

Changing the Public's Crime Control Theater Attitudes

Daniel A. Krauss, Gabriel I. Cook, Sharda Umanath, and Eunice Song

Department of Psychological Science, Claremont McKenna College

Crime control theater (CCT) are criminal justice policies and laws that are widely favored by the public but are demonstrated empirical failures. Across two experiments, this research examines video interventions designed to change the public's views toward two sexual offender CCT laws—sex offender housing restrictions and sex offender registry laws. In Experiment 1 ($N = 217$), both factual and narrative-based interventions were successful in lowering participants' ratings of *support* but not *effectiveness* for these policies. However, whereas participants viewed the narrative-based videos as more engaging, coherent, and emotional, these differences did not translate into more significant attitude transformations or learning of the unintended consequences of these policies. In Experiment 2 ($N = 133$), these findings were largely replicated, and, importantly, the decrease in participants' *support* for these policies after the intervention was maintained a week later, with smaller decreases in participants' ratings of *effectiveness* evident. Yet, the exact mechanism of these attitudinal changes remains unclear but appears unrelated to their memory for or engagement with the interventions. The policy implications of these findings are discussed.

Keywords: crime control theater, criminal justice policy, learning and memory, narrative interventions, public attitudes

Crime control theater (CCT) refers to a category of laws and/or legal policies that are intuitively appealing as solutions to important criminal justice goals but are actually well-documented empirical failures. Despite their lack of success, these policies continue to receive exceptional public and legislative support (e.g., Krauss et al., 2021). This unflagging backing for CCT laws is problematic for several reasons, including but not limited to the facts that these laws (a) suggest that steps are being taken to confront important criminal justice issues when current policy is not successful, (b) waste resources that could be used to reform these ineffective laws or to develop and promote more appropriate alternative solutions, and (c) create unintended consequences that may further exacerbate the problems these laws were intended to solve (DeVault et al., 2016; Griffin & Miller, 2008). Unfortunately, limited, extant research has found that CCT laws are relatively impervious to attempts to change the public's view toward them, leading to little pressure and impetus for

legislators to amend, reform, or discontinue these laws (e.g., Campbell & Newheiser, 2019; Griffin & Miller, 2008).

Using two prototypical CCT policies, sex offender registration and sex offender housing restriction laws, the present research examines a novel attempt to shift the public's favorable attitude for these laws through video presentations that highlight their unintended harmful repercussions. First, we focus on exploring whether narrative appeals targeted at more emotion-based decision-making are more successful than fact-based approaches in changing the participant samples' views. Second, we assess the stability of change (if any) in the public's attitudes toward these CCT laws by examining shifts in attitude immediately after intervention as well as after a week has passed. Narrative information regarding CCT laws may elicit more engagement or may cause participants to direct their attention toward the emotional details of an intervention, thus causing immediate change in their perceptions of *support* and *effectiveness* for these laws. However, it is a critical next step to consider the longevity of such effects because without stable change in attitudes these laws will not be reformed or reconsidered. These two studies also begin to investigate the impact of learning and memory on potential attitudinal changes, and their interaction with the factual or narrative content of the interventions.

CCT Laws

Griffin and Miller (2008) created the term “crime control theater” in response to the development of America's Missing Broadcast Emergency Response (AMBER) alerts. These alerts began in the 1990s and were eventually adopted in all 50 states to notify the public of the kidnapping of children by strangers in the hopes that these

This article was published Online First January 13, 2022.

Daniel A. Krauss  <https://orcid.org/0000-0002-4144-800X>

Gabriel I. Cook  <https://orcid.org/0000-0002-1921-5082>

Sharda Umanath  <https://orcid.org/0000-0002-6699-4357>

Portions of this research were presented at the annual AP-LS conference in New Orleans, LA, 2020. Special thanks to Monica Miller for her comments on aspects of the research. Thanks also to William Ellsworth, Serena Faruqee, Maxwell Fisher, and Dhara Singh for help with this research project. The Data files for this project can be found at <https://osf.io/3dr2j/>.

Correspondence concerning this article should be addressed to Daniel A. Krauss, Department of Psychological Science, Claremont McKenna College, 850 Columbia Avenue, Claremont, CA 91711, United States. Email: daniel.krauss@claremontmckenna.edu

alerts would lead to increased conviction of perpetrators (Alvarez & Miller, 2016; Yelderman et al., 2018). Yet, these alerts, although widely supported by the populace, did not actually increase apprehension rates of strangers who have abducted children (e.g., Alvarez & Miller, 2016; Sicafuse & Miller, 2012). As a result, these laws were deemed “theater” because they failed to accomplish their goals and instead diverted resources from other possible interventions that could be effective. In further developing the CCT concept, scholars argued that three other elements beyond being an empirical failure are required: (a) the laws’ adoption resulting from “moral panic,” or fear of actions that is disproportionate to the actual likelihood of occurrence (Goode & Ben-Yehuda, 1994); (b) the laws receive unquestioned acceptance and promotion from the public (e.g., Hammond et al., 2010); and (c) the laws are based on mythic narratives that inaccurately portray the characteristics of the crime and perpetrators (e.g., the idea that most sexual offenses are committed against children by strangers (Socia & Harris, 2016)). Psycho-legal and criminal justice scholars have demonstrated that a small number of legal policies clearly fit these characteristics, such as, Safe Haven laws, which allow parents to relinquish their children at designated shelters to putatively protect the parents from criminal prosecution and advance the newborn’s health, and Three-Strikes laws, which punish repeat offenders with lengthy or lifetime prison sentences in an attempt to lower crime rates (DeVault et al., 2016).

Sex offender registration and notification (SORN) and sex offender housing restriction laws also represent examples of CCT policies (Campbell & Newheiser, 2019; Krauss et al., 2021; Socia & Harris, 2016). SORN laws are now mandated by federal law and have been established in some form in all 50 states. They require that sexual offenders who have committed certain specified crimes register themselves with authorities, notify the community where they live, and have their names noted on privately or publicly available listings (e.g., Levenson & Cotter, 2005; Zgoba & Mitchell, 2021). Sex offender housing restriction laws are laws adopted by counties and municipalities that prohibit offenders who have committed certain designated sexual crimes from living within certain distances of schools, recreational centers, parks, or other places children might congregate (e.g., Budd & Mancini, 2016). Roughly three-quarters of states have counties and municipalities that have promulgated these laws (Campbell & Newheiser, 2019). Both sets of policies resulted from moral panic over “stranger” sexual offenders perpetrating additional crimes against children after they have been released from incarceration. Most notably, in 1994, the widely publicized case of Megan Kanka, who was abducted from her home and killed by a released sex offender, Jesse Timmendequas, created moral panic about the absence of SORN laws in many jurisdictions. Her parents and the local populace were extremely distraught that a repeat sex offender resided down the street from her house, and that they had no awareness of his presence in their community. “Megan’s” law, which was a registry law promoted by Kanka’s parents, further spurred the adoption of SORN laws across the United States (Campbell & Newheiser, 2019; Socia & Harris, 2016; Zgoba & Mitchell, 2021). The subsequent federal Adam Walsh Child Protection Act (2006) attempted to standardize SORN laws across jurisdictions, and also attempted to create a national searchable database of sex offenders (Zgoba & Mitchell, 2021).

These laws were promulgated despite the fact that rates of stranger-perpetrated sexual violence against children had been

decreasing for years (Prescott, 2016; Prescott & Rockoff, 2011; Wakefield, 2006). Furthermore, both sets of sex offender policies invoke mythic narratives concerning sexual offenders, such as “stranger danger,” in which the most violent sexual offenses are committed against children by unknown assailants when empirical research and crime statistics clearly do not support this proposition (e.g., Budd & Mancini, 2016). Perhaps the most troubling aspect of these two sex offender laws, however, is that the public continues to vehemently favor them when more than 2 decades of research demonstrates they are ineffective in reducing sexual recidivism (Duwe et al., 2008; Zgoba et al., 2018; Zgoba & Mitchell, 2021). In fact, although some initial research indicated that SORN laws might have some small effects on decreasing sexual recidivism in some jurisdictions, a recent meta-analysis involving approximately 475,000 offenders found no evidence that these laws were effective, regardless of whether recidivism or sexual recidivism was examined through arrests or convictions (Zgoba & Mitchell, 2021). Moreover, the further social stigmatization of sexual offenders these policies engender have been suggested to increase rather than decrease reoffense rates, and also create several unintended consequences that are harmful to the very communities they are purported to protect (Hipp et al., 2010; Tewksbury et al., 2012).

Unfortunately, in a nationally representative participant sample, recent research highlighted that these two sex offender CCT policies evidenced extremely high public *support* ratings, and a substantial difference between the public’s ratings of *support* compared with their *effectiveness* than even other CCT policies (Krauss et al., 2021). In particular, the public’s outsized *support* was striking, whereby approximately 60% of the sample offered the highest rating of *support*, a “10” on a 0–10 sliding scale. Yet, the other finding, the substantial discrepancy between *support* and *effectiveness* ratings, points to the disconcerting possibility that even when the public has some awareness of the lack of *effectiveness* of these laws, they still *support* them. Further, it suggests that the public’s ratings of *support* and *effectiveness*, although correlated, may have different underlying motivations and processes for change (Krauss et al., 2021). This former contention is bolstered by another study examining these two sex offender CCT policies, across three studies and more than 750 participants, that found approximately 56% of their sample supported the statement “I would be in favor of these laws even without any evidence that they stop sex crime” (Campbell & Newheiser, 2019, p. 13).

In both these studies (Campbell & Newheiser, 2019; Krauss et al., 2021), favoritism toward the two sexual offender CCT laws was higher among self-identified women. This outcome is consistent with considerable literature findings that women favor laws targeted at sexual offenders (e.g., Krauss & Scuirich, 2014; see Schutte & Hosch, 1997 for a meta-analysis). The rationale for this difference is likely a combination of greater sexual victimization among women, self-identified women’s emotional reaction to sexual offenders, their greater caretaking responsibilities for children, and their desire to punish perpetrators of sexual crimes (Campbell & Newheiser, 2019; Krauss et al., 2021; Yelderman et al., 2018).

Changing Attitudes Toward CCT Laws

Given the widespread public support of these sex offender CCT policies, it is necessary to investigate methods to change these attitudes, so that more effective policies might be created or at least

contemplated. However, little research has directly addressed interventions designed to affect the public's viewpoint regarding *support* or *effectiveness* of these laws, and the little work that has been done has only been marginally successful (Campbell & Newheiser, 2019). This is not altogether surprising because correcting misconceptions and misinformation beliefs is extremely difficult (e.g., Cook et al., 2015; Ecker et al., 2011; Lewandowsky et al., 2012; Schwarz et al., 2016; for meta-analyses, see Chan et al., 2017; Walter & Murphy, 2018; Walter & Tukachinsky, 2020). However, one notable exception in the sex offender arena found that a 10-minute discussion-based intervention could at least lead a group of undergraduates to change their negative views toward sex offenders and increase their beliefs about the likelihood of successful treatment for sex offenders more broadly (Kleban & Jeglic, 2012).

