Digital Altruists:

Resolving Key Questions about the Empathy-Altruism Hypothesis in an Internet Sample

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Emotion

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Abstract

Researchers have identified the capacity to take the perspective of others as a precursor to empathy-induced altruistic motivation. Consequently, investigators frequently use so-called perspective-taking instructions to manipulate empathic concern. However, most experiments using perspective-taking instructions have had modest sample sizes, undermining confidence in the replicability of results. In addition, it is unknown whether perspective-taking instructions work because they increase empathic concern or because comparison conditions reduce empathic concern (or both). Finally, some researchers have found that egoistic factors that do not involve empathic concern, including self-oriented emotions and self-other overlap, mediate the relationship between perspective-taking instructions and helping. The present investigation was a high-powered, preregistered effort that addressed methodological shortcomings of previous experiments to clarify how and when perspective-taking manipulations affect emotional arousal and prosocial motivation in a prototypical experimental paradigm administered over the internet. Perspective-taking instructions did not clearly increase empathic concern; this null finding was not due to ceiling effects. Instructions to remain objective, on the other hand, unequivocally reduced empathic concern relative to a no-instructions control condition. Empathic concern was the most strongly felt emotion in all conditions, suggesting that distressed targets primarily elicit other-oriented concern. Empathic concern uniquely predicted the quality of social support provided to the target, which supports the empathy-altruism hypothesis and contradicts the role of self-oriented emotions and self-other overlap in explaining helping behavior. Empathy-induced altruism may be responsible for many prosocial acts that occur in everyday settings, including the increasing number of prosocial acts that occur online.

Keywords: empathy-altruism hypothesis; perspective taking; self-other overlap; social support

Introduction

People often claim that if individuals would take the effort to "put themselves in other people's shoes," then we would have a more caring society. Daniel Batson and his colleagues have provided indirect support for this dictum in their research testing the empathy-altruism hypothesis, which states that empathic concern generates altruism—that is, a non-instrumental desire to benefit another person (for a review, see Batson & Shaw, 1991). Batson theorized that perspective taking (the cognitive act of considering the feelings or thoughts of another person) is an important precursor to generating empathic concern (Coke, Batson, & McDavis, 1978).

Because of perspective taking's presumed instrumental role in motivating empathy-based altruism, scientists and public figures alike have paid considerable deference to the power of perspective taking (e.g., Pinker, 2011, pgs. 583-590; Wallace, 2009). Indeed, empirical reports suggest that perspective taking is a panacea for social problems, as it has been implicated in everything from reducing transphobia (Broockman & Kalla, 2016) to increasing concern for the environment (Berenguer, 2010; see Batson & Ahmad, 2009 and Hodges, Clark, & Myers, 2011 and for reviews of prosocial effects). Moreover, many of the researchers producing these findings have shown that empathic concern mediates these prosocial outcomes (e.g., Batson, Chang, Orr, & Rowland, 2002; Vescio, Sechrist, & Paolucci, 2003).

Researchers have confirmed that consciously adopting a certain cognitive perspective toward a person in need alters levels of empathic concern. Beginning with Stotland (1969), experimental social psychologists have manipulated empathic concern using "perspective-taking instructions." A typical experiment of this sort (e.g., Toi & Batson, 1982) involves asking participants to (a) imagine the feelings and thoughts of a given person in distress (the "imagine-other" condition), (b) imagine themselves experiencing the thoughts and feelings they would

have were they in the situation of a given person in distress ("imagine-self" condition), or (c) remain objective and detached while learning about a given person in distress (the "remain-objective" condition). The former two conditions are considered "high-empathy" conditions while the latter condition involves actively inhibiting perspective taking, and thus is used as a "low-empathy" condition.

Overall, perspective taking is a valuable object of study on many levels. Perspective taking is of substantial *theoretical* value, as it explains how people come to experience altruistic motivation. Perspective taking also has *methodological* value, as perspective-taking instructions are useful for manipulating empathic concern. Finally, perspective taking may have *practical* value, as deliberate attempts to take the perspective of others could help individuals understand and care about each other more.

Unresolved questions about perspective taking

Reliability of effects. Despite the fanfare that perspective taking research has generated, questions remain about the exact nature and robustness of its effects. First, with rare exceptions (e.g., Habashi, Graziano, & Hoover, 2016) experiments manipulating empathic concern using perspective-taking instructions have had 20 or fewer participants in each cell (e.g., see the experiments cited in Batson & Shaw, 1991). Studies with low statistical power are more likely to yield non-replicable significant findings (Button et al., 2013). In addition, there are published failures to manipulate empathic concern in the intended fashion using perspective-taking manipulations (e.g., Smith, Keating, & Stotland, 1989; van Lange, 2008). In light of recent research indicating that the replicability of social psychology research is low (Open Science Collaboration, 2015), it is important to assess whether influential findings, such as the relationship between perspective taking and empathic concern, represent reliable effects.

What psychological processes do perspective-taking manipulations alter? Second, it is unclear whether perspective-taking instructions actually increase empathic concern. The lack of clarity on this point is due to the fact that the remain-objective instructions tell participants to actively refrain from paying attention to the thoughts and feelings of the distressed person, which may reduce the amount of empathic concern that participants would experience in the absence of any instructions. Consistent with this possibility, Davis et al. (2004) found that participants who received no instructions about how to observe a needy person reported similar cognitions (mostly related to the self and the needy person) to participants who received imagine-other instructions, whereas participants who received remain-objective instructions had qualitatively different cognitions (mostly related to increasing the psychological distance between the self and the needy person). It is therefore possible that remain-objective instructions are wholly responsible for the group differences in empathic concern caused from assigning participants to read either "imagine" instructions or "remain-objective" instructions (Batson, Eklund, Chermok, Hoyt, & Ortiz, 2007).

If remain-objective instructions reduce empathic concern (and downstream helping) relative to people who experience empathy in the absence of any prompt of any kind, then it would seem warranted to conclude that humans spontaneously respond empathically to persons in need. Such spontaneity would suggest that the perspective taking-empathy-helping relationship is not merely an occurrence that can be contrived in the laboratory, but instead is a default response to human suffering. Furthermore, if instructions to imagine the feelings of others do not increase empathy-based helping, then there may be limits on the role that adopting the perspective of a suffering person can play in increasing prosociality, at least for the types of

situations studied in the empathy-altruism literature (viz., situations in which there is one victim who is clearly in need and is presented in a sympathetic light).

The role of perceived need. Third, van Lange (2008) argued that the content of perspective-taking manipulations is causally inert in emotionally intense situations. Supporting this conjecture, he found that imagine-other instructions and remain-objective instructions both elicited more empathy than a condition in which participants were neither given perspective-taking instructions nor presented with a needy victim. Moreover, he found no differences in empathy between the imagine-other and remain-objective conditions.

