Crowded Minds: The Implicit Bystander Effect

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Five studies merged the priming methodology with the bystander apathy literature and demonstrate how merely priming a social context at Time 1 leads to less helping behavior on a subsequent, completely unrelated task at Time 2. In Study 1, participants who imagined being with a group at Time 1 pledged significantly fewer dollars on a charity-giving measure at Time 2 than did those who imagined being alone with one other person. Studies 2–5 build converging evidence with hypothetical and real helping behavior measures and demonstrate that participants who imagine the presence of others show facilitation to words associated with *unaccountable* on a lexical decision task. Implications for social group research and the priming methodology are discussed.

The *bystander apathy effect* is generally regarded as a well-established empirical phenomenon in social psychology (e.g., Darley & Latane, 1968; Latane & Darley, 1968; Latane & Nida, 1981). A person who faces a situation of another person in distress but does so with the knowledge that others are also present and available to respond is slower and less likely to respond to the person in distress than is a person who knows that he or she is the only one who is aware of the distress. The research demonstrating this effect, entirely appropriately, has manipulated the perceived presence of others and consistently has found this effect. Traditional theoretical accounts, such as the diffusion of responsibility explanation, focus on explaining how other actors present in the immediate situation influence would-be helpers' responses. A question left open that the present research seeks to begin to answer is at what level of calculational ideation this effect exists. In plainer English, does it depend on the respondent's calculations, either conscious or not, that there are other persons present who might help, or, at the other extreme, is it possible that simply imagining others may induce a similar mental state of diffused responsibility, regardless of whether those others are available to respond? Drawing on this latter perspective, our suggestion is that merely priming the presence of others at Time 1 can affect helping behavior on a completely unrelated task at Time 2, even when the primed others cannot possibly contribute to the helping task. If this is so, then the bystander apathy effect is not one that always must depend on a calculational effort but rather one that can be brought about by the priming of the presence of groups and the resulting mental state that is induced. Thus, real or imagined persons need not be built into the facets of a helping behavior situation for bystander apathy to occur.

Bystander Apathy

The classic bystander intervention studies (e.g., Clark & Word, 1974; Darley, Teger, & Lewis, 1973; Latane, 1970; Latane & Darley, 1968, 1970) have consistently shown that the presence of others inhibits helping behavior. However, current theoretical accounts stipulate that the immediate or imagined presence of others exerts its influence on helping because these others are involved in the situation at hand. In fact, if individuals know that immediate or imagined others cannot possibly help, then bystander apathy will not occur; individuals will behave as if alone (Bickman, 1972; Korte, 1971).

Along these lines, investigators have put forth several theoretical accounts of the bystander intervention findings. *Diffusion of responsibility* accounts (Darley & Latane, 1968) reflect the notion that as the number of people present in a situation increases, each individual feels less compelled or responsible to help. In fact, with

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so many people present, an individual might just assume that a victim is receiving help or that help is already on the way. *Social influence* (Darley & Latane, 1968; Darley et al., 1973) and *pluralistic ignorance* (Prentice & Miller, 1996) explanations, on the other hand, reflect the notion that people look to others to evaluate an emergency situation. If the others present are just standing around and appear calm, then would-be helpers infer that perhaps the situation really is not an emergency. Smoke spewing from a vent may just indicate a foggy vent, not a burning fire. Cacioppo, Petty, and Losch (1986) more recently offered another account for bystander apathy, namely, *confusion of responsibility.* This explanation argues that would-be helpers refrain from helping a victim in the presence of others because they do not want to be perceived as the perpetrator of the victim's pain and suffering. That is, in some cases, it might appear to observers that someone helping a victim is actually the cause of the victim's harm.

Although these situational accounts are compelling (for a review, see Latane & Nida, 1981) and are all likely contributors to bystander apathy, our purpose in this paper is to offer a *nonsituational* account that could provide an alternative or additional explanation for the findings. However, we hasten to add that situational and nonsituational accounts of bystander apathy need not be mutually exclusive. As we review, research on priming suggests that merely activating knowledge structures can affect social perception and behavior (e.g., Bargh, Chen, & Burrows, 1996; Higgins, Rholes, & Jones, 1977). In light of these findings, we seek to examine whether merely activating the construct of group in the minds of participants could also result in the bystander apathy effect. In particular, we are interested in examining whether activating the construct of a group of people who could not possibly help leads to an implicit bystander effect on a subsequent helping behavior.

Priming Affects Social Perception and Behavior

Bargh et al. (1996) defined priming as "the incidental activation of knowledge structures, such as trait concepts and stereotypes" (p. 230). Research has shown that the cognitive accessibility of these types of knowledge structures can have real and important effects on social perception and behavior (e.g., Carver, Ganellen, Froming, & Chambers, 1983; Dijksterhuis, Spears, & Lepinasse, 2000; Dijksterhuis & van Knippenberg, 1998; Epley & Gilovich, 1999; Higgins et al., 1977; Kawakami, Young, & Dovidio, 2000; Macrae & Johnston, 1998; Neuberg, 1988). More specifically, research on priming effects on social behavior has demonstrated that subtle cues or primes in the environment can subsequently affect behavior. For instance, in a new but already classic study, Bargh et al. (1996) found that priming the elderly stereotype can affect people's subsequent behavior. In their study, participants were primed through a scrambled sentence task. In the elderly condition, the sentences included words related to the elderly stereotype (e.g., *old, wrinkle, Florida*), whereas in the neutral condition, the scrambled sentences included age-nonspecific words. Findings showed that participants who were primed with the elderly stereotype walked out of the laboratory significantly more slowly than did participants in the neutral condition.

Similarly, other research looking at priming and behavior has found that individuals primed with the concept of professors perform better on a general knowledge test than do control participants (Dijksterhuis & van Knippenberg, 1998). The mechanism underlying these priming effects is based on the premise that triggering a knowledge structure such as a stereotype activates the semantic knowledge associated with it. This semantic knowledge includes, among other things, certain types of behaviors. For instance, part of the stored representation of elderly persons is that they walk slowly. Hence, when the construct or stereotype of an elderly person is primed in an individual, the behavioral representation is activated, and the individual walks more slowly.

