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Imagination and the prosocial personality: Mapping the effect of episodic simulation on helping across prosocial traits

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Abstract

Prior work suggests that imagining helping others increases prosocial intentions and behavior toward those individuals. But is this true for everyone, or only for those who tend toward—or away from—helping more generally? The current study ($N = 283$) used an imagined helping paradigm and a battery of behavioral and self-report measures of trait prosociality to determine whether the prosocial benefits of imagination depend upon an individual's general tendency to help others. Replicating prior work, we found links between imagination and prosociality and support for a three-factor model of prosociality comprising altruistically, norm-motivated, and self-reported prosocial behaviors. Centrally, the effects of imagination on prosociality were slightly larger for *less* altruistic individuals but independent of norm-motivated and self-reported prosociality. These results suggest leveraging people's abilities for episodic simulation as a promising strategy for increasing prosociality in general, and perhaps particularly for those least likely to help otherwise.

KEYWORDS
charity, cooperation, economic games, episodic simulation, prosocial behavior

1 | INTRODUCTION

The success of social groups depends on the ability of individuals within those groups to cooperate with and behave prosocially toward one another. More than half of Americans engaged in one of the more common forms of prosociality—donating to charity—in 2018, giving over $400 billion to various domestic and international causes in this year (Giving USA, 2019) and 58% of Americans in a 2020 survey reported volunteering time to a charitable organization within the past year (Gallup, 2020). Accordingly, social scientists have devoted significant time and effort to better understand prosocial behavior and have made significant strides in understanding the contributions of things like state levels of empathy (Batson et al., 1989; Tusche et al., 2016), group affiliation (Hein et al., 2010), and personality (Penner et al., 1995) to prosociality. Yet only recently have researchers begun to uncover a similar role for a basic psychological capacity with a non-obvious connection to prosocial behavior: episodic simulation, or our ability to imagine specific events unfolding in time and space (Schacter et al., 2008). Vividly imagining different versions of future events and their hypothetical outcomes informs judgments about what could occur (Szpunar & Schacter, 2013) and the emotional responses that imagined scenes induce are thought to incentivize behaviors directed at subjectively preferred outcomes (Benoit et al., 2011, 2014; Wimmer & Buchel, 2016). Vivid imagery and one's episodic ability have also been shown to broadly affect people's moral judgments (Amit & Greene, 2012; McCormick et al., 2016).

Recent empirical findings have shown a direct link between episodic simulation and prosocial intentions and behavior. After imagining themselves helping others in need, people become more willing to help these targets (Gaesser & Schacter, 2014) and devote a greater amount of resources (e.g., time, money) to them (Gaesser et al., 2018; Gaesser et al., 2020) as compared to conditions controlling for exposure to the person in need and conceptual priming of helping. This appears to be true even when those in need are outgroup members whom people might typically neglect to help (Gaesser et al., 2020; Hein et al., 2010). Moreover, patients with medial temporal lobe lesions, a core set of brain regions supporting episodic simulation, do not show a similar increase in prosocial intentions after imagining

helping (Sawczak et al., 2019). Prior work suggests that this effect depends on factors like the spatial representation and vividness of the imagined scene, perspective-taking that results from episodic simulation (Gaesser et al., 2018), and positive affect that results from imagining oneself helping (Gaesser, et al., 2017).

Despite the fact that the relationship between imagination, willingness to help, and actual helping behaviors has been demonstrated across several studies, relatively little is known about whether and how the effectiveness of imagination-based interventions such as those described above varies with individuals' broader propensities toward prosociality and cooperation. One possibility is that an interaction between state (i.e., increases in helping resulting from an imagined helping intervention) and trait (i.e., more stable tendencies toward helping) measures exists. Specifically, it could be the case either that the positive effects of imagination on helping are driven by individuals in these studies who were already highly prosocial and might be more receptive to interventions to increase helping, or that these effects will be witnessed only among those who tend *not* to help and who may benefit more from such an intervention than those with greater dispositional tendencies toward helping. Alternatively, the effect of imagination on helping might be independent of these propensities, enhancing prosocial intentions and behavior for people no matter their general tendencies toward prosociality (i.e., no significant state–trait interaction exists). The answer to this question would have applied implications for who is most likely to benefit from interventions centered on imagined helping and in what contexts such interventions would be most likely to succeed.

However, obtaining an answer to this question requires considering another important issue—how best to operationalize general tendencies toward helping and cooperation given the array of behavioral and self-report measures that purport to do so. Behavioral economists, for example, have often employed "games" in which participants make decisions about how to apportion resources between themselves and others (e.g., the Dictator Game, in which participants are given money and must decide how much of this money to give to another anonymous player in a one-shot interaction; Camerer, 2003). Others have used self-report scales, such as those assessing empathy (Davis, 1983), reciprocity (Perugini et al., 2003), or prosociality more broadly (Penner et al., 1995). Still others have attempted to more directly measure prosocial behaviors themselves, for example in the form of charitable donations (e.g. Freeman et al., 2009) or time spent volunteering (Bartlett & DeSteno, 2006).

Because of the multitude of ways prosociality has been measured, there have been several recent investigations of the dimensionality (i.e., underlying factor structure) of prosociality (Böckler et al., 2018, b; McAuliffe et al., 2019; Peysakhovich et al., 2014). Multiple prosocial measures can be integrated into underlying latent factors (or latent variables). Adopting a latent factor approach may present a way to improve the psychometric properties of individual prosocial measures (Tusche & Bas, 2021). Initial evidence suggests that latent factors of human prosociality may be domain general and stable over time (Böckler, Tusche, Schmidt et al., 2018; Peysakhovich et al., 2014). These results mirror evidence of more robust latent factor scores in other domains to capture trait-like qualities (e.g., self-regulation, Eisenberg et al., 2019; or risk-preferences, Frey et al., 2017). Latent factors may therefore be more suitable for trait-like assessments of individual differences in prosociality that generalize across time and contexts. To date, several models have been proposed: For example, some such investigations have found support for a single latent factor accounting for variance across diverse measures of altruistic and cooperative behaviors (Hubbard et al., 2016; Peysakhovich et al., 2014). Alternatively, others have examined behavior across charitable donations, hypothetical distribution tasks, economic games, interactive computer tasks, and self-report measures and concluded that performance across these varied measures can be interpreted as reflecting three subcomponents of prosociality with distinct motivational underpinnings (Böckler et al., 2016; Böckler, Tusche, & Singer, 2018; Böckler, Tusche, Schmidt, et al., 2018[1]). These included the motivation to engage in costly behaviors to help others (*altruistically motivated prosocial behavior*), the motivation to behave in line with social norms and enforce them in others (*norm motivated prosocial behavior*), and the tendency to perceive and describe oneself as a generous and moral person (*self-reported prosocial behavior*). More broadly, other research has explored links between personality traits, situational affordances (i.e., circumstances allowing for the expression of those traits), and prosocial behavior across various economic games, finding significant correlations between a number of stable facets of personality, including some of those we measured here (e.g., social value orientation, Machiavellianism, agreeableness) and behavior across different games (Thielmann et al., 2020; Zhao & Smillie, 2015). Here, we opted to use the labels and factor interpretations suggested by some of this recent research (with a focus on Böckler et al., 2016; Böckler, Tusche, & Singer, 2018; Böckler, Tusche, Schmidt, et al., 2018) and apply it in the context of our central research question while acknowledging that the dimensionality of individual differences in prosociality and its underlying motivations is a matter of ongoing research (e.g., Andreoni et al., 2017; Dunning et al., 2019; Thielmann et al., 2020; Zhao et al., 2017).