Yet, only one empirical study has directly examined changing individuals' views toward sex offender CCT laws. Campbell and Newheiser (2019) provided participants with specific counterevidence to demonstrate that sex offender CCT laws do not accomplish their intended goals are ineffective: They do not reduce sex offender recidivism. Yet, this counterevidence only produced small effects in reducing the sample's support for these laws, even when the counterevidence was deemed highly credible by the participants and was directly targeted to counter false beliefs the participants held toward the laws (Campbell & Newheiser, 2019). This led these researchers to conclude "support for crime control theater policies persists despite explicit knowledge that they do not reduce crime, highlighting the need for alternative methods of dissuading people for their support for these ineffective laws" (p. 1). This research is intended to answer this call.

Attitudes toward CCT laws are not the only attitudes or conceptual understandings that have proven difficult to change. Across a number of topics, research bears out that changing someone's view is difficult and is affected by cognitive engagement (Jones et al., 2015; Vaughn & Johnson, 2018), emotion (Lerner et al., 2015), confidence in the misconception (Dole & Sinatra, 1998; Ecker et al., 2011) as well as people's worldviews (Cook et al., 2015; Lewandowsky et al., 2012), among other factors. To build an appropriate intervention strategy, we drew on the extensive literature on conceptual and attitudinal change regarding misconceptions and misinformation as well as the memory literature in cognitive psychology.

Retraction of a misconception (simply negating it or indicating it is false) alone does not seem to be particularly effective for reducing misinformation (e.g., Ecker et al., 2011), but direct refutation of misconceptions seems to have some degree of effectiveness (Guzzetti et al., 1993; Tippett, 2010). For example, studies on conceptual change (e.g., Hynd & Alvermann, 1986; Palmer, 2003; see Chi, 2008), education policy (Aguilar et al., 2019), and teaching of psychology (e.g., Vaughan, 1977; Kowalski & Taylor, 2009; Reddy & Lantz, 2010) demonstrate that explaining *why* a misconception is incorrect produces substantial benefits in student knowledge. Similarly, within the persuasion literature, one of the major tenets of Petty and Cacioppo's (1986) Elaboration Likelihood Model (ELM) is that enduring effects are a function of the degree of elaboration of the persuasive message. Message recipients who are both cognitively able and motivated to engage with information will evaluate and elaborate upon the contents of the message to compare it with preexisting knowledge. This elaboration

can increase the likelihood of the cognitive process responsible for changing belief systems. Given the public's strongly held beliefs regarding CCT laws as well as a social consensus to support them, individuals may not often be motivated to elaborate upon the details of factual information offered to correct their misunderstandings. This motivational neglect may be a reason why Campbell and Newheiser (2019) and others have found little movement in participants' support for these laws.

In addition, scholars and researchers have highlighted that *support* for CCT laws might originate from emotional-based decision-making. Emotions fundamentally affect and can strongly bias decision-making (for reviews, see Lerner et al., 2015; Loewenstein, 1996). So, one explanation for the public's persistence unquestioned acceptance of CCT laws despite evidence to the contrary may be driven by the emotions these laws evoke and stem from reliance on System 1 processing (Kahneman, 2011; Kahneman et al., 1982), or peripheral/heuristic processing models (Chaiken, 1987; Petty & Cacioppo, 1986). These processes are likely active when counterevidence is presented (Campbell & Newheiser, 2019; Sicafuse & Miller, 2012; Yelder et al., 2018). If the public is primed for intuitive or emotional processing of information related to sex offenders, they may simply ignore or discount more rational evidence. In fact, recent research suggests that attitudes based on emotion tend to be stronger, more stable over time, and that the strength of this relationship between emotionality and persistence of beliefs remains relatively unknown to individuals in their decision-making (Rocklage & Luttrell, 2021). As a result, more fact-based interventions, constructed to create attitudinal change based upon statistics or failures of the laws to achieve their avowed goals, may be misplaced or ineffective.

Building on these findings, we aimed to create a more successful intervention by using videos containing both emotional and narrative content to hopefully make the refutations more effective for changing people's attitudes. Multiple aspects of each CCT sex offender related law were broken down into several misconceptions. Each aspect was directly refuted by providing not only the correct information instead but also with elaborative information to provide more context (Sinatra & Broughton, 2011) and details (Ecker et al., 2011; Kendeou et al., 2013; Swire et al., 2017). The elaborative information that accompanied the retraction of the misconception targeted unintended harmful consequences of the two sex offender CCT laws. We made this choice for two reasons: (a) Campbell and Newheiser (2019) already demonstrated that counterevidence alone was insufficient to significantly reduce support for these CCT laws and (b) because "corrections that tell an alternative story that fills the coherence gap otherwise left by the retraction" (Lewandowsky et al., 2012, p. 116; see also, Ecker et al., 2011; Swire et al., 2017) seem to be one of the few effective ways to make corrections stick. Part of the reason that these CCT laws may be so difficult to change is likely attributable to an intuitive sense that the laws ought to work and that there is not a clear explanation as to why exactly they do not. Thus, providing concrete information about the harm they cause, albeit somewhat unintentionally, may help reduce the continued influence of the misconception. Indeed, Lewandowsky et al. (2012) argue that offering the motivation for the misconception (and perpetuating it) may create especially effective corrections.

Another potential reason why beliefs or attitudes toward CCT laws are difficult to change in research studies may be attributable

to participants' perceived external pressure to change their attitudes or behaviors. In such cases, there is often a "reaction against change" (Knowles & Linn, 2004, p. 4). Research on persuasion has highlighted that one successful way to defend oneself against persuasive arguments is to mentally construct counterarguments. Strong narrative involvement or engagement in the story, however, may inhibit reactance and construction of counterarguments because the engagement can reduce awareness of the persuasive attempt (Dal Cin et al., 2004; Knowles & Linn, 2004; Moyer-Gusé, 2008). Presenting information about CCT laws in narrative form may give way to attitudinal or behavioral effects in ways that factual or statistical information cannot.

The particular use of video presentations of the refutation material was also driven by the finding that high engagement with material increases the likelihood of conceptual change (Dole & Sinatra, 1998; Jones et al., 2015). Similarly, adding visual content and narratives promote truthiness or the perception of truth of content (see Brashier & Marsh, 2020, and Schwarz et al., 2016, for reviews).

Thus, based on available research, the public may be more persuaded to change their views of sex offender CCT laws if presented with a story concerning an individual sex offender and their experience of the unintended consequences of the laws than if presented with the same content in the form of factual details. In elaboration likelihood terms, the narrative presentations may serve as cues to motivate individuals to interact with and elaborate upon the content. This may be especially true if these troubling unintended consequences cause participants to further invoke, utilize, and weigh more heavily in emotion- or heuristic-driven process decision-making. Furthermore, self-identified women may be especially amenable to such narrative and unintended consequences interventions because they already have stronger negative emotional reactions to sex offenders (e.g., Levenson et al., 2007) and higher support and favoritism for sex offender CCT laws (Campbell & Newheiser, 2019; Krauss et al., 2021).

Learning and memory also have roles to play here. First, effectively learning the refutation and associated facts may be necessary albeit insufficient for then changing the related belief or attitude (Swire & Ecker, 2018). Perhaps more memorable refutation content could lead the public to be unable to ignore the evidence "staring them in the face" that these laws are ineffective and indeed, harmful rather than helpful. Learning and retention of the corrective content may be one means of accomplishing this effort, especially longer term. Although Campbell and Newheiser (2019) showed that their participants did know that the CCT laws were ineffective and chose to support them anyway, the present work also examines the contribution of memory in people's beliefs around these laws more systematically. It is also plausible that the effects and/or differential effects of the interventions could only emerge after delay because of different rates of forgetting. Indeed, prior work has found that narratives, as opposed to directly expository texts, can be more effective for learning corrective information, with the suggestion that narratives may be more memorable (Maria & Johnson, 1990). The persuasive impact of a narrative-video intervention may decay at a slower rate than that for non-narrative interventions (Appel & Richter, 2007). Differences in message engagement immediately may then influence delayed persuasive effects as the engagement may facilitate either stronger or more elaborate mental representations, which can be used when

making judgments in the absence of the intervention. Thus, perhaps our approach of using video presentations and providing anecdotal narrative content may be beneficial for retaining the material and therefore help change people's attitudes over a delay.

Present Research and Hypotheses

The present studies add to existing research concerning the public's perception of CCT laws and interventions that might successfully transform these attitudes in several important ways. First, it examines interventions specifically targeted to build upon direct refutation and provide elaborative feedback that highlights the harmful unintended consequences and failures of sex offender CCT laws. The examined approaches are designed to give participants the context and details to disengage from the appeal of the CCT laws and motivate them to fully process the counterevidence. As a result, we hypothesized that the presentations, both factual and narrative, will be successful in lowering participants' backing for sex offender CCT laws.

Second, the narrative-based anecdotal story interventions were created to further target the more heuristic/peripheral and emotional processing of counterevidence, in hopes that this would be a more effective strategy for lowering the public's unquestioned acceptance of CCT laws than a more factual presentation. For example, observing the ways in which sex offender housing restriction laws drive individuals to homelessness and prevent proper social reintegration may be more convincing than a factual presentation highlighting these same facts. It is further hypothesized that this narrative presentation would be even more effective with self-identified women, who are likely more resistant to decreasing their favoritism for sex offender CCT laws.

Third, the learning and memory processes associated with attitudinal change toward sexual offender CCT laws will be investigated. In Experiment 1, participants' learning of the intervention content about the harmful unintended consequences from the videos will be assessed. This will allow us to observe links between learning of the material and immediate changes in participants' perceptions of *support* and *effectiveness* of the CCT laws. To address the potential longevity of our interventions for changing beliefs about CCT laws or perhaps the emergence of differential effects over time, in Experiment 2, we examine not only participants' maintained retention of the intervention content 1 week later but also any additional changes in their perceptions of the CCT laws.

Experiment 1

Method

Participants

Participants were recruited via Qualtrics Panels, an online survey platform (Qualtrics Panels, 2020; see generally <http://www.qualtrics.com/panel-management> and <https://www.qualtrics.com/research-services/online-sample/> for more information about participant sampling) in the summer of 2019. We used proportionate sampling for age and race based upon U.S. census data from 2010. Qualtrics Panels provides participant samples based upon prespecified demographic considerations in a similar manner to market

research firms. Qualtrics received \$5.50 as compensation for each completed survey in this data collection. Qualtrics Panels has been demonstrated to provide the most representative participant sample compared with other sampling platforms in terms of demographic and political characteristics in empirical studies (Boas et al., 2020).

Two hundred eighty-four respondents completed the Experiment 1 survey. The survey included an attention check question to ensure participants were providing meaningful responses as well as a Captcha test to restrict machine-based responses (i.e., BOTs; Oppenheimer et al., 2009). Twenty-one participants were eliminated for young age (i.e., <18), 13 for failing to provide self-identified gender, race, age, or political affiliation, and 32 for failure to pass attention or manipulation check questions. The attention check was embedded in the survey, such that respondents were asked to move a slider rating to the midpoint for one response during the survey. We excluded an additional participant for failing to complete any ratings after rating the intervention video. This resulted in a final sample consisting of 217 respondents.¹ Demographic information for the Experiment 1 sample is provided in Table 1.

