ABSTRACT

MERE EXPOSURE AND PRO-SOCIAL BEHAVIOR: CAN REPETITION FACILITATE HELPING?

by Isaiah F. Jones

Previous research indicates that repeated mere exposure creates a diffuse feeling of positivity. Further, individuals induced to feel positive are more likely to help. Thus, repeated exposure should produce later helping behavior, even if the exposed stimulus is not relevant to the help-seeking target. In Experiment 1, participants were asked to help a target either after subliminal repeated exposure to the target, subliminal repeated exposure to a non-target, or non-repeated subliminal exposure. Counter to predictions, I found increased helping only after exposure to a relevant target. Experiment 2 examined the possible effect of delay between exposure and help solicitation on Experiment 1's results, but failed to replicate the findings of Experiment 1 or produce any meaningful results of its own. Implications of these results and ideas for future research are discussed.

Keywords: Affect, Fluency, Mere exposure, Pro-social

MERE EXPOSURE AND PRO-SOCIAL BEHAVIOR: CAN REPETITION FACILITATE
HELPING?

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CONTENTS

INTRODUCTION.....	1
Positivity and Helping.....	3
EXPERIMENT 1.....	5
Participants.....	5
Materials.....	5
Procedure.....	5
Results.....	7
Help likelihood.....	7
Help quality.....	8
Discussion.....	8
EXPERIMENT 2.....	10
Participants.....	10
Materials.....	11
Procedure.....	11
Results.....	12
Help likelihood.....	13
Help quality.....	13
Discussion.....	14
GENERAL DISCUSSION.....	14
Conclusion.....	16
REFERENCES.....	17
FOOTNOTES.....	21

TABLES

1. Percentage of participants, by condition, who chose to help the ostensible professor (Experiment 1).....	22
2. Percentage of participants, by condition, who chose to help the ostensible professor (Experiment 2).....	23

FIGURES

1. Help quality as a function of exposure condition. Y-axis represents z-score of log transformed data. Error bars indicate standard error of the mean.....24
2. Number of survey questions answered as a function of delay and exposure condition. Y-axis represents z-score of log transformed data. Error bars indicate standard error of the mean.....25
3. Time spent writing as a function of delay and exposure condition. Y-axis represents log 10 transformation of time spent writing in milliseconds. Error bars indicate standard error of the mean.....26

MERE EXPOSURE AND PRO-SOCIAL BEHAVIOR: CAN REPETITION FACILITATE HELPING?

In 1968, Robert Zajonc investigated what he called the “mere exposure effect.” Quoting an article from a local newspaper in which students’ attitudes toward a weirdly “costumed” peer improved over time, Zajonc (1968) hypothesized that, “mere repeated exposure of [an] individual to a stimulus is a sufficient condition for the enhancement of his [or her] attitude toward it” (p.1). Zajonc (1968) bolstered his observation by demonstrating that words used more frequently in everyday discourse are also rated as more pleasant than words used less often (Johnson, Thomson, & Frincke, 1960). He also showed that subjective ratings of “goodness” increased as a function of manipulated frequency for nonsense words, Chinese ideographs, and yearbook photos (Zajonc, 1968). That is, as participants were increasingly exposed to these stimuli, their ratings of them were enhanced.

The mere exposure effect is robust and pervasive. Since Zajonc’s pioneering work, the effect has been examined over 200 times (Bornstein, 1989). Mere exposure has led to higher ratings for line drawings (Berryman, 1984), research confederates (Moreland & Beech, 1992), names (Jacoby, Kelley, Brown, & Jasechko, 1989), music (Heingartner & Hall, 1974), and random shapes (Seamon, Brody, & Kauff, 1983). Further, mere exposure has led to higher subjective ratings for subliminally presented stimuli (Kunst-Wilson & Zajonc, 1980), and the effect has even been evidenced by non-human animals (Hill, 1978).