Van Lange's (2008) claim that the content of perspective-taking instructions have no effect in emotionally powerful situations has two implications. First, instructions (including instructions to remain objective) should neither increase nor decrease empathic concern in emotionally intense situations. Second, each type of instruction may have its intended effect in relatively less emotionally evocative situations. Thus, in less intense situations imagine-other and imagine-self instructions should increase empathic concern, and remain-objective instructions should reduce empathic concern. Clarifying whether van Lange (2008) is right would elucidate whether the ability of humans to alter their default emotional reaction to victims depends on the level of the victim's need.

Lack of specificity? Fourth, perspective-taking instructions may manipulate other emotions besides empathic concern. Batson, Early, and Salvarini (1997a) advised using imagine-other instructions to manipulate empathic concern because participants who receive imagine-self instructions report predominantly experiencing self-oriented distress, not empathic concern. But Maner et al. (2002) found that even imagine-other instructions lack specificity: Participants who received imagine-other instructions evinced not only more empathic concern than participants

who received remain-objective instructions, but also similar levels of self-oriented distress and sadness (see also Cialdini et al., 1987). The authors found that self-oriented sadness, not empathic concern, mediated the relationship between perspective-taking instructions and helping behavior. Consequently, they argued that participants helped because they wanted to eliminate their own vicarious negative arousal, not because they wanted to relieve the perspective-taking target's distress per se.

However, an unresolved issue is whether the self-reported distress and sadness in this experiment really was self-oriented, or was instead distress and sadness for the person in distress, which could also generate altruistic motivation. Batson (2011, p. 154) dismissed Maner et al.'s (2002) findings because of "the very high correlation of responses on the empathic-concern items with responses on the sadness items, $r = .79$ (and the distress items, $r = .72$)," which led him to conclude that "it certainly seems that all of these items are measuring essentially the same thing." No researchers to our knowledge have resolved this issue with new empirical data that cleanly distinguishes between self- and other-oriented distress and sadness. An experiment that measures the effect of perspective-taking instructions on empathic concern, self- and other-distress, and self- and other-oriented sadness, as well as their downstream effects on helping, is therefore necessary to clarify the evidential status of the empathy-altruism hypothesis.

In addition to manipulating multiple emotions, it is possible that perspective-taking instructions also manipulate "self-other overlap," or a feeling of "one-ness" with the person in distress (Cialdini, Brown, Lewis, Luce, & Neuberg, 1997; Maner et al., 2002). Maner et al. (2002) presented evidence that self-other overlap mediates the perspective taking-helping relationship, which would speak against the existence of altruistic motivation because insofar as an individual is helping somebody that is merged with the self, that individual is helping the self.

We do not think self-other overlap is a true threat to the existence of altruistic motivation because perceptions of "oneness" with another person would only be egoistic if the empathizer literally confused herself with the other person, which we find implausible. However, the empathy-altruism hypothesis would not be supported if empathic concern no longer predicted helping after controlling for self-other overlap.

The extent to which perspective taking modulates self-other overlap is also a valuable question in and of itself, as researchers have found that increasing self-other overlap has a range of prosocial effects, such as improving social coordination (Galinsky, Ku, & Wang, 2005) and reducing stereotyping (Galinsky & Moskowitz, 2000). However, the evidence that perspective taking really does manipulate self-other overlap is mixed. Maner et al. (2002) and Myers and Hodges (2011) found that imagine-other instructions did increase self-other overlap, whereas Batson et al. (1997c) did not. Myers, Laurent, and Hodges (2014) found that imagine-self but not imagine-other instructions increase self-other overlap, whereas imagine-other and imagine-self instructions increase empathic concern to the same extent, relative to remain-objective instructions. A high-powered re-examination of this issue may clarify conflicting findings.

The present experiment

The present preregistered experiment was designed to assess the robustness, scope, and prosocial consequences of the relationship between perspective taking and empathic concern. To do so, we addressed the low power of previous experiments on the topic by recruiting a large online sample. Second, we included a no-instructions condition to determine whether perspective-taking instructions upregulate emotions, remain-objective instructions downregulate emotions, or both. Third, we measured both self-oriented and other-oriented distress and sadness to adjudicate whether perspective-taking instructions primarily induce self-oriented or other-

oriented affect. We also measured social support by rating the quality of voluntary typed notes from the participant to the interaction partner to investigate Maner et al.'s (2002) claims that sadness and self-other overlap (egoistic motivators) mediate the perspective taking-helping relationship, but that empathic concern (a putatively altruistic motivator) does not.

Method

Preregistration and Institutional Approval

Our hypotheses, measures, data analysis plan, and data collection plan were all preregistered. The preregistration and study materials can be found at https://osf.io/7hw52/. All study materials and procedures were approved by the Institutional Review Board of University of Miami.

Recruitment

We recruited participants from Amazon.com's Mechanical Turk (MTurk) who had at least a 90% completion rate on previous tasks posted on the website. Participants were led to believe that the study was a communication study in which people learn about and respond to events occurring in others' lives. Participants completed the experiment through the software platform SoPHIE (Software Platform for Human Interaction Experiments; Hendricks, 2012), an interaction platform used for research experiments. Participants received $4.00 as compensation.

The experiment had a 4 (imagine-self, imagine-other, remain-objective, no-instructions) x 2 (high need, medium need) between-subjects factorial design with one covariate (dispositional perspective taking). A power analysis using G*Power (Faul, Erdfelder, Lang, & Buchner, 2007) revealed that a sample size of 800 participants can detect main effects and interactions of small magnitude (d = .23) with 80% statistical power. Consequently, we aimed to assign 100 non-suspicious participants to each of eight cells in our 4x2 design. We excluded data from

participants who took more or less than two standard deviations than the average amount of time to complete the experiment in order to avoid analyzing data from participants who were likely not engaged in the procedure (Arditte, Cek, Shaw, & Timpano, 2016).

However, as we stated in the preregistration, we planned to pause the experiment at 1200 participants to evaluate if we had sufficient money and time to continue data collection. After collecting 1219 participants, we found that three participants did not have data due to a computer error, 501 participants reported suspicion about the veracity of the interaction, and 60 participants completed the study two standard deviations faster or slower than the average time (M = 35 minutes and 57 seconds, SD = 8 minutes and 9 seconds). These exclusions left 680 participants whose data was available for the primary analysis (some participants failed both inclusion criteria), which is short of the planned 800. Having run more than 1200 participants, we decided at that time that we did not have sufficient money or time to continue data collection. However, with 680 participants we were still in a position to detect medium-to-small effects (d = .27) with 80% power. See Table 1 for the demographic characteristics of the final sample, and see the supplementary materials for examinations of demographic differences in empathy that have interested past researchers (e.g., Eisenberg & Lennon, 1983; Konrath, O'Brien, & Hsing, 2011).

