Building Empathy through Motivation-Based Interventions

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Abstract

Empathy is associated with adaptive social and emotional outcomes; as such, a crucial outstanding question is whether it can be bolstered in ways that make practical differences in people's lives. Most empathy-building efforts address one's *ability* to empathize, increasing empathy by training skills like perspective taking. However, empathy is more than the ability to share and understand others' feelings; it also reflects underlying *motives* that drive people to experience or avoid it. As such, another strategy for increasing empathy could focus on shifting relevant motives. Here we explored this idea, leveraging two intervention techniques (mindsets and social norms) to increase motivation to empathize. Two hundred ninety-two first-year college students were randomly assigned to one of three intervention conditions —malleable mindset, social norms, or a combination of the two—or a control condition. Eight weeks later, participants in the intervention conditions endorsed stronger beliefs about empathy's malleability and exhibited greater empathic accuracy when rating others' positive emotions as compared to the control condition. They also reported having made a greater number of friends since starting college. The intervention did not affect outcomes related to intergroup processes or empathic accuracy when rating others' negative emotions, indicating a boundary condition for these interventions. This experiment underscores the potential of motivation-based empathy interventions to generate positive, real-world impact.

Empathy—the ability to share and understand others' thoughts and feelings— is vital to social functioning. It drives prosocial behavior (Batson, 2011; Batson et al., 1988), promotes greater relationship satisfaction (Sened et al., 2017), and tracks the number of friends a person has (Kardos et al., 2017). As such, a central question is whether empathy can be increased through targeted interventions, and whether such efforts would also improve social functioning.

Previous research from two literatures suggests that it is possible to "train" empathy. First, evidence from lab-based experimental manipulations demonstrates that empathy can be increased, at least in the short term (Klein & Hodges, 2001). Asking one person (a perceiver) to take the perspective of someone else (a target) often leads to greater empathy for targets (Coke et al., 1978). Such techniques even generate a number of prosocial outcomes, like motivating people to help stigmatized individuals and outgroup members (Batson et al., 2002; Todd & Galinsky, 2014). However, most experiments in this body of research do not examine the persistence of such effects (Paluck & Green, 2009), often measuring changes over the course of minutes or hours. It is therefore unclear whether these techniques impart lasting changes on empathy and related behavior.

A second, smaller literature of applied research demonstrates that targeted psychological interventions can generate longer-term changes in empathic behavior. According to a recent meta-analysis, empathy interventions reliably lead to improvement in socio-emotional skills, such as recognizing and responding to others' emotions (Teding van Berkhout & Malouff, 2016). However, the effects of these interventions are often context-specific and fail to generalize to novel situations or real-world social outcomes.

For example, an emotion recognition intervention for adults with autism improved participants' ability to identify emotions in facial expressions within a stimulus set. However, it did not affect their ability to identify emotions on faces that were not part of the stimulus set used in training, or their ability to interpret characters' emotions in video clips (Golan & Baron-Cohen, 2006). Similarly, an intervention to increase doctors' expressions of empathy toward cancer patients improved communication when evaluated by trained coders. However, the intervention did not affect the physician-patient relationship as assessed by patients themselves (Epstein et al., 2015). In other cases, interventions that produce long-term change are time- or resource-intensive, for instance requiring months of daily practice (e.g.,Valk et al., 2017)—limiting their scalability.

Recently, we proposed that some of the limitations characterizing empathy-building approaches relates to the theoretical assumptions undergirding much of this work (Weisz & Zaki, 2017b, 2018). Specifically, much of the psychological literature holds that empathy is a relatively *automatic process*, an emotional reflex that is triggered when people have an adequate understanding of others. This model implies that empathy will occur when someone encounters others' emotions, proportional to factors such as (i) the observer's empathic capacity (for instance their ability to take a target's perspective), and (ii) the number of empathic "triggers" present in the situation (such as vivid depictions of others' experiences or target-observer similarity). Existing interventions often reflect these assumptions. They employ techniques like coaching participants to explicitly consider others' perspectives to build empathic capacity, or showing participants emotional media to increase the amount of empathic triggers (Davis & Begovic, 2014).

Critically, these techniques do not improve empathy uniformly. Perspective taking in competitive contexts paradoxically increases selfish and unethical behavior (Epley et al., 2006; Pierce et al., 2013). Similarly, details of a competitor's misfortunes elicit pernicious counter-empathic emotions like *schadenfreude* (Cikara et al., 2014; Lanzetta & Englis, 1989) instead of empathy, illustrating the context-dependence of empathy and efforts to increase it (Preston & de Waal, 2002; Vorauer, 2013).