The vast majority of work exploring the mere exposure effect has focused on how repeated exposure shapes attitudes. Arguably, though, attitudes toward an object are meaningless if one’s behavior toward that object goes unaffected. Accordingly, some work has examined whether the mere exposure effect might extend beyond participant judgments of stimuli to influence demonstrable behavior. Specifically, a recent investigation by Jones, Young, and Claypool (2011) has explored mere exposure’s effect on approach and avoidance behavior. These researchers hypothesized that previous exposure to stimuli would motivate approach responses. To test this prediction, individuals initially saw 15 ideographs. After a delay, these ideographs were presented again along with 15 new ideographs. Participants were told that some of these stimuli might elicit a “push” reaction whereas others might elicit a “pull” reaction, and that they should perform whichever action they felt appropriate for each stimulus with a provided joystick. After this task, they were asked whether (1) pushing felt like they were moving toward

and pulling felt like they were withdrawing from the object (an object construal) or whether (2) pushing felt like moving the object away from and pulling felt like moving the object toward the self (a self construal). Thus, pushing was analogous to approach for those who took an object frame, whereas pulling was considered approach for individuals who took a self frame (Seibt, Neumann, Nussinson, & Strack, 2008). As predicted, those who took an object frame pushed more toward familiar than novel stimuli, whereas those who took a self frame pulled away from familiar stimuli more so than novel stimuli. Therefore, individuals with either construal type approached familiar stimuli more frequently than novel stimuli.

In other work, Bornstein, Leone, and Galley (1987) found that previous mere exposure to a social target increased agreement with that person. Participants were subliminally exposed to one of two target confederates. Later, participants read 10 poems and had to come to a consensus with the other two “participants” about the sex of the poem’s unknown author. The results showed that individuals agreed significantly more often with the confederate to whom they were previously (subliminally) exposed.

Moreover, Burger, Soroka, Gonzago, Murphy, and Somervell (2001) investigated whether mere exposure might increase compliance to others’ requests. Specifically, they had participants complete a three-minute bogus task and then for two minutes afterward, the participants either sat alone (alone condition), sat quietly with a confederate who ostensibly was completing the same task (mere exposure condition), or chatted with a confederate who had also supposedly just finished the task (interaction condition). Subsequently, participants were solicited by the confederate with whom they had just completed the bogus task (or in the alone condition, a newly introduced confederate) to help peer-review an English paper. Results indicated that participants in the mere exposure and interaction conditions agreed to help significantly more than participants in the alone condition. Burger et al. (2001) argued that both interaction and mere exposure increased liking enough to promote subsequent compliance to confederate requests.

In a second experiment, Burger et al. (2001) replicated their first study with an additional condition. This additional condition was identical to the mere exposure manipulation of study 1, except that after exposure, instead of being asked for help by the confederate with whom they sat in the same room, participants were solicited by a novel confederate. Results indicated that participants complied with requests from novel confederates significantly less than requests from

previously-exposed confederates. Burger and colleagues argued that these results showed that mere exposure to a person increased liking for him/her, which, in turn, increased willingness to comply with his/her requests.

In their entirety, these studies suggest that mere exposure can indeed facilitate positive social behaviors—one's willingness to approach another, agree with another, and comply with another's request. In this study, I seek to investigate whether repeated exposure can influence another type of positive social behavior, helping, and whether this can occur even when the experience of repeated exposure is nonconscious and unrelated to the source seeking help.

Positivity and Helping

Pro-social, or helping, behavior is a heavily explored topic within social psychology. From the classic studies conducted by John Darley and Bibb Latane (1968), which found that helping decreased as the number of witnesses increased, to the theoretical arguments over the altruistic versus egoistic root of pro-social behavior (Neuberg, Cialdini, Brown, Luce, & Sagarin, 1997), helping has been at the forefront of social psychological inquiry. Not surprisingly, but of particular importance to the current investigation, evidence suggests that mood influences both the likelihood and strength of a helping response (Carlson, Charlin, & Miller, 1988). Specifically, positive affect tends to increase helpfulness, and even minor manipulations of positive affect have led to significantly increased helping.

For example, Cunningham, Steinberg, and Grev (1980) manipulated whether people in a crowded mall did or did not find a planted dime in a payphone coin return slot. Participants who received a dime, presumably pleased by the free change, were more likely to help pick up papers subsequently dropped by a confederate than those who had not found this "windfall." Similar increases in pro-social behavior have resulted from other experimental manipulations intended to increase mood, such as succeeding in an experimental task (Isen, 1970), being labeled a charitable person (Kraut, 1973), receiving a free gift (Isen, Clark, & Schwartz, 1976), being a member of a winning football team (Berg, 1978), listening to relaxing music (Fried & Berkowitz, 1979), and imagining a fantastic Hawaiian vacation (Rosenhan, Salovey, & Hargis, 1981).