Procedure

Pre-Experiment Interaction. After agreeing to participate based on the terms of an informed consent form, participants were directed to a waiting room in which they were matched with another actual MTurk user who happened to be taking the experiment at the same time (herein, "interaction partner"). Participants were asked to exchange five messages with the interaction partner using a chat message system that they would ostensibly use during the

experiment. In the chat, participants typically started the chat with a friendly introduction. No instructions for what to discuss were included; the ostensible purpose of the chat was simply to familiarize the participants with the chat function. Common topics of discussion included where each participant was from, how the weather was in their respective locations, whether they worked on MTurk often, and confirmations that the other person was a real person, rather than a chat robot.

After the brief chat, participants were told that they and their interaction partners would be discussing events occurring in their personal lives recently. One of them would be randomly designated as the "writer," who was to write a letter for five minutes about what has been going on recently in his/her life, while the other person would be designated as "reader," who was to read and respond to what the "writer" wrote. Although participants were under the pretense that they were still communicating with their interaction partners from the pre-experiment chat, they were in fact interacting with pre-programmed computer scripts and were all assigned the "reader" role through a sham random process.

While waiting to receive the letter, participants completed a demographics questionnaire and a questionnaire which we do not analyze in the context of this paper. Participants were then prompted with remain-objective instructions, imagine-self instructions, imagine-other instructions, or no instructions at all. All instructions were adapted from Batson et al. (1997a). See the supplementary materials for the full text of the instructions.

Perspective Taking Attention Check. To encourage participants to pay attention to the perspective-taking manipulation, participants who received instructions were told that they would be asked about what they just read, and given the option of returning to the instructions before proceeding to the attention check. The attention check asked participants to describe what

they had just read in one sentence. (Answering the attention check correctly was not used as a criterion for including a participant's data in statistical analyses. The goal of including the attention check was to encourage participants to pay attention to the instructions by telling them we would quiz them about the instruction's contents.) Once completed, participants received the interaction partner's letter, which was randomly chosen among the three letters available for each need condition (i.e., high-need vs. medium-need). See the supplementary materials for the text of the letters and how the letters were chosen.

Responses to the Letter. After reading the letter, participants were told that they could, if they desired, write a message with their thoughts and feelings about the letter to the writer. Participants were informed that their interaction partner would become aware of the letter response portion of the study only if they actually chose to respond. For those who did respond, we created a scale composed of three items to judge the quality of social support that they provided. (See the supplementary materials for the full text of the items.) The scale (alpha = .74) reflected emotional social support and asked about the extent to which the participant tried to provide genuine care to their interaction partner. We had three research assistants rate participants' responses on all three items (ICC = .70). If a participant did not provide a response, then raters coded each item as missing. Raters were blind to our hypotheses and the participants' other data. To encourage raters to make evidence-based evaluations we also asked participants to provide evidence, either in the form of direct quotations or general descriptions of personal impressions, that their rating was appropriate. (See the supplementary materials for the exact instructions we gave to raters.)

Emotion Measures. Once participants had chosen whether to respond to the letter writer, they completed two questionnaires measuring emotions, both of which we adapted from Batson

et al. (1997a). In the first measure, participants indicated how much from '1' (not at all) to '7' (extremely) they were feeling each of 13 emotions after reading their interaction partner's letter. Mixed among distractors were five adjectives that formed an empathic concern index (alpha = .83): *sympathetic, compassionate, concerned, empathic,* and *tender*.

Before moving onto the second questionnaire, participants read the following: "Many emotions can be experienced in different ways. For example, you can feel *directly happy*, such as when you have a great day. You can also feel *happy for* another person, such as when you celebrate another's good news. Keeping this in mind, we would like to ask you about the nature of some emotions you may or may not be feeling." Participants were then asked to report the extent to which the statement "I feel *directly* _____ after reading the letter" characterized their feelings for 13 emotion adjectives. Mixed among distractors were five words that formed a self-oriented distress index (alpha = .90): *alarmed, troubled, distressed, disturbed,* and *worried*. Also included were five adjectives that formed a self-oriented sadness index (alpha = .92): *sad, low-spirited, sorrowful, heavy- hearted,* and *melancholy*.

Next, participants were then asked to report the extent to which the statement "I feel _____ *for* the writer" characterized their feelings for the same 13 emotion adjectives that they had seen when they reported their self-oriented responses. The same adjectives that formed the self-oriented distress and sadness indexes were used to make other-oriented distress (alpha = .90) and other-oriented sadness (alpha = .88) indexes.

Perspective Taking Manipulation Check. Also adopted from Batson et al. (1997a), we included three items to assess whether our perspective-taking instructions had the intended effect on how participants focused their attention. Participants responded to the following questions on a 7-point Likert-type scale ranging from '1' (not at all) to '7' (very much): "While reading the

letter, to what extent did you focus on…" (1) "…imagining how you would feel if you experienced what the writer described?"; (2) "… imagining how the writer felt during the experience that he or she described?"; and (3) "…remaining objective?" Participants then completed a measure of how much need the participant perceived the interaction partner to be in, which is described in supplementary materials.

Perceived Need Manipulation Check. We checked the efficacy of the manipulation of perceived need by asking participants, "Did the writer describe any positive events that he/she experienced?" and "Did the writer describe any negative events that he/she experienced?" (We included the item about positive events to avoid cuing participants to the fact that we expected them to view the stories—which they believed had been typed in real-time—to be negative.) Participants responded "yes" or "no" to each item. If a participant said "yes" to an item, they were asked to give an open-ended response to the prompt, "Describe any positive [negative] aspect(s) of what the writer experienced in 1-2 sentences," and to indicate how positive/negative the experience was on a 9-point Likert scale ranging from '1' (a little) to '9' (extremely).

Inclusion of Other in the Self Scale (IOSS). Participants were then given the Inclusion of Other in the Self Scale (Aron, Aron, & Smollan, 1992), a validated, one-item pictorial measure of psychological closeness. It is the standard instrument used to measure self-other overlap. Participants chose among seven pictures of increasingly overlapping circles the picture that best represented the connection that they felt with their interaction partner after reading the interaction partner's letter.

Suspicion Probe. After completing a measure of social anxiety that is described in the supplementary materials, we assessed potential suspicion using a funnel procedure (Aronson, Carlsmith, & Ellsworth, 1990). We worried that our measure of dispositional empathy would

arouse suspicion that the letter they read was intended to evoke empathic concern, so we collected data on this measure after participants finished the suspicion probe. The probe asks increasingly direct questions about whether participants correctly guessed the hypotheses of the experiment, whether they had any suspicion of being deceived, and whether their suspicions affected their behavior. Participants who expressed suspicion that they were not actually interacting with another participant were dropped from the primary analysis (but we note in the results when including these participants qualitatively affects the results). Last, participants completed the Interpersonal Reactivity Index (Davis, 1983), a multi-dimensional measure of dispositional empathy. (Analyses involving this variable are in the supplementary materials.)