How can empathy emerge automatically in some circumstances, but fail to manifest in others? We propose that empathy's context sensitivity reflects shifts in social and emotional motives across different situations (Keysers & Gazzola, 2014; Zaki, 2014). Like many psychological phenomena, empathy is often a *motivated process*, reflecting an interplay of forces that push people toward or away from it. *Approach motives* increase perceivers' willingness to empathize. These include the desire to vicariously experience a target's positive emotions (Morelli et al., 2015), to feel closer to a target (Pickett et al., 2004), or to behave in a socially desirable manner (Klein & Hodges, 2001; Thomas & Maio, 2008). Conversely, *avoidance motives* decrease perceivers' willingness to empathize. These include the desire to avoid experiencing a target's pain vicariously (Pancer, 1988), to avoid material costs associated with helping a target (Shaw et al., 1994), to avoid cognitive and emotional fatigue (Cameron et al., 2019), and to avoid interference during competitive interactions (Galinsky et al., 2008).

Such evidence suggests that empathy-related motives are important determinants of social connection. Although motives are not a primary focus of existing empathy interventions, research from other domains suggests that intervening over motives at critical junctures elicits lasting changes in beliefs and behavior (Walton, 2014; Yeager &

Walton, 2011). In particular, brief psychological interventions that aim to influence *mindsets* and *social norms* affect important outcomes like academic performance (Blackwell et al., 2007), feelings of belongingness (Walton & Cohen, 2011) and willingness to compromise for peace during conflict (Halperin et al., 2011).

Grounded in this psychological tradition, we developed a new class of empathy interventions to encourage empathy by specifically addressing empathic motives, leveraging mindsets and social norms. Although there are many ways to intervene over motives related to empathy, we chose to target mindsets and social norms for two reasons. First, they reliably affect beliefs and behavior across different domains (Cialdini, 2003; Dweck, 2012; Lewin, 1952). Second and more importantly, recent findings suggest that they are directly related to changing empathy in the short-term. Mindset interventions function by changing one's beliefs about an attribute (in this case, empathy). The mindset intervention aimed to address the belief that empathy is a stable trait, replacing it with the belief that empathy can grow over time with effort. Because empathy is often seen as a desirable attribute, and because people differ in their empathy mindsets, people are generally interested in increasing empathy when they learn that empathy is malleable (Schumann et al., 2014, Pilot Study and Study 7). As such, previous research demonstrates that shifting empathy mindsets affect willingness to empathize with others. Those with growth mindsets of empathy (who think empathy is malleable) try harder to empathize than those with fixed mindsets of empathy (who think that empathy is relatively stable) when empathy feels challenging, for instance, when interacting with new people or those who seem different from oneself (Schumann et al., 2014).

Similarly, previous research suggests that normative influence can promote prosociality. Norms-based interventions work by conveying information about the accepted behaviors within a group or community to which one is expected to conform (Lewin, 1952; Schultz et al., 2007). Such interventions are especially effective when injunctive norms (what people ought to do) are aligned with descriptive norms (what people actually do, Cialdini, 2003).When people believe that others around them are empathic and prosocial, they are more empathic and prosocial themselves (Nook et al., 2016; Tarrant et al., 2009). We expected this norms intervention could be particularly potent among first-year college students, as they are new to the community and therefore do not hold strong pre-existing views about local norms.

By changing people's desire to connect with others, motive-based empathy interventions have the potential to impact a broad range of social encounters. Applying techniques from brief psychological interventions within a framework of motivated empathy could therefore create longer-term, more generalizable changes in empathy than existing skills-based interventions. Specifically, mindsets and social norms each show unique potential to motivate such lasting changes, and combining these two strategies may have an even greater impact on motivation to empathize. We tested these ideas in the present study by creating three novel interventions intended to strengthen empathic motives, and administered them to participants in their freshman year of college.

Methods

Participants

Previous work indicates that brief interventions are most influential when administered during critical temporal junctures, like the start of an academic year or

before transitioning to a new school (Yeager & Walton, 2011). We therefore recruited 292 college freshmen at Stanford University during their first two academic quarters. The start of college provides students with an enormous expansion in the breadth of their social network. This transition period often includes novel "empathic challenges" (e.g., meeting people from different backgrounds for the first time) at places like Stanford University, which is ranked as one of the top five most diverse national universities in the United States (*Campus Ethnic Diversity*, 2017). We estimated that an empathy intervention would exert medium effects on empathic accuracy, social integration, and empathic effort based on related experiments (Aronson et al., 2002; Nook et al., 2016; Schumann et al., 2014). Power analyses revealed that, in order to detect effects of this size with 80% power, a minimum sample of 72 participants per group would be required. Recruitment occurred over two academic years so as to enroll a sufficient number of participants to power statistical analyses, as determined by an a priori power analysis (see **Supplemental Material**). Participants were randomly assigned to one of four conditions: a malleable mindset condition, a social norms condition, a combined condition, or a control condition (n = 73 per condition). In each condition, participants completed three in-lab intervention sessions and an online follow-up session eight weeks later. Thirteen participants dropped out before completing all three intervention sessions. The remaining 279 participants (95.55% of enrolled participants) completed all three intervention sessions. Of those, 233 participants (69 male, 157 female, 7 not disclosed) returned for the follow-up session eight weeks later (64 in the malleable mindset condition, 61 in the social norms condition, 55 in the combined condition, and 53 in the control condition),

reflecting a retention rate of 79.79% of our enrolled participants. Participants were paid or given course credit for their involvement.