Thus, in the present research we first sought to replicate past work showing effects of imagination on prosocial intentions (in the form of self-reported willingness to help another) and behavior (in the form of monetary donations and time volunteered to write supportive letters) toward the specific targets of the episodic simulation. Having replicated the applied effect of episodic simulation on prosocial intentions and behavior in our sample, we proceeded by testing our central question of interest: whether this effect depends upon individuals' more general tendency to behave prosocially across situations. Thus, different from prosocial measures toward specific targets in a specific context of the Imagined Helping task ("state"), here we are interested more trait-like assessments of people's prosociality (what has been dubbed as the "cooperative phenotype" that generalizes across domains and time). To capture this general prosocial tendency, we utilized a battery of measures of prosocial and cooperative behaviors that included economic games, self-report measures, charitable donations, and time spent volunteering. Covariance among these measures

was investigated using confirmatory factor analyses that yielded support for the three-factor model introduced by Böckler et al. (2016); Böckler, Tusche, and Singer (2018); Böckler, Tusche, Schmidt, et al. (2018) described above. We then tested our key question: can variance in individuals' general tendency to act prosocially (as captured in the latent factors scores described above) predict imagination-induced changes in their prosocial intentions and behavior?

2 | METHOD

2.1 | Participants

Participants were recruited from the undergraduate research pool of the University at Albany, State University of New York for a study on "social judgment and decision-making." Participants received course credit for their participation, but upon arrival at the lab were informed that they would also receive a small cash bonus whose amount would be determined by decisions they made throughout the study. In reality, all participants received $4 at the end of the study. We collected data from $N = 388$ participants, anticipating some data loss due to the attention checks embedded throughout the study. We excluded from final analyses participants who did not complete 25% or more of all dependent measures ($n = 26$; the remainder of the sample had an average of 7% missing data) or failed any of five total attention checks embedded in the economic game measures ($n = 79$), leaving a final sample of $N = 283$ cases (mean age = 18.66, $SD = 1.17$, range 18 to 28 years; 54.1% women). A sensitivity analysis conducted using G*Power (Faul et al., 2007) indicated that this sample size would yield 95% power to detect a within-subject effect (i.e., Imagine Helping vs. Identify Source condition differences) of $d_z = 0.22$ in the focal imagined helping task. This sample size is also nearly twice that of Böckler, Tusche, Schmidt, et al. (2018) who conducted similar confirmatory factor analyses to those we planned to run here. This study was approved by the Institutional Review Board of the University at Albany, State University of New York, protocol number 17-E-011-01.

2.2 | Procedure and materials

All materials and data (including a codebook and analysis scripts) are available in full at https://osf.io/kurg6/?view_only=f9c02dbadcb5421c954b3a40aaaf46f6. After being greeted by an experimenter and completing the informed consent process, participants were given $3 cash and were asked to decide how much of this money to donate to a local foodbank and that any money not donated was theirs to keep. This decision was implemented in person with the experimenter and was included at the beginning of the study primarily to help convince participants that real money was at stake in the experiment, and we did not include this initial donation decision in subsequent analyses. Participants were then led to individual testing rooms with computers and white noise machines where they completed the remainder of the study online. Of the tasks described below, participants always completed the imagined helping and letter-writing tasks first due to these being the focal dependent measures (to avoid order effects or spillover from other measures of prosociality) and the volunteering task last, whereas the order of the remaining tasks (charitable donations, economic games, social value orientation, and self-report measures) was randomized for each participant (mean study duration = 81.29 mins, $SD = 28.42$).

2.2.1 | Imagined helping tasks (state prosociality)

Participants first completed a task taken from prior work on episodic simulation and prosocial intention (Gaesser et al., 2018; Gaesser, et al., 2017) that allowed us to replicate prior findings on the positive effects of imagination for prosocial intentions and behavior toward the specific targets of the experimental manipulation. In other words, this task measures differences in situational or *state* levels of prosociality arising from episodic simulation. Participants were told that they would be reading stories taken from the media describing people in need of help (e.g., "After returning from the beach, this person realized they're missing the watch their grandfather gave them") and that they would perform one of two tasks after reading each story: thinking of a plausible way they could actually help the person in need, then imagining this helping event with in much vivid detail as possible (Imagine Helping trials), or thinking about the type of media the story likely came from (e.g., NYTimes, local newspaper, Twitter, etc.) and what it was about the writing style or content that made them think of that particular source (Identify Source trials). The Identify Source trials were intended to serve as a control condition in which participants reflect on the content of the stories (similarly to the Imagine Helping trials) but in a way that involves neither imagination nor a specific helping event. Participants first completed two practice trials (one Imagine Helping and one Identify Source) with the experimenter in which they described what they imagined or the media source they identified. The experimenter provided feedback when necessary to ensure participants fully understood both tasks, then allowed participants to proceed on their own.

Each participant read six stories in total. After three of these stories, they were given the Imagine Helping instruction, and after the other three, they were given the Identify Source instruction. The order in which stories were presented and the assignment of stories to condition (i.e., whether the story was followed by an Imagine Helping or Identify Source instruction) were randomized while ensuring that each participant always completed three Imagine Helping and three Identify Source trials. Participants always imagined or attempted to identify the media source for a fixed period of 60 s before completing the tasks described below.

For each Imagine Helping trial, participants were first asked to provide a written description of the helping scene that they had imagined to serve as a manipulation and attention check. Following this, participants were asked two questions about the vividness of the scene they imagined ("The imagined scene in your mind was...," rated

from 1 = *vague* to 7 = *highly coherent and clear* and from 1 = *simple* to 7 = *highly detailed*), one question about their willingness to help the person in the story ("How likely would you be to help out in this situation," rated from 1 = *not at all* to 7 = *very willing*), and finally made a donation decision ("How much would you be willing to donate to the person in this story to help them out?", rated from $0 to $50 on a sliding scale). To heighten the realism of this donation choice, participants were told that some of these choices would be randomly selected and implemented by raffle (i.e., that participants would receive $50 minus whatever amount they had donated on the trial selected). On Identify Source trials participants completed these same measures (excluding the two vividness items, as they did not imagine a helping scene) and described the media source they had identified for the attention check instead of what they had imagined. Ratings were collapsed across the three trials within each condition, excluding those trials in which participants had failed to follow instructions (assessed via written responses of what they had imagined or the source they had identified; 7% of trials were eliminated for this reason). An imagined scene vividness composite was calculated comprising coherence and detail ratings for Imagine Helping trials (Spearman-Brown $r = .72$).

Participants then completed a supportive letter-writing task taken from prior work (Gaesser et al., 2020). Two stories (one from the Imagine Helping trials, one from the Identify Source trials) were selected at random to be presented a second time to participants. After re-reading these scenarios, participants were told, "It has been a few days since this happened. This person is still distressed and upset by this incident. In the space below, write a note to this person to offer them some support." We recorded the number of words written and amount of time spent writing for both of these supportive letters.