Importantly, the literature examining the mere exposure effect has demonstrated that previous stimulus presentation also induces positive mood. Perhaps the most convincing evidence of this fact comes from studies that collected psychophysiological measures. Namely,

Harmon-Jones and Allen (2001) found more activation over the zygomaticus major, or smiling muscle (indicative of positive affect), when participants viewed repeated (relative to non-repeated) stimuli. Studies using self-report measures find similar results. For example, Monahan, Murphy, and Zajonc (2000) subliminally exposed participants to either five different Chinese ideographs five times each or 25 different ideographs once. Those who were exposed to repeated stimuli reported being in a more positive mood than those in the non-repetition condition. Given that positive mood can increase pro-social behavior and repeated exposure to stimuli can increase mood, I hypothesize that repeated exposure should increase pro-social behavior.

At first glance, this hypothesis might seem redundant with the hypothesis tested and confirmed by Burger and colleagues (2001), discussed above. In my view, however, this is not the case for at least two reasons. First, Burger and colleagues did not measure actual helping behavior. Rather, they measured one's *willingness to agree* to help. Participants were asked if they would be willing to provide feedback to a confederate on an essay he/she had composed and to return that feedback to him/her the next day. The participant agreed or did not agree, and the experiment ended. Thus, these researchers did not assess whether participants actually helped (e.g., read the essay and provided feedback) or the quality of that help (e.g., whether their feedback was elaborate and well reasoned or terse and of little use). And, of course, social psychologists have repeatedly shown that what people say they will do is often a poor predictor of their own future behavior (see Epley & Dunning, 2000, for an example in the pro-social arena). Therefore, I would argue that Burger and colleagues did not measure true pro-social behavior.

Second, Burger and colleagues (2001) arguably did not manipulate mere exposure. Being in the presence of a confederate for five minutes hardly constitutes "mere." Participants believed that the "mere-exposed" confederate was a fellow participant, and, in fact, the participant and this confederate had just gone through the same study in each other's presence. Because of this shared experience, the participant likely perceived the "mere-exposed" confederate as similar to the self, a condition known to enhance liking (e.g., Byrne, 1961). Moreover, participants possibly inferred that the "mere-exposed" confederate attended the same university, was also in Introductory Psychology, and might have had the same class standing. Thus, the participants likely felt that they shared group membership with this confederate on more than one dimension, which also may have increased liking (Brewer, 1979). The novel confederate that approached

participants requesting help did not share an experience with participants and may not have been inferred to be an ingroup member. Therefore, it is quite possible that these other factors (perceived similarity, shared group membership), and not simple exposure, were responsible for the increased liking toward the “mere-exposed” person, which resulted in greater compliance with his/her request compared to the novel person.

The current studies, therefore, measured true pro-social behavior in a traditional mere exposure paradigm. In Experiment 1, I tested the prediction that nonconscious previous exposure to a help-seeking target would increase one’s willingness to help that individual, and enhance the quality of the help offered, relative to a novel help-seeking target. I also examined whether repeated exposure to a non-relevant social target would also increase helping. This second hypothesis is based on the work of Monahan and colleagues (2000) described earlier. In it, one group of participants was subliminally exposed to five ideographs, each five times (repetition condition), whereas another group of participants was subliminally exposed to 25 unique ideographs, each just once (novel condition). Results showed that self-reported mood was more positive for those in the repetition condition. Because positive mood can increase pro-social behavior, any experience of repetition should trigger increased helping behavior, even if the help-seeking target is not the repeated stimulus.

Experiment 1

Participants

One-hundred sixty-three introductory psychology students (90 females) participated for partial fulfillment of a course requirement.

Materials

Six photos of adult males, culled from the photo repositories of various university institutions, were used as the target, subliminal stimuli. These photos portrayed individuals in grayscale from the shoulders up with neutral expressions and were approximately 125 x 167 pixels in dimension. Additionally, 20 different images of 8-point random polygons 167 x 167 pixels in dimension and culled from Vanderplas and Garvin (1959) were used as the filler, subliminal stimuli. Finally, twenty-five black and white patterned designs, 167 x 167 pixels in dimension, were used as masks to achieve subliminal presentation of the target (facial) and filler (polygon) stimuli.

Procedure

After participants were welcomed to the lab and consent was obtained, they were seated in a private cubicle for an ostensibly 45-minute long experiment. A computer informed participants that they would be asked to complete several unrelated tasks, the first of which involved perception. During this initial task, participants were subliminally presented with one of three sets of stimuli. Those in the mere-exposure same condition viewed (across 25 trials) a photo of an individual five times randomly interspersed with 20 random polygons. I term this condition “mere-exposure same” because this pictured individual will later solicit help from the participant. Those in the mere-exposure different condition also viewed (across 25 trials) a photo of an individual five times randomly interspersed with 20 random polygons. This condition is so labeled because, though the individual is repeatedly shown, he will not later solicit help from the participant. Finally, those in the novel condition viewed (across 25 trials) 5 unique male photos, each just once, interspersed with 20 random polygons. Thus, participants in this condition neither see the person who will later solicit help, nor encounter any individual repeatedly.

