' power analyses, we recruited participants on MTurk until we had approximately 200 usable participants per condition. The participants' recruitment and inclusion/exclusion criteria followed the same procedures in Experiments 1 through 3. We collected 1,056 participants, a subset of which we excluded for the following reasons: (a) they were neither a Democrat nor a Republican ($n = 77$); (b) they failed to comply with the main task instructions ($n = 34$, independent coder interrater reliability = .95, see online supplemental materials on OSF for inclusion/exclusion criteria); (c) they

answered one of the team-manipulation check questions at the end of the study incorrectly ($n = 143$); (d) they reported that they did not believe that the manipulation task was factual ($n = 2$); and/or (e) the survey failed to record the outcome measure ($n = 1$). Our final sample included 799 participants ($M_{\text{age}} = 33.18$ years, $SD = 10.90$, age range = 18–71; 43% female; 68% Democrat).

Procedure. Participants were randomly assigned to one of four conditions in a 2 task (Episodic Helping vs. No Helping) \times 2 target group (in-group vs. out-group) between-subjects design. The experimental design was identical to that in Experiment 2, with a few modifications: (a) we only included scene imagery and perspective-taking items (i.e., those that showed the significant mediation effects in the previous mediation analysis in Experiments 1 through 3); (b) we set a limit on words that participants were allowed to write during the tasks (because writing more during the task induction could reduce the amount participants wrote for the main outcome measure); and (c) we presented only one scenario about a person's garage that was burned down by his neighbor's misbehavior (see online supplemental materials on OSF). This scenario elicited a high level of willingness to help in previous work on episodic simulation (e.g., Gaesser & Schacter, 2014) and made clear that the victim was not responsible for the negative outcome (Kogut, 2011).

In the main tasks (i.e., Episodic Helping and No Helping), participants were presented with the following scenario and instructions: "[Target]'s neighbor accidentally shot off fireworks into [Target]'s garage, which caught on fire"; and "Please describe [what you imagined/how you would improve the story] in 30–40 words. You must have exactly 30–40 words to advance."² Participants saw the word count while typing and were able to advance when they had written more than 30 and less than 40 words. After completing the task prompts, participants completed the scene imagery and perspective-taking ratings, and wrote in response to the following letter writing prompt: "It has been a few days since [Target]'s garage burned down. [Target] is still distressed and upset by this incident. Please write a note to [Target] to offer him some support." We never labeled this task a "helping task" to minimize demand effects.

Analysis. We analyzed helping behavior (i.e., time letter writing, letter word count), scene imagery, perspective-taking and appearance of positive and negative emotion words in the letter writing task, using a 2 task (Episodic Helping vs. No Helping) \times 2 target group (In-group vs. Out-group) between-subjects ANOVA, implemented with the 'ezANOVA' function in R.

² Before conducting Experiment 4, we conducted a pilot study with self-reported willingness to help as an outcome variable, and our central finding replicated even when we limited the number of words participants could use to describe their episodic simulation. The pilot study ($n = 179$; $M_{\text{age}} = 35.17$ years, $SD = 12.02$, age range 18–74; 47% female) was a 2 task (Episodic Helping vs. No Helping) \times 3 scenario (Burned Garage vs. Lost Watch vs. Missing Dog, see experimental materials on OSF) between-subjects factorial design. The pilot replicated our main effect of task ($F(1,173) = 37.47$, $p < .001$, $\eta^2 = .17$) showing that participants were more willing to help the target in Episodic Helping ($M = 6.23$, $SD = 1.04$) than the No Helping condition ($M = 5.00$, $SD = 1.61$). There was neither a main effect of scenario ($F(2,173) = .26$, $p = .77$, $\eta^2 = .003$) nor a Task \times Scenario interaction ($F(2,173) = .57$, $p = .57$, $\eta^2 = .006$). This result confirmed that the effects of episodic simulation in Experiments 1 through 3 replicated even when we limited the number of words participants wrote in response to the Episodic Helping and No Helping prompts.