We calculated mean condition difference scores (Imagine Helping trials minus Identify Source trials) for the four key prosocial intention and behavior variables described above (self-reported willingness to help, donation amounts, word counts for supportive letters, time spent writing supportive letters).[2]

Control variables

We additionally measured three variables (task difficulty, task success, and tendencies toward socially desirable responding) that could have plausibly served as extraneous factors influencing responses in the focal imagined helping task to serve as statistical controls in subsequent analyses. Ratings of task difficulty ("How difficult was this task for you?", rated from 1 = *not at all difficult* to 7 = *extremely difficult*) and task success ("To what degree did you succeed in following the instructions to imagine the scenario?", rated from 1 = *did not succeed at all* to 7 = *completely succeeded*) were collected after each trial. Participants also completed the 10-item M-C 1(10), a short version of the Marlowe-Crowne Social Desirability Scale (Crowne & Marlowe, 1960; Strahan & Gerbasi, 1972), which assesses socially desirable responding by asking participants whether culturally sanctioned yet improbable personality descriptors are true or false for them personally (e.g. "I'm always willing to admit it when I make a mistake"; Cronbach's $\alpha = .60$).[3]

2.2.2 | Battery of prosociality measures (trait prosociality)

The following measures (which were completed in a randomized order, with the exception of the volunteering task which always came at the end of the study) were included to gauge participants' general tendencies toward prosocial and cooperative behavior (i.e., levels of *trait* prosociality) outside of the imagined helping task described above. We had several goals in mind during the selection of these measures. First, we wanted to include a broad variety of measures intended to capture individual differences in prosociality and cooperation from the literature on these topics (Böckler, Tusche, et al., 2018; Peysakhovich et al., 2014), including those utilizing different forms of measurement (e.g., allocation of resources in economic games, actual prosocial behavior in the form of donations or volunteering, self-reported concern for others, etc.) Second, as we intended to test the replicability of the three-factor model of prosociality presented in Böckler, Tusche, and Singer (2018); Böckler, Tusche, Schmidt, et al. (2018), we used many of the same measures included in this original research (to ensure that the three theoretical dimensions of interest were tapped) while also incorporating new measures which we expected to be theoretically related to specific components of this model. Specifically, we adapted a measure of volunteering behavior (Bartlett & DeSteno, 2006; Waytz et al., 2012) with the expectation that this would load onto a factor reflecting altruistically motivated prosocial behavior and a self-report measure of reciprocity (the Personal Norm of Reciprocity scale; Perugini et al., 2003) with the expectation that this would load onto a factor reflecting self-reported prosocial behavior. Thus, the measures included here reflect a representative sample of the many ways in which prosocial tendencies have been measured in prior research which allowed us to conceptually replicate and test the generalizability of one of the models of prosocial behavior suggested by prior research (Böckler et al., 2016; Böckler, Tusche, Schmidt, et al., 2018; Böckler, Tusche, & Singer, 2018) and subsequently, to determine whether people's scores on these factors would predict their responsivity to the imagined helping task described above.

Charitable donations

Participants completed eight trials of a task tapping into real-world tendencies toward charitable giving adapted from prior work (Böckler et al., 2016; Böckler, Tusche, Schmidt, et al., 2018; Böckler, Tusche, & Singer, 2018; Tusche et al., 2016). In each trial, participants read a brief description of a real-world domestic or international charitable organization and then were asked "How much would you like to donate to this organization?" using a scale from $0 to $50. As in the imagined helping task donations, participants were told some of these choices would be implemented by raffle. As internal consistency across trials was high (Cronbach's $\alpha = .94$), we calculated mean donation amounts across the eight trials for each participant, matching previous implementations of this task (Böckler et al., 2016; Böckler, Tusche, Schmidt, et al., 2018; Böckler, Tusche, & Singer, 2018).

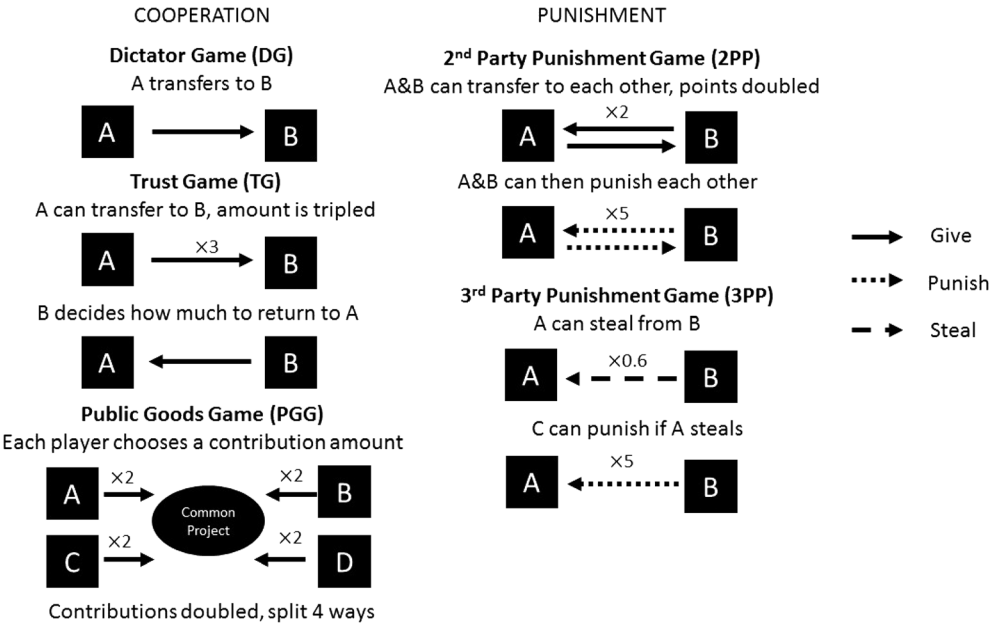

FIGURE 1 Adapted figure from Peysakhovich et al. (2014) showing the rules and payoff structure for the five economic games included

Economic games

Participants completed a series of five economic games used in prior work (Böckler et al., 2016; Böckler, Tusche, Schmidt, et al., 2018; Böckler, Tusche, & Singer, 2018; Peysakhovich et al., 2014) in a randomized order (see Figure 1). Participants were told that they would be deciding how to allocate points (to be converted to real money at the end of the experiment at a rate of 1 point = $0.01) between themselves and other ostensibly real players. Participants read through a set of rules before each game and answered multiple choice questions testing their understanding of those rules (performance on these questions was used as an attention check, as described above).

Dictator game. Participants completed one round of the dictator game (DG) (Camerer, 2003), in which they were given 100 points and decided how many points (between 0 and 50) they wanted to send to another anonymous player who began with 0 points.

Trust game. Participants completed two rounds of the trust game (TG) (Berg et al., 1995). In this game, both players start out with 50 points. The first player ("the trustor") chooses an amount of points between 0 and 50 to send to the second player. This amount of points is then tripled, and the second player ("the trustee") decides how many of these points they want to send back to the first player. Participants played one round as the trustor (choosing how many of their 50 points to send to the other player) and one round as the trustee (after being told that the trustee sent all 50 of their points to them [tripled to 150], participants chose an amount between 0 and 150 points to send back). In the confirmatory factor analyses described below, we used the raw "trustor" point values from this game ("TG_trustor") and the residuals from a linear regression predicting the "trustee" values from the "trustor" values ("TG_trustee," capturing reciprocal tendencies over and above the same baseline propensity toward charitability and sharing one's resources captured by the TG_trustor variable).[4]

Public goods game. Participants completed one round of the public goods game (PGG) (Ledyard, 1995). In this game, participants are matched with three other players, all of whom start with 100 points. Each player decides an amount between 0 and 100 points to contribute to a shared pool whose total amount is then doubled and split evenly among the four players.

