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Paying for Someone Else's Mistake: The Effect of Bystander Negligence on Perpetrator Blame

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The success of criminal acts can sometimes depend critically on the oversight or negligence of uninvolved bystanders (e.g., someone leaving a first-floor window open). Four studies examined how the contribution of a negligent bystander affects blame for the perpetrator of a crime. Although participants stated that discounting blame for the perpetrator was normatively inappropriate in this context, they expected that others would make this very "error." Instead, across all four studies, bystander negligence amplified *ascriptions of perpetrator blame. This amplification occurred because the bad action of the bystander provided an implicit standard of comparison for the perpetrator's act, framing it as more blameworthy. A variety of alternative mechanisms—that bystander negligence altered perceived crime avoidability, prompted spontaneous counterfactualizing, or increased victim empathy—were tested and ruled out. Implications for legal contexts are discussed.*

Keywords: *legal blame; contrast effect; automatic comparison; moral judgment; attribution*

Determining who deserves blame is an important thing to get right. In the legal domain, the wrong judgment can mean either unjustly punishing an innocent person or letting a guilty one go free—both costly errors to society. Fortunately, individuals seem fairly competent at determining when to assign blame. People make fairly complex ascriptions of blame that very often follow complex normative rules—that is, they make blame judgments much like theorists agree they should (e.g., Weiner, 1995).

However, some judgments of blame can be tricky. Although normative theories of responsibility posit that for an individual to receive blame for an act, he or she (among other things) should have caused the act, many real-world acts have multiple causes. Here we focus on cases in which someone's negligence contributed to a transgression. For example, carjackers may take advantage of a rolled-down window; trespassers may capitalize on a long-damaged fence; shoplifters may carry out their crime because no one turned on the security sensors. In each of these cases, the negligent bystander, although not committing the transgression, fails to take a minimal action that could have prevented the crime. How does this negligence affect people's judgments of the actual perpetrators?

Attributional Discounting

According to research on *attributional discounting*, the causal role attributed to a source is reduced to the extent that other causes are present (Kelley, 1972a, 1973; McClure, 1998). Previous research has found that additional causes do at times reduce blame for defendants (Barnett, Brodsky, & Davis, 2004; Perlin, 1994). For example, Barnett et al. (2004) found that hallucinations or drug use at the time of the crime, mental retardation, and a history of childhood abuse are all factors that can reduce ascriptions of criminal blame for the defendant. In these cases, the additional causes act as mitigating factors by calling into question the presence of *mens rea*—the

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criminal intent that legal sanctions are intended to punish. The alternative causes in these circumstances compete with criminal intent as *sufficient* explanations for the occurrence of the crimes. That is, these explanations serve as viable alternatives for why the crimes occurred.

The case of bystander negligence we are investigating here also involves the presence of multiple causes. However, in our studies, the negligence of a third-party bystander, although a *necessary* cause for the crime to have occurred, is not a *sufficient* cause. According to Kelley (1972b, 1973), the discounting rule would be used only when there is more than one sufficient cause, such that the presence of one cause renders other causes unnecessary. For example, past research has found that situational attributions only serve to reduce dispositional attributions if the situation is perceived as a sufficient cause of the person's behavior (Morris & Larrick, 1995).

The case of bystander negligence is best described as a case of multiple necessary causes (Hilton & Erb, 1996; Kelley, 1973). In the absence of either influence—the bystander's negligence or the criminal's behavior—the crime simply could not have occurred. Descriptive theories of responsibility predict that negligence should not lead to discounting in these cases (Kelley, 1972b; Leddo, Abelson, & Gross, 1984; McClure, 1998; Morris & Larrick, 1995; Reeder, Fletcher, & Furman, 1989). Although attribution theory does not predict discounting in the case of bystander negligence, we suspected that social perceivers might overapply the discounting rule to this circumstance. We suspected that people might apply a more general *zero-sum rule* of blame in which blame for the negligent bystander would lead to a fairly mindless reduction of blame for the perpetrator.

Comparison Contrast Effects

Even if bystander negligence does not lead to blame discounting, there is still reason to believe that the bystander's negligence could influence blame, but in the *opposite* direction. This may happen because the bystander's action provides a contextual standard that affects the way the perpetrator's action is perceived. That is, in comparison to a minor bystander infraction, a perpetrator's transgression may be perceived as more severe—a specific case of a more general contrast effect that has been demonstrated across a number of domains.

A 5-foot-tall 8-year-old is tall for his age, but short as a person. The appropriateness of these contradictory labels demonstrates how person judgments are made with reference to a standard (Higgins & Stangor, 1988; Schwarz & Bless, 1992). These comparison standards are malleable, shifting as a function of what is salient at the time of judgment. For example, the evaluation of a moderately unfavorable nation improved when considered in the context of an even more hostile nation (Diab, 1963). Similarly, people form a more positive impression of someone characterized by fairly neutral traits after reading negative, as opposed to positive, adjectives (Simpson & Ostrom, 1974). Higgins and Lurie (1983) found that the same judge was perceived to be fairly lenient or harsh after having considered a judge who gave harsh or lenient sentences, respectively.

In the aforementioned studies, both the target of judgment and the implicit standard of comparison were members of the same category (e.g., nations, judges) that clearly differed along the dimension of judgment (e.g., hostility, harshness). Under these conditions, the target of judgment is likely to be differentiated or contrasted away from the comparison standard (Brown, 1953; Ford & Thompson, 2000; Helson, 1964; Herr, 1986; Herr, Sherman, & Fazio, 1983; Schwarz, Munkel, & Hippler, 1990; Stapel, Koomen, & Zeelenberg, 1998). Such contrast effects have been shown to affect criminal and legal judgments in both the laboratory and the real world.

Kerr, Harmon, and Graves (1982) found that participants were more likely to convict on a subsequent armed robbery case after first being exposed to a case in which there was relatively weak evidence against the defendant than when there was strong evidence against the defendant. Most relevant to the current research, Pepitone and DiNubile (1976) found that after making judgments about an assault case, participants judged a homicide as more serious, brutal, and cruel. In an archival analysis, Kerr et al. (1982) also found that for fairly ambiguous cases (those for which the defendant was convicted on some counts and acquitted on others), jurors' decisions were contrasted away from their decisions in recent jury service, suggesting they (perhaps implicitly) used the prior case as a contrast case by which to judge their current case (but see Werner, Strube, Cole, & Kagehiro, 1985 for a null effect). Thus, even if rejected as a reason to discount blame for the perpetrator, our alternative *blame amplification hypothesis* suggests that bystander negligence might serve as a contextual comparison standard that instead augments perpetrator blame.