Participants' average age was 18.4 years (*SD* = .53). 0.43% identified as American Indian, 21.46% as East Asian, 0.86% as Pacific Islander, 9.87% as Black or African American, 32.19% as Caucasian, 13.3% as Hispanic or Latino, 2.58% as South Asian, 0.86% as Middle Eastern, 0.86% as Other, 14.59% as Mixed, and 3% not disclosed. Procedures were approved by the Stanford University Institutional Review Board.

Intervention sessions

Each condition consisted of three in-lab intervention sessions, which were each approximately 1 hour in duration. Participants completed all three intervention sessions in a 10-day window (except for two participants who completed the sessions in 12 and 20 days due to unanticipated scheduling difficulties). Modeled after work by Aronson and colleagues (Aronson et al., 2002), participants were told that they would complete tasks for a few different studies all funded by the same research grant. This cover story was used to reduce the possibility that intervention outcomes reflected demand characteristics. They were then introduced to the "Scholastic Pen Pals Program", purportedly the first of multiple tasks they'd complete. As part of their involvement with the Scholastic Pen Pals Program, participants would engage in a one-time letter exchange with a struggling high school freshman. The true purpose of the letter exchange was to affect participants' own beliefs and motivation through experimental manipulations embedded in their writing experience. This "saying is believing" framework is an effective tool for changing beliefs

and motives; by endorsing a particular set of beliefs, study participants begin to internalize those beliefs themselves (Echterhoff et al., 2009).

The four conditions were structurally similar, but differed in content and specific instructions:

Mindset condition. This intervention mirrored the format of previous lab studies and interventions teaching growth mindsets of other attributes, including intelligence (Blackwell et al., 2007) and personality (Yeager et al., 2013). However, rather than addressing lay theories of intelligence or personality, this intervention specifically targeted participants' lay theories of empathy. During the first session, participants in the mindset condition read a letter ostensibly written by a high school freshman having difficulty adjusting socially to their new school (see **Supplemental Material**). Before responding to the letter, participants read a passage describing the malleable nature of empathy, and were told that imparting this message to younger adolescents can help them overcome social difficulties. To bolster this idea, participants read a summary of research suggesting that empathy can be developed with effort, as well as a popular press article purportedly published in a psychology journal (from Schumann et al., 2014, see **Supplemental Material**).

In the second session, participants returned to the lab and wrote a letter to a different adolescent in the Scholastic Pen Pals Program. This time, they were told to describe an instance in which they had difficulty empathizing with someone else, and how they overcame that challenge. By helping participants identify instances in their own lives where they overcame difficulties empathizing, this prompt was intended to reinforce the idea that their capacity for empathy can grow. In the third session, participants were

asked to synthesize their two letters into a speech about empathy. They drafted the speech on a computer, then recorded themselves reciting the speech out loud in a private room.

Social Norms Condition. Participants in the social norms condition also attended three intervention sessions. As in the mindset condition, they wrote two letters and composed a speech for high school freshmen struggling to make social connections. In this condition, however, participants were asked to write about empathy's social normativity and desirability. Before composing their letters, participants read a passage describing how most people value and practice empathy. They also read research summaries about the normative nature of empathy, and "student testimonials" written by fellow undergraduates (see **Supplemental Material**). These testimonials—collected as part of a previous experiment—emphasize the normativity of empathy among Stanford undergraduates. They were intended to foster a pro-empathy descriptive norm. Because normative appeals are most potent when they feature complementary descriptive and injunctive norms (Cialdini, 2003), we presented the student testimonials along with an injunctive message that empathy is socially desirable.

Combined condition. This condition integrated content from both the mindset and social norms conditions. As these interventions were completely novel, it was possible that a condition including both messages could be maximally beneficial to participants. Participants were asked to write letters and record a speech for the Scholastic Pen Pals program. However, they were given instructions to emphasize both the malleable nature and normativity of empathy. To maintain consistent session length across conditions, participants were given abbreviated versions of the reading materials from the mindset condition and the social norms condition (see **Supplemental Material**).

During the second intervention session, participants wrote both about overcoming empathy-related difficulties, and about how empathy is valued among their peer group. As in the other two intervention conditions, these prompts were intended to help participants connect intervention content to their own experiences. During their third intervention session they wrote and recorded a speech based on their two letters, mirroring the third session of the other experimental conditions.

Control condition. The control condition also included two letter-writing sessions and a speech-drafting session. However, participants in this condition read letters purportedly written by adolescents experiencing academic (rather than social) difficulties. This condition was based on previous growth mindset of intelligence interventions (Paunesku et al., 2015; Yeager et al., 2016). Control condition participants read materials supporting the idea that intelligence is malleable, and were asked to share this information with their adolescent pen pals. During the second session, participants were asked to write specifically about an academic challenge they were able to overcome. During the third session, participants wrote and recited a speech based off of the letters they had composed in the first and second sessions.