In addition to influencing behavior directly, priming can also affect social perception and, as a consequence, influence behavior indirectly. In these cases, priming affects people's internal cues, which then spill over into their perceptions of other people and the social world. For instance, Higgins et al. (1977) demonstrated that priming individuals with concepts related to hostility leads them to perceive an ambiguous target person (e.g., Donald) as more hostile compared with individuals who do not receive hostility primes. Other research on *self-schemata* (e.g., Markus, 1977) and *chronic accessibility* (e.g., Bargh & Thein, 1985) speaks to the idea that people can have chronic cognitive filters that affect social perception. Research on relational schemas (Baldwin, 1994, 1995; Baldwin, Fehr, Keedian, Seidel, & Thomson, 1993; Baldwin, Keelan, Fehr, Enns, & Koh-Rangarajoo, 1996) also demonstrates that primes can affect social perception. Although the mechanism of these primes on social perception is similar to that of social behavior, one critical difference is that these primed or activated social representations color one's perspective of the world and subsequently have indirect effects on one's behavior. Behavioral primes, on the other hand, are directly linked to actual behaviors.

To better understand how primes can affect behavior indirectly through influencing social perception, we can look at competitive primes used in a Prisoner's Dilemma Game (Neuberg, 1988). In this study, participants were primed with either competitive or neutral primes and then began rounds of a Prisoner's Dilemma Game with another phantom participant. Participants made their first move before learning the move of the phantom participant. Results showed that the competitive primes had no effect on the participant's first move, indicating that the primes did not directly influence participants' behavior. However, participants with predisposed competitive dispositions who received competitive primes exhibited more competitive behavior on subsequent trials than did those participants with competitive predispositions who received neutral primes. These findings suggest that the competitive primes did not directly lead to competitive behavior but rather affected the participants' behavior indirectly through their distorted social perception of the phantom partner. In other words, the competitive lenses of these participants led them to perceive the phantom partner as being competitive only after they learned the partner's move. Their own behavior was affected when they responded in kind to what they perceived was a competitive partner.

Priming a Social Context

Research has clearly demonstrated that priming knowledge structures such as trait concepts, stereotypes, and behavior-related words can have real and important effects on both social perception and behavior. But what of more macro situations? Is it

possible to prime a social context? For instance, can simply imagining being in a group at Time 1 affect behavior at Time 2? To this end, the present study examines whether it is possible to prime a social context or a psychological situation.

In a clever study, Epley and Gilovich (1999) examined whether priming could influence how people react to a social context, namely an Asch-like conformity situation. In their study, participants first completed a computer priming task in which they viewed either conformity (e.g., *comply, conform, obey*) or nonconformity (e.g., *challenge, defy, deviate*) primes. After completing the computer exercise, participants were led into another room with confederates in a conformity situation and were asked to rate attitude questions that were projected on a screen. Findings showed that those who were primed with conformity-related words were more likely to conform than were those primed with nonconformity words. This study was an important step in examining the effects of priming on how people behave in a social context. The present research seeks to build on this study by examining whether a social context itself can be brought to mind and, in turn, influence behavior.

The Implicit Bystander Effect

Bystander apathy research shows that people do not always help in emergency situations, and the likelihood of each persons' helping decreases as the physical presence of other people becomes increased. Diffusion of responsibility accounts (e.g., Darley & Latane, 1968; Latane & Darley, 1968) explain that being in a group leads one to feel as if one is less responsible for helping or taking action, and this feeling of diminished responsibility or accountability then leads to inaction. Logically, then, any manner in which this feeling of diminished responsibility can be introduced should lead to similar behavior—inaction. For instance, priming notions of unaccountability should lead the concept to become more cognitively accessible. Once such increased accessibility, or perceptual fluency, occurs, it is likely to structure the manner in which people respond to situations. When a helping situation arises, people may search internal cues to determine whether they should help. If unaccountability has just been primed, they may help less.

We propose that being in or simply thinking about a group is enough to activate this construct, because part of the concept of being in a group is the notion of being lost in a crowd, being deindividuated, and having a lowered sense of personal accountability. Thus, although physically being in a group might lead one to experience these feelings, simply priming the notion of being in a group should also trigger or activate these concepts. Just as triggering a stereotype activates all the semantic knowledge associated with it, triggering an abstract concept such as being in a group should activate whatever knowledge is associated with that construct. In stereotyping research, we know that thinking about the elderly leads to notions of walking slowly being triggered despite these notions not being directly primed (Bargh et al., 1996). Similarly, thinking about African Americans leads to notions of hostility being triggered, despite these notions not being directly primed. Therefore, priming the psychological situation of being in a group should trigger the concepts associated with that situation. If this includes the sense of diminished responsibility and loss of accountability, as research suggests (e.g., Darley & Latane, 1968; Latane & Darley, 1968), then these notions should be triggered just as when one is in the physical presence of a group. Hence, priming the psychological situation of being in a group should lead to a bystander apathy effect whereby people are less likely to help because their internal cues reflect the increased fluency of unaccountability and suggest to them that helping is not required. We propose that implicit activation of concepts related to unaccountability operates much the same as explicit activation through physical presence in the midst of many others.

Overview

In the first three studies, we seek to establish the parallel that the implicit activation of the group leads to the same effect as does physically manipulating the presence of others. Hence, our central hypothesis is that individuals who merely imagine being in a group will exhibit less helping behavior on a subsequent, completely unrelated task. In the later studies, we seek to establish the mechanism underlying this proposed effect. Much like past research has shown that specific traits and behaviors are triggered by the activation of a broader stereotype, we assume that the specific concept of unaccountability and diffused responsibility is triggered by the activation of the concept of being with a group. To test this mechanism, we directly assess whether thinking about a group leads to heightened accessibility of these concepts.