PILOT STUDY

We first conducted a pilot study to assess whether people held the naïve belief that a negligent act by a bystander would influence perpetrator blame. Participants (N = 145 college students) read about Patricia, a branch manager of a regional bank who received notice from the bank's headquarters that their network security was flawed and that bank customers' account and personal identification numbers could be

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vulnerable to outside hacking. By the time the security patch was installed Monday morning, the bank had already received 19 calls from bank customers claiming that charges were appearing on their debit account records that they had not made. Participants were then told to imagine that they learned that the security patch was not installed in time because Patricia decided not to install it Friday afternoon because she was in a hurry to meet her friends at a local bar's happy hour. Participants then answered on 7-point scales anchored at *blame less* (1), *neither blame more nor less* (4), and *blame more* (7) whether learning such information concerning Patricia's negligence would make most people reduce blame for the perpetrators than they would have without the presence of the negligent act. They were also asked the same question in reference to their own judgment and in reference to what the rational standard would dictate.

As a whole, participants seemed to believe that most people would discount blame ($M = 3.68$, $SD = 1.13$), which was significantly less than the midpoint of 4.00, $t(144) = 3.37$, $p = .001$. But participants believed that they themselves would be unaffected by such information regarding the negligence of a bystander ($M = 3.94$, $SD = .81$), $t < 1$, and that the rational standard would be to ignore this information in judging the perpetrators ($M = 4.03$, $SD = .71$), $t < 1$. Participants also believed that bystander negligence would lead others to reduce blame more than it would lead themselves to reduce blame, $t(144) = 2.97$, $p = .003$, $d = 0.25$. In addition, participants believed that others would depart from the rational standard of nondiscounting, $t(144) = 3.68$, $p < .001$, $d = 0.31$, but that they themselves would not systematically depart from a rational standard, $t(144) = 1.35$, $p > .17$.

Taken together, the results of the pilot study indicated that participants did not think that they would let bystander negligence affect their judgments of blame for the perpetrator, that this judgment was rational, and that (unlike them) others would irrationally discount blame. On the one hand, people may correctly estimate that others will be influenced by bystander negligence, not realizing that they themselves would also discount blame. Instead, participants may stay true to their own normative standards, falsely believing that others will discount blame. Participants did not seem to recognize (or at least downplayed) the potential for the third possibility, that the branch manager's (fairly minor) act of negligence might serve as a standard of comparison that would frame the actual crime as more severe.

Overview of the Present Studies

Across all four studies, we pit the *blame amplification* hypothesis against the *attributional discounting* hypothesis by testing the effect of bystander negligence on lay judgments of perpetrator blame. Study 1 provided initial support for the blame amplification hypothesis. Study 2 varied the badness of the bystander's negligence to test whether perpetrator blame changed as bystander negligence increased. Study 3 tested whether the contrast effect reversed once the bystander's negligence became more extreme than the perpetrator's crime. In Study 4, participants played the part of mock jurors and listened to closing arguments of a legal case that either did or did not include information about bystander negligence. In addition, these participants were asked for their opinions on how bystander negligence would affect jurors' sentencing decisions, to test whether participants' intuitions matched their actual judgments. Across the studies, various alternative mediators for the blame amplification effect are tested and ruled out.

STUDY 1

We had four primary aims in our first study. We wanted to cleanly pit the *blame amplification* hypothesis against the *attributional discounting* hypothesis by testing whether bystander negligence amplified or mitigated perpetrator blame. Regardless of which hypothesis was correct, we wanted to determine whether it was necessary to explicitly anchor participants' judgments about the negligent bystander before judging the perpetrator to attain blame amplification or mitigation (Pepitone & DiNubile, 1976). Third, we expected blame for the bystander to mediate the effect of blame amplification or mitigation. If blame for the criminal is differentiated from the clearly less-bad act of negligence, then as the blame for the negligent bystander increases, so should the blame for the criminal. Finally, we wanted to rule out the alternative possibility that any effect of bystander negligence on perpetrator blame was due to the crime being perceived as more avoidable (Kahneman & Miller, 1986). It seemed possible that when a crime was seen as more avoidable, participants might blame the perpetrator less, because imagining how the crime could have been avoided may make the crime seem poorly planned and perhaps less severe. Alternatively, participants might blame the perpetrator more, perhaps because of a negative affective reaction upon imagining that the crime almost did not occur.

In this study, participants read about a crime (computer hacking) that could have been prevented had a bystander (a bank manager) not been negligent. We assessed perpetrator blame by having participants assess the immorality of the perpetrators' actions and how severely they thought the perpetrators should be punished.

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We assessed bystander blame by asking to what extent participants blamed the bystander for the crime's occurrence. The attributional discounting hypothesis would predict less perpetrator blame during bystander negligence, whereas the blame amplification hypothesis would predict accentuated blame.

Method

Participants and design. Participants were 228 undergraduates at Cornell University, who participated in the study as part of experimental sessions for which they received extra credit in psychology and human development classes. Participants were randomly assigned to a *control* or *bystander negligence* condition.

Procedure. Participants read the computer hacking scenario used in the pilot study. Only in the *negligent* condition did participants learn that the security patch was not installed in time because Patricia decided not to install it Friday afternoon because she was in a hurry to meet her friends at a local bar's happy hour.

After reading this information, participants indicated their blame for the perpetrator and the negligent bystander (Patricia) in a counterbalanced order. Two items indicated perpetrator blame ($r = .54$, $p < .001$): how severely the hackers should be punished and how morally wrong the hacking was. Ratings were made on 11-point Likert-type scales that ranged from 1 (*not at all*) to 11 (*extremely*). A single item was used for bystander blame, "To what extent do you blame Patricia for the hacking's occurring?" Responses could range from 1 (*not at all*) to 11 (*completely*).

Finally, all participants were asked how easy it was to imagine that the crime could have been avoided, on an 11-point Likert-type scale (1 = *not at all*, 11 = *extremely*).

Results

Bystander blame. The manipulation of negligence appeared to be effective. Participants blamed Patricia more for the crime's occurrence when her failure to install the security patch in time stemmed from her eagerness to get to happy hour ($M = 7.81$) than when no information about Patricia's negligence was provided ($M = 3.39$), $t(226) = 14.58$, $p < .001$, $d = 1.94$.

Perpetrator blame. The two items were standardized and added together to create a perpetrator blame index. We regressed perpetrator blame on negligence condition, the order manipulation, and the Condition × Order interaction. Consistent with the blame amplification hypothesis, participants in the negligent condition blamed the hackers more ($M = .27$) than those in the control condition ($M = -.27$), $t(224) = 2.33$, $p = .02$, $d = 0.31$. Neither the main effect of order nor the Condition × Order interaction approached significance, ts < 1.