Results

In-group and out-group identification scores. As in Experiments 1 through 3, we created composite in-group and out-group identification scores by averaging the liking/valuing/feeling connected items for each political party (in-group items $\alpha = .90$; out-group items $\alpha = .86$). Consistent with Experiments 1 through 3, participants reported that they identified more with their in-group ($M = 73.75$, $SD = 18.92$) than out-group members ($M = 22.02$, $SD = 19.36$; $t(798) = 51.69$, $p < .001$, $d = 1.83$).

Letter writing: Time and word count. Because these are both open-ended responses, we examined the distributions of the time and word count measures for outliers. We identified strong outliers as data points more than 3.0 times the interquartile range above the third quartile. Identification or removal of strong outliers has been used previously as an effective tool for dealing with right-skewed distributions (Ross & Juliano, 2015, te Nijenhuis & van der Flier, 2013). The correlation between the two outcome variables was more robust after the removal of the outliers (with outliers: $r(797) = .43$, $p < .001$; without outliers: $r(774) = .50$, $p < .001$). We report the results for these outcome measures without outliers ($n = 23$) here, but for sake of completeness, we report the results with outliers in the supplemental materials on OSF.

See Table 4 for descriptive statistics. A 2 task (Episodic Helping vs. No Helping) \times 2 target group (In-group vs. Out-group) ANOVA on time spent writing indicated a main effect of target group ($F(1, 772) = 4.44$, $p = .035$, $\eta^2 = .006$, see Figure 7a), such that people spent more time on the letter when the target was an in-group ($M = 98.17$, $SD = 57.06$) as opposed to out-group member ($M = 90.17$, $SD = 48.34$). No other main effect or interaction was significant.

The same model on word count revealed a main effect of task ($F(1, 772) = 5.87$, $p = .016$, $\eta^2 = .008$, see Figure 7b), such that participants wrote more words in the Episodic Helping ($M = 44.90$, $SD = 18.92$) than the No Helping task ($M = 41.72$, $SD = 17.50$). This main effect was qualified by the predicted, but marginal, Task \times Target group interaction ($F(1, 772) = 3.49$, $p =$

.062, $\eta^2 = .005$). No other main effect or interaction was significant.

Because we had Task \times Target Group interaction prediction, we followed up the marginal word count interaction with planned comparison analyses. Consistent with our predictions, the in-group versus out-group difference in word count was significant in the No Helping task, $t(385) = -2.09$, $p = .037$, $d = .21$, but eliminated in the Episodic Helping task, $t(387) = .61$, $p = .541$, $d = .06$.

Scene imagery and perspective-taking items. See Table 5 for descriptive statistics; note these results include the 23 participants who were counted as outliers for the helping behavior outcome measures ($n = 799$). Replicating Experiments 1 through 3, a 2 task (Episodic Helping vs. No Helping) \times 2 target group (In-group vs. Out-group) between-subjects ANOVA revealed a main effect of task on scene imagery ($F(1, 795) = 361.63$, $p < .001$, $\eta^2 = .31$) and perspective-taking ($F(1, 795) = 139.93$, $p < .001$, $\eta^2 = .15$), both of which were higher following Episodic Helping relative to No Helping. There was a marginal main effect of target group on perspective-taking ($F(1, 795) = 3.33$, $p = .06$, $\eta^2 = .068$), which was greater for the in-group than the out-group targets. No other main or interaction effects were significant.

Discussion

Experiment 4 provides further evidence that episodic simulation not only increases helping behavior overall but also decreases intergroup bias in actual helping behavior. Replicating the patterns of results for willingness to help in Experiments 1 through 3, Experiment 4 provides evidence that imagining a helping episode increased the number of words written to support a person in need (relative to thinking about how best to revise the story). Moreover, imagining the helping episode eliminated the intergroup bias in helping behavior present in the control condition.