Procedure and Material

Overall, the study employed a 2 CCT Policy or Law (Sex Offender Housing Restriction Law, Sex Offender Registration Law) × 2 Video Presentation Style (Narrative Video, Factual Video) between-subjects factorial design. Participants first completed informed consent highlighting anonymity and confidentiality, potential discomfort from video content, and the need to pay attention to presented materials.

Respondents were asked to provide ratings for their support for and perceived effectiveness of 10 legal policies at two different times (i.e., 1. Baseline and 2. After video presentation). Five of these policies could be characterized as CCT, and five were legal

policies with substantial empirical support. Two CCT policies, sex offender registration laws and sex offender housing restriction laws, were the focus of this research.² Specifically, respondents were presented with a policy along with a brief description. The two CCT laws included the following descriptions:

Sex Offender Housing Restriction Laws — Laws that affect sex offenders’ ability to live within certain distances of playgrounds, schools, public parks, and school bus stops.

Sex Offender Registry Laws — Laws that require convicted sex offenders to register where they live and to restrict their ability to perform certain activities.

For each policy, respondents were asked the following:

How effective do you think this law is?
How much do you support this law?

Ratings were made on a 11-point sliding Likert scale with options ranging from 0 = *Not at all* to 5 = *Neutral* to 10 = *Extremely*. Higher scores indicated greater support and greater perceived effectiveness for each law. Policies were presented individually and in random order between participants. However, effectiveness ratings were always endorsed before support ratings for each law.

Participants were then presented with the stimulus—either a narrative, anecdotal video news-clip of an individual affected by either CCT policy or a factual video presentation with a male voice-over primarily addressing the misconceptions of the appropriate policy. Each video presented was approximately 5 minutes long and consisted of 12 misconceptions that were identical based upon the policy presented (i.e., sex offender registration or sex offender housing restriction law) across the factual and narrative conditions. For example, the narrative sex offender registration video reveals the struggles of a young mother who, at 19 years of age, was placed on the registry for engaging in sexual activity with a 14-year-old (child molestation was the charge). The corresponding factual registry video displayed identical misconceptions in a narrated PowerPoint presentation video. Both videos dispelled common misconceptions that only the most dangerous offenders have the most restrictions and highlighted that these laws use arbitrary criteria to determine an individual’s risk. The sex offender registry videos explain that politicians rather than scientists adopted these laws. It further explained that politicians do not want to be viewed as “soft on crime” and desire to be reelected. As a result, politicians support the harsh laws

Table 1
Demographic Information

Variable	Experiment 1 (N = 217)	Experiment 2 (N = 133)
Gender		
Male	76 (35%)	45 (34%)
Female	141 (65%)	88 (66%)
Race/Ethnicity		
African American	27 (12%)	15 (11%)
Asian	17 (7.8%)	10 (7.5%)
Hispanic	28 (13%)	7 (5.3%)
Native American	1 (0.5%)	1 (0.8%)
Pacific Islander	1 (0.5%)	0 (0%)
White	140 (65%)	95 (71%)
Other	3 (1.4%)	5 (3.8%)
Political affiliation		
Conservative	89 (41%)	40 (30%)
Centrist/Middle of the Road	65 (30%)	43 (32%)
Liberal	63 (29%)	50 (38%)
Age		
M	53.2	45.1
SD	15.3	14.2
Median	54	43

Note. Values for variables other than age represent counts and percentages from the sample.

¹ Part of this samples’ demographic data was previously presented in Krauss et al. (2021). This previous research also included 200 participants not discussed or included in the present research.

² The three other CCT policies were: AMBER alerts, Safe Haven laws, and Three-Strikes sentencing laws. Five other legal policies that were designated not crime control theater (NCCT) were: seat belt laws, speeding laws, voting restriction laws, income tax laws, and age restriction of alcohol consumption laws. Previous research reported on *only baseline* measures of effectiveness and support for the five CCT laws and five NCCT laws. This previous research also included 200 participants not discussed or included in the present research. Further, this previous research did not include any data concerning interventions and changes over time for any of the sample (Krauss et al., 2021).

against sex offenders, regardless of evidence that points to their ineffectiveness.

Similarly, the narrative sex offender housing restriction law video was taken from a news broadcast discussing sex offenders living under the Julia Tuttle Causeway in Florida. The subject in the video lived under the bridge without proper sanitation even after having served his prison sentence. It highlights that such legislation is oftentimes created by lobbyists, not scientists, whose goals are motivated by personal experiences. Consequently, the rights of sex offenders are often ignored. The factual housing restriction video presents the same misconceptions in a narrated PowerPoint presentation video.

The four videos can be viewed at the following links:

Factual sex offender registry: <https://vimeo.com/623749740>

Narrative sex offender registry: <https://vimeo.com/623733172>

Factual housing restriction: <https://vimeo.com/623752785>

Narrative housing restriction: <https://vimeo.com/623757979>

After viewing the video presentation, participants were asked to rate the following aspects of the video on a sliding scale from 0—*Not at all* to 10—*Very*:

How visually engaging was the video presentation?

How emotional did the video presentation make you feel?

How coherent was the narrative of the video presentation?

Participants then rated their support and perceived effectiveness of the 10 legal policies presented in a randomized order. They then completed a series of 15 multiple-choice learning and memory questions. Manipulation check questions were also embedded in this portion of the study. The following are the factual and narrative manipulation check questions in the sex offender registration conditions.

Factual manipulation check question

What was the picture on the first slide?

- A school playground
- A school bus
- A public park
- A childcare center

Narrative manipulation check question

Who was the main character of the story?

- Shawna, a mother convicted of child molestation (sexual relations with an underage individual)
- Bill, a father convicted of sexual assault (sexual relations with his friend's daughter)
- Tracey, a teacher in a school district convicted of marrying one of her students
- Jim, an employee at a corporation convicted of sexual harassment

Both the order in which the questions and answer choices were shown were randomized. The questions were then scored and summed into a scale based on how many questions the participants answered correctly (the learning and memory questions for each of

the videos can be found in the [Appendix](#)). The following is an example of one of the learning and memory multiple-choice questions from the sex offender registration condition:

Registration places which of the following restrictions on sex offenders?

- Ability to be travel to another country
- Ability to be removed from the registry
- Ability to use public transit
- Ability to freely use the internet

Following the multiple-choice questions, participants answered a set of optional demographic questions, which included age, self-identified gender, ethnicity, education, and political orientation.

Results

As our primary research goal, we examined the influence of the type of video intervention on *support* and *effectiveness* ratings for the two sex offender CCT laws. Whereas the video interventions presented to some participants targeted sex offender registration law, the other video addressed sex offender housing restriction laws. We focused our analyses on participants' ratings for the laws targeted by the intervention they received both before and immediately after the interventions. For all analyses, we used significance testing based on $p < .05$.

Intervention Engagement, Emotionality, and Narrative Coherence

To confirm the expected differences between factual and narrative video interventions, we analyzed participants' ratings of the video intervention on three dimensions: level of engagement (0—*Not at all* to 10—*Very*), emotionality (0—*Not at all* to 10—*Very*), and narrative coherence (0—*Not at all* to 10—*Very*). We investigated whether the intervention type influenced these ratings using a 2 (CCT Law: Housing Restriction vs. Registration) \times 2 (Intervention Type: Factual vs. Narrative Video) between-subjects ANOVA (see [Table 2](#)). Ratings of engagement, emotionality, and narrative coherence for the narrative video intervention all exceeded those for the factual video intervention, with the main effect being largest in strength for engagement, $F(1, 213) = 82.39, p < .001, \eta_p^2 = .28$, next largest for emotion, $F(1, 213) = 25.24, p < .001, \eta_p^2 = .11$, and smallest for coherence, $F(1, 213) = 15.36, p < .001, \eta_p^2 = .07$. Higher ratings for the narrative video prevailed across both the sex offender housing restriction and registration laws. Ratings did not differ based on the CCT law for engagement, $F(1, 213) = .002, p = .97$, for emotion, $F(1, 213) = 1.23, p = .27$, nor for narrative coherence, $F(1, 213) = 2.49, p = .12$. Type of law did not interact with the video intervention type, for engagement, $F(1, 213) = .23, p = .63$, for emotion, $F(1, 213) = .01, p = .92$, nor for narrative coherence, $F(1, 213) = 2.12, p = .15$. These results confirmed that our efforts to make the narrative video intervention more engaging, emotional, and narratively coherent than the factual video intervention were successful and that these metrics did not vary as a function of the law.

Table 2
Evaluation Ratings for Video Interventions

Experiment	CCT law	Intervention	Engagement	Emotionality	Narrative coherence	Memory	
						Immediate	Week-delay
Experiment 1	Housing	Narrative	8.07 (0.30)	6.85 (0.35)	8.61 (0.22)	75.08 (2.38)	
		Factual	4.65 (0.35)	5.02 (0.33)	7.16 (0.30)	75.16 (2.59)	
	Registration	Narrative	7.91 (0.29)	7.21 (0.32)	8.64 (0.21)	71.71 (1.78)	
		Factual	4.83 (0.48)	5.45 (0.37)	7.98 (0.34)	77.92 (2.86)	
Experiment 2	Housing	Narrative	7.35 (0.40)	6.15 (0.42)	8.03 (0.32)	68.05 (2.96)	67.05 (2.95)
		Factual	5.79 (0.53)	5.86 (0.48)	7.97 (0.41)	75.40 (3.21)	69.42 (3.08)
	Registration	Narrative	8.48 (0.29)	8.00 (0.34)	8.90 (0.25)	75.05 (2.10)	72.25 (2.17)
		Factual	4.67 (0.43)	5.08 (0.46)	7.97 (0.30)	85.63 (1.94)	78.45 (2.39)

Note. CCT = crime control theater. Values reported are means with standard errors are in parentheses.

Perceptions of Support and Effectiveness

Before and After Intervention. Using a mixed-model ANOVA, we investigated whether the interventions influenced ratings using a 2 (CCT Law: Housing Restriction vs. Registration) \times 2 (Intervention Type: Factual Video vs. Narrative Video) \times 2 (Time: Before Intervention vs. After Intervention) with time being a repeated measure (see Table 3, Figure 1, and Figure 2). Across all conditions, *support* ratings following the intervention ($M = 7.48$) declined relative to their starting point prior to the intervention ($M = 8.53$), $F(1, 213) = 33.45$, $p < .001$, $\eta_p^2 = .14$, which demonstrated that the interventions significantly influenced participants' *support* for the two sex offender CCT laws. *Support* ratings for the two CCT laws did not differ, $F(1, 213) = .54$, $p = .46$, nor did they change as a function of the intervention type, $F(1, 213) = 1.69$, $p = .19$. Similarly, there were no two-way interactions between intervention and law type, $F(1, 213) = 3.58$, $p = .06$, intervention and time, $F(1, 213) = .45$, $p = .50$, law type and time, $F(1, 213) = 1.75$, $p = .19$, nor was there a three-way interaction, $F(1, 213) = .04$, $p = .84$. Thus, the two types of intervention were equally successful across the CCT laws in terms of reducing participants' ratings of *support* for these CCT laws. These data are depicted in Figure 1.