The goal of this initial perceptual task, participants were told, was to identify quickly and accurately the location of a “flash of light” on the screen. On a given trial, participants first viewed a fixation cross in the center of the computer screen for 1 second. Next, participants were randomly exposed to a single stimulus from the mere-exposure same, mere-exposure different, or novel sets for 15 milliseconds, forward and backward masked for 35 milliseconds by a unique pattern mask.¹ Stimuli appeared either to the left or right of the fixation cross and participants were told to press the z key if the perceived “flash” appeared to the left of the fixation cross and the / key if it appeared to the right. For those in the mere-exposure same and mere-exposure different conditions, this process continued until the male photo was presented 5 times and each of the 20 random polygons was presented once. For those in the novel condition, this process continued until all 5 faces and all 20 polygons were presented once.

After this exposure task, participants engaged in a roughly four-minute, unrelated filler task in which they were to choose the capital of every U. S. state from four given cities. Finally, participants were informed that the study was finished. At this point, the computer notified participants that they were free to see the experimenter for debriefing and leave the session, but if they had time, a male professor from the psychology department was soliciting help for a future study. The supposed professor’s photo was displayed along with the following paragraph:

Hello! My name is Dr. Williamson. For an upcoming study, I need a story written with a neutral tone. The story can be about whatever you like; I just ask that it not be particularly happy or sad. Also, the more you write, the more helpful it will be. If you are willing to help me, please click “Yes I will help.” If you don't want to do this, please click “No, thanks.”

Note that the target's face was identical to the one shown earlier in the mere-exposure same condition, but was novel for those in the other two conditions.

Importantly, by the time participants received this request for help, approximately 15-20 minutes had passed in the experimental session. Because participants signed up for what they believed would be a 45-minute experiment, they were not facing any time pressure. Thus, if participants refused to help, it was not for this reason. For those who agreed to help, they were directed to type out a neutral story of whatever length they desired into MediaLab, which recorded both what participants wrote and how much time they spent composing their story.

After completing the helping task, or if they refused to help, participants then provided basic demographic information, were probed for suspicion, debriefed, and thanked. Overall, the design was a one-factor experiment, with three levels (mere-exposure same, mere-exposure different, and novel) manipulated between-participants. Two separate dependent measures consisted of whether participants chose to help (hereafter termed help likelihood) and, if so, the quality of that help, as measured by prose length and time spent writing, (McCabe & Peterson, 1984). Quality was considered important to examine because even if participants chose to help, if the value of that assistance was poor it would not, after all, be particularly helpful.

Results

Sixteen participants were removed before all analyses, 14 for guessing that the study was in fact about helping behavior, 1 for explicitly stating that s/he purposefully did not follow instructions, and 1 whose prose-length and time-spent-writing scores were greater than 3 SDs from the mean. This left 147 participants available for further analyses.

Help likelihood. Whether participants chose to help the ostensible professor was subjected to a chi-square analysis. I predicted that participants in each of the mere-exposure conditions would be more likely to help than participants in the novel condition. However, analyses indicated that helping rates did not differ by condition, $\chi^2(2) = 2.410, p = .300$. That is, participants were equally likely to help in the mere-exposure same (24%), mere-exposure

different (27%), and novel (38%) conditions (see Table 1). Thus, on this dependent measure, the hypothesis was not supported.

Help quality. Of the 147 participants available for analyses, only 44 actually chose to help the ostensible professor. Thus, quality of help can only be assessed on those 44 participants. To remind, both prose length and time spent writing were measured. Each was positively skewed and therefore log transformed. Moreover, because these indices were highly correlated ($r = .823$, $p < .001$) each measure was standardized, combined, and averaged into a single index of help quality.

It was predicted that participants in each of the mere-exposure conditions would provide help of greater quality (i.e., would write more and for longer) than participants in the novel condition. A one-way ANOVA with condition as a between-participant's factor and help quality as the dependent variable was marginally significant, $F(2, 41) = 2.778$, $p = .074$. Contrasts were performed to further investigate a specific pattern that emerged (see Figure 1). Specifically, as is suggested by the obtained pattern, the mere-exposure same group ($M = 0.461$, $SD = 0.725$) differed from the combined value of the mere-exposure different ($M = -0.306$, $SD = 0.792$) and novel groups ($M = -0.176$, $SD = 1.03$), $t(41) = 2.343$, $p = .024$, which did not differ from one another, $t(41) = 0.414$, $p = .681$. Thus, counter to my hypothesis, only the mere-exposure same condition yielded greater quality help.