Second-party punishment game. Participant then completed the second-party punishment game (2PPG) (Fehr & Fischbacher, 2004). In this game, each player starts out with 100 points, and decides whether they want to send 30 of their points to another player. Any points transferred at this stage are then doubled. Each player is then allowed to engage in costly punishment of the other player; players are allowed to remove up to 70 points from the other player but must give up 1 point of their own for every 5 points taken away. Participants made three decisions: whether or not they would choose to transfer 30 points to the other player, the number of points between 0 and 70 to remove from the other player if they behaved generously (i.e., sent all of their 30 points on the first move) and the number of points they would like to remove from the other player if they behaved selfishly (i.e., sent 0 points on their first move). Whereas all three of these measures were collected to replicate the way this game has been conducted in prior research (e.g., Peysakhovich et al., 2014), we were interested only in participants' costly punishment behaviors (i.e., the number of points they removed from the other player if they behaved selfishly) and following prior work (Böckler, Tusche, Schmidt, et al., 2018; Böckler, Tusche, & Singer, 2018; Peysakhovich et al., 2014), included only this measure in the CFA described below.

Third-party punishment game. Participants completed one round of the third-party punishment game (3PPG) (Fehr & Fischbacher, 2004). In this game, all players begin with 100 points. The participant witnesses an initial transfer between two other players (Person A and Person B)

in which Person A decides whether to take points from Person B or not. If Person A decides to take, they will receive 30 points and Person B will lose 50 points. The participant takes on the role of Person C, a third-party observer to this interaction who can decide to punish Person A for behaving unfairly. The participant is told that Person A has decided to take points from Person B, and then is asked how many points (from 0 to 100) they would like to remove from Person A at a cost of 1 of their own points per 5 points taken away. Participants were also asked if they would have taken points from Person B if they were Player A in this exchange. Observed measures of costly punishment of an unfair player on behalf of another person were included in the CFA.

2.2.3 | Social value orientation

Participants completed the nine-item social value orientation scale (SVO) (Van Lange, 1999). For each item of this scale, participants were shown three hypothetical distributions of points between themselves and another player and asked to select which one they preferred, reinforcing that the points had value and that "the more of them you accumulate, the better for you." Though the specific amounts of points varied on an item-to-item basis, the three distributions that participants were allowed to choose from always took the form of a prosocial choice (maximizing the other player's points), an individualistic choice (maximizing your own points), or a competitive choice (maximizing the difference in points between yourself and the other player). Following the approach of Böckler et al. (2016); Böckler, Tusche, and Singer (2018); Böckler, Tusche, Schmidt, et al., (2018), we calculated the number of prosocial decisions made across the nine trials for each participant.

2.2.4 | Self-report measures

Participants completed the following self-report measures in a randomized order.

Personal norm of reciprocity (PNR)

Participants completed the 27-item Personal Norm of Reciprocity questionnaire (PNR) (Perugini et al., 2003) which measures individual differences in reciprocity as an internalized social norm. The scale consists of three, nine-item subscales measuring beliefs in reciprocity (e.g., "If I work hard, I expect it will be repaid"), positive reciprocity (e.g., "If someone does a favor for me, I am ready to return it"), and negative reciprocity (e.g., "I am willing to invest time and effort to reciprocate an unfair action"), with all items rated on 7-point scales (from 1 = *strongly* disagree to 7 = *strongly* agree). This scale demonstrated good internal consistency (Cronbach's $\alpha = .78$) and we calculated mean PNR scores for each participant. We also report the results from analyses in which we treat the three components of the PNR mentioned above separately in the Supplemental Materials, however we note that this choice had no impact on our results. This measure was not included in prior research on the dimensionality of prosociality (Böckler et al., 2016; Böckler, Tusche, Schmidt, et al., 2018; Böckler, Tusche, & Singer, 2018) and we included it here to determine whether it would load onto the same factor as other self-report measures of prosociality.

Interpersonal reactivity index–empathic concern subscale

Participants completed the 7-item empathic concern subscale of the interpersonal reactivity index (Davis, 1983). This scale indexes the tendency to experience sympathy and concern for others' wellbeing (e.g., "I often have tender, concerned feelings for people less fortunate than me," rated from 1 = *does not describe me very well* to 5 = *describes me very well*). This scale demonstrated good internal consistency (Cronbach's $\alpha = .82$). After reverse-scoring appropriate items, mean Interpersonal reactivity index–empathic concern subscale (IRI-EC) scores were calculated for each participant.

Machiavellianism

Participants completed the 20-item MACH IV (Christie & Geis, 1970; Henning & Six, 1977), which indexes trait Machiavellianism (MACH)–a willingness to manipulate and deceive others in the service of one's own goals (e.g., "The best way to handle people is to tell them what they want to hear," rated from 1 = *strongly disagree* to 5 = *strongly agree*). This scale showed moderate internal consistency in the current sample (Cronbach's $\alpha = .68$).

2.2.5 | Volunteering of time

After completing the above tasks, we measured participants' willingness to volunteer their time to help a stranger (Bartlett & DeSteno, 2006; Waytz et al., 2012). This measure was not included in prior research on the dimensionality of prosociality (Böckler et al., 2016; Böckler, Tusche, Schmidt, et al., 2018; Böckler, Tusche, & Singer, 2018) and we included it here due to its conceptual relation to altruistically motivated prosocial behavior which was *actually costly* in terms of invested time and effort. Participants were told that they had finished the experiment, but that if they wanted, they could stay extra time to help a participant working in a partnered "logical thinking lab" by helping them finish their current study, which involved solving a series of difficult logic problems. Participants who stayed to help were given a set of 50 practice LSAT (U.S. Law School Admission Test) questions and instructed to work on them for as long as they pleased or until their study session was over. We recorded the amount of time in minutes participants worked on the LSAT problems, coding participants who decided not to stay as a "0." As participants varied in how long the rest of the study took them to complete, it is possible that fatigue effects or simply running out of time in the 2-h study session would influence the amount of time they spent volunteering. However, we found a nonsignificant correlation between time spent volunteering and time spent on the rest of the study, $r(278) = .07$, $p = .243$, arguing against this possibility (and as noted above, the average study duration including this volunteering task was only