We conducted the identical statistical analysis on *effectiveness* ratings and found that, unlike *support* ratings, *effectiveness* ratings did not change in the same manner as a function of the intervention (See Table 3). That is, *effectiveness* ratings overall appeared stable before ($M = 6.60$) and after ($M = 6.54$) participants experienced the intervention, $F(1, 213) = .19$, $p = .67$. Similar to *support* ratings, time of rating failed to interact with intervention type, $F(1, 213) = 1.01$, $p = .32$. Time, however, did interact with the CCT law, $F(1, 213) = 4.58$, $p = .03$, $\eta_p^2 = .02$, indicating that the video intervention may influence *effectiveness* ratings in the different ways for two CCT laws. To unpack that interaction, we compare ratings before and after the intervention for each CCT law separately. Whereas *effectiveness* ratings did not change after the intervention for the sex offender registration laws, $t(119) = .90$, $p = .37$, ratings for the housing restriction law were surprisingly higher after the intervention compared with before, $t(96) = 2.25$, $p = .03$ by about a third of a scale point. Such an effect runs counter to our expectations but suggests interventions may influence perceptions of *support* and *effectiveness* of laws in different ways. Similar to *support* ratings, *effectiveness* ratings for the two CCT laws did not differ, $F(1, 213) = 1.60$, $p = .21$, nor did they change as a function of the intervention type, $F(1, 213) = .05$, $p = .82$. There was no two-

way interaction between intervention and law type, $F(1, 213) < .001$, $p = .98$, nor three-way interaction, $F(1, 213) = .04$, $p = .84$ (see Figure 2).

Change Scores of Support and Effectiveness. Participant's initial ratings of *support* and *effectiveness* for these laws may also mathematically influence the amount of change that may occur as a function of the intervention. We observed a negative correlation between the initial ratings and the size of change following the intervention, indicating that the higher the initial rating, the greater the rating reduction following the intervention. We observed this association for both ratings of *support*, $r(217) = -.35$, $p < .001$, and *effectiveness*, $r(217) = -.52$, $p < .001$. Controlling for this association may reveal differences between narrative and factual video interventions; thus, we covaried out the initial rating when examining the change in ratings due to the intervention. We computed *support* and *effectiveness* change scores for each participant by subtracting the rating obtained before the intervention from the rating obtained after the intervention (e.g., After Intervention—Before Intervention). As such, these measures would be negative if the intervention *reduced* ratings (supporting our hypotheses), and they would be *positive* if the intervention increased ratings (against our hypotheses). Analyzing these change scores makes the time variable no longer relevant, so it was removed from this statistical model. Rather, for both *support* and *effectiveness* ratings, we conducted separate 2 (CCT Law: Housing vs. Registration) \times 2 (Intervention Type: Factual vs. Narrative Video) ANCOVAs on the change scores, holding constant initial ratings prior to the intervention. Across both CCT law and intervention type, the change score indicated that on average participants reduced their *support* ratings by about one unit on the scale ($M = -1.05$, $SE = .17$) after receiving the intervention materials but did not change their *effectiveness* ratings on average ($M = .06$, $SE = .15$).³ Importantly, even while holding constant participants' initial

³ The changes in ratings for the intervened laws could have occurred for reasons other than the intervention itself (e.g., demand characteristics). To examine whether the intervention affected a CCT law similar to the intervened laws, we examined change scores for AMBER Alerts, which could serve as a non-intervened control law. The AMBER Alert data reported in Table 3 highlight that there was no evidence of any change in the *support* or *effectiveness* suggesting that the interventions themselves were likely responsible for the change in the ratings. Separate 2 (CCT Law: Housing Restriction vs. Registration) \times 2 (Intervention Type: Factual Video vs. Narrative Video) \times 2 (Time: Before Intervention vs. After Intervention) mixed-model ANOVAs on both *support* and *effectiveness* ratings revealed no change in ratings, with all $ps > .15$.

Table 3
Ratings of Support and Perceived Effectiveness Before and After Intervention

Experiment	CCT Law	Intervention	Support				Effectiveness			
			Before	After Immed.	Change	After 1 week	Before	After Immed.	Change	After 1 week
Experiment 1	Housing restriction	Narrative	8.48 (0.40)	7.63 (0.41)	-0.85 (0.35)		6.07 (0.41)	6.59 (0.38)	0.52 (0.29)	
		Factual	8.22 (0.34)	7.53 (0.35)	-0.69 (0.28)		6.25 (0.35)	6.53 (0.34)	0.27 (0.20)	
	Registration	Narrative	8.38 (0.27)	7.01 (0.33)	-1.37 (0.32)		6.76 (0.31)	6.68 (0.26)	-0.08 (0.32)	
		Factual	9.21 (0.24)	8.14 (0.39)	-1.07 (0.35)		7.02 (0.36)	6.57 (0.36)	-0.45 (0.29)	
Experiment 2	Housing restriction	Control	8.83 (0.14)	8.87 (0.14)	0.04 (0.09)		7.68 (0.16)	7.61 (0.16)	-0.07 (0.10)	
		Narrative	8.26 (0.35)	7.71 (0.43)	-0.56 (0.36)	7.32 (0.48)	6.26 (0.44)	6.18 (0.44)	-0.09 (0.37)	6.18 (0.41)
	Registration	Factual	8.62 (0.45)	8.10 (0.47)	-0.52 (0.23)	7.90 (0.44)	6.83 (0.56)	6.52 (0.44)	-0.31 (0.29)	6.07 (0.48)
		Narrative	9.13 (0.27)	6.26 (0.54)	-2.87 (0.54)	7.41 (0.44)	6.97 (0.33)	5.84 (0.42)	-1.13 (0.43)	6.95 (0.41)
AMBER	Factual	9.26 (0.22)	7.51 (0.45)	-1.74 (0.39)	7.16 (0.34)	6.79 (0.44)	5.95 (0.49)	-0.85 (0.45)	6.66 (0.29)	
	Control	9.11 (0.14)	9.04 (0.15)	-0.07 (0.08)	8.62 (0.19)	7.89 (0.17)	7.82 (0.17)	-0.06 (0.08)	7.66 (0.19)	

Note. CCT = crime control theater. Values reported are means with standard errors are in parentheses.

ratings, there were still no differences in *support* ratings based on video intervention type, $F(1, 212) = 1.09, p = .30$, type of CCT law $F(1, 212) = .77, p = .38$, nor for *effectiveness* ratings for intervention type, $F(1, 212) = .63, p = .43$ and CCT law, $F(1, 212) = 1.66, p = .20$. Thus, our findings discussed above held with this analytic approach.

Learning of Intervention Content

We next examined whether the type of video intervention (e.g., Factual vs. Narrative) influenced participants' learning for the intervention message content. *Learning* here refers to participants' proportion of correct answers on the 15-question multiple choice test about the intervention content, which was assessed using a 2 (CCT Law: Housing Restriction vs. Registration) \times 2 (Intervention Type: Factual

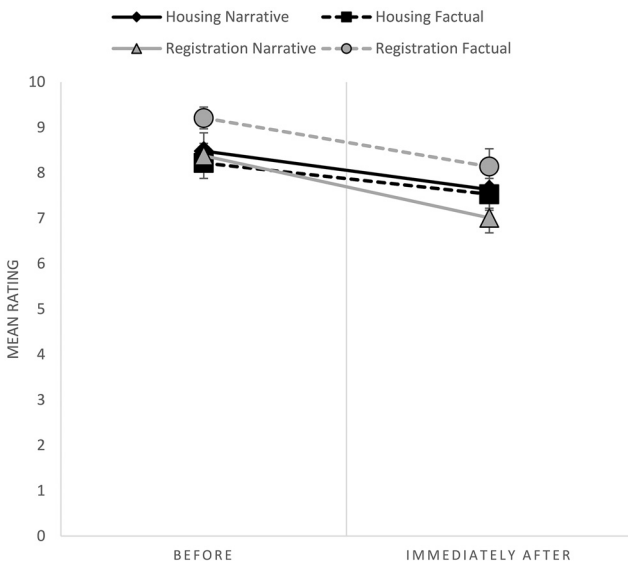
Video vs. Narrative Video) between-subjects ANOVA (see Table 2). Participants answered questions about the intervention equivalently for factual and narrative video interventions, $F(1, 213) = 1.75, p = .19$, and for sex offender housing restriction and sex offender registration laws $F(1, 213) = .02, p = .90$. The two variables also did not interact, $F(1, 213) = 1.65, p = .20$.⁴ Participants appeared to have learned equal amounts of information from the interventions with them answering approximately 75% of the learning questions correctly.

Self-Identified Gender Differences

Gender differences were hypothesized to be present initially on ratings of support and effectiveness, and the interventions were targeted to specifically change both ratings significantly more for self-identified women. We examined whether self-identified men and women differed in their initial *support* and *effectiveness* ratings prior to receiving the intervention video (see Table 4). We conducted separate 2 (Self-identified Gender: Man vs. Woman) \times 2 (CCT Law: Housing vs. Registration) between-subjects ANOVAs on both metrics. Intervention type was not included as a predictor in the model because the initial ratings of interest here preceded the intervention manipulation. Self-identified women expressed higher ratings than did men for both *support*, $F(1, 213) = 13.32, p < .001, \eta_p^2 = .06$ and *effectiveness*, $F(1, 213) = 9.11, p = .003, \eta_p^2 = .003$. No other analyses differed in the two models indicating that men and women do not support sex offender housing restriction laws differently from sex offender registration laws, largest $F(1, 213) = 3.66, p = .06$.⁵

We then examined whether self-identified men and women were affected similarly by the video interventions and the CCT laws. Because initial ratings differed prior to the intervention, we again examined the change score (e.g., After intervention—Before

Figure 1
Experiment 1 Ratings of Support Before and After Intervention as a Function of CCT Law and Intervention Type

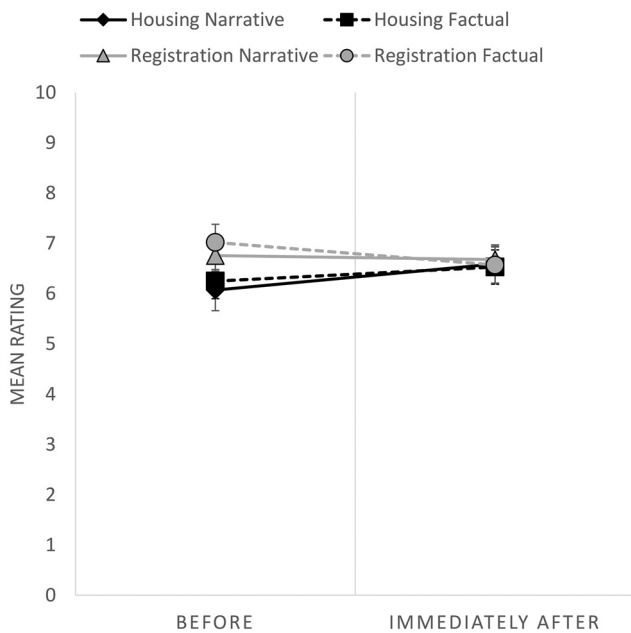


Note. CCT Law is represented by black lines (Housing) and gray lines (Registration); Intervention Type is represented by solid lines (Narrative) and dotted lines (Factual).