Study 1

Method

Dining out study. In Study 1, we attempt to identify a linear relationship with number of people imagined and helping behavior. Previous research provides evidence that diffusion of responsibility can affect charitable donations (e.g., Wiesenthal, Austrom, & Silverman, 1983), so we decided to operationalize helping behavior in Study 1 as hypothetical contributions to charity. Previous research has also found that people respond similarly to hypothetical scenarios as they do to real helping situations (e.g., J. Baron & Miller, 2000; Davis, Mitchell, Hall, Lothert, Snapp, & Meyer, 1999; Laner, Benin, & Ventrone, 2001). Our prediction is that the more people participants imagine at Time 1, the less helping behavior they will exhibit at Time 2. More specifically, we predict that people who imagine being with a group of 30 people will exhibit less helping behavior at Time 2 than will those who imagine being with a group of 10 people, who, in turn, should exhibit less helping behavior than will those in the 1-person and neutral control conditions.

Participants. A total of 129 undergraduates from Princeton University participated in a between-subjects (social context: group of 30/group of 10/1 person/neutral control) questionnaire study. This questionnaire was part of a larger questionnaire packet for a Questionnaire Day hosted by the psychology department, and the key pages were imbedded in the packet itself. Participants were paid $8 for completing the entire questionnaire packet.

Procedure. Participants completed a two-page questionnaire. On Page 1, the *group of 30 condition* read as follows: "Imagine you won a dinner for yourself and 30 of your friends at your favorite restaurant." The *group of 10 condition* was identical but referenced 10 friends. The *one person control condition* was similar but focused on only one friend: "Imagine you won a dinner for yourself and a friend at your favorite restaurant." Next, all participants answered the filler question: "What time of day would you most likely make your reservation?" The choice set

included the following times: 5 PM, 6 PM, 7 PM, 8 PM, 9 PM, or 10 PM.[1] Because this study was conducted within a larger questionnaire, our *neutral control condition* was simply the helping behavior dependent measure.

On Page 2, all participants read the helping behavior dependent measure, entitled Annual Charity Contribution. It read as follows: "Imagine you have long since graduated from college. What percentage of your annual after-tax earnings would you be willing to donate to charity? (Please Check)." At this point, participants could check one of the following percentage ranges: *1% or less, 2%–3%, 4%–5%, 6%–10%, 11%–15%, 16%–20%, 21%–25%,* or *over 25%.* On completion of the entire questionnaire packet, participants were paid for their time.

Results and Discussion

To test our a priori prediction that a linear pattern would emerge such that participants in the group of 30 condition would pledge less of their income to charity than would those in the group of 10 condition, who in turn would pledge less than those in the 1 person and neutral control conditions, we assigned the following contrast weights to the conditions: −19.5 (group of 30 condition), 0.5 (group of 10 condition), 9.5 (1 person control condition), and 9.5 (neutral control condition). A contrast analysis suggests that our data significantly fit this linear pattern, $F(1, 125) = 4.14$, $p < .05$, and the residual was not significant, $F(2, 125) = 0.63$, $p > .5$. That is, the pattern suggests that participants who imagined a group of 30 people pledged less to charity than did those who imagined a group of 10 people, who in turn pledged less than did those who imagined one person or those in the control condition. See Table 1 for the means.

These results are consistent with the literature on diffusion of responsibility in that helping behavior decreased as the numbers of others imagined increased. However, these results shed new light on the diffusion of responsibility literature and suggest that imagined others need not be immediately present or connected to a helping behavior situation for the diffusion effect to occur. Indeed, merely imagining being with others at Time 1 was sufficient to create this diffusion effect.

Study 2

Method

The movies study. Whereas participants in Study 1 imagined being with friends, we attempt to show in Study 2 that when participants are primed with a situation that includes a group of strangers or being in the presence of a crowd they will likewise demonstrate less helping behavior. We compare results from the group prime condition with results from two control conditions, one in which participants were primed with one other person, and one in which they were not primed. As for our prediction, we hypothesized that individuals who imagine being in a crowded situation with many other people would pledge significantly fewer dollars to a university's annual giving campaign, compared with those individuals in our control conditions, which should not differ significantly from each other.

Participants. A total of 38 Harvard University and Princeton University undergraduates participated in a between-subjects (social context: group/one person/neutral control) questionnaire study. Data from Harvard University participants were collected as part of a larger questionnaire packet during Questionnaire Day, and the key pages were imbedded within the packet. Data from the Princeton University participants were collected at the campus student center; only students who were sitting alone were approached to ensure that participants would not talk or confer with each other while completing the short questionnaire.

Procedure. Participants read one of three possible scenarios, two of which were about being at a movie theater. We chose the setting of a movie theater because we wanted the imagined setting to be away from the college campus. These scenarios manipulated whether participants thought about being with a friend in a crowded movie theater (*group condition*) or whether they thought about being alone with a friend in a movie theater (*one person control condition*). The group condition read as follows: "Imagine that you and a friend are sitting in a crowded movie theater. There are people in front of you, behind you, and to your sides. Although there are some children, the audience is mostly adults, and you are just watching the movie previews." The one person control condition was identical except that participants read about being alone with a friend at a movie theater: "Imagine that you and a friend are sitting alone in a movie theater. You and your friend have the entire theater to yourself, and you are just watching the movie previews." After reading the scenarios, all participants answered a filler question on a 7-point scale: "What room temperature would you prefer for the theater?" (1 = *very cool*; 7 = *very warm*). For the Princeton sample, the *neutral control condition* was simply, "What room temperature do you prefer in general?" (1 = *very cool*; 7 = *very warm*). The neutral control condition for the Harvard sample was simply the dependent variable below, as this dependent measure was already part of a larger questionnaire packet.

On Page 2, participants completed the dependent variable. Helping behavior was operationalized through an annual giving questionnaire. All participants read the following:

> Annual Giving is the yearly appeal (Harvard/Princeton) makes to all alumni, parents and friends for unrestricted funds which can be used immediately to meet the University's most important needs and opportunities. Now imagine you have long since graduated from (Harvard/Princeton). Of the following, which level of contribution do you foresee as your participation? (Please Check).