Follow Up

Eight weeks after receiving the intervention, participants completed an online battery of tasks assessing empathy and social functioning. We selected this time period to examine persistence of intervention effects, as previous empathy-training experiments often examine changes only in the short-term (e.g., over the course of a single study session). During this follow up, participants completed a battery of assessments including variables that differed in their degree of relatedness to the intervention. These variables

included measures that were closely tied to the intervention, but also measures of downstream outcomes of empathy. We made this decision for two reasons. First, including variables that differed in their relatedness to the intervention would help us ascertain whether the interventions produce only local effects (as is the case in many existing interventions), or if instead the interventions affect measures more indirectly related to empathy. Second, a goal of this intervention was to create practical changes in participants' social and emotional lives to address shortcomings in previous research where gains did not persist outside of the lab. As such, we assessed performance on tasks known to have real-world predictive validity over socio-emotional functioning (such as an empathic accuracy task, described below) and examined participants' real social experiences since coming to school. In short, although the focus of the intervention is changing empathy, the ultimate goal of this endeavor is to change empathy in service of our participants' socio-emotional wellbeing.

Beliefs about the malleability of empathy. This measure was used to examine whether participants' beliefs about the malleable nature of empathy differed meaningfully across conditions after an 8-week delay. This 6-item questionnaire assesses participants' beliefs about the malleable nature of empathy (e.g., *"No matter who somebody is, they can always change how empathic a person they are.")* using a 7-point agreement scale (Schumann et al., 2014). Two participants did not complete this questionnaire (1 in the control condition, 1 in the social norms condition).

Empathic accuracy. Previous work suggests that *empathic accuracy*—or the ability to accurately infer others' emotions—tracks a person's empathic abilities as it reflects both cognitive and affective aspects of empathy (Zaki et al., 2008). It also tracks

real world outcomes like relationship satisfaction (Sened et al., 2017). To assess empathic accuracy, we used a video task developed by Zaki and colleagues (Zaki et al., 2008, 2009). Video stimuli were collected during a previous study, in which participants (hereafter referred to as 'targets') were recorded while describing positive and negative life events (targets' mean age = 26.5 years). Targets then watched their videos and continuously rated how negative or positive they felt at each moment while talking about the life event using a 1 – 9 scale, where 1 indicated very negative and 9 indicated very positive. Target ratings were then z-transformed so that data were normally distributed. We selected four videos that differed in valence (two positive and two negative, each 3 minutes or under and featuring a white female target) and showed them to participants in the current study (hereafter referred to as 'perceivers'). We then asked perceivers to rate how they thought the target was feeling continuously throughout the duration of each video.

Affect ratings from targets were obtained in a previous experiment (Zaki et al., 2008) and were sampled at 2-second intervals. Affect ratings from perceivers in the present experiment were sampled at .5-second intervals. Perceiver ratings were averaged across 2-second intervals to be consistent with target ratings, with each 2-second interval serving as a time point in the subsequent analyses. Perceivers' affect ratings were then correlated with targets' affect ratings to yield a correlation coefficient for accuracy for each of the four videos. All correlation coefficients were r-to-*Z* transformed using the Fisher technique so that data were normally distributed, consistent with previous analytic approaches for these data (Zaki et al., 2008, 2009). Videos were presented in random

order. Accuracy scores for positive and negative videos were averaged to create a positive composite score and a negative composite score.

If participants were unable to view one of the positive or negative videos due to technical difficulties, they were not assigned a composite score for that valence and were excluded from analysis of that valence. 30 participants do not have a composite score for the positive videos (8 mindset, 7 norms, 7 combined, and 8 control), and 35 participants do not have a composite score for the negative videos (7 mindset, 8 norms, 9 combined, and 11 control). Given that participants were instructed to make continuous ratings throughout the entire video, we also excluded participants who made 3 or fewer ratings per minute from our analyses for suspected noncompliance with task instructions, which resulted in 3 further participants being excluded from analyses of positive videos (1 mindset, 1 combined, and 1 control) and 22 participants from analyses of the negative video (3 mindset, 6 norms, 7 combined, and 6 control).

Number of friends. The first year of college presents many unique social challenges, including a rapid expansion of the social network. Empathy is known to predict social integration, including the number of friends a person has (Kardos et al., 2017). As such, we asked participants to list up to 10 friends they had made since coming to Stanford in order to assess social integration as a downstream consequence of empathy. Specifically, they were asked to name up to 10 people they see regularly, people they talk to often, and people they feel close to (see **Supplemental Material** for exact prompt). This measure was scored 0 – 10 (see **Supplemental Material** for additional summary statistics).

Intergroup empathy. To assess whether the intervention affected participants' willingness to empathize when it felt challenging, we used a task in which participants read about good and bad events befalling an outgroup member (adapted from Cikara, Bruneau, Van Bavel, & Saxe, 2014). Participants read a short biography ostensibly written by a political outgroup member describing his involvement with a campus political group. They then read about 16 positive and negative events that ostensibly happened to this person.