⁴ Because the distributions of memory scores were not normal, we also analyzed the data using Mann-Whitney *U* tests. They revealed no differences in accuracy for the two CCT laws but did reveal memory was better for the factual video.

⁵ Owing to a violation of homogeneity of variance, we also conducted Mann-Whitney *U* tests, which indicated that self-identified women's *support* and *effectiveness* ratings ranked higher than did men's for the initial ratings. *Effectiveness* ratings but not *support* ratings were also higher for the sex offender registration law compared with the housing restriction law. Consistent with the ANOVA models, change scores did not alter our results.

Figure 2
Experiment 1 Ratings of Effectiveness Before and After Intervention
as a Function of CCT Law and Intervention Type



Note. CCT Law is represented by black lines (Housing) and gray lines (Registration); Intervention Type is represented by solid lines (Narrative) and dotted lines (Factual).

intervention) by holding constant the initial rating, using a 2 (Self-identified Man vs. Woman) \times 2 (CCT Law: Housing vs. Registration) \times 2 (Intervention Type: Factual vs. Narrative Video) ANCOVAs on the change scores. Overall, self-identified men and women did not differ in amount of change from the intervention for *support*, $F(1, 208) = .75, p = .39$ or *effectiveness* ratings, $F(1, 207) = .41, p = .52$. Similarly, their ratings were equivalent for the two CCT laws for both *support*, $F(1, 208) = .27, p = .61$ and *effectiveness*, $F(1, 208) = .52, p = .47$. Participants' self-identified gender also did not interact with CCT law, $F(1, 208) = 2.49, p = .12$ nor video intervention type, $F(1, 208) = 2.24, p = .14$ for support ratings. Similarly, for effectiveness ratings, self-identified gender failed to interact with either CCT law, $F(1, 208) = .47, p = .49$ or intervention type, $F(1, 208) = 3.44, p = .07$. Importantly, using initial rating as a covariate did not alter the findings and were identical to the noncovaried results.

Discussion

Both the factual and narrative video interventions were successful in lowering participants' *support* for sex offender registration and housing restriction laws, respectively. In light of the failure of other kinds of interventions to produce significant changes in participants' favoritism for sex offender CCT laws and the public's outsized acceptance for these laws, this finding is especially noteworthy. *Support* for these laws can be reduced. Yet very few other expected effects were found. Participants' ratings of *effectiveness* of the two CCT laws did not change due to the type of intervention used. This finding reinforces the notion that *support* and

effectiveness, although related, may be influenced by different factors. In addition, the narrative video was no more effective for reducing participants' *support* or perception of *effectiveness* than was the fact-based video.

One possible explanation for the difference between the participants' views of *support* and *effectiveness* may be tied to *why* the public favors the laws. Laws often have multiple purposes, some of which may be expressive or symbolic (Sunstein, 1996). So, *effectiveness* for CCT laws may also be expressly tied to their punitive nature and their ability to restrict the freedom of the individuals they are targeting regardless of their ability to achieve their purported goal of decreasing recidivism. The experimental interventions did not attempt to convince participants that these laws "ineffectively" accomplished these former goals (i.e., punish sex offenders). Further, the interventions were targeted toward the unintended consequences of the laws rather than their failure even to decrease recidivism. As a result, *support* ratings may have decreased because the unintended consequence may have raised concerns that participants might not have contemplated. These more negative attitudes toward the laws might exist even though the participants still find the punitive nature of the laws *effective*.

It is critical to both understand why the interventions were overall successful in reducing *support* for these CCT laws and why they failed in some basic ways they were expected, and indeed built, to succeed. For example, we hypothesized that the narrative-based video would likely be more successful than the factual videos on several dimensions, including: (a) engagement, coherence, and emotionality; (b) lowering *support* and *effectiveness* ratings; and (c) better learning of the material presented. Although the narrative-based interventions were rated higher for engagement, coherence, and emotionality, the narrative videos (and these characteristics) did not appear to be the mechanism by which participants immediately changed their *support* toward the two CCT sex offender policies. The narrative videos also did not produce immediate differential effects on the participants' *support* and *effectiveness* ratings when compared with the factual videos, nor did they create initially different learning and memory effects for the video content. We reserve discussions of many of the possibilities and their accompanying avenues for future research for the General Discussion.

The major possibility we address here is consideration of whether certain effects could emerge over time. A limitation of Experiment 1 is that all assessment was conducted immediately after participants encountered the intervention. Indeed, Campbell and Newheiser (2019) also only examined immediate change in their sample. It is plausible that the influence of many of the characteristics of our interventions that were targeted to change people's perceptions would only occur after some time had passed. Indeed, regardless of the exact mechanism of attitudinal change, it is promising that the video interventions were so successful in changing participants' *support* ratings. It is an open empirical question, however, if these changes in attitudes toward CCT laws are merely transitory or more long-lasting. If they are only immediate, the interventions bode poorly as a means to create more long-term impetus for change and reform in these unsuccessful laws. Understanding whether the observed influence persists over a longer duration would also be important for understanding the mechanisms or processes (e.g., System 1—emotional/heuristic processing of information vs. System 2—effortful and rational

Table 4
Ratings of Support and Perceived Effectiveness Before and After Intervention by Gender

CCT law	Intervention	Sig	Support			Effectiveness		
			Before	Immed. Delay	Change	Before	Immed. Delay	Change
Housing	Narrative	Men	8.00 (0.71)	6.68 (0.76)	-1.32 (0.50)	5.37 (0.69)	5.79 (0.70)	0.42 (0.41)
		Women	8.81 (0.46)	8.30 (0.41)	-0.52 (0.47)	6.56 (0.5)	7.15 (0.40)	0.59 (0.41)
Registration	Factual	Men	6.67 (0.72)	6.27 (0.55)	-0.40 (0.65)	5.47 (0.63)	5.60 (0.67)	0.13 (0.35)
		Women	8.86 (0.32)	8.06 (0.42)	-0.81 (0.30)	6.58 (0.41)	6.92 (0.38)	0.33 (0.25)
	Narrative	Men	7.36 (0.62)	6.04 (0.66)	-1.32 (0.72)	5.44 (0.70)	5.92 (0.49)	0.48 (0.64)
		Women	8.87 (0.23)	7.47 (0.36)	-1.40 (0.34)	7.38 (0.28)	7.04 (0.30)	-0.34 (0.36)
	Factual	Men	9.12 (0.35)	8.65 (0.53)	-0.47 (0.55)	7.24 (0.41)	7.12 (0.53)	-0.12 (0.47)
		Women	9.28 (0.32)	7.80 (0.55)	-1.48 (0.44)	6.88 (0.55)	6.20 (0.48)	-0.68 (0.38)

Note. CCT = crime control theater; SIG = self-identified gender. Values reported are means with standard errors in parentheses.

processing of information) involved in changing the public's viewpoint. Furthermore, it is unclear whether some of the aspects of the interventions that were ineffective immediately may become more effective over time. Ultimately, our goal is to create lasting change in participants' *support* and perceptions of *effectiveness* for these CCT laws so that policymakers are forced to find empirically effective alternatives. Thus, examining the effects of the interventions a week later may be more critical than these instant effects (or lack thereof).

Experiment 2

Although the video manipulations, regardless of type, influenced participants' ratings of *support* for the sex offender CCT laws, one important issue is how long-lasting such changes in perception might be. An intervention that addresses unintended harmful consequences of sex offender CCT laws may produce promising immediate results, but ideally, these changes need to be durable over time. Experiment 2 addressed this issue by reexamining participants' perceptions and their learning of the content of the interventions as well as their memory of that content 1 week later.

For maintaining a changed related belief or attitude, it could be the case that remembering the refutation and associated facts is needed (Swire & Ecker, 2018). Although memory may not be immediately important as Campbell and Newheiser (2019) demonstrated with participants supporting CCT laws despite remembering that they are ineffective, it may contribute to longer-term changes in perceptions.

Although Experiment 1 failed to find many significant differences between factual and narrative video interventions showing no narrative advantage in decreasing *support* or *effectiveness* ratings, there is reason to believe narratives may exact larger persuasive effects over a time delay. A recent meta-analysis by Oschatz and Marker (2020) focusing on long-term effects (e.g., exceeding 1 week) found that narrative messages are associated with greater persuasive impact than non-narrative messages. It very well could be the case that the impact of the heightened engagement, emotion, and narrative coherence of the narrative intervention over the factual one only appears over time when forgetting of the intervention content has occurred to some degree. Because narratives can elicit more sympathy or empathy toward the character (Appel et al., 2015; Busselle & Bilandzic, 2009; Cohen, 2001; Green & Brock, 2000), this emotional interaction with the narrative video may result in more durable memories for information that may be accessed and interacted with after initial exposure in a way

different from fact-based information. These emotional associations to the intervention may fade at a slower rate compared with non-narrative associations. If the associations fade more slowly for narrative messages, participants may be able to reflect upon the message details to a greater degree after the intervention, which may result in greater elaboration and memory changes to influence decisions after a week delay or may motivate participants to reflect upon one's own beliefs or attitudes toward the CCT laws.

As such, Experiment 2 was conducted in two phases. The first phase was identical to Experiment 1. The key change for this study comes with the addition of Phase 2. One week after the completion of Phase 1, participants completed the same learning and memory assessment of the intervention videos and related their experience of the videos as well as related their *support* and perceptions of the *effectiveness* of CCT laws. This delayed assessment allowed for understanding how "sticky" changes in perceptions around CCT law may be and further examination of the role that memory for the content of the interventions might play in influencing those perceptions.

Method

Participants

Like Experiment 1, participants were recruited via Qualtrics Panels, an online survey platform (Qualtrics Panels, 2020) in the summer of 2019. Qualtrics received \$5.50 as compensation for each completed survey in Phase 1 of data collection, and \$6.00 additional compensation for each completed survey in Phase 2 of data collection a week later.

Two hundred fifteen participants completed both phases of the study. From this sample, 63 participants were removed for failure to pass a manipulation check question either initially or after a week delay, seven for failure to pass an attention check question, and 11 for failing to provide self-identified gender, race, age, or political affiliation. The manipulation check question was specific to a video initially presented to the participants during Phase 1. We also removed one participant who took excessive time to complete the study. This resulted in a final sample consisting of 133 respondents. Demographic information for the sample is provided in Table 1.

Procedure and Material

Overall, the study again employed a 2 Sex Offender CCT Policy or Law (Sex Offender Housing Restriction vs. Sex Offender

Registration Law) \times 2 Video Intervention type (Narrative Video vs. Factual Video) between-subjects factorial design. The materials were identical to those used in Experiment 1.⁶

Respondents were asked to provide ratings for their support for and perceived effectiveness of 10 legal policies at three different time points: 1. Baseline; 2. After video presentation (Phase 1 in Experiment 2); and 3. After a week delay (Phase 2 in Experiment 2). The procedure for Phase 1 was identical to that used in Experiment 1.