At this point, participants checked off one of the following dollar ranges: *$0, $1–49, $50–99, $100–249, $250–499, $500–999, $1,000–2,499,*

Table 1
Mean Charity Contributions by Condition

Condition	Charity level	
	M	*SD*
Group of 30	3.6	1.3
Group of 10	3.9	1.5
One person and neutral controls	4.2	1.6

Note. Response options ranged from 1 = *1% or less to charity* to 8 = *over 25%.*

[1] To rule out the possibility that making a dinner reservation entails more responsibility in our group prime than it does in our one person prime, we pretested these primes by asking 40 undergraduates to read the following in a between-subjects questionnaire: "Imagine you won a dinner for yourself and (1 or 10) of your friends at your favorite restaurant. You are calling to make the reservation for a time between 5 PM and 10 PM." We then asked them, "How important is your choice of reservation time?" (1 = *not important at all*; 7 = *extremely important*). Results showed that the importance ratings of those who read the one person prime ($M = 4.35$, $SD = 1.87$) and the group prime ($M = 4.15$, $SD = 1.09$) did not significantly differ from each other ($p > .68$), which suggests that the decision to make a dinner reservation time was no more important in the group condition than in the one person condition.

$2,500–4,999, $5,000–9,999, and *$10,000 and above.* These dollar ranges were treated as a continuous variable (1 = *$0*; 10 = *$10,000 and above*) in our analyses.

After completing the questionnaire, participants were thanked for their time and debriefed.

Results and Discussion

To test our a priori prediction that individuals in the group condition would pledge significantly fewer dollars than would those in the one person and neutral control conditions, we compared the mean of the group condition ($M = 4.57$, $SD = 2.28$) with that of the one person and control conditions ($M = 6.50$, $SD = 2.62$) and found a significant difference in the predicted direction, $F(1, 35) = 5.20$, $p < .05$. A follow up a priori comparison also confirmed that the one person ($M = 6.92$, $SD = 2.43$) and neutral control conditions ($M = 6.08$, $SD = 2.84$) did not significantly differ from each other, $F(1, 35) = 0.66$, $p > .4$. The College × Social Context interaction was not significant ($p > .5$), which suggests that the Harvard and Princeton samples yielded similar data patterns.

These results suggest that participants primed with the presence of many other people exhibited less helping behavior than did those who imagined being with one other person or those who were not primed. The pattern of results in this study gives credence to the argument that diffusion of responsibility, not self-awareness, is the best explanation for the effect. That is, a possible explanation for a difference between the one person and group conditions could be that there is an increase in helping behavior in the one person condition, as opposed to a decrease in the group condition. This alternative explanation may be plausible if we inadvertently manipulated self-awareness (e.g., Duval & Wicklund, 1972) in the one person condition. However, because participants in the group condition pledged fewer dollars than did those in the one person and neutral control conditions, which did not significantly differ from each other, the implication is that diffusion of responsibility underlies this effect, as opposed to increased self-awareness.

Moreover, this study demonstrates how diffusion of responsibility can spill over into a helping task for which the imagined others are not likely contributors or would-be helpers. That is, imagining a crowded movie theater arguably evokes thinking about a bunch of strangers. So, when asked about contributing to a university's annual giving campaign, participants should not logically expect strangers in a movie theater to have any motivation or compunction to donate to a Harvard or Princeton annual giving campaign. However, visualizing these imagined strangers still affected donation pledge levels. Hence, it seems that priming the presence of others can create an implicit bystander effect that spills over into a subsequent helping task.

Study 3

Method

Dining out study—real helping. In the next study, we wanted to replicate the finding that merely imagining the presence of others at Time 1 affects helping behavior at Time 2. We also wanted to generalize this effect to real helping behavior. That is, whereas Studies 1 and 2 demonstrated the effect with hypothetical giving, it is important to demonstrate it with real helping behavior. Again, our central hypothesis is that those primed with the presence of others will offer less helping behavior. We again compared results from the group condition with results from two control conditions, one in which participants imagined one other person, and one in which they were not primed.

Participants. A total of 129 undergraduates from Harvard, Princeton, and Rutgers participated in a between-subjects (social context: group/one person/neutral control) questionnaire design. Participants were recruited at campus student centers. Again, only those who were sitting by themselves were approached to prevent communication among participants.

Procedure. The participants completed a two-page questionnaire, which included the dining out primes of Study 1. On Page 1, the *group condition* read as follows: "Imagine you won a dinner for yourself and 10 of your friends at your favorite restaurant." The *one person control condition* was identical but focused on one friend: "Imagine you won a dinner for yourself and a friend at your favorite restaurant." Next, all participants answered the filler question: "What time of day would you most likely make your reservation?" The choice set included the following times: 5 PM, 6 PM, 7 PM, 8 PM, 9 PM, or 10 PM. The *neutral control condition* participants only read the filler question, which was slightly modified to "What time of day would you make a dinner reservation?"

On Page 2, we operationalized real helping behavior as volunteering to help out with an experiment being conducted in another room. More specifically, all participants read the following on Page 2: "In addition to this survey, we are conducting a brief experiment in another room. How much time are you willing to spend on this other experiment?" At this point, participants checked off one of the following minute intervals: *0 minutes, 2 minutes, 5 minutes, 10 minutes, 15 minutes, 20 minutes, 25 minutes,* and *30 minutes.*

After the questionnaires were collected, participants who had volunteered their time were ready (and some eager) to participate in another experiment. However, at that point, we explained to them our hypothesis and that there was actually no other experiment.

Results and Discussion

As in the previous studies, we made the a priori prediction that the group condition would differ significantly from the one person and neutral control conditions and that the one person and neutral control conditions would not differ significantly from each other. A comparison of the mean of the group condition ($M = 2.34$, $SD = 2.99$) with the mean of the one person and control conditions ($M = 4.36$, $SD = 6.15$) showed a significant difference in the predicted direction, $F(1, 126) = 3.98$, $p < .05$ (see Figure 1). A follow-up comparison confirmed that the one person control condition ($M = 4.93$, $SD = 7.07$) and the neutral control condition ($M = 3.80$, $SD = 5.09$) were not significantly different from each other, $F(1, 126) = 0.99$, $p > .3$.