Experiment 5: The Effects of Episodic Simulation on Donating Money to a Person in Need

There has been strong precedent in the field for conceptualizing social support and comforting as prosocial behavior (e.g., Batson, 1998; de Waal & van Roosmalen, 1979; Dovidio, Piliavin, Schroeder, & Penner, 2006) and letter writing in particular (e.g., Masten et al., 2011). Nevertheless, we further sought to provide convergent evidence for the role of episodic simulation in prosocial behavior using another measure of altruistic behavior: monetary donation to people in need. In previous experiments, episodic simulation led to greater amount of donation to victims of misfortune (Gaesser et al., 2018). Given this finding and results of Experiments 1 through 4, we hypothesized that people would donate more to help others in need in the Episodic Helping task relative to the No Helping task. We also hypothesized that the intergroup difference in donations would be reduced in the Episodic Helping compared with the No Helping task.

Method

Participants. We conducted an a priori power analysis of the effect size ($d = .25$) corresponding to the contrast of interest in Gaesser et al. (2018; i.e., the difference in donation to help people

Table 4

Summary of Mean Values (M) and SD of Outcome Variables Without Outliers by Task, Target Group, and Task \times Target Group in Experiment 4

Condition	Time letter writing (without outliers)		Letter word count (without outliers)	
	M	SD	M	SD
Task				
Episodic Helping (EH)	93.64	51.98	44.90	18.92
No Helping (NH)	94.79	54.16	41.73	17.50
Target group				
In-group	98.18	57.06	43.96	17.50
Out-group	90.17	48.34	42.67	19.06
Task \times Target Group				
EH \times In-Group	95.89	54.74	44.33	17.02
EH \times Out-Group	91.30	48.99	45.50	20.74
NH \times In-Group	100.50	59.39	43.58	18.01
NH \times Out-Group	89.05	47.80	39.87	16.82

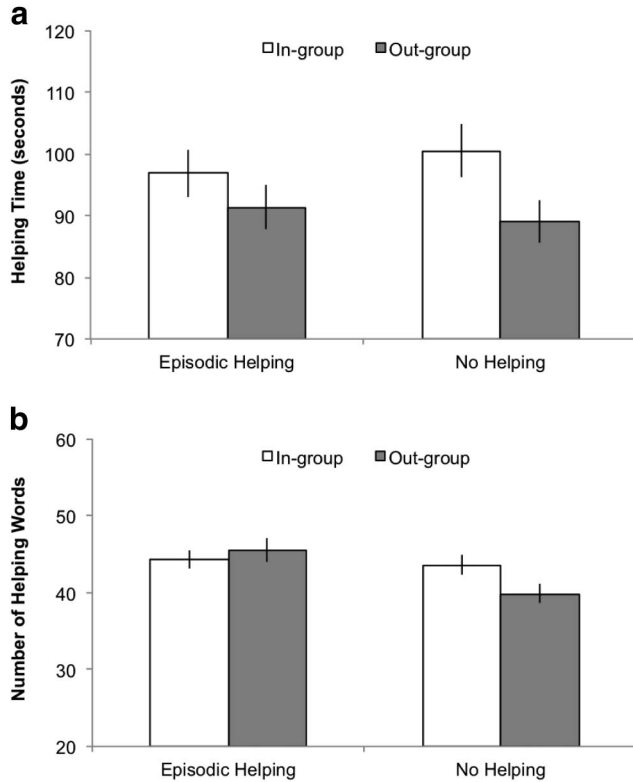


Figure 7. (a) Mean value for time letter writing (seconds) in both Episodic Helping and No Helping tasks without outliers in Experiment 4. Error bars denote $\pm SEM$. (b) Mean value for letter word count in both Episodic Helping and No Helping tasks without outliers in Experiment 4. Error bars denote $\pm SEM$.

in need between the Episodic Helping and the No Helping task). The analysis indicated that running approximately 342 participants in each task would allow for the detection of the predicted effect (power $>.95$). Based on this analysis, we recruited participants on MTurk until we had approximately 171 usable participants per one of four experimental conditions (equivalent to 342 usable partici-

pants per task). Recruitment and inclusion/exclusion criteria followed the same procedures in Experiments 1 through 4.