For each event, they used a 1 – 10 scale to rate how *bad* the story made them feel and how *good* the story made them feel. Congruent valence between story and rating (e.g., a negative story and a "how bad" rating) provided a measure of empathy. Incongruent valence between story and rating (e.g., a negative story and a "how good" rating) provide a measure of counter-empathy (or schadenfreude, Cikara, Bruneau, Van Bavel, & Saxe, 2014).

Outgroup member evaluation. Participants were also asked to rate how similar they were to the outgroup member, how friendly and how sincere the outgroup member seemed, how much they would like the outgroup member, how much they would like to meet the outgroup member, and how interested they were in hearing the political outgroup member's opinion on other issues (each rating made on a 1 – 10 scale). Because these items were positively and significantly correlated with each other and reliable (α = .83), they were averaged to create an overall evaluation score for the outgroup member (see **Supplemental Material** for correlations). Given that the majority of our participants identified as liberal, the outgroup target was ostensibly conservative, and only participants who identified as liberal were included in analyses (n = 167, 48 mindset, 47

norms, 38 combined, 34 control). Of these, 1 participant does not have a score for negative empathy (from the combined condition) and 2 participants do not have scores for schadenfreude (1 control and 1 mindset) due to missing ratings. They are therefore not included in that particular analysis.

Empathic Effort. Adapted from Schumann and colleagues (2014), empathic effort was measured using an audio-based task. Participants listened to an audio recording approximately 10 minutes in length that featured a person describing her grandmother's battle with cancer, an instance where empathy is painful and challenging and therefore might be avoided (Zaki, 2014). Crucially, they were able to fast-forward through as much of the recording as they wanted by dragging the slider on the audio controller. Empathic effort was operationalized as the amount of time participants spent listening to the audio recording. Five participants did not complete this task (1 malleable, 2 norms, 1 combined, 1 control). Given that the entire recording was approximately 10 minutes, 11 participants who spent over 12 minutes on the recording page were excluded from analysis for suspected noncompliance with the task instructions—leaving a sample size of n = 217 (60 mindset, 55 norms, 53 combined, 49 control).

State Empathy. After the audio recording, participants completed a questionnaire assessing different emotions they had experienced while listening to the audio recording (Fultz et al., 1988). This 12-item measure asks participants to rate the extent to which they experienced different feelings on a 7-point scale (1 = not at all, 7 = extremely), and is comprised of three subscales that measure distinct but related affective responses to others' suffering: an empathy subscale (how softhearted, touched, sympathetic, and compassionate they felt), a sadness subscale (how low-spirited, feeling low, heavy-

hearted, and sad they felt), and a distress subscale (how uneasy, troubled, distressed, and disturbed they felt). Five participants did not complete this questionnaire (1 mindset, 2 norms, 1 combined, 1 control). Participants who were excluded from empathic effort analyses for suspected non-compliance were also excluded from state empathy analyses, as we assessed empathy for the speaker in the audio recording.

To determine the effects of intervention condition on outcome measures, we ran a series of one-way ANOVAs with condition as a between-subjects variable. For all analyses, we report partial eta-squared effect sizes for ANOVAs and Cohen's d for t tests (with 95% confidence intervals [CIs]). Means and standard deviations are presented in **Table 1**. Bonferroni corrections were applied to the omnibus tests and results are reported below.

Data, code for analyses, and supplemental materials are available in an Open Science Framework repository, https://osf.io/f4czb/?view_only=1e026638842e4bcd9f7ad13d9249dd1f.

Results

Beliefs about the malleability of empathy

A one-way ANOVA revealed a significant overall effect of condition on beliefs about the malleability of empathy, $F(3, 227) = 4.67$, $p = .003$, $\eta^2 = .058$. This result was robust to Bonferroni correction. Participants in the malleable mindset condition endorsed greater beliefs about the malleability of empathy than participants in the control condition, $t(114) = 2.98$, $p = .004$, 95% CI [1.17, 5.82], $d = .56$, and social norms condition, $t(122) = 2.99$, $p = .003$, 95% CI [1.15, 5.66], $d = .54$, see **Figure 1a**. Participants in the combined condition also endorsed greater beliefs about the

malleability of empathy than participants in the control condition, $t(105) = 2.20$, $p = .030$, 95% CI [.27, 5.17], $d = .43$, and the social norms condition, $t(113) = 2.19$, $p = .031$, 95% CI [.25, 5.01], $d = .41$. Differences between participants in the malleable mindset and combined conditions, $t(117) = .72$, $p = .47$, 95% CI [-1.35, 2.90], $d = .13$, and social norms and control conditions, $t(110) = .07$, $p = .94$, 95% CI [-2.49, 2.68], $d = .01$, were not significant.

A linear model was fit to compare participants who received mindset messaging as part of their intervention (i.e., mindset and combined conditions) to those who did not (i.e., norms and control). Compared to participants in the norms and control conditions, participants in the mindset and combined conditions endorsed significantly stronger beliefs about the malleability of empathy eight weeks after the intervention, $b = 3.09$, 95% CI [1.44, 4.74], $t = 3.69$, $p < .001$.