Additional analyses. If participants in the negligent condition amplified perpetrator blame because of an implicit comparison they made with Patricia's act of negligence, then blame for Patricia should be related to an increase in blame for the hackers. On the other hand, if participants were using a zero-sum conception of blame, increased blame for Patricia should lead to a decrease in blame for the hackers. Consistent with the blame amplification hypothesis, perpetrator blame and bystander blame were significantly correlated, $r = .26$, $p < .001$. We then regressed perpetrator blame on the condition, bystander blame, and the Condition × Bystander Blame interaction. The effect of bystander blame on perpetrator blame remained significant, $t(224) = 3.32$, $p = .001$, whereas the main effect of condition dropped to nonsignificance, $t < 1$. Although the criteria for full mediation were met, Sobel $z = 3.23$, $p = .001$ (see Figure 1), our specific prediction is that bystander blame should predict perpetrator blame only when it is a relevant standard of comparison (i.e., in the negligence condition). Accordingly, the Condition × Bystander Blame interaction was significant, $t(224) = 2.00$, $p = .05$. This interaction reflected that the relationship between bystander and perpetrator blame was stronger in the negligence condition ($r = .35$, $p < .001$) than in the control condition ($r = .09$, *ns*). This interaction is also consistent with the hypothesized amplification effect because of contrast. The bystander's behavior is only an applicable implicit standard for judging a bad deed (the hacking) when the bystander has done some wrong herself, making her a relevant standard of contrast (Brown, 1953; Schwarz et al., 1990).

Avoidability of the crime. Although the crime was judged as more avoidable by participants in the negligent condition ($M = 9.00$) than participants in the control condition ($M = 6.50$), $t(216.62) = 8.19$, $p < .001$, $d = 1.11$, there was no correlation between the avoidability of the crime and perpetrator blame, $r = .01$, *ns*, ruling it out as a potential mediator.

Discussion

The data strongly supported the blame amplification hypothesis over the attributional discounting hypothesis. After learning that a bystander's negligence allowed a crime to occur, participants increased their blame both for the bystander and the perpetrator. The results directly contradicted our pilot participants' estimates that others would reduce blame as a product of bystander negligence and that they themselves would be

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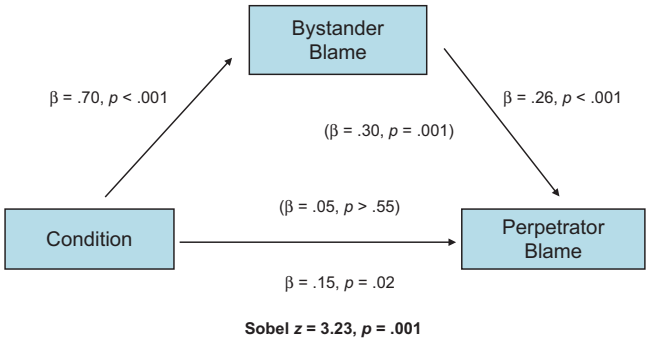

Figure 1 The effect of the negligence manipulation on perpetrator blame is mediated by bystander blame (Study 1).
NOTE: The beta weights in parentheses come from the model in which negligence condition and bystander blame simultaneously predicted perpetrator blame. Condition: +1 = bystander negligence, –1 = nonnegligent control.

unaffected by the information. Equally important, although bystander blame and perpetrator blame were significantly correlated when the bystander was negligent, and thus an applicable standard of comparison, there was no bystander-perpetrator blame correlation in the control condition. This interaction demonstrated that the bystander-perpetrator blame correlation did not simply result from individual differences in the tendency to blame others, as this would have caused a significant correlation regardless of bystander negligence. Finally, although the negligence manipulation did increase the perceived avoidability of the crime, this effect did not account for the effect on perpetrator blame, allowing us to rule out an avoidability explanation for our effect.

Types of contrast effects. Contrast effects can be one of two types: perceptual or artifactual (Campbell, Lewis, & Hunt, 1958; Pepitone & DiNubile, 1976). To the extent that people simply wish to use a broad range of a scale to help differentiate between two targets they are judging, an artifactual contrast effect will result (Simpson & Ostrom, 1974). In a different version of an artifactual contrast effect, an initial rating target may alter the subjective interpretation of the endpoints of a Likert-type scale. In both cases, the contrast effects do not reflect actual differences in target perception and are thus artifactual.

True perceptual contrast effects occur when a salient standard of comparison produces an actual shift in the way a subsequent target is perceived. At least two methodological details can lend confidence to the conclusion a particular contrast effect is perceptual and not artifactual. First, when a response scale is extraexperimentally defined and anchored (e.g., a prison sentence), as opposed to defined by arbitrary and ambiguous scale end points, contrast effects are not simply artifactual. This measure was added in Study 2 and used as the sole dependent measure in Study 4. Second, it is important to demonstrate that the magnitude of the contrast effect does not depend on whether people explicitly anchored their judgments on the first target. For example, Pepitone and DiNubile (1976) only found evidence of contrast effects on judging assaults after judging homicides when participants actually judged the homicides as opposed to merely considering them. The lack of an order effect in Study 1 indicated it was not necessary to explicitly focus participants' attention on or have them assess the bystander prior to judging the perpetrator for the contrast effect to emerge. Instead, a negligent Patricia seemed to serve as a natural point of contrast even when her selection as a reference was unsolicited. To demonstrate more conclusively that participants need not judge the bystander for the contrast effect to occur, Study 2 did not include questions about the negligent bystander.

STUDY 2

Study 2 used a new scenario to attempt to conceptually replicate the blame-amplifying contrast effect observed in Study 1. In addition to including new features aimed at ruling out alternative explanations, we had two levels of bystander negligence (mild and moderate) in addition to a nonnegligence control condition. Although we expected both negligence conditions to produce more perpetrator blame than the control condition, we had conflicting predictions about which would produce greater blame. On one hand, what may be most important is the gap in severity between the bystander's negligence and the crime, such that the milder bystander's negligence frames the crime as most severe. Alternatively, blame may be contrasted away from the standard, but "worse than a not-that-bad thing" may not produce as harsh a judgment as "worse than a fairly bad thing." Research that simply uses a higher or lower standard of comparison cannot distinguish between these two possibilities. But given that in Study 1 bystander and perpetrator blame were *positively* correlated, we predicted that moderate negligence would produce more perpetrator blame than mild negligence.

Also, although participants in Study 1 who answered questions about the perpetrator before answering questions about the bystander did not respond differently from those who completed the measures in the reverse order, we went to greater lengths in Study 2 to make

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certain that participants were not prompted to consider the bystander before answering questions about perpetrator blame. Specifically, we did not include measures of bystander blame in Study 2. Although this strategy precludes an assessment of mediation, it did provide the cleanest test of whether participants would naturally use bystander negligence as an implicit point of comparison in blaming the perpetrator, even when not explicitly prompted to evaluate the bystander.

