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## Do Injustice and Mortality Salience Impact Secondary Victimization Through the Need to Believe in a Just World?

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### ABSTRACT

According to just-world theory, people need to believe in a just world (NBW). Theoretically, exposures to injustice and confronting mortality threaten this belief, prompting attempts to restore it. Past research has found that victimization of innocents and mortality salience prompts observers to engage in secondary victimization (e.g., blaming or derogating victims and underestimating their suffering). Theoretically, secondary victimization helps restore perceptions that the world is just. To test whether NBW might explain these effects, three experiments conceptually replicated prior work relying on this process explanation. Although our goal was to test whether NBW could be measured and might explain why secondary victimization occurs, we failed to find any substantive effects of exposure to injustice or mortality salience on secondary victimization.

Just-world theory asserts that people are motivated to perceive the world as predictable and fair (Lerner, 1980). The belief in a just world (BJW) is common and widespread (Alves et al., 2019; Alves & Correia, 2008, 2010a, 2010b), adaptive (Dalbert, 2001), and theorized to enable just-world believers to invest in future goals (Lerner, 1980; for a review, see Hafer & Rubel, 2015). BJW is also persistent: Even severe and chronic injustices do not always permanently reduce BJW (for a review, see Dalbert, 2001; see also Janoff-Bulman, 1992). However, stable individual differences in BJW exist (i.e., people vary in the extent to which they think the world is, in fact, just). Importantly, the *need to believe* in a just world (NBW; Hafer & Bègue, 2005), which is theorized to underlie how and why people develop their just-world beliefs, also likely varies not only person to person but within people and dependent on contexts. That is, the extent to which a person feels it is *important* to maintain a belief that the world is just at any given moment is variable and conceptually distinct from overall beliefs about the world as just. This suggests that whenever perceptions of the world's justice or predictability are threatened, NBW will increase, motivating people to restore BJW to its initial status (e.g., Ellard & Bates, 1990; Jost et al., 2004; Lupfer et al., 1998).

Although individual BJW varies, the world is not consistently fair: innocent victims *do* regularly suffer severe, unpredictable, and undeserved misfortunes. When observers are exposed to injustices, their BJW should therefore be challenged, prompting attempts to restore or defend it. Thus, when observers cannot personally restore justice (e.g., Lerner & Simmons, 1966), they may restore their *perceptions* of justice by engaging in secondary victimization (SV). For example, if innocent victims are blamed for their misfortune (Correia et al., 2001) or classified as bad people who deserved their misfortune (Landau et al., 2004), perceptions of injustice should decrease, helping to protect just-world beliefs.

Theoretically, more extreme instances of injustice pose a greater threat to BJW, motivating stronger efforts to restore it (for reviews, see Hafer & Bègue, 2005; Montada & Lerner, 1998). That is, more severe injustices require stronger coping strategies to preserve BJW. For example, observers engage in more SV when victims suffer continually (Correia & Vala, 2003), severely (Burger, 1981; Robbenolt, 2000), are unambiguously innocent (Correia & Vala, 2003), or when injustice seems unlikely to be corrected (Hafer, 2000; Loseman & van den Bos, 2012). Dispositional BJW is also positively associated with SV (e.g., De Judicibus & McCabe, 2001; Kleinke & Meyer, 1990),

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further suggesting that in many cases, SV is driven by NBJW (see also Furnham, 2003).

The strongest evidence in favor of the idea that NBJW causes SV has involved experimentally activating NBJW and then measuring reactions to an innocent victim. For example, Correia and Vala (2003, Study 2) found that student participants engaged in more victim derogation after their BJW was threatened by being told a master's degree is not likely to pay off financially. Similarly, Loseman and van den Bos (2012, Study 2) found that innocent assault victims were blamed more when the assault perpetrator was not caught. Conceptually, this is because unpunished perpetrators pose a greater threat to the BJW, motivating victim blaming to reduce the threat.

Related work has also shown how SV increases when people think about their own ultimate demise, with NBJW proposed as one reason why this should be the case (e.g., Hirschberger, 2006; Landau et al., 2004). That is, people may come to terms with their inevitable mortality by reinforcing their beliefs in cultural norms, creating the sense that their culture and way of life are immortal, even if their individual selves are not (Burke et al., 2010; Greenberg et al., 1986). According to Alves et al. (2019), BJW is one such cultural norm, and priming death has been shown to motivate people to reinforce their BJW (e.g., Greenberg et al., 1997; Landau et al., 2004; Pyszczynski et al., 2003). Furthermore, believing the world is fair may help people to avoid thinking about the possibility of suffering or dying unexpectedly or undeservingly, mitigating concerns over vulnerability and risk of death (Hirschberger, 2006).