Discussion

Once again, it was predicted that those in both mere-exposure conditions would be more likely to help and offer better quality help than those in the novel condition. Because the experience of mere exposure increases participant mood, and increased mood predicts future helping, it follows that mere exposure will augment helping behavior relative to those in the novel condition, even if that exposure is to a non-relevant target.

However, the obtained results were not entirely in line with my predictions. First, there were no differences in help likelihood. Participants in all three conditions were equally likely to offer help to the ostensible professor. This is particularly surprising, given that the majority of work in the helping literature, reviewed earlier, typically finds significant movement in help likelihood. It is possible that a floor effect constrained the data too greatly to have meaningful variability. After all, only 44 of 147 participants (30%) chose to help. However, this possibility seems somewhat unlikely given I was still able to find meaningful differences between groups on

the help quality measure. More troubling still is that the descriptive pattern of results on the help likelihood measure was in a direction counter to my hypothesis (i.e., those in the mere-exposure conditions were *less* likely to help than those in the novel condition).

Additionally, I predicted increased help quality from both mere-exposure conditions relative to the novel group. Instead, I found augmented help quality only from participants who were exposed to the help-seeking target. This could imply that there is something special and unique about re-exposure to the help-seeking target compared to having an experience of repetition more broadly (that is not target specific). Indeed, some previous work in the fluency literature would corroborate this idea.

Fluency is the ease with which stimuli can be identified or processed (Jacoby & Dallas, 1981). Previous exposure to a stimulus makes it easier to process, and the positivity this generates may be attributed to the stimulus itself, resulting in classic mere-exposure effects (e.g., Reber, Schwarz, & Winkielman, 2004). Importantly, the work of Whittlesea and Williams (2001a, 2001b) implies that a discrepancy between expected and observed fluency is necessary for fluency effects to emerge. For example, Whittlesea and Williams (1998) presented participants with real words, regular non-words (e.g., Hension, Pingle) and irregular non-words (e.g., Stofwus, Lictpub). Later, old and new examples from each category were presented, and participants had to pronounce them out loud and then judge whether they were old or new. Their participants produced the most false alarms (saying that a new word was old) with regular non-words. Whittlesea and Williams claim that this is because these words produced the greatest discrepancy between expected and observed fluency. That is, one would expect a non-word to be difficult to pronounce (non-fluent). However, the unexpected ease of pronouncing these words essentially tricked one into thinking the word must be old. In my experiment, only participants in the mere-exposure same condition would have experienced a feeling of discrepancy between expected and experienced fluency/familiarity. A stranger's face appears on the screen, that they would expect to be unfamiliar, but because they have subliminally been exposed to him earlier, he appears surprisingly familiar. Participants in the other two conditions would not have experienced a similar violation between expected and observed fluency.

Thus, different processes could have been working in the mere-exposure same and mere-exposure different conditions. Those in the mere-exposure different condition should have experienced the mood-amplifying effects of repeated exposure, as suggested by the work of

Harmon-Jones and Allen (2001) and Monahan, Murphy, and Zajonc (2000). Those in the mere-exposure same condition should also have experienced this positive affect but also may have benefited from the fluency-violation process, which, independently, might have triggered its own positive affect. Thus, from this perspective, perhaps those in the mere-exposure same condition should have helped more than those in the mere-exposure different condition, with both helping more than those in the control condition. The fact that those in the mere-exposure different condition helped at equal rates to those in the novel condition suggests that something may have disrupted the ability of their repetition-induced positive affect to influence helping. Mood decay may be the answer.

Recall that between exposure and help solicitation, participants engaged in a roughly four-minute filler task. A filler task was employed because stronger mere exposure effects are typically observed following a delay (Bornstein, 1989). However, this is simply because a delay makes participants less likely to attribute their diffuse mood states to the process of repeated exposure (Bornstein & D'Agostino, 1994). Because exposure in my experiment was subliminal, however, a filler task is unnecessary. Yet, the filler task may have had the adverse effect of allowing time for participants' moods to decay (that is, to return to baseline). Thus, by the time the help request was presented, participants in this condition may have been in the same (or quite similar) affective state as those in the novel condition, leading both to offer relatively low-quality help. Though participants in the mere-exposure same condition should also have had their repetition-induced positive affect decay, they may have experienced an independent "burst" of positive affect upon seeing the unexpectedly fluent/familiar target person when the help request was issued. If indeed only these participants experienced positive affect at this time, only they should have offered high-quality help.