Predictions

Our predictions were as follows (see other predictions in supplementary materials):

P1: Both of the perspective-taking instructions (i.e., the "imagine-other" and "imagine-self" instructions) will elicit higher levels of emotional arousal (i.e., all emotions, both self-oriented and other-oriented) than will a no-instructions condition, which will in turn result in higher levels of emotional arousal than will instructions to remain objective while reading the interaction partner's letter.

P2: Perceptions of need will be positively associated with emotional arousal. We did not have a specific prediction about whether level of need would interact with type of perspective-taking instructions to influence emotional arousal. Van Lange (2008) would have predicted that none of the instructions would alter default levels of empathy in the high-need condition, and that each type of instruction would influence empathy in the medium-need condition (i.e., imagine-other and imagine-self instructions should increase empathy, and remain-objective instructions should reduce empathy).

P3: Self-oriented emotions (i.e., personal distress and personal sadness) will predominate (i.e., evince the high mean score) in the imagine-self condition, whereas other-oriented emotions (i.e., empathic concern, other-oriented distress, and other-oriented sadness) will predominate in the imagine-other condition.

P4: The imagine-self instructions will elicit higher levels of self-other overlap than the other perspective-taking instructions.

Additional Tests

We had no *a priori* predictions about whether social support in this experiment would be affected by perspective-taking instructions, or whether social support would be best predicted by other-oriented emotions rather than self-oriented emotions or self-other overlap. Instead, we tested the empathy-altruism hypothesis, which predicts that empathic concern will mediate the relationship between perspective-taking instructions and social support, and that empathic concern will still predict social support even controlling for self-oriented emotions and self-other overlap. We operationalized social support in two ways—the rated quality of social support among participants who sent a message to their interaction partner after reading the partner's note, and whether participants sent a message at all.

Results

We conducted all analyses using R 3.1.2 (R Core Team, 2013). In addition to the base package, we used the car (Fox, 2007), lmerTest (Kuznetsova, Brockhoff, & Christensen, 2015), psych (Revelle, 2016), reshape (Wickham, 2007), lavaan (Obserski, 2014), and compute.es (Del Re, 2013) packages. The data and syntax to perform the analyses can both be found at https://osf.io/7hw52/.

The results reported below are from participants who were not suspicious of the deceptive elements of the experiment. We note below when the results qualitatively differ when including suspicious participants. Participants were dropped from analyses in which they provided no data (either because of a computer error in recording data or because they skipped the items). Participants were included in analyses in which they provided partial data; whatever data was available was used to estimate their score on each measure. Overall, there was very little missing data.

Manipulation Checks

Did participants adjust their focus according to the instructions they received?

***Yes*.** (See Table 2 for means and standard deviations.) Participants in the imagine-self condition focused more on themselves while reading the story than did participants in the no-instructions condition, b = .65, SE = .15, $t(676) = 4.31$, $p < .001$, 95% CI [.35, .94], $d = .46$. The imagine-other condition elicited more focus on the interaction partner than did the no-instructions condition, b = .52, SE = .16, $t(676) = 3.30$, $p = .001$, 95% CI [.21, .84], $d = .37$. Last, participants in the remain-objective condition focused more on remaining objective while reading about their interaction partner than did those in the no-instructions condition, b = 1.30, SE = .20, $t(676) = 6.53$, $p < .001$, 95% CI [.91, 1.68], $d = .71$.

Which condition is the appropriate control condition?

The manipulation check also confirms that the no-instructions condition is a more appropriate control than the remain-objective condition. One may argue that the remain-objective condition is a more natural reference group than the no-instructions condition because having participants read instructions of some sort facilitates comparable levels of engagement, whereas the no-instructions condition does not. However, the patterns of attentional focus in the no-

instructions group suggests comparable levels of engagement (see Table 2): The no-instructions condition elicited more focus on the self and the other person than did the remain-objective instructions (but less than did the imagine conditions), and less focus on remaining objective than did the remain-objective instructions (but more than did the imagine conditions). The amount of attentional focus toward each target (viz., the self, the interaction partner, and remaining objective) in the no-instructions condition would have been uniformly low if it had elicited less engagement. The intermediate level of focus in the no-instructions condition in each analysis therefore strongly suggests that the no-instructions condition represents the reference point from which perspective-taking instructions alter attentional focus.

Interestingly, as Table 2 also shows, the imagine-other instructions increased focus on the self. This finding is consistent with Batson and Ahmad's (2009) suggestion that "imagining how you would feel in [another's] situation (an imagine-self perspective) may provide a useful, possibly essential, stepping-stone to sensitive understanding of the other's plight (an imagine-other perspective)" (p. 146). Supporting this possibility, a multiple regression analysis in which the instructions manipulation (represented as three dummy-coded variables) and self-reported focus on the self predicted focusing on the interaction partner revealed a significant effect of focusing on the self, b = .62, SE = .03, $t(675) = 19.80$, $p < .001$, 95% CI [.56, .68], and only a marginally significant effect of the imagine-other instructions, b = .23, SE = .13, $t(675) = 1.81$, $p = .071$, 95% CI [-.02, .48], $d = .20$.

Did participants in the high-need condition perceive that the stories they read were more negative than participants in the medium-need condition?

Yes. The medium-need stories were perceived overall as less negative than were high-need stories, b = -.49, SE = .13, $t(655) = -3.66$, $p < .001$, 95% CI [-.76, -.23], $d = -.28$. (See

Table 3 for means and standard deviations of perceptions and emotions at each level of the perceived need manipulation.)

Tests of Hypotheses

Did instructions affect emotional arousal? (P1)

***Yes*, but only some of our predictions were borne out.** (See Table 4 for means and standard deviations.) We first conducted a linear regression of the effect of instructions on empathic concern. The model was significant, $F(3, 676) = 15.26$, $p < .001$, $R^2_{adj} = .06$. We then examined the effects of the perspective-taking manipulation with the no-instructions condition dummy-coded as the reference group, and the other conditions dummy-coded as treatment groups. Against our predictions, neither the imagine-other instructions, b = .18, SE = .13, $t(676) = 1.42$, $p = .157$, 95% CI [-.07, .43], $d = .16$, nor the imagine-self instructions, b = .03, SE = .12, $t(676) = .27$, $p = .789$, 95% CI [-.21, .27], $d = .03$, significantly increased empathic concern. These non-significant findings do not reflect a ceiling effect: The mean empathic concern of the "imagine-self" and "imagine-other" conditions could have been statistically significantly different from the no-instructions condition at values as low as 5.85 (using the no-instructions condition standard error), which is well below the ceiling of the 7-point scale. (However, including suspicious participants does reveal a significant effect of imagine-other instructions: b = .25, SE = .09, $t(1152) = 2.74$, $p = .006$, 95% CI [.07, .43], $d = .23$.) Finally, participants who received remain-objective instructions reported feeling less empathic concern than did participants in the no-instructions condition, b = -.60, SE = .12, $t(676) = -4.80$, $p < .001$, 95% CI [-.84, -.35], $d = -.52$. So, the negative effect of remain-objective instructions is about twice as strong as the positive effect of imagine-other instructions, (if there is a real effect of imagine-other instructions at all).