Taken together, these findings suggest that NBJW may be causally implicated in the way that perceiving injustice or considering one's own death can lead to SV, even when an NBJW-activating stimulus is not directly related to the victim being blamed. However, although this argument has been used to explain various SV effects (e.g., Callan et al., 2006; Callan et al., 2013; Correia & Vala, 2003; Dawtry et al., 2018; van den Bos & Maas, 2009; for reviews of literature implicating BJW or NBJW in SV, see Furnham, 2003 and Hafer & Bègue, 2005), NBJW has not typically been measured. Instead, NBJW has been hypothesized as a mechanism that *transmits* the effects of these manipulations to SV, with its role assumed because of the success of the manipulations.

This gap in the literature led to the current research. Using conceptual replications of established paradigms, we set out to experimentally manipulate variables previously shown to impact SV (i.e., Correia

& Vala, 2003, Study 2; Hirschberger, 2006, Study 3; Loseman & van den Bos, 2012, Study 2), intending to test whether temporary increases in observers' NBJW could be measured directly and whether they could explain subsequent increases in SV. Importantly, although we focused on a direct measure of NBJW that we created for the purpose, we also considered that changes in NBJW as a function of these manipulations might not be consciously accessible or reported, necessitating some form of indirect measurement (see Hafer, 2000; see also Modesto & Pilati, 2017). Likewise, we understood that any relationship between experimental manipulations and SV might be better explained by other mediators than NBJW. As one possibility, SV effects that follow confronting one's mortality, seeing others unjustly harmed, or being told that the world is not fair may be driven by a need to repudiate one's *personal* vulnerability (see Hirschberger, 2006). For example, to the extent that victims' serious injuries can be blamed on the victims rather than an unjust world, observers could then deny the reality that their own lives are fragile and might be unpredictably and unjustly ended at any time. If true, this would suggest that engaging in SV can help people restore the belief that they are not vulnerable to unprovoked attack or injury, and that their own lives will not end unexpectedly or without just cause. Likewise, other mediators, such as an increased focus on long-term goals (see Callan et al., 2013; Hafer, 2000), might better explain the relationship between various experimental manipulations and SV.

As we began this work, we were prepared to engage with these ideas. However, even using relatively large sample sizes that should have provided reasonably stable parameter estimates<sup>1</sup> (which we expected would be similar to previously reported effect sizes), we were unable to replicate the effects reported by Correia and Vala (2003), Loseman and van den Bos (2012), and Hirschberger (2006) on SV. Thus, we did not continue with further investigation of these possibilities. Although we discuss potential limitations of our attempts to find SV effects in the General Discussion and believe further research is needed to draw firm conclusions, we also believe that these limitations are not profound enough to explain why our manipulations consistently had little effect. Ultimately, despite knowing that our failed replications might be due, for instance, to differences in sample characteristics (see, e.g., Ma-Kellams & Blascovich, 2011) or in experimental implementation, we also considered whether the size of the SV effect across these experimental

contexts is smaller and harder to reproduce than previous research might suggest.

## The present research

The present research began with the goal of examining whether conscious endorsement of the need to believe in a just world (NB JW) helps explain why manipulations of justice or mortality salience influence secondary victimization (SV). Although direct messages about the world's injustice (Correia & Vala, 2003), descriptions of criminals unpunished for their crimes (Loseman & van den Bos, 2012), and mortality salience (Hirschberger, 2006) have each been found to increase SV, direct evidence for the role of NB JW in this process has not been provided, even though NB JW has frequently been put forward as an explanatory mechanism.

Three experiments were designed to test whether NB JW explains the tendency to engage in SV. We adapted previous manipulations intended to activate NB JW: describing the world as economically unjust (Experiment 1; e.g., Correia & Vala, 2003), presenting participants with an attacker who goes uncaught and unpunished after severely harming an unambiguously innocent victim (Experiment 2; e.g., Loseman & van den Bos, 2012), and making participants' mortality salient to them (Experiment 3; e.g., Hirschberger, 2006). In each study, we also used a straightforward, face-valid measure of NB JW that we developed. If NB JW explains why manipulations of justice or mortality salience increase SV, we expected these same manipulations to influence scores on this measure, helping explain the effects of these manipulations on SV.

We also aimed to examine the implicated causal role of NB JW by varying the order of NB JW and SV measures. We hypothesized that if NB JW was measured before SV, it would be heightened in unjust (vs. just) and mortality salience (vs. control) conditions and might mediate SV. However, because SV theoretically helps restore just-world beliefs, measuring NB JW *after* SV should reduce the extent to which experimental condition influenced NB JW responses. In sum, regardless of whether NB JW was measured before or after SV, we expected participants to secondarily victimize (e.g., blame, derogate) innocent victims to a greater extent when presented with information suggesting the world is unjust (vs. just) or when asked to consider their own mortality (vs. control). In contrast, we expected an interaction between manipulations of justice or mortality salience

and ordering of measures on NB JW, such that when NB JW was measured first, unjust and mortality conditions would produce higher NB JW than comparison conditions, but when it was measured after SV, differences would be smaller. As noted earlier, our goal in conducting this research was straightforward. Yet, in each of these experiments, we failed to even find consistent effects on SV, regardless of whether it was measured before or after NB JW.