Participants in our control condition endorsed relatively strong beliefs about the malleability of empathy. We therefore wondered whether there were meaningful differences between the control group participants' mindsets of empathy and mindsets of empathy observed in other populations. We compared scores from our control group participants to scores obtained from participants in a previous study using this measure among a sample of adults online (Schumann et al., 2014, Pilot Study 2). Notably, participants in our control condition endorsed significantly stronger beliefs about the malleability of empathy compared to participants from a sample collected in the other study, $t(125.54) = 5.21$, $p < .001$, 95% CI [4.57, 10.16], $d = .89$. This could indicate that either Stanford undergraduates hold stronger beliefs about the malleability of empathy than their non-student counterparts, or that the control condition—which featured an

intervention promoting beliefs about the malleable nature of intelligence—inadvertently shifted beliefs about the malleability of constructs beyond intelligence (including empathy, see **Discussion**).

Empathic accuracy

We observed condition-based differences in participants' empathic accuracy for targets' positive videos (see **Figure 1b**), $F(3, 196) = 4.59$, $p = .004$, $\eta^2 = .066$. This result was robust to Bonferroni correction. Participants in the mindset condition more accurately rated targets' emotions than participants in the control condition, ($t(71.72) = 3.18$, $p = .002$, 95% CI [.09, .41], $d = .67$). Participants in the combined condition also rated targets' emotions more accurately than participants in the control condition, $t(78.60) = 2.45$, $p = .016$, 95% CI [.04, .37], $d = .52$). Participants in the social norms condition also rated targets' emotions more accurately than those in the control condition, but this difference was only marginally significant, $t(75.79) = 1.71$, $p = .091$, 95% CI [-.02, .30], $d = .36$. Participants in the malleable mindset condition had marginally higher scores than participants in the social norms condition, $t(106.09) = 1.89$, $p = .062$, 95% CI [-.01, .23], $d = .36$. There were no significant differences when comparing scores from mindset condition participants to those in the combined condition, or comparing scores from participants in the combined condition to those in the social norms condition (see **Supplemental Material** for pairwise comparisons).

There were no statistically significant differences between conditions for accuracy for the negative videos, $F(3, 172) = .625$, $p = .60$, $\eta^2 = .011$.

Number of friends

Participants in the combined condition reported having made a greater number of close friends since coming to college than participants in the malleable mindset condition, social norms condition, and control condition, see **Figure 1c**. Though the overall effect was only marginally significant, $F(3, 229) = 2.42$, $p = .067$, $\eta^2 = .031$, t-tests revealed a statistically significant difference between participants in the combined condition and the control condition, $t(89.64) = 2.60$, $p = .011$, 95% CI [.24, 1.79], $d = .51$, and between participants in the combined condition and social norms condition, $t(105.31) = 2.34$, $p = .021$, 95% CI [.13, 1.59], $d = .43$. This difference was marginally significant between participants in the combined condition and the malleable mindset condition, $t(114.70) = 1.71$, $p = .091$, 95% CI [-.09, 1.25], $d = .31$. Differences between the malleable mindset and control conditions, malleable mindset and social norms conditions, and social norms and control conditions were not statistically significant (see **Supplemental Material** for pairwise comparisons).

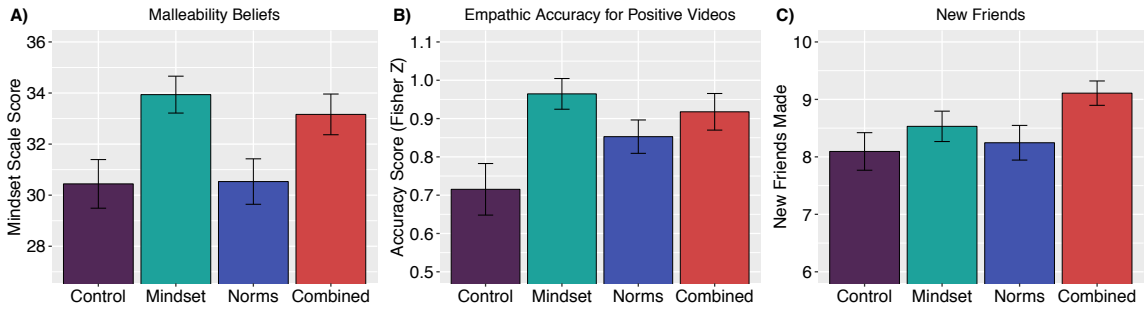

Figure 1. Outcome measure by condition. (a) Mean beliefs about the malleability of empathy are displayed on the y-axis for each of the four conditions (Control, Mindset, Norms and Combined, scale range: 0 - 42). **(b)** Mean empathic accuracy scores (fisher transformed Z-score) for the two positive videos are displayed on the y-axis for each of the four conditions. **(c)** Mean friends made since starting college are displayed on the y-axis for each of the four conditions. Error bars reflect +/- 1 standard error.