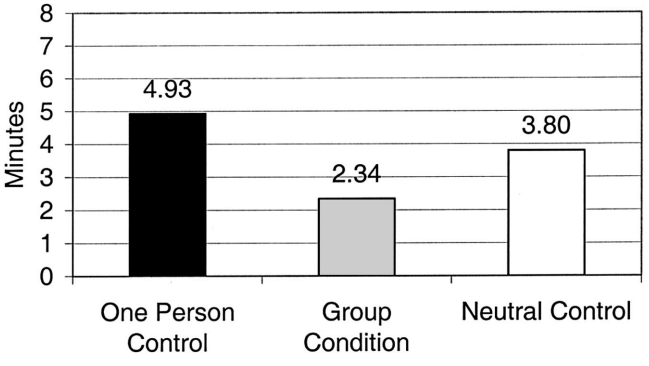

Figure 1. Study 3 helping behavior by condition.

These results suggest that participants who imagined a group of 10 people at Time 1 offered less helping assistance at Time 2 than did control participants, who either imagined 1 other person or saw no prime. Again, this data pattern is consistent with a diffusion of responsibility interpretation, as opposed to a self-awareness one, as the 1 person and neutral control conditions are not significantly different from each other. Another interesting aspect of this study is, again, the disconnect between those imagined and those who can help. That is, even though the participants in the group condition imagined their friends, these imagined friends were not in the immediate vicinity to offer helping behavior assistance. Hence, these results suggest that others need not be physically present for diffusion of responsibility to be created. Although we have demonstrated this implicit bystander effect with both real and hypothetical forms of helping behavior, in the next set of studies, we explore in more detail the mechanism or psychological processes behind the effect.

Study 4

Method

As discussed earlier, although the concept of reduced helping per se is not likely to be a part of the stored mental representation of a group or crowd, it is possible that the conceptual construct of being with a group or in the presence of others is linked to specific notions of deindividuation, being lost in a crowd, and a lowered sense of personal accountability. Rather than priming these constructs directly in the first studies, we assumed that they were triggered by thinking about being in a group. Much like past research has shown that specific traits and behaviors are triggered by the activation of a broader stereotype, we assume that the specific concept of diffused responsibility is triggered by the activation of the concept of being with a group. To test this assumption, in Study 4 we directly assess whether thinking about a crowd leads to the heightened accessibility of concepts related to unaccountability. Hence, we seek evidence that concepts related to unaccountability and lack of responsibility are more accessible after participants imagine being with a group than after they imagine being with one other person.

Additionally, another interesting possibility is that thinking about being in a group or crowd triggers feelings of comfort and safety in numbers, and this might be what is disrupting helping behavior. Feeling a sense of comfort might lead one to interpret a helping situation as less in need of help. That is, when one feels comfortable and secure, one may look to an ambiguous helping situation, project one's own feelings, and see a situation that is more secure and comfortable than one might have otherwise surmised. If this explanation is correct, then participants in the group condition in Study 3 helped us out less with our study in the other room not because they felt less accountable but instead because they perceived us as being less in need than did those in the control conditions. Hence, to investigate this possibility, we also examined whether notions of safety and comfort become more accessible as a consequence of thinking about a group.

Participants. A total of 52 Lehigh University undergraduates participated in a between-subjects design (social context: group/one person/neutral control) study for course credit.

Procedure. Participants were escorted to individual experimental rooms equipped with computers. Participants then followed self-guided instructions through the software program MediaLab (Jarvis, 2002). MediaLab instructed participants that they would be completing a computer exercise in which letter strings would flash on the screen. The letter strings themselves would either be words or nonwords. Participants were told to press the keyboard key marked *yes* if the string of letters was a word or the key marked *no* if the string was a nonword. At this point, all participants performed a lexical decision practice exercise that included 30 trials—15 words (e.g., *chair, table, door, knob*) and 15 nonwords. All trials began with an orienting stimulus (+) that was presented in the center of the screen for 250 ms and was followed by a blank screen for 400 ms. The letter string (word or nonword) then appeared in the center of the screen for 50 ms. Once participants indicated word or nonword, another trial began.

After this trial session, the computer introduced participants to our manipulation. Participants in the group and one person control conditions read the movie scenario used in Study 2. Participants were randomly assigned to read one of the versions associated with the following conditions: *group condition* ("Imagine that you and a friend are sitting in a crowded movie theater . . . ") and *one person control condition* ("Imagine that you and a friend are sitting alone in a movie theater . . . "). Participants in the *neutral control condition* read no version. After reading the scenario, participants answered the filler question on room temperature described in Study 2.

After this experimental manipulation, the computer instructed participants that they would do another lexical decision task. Forty-two letter strings were then presented in a random order. Again, each word or nonword was preceded by an orienting stimulus (+) that appeared for 250 ms and was followed by a blank screen for 400 ms. The word or nonword was then presented in the center of the computer screen for 50 ms.

Because most priming studies show facilitation effects, for our responsibility composite, we chose single words that were related to the concept of unaccountability. For our main unaccountability composite, we selected three synonyms of unaccountability—*unaccountable, innocent,* and *exempt.*[2] These words were matched to three neutral words representing the same degree of positivity to form a matched neutral composite (e.g., *whimsical, impenetrable*).

Six comfort-related words were also included: *protected, safe, unthreatened, secure, comforted,* and *peaceful.* These words were also matched to 6 neutral trait words (e.g., *diligent, intelligent*). Finally, 5 neutral filler nouns (*table, mountain, flower, program,* and *compass*) and 19 nonwords were included.

On completing the computer task, participants were thanked for their time and then debriefed about the purpose of the experiment.

Results

Data reduction. To analyze the reaction time data, we followed procedures outlined in Bargh and Chartrand (2000). Data points that were three standard deviations above or below the mean for each word were considered outliers and were dropped from subsequent analyses. This affected less than 3% of the data points. Because reaction time data are often skewed, we applied a log transformation to normalize the data to meet the assumptions of the statistical tests. Although the log transformed values were used in the analyses, we report the actual reaction time means in milliseconds.

Filler nouns. We analyzed participants' reaction times to the filler nouns first to ensure that participants in the group and control conditions did not show a difference in overall reaction time by condition. Results showed that there were no significant differences in response times to the filler nouns (group condition, $M = 516.00$, $SD = 129.00$; control conditions, $M = 526.00$, $SD = 99.00$), $F(1, 49) = 0.42$, $p = .52$.