All research reported in this manuscript was approved by the Institutional Review Board at the University of Illinois at Urbana-Champaign. Prior to participating in any experiment, participants provided consent to participate by reading a consent document on a computer monitor and clicking a button to continue, indicating their willingness to participate. Fully deidentified data for all experiments can be found at <https://tinyurl.com/5btcv3rd>. At the same location, an Online Supplemental Materials (OSM) document can be found that provides verbatim wording of all manipulations and measures.

## Experiment 1

Correia and Vala (2003) found that student participants derogated innocent victims more after being told that a master's degree is not likely to pay off financially. However, no measure of NB JW was included in this study, making it difficult to know whether increases in SV were caused by increases in consciously accessible NB JW, or instead by changes in another variable affected by the justice manipulation. Experiment 1 conceptually replicated Correia and Vala (2003) by using a manipulation designed to threaten the BJW of our American (MTurk) sample. We expected that when the world was described as less versus more just, participants would engage in more SV. We also created and employed a straightforward, face-valid measure of NB JW, hypothesizing that participants would report stronger NB JW after the world was described as unjust (vs. just). We varied the order of the NB JW and SV measures, predicting that the effects of our manipulation on NB JW would be lessened when NB JW was measured after participants were given an opportunity to restore perceptions of justice by engaging in SV.

## Method

### Participants

After excluding participants who completed the study more than once (participants' first completion was

retained), responded incorrectly to an attention check question, or did not respond to all measures ( $n = 69$ ), participants were 302 US residents recruited from Amazon Mechanical Turk (125 females, 176 males, 1 unreported;  $M_{\text{age}} = 36.10$ ,  $SD = 11.99$ ), more than doubling the final sample size of  $n = 143$  reported in Correia and Vala (2003; Study 2; effect size  $d = 0.40$ , calculated from their provided  $M$  and  $SD$ ). In our sample, participants were residents of 43 US states and self-identified as White/European American (71.5%), Black/African American (9.3%), Hispanic/Latino(a) (7.0%), Asian/Asian American (6.3%), Native American/Pacific Islander (2.3%), other (3.3%), or did not respond (0.3%). Most (98.0%) were native English speakers, with all nonnative English speakers having spoken English for at least 10 years. Participants leaned slightly liberal ( $M = 3.63$ ,  $SD = 1.82$ ) in response to the question, “Where would you place yourself on the following ideological spectrum?” (1 = *very liberal*, 2 = *liberal*, 3 = *somewhat liberal*, 4 = *moderate (middle-of-the-road)*, 5 = *somewhat conservative*, 6 = *conservative*, 7 = *very conservative*).

### Procedure and measures

Participants read a realistic but fabricated news article describing economic conditions in the US as either (just) or [un]just], titled, “Bureau of Economic Analysis (Reinforces Confidence in) [Dispels Myths of] American Meritocracy.” The article stated that a large, 10- year longitudinal study with 480,000 participants measured the number of hours worked, sick days taken, and self-reports of hard work, finding (a strong, positive relationship) [virtually no relationship] between hard work and career success (see OSM for verbatim materials and measures). Participants then responded to a comprehension check, “According to the article, in the United States...” (1 = *hard work does not lead to success*, 2 = *hard work leads to success*, 3 = *most adults are employed*)<sup>2</sup> and, to distract from our true hypotheses, also rated how informative, interesting, understandable, and clearly written the article was.

Participants then read a second fabricated news article about a man who contracted HIV from his girlfriend. Because Correia and Vala (2003) found the strongest effect ( $d = 0.62$ ) of their justice manipulation on victims who were unambiguously innocent and when the victim suffered seriously and chronically, the man in the story was described as having contracted HIV from his long-term girlfriend who did not know she was HIV-positive. The man said he and his girlfriend “always used condoms,” but that once, a

condom broke, and “one time was enough” to contract HIV. Likewise, the article stated that “[HIV] remains uncured and life-changing,” and that “even insured patients ... spend, on average, \$14,000 per year suppressing the disease.”

After this, participants were randomly assigned to respond to NBJW and SV measures, with either NBJW or SV coming first. A face-valid, 8-item measure of NBJW ( $\alpha = .88$ ) was created expressly for this study: “Whether or not it is ‘true,’ it is important for me to believe that the world is a fundamentally fair place,” “It is important for me to believe that working hard eventually pays off,” “I need to believe the world is a predictable place,” “I would like to believe that injustices are rare,” “I need to believe the world is fundamentally just,” “I want to believe that injustice is the exception rather than the rule,” “It is important to believe that good things happen to good people,” and “It is important to believe that people do not get away with bad behavior” (1 = *strongly disagree*, 7 = *strongly agree*).

Secondary victimization was assessed in a number of ways, with like items presented together and the order of items individually randomized. Victim liking and derogation ( $r = -.13$ ) were measured by asking participants, “Which of the following words do you think best describe George? Select all that apply” (liking: *polite, responsible, mature, nice, warm*; derogation: *stupid, selfish, nervous, thoughtless, a liar*) and respectively summing the number of positive and negative adjective selected. Three items measured deservingness of the target’s situation ( $\alpha = .87$ ): “How would you describe George’s situation?” (1 = *completely unfair*, 7 = *completely fair*), “George deserves what happened to him” (1 = *completely disagree*, 7 = *completely agree*), “It is George’s fault that he contracted HIV” (1 = *completely disagree*, 7 = *completely agree*). A single item asked about the target’s suffering, “How much do you think George suffers because of his HIV?” (1 = *very little*, 7 = *very much*).