Next, we conducted regression analyses to examine the effect of perspective-taking instructions on self-oriented distress and other-oriented distress. Against our predictions, the overall model of the effect of instructions on self-oriented distress was not significant, $F(3, 676) = 1.56$, $p = .145$, $R^2_{adj} = .004$. In contrast, instructions did have a significant effect on *other*-oriented distress in the overall model, $F(3, 676) = 3.64$, $p = .013$, $R^2_{adj} = .012$. However, an examination of the simple effects revealed that none of the conditions were significantly different from the no-instructions condition. In an exploratory analysis, we reran the analysis with the remain-objective condition as the reference group. Both the imagine-other condition, b = .46, SE = .18, $t(676) = 2.60$, $p = .010$, 95% CI [.11, .81], $d = .29$, and imagine-self condition, b = .53, SE = .17, $t(676) = 3.06$, $p = .002$, 95% CI [.19, .87], $d = .33$, elicited significantly more other-oriented distress than did the remain-objective instructions condition. This pattern of findings suggests that the non-significant decrease in other-oriented distress in the remain-objective instructions relative to the no-instructions condition, as well as the non-significant increases in other-oriented distress in the imagine conditions, may reflect effects that are real but smaller than our experiment was designed to be able to detect. (Interestingly, there were no significant effects, even with the remain-objective instructions as the reference group, when including suspicious participants.)

Finally, we ran regression analyses to examine the effect of instructions on self- and other-oriented sadness. The regression analysis of perspective-taking instructions' effect on self-oriented sadness was not significant, $F(3, 676) = 2.21$, $p = .09$, $R^2_{adj} = .005$. However, the overall model of perspective-taking instructions' effect on other-oriented sadness was significant, $F(3, 676) = 4.98$, $p = .002$, $R^2_{adj} = .017$. We probed this effect by examining the simple effects, with the no-instructions condition as the reference group. As predicted,

participants in the imagine-self condition reported significantly more other-oriented sadness than did the no-instructions group, b = .37, SE = .17, $t(676) = 2.22$, $p = .027$, 95% CI [.04, .69], $d = .24$. Against our predictions, the imagine-other condition, b = .28, SE = .17, $t(676) = 1.61$, $p = .108$, 95% CI [-.06, .61], $d = .18$, and remain-objective condition, b = -.20, SE = .17, $t(676) = -1.22$, $p = .223$, 95% CI [-.53, .12], $d = -.13$, did not affect other-oriented sadness.

Did high-need stories evoke stronger emotions than the medium-need stories? (P2)

Yes, but the perceived need manipulation did not interact with the perspective-taking manipulation. (See Table 4 for means and standard deviations.) The linear regression of the effect of story need on empathic concern was significant, $F(1, 678) = 5.82$, $p = .016$, $R^2_{adj} = .007$. Participants evinced less empathic concern in the medium-need condition than in the high-need condition, b = -.22, SE = .09, $t(678) = -2.41$, $p = .016$, 95% CI [-.40, -.04], $d = -.19$. However, adding the instructions variable into the model did not yield a significant interaction, $F(3, 672) = .99$, $p = .395$, suggesting that the perceived need of the interaction partner does not moderate the effect of instructions.

Next, we analyzed the effect of need on distress. The linear regression of the effect of need on self-oriented distress was significant, $F(1, 678) = 32.22$, $p < .001$, $R^2_{adj} = .044$. Participants in the medium-need condition reported less self-oriented distress scores than did participants in the high-need condition, b = -.67, SE = .12, $t(678) = -5.68$, $p < .001$, 95% CI [-.90, -.44], $d = -.44$. However, adding the instructions variable into the model did not yield a significant interaction, $F(3, 672) = .65$, $p = .582$.

The effect of need on other-oriented distress also was significant, $F(1, 678) = 82.92$, $p < .001$, $R^2_{adj} = .11$. Participants in the medium-need condition reported less other-oriented distress than participants in the high-need condition, b = -1.08, SE = .12, $t(678) = -9.11$, $p < .001$, 95%

CI [-1.32, -.85] , d = -.70. However, adding the instructions variable into the model did not yield a significant interaction, $F(3, 672) = .73$, $p = .535$.

Next, we looked at the effect of need on sadness. The effect of need on self-oriented sadness was not significant, $F(1, 678) = .79$, $p = .374$, $R^2_{adj} = .00$. Similarly, the interaction of story need and perspective-taking instructions on self-oriented sadness was nonsignificant, $F(3, 672) = .27$, $p = .846$.

The effect of story need on other-oriented sadness was significant, $F(1, 678) = 17.57$, $p < .001$, $R^2_{adj} = .02$. Participants in the medium-need condition reported less other-oriented sadness than participants in the high-need condition, b = -.50, SE = .12, $t(678) = -4.19$, $p < .001$, 95% CI [-.73, -.26], d = -.32. However, adding the instructions variable into the model did not yield a significant interaction, $F(3, 672) = 1.18$, $p = .318$.

Did other-oriented emotions predominate in the imagine-other condition? Did self-oriented emotions predominate in the imagine-self condition? (P3)

No. Other-oriented emotions, especially empathic concern, predominated in all conditions. (See Table 4 for means and standard deviations.) We created a 4 (instructions) x 5 (emotion type: empathic concern, self-oriented distress, self-oriented sadness, other-oriented distress, other-oriented sadness) linear mixed-factor model with repeated measures on the emotion type factor. Empathic concern was the reference group for the emotion type factor, and the no-instructions condition was the reference group for the instructions factor.

The results of the mixed-factor ANOVA revealed a main effect of emotion type, $F(4, 2720) = 709.07$, $p < .001$, and a main effect of perspective-taking instructions, $F(3, 680) = 6.38$, $p < .001$. The interaction between emotion type and perspective-taking instructions was not significant, $F(12, 2720) = 1.49$, $p = .12$, suggesting that the rank-ordering of how strongly each

emotion was felt did not differ at different levels of the perspective-taking manipulation. (The interaction was significant when including suspicious participants, $F(12, 4624) = 2.19$, $p = .010$, but an examination of simple effects revealed no change in the rank-ordering of emotions, regardless of which type of instructions was used as the reference group.) The main effect of perspective-taking instructions on each emotion does not provide any information over and above the analyses we conducted to address P1, so we omit reporting those results here. Instead, we focus on the rank-ordering of the five emotions.