We collected data from 1319 participants, a subset of which we excluded for the following reasons: (a) they were neither a Democrat nor a Republican ($n = 52$); (b) they failed to comply with the main task instructions ($n = 58$, independent coder interrater reliability = .94, see supplemental materials on OSF for inclusion/exclusion criteria); (c) they answered one of the team-manipulation check questions at the end of the study incorrectly ($n = 427$); (d) the survey failed to record at least one of the demand characteristic measures ($n = 7$); and/or (e) they participated in the study more than once ($n = 1$). Our final sample included 774 participants ($M_{age} = 34.95$ years, $SD = 11.91$, age range = 18–87; 55% female; 66% Democrat).

Procedure. Participants were randomly assigned to one of four conditions in a 2 task (between-subjects: Episodic Helping vs. No Helping) \times 2 target group (between-subjects: in-group vs. out-group) between-subjects factorial design.

The experimental design was identical to Experiment 4, with a few modifications: (a) we replaced the letter writing task with the donation task; (b) we made all participants write at least 30 words during the tasks for consistency across participants and tasks; and (c) we included a 10-item social desirability scale (MC-1; Strahan & Gerbasi, 1972), a standard measure of a participant's tendency to respond in a manner that would be perceived favorably by others, to control for the possibility that responses to the episodic simulation manipulation were driven instead by this tendency.

For the donation task, we adapted instructions and procedures from the economic dictator game (see supplementary methods from Peysakhovich, Nowak, & Rand, 2014). At the beginning of the Experiment 5, participants read the following instructions: "During the course of this study, you can earn up to an extra \$1.50 with the option to transfer up anywhere between \$0 to \$1.50 to someone like a person in the episode." After engaging in either the Episodic Helping or the No Helping task, participants made their decisions on a scale of \$0–1.50 to offer to help someone like the person in the episode, via *YouCaring*, a crowdfunding website for charitable causes. At the end of this experiment, participants completed the social desirability scale (see online supplemental materials on OSF for the full instruction and questions).

Table 5

Descriptive Statistics for Representational, Perspective-Taking, and Emotion Items by Task, Target Group, and Task \times Target Group in Experiment 4

Condition	Scene imagery		Perspective-taking		Positive emotion		Negative emotion	
	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>
Task								
Episodic Helping (EH)	6.25	1.11	5.31	1.79	7.10	4.34	4.15	2.93
No Helping (NH)	4.04	2.04	3.71	2.03	7.30	4.21	4.73	3.00
Target group								
In-group	5.12	1.99	4.39	2.07	6.95	3.80	4.43	3.07
Out-group	5.17	1.97	4.63	2.07	7.46	4.70	4.46	2.89
Task \times Target Group								
EH \times In-Group	6.23	1.12	5.13	1.81	6.74	3.78	4.27	3.22
EH \times Out-Group	6.28	1.11	5.50	1.76	7.48	4.83	4.03	2.60
NH \times In-Group	4.01	2.05	3.65	2.05	7.16	3.81	4.59	2.90
NH \times Out-Group	4.08	2.03	3.77	2.00	7.43	4.57	4.87	3.10

After the entire study was complete, participants were told that they would receive the amount they did not donate. Although donations could not be donated to the actual person in the episode, donations were provided to *YouCaring* to support actual people in need suffering plights related to the situation described in the episode.

Analysis. We analyzed the data using a 2 task (Episodic Helping vs. No Helping) \times 2 target group (In-group vs. Out-group) ANOVA, implemented with the ‘aov’ function in R. Our model included the main effects of task, the main effects of target group, and the interaction terms.