We also added a third, non-Likert-type measure of perpetrator blame—participants' suggested length of prison sentence.

As in Study 1, we included a measure of the avoidability of the crime, but we also asked participants to what extent they spontaneously counterfactualized while reading the scenario, thinking "If only . . ." things had been different, this crime would not have occurred. It seemed possible that by identifying a necessary cause of the crime (the bystander's negligence), it may have been easier to spontaneously undo the crime's occurrence. This may have then provided a standard of comparison that framed the crime's occurrence as all the more terrible.

Method

Participants and design. Participants were 102 undergraduates at Cornell University, who took part in the study in exchange for extra credit in psychology and human development classes. Participants were randomly assigned to a *no-negligence control* condition, a *mild negligence* condition, or a *moderate negligence* condition.

Procedure. All participants read about Joseph, a tenant in an apartment building where it is strict security policy that residents not prop open the main door to the building. In the nonnegligent control condition, participants read that as Joseph exited the building, a burglar caught the door and entered the building. In the mild-negligence condition, it was said that Joseph needed to walk down to check his mail and propped open the door for the short duration he was to be gone. In the moderate-negligence condition, participants learned that Joseph needed to walk down to check his mail but also to run a number of errands; he propped the door even though he had no intention of returning any time soon. In both negligence cases, a burglar was able to enter the apartment building while the door was propped open. In all conditions, the burglar mugged a resident who was walking in the hallway of the apartment, taking his Rolex watch and wallet.

In addition to the two dependent measures used in Study 1 (punishment severity and immorality of action), we had participants offer a suggested prison sentence. Participants specified the number of years and months they would sentence the perpetrator, not to exceed 7 years. Finally, all participants answered the same avoidability item from Study 1 as well as a new item that asked "As you read the story, did you spontaneously think that this crime might not have occurred 'if only' certain things had been different?" The response scale ranged from 1 (*not at all*) to 11 (*completely*).

Results

Perpetrator blame. We created a three-item blame composite by standardizing and adding the three measures ($\alpha = .71$). To test the expected linear trend, that blame for the perpetrator's crime would be most severe when the bystander was moderately negligent and least severe when the bystander was not negligent, we first dummy coded the control (–1), mild-negligence (0), and moderate-negligence (1) conditions. We then regressed the perpetrator blame index on the condition dummy-coded variable. As can be observed in Figure 2, a significant linear trend emerged, $t(99) = 2.33$, $p = .02$. Participants blamed the mugger least when Joseph was not negligent ($M = -.21$), slightly more when Joseph was mildly negligent ($M = -.03$), and the most when Joseph was moderately negligent ($M = .23$). Only the difference between the control and moderate-negligence conditions was significant, $t(69) = 2.28$, $p = .03$, $d = .55$, with the mild-negligence condition lying nonsignificantly between the two, ts < 1.39, ps > .17, ds < 0.34.

Additional variables. Both perceived avoidability and spontaneous counterfactualizing varied as a function of negligence condition, $F(2, 99) = 20.84$, $p < .001$, and $F(2, 99) = 19.46$, $p < .001$, respectively. Post hoc (least significant difference) analyses revealed that both negligence conditions differed from the control condition (ps < .001), although the negligence conditions did not differ from each other (ps > .28). Participants exposed to the negligence scenarios saw the crime as more avoidable and were more likely to spontaneously "undo" the crime with a counterfactual. Although these variables were clearly affected by our manipulation, perpetrator blame did not correlate with the perceived avoidability of the crime ($r = .15$, $p > .12$) or with spontaneous counterfactualizing ($r = .08$, *ns*), ruling them out as mediators of the effect.

Discussion

Study 2 conceptually replicated Study 1, lending further support to the amplification hypothesis that bystander negligence augments perpetrator blame. The study also helped to distinguish between two possible understandings of the contrast effect. It was as though participants in both conditions evaluated the crime as

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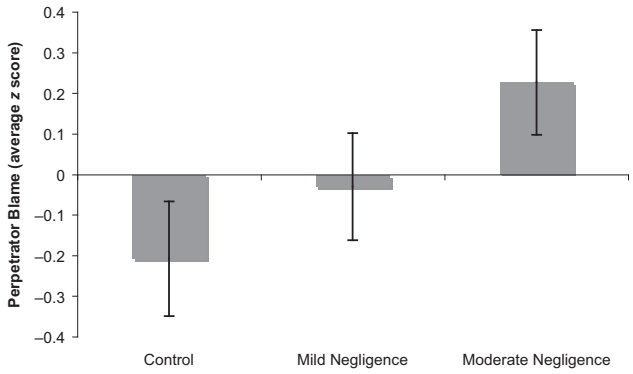

Figure 2 Perpetrator blame as a function of level of bystander negligence (Study 2).

"clearly worse than the act of negligence." Consistent with the correlational evidence from Study 1, when this state of negligence was greater, the worse action ended up being judged to be more blameworthy as well. Also, given that we did not have participants evaluate the act of bystander negligence, nor did we have participants explicitly judge the bystander, it seems that this contrast effect does not require that participants anchor their evaluations of the comparison standard (the act of bystander negligence) for it to exert an effect on target judgments (perpetrator blame). Finally, the effect of bystander blame on perpetrator blame does not seem to be mediated by the perceived avoidability of the crime or any tendency for negligence to prompt more spontaneous counterfactualizing undoing the crime.

An alternative explanation for our results from Studies 1 and 2 that does not rely on the notion of contrast effects is that blame may beget blame through a process of *blame inertia*. It may be that once people chide someone for one wrongdoing, they enter into a blame mind-set that makes them more likely to find fault with someone else. This possibility seems to be at odds with past work that has found that after making judgments about a more serious crime or strong legal case, people are actually less likely to judge harshly a subsequent offender (Kerr et al., 1982; Pepitone & DiNubile, 1976). Nonetheless, we included an extreme-negligence condition in Study 3, one in which the act of bystander negligence included actions and intentions even more heinous than those of the perpetrator, to see if the blame amplification effect would disappear.

STUDY 3

Using the same scenario used in Study 2 (breaking into an apartment building that had the door propped open), we created an extreme-negligence condition in which the negligent bystander propped open the apartment door as he left to go commit a murder. The reason he left the door propped was so that the electronic key card reader would not record the time of his entry into the building after having committed the murder. An advantage of this manipulation was that we were able to manipulate the severity of the implicit standard of comparison without changing anything about the specific act of negligence (i.e., propping open the door).