### Results

Main effects (i.e., effect sizes) of article condition, order condition, and their interaction were examined. Means and standard deviations are provided in Table 1.

Regarding our measure of NBJW, the effect size of article condition was extremely small ( $d = 0.08$ ),<sup>3</sup> with the mean in the just-world condition actually *higher* than that in the unjust-world condition. The order of the NBJW measure also had minimal impact

**Table 1.** Means and standard deviations of dependent measures in experiment 1.

	NB JW measured first				NB JW measured second				Both conditions			
	Unjust world		Just world		Unjust world		Just world		Unjust world		Just world	
	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>
NBJW	4.78	1.26	4.99	1.11	5.05	1.20	4.98	1.19	4.89	1.24	4.99	1.14
Liking	2.07	1.53	2.41	1.60	2.05	1.77	1.76	1.41	2.06	1.64	2.13	1.55
Derogation	0.61	1.09	0.69	1.08	0.95	1.33	0.57	0.84	0.76	1.21	0.64	0.98
Deservingness	2.82	1.70	2.73	1.58	3.23	1.89	3.30	1.82	3.00	1.79	2.98	1.71
Suffering	5.67	1.45	5.66	1.14	5.73	1.33	5.63	1.27	5.69	1.40	5.64	1.19

Note. NBJW: Need to Believe in a Just World measure. Respectively, when measured first and second, NBJW was measured before and after participants had the opportunity to secondarily victimize the victim.

( $d=0.10$ ), with reported need to believe in a just world higher when it was measured *after* rather than before participants had an opportunity to derogate the innocent victim. The interaction of the two factors had a similarly small associated effect size,  $\eta_p^2 = .004$ .

Similar to the effect on NBJW, effect sizes capturing the impact of article condition (i.e., just vs. unjust world) on liking ( $d=0.04$ ), derogation ( $d=0.11$ ), deservingness ( $d=0.01$ ), and perceived victim suffering ( $d=0.04$ ) were all small, with some effects (e.g., derogation) consistent with the hypothesized direction (i.e., unjust world > just world) but others (e.g., victim suffering) in the opposite direction. Effects associated with whether NBJW was measured before or after other dependent measures were small for liking ( $d=0.21$ ), derogation ( $d=0.10$ ), deservingness ( $d=0.28$ ), and suffering ( $d=0.02$ ), suggesting little impact—albeit slightly higher than impacts of the just-world manipulation—on attitudes toward the target. Finally, interactions between the factors accounted for minimal variance in dependent measures ( $\eta_p^2$  for liking, derogation, deservingness, and suffering were, respectively, .01, .01, .00, and .00).

## Discussion

Experiment 1 was a conceptual, but relatively close, replication of the experimental conditions in Correia and Vala (2003; Study 2) that found the largest impacts of a just-world manipulation on victim blaming. Our intention was to examine whether a consciously endorsed measure of the need to believe in a just world might statistically mediate the victim blaming we expected would be higher in the unjust-world condition. As an experimental test, we also expected that NBJW scores, to the extent that they captured the hypothesized sentiment, would be lower when measured after (vs. before) participants had the opportunity to derogate the victim (i.e., by restoring just-world beliefs). However, each of the hypothesized effects was very small, and for some, the direction of findings was opposite from our prediction. This included

primary effects of the just-world manipulation on secondary victimization.

Of course, there could be many reasons for our failure to replicate effects. For example, the participants in our sample may not have been affected by our economic justice manipulation in the same way that Portuguese undergraduates in the original study were affected by a statement that higher education does not pay off. However, even if true, this calls into question whether results (i.e., regarding SV) by Correia and Vala (2003) would generalize to a new sample from a different culture (see, e.g., Ma-Kellams & Blascovich, 2011) or replicate using a slight modification in experimental materials. In addition, it remained an open question whether the SV others have found was driven by threats to participants' NBJW, or whether past effects were caused by some other consequence of their manipulation(s) (e.g., participants' increased feelings of personal vulnerability or anxiety).

## Experiment 2

Having failed to find any hypothesized effects of NBJW, and indeed, any substantive effects of our just-world manipulation on any of several measures we used to capture SV, we ran a second study using a new (in)justice manipulation. We again employed a previously used method that was also an attempt to simplify our design, in which the innocent victim being evaluated was directly connected to the manipulation of justice. Specifically, we adapted a method used by Loseman and van den Bos (2012, Study 2), who found that an innocent victim of assault was blamed less for their victimization when the perpetrator was caught relative to when they were not ( $d=0.72$ ; see also Hafer, 2000), a finding consistent with the notion that people engage in SV to defend their BJW. To increase personal relevance (and therefore threat to just-world beliefs), participants from a university subject pool read a fabricated news article describing a violent mugging in a well-frequented area

near their campus. Justice was manipulated by describing that the mugger had been caught and would likely be punished or had escaped and would likely not be punished. As in Experiment 1, we included an NBJW measure, varying the order of this measure with SV measures. Our hypotheses were the same as in Experiment 1.