We calculated the social desirability score for each participant based on the scale’s coding scheme (see online supplemental materials on OSF for the scoring guide). Each score was a number on a scale of 0 (*low social desirability*) to 10 (*high social desirability*). To examine the possible effects of social desirability on economic donation and other measures, we examined the correlation between social desirability and willingness to help. We also conducted an analysis of covariance (ANCOVA) analysis to account for social desirability. Our ANCOVA model included task as a predictor, social desirability score as a covariate, and the amount of economic donation as a response variable.

Results

In-group and out-group identification scores. Consistent with Experiments 1 through 4, we created composite in-group and out-group identification scores by averaging the liking/valuing/feeling connected items for each political party (In-group $\alpha = .95$; Out-group $\alpha = .89$). Consistent with Experiments 1 through 4, participants reported that they identified more with their in-group ($M = 76.02$, $SD = 20.14$) than out-group members ($M = 21.14$, $SD = 20.16$; $t(773) = 51.79$, $p < .001$, $d = 1.86$).

Social desirability. Donation amounts were not significantly correlated with participants’ social desirability, $t(772) = 1.64$, $p = .102$, $r = .06$. ANCOVA results showed that the main effect of task on the amount of economic donation remained significant ($F(1, 771) = 18.64$, $p < .001$, $\eta^2 = .024$) even when social desirability was accounted for. These results indicated that social desirability as measured by the social desirability scale could not account for the effects we describe in Experiments 1–4, so we exclude social desirability scores from further analyses.

Donations. The ANOVA results revealed a predicted main effect of task ($F(1, 770) = 18.28$, $p < .001$, $\eta^2 = .023$), such that participants donated more to a person in need in the Episodic Helping ($M = 0.69$, $SD = 0.57$) than the No Helping task ($M = 0.52$, $SD = 0.52$); however, the main effect of target group was not significant ($F(1, 770) = 1.72$, $p = .191$, $\eta^2 = .002$). There was no Task \times Target Group interaction effect ($F(1, 770) = 1.77$, $p = .184$, $\eta^2 = .002$; Figure 8).

Because we had specific Task \times Target Group interaction prediction, we turned to our planned comparison analyses between in/out-group and tasks. Consistent with our predictions, the in-group versus out-group difference in donations was significant in the No Helping task, $t(366) = -1.97$, $p = .050$, $d = .21$, but not significant in the Episodic Helping task, $t(404) = -.03$, $p = .974$, $d = .003$.

Scene imagery and perspective-taking items. See Table 6 for descriptive statistics. Replicating Experiments 1 through 4, a 2 task (Episodic Helping vs. No Helping) \times 2 target group (In-group vs. Out-group) between-subjects ANOVA revealed a main effect

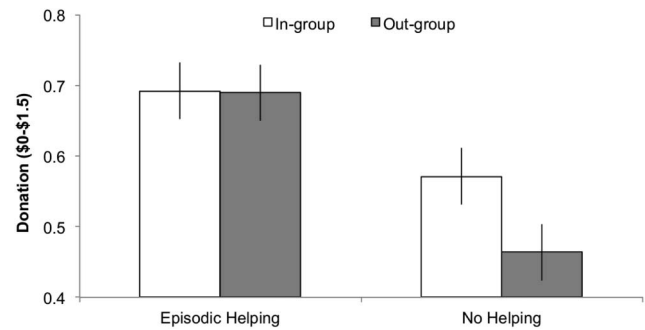


Figure 8. Mean value for the amount participants donated to a person in need in both Episodic Helping and No Helping tasks in Experiment 5. Error bars denote $\pm SEM$.

of task on scene imagery ($F(1, 770) = 400.55$, $p < .001$, $\eta^2 = .34$) and perspective-taking ($F(1, 770) = 160.02$, $p < .001$, $\eta^2 = .17$), both of which were higher following Episodic Helping relative to No Helping. Neither the main effect of group nor the interaction was significant.