Experiment 2 was designed to test these ideas. The methodology of Experiment 1 was again used, but with the addition of a condition with no delay between exposure and help seeking. I predicted that those in the delay condition would reproduce the findings of Experiment 1. However, in the no delay condition, I expected participants in each of the mere-exposure groups to help more frequently and with better quality than participants in the novel condition.

Experiment 2

Participants

One-hundred three introductory psychology students (41 males) participated for partial fulfillment of a course requirement.

Materials

All of the same stimuli from Experiment 1 were reused for Experiment 2.

Procedure

After participants were welcomed to the lab and consent was obtained, they were seated in a private cubicle for an ostensibly 30-minute long experiment. A computer informed participants that they would be completing several tasks related to working memory. During the first task all participants were presented a series of scrambled letter strings. Participants had 15 seconds to unscramble the letters into a word (e.g., oarancflii [California]). After 14 trials, participants were presented with the next task. For participants in the delay condition, the rest of the procedure is identical to Experiment 1. That is, these participants were next exposed to one of three sets of stimuli, completed a final filler task, and then were presented with the ostensible professor seeking help.

Participants in the no-delay condition, on the other hand, were next presented with the filler task from Experiment 1 in which they had to choose the capital of every U. S. state from four given cities. These participants were then exposed to one of the three sets of stimuli from Experiment 1, and then solicited for help. Thus, crucially, in the delay condition, participants complete a filler task between the exposure and helping tasks. However, those in the no-delay condition moved directly to the helping task following exposure.

The helping task itself also differed from Experiment 1. Namely, instead of being asked to write an essay for another professor, participants were asked to complete an optional survey. Specifically, the computer again notified all participants that they were free to see the experimenter for debriefing and leave the session, but if they had time, a male professor from the psychology department was soliciting help for a future study. The supposed professor's photo (subliminally presented five times to participants in the mere-exposure same condition earlier; never before presented to participants in the mere-exposure different and novel conditions) was displayed along with the following paragraph:

Hello! My name is Dr. Williamson. I need your help filling out a survey for an experiment I am conducting. You can answer as many or as few of the items as you like, but the more of the survey you complete, the more helpful to me it would be. However,

even one or two questions answered would be helpful. You can quit at any time and also feel free to skip any of the items that you wish. If you are willing to help me, please click “Yes I will help.” If you don't want to do this, please click “No, thanks.”

Note that by the time participants received this request for help that approximately 10 minutes had passed in the experimental session. Because participants signed up for what they believed would be a 30-minute experiment, they were not facing any time pressure. Thus, if participants refused to help, it was not for this reason. For those who agreed to help, they were then provided with the first item of a bogus survey. Survey questions were culled from preexisting scales unrelated to the current topic of interest. Forty-five of the items were measured with a seven-point Likert scale (1 = “Definitely not true of myself”; 4 = “Neither true, nor untrue, of myself”; 7 = “Definitely true of myself”). After every 15 Likert items, participants were provided 3 open-ended short answer questions (e.g., “Please describe your favorite subject in school.”). Thus, the survey consisted of 54 total items, 45 Likert-type items and 9 open-ended items. Having a mixture of Likert and open-ended items allowed me to again examine both help likelihood (whether participants chose to help) and help quality (how many total items were answered and how long/how much participants chose to write on the open-ended questions). Importantly, participants were not told how many items the survey contained. Additionally, participants were given the option with each item to either skip the question or stop answering questions entirely.

After completing the helping task, or if they refused to help, participants then provided basic demographic information, were probed for suspicion, debriefed, and thanked. Overall, the experiment employed a 2 (delay, no delay) $\times$ 3 (mere-exposure same, mere-exposure different, novel) factorial design, with both factors manipulated between-participants. Dependent measures consisted of whether participants chose to help and if so, how many items and of what type (Likert or short answer) participants completed. Moreover, I measured how long participants spent writing the short-answer questions.

Results

One participant was removed before analyses because her/his amount of time spent writing was greater than 3 SDs from the mean. This left 102 participants available for further analyses.