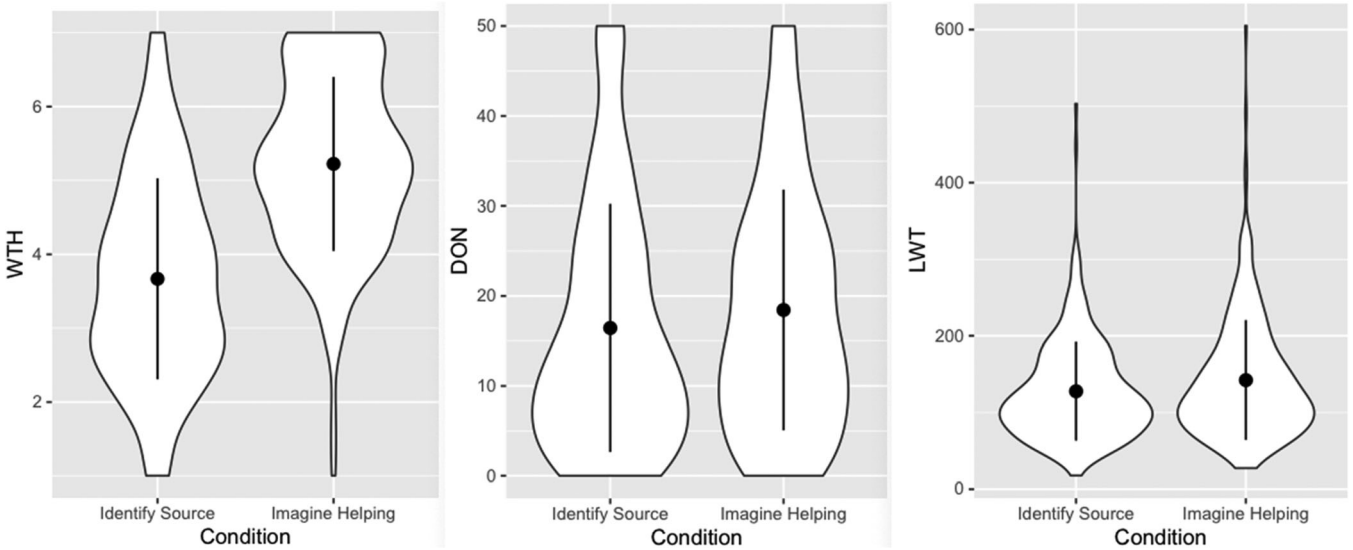

FIGURE 2 Mean condition differences (identify source vs. imagine helping trials) in willingness to help ("WTH," rated from 1 = not at all to 7 = very willing), charitable donations ("DON," $0–$50), and time spent writing a supportive letter ("LWT," in seconds). Means ± 1 *SD* are displayed as black dots and lines. Probability densities displayed here used a Gaussian kernel density estimator. We found a significant increase in each of these prosocial behaviors toward the target of the imagine helping manipulation ($p < .05$, false discovery rate corrected)

81.29 min, *SD* = 28.42, meaning that participants typically had ample time in which to volunteer).

3 | RESULTS

Descriptive statistics for all variables included in the study are provided in Tables S1 and S2 in the Supplemental Materials and correlations among all measures are located online at https://osf.io/m3q72/?view_only=f9c02dbadcb5421c954b3a40aaaf46f6.

3.1 | Imagined helping tasks

3.1.1 | Imagination increases prosocial intentions and behaviors

We first sought to replicate the effect of imagined helping on helping intention (self-reported willingness to help) and actual helping behaviors (monetary donations and supportive letters written to targets in need). Paired samples *t* tests were used to compare ratings between Imagine Helping and Identify Source trials (see Figure 2). For these tests, we report Cohen's d_Z effect sizes (Lakens, 2013; Rosenthal, 1991) with 95% CIs using the calculation from Hedges and Olkin (1985). Replicating past work, participants expressed greater willingness to help targets in need on Imagine Helping trials ($M = 5.22$, $SD = 1.18$) as compared to Identify Source trials ($M = 3.69$, $SD = 1.36$), $t(272) = 18.26$, $p < .001$, $d_Z = 1.10$, 95% CI (0.95, 1.26). Statistically significant differences between conditions were also found for prosocial behaviors directed at targets in need; participants donated more money on Imagine Helping trials ($M = \$19.29$, $SD = \$13.35$) as compared to Identify Source trials ($M = \$16.86$, $SD = \$13.90$), $t(217) = 2.64$, $p = .009$, $d_Z = 0.18$, 95% CI [0.05, 0.31], and spent a greater amount of time writing supportive letters to targets in need on Imagine Helping trials ($M = 142.36$ s, $SD = 78.25$ s) as compared to Identify Source trials ($M = 131.28$ s, $SD = 87.68$ s), $t(282) = 2.11$, $p = .036$, $d_Z = 0.13$, 95% CI [0.01, 0.24]. However, the difference in word count for supportive letters between conditions did not reach statistical significance (Imagine Helping $M = 52.30$, $SD = 26.00$; Identify Source $M = 51.35$, $SD = 25.45$, $t[282] = 0.80$, $p = .424$, $d_Z = 0.05$, 95% CI [−0.06, 0.16]); due to this nonsignificant difference, we did not further explore whether individual differences in prosociality predicted condition differences in this variable. All of the aforementioned patterns of statistical significance held when including the full sample of participants (i.e., prior to performing the data exclusions described above) in these analyses as well as when controlling for task difficulty, task success, and participant gender in repeated-measures Analyses of Covariance (ANCOVAs; full results of these analyses are reported in the Supplemental Materials). All statistically significant condition differences reported above survive correction for multiple comparisons at $p < .05$ (false discovery rate corrected, Benjamini & Hochberg, 1995). Together, these results replicate prior findings that episodic simulation heightens prosocial intentions and, to a weaker degree, behavior (Gaesser et al., 2018, 2020; Gaesser & Schacter, 2014). However, it is worth noting that the nonsignificant effect of episodic simulation on word count in the letter-writing task diverges from what was found in prior work using this same measure (Gaesser et al., 2020), necessitating further research to determine the replicability and size of this effect across contexts.

3.1.2 | Imagined scene vividness predicts prosocial intentions and behaviors

We proceeded by examining the relationship between self-reported vividness (i.e., coherence and detail) of participants imagined helping episodes and prosocial intention and behavior (for Imagine Helping trials only, as these measures were not collected on Identify Source trials) as vividness has been found to be an important mediator of the effect of episodic simulation on helping in prior work (Gaesser et al., 2018; Gaesser & Schacter, 2014). Replicating this prior work, vividness (collapsed across Imagine trials) had a significant positive relationship with willingness to help, $r(271) = .56$, $p < .001$, the amount of time spent writing a supportive letter to a person in need, $r(271) = .15$, $p = .011$, and word count for these supportive letters, $r(271) = .28$, $p < .001$. However, vividness did not significantly predict amount of money donated, $r(238) = .06$, $p = .338$.

3.2 | Relationships between imagined helping effects and prosocial personality

3.2.1 | CFA on battery of prosocial measures

Having established that episodic simulation in the imagined helping tasks increases helping intention and behavior toward the specific targets of the experimental manipulation, we proceeded to the key question of the study: does the effectiveness of imagined helping depend on an individual's overall tendency to behave prosocially? In other words, do increases in state levels of prosociality resulting from episodic simulation depend upon trait levels of prosociality (i.e., a state–trait interaction)? To characterize people's more general prosocial tendencies we integrated 12 widely used measures (see Battery of Prosociality Measures) according to a previously identified and confirmed three-factor structure of human prosociality (Böckler et al., 2016; Böckler, Tusche, Schmidt, et al., 2018; Böckler, Tusche, & Singer, 2018) (see Figure 3). Extending prior work, we added two measures (Personal Norm of Reciprocity scale, measure of actual time spent volunteering) that are conceptually related to the proposed factors of altruistically motivated, norm motivated, and self-reported prosocial behavior.

We first aimed to validate the proposed factor structure by assessing the model fit of all available data of our subject sample. Model estimation was performed using a full information maximum likelihood (FIML) approach based on unstandardized raw data. Levels of missing data were low (7.0% on average). Model fit was assessed using multiple Indices: The Tucker-Lewis Index (TLI = 0.94) and the comparative fit index (CFI = 0.96) with values greater than 0.90 and 0.95 indicating adequate and good fit respectively (Hu & Bentler, 1999; West et al., 2012), the root mean square error of approximation (RMSEA = 0.03) with values lower than 0.07 indicating good fit (Steiger, 2007; West et al., 2012), and the Chi-Square value (chi-square = 59.04, $p = .154$) as a traditional measure for evaluating overall model fit provided an insignificant result at a 0.05 threshold (Barrett, 2007). In sum, results of the CFA showed adequate fit to our data, supporting the validity of the proposed structure of human prosociality,[5] although it is worth noting that the newer measures included here but not featured in prior research (the measure of volunteering behavior and the PNR) had the weakest factor loadings of the included measures (see Figure 3).