Discussion

Experiment 5 provided converging evidence that episodic simulation could reduce intergroup bias in helping behavior in the form of donations. Replicating the pattern of results for helping intention (Experiments 1 through 3) and actual prosocial behavior (Experiment 4), Experiment 5 found that imagining a helping episode increased the amount of donation participants offered to help a person in need despite his group membership.

General Discussion

We investigated whether episodic simulation of a helping event would increase helping overall and decrease intergroup bias in helping intentions and behavior. In line with our predictions, and replicating previous findings (Gaesser et al., 2017; Gaesser et al., 2015, 2018; Gaesser & Schacter, 2014), we found that imagining helping a person in need increased participants’ willingness to help overall. More important, we found that the episodic simulation of helping events reduced intergroup bias in prosocial responses, both in the form of explicit self-reports (Experiments 1, 2, and 3) and actual helping behavior (Experiments 4 and 5). These results were not accounted for by disrupted encoding of targets’ group membership merely imagining positive contact with an individual target, or the social desirability scale, but rather by richer representations of the sensory properties of the scene and the targets’ mental states, respectively.

Mechanism: Scenes and Minds

The multiple mediation analyses in Experiments 1 through 3 indicated that episodic simulation contributed to prosocial responses via scene imagery and perspective-taking, independently, with little to no effect through person imagery. The finding that the vividness of scene imagery affects willingness to help in-group and out-group targets observed here is consistent with previous correlational findings using unaffiliated targets (Gaesser et al., 2017; Gaesser et al., 2015, 2018;

Table 6

Descriptive Statistics for Representational and Perspective-Taking Items by Task, Target Group, and Task × Target Group in Experiment 5

Condition	Scene imagery		Perspective-taking	
	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>
Task				
Episodic Helping (EH)	6.49	.85	5.56	1.68
No Helping (NH)	4.33	1.96	3.95	1.89
Target group				
In-group	5.38	1.86	4.86	1.86
Out-group	5.55	1.81	4.73	2.05
Task × Target Group				
EH × In-Group	6.37	1.01	5.68	1.59
EH × Out-Group	6.60	.65	5.45	1.76
NH × In-Group	4.37	1.98	4.02	1.73
NH × Out-Group	4.30	1.95	3.86	2.04

Gaesser & Schacter, 2014): the more vividly the scene imagery of the helping episode is experienced, the more easily the event is brought to mind, and the more willing we are to help a person in need.

Emerging work sheds additional light on the underlying mechanism driving this effect, suggesting that the spatial representation of an episode in particular may heighten the vividness of an imagined scene and willingness to help. Broadly speaking, the spatial context of imagined episodes is thought to serve as a platform on which fragmented details can be combined into an integrated and vivid experience (Addis & Schacter, 2012; Maguire & Hassabis, 2011; Maguire & Mullally, 2013; Robin, Wynn, & Moscovitch, 2016; Suddendorf & Corballis, 2007). This explanation is supported by recent experiments, in which we manipulated the location of an imagined helping episode, which heightened the vividness of scene imagery and led to a corresponding increase in willingness to help (Gaesser et al., 2018). Recent neuroimaging dovetail with this account: when participants imagined helping episodes in the scanner, BOLD signal within the medial temporal lobe subsystem—specifically regions supporting spatial and episodic processing (Andrews-Hanna et al., 2010; Andrews-Hanna, Smallwood, & Spreng, 2014; Ranganath & Ritchey, 2012)—predicted willingness to help when participants imagined helping episodes (but not when participants completed nonepisodic control tasks Gaesser, Hirschfeld-Kroen, Wasserman, Horn, and Young (2019).

As we saw in Experiments 1 through 3, episodic simulation also engaged perspective-taking (Gaesser et al., 2017; Gaesser et al., 2018), augmenting the representation of the mental states of the person in need, for both in-group and out-group targets. These findings do not support an interpretation that the vividness of an imagined episode affects prosocial decisions *exclusively* by making it easier to consider of the thoughts and feelings of the person in need, but rather suggest multiple channels by which episodic simulation guides prosocial decisions.