Help likelihood. It was predicted that after a delay participants in the mere-exposure same condition would be more likely to help than participants in the mere-exposure different and novel conditions. However, with no delay between exposure and help solicitation, it was predicted that both mere-exposure conditions would be more likely to help than the novel group. Accordingly, whether participants chose to help the ostensible professor was subjected to a chi-square analysis which produced a significant interaction $\chi^2(7) = 50.12, p < .001$ (see Table 2). Follow-up chi-square analyses indicated that when there was a delay between exposure and help solicitation, participants were more likely to help in the novel condition (89%) than the mere-exposure same (59%) and mere-exposure different conditions (60%), (all $\chi^2 > 3.718$, all $p \leq .05$), which did not differ from one another, $\chi^2(1) > 0.005, p = .946$. This pattern did not support predictions.

When there was no delay, participants were trending toward helping more in the mere-exposure different condition (78%) than the mere-exposure same condition (50%), $\chi^2(1) = 3.010, p = .083$, but not more than participants in the novel condition (63%), $\chi^2(1) = 0.952, p = .329$. Results for participants in the no delay condition also did not follow my predictions.

Help quality. Of the 102 participants available for analyses, 68 actually chose to help the ostensible professor. Thus, quality of help can only be assessed on those 68 participants. To remind, I measured how many items and of what type (Likert or short answer) participants completed and the time spent writing during the short answer questions. Each measure was log transformed. Moreover, number of answered Likert and number of answered short-answer questions were correlated ($r = .515, p < .001$) and thus standardized, combined, and averaged into a single index of questions answered.

I predicted that after a delay, participants in the mere-exposure same condition would offer greater quality help than participants in the mere-exposure different and novel conditions. However, with no delay, I expected both mere-exposure conditions to produce greater quality help than participants in the novel condition. A 2 (delay, no delay) $\times$ 3 (mere-exposure same, mere-exposure different, novel) factorial ANOVA, with both factors manipulated between-participants, revealed no significant effects on amount of questions answered (all $F < 1.567$, all $p > .215$, see Figure 2). However, the same analyses revealed a significant main effect of exposure type on time spent writing, $F(2, 36) = 6.397, p = .004$, indicating that participants wrote for a longer time in the mere-exposure different condition ($M = 49.366, SD = 5.727$) than participants in the mere-exposure same ($M = 25.172, SD = 5.249$) or novel conditions ($M = 22.664, SD =$

4.290). No other effects reached conventional levels of significance (all $F < 1.488$, all $p > .239$, see Figure 3). Again, these findings did not support predictions.

Discussion

There were several predictions for Experiment 2. First, for participants in the delay condition, I expected an exact replication of Experiment 1's findings. That is, I anticipated that participants in the mere-exposure same condition would be more helpful than participants in the other two conditions. On the other hand, for participants in the no delay condition, I expected to support my original prediction. Namely, I anticipated that participants in both mere-exposure conditions would produce greater help than those in the novel condition. Of course, as just shown, none of these predictions were confirmed.

Particularly noteworthy is the failure of the delay condition to replicate the findings of Experiment 1. The only meaningful change between this condition and Experiment 1 is the helping measure. This leaves a few potential reasons for the discrepant findings between Experiment 1 and the delay condition of Experiment 2. First, it is reasonable to suspect that the results of Experiment 1 were simply spurious, given that I was unable to replicate. Conversely, the failure to replicate may be the error. This is not entirely unreasonable. Experiment 2 has a smaller sample size and thus has less power than Experiment 1. Finally, the change in dependent variables between experiments may be exerting an effect. Help likelihood rates increased from 30% in Experiment 1 to 67% in Experiment 2. It is fairly obvious, then, that the dependent measure in Experiment 2 prompted more help. This is presumably because answering items on a survey required less effort than writing a short essay. Because most participants could easily provide some "help," perhaps by mindlessly answering a few questions, this measure may not have truly captured one's pro-social tendencies particularly well. Future attempts to reproduce the current results would help identify the potential source of inconsistency.

General Discussion

It was predicted that the experience of mere exposure would increase affect and that participants enjoying augmented mood would offer greater help to a target in need. The current results did not, however, corroborate this hypothesis. First, Experiment 1 failed to find any differences in help likelihood across conditions. That is, all participants, regardless of manipulation, were equally likely to offer help. And when examining help quality, only participants who were earlier exposed to the help-seeking target produced better help.

Experiment 2 examined one potential explanation for Experiment 1's findings, the effect of delay between exposure and help solicitation. Once again, it was thought that the filler task implanted between the exposure phase and the help solicitation task of Experiment 1 may have allowed time for participants' moods to decay. However, this second experiment's contribution is challenged, given that it failed to replicate the first and produced findings of its own that are difficult to interpret.