Participants reported less self-oriented distress, b = -2.64, SE = .10, $t(2720) = -47.13$, $p < .001$, and less self-oriented sadness, b = -2.16, SE = .06, $t(2720) = -38.48$, $p < .001$, than empathic concern. Thus, empathic concern predominated over the self-oriented emotions. Other-oriented distress, b = -1.15, SE = .06, $t(2720) = -20.54$, $p < .001$ and other-oriented sadness, b = -.86, SE = .06, $t(2720) = -15.33$, $p < .001$ were also felt more weakly than was empathic concern. Collectively, these results suggest that learning about distressed persons primarily induces empathic concern, regardless of what instructions are used.

We were also interested in whether other-oriented distress and sadness predominated over self-oriented sadness and distress. To do so, we re-ran the model with other-oriented distress as the reference group. Indeed, self-oriented distress, b = -1.49, SE = .06, $t(2720) = -26.59$, $p < .001$, and self-oriented sadness, b = -1.00, SE = .06, $t(2720) = -17.93$, $p = .001$, were felt less strongly than was other-oriented distress. Participants also reported more other-oriented sadness than other-oriented distress, b = .29, SE = .06, $t(2720) = 5.21$, $p < .001$. Thus, other-oriented emotions predominated over self-oriented emotions, with empathic concern felt most strongly of all.

Was self-other overlap higher in the imagine-self condition than in the other conditions? (P4)

No. (See Table 4 for means and standard deviation.) The regression analysis of the effect of perspective-taking instructions on the IOSS was significant, $F(3, 676) = 4.35$ $p = .005$, $R^2_{adj} = .015$. We next examined the simple effects, with the no-instructions condition as the reference group. Against our prediction, participants in the imagine-self condition did *not* report significantly more self-other overlap than did participants in the no-instructions condition, b = .17, SE = .19, $t(676) = .92$, $p = .360$, 95% CI [-.20, .54], $d = .10$. However, participants in the imagine-other condition did report significantly more self-other overlap than participants in the no-instructions condition, b = .41, SE = .20, $t(676) = 2.10$, $p = .037$, 95% CI [.03, .79], $d = .23$. As expected, participants in the remain-objective condition did not report significantly less self-other overlap than did participants in the no-instructions condition, b = -.27, SE = .19, $t(676) = -1.39$, $p = .166$, 95% CI [-.64, .11], $d = -.15$.

Did perspective-taking instructions affect social support? (exploratory)

Yes. First, we used a logistic regression model to confirm that perspective-taking instructions had an effect on whether participants sent a message to the interaction partner. Relative to the no-instructions condition, the remain-objective instructions reduced the odds of sending a note, OR = .37, $Z = -3.38$, $p < .001$. The imagine-other instructions, OR = .85, $Z = -.50$, $p = .617$, and imagine-self instructions, OR = .88, $Z = -.43$, $p = .671$, did not. (The imagine-other instructions did not increase the probability of sending a note when including suspicious participants in the analysis, either, despite the fact that imagine-other instructions did increase empathy in an analysis that included suspicious participants.)

Next, we used a regression model to confirm that perspective-taking instructions indeed influenced the quality of social support among participants who sent a message to their interaction partners, $F(3, 556) = 5.41$, $p = .001$, $R^2_{adj} = .023$. (See Table 3 for means and

standard deviations. Examining the simple effects with the no-instructions group as the reference group, we found that remain-objective instructions had a negative effect on quality of social support, b = -.43, SE = .15, $t(264) = -2.93$, $p = .003$, 95% CI [-.72, -.14], $d = -.32$. The imagine-other, b = .12, SE = .14, $t(556) = .80$, $p = .427$, 95% CI [-.17, .40], $d = .09$, and imagine-self instructions, b = .04, SE = .14, $t(556) = .31$, $p = .757$, 95% CI [-.23, .32], $d = .03$, had no effect on quality of social support. (The imagine-other instructions did not increase the supportiveness of notes when including suspicious participants in the analysis, either, despite the fact that that imagine-other instructions did increase empathy in an analysis that included suspicious participants.)

Did empathic concern predict social support? (exploratory)

Yes. First, we conducted a binary logistic regression of the effect the five emotional states and self-other overlap on whether participants typed a message to their interaction partners. Empathic concern, b = .66, SE = .11, Z(673) = 5.91, OR = 1.93, $p < .001$, 95% CI [1.56, 2.43], and self-other overlap, b = .21, SE = .07, Z(673) = 3.06, OR = 1.23, $p < .002$, 95% CI [1.08, 1.42] both significantly increased the odds of sending a message to the interaction partner. No other predictors were significant. (See Table 6 for the regression model.)

We conducted a regression analysis of the effect of each emotional state and self-other overlap on quality of social support among participants who sent a message. The model was significant, $F(6, 553) = 22.47$, $p < .001$, $R^2_{adj} = .19$, and empathic concern significantly predicted quality of social support, b = .42, SE = .05, $t(553) = 7.91$, $p < .001$, 95% CI [.32, .52], $d = .67$. No other predictors were significantly associated with quality of social support. (See Table 5 for the regression model.)

The predictors in the models predicting social support are all closely conceptually related, which raises the possibility that some of the nonsignificant regression coefficients were artifacts of multicollinearity. The moderate correlations among the predictors (see Table 7) are not particularly concerning, but nevertheless we calculated the variance inflation factor (VIF) of each predictor in the models that predicted the quality of social support and the probability of sending a note. All VIFs were less than 4, a cutoff for concern about multicollinearity which if anything is too conservative (O'Brien, 2007). Thus, the fact that empathy and self-other overlap were the only significant predictors of social support cannot be attributed to multicollinearity.

Did empathic concern mediate the relationship between instructions and social support? (exploratory)

Yes. We ran a nonparametric bootstrap mediation model (Preacher & Hayes, 2004) using probit regression in which perspective-taking instructions predicted empathic concern, and both perspective-taking instructions and empathic concern predicted the likelihood of sending a message (see Figure 1). We treated the no-instructions group as the control condition and the remain-objective instructions group as the treatment condition. We found that empathic concern was a significant mediator, b = -.13, SE = .04, Z= -3.33, p < .001, 95% CI [-.22, -.06]. The instructions also had a significant direct effect in the model, b = -.42, SE = .17, Z = -2.50, p = .013, 95% CI [-.77, -.07]. Thus, remain-objective instructions apparently cause a reduction in the probability of offering social support through their intermediate effect on empathic concern. A path analysis in which all emotions and self-other overlap were entered as mediators of the relationship between the perspective-taking manipulation (no-instructions condition vs. remain-objective condition) and the likelihood of sending a message yielded the same result: Empathic concern was the only candidate mediator that was affected by the perspective-taking

manipulation, and that also predicted the probability of sending a message (self-other overlap had a direct effect, but not an indirect effect). See the R code for the syntax to run this model.