[2] We considered including the term *irresponsible;* however, this term has a slightly different connotation than *not responsible.* More specifically, *irresponsible* seems to evoke thoughts of incompetence, whereas our intended usage of *not responsible* does not.

Unaccountable composite. Our a priori prediction was that individuals in the group condition would respond more quickly to the unaccountable composite words than would participants in the one person and neutral control conditions, whereas the two groups would show equal response times to the matched neutral words. To test this prediction, we conducted a planned contrast using standardized contrast scores (*L* score) computed for each participant. We multiplied the following contrast weights to our dependent measures: 1 (unaccountable words) and −1 (matched neutral words). This procedure resulted in *L* scores representing the sum of each participant's residual score after each score had been multiplied by the appropriate contrast weight (Rosenthal & Rosnow, 1985; Rosenthal, Rosnow, & Rubin, 2000). An analysis of these scores by type of prime yielded a significant difference in the predicted direction, $F(1, 50) = 5.27$, $p < .05$.[3] Participants in the group condition responded more quickly to the unaccountable composite words ($M = 517.00$ ms, $SD = 129.00$ ms) relative to those in the one person and neutral control conditions ($M = 587.00$ ms, $SD = 151.00$ ms). In contrast, those in the group ($M = 559.00$ ms, $SD = 158.00$ ms) and control ($M = 558.00$ ms, $SD = 140.00$ ms) conditions showed similar reaction times to the matched neutral words. See Figure 2 for means. A simple effects test on the unaccountable composite only, which does not take into account baseline reaction times to the matched neutral words, showed a marginally significant difference between the group condition and control conditions in the predicted direction, $F(1, 49) = 2.96$, $p = .09$. A simple effects test on the matched neutral composite showed no significant difference by condition, $F(1, 49) = 0.05$, $p = .81$, as expected.

We conducted a follow-up planned comparison on the *L* scores to ensure that the two control conditions did not differ from each other. This comparison indicated that the two control conditions (one person and neutral control) were not significantly different from each other, $F(1, 29) = 0.00$, $p = .97$.

Comfort composite. To test the possibility that thinking about a group has its effect on helping behavior because group or crowd primes thoughts about comfort, we performed a similar contrast using *L* scores. We multiplied the following contrast weights to our comfort dependent measures: 1 (comfort words) and −1 (matched neutral words). Results from this analysis did not reveal a significant difference by experimental condition, $F(1, 50) = 0.17$, $p = .68$.

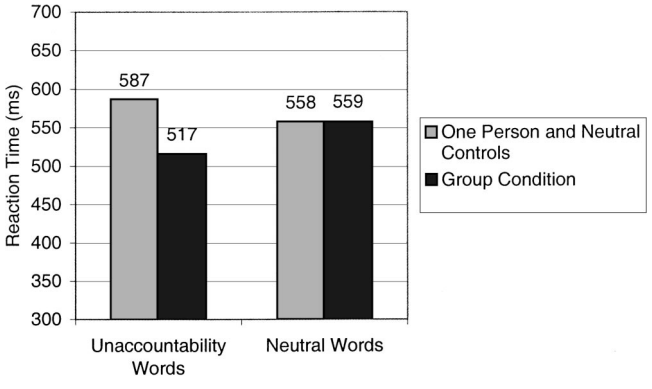

Figure 2. Study 4 reaction time (in ms) by condition and composite.

Discussion

In light of these findings, it appears that merely imagining the presence of others or a crowded situation leads participants to respond more quickly to words related to the concept of unaccountability relative to matched neutral words. The fact that the crowded and control participants responded equally quickly to the matched neutral words indicates that this effect is specific to words related to unaccountability and is not due to participants in the group condition responding more quickly across the board. Hence, it seems that the increased accessibility of the concept *unaccountable* may be the mechanism by which priming the presence of others produces less helping on a subsequent, unrelated task. In addition, results from Study 4 are not consistent with the comfort hypothesis. Participants primed with a crowded situation did not respond significantly more quickly to words related to comfort than did those in the one person or neutral control conditions.

Study 5

Our purpose in Study 5 was twofold. First, we wanted to conceptually replicate the results of Study 4 to ensure the reliability of the reaction time findings. Second, we wanted to investigate whether the comfort hypothesis would be supported if we used a stronger group prime. For this reason, we not only had participants read the movie theater primes used in Study 4 but also asked them to list their thoughts about the theater for 3 min. The thought listing as a prime strength manipulation was modeled after one used by Dijksterhuis and van Knippenberg (1998), which showed that the longer individuals are exposed to primes, the stronger the effects are.

Method

Participants. A total of 74 Princeton University undergraduates participated in a between-subjects design (social context: group/one person/neutral control) study for course credit. Participants were run in individual experimental rooms.

Procedure. The entire procedure of Study 5 was the same as that used in Study 4, except for the prime manipulation. After the practice lexical decision session, the computer introduced participants to our manipulation. Participants in the group and one person control conditions read the movie scenario used in Studies 1 and 4—*group condition* ("Imagine that you and a friend are sitting in a crowded movie theater . . . "), and *one person control condition* ("Imagine that you and a friend are sitting alone in a movie theater . . . "). They were then given a blank sheet of paper and were asked to list as many thoughts as came to mind for 3 min. Participants in the *neutral control condition* read no version and went directly to the reaction time task. The lexical decision task used the same words and the same specifications as did Study 4.

Results and Discussion

Data reduction. Before analyzing the reaction time data, we again followed procedures outlined in Bargh and Chartrand (2000) for dealing with reaction time data. Data points that were three standard deviations above or below the mean for each word were

[3] Note that we use the error term only for the groups involved in the between-subjects comparison (group/one person and neutral control) when the *L* scores involve a within-subject comparison.

considered outliers and were replaced with missing values in subsequent analyses. This procedure again affected less than 3% of the data points. Because reaction time data are often skewed (for a discussion, see Bargh & Chartrand, 2000), we applied a log transformation to normalize the data to meet the assumptions of the statistical tests. Again, although we used logs in the analyses, we report the actual means in milliseconds.