The failure to replicate is particularly troublesome. The only meaningful difference between Experiment 1 and the delay condition of Experiment 2 is that Experiment 2 used a different helping (dependent) measure. Participants were certainly less resistant to completing a survey (Experiment 2) than they were to writing a short essay (Experiment 1), as evidenced by the increase in help likelihood rates across experiments. With the majority of participants choosing to help in Experiment 2, it is possible that some sort of ceiling effect constrained the data. Of course, this is unlikely given that there was obviously enough variability between conditions to produce the atheoretical pattern that I obtained, in which help likelihood was greatest for those in the *novel* condition.

It is also possible that the survey items contained in Experiment 2's helping task exerted an influence on the help quality measure (they could not have influenced the help likelihood measure, as these items were not encountered until *after* the decision to help). However, though I have no pilot data on the matter, the items culled from the Rational-Experiential Inventory (Pacini & Epstein, 1999) and the Regulatory Focus Scale (Lockwood, Jordan, & Kunda, 2002) seem subjectively innocuous and thus unlikely to have altered mood or engendered other processes that may have interfered with my ability to replicate the findings of Experiment 1. Sample items include, "I have a logical mind." and "I often think about the person I would ideally like to be in the future." Moreover, even if these items were not relatively neutral in valance, I find it difficult to generate a plausible explanation for why these items would enable participants in the mere-exposure different group to produce better quality help than participants in the other two conditions.

Unfortunately, given the failure to replicate, it is difficult to believe the findings of either study. Therefore, future work should attempt to reproduce the current results to lend legitimacy to the findings and provide a springboard from which to explore other possibilities for the obtained effects. Step one, in my mind, would be to attempt an exact replication of Experiment 1.

If I can find the same results again, that those in the mere-exposure same condition offer better quality help than those in the other two conditions, I would be confident that these results are legitimate (i.e., not a simple type I error). Once firmly established, follow-up studies, like Experiment 2 here, could further explore why mere-exposure to a help-seeking target exerts a unique influence on helping that mere-exposure in general does not.

Indeed, this work is worth further exploration because of its potential unique and meaningful contribution to several literatures. First, though earlier work has examined the ability of previous exposure to another to facilitate one's *agreement* to help that person (Burger et al., 2001), no research to date has examined whether mere exposure influences one's *actual helping behavior* and/or the quality of that help. Also, it is debatable whether Burger and colleagues' work utilized a true *mere* exposure manipulation. In addition, as previously discussed, the mere-exposure effect has largely been shown on attitudinal and judgment measures. Though such evaluative shifts are interesting, showing that mere exposure/repetition manipulations can shape actual behavior, especially one so socially desirable, is perhaps more compelling.

This work continues to have implications for the pro-social literature as well. Namely, if mere exposure can be found to have an effect on helping behavior, researchers, educators, policy makers, and laypeople who are interested in improving humans' proclivities to help one another (arguably the most important pursuit of the social sciences) will be aided in their pursuit to shape programs and interventions aimed at facilitating charitable and generous acts.

Conclusion

In sum, the current experiments did not support my hypotheses. Moreover, some effects failed to replicate across experiments and others are difficult to meaningfully interpret. However, mere exposure and its potential effect on helping behavior is still a topic worthy of pursuit and exploration. Future research should attempt to concretely explain the current findings so that other work can endeavor to understand related questions.

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Footnotes

¹Twenty-five unique pattern masks were employed to prevent repeated viewing of the mask.

Tables and Figures

Table 1. Percentage of participants, by condition, who chose to help the ostensible professor (Experiment 1).

	Help (%)
Novel	38
ME – Same	24
ME – Diff	27

Table 2. Percentage of participants, by condition, who chose to help the ostensible professor (Experiment 2).

		Help (%)
Delay	Novel	89
	ME – Same	59
	ME – Diff	60
No Delay	Novel	63
	ME – Same	50
	ME – Diff	78

Figure 1. Help quality as a function of exposure condition. Y-axis represents z-score of log transformed data. Error bars indicate standard error of the mean.

Figure 2. Number of survey questions answered as a function of delay and exposure condition. Y-axis represents z-score of log transformed data. Error bars indicate standard error of the mean.

Figure 3. Time spent writing as a function of delay and exposure condition. Y-axis represents log 10 transformation of time spent writing in milliseconds. Error bars indicate standard error of the mean.