## Method

### Participants

After excluding 19 participants for failing one or more comprehension checks, participants were 201 students recruited from a subject pool from a large Midwestern university, in exchange for course credit (120 female, 80 male, 1 other;  $M_{\text{age}} = 19.51$ ,  $SD = 1.42$ ). This sample size nearly doubled the sample size ( $n = 112$ ) reported in Loseman and van den Bos (2012, Study 2,  $d = 0.72$  calculated from provided means and standard deviations). Participants self-identified as White/European American (38.3%), Asian/Asian American (28.9%), Hispanic/Latino(a) (17.9%), Black/African American (11.9%), Native American/Pacific Islander (0.5%), or other (2.5%). Most participants reported English as their native language (77.6%); of those who reported otherwise, average years having spoken English was  $M = 13.64$ ,  $SD = 3.82$ . In response to the same political ideology question used in Experiment 1, participants again leaned liberal ( $M = 3.10$ ,  $SD = 1.22$ ).

### Procedure and measures

Participants read a fabricated but realistic news article describing an innocent victim who was violently mugged. The victim was described as walking home from his job at 10:30 PM when he was approached and robbed at knifepoint. The victim tried to run from the mugger, but was tackled, stabbed in the back, and taken to the hospital in critical condition (see OSM for full wording). To increase the relevance and impact of the stimulus, the victim was described as a student at the university where the experiment was conducted, and the crime was said to have occurred recently and on a street near the university campus, in a location where many bars and restaurants frequented by students were also located, and where armed assaults are not uncommon. Justice was manipulated by stating that the criminal escaped and was unlikely to be caught (unjust) or that the criminal was in custody and facing up to 20 years in prison (just).

After this, participants' comprehension of critical details was checked, and they responded to primary dependent measures, with participants randomly assigned to complete either SV or NBJW items first. NBJW was measured using the same items as in Experiment 1 ( $\alpha = .77$ ). As in Experiment 1, SV was measured in several different ways, with like items being presented together in random order, and overall order of item presentation also randomized. Blame ( $\alpha = .75$ ) was measured with five items: "Jacob Davis was stabbed because of how he behaved," "Jacob Davis is to blame for what happened to him," "Jacob Davis behaved irresponsibly," "Jacob Davis behaved carelessly," and "I feel sorry for Jacob Davis" (reverse-scored) (1 = *completely disagree*, 2 = *disagree*, 3 = *somewhat disagree*, 4 = *neither agree nor disagree*, 5 = *somewhat agree*, 6 = *agree*, 7 = *completely agree*). Single items measured fairness, "How would you describe Jacob Davis's situation?" (1 = *completely unfair*, 7 = *completely fair*) and perceived suffering, "How much do you think Jacob Davis suffered?" (1 = *very little*, 7 = *very much*).

## Results

Main effects (i.e., effect sizes) of article condition, order condition, and their interaction were examined. Means and standard deviations are provided in Table 2.

As with Experiment 1, there was almost no effect of either just-world condition ( $d = 0.02$ ) or NBJW order ( $d = 0.10$ ) on self-reported NBJW, and their interaction ( $\eta_p^2 = .00$ ) did not explain any substantive variance. As for the expected impact of just-world condition on SV (respectively, victim blaming, perceived fairness, and perceived victim suffering), the effects were again very small (respectively,  $ds = 0.09$ , 0.16, and 0.04) and in mixed directions. That is, although blaming was slightly higher in the unjust (vs. just) condition, perceived fairness was higher in the just (vs. unjust) condition.<sup>4</sup> The effects of NBJW order on blaming, fairness, and victim suffering, respectively, were also small ( $ds = 0.01$ , 0.14, and 0.07), as were the interactions,  $\eta_p^2 = .01$ , .01, and .00.

## Discussion

As in Experiment 1, not only was the effect of the just-world manipulation on measured NBJW small, but its effect on three measures of SV (victim blame, perceived fairness of the outcome, and victim suffering) were also very small. Moreover, the effects of

**Table 2.** Means and standard deviations of dependent measures in experiment 2.

	NBJW measured first				NBJW measured second				Both conditions			
	Unjust world		Just world		Unjust world		Just world		Unjust world		Just world	
	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>
NBJW	4.48	0.98	4.45	1.07	4.36	0.90	4.39	0.98	4.41	0.93	4.43	1.03
Victim Blame	1.68	0.71	1.78	0.83	1.82	0.85	1.58	0.58	1.76	0.79	1.69	0.73
Fairness	1.54	0.81	2.00	1.61	1.66	1.15	1.57	1.09	1.61	1.01	1.81	1.42
Suffering	6.54	0.59	6.51	0.74	6.48	0.73	6.45	0.71	6.51	0.67	6.48	0.72

Note. NBJW: Need to Believe in a Just World measure. Respectively, when measured first and second, NBJW was measured before and after participants had the opportunity to secondarily victimize the victim.

exposure to a just-world manipulation were mixed, depending on measure. Thus, although our goal was again to examine the role of NBJW in explaining SV differences, those differences did not consistently emerge, particularly compared with the findings on which the experiment was based (Loseman & van den Bos, 2012; Study 2;  $d = 0.72$ ).