Filler nouns. We analyzed participants' reaction times to the filler nouns first to ensure that participants in the group and control conditions did not show a difference in overall reaction time by condition. Results showed that those in the group condition (M = 436.00, SD = 73.00) and the control conditions (M = 446.00, SD = 94.00) responded equally to the filler nouns, $F(1, 71) = 0.23$, $p = .63$.

Unaccountable composite. Our a priori prediction was that individuals in the group condition would respond more quickly to the unaccountable composite words than would participants in the one person and neutral control conditions, whereas the two groups would respond equally quickly to the matched neutral words. To test this prediction, we again conducted a planned contrast using a standardized contrast score (L score) computed for each participant. We multiplied the following contrast weights to our dependent measures: 1 (unaccountable words) and −1 (matched neutral words). This resulted in L scores representing the sum of each participant's residual score after each score had been multiplied by the appropriate contrast weight. An analysis of these scores by experimental condition yielded a significant difference in the predicted direction, $F(1, 72) = 4.74$, $p < .05$. A simple effects test on the unaccountable composite words only showed that participants in the group condition responded more quickly to the unaccountable composite words (M = 429.00 ms, SD = 99.00 ms) than did those in the two control conditions (M = 476.00 ms, SD = 95.00 ms), $F(1, 71) = 5.20$, $p < .05$. In contrast, those in the group (M = 450.00 ms, SD = 101.00 ms) and control (M = 461.00 ms, SD = 94.00 ms) conditions showed similar reaction times to the matched neutral words, $F(1, 71) = 0.33$, $p = .57$. See Figure 3 for the means.

A follow-up comparison to ensure that the two control conditions did not differ in their L scores again showed that the two control conditions (one person and neutral control) were not significantly different, $F(1, 45) = 0.07$, $p = .80$.

Comfort composite. To examine the possibility that thinking about a group has its effect on helping behavior because group or crowd primes thoughts about comfort, we performed a similar contrast using L scores. We multiplied the following contrast weights to our comfort dependent measures: 1 (comfort words) and −1 (matched neutral words). Although the pattern of means was in the predicted direction, an analysis of these scores by experimental condition did not reveal a significant difference by condition, $F(1, 72) = 1.40$, $p = .24$. Despite the stronger prime, results from this analysis did not reveal a significant effect of social context on comfort L scores.

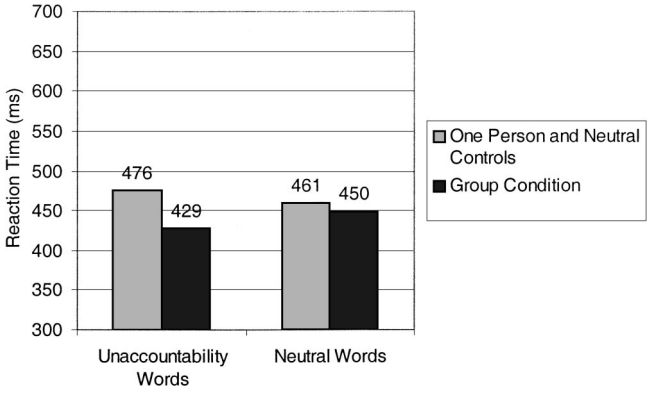

Figure 3. Study 5 reaction time (in ms) by condition and composite.

Exploring unaccountable and comfort. To further examine the relationship between unaccountable and comfort, we ran some additional regression analyses. Because Studies 4 and 5 were virtually identical (except for the duration of the prime), we collapsed the data from the two studies to increase the power for the analyses. Before collapsing the data, we first transformed the data from each of the two studies into standardized z scores and then concatenated these data sets.[4] Our goal for these analyses was twofold. First, we were interested in whether a boost in power would lend support to the comfort hypothesis and, if so, whether this relationship would hold when participants' reaction times to the unaccountable composite were held constant. Second, we wanted to investigate whether the relationship between social context (one person and neutral control/group) and unaccountable would hold if we controlled for participants' response times to the comfort composite.

To address these questions and thus further investigate the mechanism of the implicit bystander effect, we followed the procedures outlined in R. M. Baron and Kenny (1986) and Kenny, Kashy, and Bolger (1998). First, we were interested in whether the relationship between social context and the comfort composite would become significant with the boost in power brought about by collapsing the two studies. To test this, we dummy coded social context (1 = one person/control; 2 = group) and used it as the independent variable in the analyses. Regression analyses indeed showed that social context had a significant relationship with the comfort composite ($\beta = -.18$, $p < .05$). Then we tested whether the relationship between social context and the comfort composite would remain significant when we controlled for the unaccountable composite by treating it as a mediator. Social context had a significant relationship both with participants' reaction times to the comfort composite ($\beta = -.18$, $p < .05$) and with participants' reaction times to the unaccountable composite (the mediator in this case; $\beta = -.25$, $p < .01$). The unaccountable composite also had a significant relationship with the comfort composite while controlling for the effect of social context ($\beta = .73$, $p < .001$). However, when participants' reaction times to the unaccountable composite were controlled, the relationship between social context

[4] Even before standardizing the scores, we initially concatenated the raw data from Studies 4 and 5 to verify that there were no significant interactions between social context (group/one person and neutral control) and school (Lehigh/Princeton) across our key dependent measures. All Fs were less than 1.00; unaccountable composite, $F(1, 122) = 0.05$, $p > .82$; comfort composite, $F(1, 122) = 0.05$, $p > .80$; unaccountable matched neutral, $F(1, 122) = 0.01$, $p > .90$; and comfort matched neutral, $F(1, 122) = 0.21$, $p > .64$.

and comfort dropped dramatically ($\beta = .01$, $p = .94$).[5] A further analysis using the ratio of *ab* over the standard error of *ab* (see R. M. Baron & Kenny, 1986) revealed that including the unaccountable composite as a mediator produced a significant reduction in the relationship between condition and comfort (ratio = 2.76, $p < .01$).