According to the contrast effect hypothesis, a mild act of bystander negligence provides a standard of comparison that frames the crime as especially severe. But if the bystander is to commit an especially horrendous act, the perpetrator's crime will if anything seem less severe. By contrast, the blame inertia hypothesis suggests that heaping blame on the extremely immoral bystander will produce the strongest perpetrator blame.

Second, we wanted to more precisely test that it was the perceived badness of the bystander that served as the standard of comparison, not how blameworthy he was for the crime's occurrence. In Study 1, blame for Patricia, the negligent bank teller, was likely very much about the perceived badness of Patricia's action. But in Study 3, especially for our extreme-negligent condition in which the fact that the bystander left to commit a murder should not have any influence on how much to blame he was for a subsequent robbery, these two judgments would likely differ. Because we believed it is the badness of the bystander act, not the amount of blame he or she is accorded, that provides the standard of comparison, we expected the moderate- and extreme-negligence conditions to produce different amounts of perpetrator blame, even though their crime-related negligence (leaving a door propped open) was equivalent. Finally, we included a fourth measure of perpetrator blame: the quickness with which people would be able to forgive the perpetrator for his crime.

Method

Participants and design. Fifty-five undergraduates at Cornell University participated and received extra credit in their psychology or human development courses. Participants were randomly assigned to a moderate-negligence, extreme-negligence, or nonnegligent control condition.

Procedure. Participants read the same background information as in Study 2, only this time the apartment tenant (the bystander) was named Edward. As in Study 2, some participants read that a mugger caught the door to the apartment building as Edward exited the building (nonnegligence control), and some read that Edward propped the door open with no intention of returning

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any time soon (moderate negligence). In place of the mild-negligence condition, an extreme-negligence version read that Edward propped open the door as he left to commit a homicide, hoping to avoid using the security key card scanner upon his return. In both negligence conditions, although the door was propped open, a mugger entered the building. In all conditions, the mugger stole a tenant's Rolex watch and wallet. In addition to the three perpetrator blame items used in Study 2 (immorality, severe punishment, suggested prison sentence), participants indicated on an 11-point Likert-type scale ranging from 1 (*not at all*) to 11 (*extremely*) "How quick [they] would be to forgive the mugger for what he did?"

Results

We created a perpetrator blame index by creating standard normal z scores for each of the four blame measures. After reverse scoring the forgiveness item, we averaged the four measures to create a scale with decent reliability ($\alpha = .76$). The means by condition are depicted in Figure 3. We then ran an ANOVA on the perpetrator blame index using condition, order, and the Condition × Order interaction as predictor variables. The main effect of condition was significant, $F(2, 49) = 3.76$, $p = .03$, although neither order term approached significance, Fs < 1.79, ps $> .18$. Replicating Study 2, participants blamed the perpetrator more when the bystander was moderately negligent ($M = .34$) than when the bystander was nonnegligent ($M = -.13$), $t(37) = 2.07$, $p = .05$, $d = 0.68$. Consistent with the contrast hypothesis, but contrary to the blame inertia hypothesis, participants blamed the perpetrator more in the moderate-negligence condition than in the extreme-negligence condition ($M = -.32$), in which the bystander left the door open to commit a murder, $t(35) = 3.03$, $p = .005$, $d = 1.02$. The extreme-negligence and control conditions did not significantly differ from each other, $t < 1$.

Discussion

Study 3 replicated the contrast effect produced by a negligent bystander on perpetrator blame. In addition, Study 3 was able to rule out the alternative blame inertia hypothesis that the more one blames one person (the bystander), the more likely one is to blame a subsequent target (the perpetrator). Instead, when the bystander's actions (a murder) were clearly worse than the perpetrator's (a mugging), blame was no longer amplified and was, if anything, diminished.

In addition, it need not be the actions of the bystander that are relevant to the perpetrator's actions but instead the degree to which people encode the bystander's actions and intentions (even when perpetrator-unrelated) as horrible and wrong. In both the moderate- and extreme-negligence conditions, the bystander performed the exact same negligent action, making him equally culpable for the crime's occurrence. What differed between these two conditions was whether the bystander's actions and intentions were clearly less severe than the mugging or part of a scheme more severe than the mugging. In the context of a more or less egregious standard of comparison, blame for the perpetrator was lower or higher, respectively.

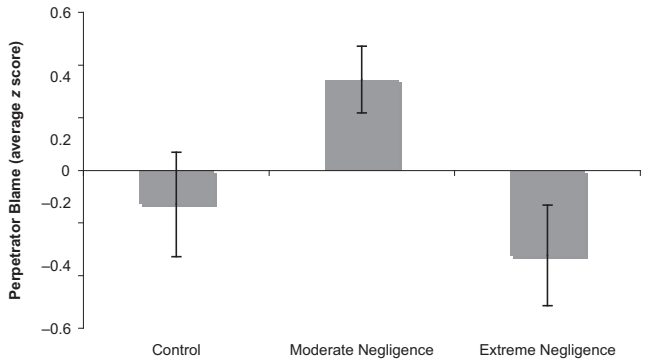

Figure 3 Perpetrator blame as a function of level of bystander negligence (Study 3).

STUDY 4

In a final study, we used a more involving experimental paradigm to test our blame amplification hypothesis. Once participants arrived at the lab, they were told they would be participating in a mock jury study. Participants read some background information about a legal case in which the defendant had impersonated a museum security guard to gain access to a museum bag check room. Once inside, he was able to run off with thousands of dollars worth of museum visitors' personal belongings. Participants listened to an audio recording of an excerpt of the closing arguments from the trial. In the excerpt, the details of the case were reviewed. What was varied was whether the reason the defendant had gained access to the bag check room was due to the negligence of an actual museum guard.

Whereas in the previous studies we used multiple measures to form a perpetrator blame index, in the present study, we used a single measure: a suggested prison sentence. We made this modification for two reasons. First, real-world jurors are confronted with the important task of recommending sentencing, making the measure

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highly representative of real-world judgments. Second, the meaning of the sentence cannot be distorted by context, although Likert-type scales are always vulnerable to this possibility.

All participants made their sentencing decision before judging the bystander. Although all order effects were nonsignificant in Study 1, observing a contrast effect in the present study would strongly demonstrate that the contrast effect occurs in the absence of any external prompt to consider the bystander's actions. Unlike in Studies 2 and 3, retaining the bystander badness measures allowed us to conduct internal analyses on the correlations between bystander badness and perpetrator blame. In addition, we tested another alternative explanation for the observed effects: that differences in victim empathy mediate the effects. It seemed possible that a crime that occurred because of bystander negligence might make participants be more empathetic with the victims of the crime, leading to increased perpetrator blame.