Empathy for an outgroup member

A one-way ANOVA revealed no significant differences across groups in self-reported empathy for an outgroup member on positive events, $F(3, 163) = .36$, $p = .78$, $\eta^2 = .007$, or negative events, $F(3, 162) = .47$, $p = .71$, $\eta^2 = .009$. There was a condition-based difference in self-reported schadenfreude for the outgroup member, $F(3, 161) = 2.22$, $p = .088$, $\eta^2 = .040$, but this difference did not reach statistical significance. Finally, there were no condition-based differences on evaluation of the outgroup member $F(3, 163) = .95$, $p = .42$, $\eta^2 = .017$.

Empathic effort

There were no condition-based differences on the measure of empathic effort, $F(3, 213) = .51$, $p = .67$, $\eta^2 = .007$, or on the state emotion scales, including the empathy subscale, $F(3, 213) = .32$, $p = .81$, $\eta^2 = .005$, sadness subscale $F(3, 213) = .07$, $p = .98$, $\eta^2 = .001$, and distress subscale, $F(3, 213) = .086$, $p = .97$, $\eta^2 = .001$.

Discussion

Our findings suggest that a motive-based framework is relevant to intervention efforts aimed at shifting individuals' empathy over the long-term and perhaps even to producing practical social benefits. However, it is important to note that these effects are nuanced and varied across intervention strategies. Participants in the mindset and combined conditions endorsed stronger beliefs about the malleability of empathy, even after an eight-week delay. Participants in all three intervention conditions exhibited improved accuracy when evaluating others' positive emotions. Finally, participants in the combined condition reported having made a greater number of friends since coming to college than participants in the control condition.

The present study builds on past work by introducing a novel approach to intervening over empathy. The majority of existing interventions in this space focus on building empathy-related skills, such as emotion recognition, perspective taking, and communication. However, such efforts may be limited in their impact, insofar as they address one's *ability* to empathize but inadvertently discount one's *motivation* to empathize. Here we designed and tested three interventions that specifically targeted empathic motives by teaching participants that empathy was malleable, socially normative, or both. We found that these interventions affected real-world socio-emotional outcomes during the transition to college. This experiment offers new evidence in support of a motivated framework of empathy and its relevance to intervention. It also introduces a new tool for building empathy which could be used alongside existing intervention techniques. By pairing skills-based interventions with complementary motivation-based approaches, researchers are positioned to create highly effective interventions that address both ability-based and motivation-based empathy failures.

Though our findings demonstrate the promise of motivation-based interventions in shifting empathy, the present work has some important limitations. First, motivation-based interventions did not shift measures of intergroup empathy or indices of effort in empathizing with a stranger's pain, indicating boundary conditions for these interventions. One plausible explanation is that our interventions—designed to *bolster empathic approach motives*—are not effective tools for changing empathy in contexts where people routinely avoid it. Notably, these two outcome measures entail empathizing when it is painful or goal-antithetical, two examples of empathic "avoidance motives" that drive people away from empathizing (Zaki, 2014).

As such, interventions that *reduce empathic avoidance motives* may produce more robust effects on social functioning in contexts where empathy often fails (Weisz & Zaki, 2017a). For example, an intervention by Halperin and colleagues (2011) improved attitudes toward outgroup members and increased willingness to compromise for peace by addressing Israelis' and Palestinians' perceptions of group malleability. This intervention—which deliberately made no mention of the Israeli-Palestinian conflict—artfully circumvented defensive reactions that often arise when conflict is addressed directly. Whereas direct attempts to improve attitudes toward an outgroup in long-standing conflict can backfire and make matters worse (Bar-tal & Rosen, 2009), interventions that subtly reduce avoidance motives may be more successful in improving intergroup relations (Zaki & Cikara, 2015).

The notion that these interventions increased empathic approach motives (but did not reduce empathic avoidance motives) may also help explain why we observed significant differences on the measure of empathic accuracy for positive—but not for negative—emotions. Accurately tracking a target's emotions involves both cognitive aspects of empathy (such as perspective taking) and affective aspects of empathy (such as experience sharing, Zaki et al., 2008). Because people enjoy sharing in others' positive affect (Morelli et al., 2015; Zaki, 2014), it is possible that an intervention strengthening approach motives encouraged individuals to try harder at something they were already inclined to do. However, sharing in negative emotions is often an experience people are motivated to avoid (Shaw et al., 1994). As such, interventions that reduce avoidance motives (rather than strengthening approach motives) may be better positioned to increase empathic accuracy for negative emotions than ours were.

Second, our control condition featured an intervention promoting beliefs about the malleable nature of intelligence. It is possible that this intervention inadvertently shifted beliefs about the malleability of constructs beyond intelligence, including empathy. Indeed, our control group endorsed stronger beliefs about the malleability of empathy compared to other samples (see Schumann, Zaki, & Dweck, 2014). Although this consequently provides a conservative test of our intervention, alternative experimental designs (e.g., wait-list or no-treatment groups) could provide a more naturalistic control against which to compare intervention outcomes in future research.