We then calculated weighted factor scores for each of these three factors for use in subsequent analyses, following previous implementations (Böckler et al., 2016) (below, we refer to these factor scores as "Altruism," "Norm," and "Self-Report"). Scores on the Self-Report factor were reversed in sign so that higher scores would represent greater levels of prosociality (i.e., higher empathic concern and lower Machiavellianism).

3.2.2 | Altruistically motivated prosociality and imagined helping

Altruism scores were significantly negatively correlated with the efficacy of the Imagined Helping manipulation as captured in condition differences in willingness to help, $r(168) = -.19$, $p = .015$, and charitable donations, $r(140) = -.20$, $p = .016$. These results indicate that individuals who demonstrated relatively lower tendencies toward altruistically motivated prosociality showed greater increases in willingness to help and charitable donations as a result of imagining themselves helping (see Figure 4). However, no significant association was found between Altruism scores and time spent writing a supportive letter, $r(174) = -.03$, $p = .662$. This negative link between the efficacy of the Imagined Helping manipulation on willingness to help and donations with altruism scores survives correction for multiple comparison at $p < .05$ (false discovery rate corrected, Benjamini & Hochberg, 1995).

3.2.3 | Norm-motivated and self-reported prosociality and imagined helping

Nonsignificant associations were found between Norm scores and condition differences in willingness to help, $r(163) = .01$, $p = .941$, charitable donations, $r(142) = -.16$, $p = .053$, and time spent writing a supportive letter, $r(169) = .00$, $p = .993$. Similarly, nonsignificant associations were found between self-report scores and condition differences in willingness to help, $r(270) = .04$, $p = .479$, charitable donations, $r(215) = -.07$, $p = .294$, and time spent writing a supportive letter, $r(280) = .02$, $p = .745$.

3.2.4 | Relationships between altruism, norm, and self-report and imagined helping controlling for imagined scene vividness

Due to the importance of scene vividness in mediating the effects of episodic simulation of helping events on subsequent intentions and

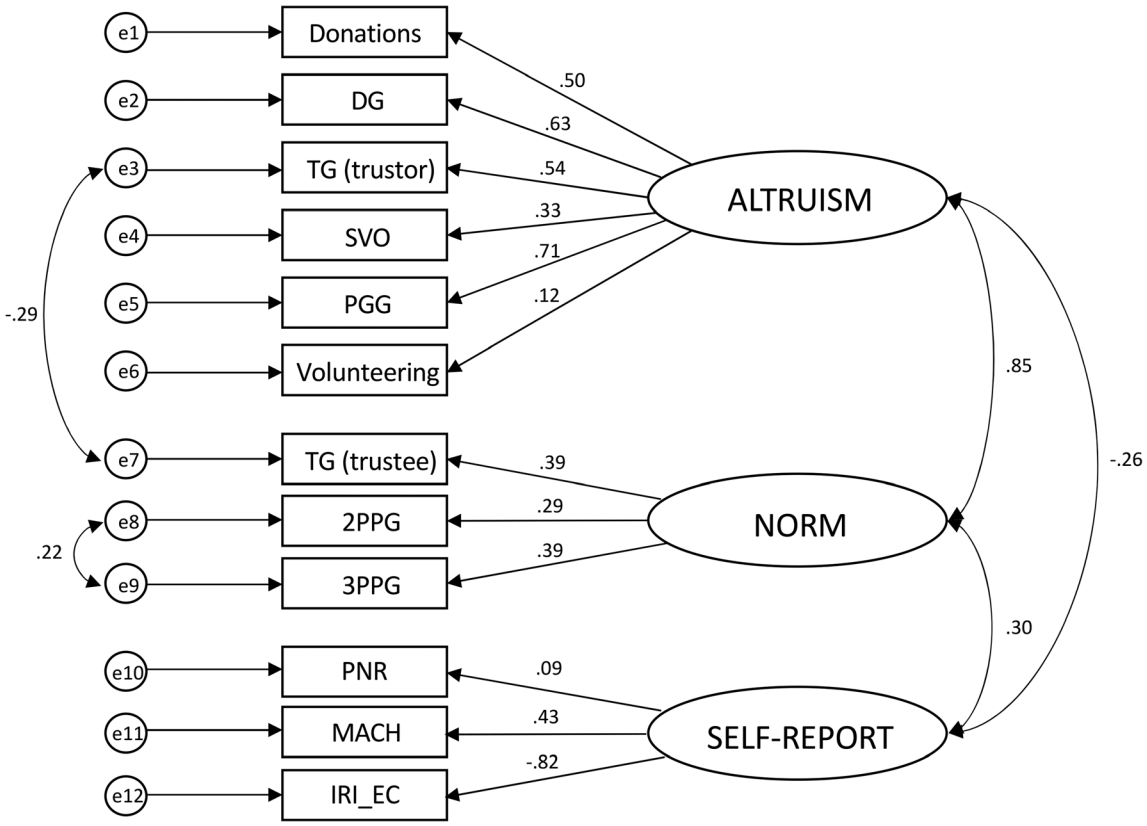

FIGURE 3 Path model for the three-factor model of human prosociality (Böckler, Tusche, Schmidt, et al., 2018; Böckler, Tusche, & Singer, 2018) tested using CFA in the current sample. DG, dictator game; IRI_EC, interpersonal reactivity index, empathic concern subscale; MACH, Machiavellianism; PGG, public goods game; PNR, personal norm of reciprocity; SVO, social value orientation; TG, trust game; 2PPG, 2nd party punishment game; 3PPG, 3rd party punishment game