Finally, at the conclusion of the study, we told all participants about the bystander's negligence. We then asked them whether they thought that this information would be more helpful to the defense, who would want lighter sentencing, or to the prosecution, who would prefer tougher sentencing. Although our pilot study suggested people believe bystander negligence will prompt others to discount blame, it seemed possible that following the experience with the judgment task, people would have improved intuitions. Therefore, in addition to looking at participants' intuitions in general, we were particularly interested in whether negligent-condition participants would differ from control participants in their intuitions.

Method

Participants and design. Participants were 140 undergraduates (38 men) at Cornell University, who participated in exchange for extra credit in their psychology and human development courses. Participants were randomly assigned to a negligence or control condition.

Procedure. All participants were informed that they were going to participate in a mock juror study. After being seated in a private cubicle in front of a computer, they read background information about the case. The defendant was said to have impersonated a museum security guard to gain access to a bag check room, where he was able to gather and run off with thousands of dollars of museum patrons' personal belongings. Once participants had read the background information, they were asked to play the audio file that was open on the computer. They were informed that this was a 2-minute excerpt of the closing arguments from the case. Participants were provided with blank paper on which they could keep notes about details of the case.

At the beginning of the excerpt, the attorney said that he would like to review the undisputed facts of the case. He reviewed a more detailed summary of the crime that included information about the layout of the museum, the times at which the defendant had entered the bag check room and had fled through a fire escape door, and details about the irreplaceability of some of the stolen items. This summary introduced the bystander: Mr. Jack Davis, the security guard in charge of guarding the door of the bag check room. In the control condition, participants heard,

> The security guard, Mr. Jack Davis, from whom you have heard earlier testimony, did not recognize the defendant as a museum employee. Accordingly, he asked the defendant for his ID as is consistent with museum security policy. Because the ID appeared legitimate, he permitted the defendant to enter the room.

Given this information, the bystander does not appear to have done anything wrong, and as a result, he should not provide an appropriate standard of comparison by which to judge the crime. Participants in the negligent condition heard the same audio file, except we spliced into the recording the following sentence:

> Mr. Jack Davis was distracted with a cell phone call he was having with his girlfriend, and contrary to museum policy, he did not run the defendant's fake ID through the ID scanner or call the security office to verify the legitimacy of this ID Mr. Davis had never seen before.

In this case, the bystander acted contrary to museum policy, and as a result, did provide a relevant standard of comparison that should have framed the perpetrator's crime as more severe.

After listening to the audio file, participants were then asked what prison sentence they would recommend for the perpetrator. They were asked to specify the number of years and months, with a maximum sentence of 7 years. Instead of using the bystander blame item in Study 1, we used items that more directly tapped into the perceived badness of the bystander's actions, which Study 3 suggested would be the more relevant comparison (although in Study 1, these were likely equivalent). Participants rated the wrongness, horribleness, and severity of deserved punishment of the bystander's actions and intentions from 1 (*not at all*) to 11 (*extremely*).

Participants then answered how easy it was to imagine how the crime could have been avoided. Two additional items assessed an alternative hypothesis that

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between-condition differences were due to differences in empathy for the victims: "How sorry do you feel for the victims?" and "How much do you empathize with the people whose things were stolen?" The response scales for all three questions ranged from 1 (*not at all*) to 11 (*extremely*).

Finally, all participants were told that, even though they might not have heard this in the version of the closing arguments that they heard, the reason that the defendant was able to gain access to the bag check room was that the security guard did not follow the museum security policy of scanning the ID of any unrecognized security personnel. To assess their lay beliefs as to how individuals would use the negligence information, they were then asked, "Do you think that including this detail is more advantageous to the prosecution, who would like the defendant to be punished more harshly, or for the defense, who would like to get a reduced sentence for its client?" The response scale ranged from 1 (*definitely prosecution*) to 11 (*definitely defense*).

Results

Bystander blame. The three bystander blame items—wrongness, horribleness, meriting punishment—were standardized and averaged to create a composite of bystander blame that displayed high reliability ($\alpha = .90$). Confirming that the manipulation was successful, participants saw the bystander as more blameworthy in the negligence condition ($M = 0.37$) than in the control condition ($M = -.36$), $t(138) = 5.14$, $p < .001$, $d = 0.88$. Judgments did not differ by participant gender, $t < 1$.

Perpetrator blame. The suggested prison sentence was regressed on negligence condition, gender, and the Condition × Gender interaction. A main effect of condition emerged, $t(136) = 1.98$, $p = .05$, $d = 0.34$. Participants in the bystander negligence condition suggested a harsher sentence ($M = 4.59$ years) than did participants in the control condition ($M = 4.09$ years). Although there was a tendency for women to give harsher sentences than men, $t(136) = 1.47$, $p = .14$, the effect of condition did not interact with gender, $t(136) = 1.05$, $p = .29$.

Additional analyses. Mediation analyses are summarized in Figure 4. Once again, bystander blame was a significant predictor of perpetrator blame, $t(138) = 2.74$, $p = .01$. When perpetrator blame was regressed on bystander blame, condition, gender, the Bystander Blame × Condition interaction, and the Condition × Gender interaction, the effect of bystander blame remained significant, $t(134) = 2.18$, $p = .03$, whereas the effect of condition dropped to nonsignificance, $t < 1$. Although a Sobel test confirmed the significance of the mediation model, $z = 2.01$, $p = .04$, we (as in Study 1) expected the relationship between bystander badness and perpetrator blame to differ by negligence condition. Consistent with this, the Bystander Blame × Condition interaction was also significant, $t(134) = 2.30$, $p = .02$. The correlation between perpetrator and bystander blame was significant in the negligence condition, $r = .39$, $p = .001$, but not in the control condition, $r = .01$, *ns*, confirming the logic of the contrast explanation for blame amplification.

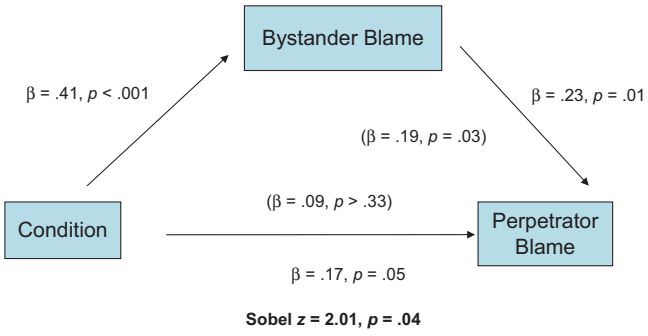

Figure 4 The effect of the negligence manipulation on perpetrator sentencing is mediated by bystander blame (Study 4).
NOTE: The beta weights in parentheses come from the model in which negligence condition and bystander blame simultaneously predicted perpetrator blame. Condition: +1 = bystander negligence, −1 = nonnegligent control.