Furthermore, patterns of results were somewhat heterogeneous across outcome measures. For example, mindset condition participants and combined condition participants had comparable empathic accuracy scores, but combined condition participants made a greater number of friends. One possibility is that these interventions activate different motivational imperatives. Recently, researchers have identified basic motives often targeted in brief psychological interventions, which include the need for self-integrity and the need to belong (Walton & Wilson, 2018). Because our interventions differentially affected outcome measures, it is possible that they activate different basic motives. For example, it is possible that the social normativity intervention in the present study appeals specifically to one's need to belong.

Though a precise mechanistic account of these intervention effects is beyond the scope of this experiment, characterizing underlying mechanisms should be a priority for empathy intervention research going forward. Brief psychological interventions are most effective when they account for (i) the motivations and experiences of the individuals receiving them, and (ii) the context in which they are administered. In the present

experiment, the mindset and combined interventions affected more outcome variables than did the norms intervention. Though speculative, it is possible that mindset-based (rather than norms-based) empathy interventions may be more appropriate for college students considering the motivational experiences characterizing people this age. Personality research indicates that there is a sharp increase in people's openness during this stage of life (Roberts, Walton, & Viechtbauer, 2006). As such, college-age individuals may be particularly receptive to a mindset-based empathy intervention, in that the message of malleability likely aligns with growth they're already experiencing. Future work should therefore evaluate the importance of creating synchrony between people's existing motivations and the change strategy used to increase empathy among them.

More broadly, an important direction for future research will be to examine the way that intervention content interacts with the context in which it is delivered. The alignment between intervention content and context warrants further consideration from investigators. Recently, experiments comparing different motivation-based interventions in the workplace demonstrated that appealing to one's occupation-related motives engenders important behavior change. For example, doctors washed their hands more frequently after being reminded that hand hygiene promoted patient health, as compared to doctors reminded that hand hygiene protects their own health (Grant & Hofmann, 2011). Similarly, lifeguards volunteered more hours after learning about heroic water rescues than they did after learning about personal benefits that the job confers (Grant, 2008).

In both of these instances, interventions that appealed to role-based motivation—namely, to promote others' welfare—effected greater change than interventions appealing to personal gain. Future work could examine whether similar approaches increase empathy in occupation-based relationships where empathy is known to be especially important (for example, in the doctor-patient relationship). Taken together, these findings underscore the importance of accounting for motivation when designing and administering empathy interventions. Future research should explore the interplay of different empathy-related motives, intervention strategies, and intervention contexts to maximize precision in empathy building programs.

Finally, a critically important aspect of future research is to examine how interventions like those described in this manuscript affect not just empathy as a whole, but individual empathy-related processes. Empathy is an umbrella term, encompassing several related but distinct subcomponents (such as vicariously experiencing others' emotions and explicitly considering their perspectives). Although these processes can occur simultaneously, they can also dissociate and operate independently (Weisz & Zaki, 2018). The separability of empathy-related processes suggests that interventions may affect individual processes in different ways. Consistent with many previous interventions, the present work aimed to change empathy as a whole (rather than targeting individual subprocesses of empathy). However, recent work highlights the potential to intervene over specific subprocesses to create lasting changes in empathy (Singer & Engert, 2019), indicating an important new direction in empathy intervention research.

Empathy interventions are context sensitive, and "one-size-fits-all" approaches are often unsuccessful. The present study suggests that motivation plays an important role in building empathy through intervention, and illustrates the promise of this novel approach in shifting socio-emotional outcomes. These findings have exciting implications for researchers aiming to improve the social and emotional functioning of individuals during challenging periods like the transition to college, and across a broad range of social contexts.

Table 1. Means and standard deviations for outcome measures by condition.

	Condition							
	Mindset		Social Norms		Combined		Control	
Measure	*M*	*SD*	*M*	*SD*	*M*	*SD*	*M*	*SD*
Malleability Beliefs	33.94	5.78	30.53	6.90	33.16	5.91	30.44	6.87
Empathic Accuracy (pos)	0.96	0.30	0.85	0.32	0.92	0.33	0.72	0.45
Empathic Accuracy (neg)	0.76	0.25	0.73	0.39	0.73	0.32	0.81	0.29
New Friends	8.53	2.12	8.25	2.36	9.11	1.57	8.09	2.38
Outgroup Empathy (pos)	5.89	1.81	6.26	2.04	6.26	2.02	6.24	2.35
Outgroup Empathy (neg)	5.72	1.77	6.12	1.73	5.78	1.95	6.01	1.99
Outgroup Schadenfreude	1.31	0.51	1.31	0.68	1.63	0.78	1.44	0.62
Outgroup Member Eval.	4.47	1.61	4.50	1.48	4.70	1.40	4.98	1.43
Empathic Effort	302.7	203.3	280.1	199.9	310.6	208.0	266.7	205.3
State Empathy	19.05	5.28	19.65	5.38	18.60	6.12	19.20	5.66
State Sadness	15.07	5.48	15.29	5.82	15.49	5.97	15.49	5.12
State Distress	10.30	5.99	10.09	5.15	10.21	5.20	10.61	5.30

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