As with Experiment 1, although this work was not a direct replication and necessarily involved a new sample, it was similar to Study 2 in Loseman and van den Bos (2012). For example, both used similar descriptions and sampled university students. However, in this earlier study, it is worth noting that the SV effect only emerged in a control condition where a neutral distractor task (recalling and writing down three brands of detergent) was used to balance a self-affirmation task intended to decrease SV. In our study, we used no such task, as manipulating self-affirmation was not part of our design. Possibly, this brief delay may have amplified the effects of the justice manipulation on SV. We addressed this concern in Experiment 3, when we switched from direct manipulations of justice to examine a related construct that has been previously shown to impact SV: mortality salience.

### Experiment 3

Although Experiments 1 and 2 failed to find a role for NBJW in explaining SV effects (i.e., because neither NBJW nor SV responded strongly or consistently to our manipulations), we remained interested in this topic. However, after two different conceptual replication attempts using just-world manipulations returned weak results, we decided to use a different approach. In particular, we decided to use a mortality salience induction.

Because priming death has been shown to motivate people to reinforce their BJW (Greenberg et al., 1997; Landau et al., 2004; Pyszczynski et al., 2003) and engage in SV (e.g., Hirschberger, 2006; Landau et al., 2004, Study 5), we first instructed participants to think about their own mortality (vs. a commonly used

control topic of visiting the dentist). Prior research (e.g., Pyszczynski et al., 1999; Steinman & Updegraff, 2015; cf. Trafimow & Hughes, 2012) has suggested that proximal defenses are rational and threat-focused when death-related thoughts are conscious, but when these thoughts are accessible but not conscious, distal defenses involve increasing faith in cultural world-views (e.g., BJW). To achieve this state, a delay was introduced between the mortality salience manipulation and dependent measures. In addition, participants were presented with an unambiguously innocent victim who suffered severe harm that should be relevant to participants. We believed that this combination of factors would be sufficient to reveal, at least, an effect on SV.

### Method

#### Participants

After excluding twenty-six participants who responded incorrectly to one or more comprehension checks, participants were 188 students recruited from a large midwestern university subject pool in exchange for course credit (131 female, 56 male, 1 prefer not to say;  $M_{\text{age}} = 18.89$ ,  $SD = 1.22$ ). Although this sample size is only slightly larger than that reported in Hirschberger (2006, Study 3;  $N = 169$ ), it is considerably larger than the assumed sample size ( $\approx 42$ – $44$ ) where the largest effects of SV were found. That is, the overall main effect of SV (victim blaming) across all between-participants conditions (calculated from provided means; Hirschberger, 2006) was  $d = 0.24$ . A slightly larger effect ( $d = 0.40$ ) was found within a severe (vs. mild) injury condition, which, because of the between-participants design, likely had approximately half of this number of participants (e.g.,  $n \approx 85$ ). However, the largest effect ( $d = 1.02$ ), and likely the effect driving other main and simple main effects (i.e., because other simple, simple main effects were of negligible size, based on provided means), was found for a careful (vs. reckless) driver who was severely (vs. mildly) injured. Again, given the design,



**Table 3.** Means and standard deviations of dependent measures in experiment 3.

	NB JW measured first				NB JW measured second				Both conditions			
	Mortality		Anxiety		Mortality		Anxiety		Mortality		Anxiety	
	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>
NB JW	4.45	0.98	4.30	0.90	4.65	0.85	4.50	0.94	4.55	0.91	4.40	0.92
Victim Blame	2.18	0.92	2.11	0.90	2.35	1.17	2.25	0.93	2.27	1.05	2.18	0.91
Fairness	3.00	2.06	2.47	1.83	2.82	2.18	3.00	2.17	2.91	2.11	2.73	2.01
Suffering	6.46	0.69	6.45	0.65	6.51	0.62	6.15	0.99	6.48	0.65	6.30	0.84

Note. NB JW: Need to Believe in a Just World measure. Respectively, when measured first and second, NB JW was measured before and after participants had the opportunity to secondarily victimize the victim.

the means that form the basis for this effect size were likely around 21–22 participants in each cell.

Participants identified as White/European American (36.2%), Asian/Asian American (32.4%), Hispanic/Latino(a) (18.1%), Black/African American (9%), or other (4.3%). Most of these participants reported being native English speakers (68.6%); those who reported otherwise reported having spoken English for 12.19 years on average ( $SD = 4.69$ ). On the same political ideology scale used in Experiments 1 and 2, participants again skewed slightly liberal ( $M = 3.09$ ,  $SD = 1.19$ ).