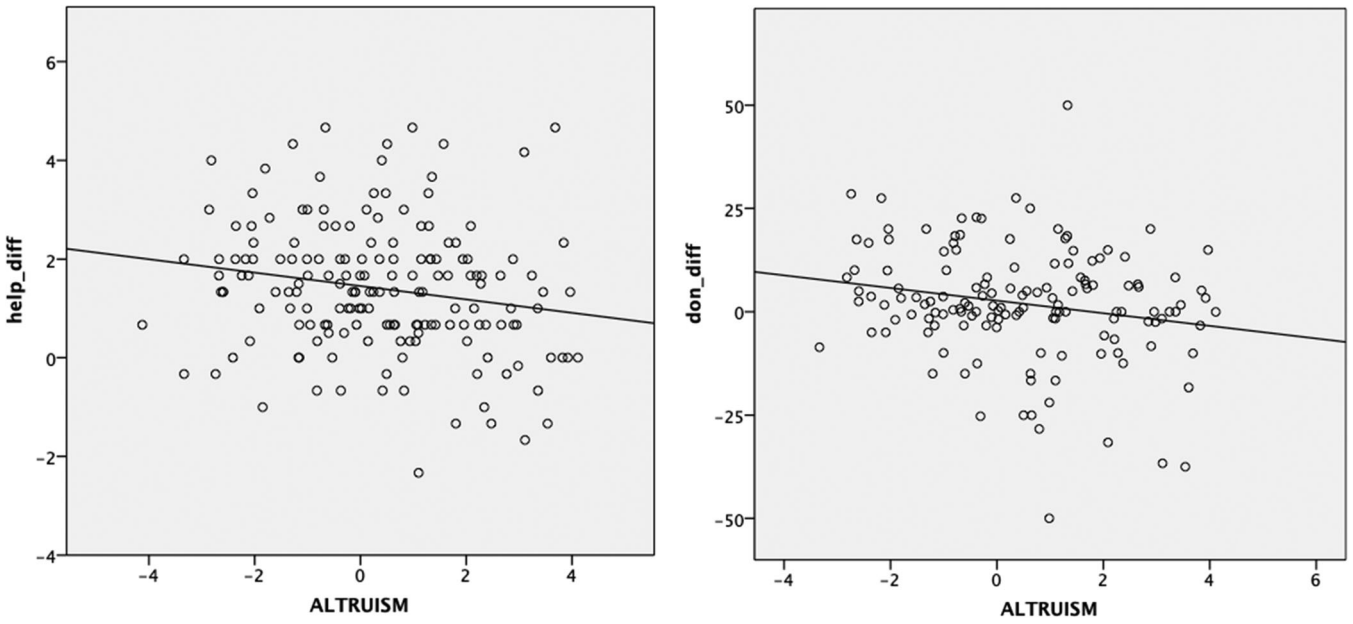

FIGURE 4 Scatterplots depicting the relationships between people's more general propensity to behave altruistically (altruism factor scores, altruism) and the state-related effects of episodic simulation (identify helping minus identify source) on the willingness to help (help_diff) and charitable donations (don_diff) to the specific targets of the imagined helping task

behavior (Gaesser et al., 2018; Gaesser & Schacter, 2014) we also explored how individual differences in prosocial behavior and relationships between these individual differences and condition differences in the imagined helping task were related to participants' self-reported vividness of imagined helping scenes. We first determined whether scores on the three factors of prosociality described above were correlated with ratings of imagined scene vividness. Self-Report scores were significantly positively correlated with vividness, $r(270) = .21$, $p < .001$. Nonsignificant correlations were found between Altruism scores and vividness, $r(168) = .14$, $p = .066$, as well as between Norm scores and vividness, $r(163) = .01$, $p = .945$. Thus, the vividness with which people imagined helping episodes did not significantly differ as a function of individuals' altruistically motivated or norm motivated prosocial tendencies. Further, after controlling for individual differences in imagined scene vividness, the pattern of significant correlations between factor scores and condition differences in the imagined helping task reported above remained the same (see Supplemental Materials). This suggests that, whereas vividness may play an important role in mediating the link between imagination and state-like measures of prosociality (in specific contexts toward specific targets), the greater efficacy of imagined helping witnessed among individuals with a lower tendency to behave altruistically (altruism factor scores) were not due to these individuals imagining helping scenes in more vivid detail.

4 | DISCUSSION

Whereas past research demonstrated that imagining helping others increases prosocial intentions and behaviors directed at people in need, it was previously unknown whether this relationship was particularly strong among those who already display a cross-situational tendency to help or operated independently of baseline propensities toward prosociality. Here, we found evidence for a state–trait interaction: effects of imagined helping on prosocial intentions and behavior were modulated by individual differences in distinct forms of prosociality. The effects of imagination on willingness to help were somewhat stronger for individuals who displayed lower levels of altruistically motivated prosocial behavior across other measures. Additionally, the positive effect of imagination on charitable donations to targets in need was only witnessed within this subgroup; individuals scoring higher in altruistically motivated prosocial behavior showed negligible condition differences on this variable. Individual differences in prosociality as captured by norm-motivated prosociality and self-report measures, however, did not predict the extent to which imagined helping influenced prosocial intentions or behavior. Thus, the different measures of prosocial behavior included in our study seemed to capture distinct dimensions of prosociality as suggested in prior work (Böckler, Tusche, Schmidt, et al., 2018; Böckler, Tusche, & Singer, 2018) and individual differences on these dimensions in turn predicted how effective episodic simulation was in fostering greater levels of prosociality among less prosocial individuals.

What is less clear based on our findings alone is why this state–trait interaction was found only for altruistically motivated (but not norm-motivated or self-reported) prosocial behavior. It could be the case, for example, that high-altruism individuals are more prone to the spontaneous generation of mental imagery when hearing about individuals in need. If so, one might expect to find smaller effects of a specific "imagined helping" instruction for these individuals as compared to low-altruism individuals who may be less likely to generate such imagery on their own. If this role of imagination was specific to altruistically motivated prosociality, we might expect to see a pattern of results very similar to that found here. On the other hand, it might be that imagination simply helps compensate for a lack of other traits unrelated to episodic simulation but which nonetheless contribute to the prosociality of high-altruism individuals. This, however, raises further questions about why similar compensatory effects were not witnessed for individuals lower in norm-motivated and self-reported prosocial behavior. One possibility is that episodic simulation may be particularly effective for altering altruistically motivated behaviors (compared to norm-motivated and self-reported prosocial tendencies), explaining the specific link with people's altruism factors scores. Future research could examine the effects of imagined helping on other prosocial measures (e.g., targeting norm-motivated social behaviors) to further explore this notion and the specificity of the identified state–trait measures of human prosociality. Ultimately, further research will be needed to test these competing explanations and better understand the contribution of imagination to prosociality in naturalistic settings where no specific instructions to imagine helping others are given.

Our findings contribute to a growing body of work causally linking imagination and memory to prosocial behavior (Gaesser et al., 2018, 2019, 2020; Gaesser & Schacter, 2014; Sawczak et al., 2019). Replicating this prior work, we found that people who briefly imagined themselves helping strangers in need expressed a greater willingness to help those individuals afterward, and albeit with smaller effect sizes, actually did devote more effort and resources to helping those individuals (in the form of monetary donations and supportive letters written to the individuals in need). The weaker effects of this manipulation on behaviors as compared to intentions is consistent with prior work (Gaesser et al., 2018, 2019) and broadly aligns with work showing that a number of factors beyond intentions (e.g., beliefs, perceived control) predict actual behavior (Ajzen, 1985; Sheeran & Webb, 2016). This difference may also be driven by a lack of correspondence between the type of help participants imagined and the behaviors actually measured. For example, reading that, "This person lost their voice the day they are supposed to run a charity auction," participants would likely imagine themselves filling in for the person or helping them find someone else to run the charity auction for them as opposed to the more indirect forms of helping we measured (monetary donations and supportive letters). It is thus possible that our study underestimates the true relationship between imagining and actually helping others in need, a question that future research could address by heightening the similarity between imagined and measured prosocial behaviors.

Our results are also informative with regard to research on the factors underlying shared variance across distinct measures of prosociality. We tested and found support for the three-factor model of human prosociality proposed by Böckler, Tusche, and Singer (2018); Böckler, Tusche, Schmidt, et al., (2018) that includes three forms of prosociality with distinct motivational underpinnings: altruistically motivated, norm motivated, and self-reported prosocial behaviors. This is notable particularly due to the variations between the specific array of measures utilized here and those used in prior work, suggesting that this model is robust to modifications of specific measures and in fact captures the latent variables of interest. These results suggest the following with regard to various measures of prosocial behavior: some measures of helping appear to capture the basic tendency to share one's resources with and place trust in others given no prior knowledge about them (e.g., giving more resources to a stranger without any expectation of reciprocity), others capture more specific concerns surrounding fairness and reciprocity (e.g., giving more resources back to generous others, punishing others who have behaved unfairly), whereas others pick up primarily on what people say about their own levels of concern (or lack thereof) for others. Whereas these tendencies are positively correlated (e.g., more altruistic individuals also tend to be more reciprocal and report being more prosocial), these correlations are moderate in strength and individuals nonetheless appear to systematically differ on these various dimensions to some degree. Researchers studying prosocial behavior should be mindful of these distinctions when selecting the measures they use, as our findings challenge the assumption that all purported measures of prosocial behavior in fact capture the exact same construct.