We tested two alternative mediators: the perceived avoidability of the crime and empathy for the victim. The crime was indeed seen as more avoidable in the negligence condition ($M = 9.46$) than the control condition ($M = 7.03$), $t(138) = 8.00$, $p < .001$, $d = 1.36$. We therefore regressed the suggested prison sentence on condition, gender, the Condition × Gender interaction, and the perceived avoidability of the crime. The perceived avoidability of the crime did not predict the prison sentence, $t < 1$, ruling it out as a plausible mediator. To test for the role of victim empathy, we combined our two measures of victim empathy ($r = .76$) into a single victim empathy index. There was no difference in victim empathy between the negligence and control conditions, $t < 1$, again ruling out this alternative mediator.

Participant intuitions. Our results have consistently demonstrated that bystander negligence frames (more severe) criminal acts as more immoral and more worthy of punishment than when these acts are considered in isolation. We expected that, like participants in our pilot study, most would have the intuition that bystander negligence would mitigate punishment. Consistent with this hypothesis, participants' opinions about whether bystander negligence would exacerbate or mitigate punishment lay significantly above the midpoint of 6 ($M = 7.74$),

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$t(139) = 41.58$, $p < .001$, suggesting they believed that bystander blame would serve as a sentencing reducer. We recoded participants' responses to this item according to whether they thought bystander blame was a sentencing mitigator, sentencing exacerbator, or information that would not affect sentencing. Seventy-one percent believed bystander negligence was a sentencing mitigator, 8% believed that it was a sentencing exacerbator, and 21% believed it would make no difference. A clear majority of participants were misguided about how bystander negligence tends to affect perpetrator blame.

We then tested whether participants who had made their sentencing decision while knowing of the bystander's negligence (negligent condition) would have more accurate intuitions than those without recent experience making the judgment. Contrary to this possibility, participants did not see bystander negligence as more likely to amplify sentencing decisions in the negligence condition ($M = 7.59$) than in the control condition ($M = 7.89$), $t < 1$. These results also rule out an alternative explanation for participants' flawed intuitions: that participants in the negligence condition may have assumed that the version of the closing arguments that they heard came from the defense attorney, and as a result, they may have assumed that the defense attorney would only have included this information if it were likely to mitigate blame.

Discussion

In a more involving and externally valid paradigm, participants once again seemed to use the badness of a bystander's negligence as a standard of comparison when determining an appropriate sentence for a defendant. As a result of the security guard's negligence, participants recommended a 6-month increase in the perpetrator's prison sentence. Two features of Study 4 speak to the robustness and generality of the contrast effect. First, all participants made their judgments of perpetrator blame before making judgments of the bystander. Second, the single measure of defendant blame was not made using a Likert-type scale but a response unit that has well-defined meaning: a prison sentence in years and months. The contrast effect, therefore, does not merely surface when the meaning of the response options is ambiguous and shapeable by context (Campbell et al., 1958).

Although we have consistently observed evidence of bystander negligence leading to contrast effects on judgments of perpetrator blame, Study 4 confirmed our pilot data that this effect is quite counterintuitive, even among those who had just displayed the effect. Whereas 100 of our participants believed that bystander negligence would lead to reduced sentencing, only 11 (of 140) participants correctly believed that bystander negligence would amplify sentencing.

GENERAL DISCUSSION

There are many examples of transgressions that could only occur because of the negligence of bystanders. How do the mistakes of these bystanders affect the degree to which people blame and punish someone for a crime that was facilitated by such negligence? When asked, most participants (in the pilot study and Study 4), seemed to think that, in general, individuals would reduce blame and promote less harsh sentencing for such acts (even though they reported that their own judgment of blame would be unaffected by the presence of a negligent bystander). However, contrary to the intuitions of our participants, as well as to normative theories of blame, participants' actual judgments of blame were consistently higher under these conditions—an effect we refer to as "blame amplification." This effect could not be explained by a tendency for bystander negligence to frame the crime as more avoidable, to prompt spontaneous counterfactualizing or a mental "undoing" of the crime, nor even to heightened empathy for the victim. These accentuated judgments of blame appeared to be driven by a contrast effect—the milder badness of the bystanders' negligent actions provided a point of comparison that caused the perpetrators' actions to be contrasted away from this standard, being seen as more immoral, less forgivable, and more worthy of punishment.

These findings appear to challenge the accuracy of descriptive theories of blame attribution within psychology. Indeed, it is becoming increasingly clear that the process of attributing blame and responsibility does not seem to proceed in the stagelike, normatively defensible fashion of the sort specified by descriptive theories of blame attribution (e.g., Weiner, 1995). A growing body of research has pointed to a number of judgmental "quirks" when attributing responsibility for negative and positive acts (e.g., Alicke, 2000). The current findings add to this work by highlighting the way that even actors who are explicitly judged to be irrelevant to a moral judgment provide a contextual standard that unknowingly affects judgment.

Comparison Contrast or Overcorrection?

Throughout this article, we have argued that our results are the process of an automatic comparison process, such that a negligent bystander provides an implicit standard of comparison in evaluating a perpetrator. Even though our studies consistently demonstrated that participants did not engage in attributional

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discounting, might our results have actually resulted from an attempt to avoid attributional discounting? Given that people felt that attributional discounting was not normative for the bystander negligence problem (pilot study), might people have feared that they automatically discounted and then tried to correct for this possibility by increasing their blame for the perpetrator? We see at least three reasons to doubt this possibility.

First, there is no reason to believe that participants suspected that they were automatically discounting. Although participants did state that they expected *others* to blame discount, they actually did not believe that they would be influenced by the information. To the extent they thought the information did not distort their judgment, there would be no reason for them to try and correct for it. Of course, it is possible that participants in our pilot study believed that bystander negligence would not influence their judgments because they anticipated that they would correct for its influence.

But even if this is true, it begs the question of when such attempts at correction are likely to occur. Even if upon reflection people may be able to report that their judgment may be biased, this does not mean that in making the judgment, they will spontaneously correct for the source of bias (Wegener & Petty, 1997). For example, Stapel, Martin, and Schwarz (1998) found that in the absence of at least a subtle warning, there was no correction for comparison contrast effects. And even when participants were subtly warned not to be influenced by irrelevant sources, participants only corrected for the biasing contextual factors when (a) they actually made ratings of the biasing contextual stimuli and (b) these ratings were on the same dimension as the influenced ratings. In Studies 2 and 3, participants were never induced to explicitly consider the bystander, thus no special attention was drawn to it as a potentially biasing cue. Furthermore, participants in Study 1 who judged the bystander before the perpetrator showed a contrast effect of similar magnitude as those who judged the perpetrator before the bystander. Thus, not only did our judgment contexts not meet criteria previously observed to be required for attempts at bias correction, but features that increased attention to the potentially biasing sources did not attenuate the effect.