### Procedure and measures

Participants completed the Mortality Attitudes Personality Survey (MAPS; Rosenblatt et al., 1989), a widely used mortality salience manipulation (see Burke et al., 2010). Participants in the mortality salience condition described in their own words what would happen to them when they physically die, as well as what emotions thinking about their own death evokes. Participants in an anxiety control condition instead described what would happen to them physically the next time they went to the dentist, as well as what emotions thinking about this evokes. A meta-analysis by Burke et al. (2010) found that a delay between the mortality salience manipulation and key dependent variables tends to increase the size of mortality salience effects, so participants next completed the Positive and Negative Affect Schedule (PANAS; Watson et al., 1988), which has frequently been used as a distractor in studies testing the effects of mortality salience, and is similar in length to the 19-item distractor measure reported in Hirschberger (2006).<sup>5</sup>

Participants then read an article similar to the unjust version of the fabricated news article from Experiment 2, where a man was violently mugged near the university where the experiment was conducted, and the assailant escaped and was not expected to be caught (see OSM for full wording). After responding to the same comprehension checks as in Experiment 2, participants completed the same NB JW ( $\alpha = .72$ ) or SV measures (victim blaming  $\alpha =$

.71; single item measures of fairness and suffering) as were used in Experiment 2, with the order of these measures randomly assigned.

### Results

Main effects (i.e., effect sizes) of salience condition (i.e., mortality vs. anxiety), order condition (NB JW measured before vs. after SV measures), and their interaction were examined. Means and standard deviations are provided in Tables 2 and 3.

As in Experiments 1 and 2, the effect of the primary manipulation on NB JW was quite small ( $d = 0.16$ ), although reported NB JW was slightly higher in the morality salience (vs. dentist anxiety) condition. The order of when it was measured had a similarly small effect ( $d = 0.23$ ), but was *higher* after participants had an opportunity to secondarily victimize the victim. The interaction did not account for any appreciable variance,  $\eta_p^2 = .00$ .

Likewise, the effects of the salience condition on variables measuring SV were small (respectively for victim blaming, perceived fairness of the outcome, and the suffering of the victim,  $ds = 0.09$ ,  $0.09$ , and  $0.24$ ), with blaming and fairness slightly higher in the morality salience (vs. anxiety) condition, but higher perceived suffering in the same condition, which is contrary to the main hypothesis.<sup>6</sup> Effects of the NB JW order variable were similarly small (blaming  $d = 0.15$ ; fairness  $d = 0.09$ ; suffering  $d = 0.15$ ). Although blaming and fairness were slightly higher when measured before NB JW, suffering was slightly higher when measured after NB JW. Variance accounted for by interactions was minimal for all variables (respectively for blaming, fairness, and suffering,  $\eta_p^2 = .00$ ,  $.01$ , and  $.01$ ), suggesting that when NB JW was measured only minimally impacted the effects of the salience manipulation on dependent measures.

### Discussion

Experiment 3 was a third attempt to show how a consciously endorsed need to believe in a just world

might help explain effects of a manipulation on secondary victimization. Rather than manipulating beliefs about justice as in Experiments 1 and 2, Experiment 3 used a manipulation of mortality salience, which has also been shown to impact how people evaluate innocent victims, theoretically by increasing participants' need to believe the world is just. Although this was the goal, once again, effects of the primary manipulation failed to have any substantive effect on our NBJW measure. More importantly, measures of secondary victimization also failed to reveal any substantive effects, with effect sizes much smaller than those reported in the literature.

### General discussion

The present research was motivated by a desire to uncover whether people's need to believe (or to restore their beliefs) in a just world helps explain why, after challenging their beliefs that the world is fair, they sometimes secondarily victimize innocent victims—particularly when victims' suffering is severe and long-lasting and the injustice has not been remedied (e.g., Burger, 1981; Correia & Vala, 2003; Hafer, 2000; Loseman & van den Bos, 2012; Robbennolt, 2000). In setting out to study this idea and test this theoretical explanation, we understood that threats to people's just-world beliefs might not be consciously accessible. We were therefore prepared to seek out or develop additional measures of NBJW (e.g., Hafer, 2000) or test other process accounts to examine what underlies secondary victimization in these cases. However, we were surprised to find that not only did our measure fail to respond to the manipulations we used, but there was no consistent and strong effect of our manipulations on the secondary victimization effects we hoped to explain.