Perspective-taking has long been recognized as an effective strategy for promoting helping behavior (Batson & Ahmad, 2009; Carlo, Allen, & Buhman, 1999; Todd, Bodenhausen, Richeson, & Galinsky, 2011). Specifically, manipulating perspective-taking by instructing participants to adopt the thoughts and feelings of the

person in need reliably increases concern for others and motivates altruistic behavior (Batson, 2011) even for stigmatized out-group targets (Batson, Chang, Orr, & Rowland, 2002; Batson, Early, & Salvarani, 1997). Indeed, recent work has isolated a unique contribution of perspective-taking to charitable decisions distinct from affect-sharing and general shifts in attention (Tusche, Böckler, Kanske, Trautwein, & Singer, 2016; see also for related review Zaki & Ochsner, 2012).

The present studies replicate and enrich a novel multipath model that sheds light on when and how scene-related representations interact with perspective-taking (Gaesser et al., 2017; Gaesser et al., 2018) to (a) facilitate decisions to help others in need and (b) overcome the bias generated by social group boundaries. Notably, these effects were not explained by how vividly the *person* in need was represented (as measured by person imagery) or by attenuating memory for group membership. Specifically, Experiment 2 demonstrated that willingness to help targets increased after the episodic simulation manipulation, despite the fact that they still remembered the targets' group memberships. As such, the effect of episodic simulation on reducing intergroup bias in helping is not attributable to a dilution effect of group membership (i.e., failing to encode and retrieve group membership).

A vast majority of intergroup bias intervention studies have emphasized changing people's representations of and emotional responses to out-group members (Cikara & Van Bavel, 2014). Of particular relevance is the insightful work on *imagined contact* (i.e., imagining positive interactions with out-group members), which points to a role for representing hypothetical experiences to reduce intergroup biases. However, in those studies, the focus is on person representation, attitudes, and semantic behavioral scripts of positive interaction (Miles & Crisp, 2014). While episodic simulation does involve representing a person, it also involves the broader scene in which a person is embedded, binding together details of the location, people, and objects, to form an event that is specific in time and place. Critically, the findings from Experiment 3 demonstrate that merely imagining a positive interaction with a person in need does not account for the prosocial effect of episodic simulation on intergroup interaction. Specifically, the effects of imagined contact on helping were not mediated by scene imagery, which was the strongest mediator of the relationship between episodic simulation and helping.

The present findings reveal the importance of episodic and mental state representation, processes drawing on cognitive and neural mechanisms (i.e., medial temporal lobe subsystem, theory of mind network), which are qualitatively distinct from those previously highlighted in the contact literature. In addition, previous research has shown that episodic simulation increases willingness to help above and beyond semantic priming of helping responses (Gaesser et al., 2017; Gaesser et al., 2019; Gaesser & Schacter, 2014). To be clear, we do not claim that episodic simulation accounts for all of the effects that imagined contact has been shown to elicit or that episodic simulation and imagined contact will have completely divergent effects. For example, we have yet to examine whether episodic simulation can be used to change explicit and implicit attitudes toward out-group members to the extent that imagined contact does. Instead, we believe the application of episodic simulation, is an additional exciting avenue for research into effective interventions for intergroup conflict. In what follows, we contemplate further future directions to investi-

gate the benefits and limits of episodic simulation in addressing intergroup bias.

Further Future Directions

The role of memory. While we did not observe an effect of memory for group membership, that does not rule out a possible contribution of *episodic memory* of specific past helping events informing prosocial decision-making. Episodic simulation and episodic memory recruit many of the same cognitive processes and content (Atance & O'Neill, 2001; Conway, 2009; Schacter et al., 2008, 2012; Szpunar, 2010). As such, the present experiments suggest potential promise for the role of episodic memory in attenuating intergroup bias in helping. For example, previous experiments have demonstrated that episodic simulation and episodic memory equally increased willingness to help to a person in need (when the group membership of target was unspecified; Gaesser & Schacter, 2014). Therefore, it seems possible remembering past helping episodes involving out and in-group targets may evoke a comparable prosocial response as imagining helping episodes that have yet to take place.