One week after completing Phase 1, participants were recontacted by Qualtrics Panels via email to complete the second part of the study. They were asked the same manipulation check question corresponding to the video they initially viewed a week before. They were then asked to re-rate their views of the 10 legal policies, re-rate how engaging, emotional, and coherent their initial video presentation was, and re-answer the 15 multiple-choice questions related to their initial video. Participants finally completed identical demographic questions and had the option to include any comments, questions, or concerns regarding the study. Once data collection had been completed, participants were provided a debriefing statement regarding the research and nature of their participation.

Results

Intervention Engagement, Emotionality, and Narrative Coherence

We analyzed level of engagement, emotionality, and narrative coherence associated with the video interventions using three 2 (CCT Law: Housing Restriction vs. Registration) \times 2 (Intervention Type: Factual Video vs. Narrative Video) between-subjects ANOVAs (see Table 2). For level of engagement, ratings for the narrative video interventions exceeded those for the factual video interventions, $F(1, 129) = 40.03, p < .001, \eta_p^2 = .24$, and was equivalent for the two CCT laws, $F(1, 129) < .01, p = .99$. However, these main effects are qualified by a CCT Law and Intervention type interaction, $F(1, 129) = 7.06, p < .01, \eta_p^2 = .05$. Examining the interaction more closely reveals that for the narrative video intervention, engagement was higher for the sex offender registration law, $t(63) = 2.23, p = .03$ compared with the sex offender housing restriction law, and this order reversed numerically but did not differ statistically for the factual video intervention, $t(66) = 1.67, p = .10$. Ratings of emotionality followed the same general pattern as engagement ratings. Ratings for the narrative intervention video exceeded those for the factual intervention video, $F(1, 129) = 13.65, p < .001, \eta_p^2 = .10$, and the two CCT laws did not differ overall in emotionality ratings. Importantly, again, these main effects were also qualified by a CCT law by intervention type interaction. For the narrative video intervention, emotionality was higher for the sex offender registration law than for the housing restriction law, $t(63) = 3.41, p < .001$ whereas for the factual video, intervention emotionality was equivalent for the two laws, $t(66) = 1.17, p = .25$. Finally, participants' ratings of narrative coherence did not differ as a function of intervention type, $F(1, 129) = 2.35, p = .13$, CCT law, $F(1, 129) = 1.86, p = .18$, nor did the predictors interact, $F(1, 129) = 1.79, p = .18$. Note that these findings are somewhat different from

Experiment 1 in which all three ratings were significantly higher for the narrative video versus the factual one, consistent across the laws. Here, although the narrative interventions were still higher across most these measures, the strength of the effect appears to be primarily originating from the narrative, sex offender registration law video.

Perceptions of Support and Effectiveness

Before and After Intervention. Using a similar model to that used in Experiment 1, we examined the influence of interventions on ratings using a 2 (CCT Law: Housing vs. Registration) \times 2 (Intervention Type: Factual Video vs. Narrative Video) \times 3 (Time: Before Intervention vs. Immediately After Intervention vs. One-Week After Intervention) with the time variable being a repeated measure (see Table 3 and Figure 3). *Support* ratings reduced as a function of Time, $F(2, 258) = 35.41, p < .001, \eta_p^2 = .22$, demonstrating again that interventions can influence participants' support for housing restriction and registration laws. Importantly, the initial reduction in *support* ratings observed following the intervention persisted over a week delay. *Support* ratings for the sex offender housing restriction law did not differ from ratings for the sex offender registration law, $F(1, 129) = .48, p = .49$, nor did they change as a function of the factual video or narrative video intervention, $F(1, 129) = 2.26, p = .14$. Time did, however, interact with CCT law, $F(2, 258) = 10.60, p < .001, \eta_p^2 = .08$. Examining this interaction, *support* ratings for the sex offender registration law started higher than for the sex offender housing restriction law, $t(131) = 2.42, p = .02$ but immediately after the intervention they started to converge, $t(131) = 1.97, p = .05$ and 1 week after the intervention, ratings for both laws were equivalent, $t(131) = .91, p = .36$. For the housing restriction law, paired-samples *t*-tests revealed that *support* before the intervention differed from those both immediately and one-week after intervention, smallest $t(62) = 2.44, p = .02$, but they did not differ between the immediate and week-delay measures, $t(62) = 1.30, p = .20$, indicating maintenance of the initial reduction of *support* across the week delay. The paired-samples *t* tests revealed the same effect for the sex offender registration law; *support* ratings before the intervention differed from those both immediately and one-week after intervention, smallest $t(69) = 6.81, p < .001$ but they did not differ between the immediate and delayed measures, $t(69) = .79, p = .43$.

We used the same statistical model on *effectiveness* ratings (see Table 3 and Figure 4). Although the effect size was much smaller, *effectiveness* ratings reduced as a function of Time, $F(2, 258) = 3.85, p = .03, \eta_p^2 = .03$, demonstrating that interventions can influence participants' perceptions of *effectiveness* for sex offender housing restriction and registration laws collectively. Perceived *effectiveness* ratings did not differ for the sex offender housing restriction law compared with the registration law, $F(1, 129) = .12, p = .74$, nor did ratings change as a function of the type of factual or narrative video intervention, $F(1, 129) = .39, p = .53$. We observed no two-way interactions between intervention and law type, $F(1, 129) = .008, p = .93$, intervention and time, $F(1, 129) =$

⁶ Given our inability to find significant differences in self-identified gender, our largely self-identified female sample, and our smaller sample size for Experiment 2, we did not replicate the nonsignificant gender analyses from Experiment 1.

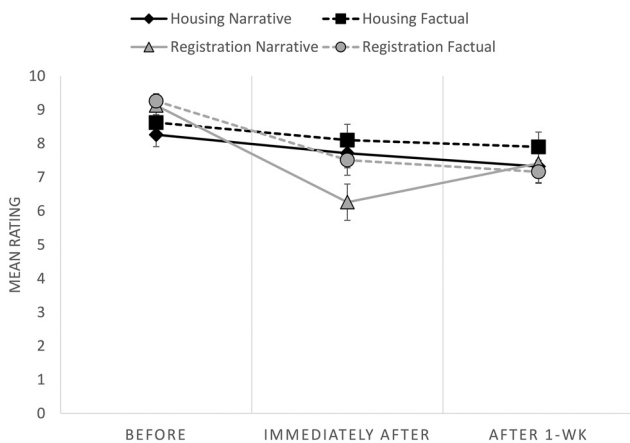
.02, $p = .98$, or law type and time, $F(1, 129) = 2.80$, $p = .07$. Similarly, the three-way interaction between all predictors was also not statistically significant, $F(1, 258) = 1.58$, $p = .21$.

Change Scores of Support and Effectiveness. We examined change in *support* and *effectiveness* ratings for Experiment 2 to align with the analyses reported in Experiment 1 (see Table 3). Across both CCT laws and video interventions, the change score indicated that on average, participants reduced their *support* ratings by over one unit on the scale ($M = -1.40$, $SE = .21$) after receiving the intervention materials, and they reduced *effectiveness* ratings on average by about half a scale point ($M = -.60$, $SE = .20$). The change scores for both *support* $t(132) = 6.69$, $p < .001$ and *effectiveness*, $t(132) = 2.99$, $p = .003$ differed statistically from the hypothetical change of zero as indicated by one-sample t test.

As with Experiment 1, we computed change scores to represent any shift in ratings from before and after the intervention. For both *support* and *effectiveness* ratings, we conducted 2 (CCT Law: Housing vs. Registration) \times 2 (Intervention Type: Factual Video vs. Narrative Video) ANCOVAs on the change scores, holding constant initial ratings prior to the intervention. The outcomes of the analyses differed for *support* and *effectiveness*, so we report them separately. As with Experiment 1, *support* for the factual and narrative video interventions was equivalent, $F(1, 128) = 2.51$, $p = .12$ whereas participants who viewed the sex offender registry video reduced their *support* to a greater degree than did participants who viewed the sex offender housing restriction video $F(1, 128) = 15.57$, $p < .001$, $\eta_p^2 = .11$. Although the intervention decreased *support* most for participants who viewed the narrative sex offender registration intervention, intervention type did not interact with the CCT law, $F(1, 128) = 1.68$, $p = .20$.

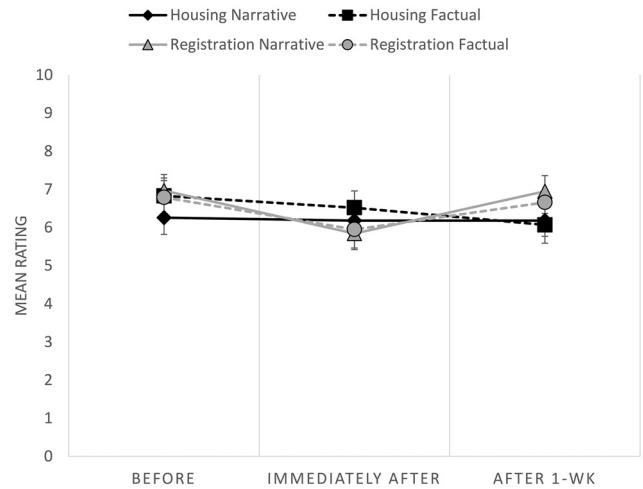
Turning to *effectiveness* ratings, change was equivalent for intervention type, $F(1, 128) = .083$, $p = .77$ as well as for CCT laws, $F(1, 128) = 3.24$, $p = .07$ and intervention type did not interact with the CCT law, $F(1, 128) = .09$, $p = .76$. The ANOVA models without the covariate did not change the interpretation but the

Figure 3
Experiment 2 Ratings of Support Before and After Intervention as a Function of CCT Law and Intervention Type



Note. CCT Law is represented by black lines (Housing) and gray lines (Registration); Intervention Type is represented by solid lines (Narrative) and dotted lines (Factual).

Figure 4
Experiment 2 Ratings of Effectiveness Before and After Intervention as a Function of CCT Law and Intervention Type



Note. CCT Law is represented by black lines (Housing) and gray lines (Registration); Intervention Type is represented by solid lines (Narrative) and dotted lines (Factual).

p -value was now smaller for the main effect of CCT law, $F(1, 128) = 3.82$, $p = .053$, $\eta_p^2 = .03$. Taken together, these findings suggest that *support* ratings continue to be affected by the interventions but unlike Experiment 1 there was a slight advantage for the sex offender registry video interventions (both factual and narrative) as compared with the sex offender housing restriction video for *support* ratings. Also, unlike Experiment 1 there was significant change in *effectiveness* ratings across all conditions (although much smaller than *support* rating), which was somewhat mitigated when the initial ratings were added as a covariate to analyses.⁷

Learning and Memory for Intervention Content

As in Experiment 1, we examined learning and memory of the message content immediately postintervention using a 2 (CCT Law: Housing Restriction vs. Registration Law) \times 2 (Intervention Type: Factual Video vs. Narrative Video) between-subjects ANOVA (see Table 2). For intervention type, participants answered more questions correctly following the factual video compared with the narrative video intervention, $F(1, 129) = 12.23$, $p = .001$, $\eta_p^2 = .09$, and for CCT laws, participants who viewed the sex offender registration video answered significantly more questions correctly than did participants who viewed the sex offender

⁷ As with Experiment 1, the changes in ratings for the intervened laws could have occurred for reasons other than the intervention itself (e.g., demand characteristics). To examine whether the intervention affected a CCT law similar to the intervened laws, we examined change scores for AMBER Alerts, which could serve as a non-intervened control law. The AMBER Alert data reported in Table 3 highlight there was no evidence of any change in the *support* or *effectiveness*, suggesting that the interventions themselves were likely responsible for the change in the ratings. The separate 2 (CCT Law: Housing Restriction vs. Registration) \times 2 (Intervention Type: Factual Video vs. Narrative Video) \times 2 (Time: Before Intervention vs. After Intervention) mixed-model ANOVAs on both *support* and *effectiveness* ratings revealed no change in ratings, with all $ps > .15$.

housing restriction video $F(1, 129) = 11.31, p = .001, \eta_p^2 = .08$. Intervention type and CCT law, however, did not interact, $F(1, 129) = .40, p = .53$. This pattern of data was different from Experiment 1 in which learning performance was equivalent across the intervention types and the CCT laws.