We also ran a nonparametric bootstrap mediation model with 1,000 Monte Carlo draws in which perspective-taking instructions predicted empathic concern, and both perspective-taking instructions and empathic concern predicted the quality of social support among participants who sent messages (see Figure 2). We treated the no-instructions group as the control condition and the remain-objective instructions group as the treatment condition. We found that empathic concern was a significant mediator, b = -.26, SE = 06, Z = -4.25, p < .001, 95% CI [-.48, -.16]. The instructions did not have a significant direct effect in the model, b = -.12, SE = .15, Z = -.83], p = .41, 95% CI [-.41, .17]. Thus, among participants who chose to send a note, remain-objective instructions cause a reduction in the quality of social support through their intermediate effect on empathic concern. A path analysis in which all emotions and self-other overlap were entered as mediators of the relationship between the perspective-taking manipulation (no-instructions condition vs. remain-objective condition) and the note's emotional supportiveness yielded the same result: Empathic concern was the only candidate mediator affected by the perspective-taking manipulation, and also uniquely predicted rated quality of emotional social support (there was no significant direct effect of perspective-taking manipulation). See the R code for the syntax to run this model.

Discussion

The present experiment was a high-powered effort to test several pivotal theoretical claims about the effects of empathic concern on helping. The investigation produced four main findings. First, the remain-objective instructions reduced empathic concern for the needy target; the imagine-self instructions had no significant effect on empathic concern relative to the no-

instructions condition and imagine-other instructions; and the positive effect of imagine-other instructions on empathic concern was small, and only evident when we analyzed both suspicious and non-suspicious participants. Second, remain-objective instructions reduced the social support offered to the interaction partner. This effect was mediated by empathic concern, which also uniquely predicted the quality of social support. In contrast, neither the imagine-other instructions nor the imagine-self instructions affected social support relative to the no-instructions condition. Third, the high-need stories did evoke more emotional arousal than the medium-need stories. However, there was no evidence that the efficacy of perspective-taking manipulations depended on the perceived need of the interaction partner. Fourth, other-oriented emotions predominated in all conditions, suggesting that participants did not respond in a primarily self-oriented way to their interaction partner's distress.

These results are in tension with many other findings. First, our results contradict reports that participants who receive imagine-self instructions predominantly feel self-oriented distress, whereas participants who receive imagine-other instructions predominantly feel empathic concern (Batson et al., 1997a; Lamm, Porges, Cacioppo, & Decety, 2008; Stotland, 1969). Other-oriented emotions were felt more strongly than self-oriented emotions in all perspective taking conditions, with empathic concern felt most strongly of all.

Second, the lack of interaction between perceived need and our perspective-taking manipulations sit uneasily with van Lange's (2008) claim that perspective-taking instructions do not affect empathic concern in emotionally evocative situations. If we are correct that van Lange would have also predicted that perspective-taking instructions *would* have their intended effect in less emotionally evocative situations, then he is only partly correct: remain-objective instructions indeed reduced empathy, but imagine-other and imagine-self instructions did not increase

empathic concern. Indeed, our results suggest that perspective-taking manipulations affect empathic concern to the same extent irrespectively of the severity of the victim's need. Thus, humans seem to have a robust ability to reduce their empathic response to others' need by seeking to remain objective, no matter how great the need, but much less ability to increase empathic response to others' need by trying to take the needy person's perspective, no matter how minor the need. An important caveat to this conclusion, however, is that participants viewed high-need stories as only slightly more negative than medium-need stories.

Third, we did not find that any of the instructions altered self-oriented sadness or distress. This suggests, contra Maner et al. (2002), that perspective-taking manipulations do not confound altruistic and egoistic motivation. Also contra Maner et al. (2002), we did not find that self-oriented sadness or self-other overlap explain helping behavior. Instead, we found unambiguous support for the empathy-altruism hypothesis: Empathic concern uniquely predicted the supportiveness of participants' letters to their interaction partners, mediated the relationship between the perspective-taking instructions and the quality of supportive notes, and even predicted (along with self-other overlap) whether participants sent a note at all. Our results are more definitive than those of Maner et al. (2002) because we used a much larger sample and distinguished between sadness and distress that are self-oriented versus other-oriented. Because Maner et al.'s (2002) findings were the primary results that left the status of the empathy-altruism hypothesis unclear (Batson, 2011), our results restore confidence that the current state of evidence does indeed support the empathy-altruism hypothesis.

Limitations and Future Directions

The present experiment had much higher statistical power than previous investigations have had, which may explain some of the discrepancies between the current findings and

previous findings. However, an alternative explanation for these discrepancies is that previous experiments in this literature were conducted in the laboratory whereas the current investigation was conducted online on Mechanical Turk. However, we think it is doubtful that our results are seriously biased by the online nature of the study, as many investigators have been able to replicate laboratory findings on Mechanical Turk (Amir, Rand, & Gal, 2012; Levay, Freese, & Druckman, 2016).

Moreover, the online setting could have made the likelihood of observing significant effects of perspective taking on empathy, and empathy on helping, higher than in the laboratory because the perceived costs of empathizing are lower online. Cameron and Payne (2011) found that people spontaneously downregulate empathic concern in order to reduce motivation to act on opportunities to provide costly help. Empathizing is arguably less costly in online settings because (a) there is no obvious way to provide costly help during an emotionally arousing experience, and (b) the participant and the person in need are neither in the same place nor embedded in the same community, thereby weakening the felt obligation to help. Thus, participants in our study were likely not strongly motivated to attenuate their default reactions to victims in need, especially because the helping opportunity we provided participants was not announced beforehand.

The fact that the present investigation was conducted online also makes its results more generalizable than findings from laboratory experiments. The Mechanical Turk participant pool is more representative of the national population than undergraduate populations typically are (Berinsky, Huber, & Lenz, 2012), and nearly 200 million people regularly engage in prosocial acts on the internet (Klisanin, 2011), so one can hardly argue that the internet is not an important venue in which prosocial acts occur. Thus, our experiment is high in ecological validity, and our

results are consistent with the possibility that much of internet-based prosociality is caused by empathy-induced altruistic motivation.

The only substantive limitation to our use of an online setting that could present a cause for concern, we believe, was that participant suspicion was much higher than is reported for comparable laboratory studies. Including suspicious participants in the analyses revealed a significant (but small) effect of imagine-other instructions on empathic concern. Thus, we cannot draw any strong conclusions about the null effect of imagine-other instructions on empathic concern that we observed when analyzing only non-suspicious participants. We can firmly conclude, however, that imagine-other instructions had no downstream effect on behavior, indicating that any increase in empathic concern that the imagine-other instructions did cause is not relevant to explaining helping behavior.