The three experiments we conducted were conceptual replications of past work. In order to get adequate estimates of effect sizes, we collected sample sizes that were larger than those for which effect sizes on secondary victimization—often large—had been reported. Our first experiment conceptually replicated the justice manipulation reported in Correia and Vala (2003; Study 2), where college students who were told that their master's degree would not pay off financially derogated an unambiguously innocent victim whose suffering was serious and chronic. Our replication, which used MTurk participants rather than students, informed them that hard work would either pay off or not before they were offered a chance to derogate a person who, through no fault of their own, contracted

HIV. Our second experiment conceptually replicated Loseman and van den Bos (2012, Study 2), who found that college students blamed an innocent victim of assault less for their victimization when the perpetrator was caught relative to when they were not. We also used a college student sample and believed the threat to just-world beliefs would be strong when the perpetrator had not been apprehended, given that the victim was a student from the same university and the crime occurred in a well-frequented location near campus. The last experiment was a conceptual replication of work by Hirschberger (2006, Study 3), which relied not on a manipulation of justice in the sense of hard work not paying off or unpunished harms to innocent victims, but in the ultimate knowledge of our own mortality. As in their study, ours again relied on a college student sample. Across each of these studies, our results indicated both inconsistency (i.e., in terms of the hypothesized relations between our manipulations and measures of SV) and effect sizes that were much smaller than those previously reported, regardless of their direction.

To be clear, we wanted these studies to “work” (i.e., return reasonably large effect sizes in the predicted directions). We also fully expected that the manipulations we chose *would* work, at least in terms of finding effects on SV, even if we were less certain about our ad hoc measure of consciously endorsed NBJW. Yet, despite these expectations, we did not find the effects we sought.

We are not claiming that manipulations of justice or mortality salience have no impact on SV. Our only claim is that in these studies, we found little, if any, evidence for their impact. Of course, we acknowledge that there could be more than one reason why our manipulations failed to capture the same phenomenon that others captured. For example, the samples other authors used were drawn from student populations in Europe and Israel. Our samples were drawn from American MTurk workers and college students from the American Midwest (see, e.g., Ma-Kellams & Blascovich, 2011). Our studies were also conducted in 2018–2019, while these others were conducted 10–20 years ago. Speculatively, given the recent and current media environment, Westerners' concerns for victims have increased, or norms have changed such that holding victims to account for their misfortunes is not socially desirable. In addition, our studies were not exact replications; instead, we attempted to draw on what we believed to be the most important aspects of the studies we were conceptually replicating, and small differences between our studies and these others

could have had important impacts. Another consideration is that the victims in our experiments were always male, which differed from the work we were attempting to conceptually replicate. That is, in Correia and Vala (2003; Study 2), the gender of the victim was anonymous, and in Study 2 reported by Loseman and van den Bos (2012), the victim was female. Possibly, participants might have been more hesitant to secondarily victimize these male targets than they were to victimize targets in these earlier reports. In addition, if target gender plays a role in determining effects of secondary victimization, effects of perceiver gender and perceiver  $\times$  target gender interactions become important to consider (e.g., Xie et al., 2019). Although questions such as these were not the focus of the current research, it may be important moving forward to develop studies that systematically manipulate target gender along with other key variables.

In sum, we reiterate that various factors might be implicated in our inability to replicate the substantial effect sizes reported in past research on this topic. That is, any of these differences alone or in combination could help explain why we failed where others succeeded. However, if our failure to find substantive effects does reside in these differences, it nevertheless implies some need for caution in interpreting past findings. For example, it might suggest that these effects aren't as robust or strong in the present as in the past, that SV as a function of these variables does not emerge in the same way for samples from the United States or for female and male targets or participants, or that the effect sizes previously documented might sometimes fail to replicate when using conceptually similar (i.e., rather than exact) manipulations. However, these limitations may not be profound enough to explain why our manipulations had little effect, particularly when considering the consistently small effects *across* all three experiments. That is, another possibility is that the size of the SV effect may be smaller than previous research suggests.

Based on our findings, we believe that the question of what mechanism drives SV effects in the context of justice or mortality salience manipulations remains open and unresolved. Moreover, we believe that additional research, using preregistered designs along with exact and conceptual replications conducted by different labs, is needed to verify the size and scope of these effects involving secondary victimization.

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## Notes

1. Or, if using traditional NHST, sufficient power.
2. Respectively, most participants in the (unjust, just) world condition (128/150, 141/152) responded that the article's conclusion was that hard work (does not lead, leads) to success. No participants were excluded for answering this question incorrectly. Analyses that removed these participants were similar to those where they were retained, with no consistent pattern of results in evidence across measures.
3. Effect size  $d$  is reported as an absolute value throughout this article.
4. When retaining all participants, including inattentive ones, the effect of just-world condition on fairness when NBJW was measured first was  $d = 0.42$ , with participants in the unjust (vs. just) world condition indicating that the outcome was less fair (i.e., contrary to hypotheses). When NBJW was measured second, participants in the unjust (vs. just) condition blamed the victim more ( $d = 0.57$ ), consistent with hypotheses. Effect sizes for all other variables were small. Given that these results including inattentive participants were found for only one of four variables, for different variables in different order conditions, and returned inconsistent directional effects suggests they should be interpreted with caution.
5. An anonymous reviewer pointed out that this delay task might not be substantial enough for distal defenses to emerge. We agree that this is possible, but wanted to match our methods as closely as possible to those of Hirschberger (2006; Study 3).
6. Analyses that included all participants, including inattentive ones, returned uniformly small effect sizes for all factors on all dependent measures.

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