To examine whether these initial differences in memory performance were fleeting or durable across a week delay, we conducted a 2 (Time: Immediate vs. Week Delay) \times 2 (CCT Law: Housing Restriction vs. Registration) \times 2 (Intervention Type: Factual Video vs. Narrative Video) mixed-model ANOVA with time as a repeated measure. Overall, as expected, participants answered fewer details correctly after a week, $F(1, 129) = 22.02, p < .001, \eta_p^2 = .15$. Memory performance also depended on both the CCT law and intervention type. Consistent with the findings above regarding just the immediate test, participants answered significantly more questions correctly for the sex offender registration video compared with the housing restriction video, $F(1, 129) = 10.24, p = .002, \eta_p^2 = .07$, and opposite of prediction, they answered significantly more questions correctly for the factual videos compared with the narrative video interventions, $F(1, 129) = 7.26, p = .008, \eta_p^2 = .05$. Memory, however, declined over time at significantly different rates depending on the intervention type, as evidenced by the small effect interaction, $F(1, 129) = 6.71, p = .01, \eta_p^2 = .05$. Although participants answered numerically more questions accurately for the factual intervention than for the narrative intervention both immediately and after a week, the statistical benefit present immediately, $t(131) = 3.74, p < .001$, disappeared after the week delay, $t(131) = 1.87, p = .06$. As such, memory across the intervention types equated over time because as we predicted, the memory decline was smaller for the narrative intervention, suggesting that memory for the narrative video did not fade like that for the factual video and instead was relatively stable over time. The two-way interactions between time and law type, $F(1, 129) = .69, p = .41$, and intervention and law type, $F(1, 129) = .51, p = .48$, as well as the three-way interaction between all predictors, $F(1, 129) = .03, p = .87$ were not significant.

Discussion

Experiment 2 was undertaken for several key considerations: (a) to determine whether participants' changes in attitudes maintained after a significant delay; (b) to determine whether the narrative video intervention had differential effects from the factual video intervention over a significant delay in participant ratings of *support*, *effectiveness*, and memory; and (c) to determine whether the main effects from Experiment 1 were replicable on a new sample. Most importantly, the designed video interventions, both narrative and factual, produced "sticky" effects in participant attitudinal change (see Figure 2). After a substantial time delay, individuals continued to have lowered *support* and *effectiveness* ratings of the two sex offender CCT laws. Although the participants' change in *effectiveness* ratings was smaller than for that of *support*, it still suggests that the video interventions were successful in accomplishing the primary objective. Additionally, whereas memory for the video content significantly decreased after the substantial delay, it was still high (approximately 70% of questions were answered correctly at delay). All this suggests that the interventions were successful in creating long-term change in individuals' attitudes toward the sex offender CCT laws and indicates that such

interventions are promising. In particular, such approaches appear well-designed to decrease individuals' *support* for these ill-conceived laws, which could lead to calls for policy reform.

The narrative video interventions did not evidence a long-term advantage nor more positive effects than the factual videos. It appears that the delay did not lead to further elaboration of the material contained in the narrative videos, or if it did, this did not result in significant changes in participants' *support* or *effectiveness* opinions or memory for content. The reason why this is the case will be explored more in the General Discussion.

By and large, the results from Experiment 2 replicated those from Experiment 1 on a different participant sample: Participant *support* ratings decreased regardless of video condition, and the narrative video condition was generally viewed as more engaging, emotional, and coherent than the factual video condition. Yet, these differences in participant views toward the videos did not translate into significant differences in attitudinal change or memory. One primary difference between the key findings in Experiments 1 and 2 was the fact that in Experiment 2, *effectiveness* ratings in addition to *support* were significantly affected by the video interventions, albeit to a lesser degree. Additionally, immediate learning was slightly higher following the factual videos than narrative ones. Last, one analysis found that the narrative sexual offender registry video provided stronger participant ratings of engagement and emotionality than did the other videos. The importance of these divergent findings will be discussed more in the General Discussion.

General Discussion

The extant literature on correcting misconceptions demonstrates that it is extremely challenging and that many methods are often unsuccessful (e.g., Chan et al., 2017; Walter & Murphy, 2018; Walter & Tukachinsky, 2020). Perceptions of CCT laws seem to be no exception to this general set of findings and may even be more difficult to change because of the strong emotion associated with them (DeVault et al., 2016). The single previous empirical study specifically addressing changing individuals' views toward sex offender CCT laws found that even providing participants with specifically targeted, credible counterevidence to demonstrate that sex offender CCT laws do not accomplish their intended goals are largely ineffective—only resulting in small changes to participant attitudes toward these laws (Campbell & Newheiser, 2019). The experimental interventions examined were not only designed to refute the idea that these laws are effective but also included elaborative information that highlighted unintended negative consequences of the two sex offender CCT policies. Furthermore, the interventions were designed to be visually appealing with the understanding that visual content can promote truthiness or the perception of truth of content (see Brashier & Marsh, 2020, and Schwarz et al., 2016, for reviews). Importantly, our approach was effective in reducing participants' *support* for the sex offender CCT laws across two studies.

Particularly exciting is that this reduction in *support* was maintained over a week delay, suggesting that the interventions used could have longer lasting effects on participants' perceptions of the CCT laws. Indeed, it is the longevity of attitudinal change that needs to occur if these CCT laws are going to be reconsidered or discontinued. Effective policy change requires that the public's

attitudes endure and result in behavioral change, whether that is advocating for policy reform, calling representatives for change, or voting in the next election when a related issue is on the ballot. Here, we demonstrated that providing information about the harmful unintended consequences of sex offender CCT laws via video presentations, regardless of whether they are factual or narrative in form, can have more lasting effects on people's favoritism for these problematic laws. These effects were particularly strong for *support* ratings and relatively weak (Experiment 2) or nonexistent (Experiment 1) for the participants' *effectiveness* ratings. This finding elevates not only the importance of research measuring both *support* and *effectiveness* pertaining to laws in general and CCT laws in particular, but also the importance of studying the distinguishing characteristics that differentiates individuals' views toward these laws.

The experimental manipulations failed, however, to significantly lower *effectiveness* ratings immediately (Experiment 1) or only weakly did so (Experiment 2). This was not hypothesized to be the case but is not altogether surprising. Compared with other intervention studies (e.g., Campbell & Newheiser, 2019), our intervention highlighted the unintended consequences associated with individual cases related to the two CCT laws. Our approach may have focused participants' thinking on the unintended consequences more directly related to participants' *support* beliefs than their *effectiveness* ones. In other words, the intervention content was less focused on the empirical failure of these laws and more on other harms they caused, leading participants to change their beliefs related to overall *support* rather than *effectiveness*. Perhaps highlighting the unintended consequences of these laws and putting them in attentional focus allowed participants to evaluate information that they otherwise would or could not retrieve from memory or recent experience, which they used to inform their ratings. If so, highlighting those details primarily changed *support* but not *effectiveness* ratings. For example, the sex offender housing restriction law video highlighted the fact that the law caused difficulties for sex offenders to find housing, jobs, and further ostracized this group, but a participant could still reasonably believe that the law was effective in lowering sex offender recidivism. This focus on unintended consequences may also explain the slight rise in participants' ratings of the *effectiveness* of the sex offender housing restriction law following the video intervention in Experiment 1.

Alternatively, the lack of a decrease in our participants' *effectiveness* ratings could result from their belief that these laws are or should be intended to punish sexual offenders despite their putative purpose to decrease recidivism (e.g., Levenson et al., 2007). As such, these CCT laws are still *effective* in accomplishing this goal by restricting housing or limiting job opportunities, or even more narrowly by simply notifying the populace of their crime or prohibiting them from living certain distances from designated public places. In contrast, the participants' new knowledge of the unintended consequences of these laws may have made them more aware of other aspects of these laws that seemed disproportionately unfair. Consequently, this information led to a decrease in their *support* for such policies while still allowing them to recognize they are *effective* punishment or restrictive measures.

Prior (Krauss et al., 2021) and current research demonstrates that participants' *support* ratings for these two sex offenders CCT policies is significantly higher at the outset than their *effectiveness*

ratings. Nevertheless, both sets of ratings were high (i.e., *Support* $M = 8.64$; *Effectiveness* $M = 6.61$) across our two experiments, and analyses controlling for participants' preintervention ratings of *support* and *effectiveness* still found significant lowering of *support* while *effectiveness* did not change, at least in Experiment 1, suggesting that this finding was not merely a result of higher initial *support* ratings. It remains possible that *effectiveness* is more difficult to change in general, or again, that our particular intervention did not successfully target it.

More broadly, the present work provides evidence that *support* and *effectiveness* are related, but not perfectly so. Exploring the relationship between measures in the present data revealed a similar correlation for the CCT laws in Experiment 1, $r(217) = .52, p < .05$ and for Experiment 2, $r(133) = .47, p < .05$. This relationship was also stable after the intervention in Experiment 1, $r(217) = .63, p < .05$, and Experiment 2, $r(217) = .69, p < .05$. With between 20% and 30% of the variance in ratings of CCT laws shared prior to interventions, manipulations targeted to influence *support* may also influence *effectiveness* ratings to the extent they influence the same decision process. Yet, because of the variations in the participants' ratings (indicated by these moderate but not high correlations prior to intervention), change in these ratings may also be accounted for by different cognitive processes. This suggests that interventions designed to change *support* may likely differ from interventions designed to change *effectiveness*.

Research suggesting that *support* and *effectiveness* ratings are quite similar may unintentionally homogenize them, thereby reducing research aimed to understand them more clearly. Our video intervention may have differentially affected the judgment or decision processes that contribute to *support* and *effectiveness* ratings. By whatever cognitive process ratings changed, importantly, *support* and *effectiveness* did not move in lockstep, presumably because the underlying judgments themselves (*support* vs. *effectiveness*) may be informed by different types of information, experiences, emotion, or persuasive structure. Attempting to change the public's view toward these CCT laws and create an impetus for legislators to amend them may extend also to careful investigation of the measures used to assess the effectiveness of those intervention attempts.