The above evidence is indirect; the strongest evidence against a correction account can be found in the data. Only a comparison contrast account predicts that perceptions of the perpetrator are actually a function of perceptions of the bystander. The correction account predicts that perceptions of the perpetrator will instead be tied to individual differences in participants' intuitions about how much they may have automatically engaged in attributional discounting. As such, only the comparison correction account can explain the correlations between bystander and perpetrator blame in the negligence conditions in Studies 1 and 4.

Automatic Comparison Processes

Although the effects in this article have dealt with comparisons between a contextual exemplar (the bystander) and an external target (the perpetrator), recent research on social (self-other) comparison phenomena has suggested that these comparison processes occur spontaneously, even when one is not aware of exposure to the comparison target (Mussweiler, Rüter, & Epstude, 2004; Stapel & Blanton, 2004). For example, even a subliminal exposure to a person automatically activates the self and leads to subsequent self-judgments that are contrasted away from extreme exemplars (Stapel & Blanton, 2004). Mussweiler et al. (2004), though, found that the social comparison effect required that people be considering the self at the time of prime exposure.

Displaying a divergence between automatic social comparison and automatic contextual comparisons, Stapel and Blanton (2004) found that subliminal exposure to Gandhi or Hitler led participants to judge themselves as less or more friendly, respectively, even though there was no influence of the prime on judgments of an ambiguously friendly novel target "Erik." This seems to suggest that automatic comparisons between two social targets—as occurs in the bystander negligence problem—occur less efficiently. But to the extent that the self is always somewhat accessible and comparison effects may be strongest when one is thinking about the target at the time of exposure to the comparison standard (Mussweiler et al., 2004), this suggests that automatic comparisons are most likely to occur when exposure to the comparison standard occurs together with exposure to the target. Given that the bystander and perpetrator are both introduced in the same problem, this may explain the apparent robustness of the comparison process in the bystander negligence problem.

This may speak to why Pepitone and DiNubile (1976) found that it was necessary to explicitly anchor participants' judgments for a contrast effect to emerge. In their studies, the order in which an assault and a homicide case were considered affected judgments about the second case. Without any reason to think about the first case in the context of the second, participants may have been better able to inoculate themselves from the contrast effect unless they had explicitly anchored their judgments for the first case. Under this circumstance, committing to scale responses about the first target may have shifted their interpretation of the scale, artifactually producing contrast effects. In our

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studies, bystander negligence is introduced in the context of the perpetrator's crime, making it a part of the judgment context while an impression of the perpetrator is being formed. This explanation is also consistent with a finding by Petty and Wegener (1993) that making the comparison standard and judgment targets part of "separate groups" eliminated comparison contrast. After rating a list of highly desirable locations (e.g., Jamaica), a more neutral place (e.g., Kansas City) was not rated as negatively if it was part of "the next group" of cities to rate. Although the authors stated that this cue served to "end the first task" and thereby prompt a correction process, a more parsimonious explanation may be that it simply reduced the likelihood of comparison to begin with. Although the present research coupled with past research is suggestive, future research will be necessary to specify the precise conditions under which automatic comparison contrast occurs.

Finally, this research raised a question about what exactly drives a comparison contrast effect. Although across an entire range of comparison standards (including those that are both greater than and less than the target of judgment), a contrast effect can be expected to manifest itself as a negative correlation (e.g., implied in Study 3; Tiedens, Unzueta, & Young, 2007), this should not necessarily be the case when looking at a restricted range of comparison standards that are uniformly less than or greater than the target of judgment (e.g., Studies 1 and 4). At times, even within a restricted range, this negative correlation likely holds. What predicts when one type of contrast or another will occur? Admittedly speculative, the positive correlation we observed may be more likely to occur when it is more difficult to assess the size of the difference between the comparison standard and the judgment target, such that the judgment target is then more likely to be encoded as "somewhat more/less than" the standard. As such, the target gets more of a contrast boost by the moderate low than the extreme low standard. When this difference in magnitude is more easily quantifiable, then the comparison may become more nuanced. Compared to a moderate low standard, one may judge the target as "a little more X" and thus see it as "fairly X," whereas, compared with an extreme low standard, one may see the target as "a whole lot more X" and thus see it as "extremely X." As a result, a negative correlation may emerge.

Applied Implications

In addition to these theoretical implications, the current findings may have important legal implications. If lawyers' intuitions are like our participants', they may unwittingly sabotage their own cases by providing information to the jury that ends up harming their clients.[1] A defense attorney may think that by highlighting a negligent act that enabled a crime, she is dividing up the total blame, thereby reducing blame for her client. Instead, our findings suggest that she may be paradoxically amplifying blame for her client by offering a standard of comparison that will likely frame her client's actions as more egregious and worthy of punishment. In defending ourselves from accusations, we often have the impulse to point out that we are not the only ones at fault, that others have committed small grievances as well. To the extent that we are the ones primarily at fault, resisting such reflexive blaming may save us not only from being labeled a tattler but from a blame-amplifying contrast effect as well.

To the extent that there is agreement that blame amplification from bystander negligence is not normatively justified, this research questions to what extent information about bystander negligence should be permitted in courts of law or other contexts where blame must be assessed. In each of our control conditions, we were able to describe the details of the crime without reference to the negligent bystander. A better understanding of the boundaries of the effect will enhance our ability to predict the applied contexts in which judgments will be contaminated. One question is whether a time delay between the presentation of information and the blame judgments would diminish the amplification of blame because of contrast. This would depend on whether the effect of the negligent bystander occurs at encoding or at the time of judgment (Higgins & Stangor, 1988). That is, if the bystander biased encoding, shifting a perception of the crime from "bad" to "very bad," then we can expect it to persevere despite a time delay. However, if blame amplification is occurring at the time of judgment, then as the salience of the bystander fades over time, amplification may be diminished. Future research is needed to tease apart these possibilities.

Conclusion

Judgments are not made in a vacuum. Instead, salient contextual standards influence how we perceive targets of judgment. In many cases of wrongdoing, bystanders commit minor acts of negligence that are necessary to permit the crime to occur. Such wrongdoing did not lead people to discount the blame for the actual perpetrators but did serve as a standard of comparison that framed the crimes as more severe. After repeatedly demonstrating these effects in the lab, future research will uncover to what extent these effects are demonstrated by grievance committees, courts of law, and other real-world allocators of blame.

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NOTE

1. The expertise of legal professionals has not immunized them from other basic judgmental biases. For example, Englich, Mussweiler, and Strack (2006) found that legal experts were just as prone to anchoring effects in sentencing decisions as were other participants.

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