Critically, however, a prosocial effect of memory on helping for an out-group target requires that an individual has had previous experience helping an out-group target in the first place. Without the initial past experience there is no memory trace that can subsequently influence decision-making and behavior in the present. Given the well documented intergroup bias in helping (Brewer & Miller, 1984; Bruneau & Saxe, 2012; Dixon et al., 2007; Dovidio et al., 2009; Hewstone & Brown, 1986; Lai et al., 2014, 2016; Paluck et al., 2018; Pettigrew & Tropp, 2006; Vorauer & Sasaki, 2009), we suspect that the range of prosocial behaviors episodic memory could be used to facilitate is much more limited compared with the less constrained prosocial effect of episodic simulation (see Gaesser & Schacter, 2014 for related discussion on flexible advantage of episodic simulation vs. episodic memory).

Intergroup differences and gender. In the present studies, we held the gender of the person in need constant across conditions to control for possible differences gender of the target may have elicited. Such an approach, however, leaves open whether target gender interacts with an effect of episodic simulation on willingness to help out-group targets. From the perspective of Social Dominance Theory (Sidanius & Pratto, 2001) and the Theory of Gendered Prejudice (Sidanius, Hudson, Davis, & Bergh, 2018), men are more likely targets of intergroup discrimination than are women. Specifically, in-group men and women are predicted to be equal recipients of in-group love, but out-group men are more likely to be targets of out-group denigration and hostility than out-group women. On one hand, it is possible that including only male targets in our studies yielded a relatively conservative test of the efficacy of episodic simulation as an intervention: because out-group male targets should have elicited more firmly entrenched hostilities, it could have been harder for an intervention to have an impact. On the other hand, one could argue that using only male targets widened the intergroup helping gap in the control condition making the experiment more sensitive to detecting an effect of episodic simulation. Future research is needed to adjudicate possibilities.

Different manifestations of prosociality. There are many approaches to helping someone in need (e.g., providing emotional support, volunteering time, donating money, or other material resources), a fact reflected in the diversity of ways helping behavior has been operationalized and measured (Hare, Camerer, Knoepfle, O'Doherty, & Rangel, 2010; Latané & Nida, 1981; Leiber, Klimecki, & Singer, 2011; Masten et al., 2011; Warneken & Tomasello, 2009). Thus, an open question is whether episodic simulation will have a similar effect facilitating other forms of prosocial behavior in addition to writing an emotionally supportive letter and making monetary donations. To the extent that prosociality is a cohesive cooperative phenotype (Peysakhovich et al., 2014), one prediction would be that the effect of episodic simulation will similarly extend to other forms of helping behavior. In contrast, to the extent that prosociality is a multifaceted construct (Böckler, Tusche, & Singer, 2016) episodic simulation may be particularly effective at motivating specific subtypes of prosocial behaviors (but not others). While delineating the unified versus multifaceted nature of prosociality is beyond the scope of the present article, examining whether episodic simulation is similarly associated with different forms of prosocial behavior or whether particular forms of prosocial behavior may be more strongly facilitated by episodic simulation will better position researchers to most effectively translate an effect of episodic simulation on intergroup helping in real-world contexts.

Conclusion

While humans' tendency to help others is considered central to our morality and most cherished social institutions, our decisions to help people in need are not without bias. To the extent that reducing intergroup bias and conflict is critical for fostering a more peaceful and equitable society, discovering and characterizing the psychological processes that can be used to attenuate our group-based biases is of the highest priority. Given the complexity and self-reinforcing nature of group-based bias, this is by no means an easy problem to address and will likely require a multimechanism approach. Researchers have made significant progress uncovering a number of group-related cognitions and emotions that reduce intergroup bias. Here we revealed a new tool for intergroup researchers and policymakers to add to their debiasing armory: episodic simulation.

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