Our results point to episodic simulation as a promising tool for increasing actual prosocial behavior and helping in the real-world particularly among those with a lower baseline propensity for helping. Future research could focus on ways to implement the kind of intervention used here outside of the laboratory while maximizing the correspondence between imagined and desired helping behaviors. For example, inducements to imagine oneself helping at a specific time and place could be embedded in materials (e.g., video clips, flyers, emails) soliciting charitable donations or volunteering, and this method could potentially be extended to increase other types of normatively desirable behaviors (e.g., voting, health behaviors, environmentally conscious behaviors). Such interventions might be especially potent if coupled with other factors believed to strengthen the link between intentions and behavior such as perceived control over the behavior (e.g., by providing a clear and accessible means of enacting the behavior; Ajzen, 1985) and perceptions of social norms surrounding the behavior (e.g., by reinforcing that the behavior is common; Ajzen & Fishbein, Azjen & Fishbein, 1980). Additionally, we emphasize that the particular model of prosociality tested here, while demonstrating good fit to the observed data, is but one among several models suggested in the recent literature on this topic (e.g., Hubbard et al., 2016; Peysakhovich et al., 2014). Our primary focus here was on exploring the relationship between episodic simulation and prosociality (rather than engaging in an extensive comparison of different models of prosociality) and ultimately, further research will be needed to settle questions of whether one or several motivations drive individual differences in prosociality and how these differences fit into broader models of personality (Thielmann et al., 2020).

We note several limitations in the current study. First, our results raise potential concerns regarding the validity of the letter-writing task used during the imagined helping manipulation. Following prior work (Gaesser et al., 2020), we took word counts and amount of time spent writing these letters as measures of prosocial effort to assist targets in need. However, the small and/or nonsignificant condition differences found for this measure as compared to actual donations during this task (and the fact that word count and letter-writing time failed to predict other well-established measures of prosociality such as the SVO or DG) raise reasonable doubts about whether this measure in fact captures prosocial behavior as intended. Ultimately, further research will be needed to probe the validity of the letter-writing task as a measure of prosociality and researchers may wish to consider alternate metrics (such as the monetary donations also measured here) or other potentially more valid ways of scoring this task (e.g., coding the actual content of letters along dimensions related to prosociality; Masten et al., 2011). Second, ethical concerns may be raised about the use of deception in our study. Participants in our study were led to believe that they were playing economic games with other real participants and that their decisions in these games would actually affect their bonus compensation amounts, while in reality all participants made their decisions in isolation and received an equivalent compensation amount ($4) at the close of the study. This methodology was chosen to heighten the validity of responses during these games (Bonetti, 1998) and ensure that all participants would receive equal compensation for their time and effort. Nonetheless, we acknowledge that non-deceptive alternatives (e.g., implementing these economic games with real players and real behavior-contingent incentives) could have been used in this case. Finally, we acknowledge that assessing many conceptually related measures within a single lab session as we did here raise potential concerns about consistent responding in our data (Meade & Craig, 2012). However, we note that the data quality and attention checks performed here and the fact that we did not find support for a single underlying factor driving scores across all measures (i.e., the result one might expect based on systematic issues with consistent responding) make this possibility less likely. Nonetheless, future research might examine the effect of multiple sessions to collect prosocial measures (including those related to the effect of episodic simulation and those aiming to capture more general tendencies to help others).

Prior research in this area (Gaesser et al., 2018, 2019, 2020; Gaesser & Schacter, 2014) has emphasized that episodic inputs to prosociality should be considered alongside other well-known factors crucial to helping such as perspective-taking (Batson et al., 1989), group boundaries of moral concern (Balliet et al., 2014; Crimston et al., 2016), and affect (Carlson et al., 1988; Contreras-Huertas et al., Contreras-Huerta et al., 2020; Gaesser, et al., 2017). Our findings lend further support to such arguments by demonstrating that intuitive tendencies toward helping and cooperation may not be equally

distributed in the population and further, that the contribution of episodic processes to prosocial behavior varies to some degree with these tendencies. The present findings suggest that those seeking to increase prosocial behavior or establish comprehensive models of such behaviors should not neglect the role of imagination in driving helping and cooperation.

CONFLICT OF INTEREST

The authors have no conflicts of interest to declare.

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ENDNOTES

[1] See Wilhelm et al., 2018 for a critique of an earlier four-factor model of human prosociality proposed by Böckler et al., 2016; and Böckler et al., 2018a for the revised three-factor model adopted here to capture variance in peoples more general tendency to act altruistically.

[2] Some have advised against the use of difference scores due to their purported low reliability (e.g., Johns, 1981; Peter et al., 1993). However, the reliability of difference scores is known to vary according to multiple factors and there is good reason to believe our use of difference scores in this case did not pose major issues. Specifically, the fact that variance in our difference scores was comparable to that of the component measures which comprised them (see Table S2 in the Supplemental Materials) and that correlations between the component measures were medium in size (*rs* ranged from .41 to .50) make it less likely that our difference scores suffered from inadequate reliability (Chiou & Spreng, 1996; Rogosa et al., 1982; Rogosa & Willett, 1983; Trafimow, 2015).

[3] During the review process, our attention was brought to recent evidence that social desirability scales such as this are lacking in construct validity (e.g., Connelly & Chang, 2016; de Vries et al., 2014; Uziel, 2010) and measure aspects of personality which are (at least partially) unrelated to social desirability as it is typically construed. Due to these findings, in combination with the low level of internal consistency this measure yielded in our study, we made the decision to exclude this variable from subsequent analyses.

[4] An alternative CFA using the raw TG_trustee scores (rather than residuals from a linear regression) yielded comparable model fit statistics (CFI = .95, TLI = .92, RMSEA = .03) to the model we present in the Results section. However, this model also yielded standardized regression weights above one, pointing to improper solutions (Heywood cases) and precluding reliable interpretation (Wilhelm et al., 2018). We additionally tested an alternate CFA with TG_trustee scores controlling for decisions in the Dictator Game (rather than as trustor during the Trust Game), as one might argue that the DG is a cleaner measure of sharing and charitability than are TG_trustor scores. However, this model yielded substantially worse fit to our data as compared to the model currently presented in our results section below (CFI = .91, TLI = .86, RMSEA = .04) and similarly to the model using raw TG_trustee scores reported above, produced standardized regression weights above one.

[5] As some research has found support for a unidimensional factor structure underlying performance across distinct measures of prosociality (e.g., a "cooperative phenotype" or "general benevolence dimension"; Hubbard et al., 2016; Peysakhovich et al., 2014), we conducted an alternative CFA in which all measures of trait prosociality from our study were placed onto a single latent factor. However, the fit of this model to our data was substantially worse (CFI = .82, TLI = .73, RMSEA = .06) than that of Böckler et al. (2018a, b) which we focus on here.

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