In comparing the relative merits of including versus excluding suspicious participants in analyses, it is worth speculating about the plausible explanations of why results would qualitatively differ as a function of whether suspicious participants were included. Our examination of participants' responses to the suspicion probe (see the dataset in the supplementary materials for transcripts of responses) suggests that suspicion is largely a function of experience with taking social-psychological experiments, not a personality trait per se. This potential explanation fits well with the high suspicion rate, given that Mechanical Turk workers have typically completed many studies (Rand, Peysakhovich, Kraft-Todd, Newman, Wurzbacher, Nowak, & Greene, 2014). Suspicious participants may therefore have responded more strongly to our imagine-other manipulation because (a) they believed we were interested in how people would respond to a person in distress or need, and (b) the knew that the normatively appropriate way to respond to someone in need—especially when having "put themselves in the

other person's shoes"—is to respond empathically. Nevertheless, we are certainly open to the possibility that the suspicious participants may be higher in some trait that makes them more susceptible to the intended effects of imagine-other manipulation.

Another potential limitation of the present experiment is that we used only the Inclusion of Other in Self Scale to measure self-other overlap. We used the Inclusion of Other in Self Scale because it is a validated scale that has been used in many experiments that challenge the empathy-altruism hypothesis (e.g., Cialdini et al., 1997; Maner et al., 2002), but Myers and Hodges (2011) found that the Inclusion of Other in Self Scale most likely reflects psychological closeness. Many authors do not construe self-other overlap as psychological closeness per se, but rather as reflecting the degree to which mental representations of the self and other person are congruent. Thus, it may have been more appropriate for us to use measures that Myers and Hodges (2011) identified as reflecting overlapping mental representations between the self and another person, such as an attribute checklist (Davis, Conklin, Smith, & Luce, 1996) or difference ratings (Galinsky and Moskowitz, 2000).

The most surprising result of our investigation was that the imagine-other and imagine-self instructions did not reliably increase empathic concern. This finding was not a foregone conclusion, as empathic concern was not at ceiling in the no-instructions condition. Instead, participants were able to alter their empathic concern only by remaining objective, which reduced empathic concern. Imagine-other instructions did seem to have an effect when including suspicious participants in the analysis, although the magnitude of this positive effect was only about half of the negative effect that remain-objective instructions had on empathic concern.

The most likely explanation for this set of results is that people spontaneously feel empathic concern for others in distress, *at least when the presentation of the distressed person is*

sympathetic. The sympathetic presentation of perspective-taking targets is nearly ubiquitous in the experimental literature. Even in experiments in which participants take the perspective of murderers or other stigmatized groups (Batson et al., 1997b; Batson et al. 2002), the distressed person is presented as remorseful. The stories we used here were no different in this respect, and this factor alone may be responsible for the spontaneous empathic concern that we observed. Future research, therefore, might fruitfully investigate whether deliberate attempts to take the perspective of others more robustly increases empathic concern (and downstream helping) for needy individuals in situations that do not strongly elicit empathic concern, such as when the target is unsympathetic or when there is more than one person in need (Västfjäll, Slovic, Mayorga, & Peters, 2014). Promisingly, the available evidence regarding the effect of personality traits on the empathy-helping relationship hint that effortful perspective taking can make up for factors that militate against empathic concern (perhaps regardless of whether those factors are personological or situational). For instance, we report in the supplementary materials that imagine-other instructions may increase empathic concern for those low in dispositional empathy. Similarly, Habashi et al. (2016) report that imagine-other instructions increase empathic concern for persons low in agreeableness.

The fact that perspective taking manipulations influenced empathic concern and helping behavior primarily through reducing empathic concern in the present setting also has implications for the theoretical understanding of the perspective taking-empathy-helping relationship. First, our results imply that altruistic motivation is not purely a laboratory phenomenon that requires inculcation via effortful perspective taking that is irrelevant to the potential help-giving situations of everyday life. Instead, the perspective taking-empathy-helping relationship seems to be part of the standard motivational set of humans. That is, experiencing an

empathy-based desire to help after taking the perspective of a distressed person looks less like an act that humans are merely capable of (such as applying language skills to learn how to read, which humans only do in highly-specific cultural contexts that arose recently), and more like an act that people engage in as a matter of course in response to observing a relatable individual experience distress (just as humans naturally develop oral and receptive linguistic competence in response to becoming socialized within a language community; Pinker & Bloom, 1990). This finding increases the plausibility of evolutionary theorizing about the function of empathy (e.g., De Waal, 2008; Goetz, Keltner, & Simon-Thomas, 2010), given that evolved emotion systems are theorized to be species-typical and triggered automatically in response to certain environmental inputs that were recurrent over human evolutionary history (Cosmides & Tooby, 2000).

Second, several researchers (e.g., Cameron & Payne, 2011; Zaki, 2014) have argued that humans strategically regulate their emotions in order to selectively feel empathy according to their interests in a given situation. Our results may have something to contribute to such arguments inasmuch as they suggest that (a) people can avoid empathic concern by actively preventing themselves from thinking about the plight of a needy person, but (b) cannot as easily increase their empathic concern for a needy person by consciously considering the needy person's plight and feelings. Cameron and Payne (2011) and Zaki (2014) might interpret this pattern of findings, we think, as reflective of the abovementioned notion that participants may have perceived the costs of empathy to be low—and if so, might have been prepared to experience relatively high empathy (which might have consequently enabled them to reap the affiliative benefits that ensue from empathy-based helping at a minimal personal cost). If our post hoc interpretation here is correct, it would explain why a deliberate emotion regulation

strategy was particularly effective only for reducing empathic concern: Empathic concern may have already been at near (psychological, not statistical) ceiling for unprompted participants because of its relatively low costs in light of the social benefits it might have yielded.

Conclusion

The status of the empathy-altruism hypothesis has long been unclear because of discrepant findings regarding the effects of perspective taking on emotional arousal and helping behavior (Batson, 2011; Maner et al., 2002). The present experiment was an attempt to clarify such discrepant findings, and the results were unambiguously supportive of the empathy-altruism hypothesis: Empathic concern uniquely predicted quality of social support, and mediated the relationship between the perspective-taking manipulation and quality of social support. Moreover, the fact that instructions to remain objective while reading of the plight of a person in need (but not instructions to take the needy person's perspective) had a robust effect on default levels of empathic concern suggests that empathy-induced altruism is likely not just a laboratory phenomenon, but is rather a feature of human nature that is spontaneously expressed in everyday life. The empathy-altruism hypothesis is decades old, but appears to us to remain in good shape—even in an era of digital altruism.

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