The purpose of our second analysis was to test whether the relationship between social context and the unaccountable composite would remain significant when we controlled for the comfort composite by treating it as a mediator. Social context had a significant impact both on participants' reaction times to the unaccountable composite ($\beta = -.25$, $p < .01$) and on participants' reaction times to the comfort composite (the mediator in this case; $\beta = -.18$, $p < .05$). The comfort composite also had a significant relationship with participants' reaction times to the unaccountable composite when social context was controlled for ($\beta = .71$, $p < .001$). Results were not consistent with a mediational interpretation, however, because analyses showed that the relationship between social context and the unaccountable composite remained statistically significant, even when the comfort composite was controlled for ($\beta = -.13$, $p < .05$).[6]

These analyses suggest an intriguing dynamic between the concepts of unaccountable and comfort and suggest that the diffusion of responsibility explanation may provide a tighter account of the implicit bystander effect than does a comfort explanation. That is, a significant relationship exists between unaccountable thoughts and social context, even when one controls for words associated with comfort. However, comfort no longer has a significant relationship with social context when one controls for participants' reaction times to unaccountable words. Although these results do not categorically determine whether a diffusion of responsibility account is better than a comfort one, they do suggest that any relationship comfort has with social context may have something to do with responsibility. We speculate that some people may feel more comforted and secure as a consequence of feeling less responsible.

General Discussion

The present studies help advance our current understanding of the bystander apathy literature by suggesting an alternative account in which imagined others need not be imbedded or implied in the helping situation at hand for the bystander apathy effect to be produced. That is, in our studies, individuals who just imagined the presence of others at Time 1 exhibited less helping behavior on a subsequent, completely unrelated task at Time 2, even when imagining others who could not possibly help out. For instance, imagining strangers in the movie theater should have no bearing on annual giving to a university alumni campaign, but from the current research it is clear that it does.

Although we feel this research provides evidence that, under some circumstances, a more social–cognitive account may be sufficient to explain bystander apathy, it by no means suggests that traditional accounts such as classic diffusion of responsibility (e.g., Darley & Latane, 1968) or confusion of responsibility (e.g., Cacioppo et al., 1986) are wrong. That is, our alternative account may interact with traditional ones in an additive or emergent way. Nevertheless, contrary to our current understanding of bystander apathy, the present research suggests that the presence of others need not be imbedded in or associated with the helping situation to exert diffusion of responsibility effects.

The present research also presents evidence that merely imagining a group can lead to lessened levels of responsibility. That is, from our reaction time studies, we have learned that participants who imagine the presence of others respond more quickly to words such as *unaccountable*, relative to participants in the control and one person conditions. Although diffusion of responsibility appears to characterize the mechanism behind the implicit bystander effect, future research must investigate more precisely how this mechanism works. The account that we put forth holds that a parallel of the explicit type of bystander apathy is what is being evidenced. That is, people who imagine group situations have notions of unaccountability triggered, and this leads them to see the situation as one that does not call for their personal help. However, there is the possibility that the mechanism through which the group primes exert an influence is less complicated than we surmised. Past research (e.g., Bargh et al., 1996; Dijksterhuis & van Knippenberg, 1998) has shown that primes can drive behavior unmediated through such construal processes. It remains wholly possible that bystander apathy could also be caused by behavioral manifestations of unaccountability, which are directly triggered by priming groups (or physically being in a group), and these behaviors interfere with or directly oppose helping behavior. The current studies do not distinguish between the construal hypothesis and the direct priming of behavioral manifestations, and, in fact, both processes are compatible with our data. What the data reveal is that the priming of group representations includes and simultaneously primes notions of lacking responsibility and accountability that appear to disrupt helping behavior. That is, even when one is not physically in a group, there is the possibility for an implicit bystander effect, in which diffusion of responsibility is triggered implicitly through primes and spreading activation.

Future Directions

From the present study, we know that priming the concept of group leads to the implicit bystander effect. However, an interesting avenue for future research is to examine whether priming the concept of being with a group below participants' level of conscious awareness still results in this effect. That is, one can envision words related to the concept of being with a group flashing on a computer screen to test whether the implicit bystander effect can be induced by unconscious priming. To the extent that activating the concept of a group or crowded situation makes accessible notions of unaccountability, it should not matter whether participants are aware of the group primes or whether they are primed below their conscious awareness. Nevertheless, this remains an empirical question.

Future research should also explore whether the type of group imagined plays a role in the implicit bystander effect. For instance, the interconnectedness of individuals in a group situation may influence responding. In classic bystander studies, the bystanders

[5] Incidentally, these analyses yield similar results with the unstandardized data set combining Studies 4 and 5.

[6] Again, these analyses yield similar results with the unstandardized data set combining Studies 4 and 5.

at hand often have no real social connection to each other except for being at the same place at the same time. That is, in these cases, the group is just a collection of independent individuals. Bystander apathy studies have also demonstrated this effect with bystanders with social connections to the others around them. In these cases, an individual may be a member of the group at hand. Along these lines, our studies provide evidence for the implicit bystander effect using both unconnected others and connected others. Namely, our dining out prime shows this effect with imagined acquaintances, whereas our movie theater prime shows the bystander effect when participants were simply imagining strangers. Although we grant that both types of primes lead to less helping behavior, it might be interesting to explore whether imagining one type of group becomes a stronger prime than another. Do people experience a greater sense of unaccountability when they are primed with being in an interconnected group than with being with random strangers?

Additionally, there is a possibility that some types of group primes may actually prime more helping behavior rather than less. For instance, leading people to think about groups of prosocial others, such as a group of lifeguards or doctors, may increase helping. Along these lines, manipulations that increase self-awareness (e.g., Duval & Wicklund, 1972) may also interact with group primes to increase helping behavior. For instance, if a person imagines that everyone in a crowded place is focused on him or her or if a person thinks of a crowded situation while standing in front of a mirror, helping will likely increase as people attempt to make their behavior consistent with their standards. Furthermore, level of identification with a particular group may affect how a group prime influences helping behavior. All of these questions are beyond the scope of this article and deserve future research.

Conclusion

Overall, this research suggests that priming the presence of others at Time 1 affects helping behavior on a completely unrelated task at Time 2. This research contributes to the priming methodology in that it primes a social context. Secondly, this research advances our understanding of bystander apathy and proposes an additional account for the bystander apathy effect. Future research must continue to explore the boundary conditions of the implicit bystander effect as well as the mechanisms that contribute to this process.

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