Additionally, it is important to recognize that the specific targeting of *support* and changing the public's view for these laws may be more important than assessing and modifying individuals' attitudes about *effectiveness*. Remember that Newheiser and Campbell's participants evidenced a strong desire to support sex offender CCT laws regardless of their beliefs about their *effectiveness*, and that past (Krauss et al., 2021) and this research found outsized *support* as compared with *effectiveness* for the two sex offender CCT laws. As a result, if necessary attempts to reform or change these ineffective laws are to occur, changing the public's *support* of the laws may be a more fruitful avenue of inquiry for future research than simply focusing on changing their perceived *effectiveness*.

Although the interventions were able to reduce *support* for the CCT laws both immediately and maintain this reduction over a week's delay, the narrative and factual interventions did not differentially affect ratings of *support* or *effectiveness*. Based on prior work, we expected the narrative intervention to be more successful, but the factual intervention videos were essentially equally successful. This occurred despite the narrative interventions being considered more engaging, emotional, and narratively coherent than the factual ones (Experiment 1) and particularly the narrative intervention for the sex

offender registration law being more engaging and emotional (Experiment 2). Why were the narrative video interventions no more successful for changing CCT sex offender attitudes? Importantly, a core element of ELM is that long-term persuasive effects (Petty & Cacioppo, 1986) occur as a function of message elaboration. By contrast, long-term effects tend to fail when elaboration is low or disrupted. We theorized that the narrative intervention would increase participant engagement, thus increase elaboration of the content of the video, but we failed to observe immediate and long-term changes on *support* in favor of the narrative intervention. Even though narrative advantage in persuasion has been related to the degree of narrative engagement (Busselle & Bilandzic, 2008), Kreuter et al. (2007) claim that participants may not be motivated to elaborate because doing so would disrupt the pleasure associated with an engaging narrative (for similar arguments, see Moyer-Gusé, 2008; Slater & Rouner, 2002). At least in Experiment 1, it may be the case that our participants' engagement with the narrative videos actually disrupted or inhibited their ability to effectively elaborate on the content of the material presented, causing the narrative video to be no more effective than the factual video.

Alternatively, if the narrative did not directly inhibit the elaboration process, some participants may have lacked the cognitive capacity or motivation to further elaborate on the material presented to them. An attentional tradeoff may also explain why memory was lower for the narrative, compared with the factual intervention, at the immediate test. Moreover, because elaboration depends on one's cognitive capacity to elaborate and not simply one's motivation to do so, an engaging narrative may influence individuals differently. For example, those who have the available cognitive capacity to elaborate may see lasting changes, whereas individuals who are less cognitively able to elaborate may not change or may have only fleeting changes (See also Kreuter et al., 2007; Oschatz & Marker, 2020). On one hand, our narrative intervention might not have systematically changed *support* ratings more than did the factual video because it did not influence the elaboration process in a different way. On the other hand, the narrative video could have inhibited the elaboration process but also led to a reduction in *support* ratings through a different mechanism. Unfortunately, we did not design these experiments to separate out these potential influences. Any of these possibilities, however, may explain the failure of the narrative-based videos to differentially affect our sample overall ratings and specifically those of self-identified women.

Moreover, self-identified women were hypothesized to be more susceptible to attitudinal change through emotion-based, heuristic, peripheral, or System 1 processing associated with more narrative attempts to change viewpoints. Yet, much like our general findings, results with regard to self-identified women from Experiment 1 suggest that while the narrative-based interventions again were perceived highly on the dimensions associated with these kinds of processing, they did not have specific effects in changing self-identified women's attitudes as opposed to men's. When self-identified women's higher overall favoritism for the two sex offender CCT policies was statistically controlled for, the effects of the all interventions including the narrative versus factual manipulations were similar between the two gender groups.

Learning and Memory

Does learning and memory of the intervention content actually matter for participants' ratings of *support* and *effectiveness* for these

sex offender CCT laws? Campbell and Newheiser (2019) found that they are unlikely to affect participants' ratings. The literature on misconceptions more broadly is more mixed but does make the basic claim that remembering the refutation may be necessary but not sufficient for correcting a misconception (Swire & Ecker, 2018). Here, we found that in Experiment 1, participants learned about the content of the interventions relatively similarly, regardless of the CCT law and whether the intervention was narrative or factual. That is, participants answered about 75% of the memory questions correctly immediately after the interventions were presented. Perhaps this was related to participants' reduction of *support* for the laws immediately, but certainly, perceptions of *effectiveness* were not influenced by the learning of the intervention content, whereas in Experiment 2, participants actually learned more from the factual videos than from the narrative ones, in direct opposition to our predictions. Participants also answered significantly more questions correctly for the sex offender registration law than for the housing restriction law. This pattern mirrors the pattern of ratings for engagement and emotion regarding the two CCT laws in Experiment 2, suggesting a link between these ratings, learning, and memory. However, there is little reason to expect that these differences in initial learning are solely responsible for overall changes in attitudes.

Interestingly, one of the findings for initial learning in Experiment 2 holds for memory for the intervention content a week later: Participants retained more information about the sex offender registration law interventions. In contrast, critically, after a week's delay, whatever advantage there was for the factual video intervention disappeared. Memory performance for the two types of interventions equalized. Examining the pattern of means, though forgetting occurred for all conditions, it appears that the degree of forgetting over a week was smaller for the narrative interventions from the immediate test to the delayed test. Such a finding is consistent with prior work showing that narratives may be more memorable (Maria & Johnson, 1990). Although ultimately there was no difference between memory retention of the intervention content for the factual versus narrative videos, we thus suspect the underlying processes at play may be different. For the factual intervention, it could again be that these particular participants remembered this content better, and even with forgetting, maintained a reasonable degree of memory. Whereas for the narrative intervention, due to its nature, the rate of forgetting was slowed. The narrative intervention may have evoked more sympathy or empathy toward sex offenders suffering the unintended consequences of the CCT laws (see Appel et al., 2015; Busselle & Bilandzic, 2009; Cohen, 2001; Green & Brock, 2000), leading to memories slightly more resistant to forgetting. However, with neither intervention being learned or remembered significantly better than the other over the delay and neither intervention being more effective for changing participants' ratings of the two sex offender CCT laws, it is difficult to make strong claims about the contribution of learning and memory to the success of the interventions. What is heartening is that participants learned and retained over a week most of the content in the intervention.⁸ It may

⁸ In fact, in Experiment 1, those who reduced their *support* ratings remembered more content (79.0%) from the intervention than those who did not change their ratings (70.5%) and those who increased their *support* (63.9%). In Experiment 2, those who reduced their *support* (80.0%) remembered more information immediately than those who did not change ratings (70.5%) and those who increased *support* (73.9%), a difference which preserved over a delay for the three conditions, 76.6%, 67.6%, and 68.5%, respectively.

be the case that participants who are receptive to the intervention may be motivated to attend to and remember the intervention content or, vice versa, that the remembered content is influencing their support.

Limitations and Future Directions

Although the results of this research are important, they are clearly just a first step in creating interventions that might lead to reform in CCT laws, and sex offender CCT laws in particular. The present research is necessarily limited by the participant sample used, the video interventions created, and perhaps most importantly by whether changes to the participants' attitudes would actually lead to calls or impetus for modification or reform of the laws. Although the participant sample selected was designed to be representative based on 2010 Census data and Qualtrics selection criteria, it is not possible to determine whether it is truly representative of the general public and the results are generalizable more broadly. It may also be the case that interventions need to be targeted to specific individual characteristics and differences of the population sampled, and some groups may be more amenable to certain types of interventions than others. Unfortunately, analyses were not conducted to specifically examine the effects of ethnicity and political viewpoint on these CCT laws and the presented interventions, and there is reason to believe from past research that these factors may be significant both in initial view toward CCT laws, and later group reactions to these interventions (Krauss et al., 2021).

This research is also limited by the idiosyncratic nature of the videos. Although the sex offender CCT law videos were designed to have the same number of arguments detailing problems with the laws, it is possible that they were not truly equivalent making comparisons between the videos' effects on the public's attitudes problematic. Further, the laws themselves are not identical, and our research indicates that initial differences in viewpoints existed between these related but not equivalent laws. As a result, any of these factors could have affected the findings, and may partially explain the slightly stronger effects for the sex offender registry video interventions in Experiment 2. Relatedly, the narrative videos necessarily introduced different characters with different stories that may or may not have been less or more compelling and/or persuasive to the audience. The main characters in both the narrative videos differed in age, ethnicity, and gender, and neither was a prototypical example of a sex offender. Moreover, the video scenarios presented represented atypical circumstances, it is unusual for there to be a female sex offender subject to SORN laws, and it is also uncommon for sexual offenders to be limited to living under a bridge because of housing restrictions. These instances were chosen, however, because they demonstrated the unintended consequences of the laws. Although this research did not generally find any specific differences specific to the content of individual videos, future research should examine whether factors, such as the ethnicity, gender, and other background characteristics of the individuals or circumstances portrayed in the videos have differential effects.

Additionally, our exploration of the mechanisms that led to changes in participant attitudes was largely unsuccessful. Although it is important to determine that engagement and emotional reactions to the videos did not seem to play a significant role in attitudinal change nor did their memory for the content of the

intervention, it would be useful to have a better understanding of how change occurred. Importantly, future research should consider baseline measures of participant knowledge of various aspects of the laws it wishes to affect, so that a clearer picture of what participants gained through the interventions could be assessed. Likewise, future research could also examine how the participants elaborated on provided information through self-report or other means to more directly determine the relationship between the information provided in interventions and attitude change. Further, a discussion-based format was successful in changing undergraduates' immediate attitudes toward treatment for sexual offenders and their general attitudes toward sex offenders (Kleban & Jeglic, 2012). Such a discussion-based intervention might be successful here as well because it might compel participants to elaborate on content through these discussions, which consequently might act as a mechanism of change.

Last, although the delay condition was a substantial innovation over past research, it would be prudent to determine whether the attitudinal effects lasted longer than a week. Furthermore, future research should determine even if these attitudinal changes are even more long lasting, and whether they actually lead to participant action. It may be one thing to significantly change attitudes toward ineffective laws, but quite another to change them sufficiently to lead to specific behavioral impetus for change and reform of these laws.

In the end, this research underscores the importance of examining different metrics of perceptions surrounding the laws. In practical terms, changing one's *support* either short-term or long-term may involve different cognitive processes than changing one's perceived *effectiveness* of those same laws. In addition, research has not yet identified whether *support* or *effectiveness* is more predictive of actual behavioral action that would lead to these law's reform.

References

- Aguilar, S. J., Polikoff, M. S., & Sinatra, G. M. (2019). Refutation texts: A new approach to changing public misconceptions about education policy. *Educational Researcher*, 48(5), 263–272. <https://doi.org/10.3102/0013189X19849416>
- Alvarez, M. J., & Miller, M. K. (2016). Counterfactual thinking about crime control theater: Mock jurors decision making in an AMBER alert trial. *Psychology, Public Policy, and Law*, 22(4), 349–361. <https://doi.org/10.1037/law0000098>
- Appel, M., & Richter, T. (2007). Persuasive effects of fictional narratives increase over time. *Media Psychology*, 10(1), 113–134.
- Appel, M., Gnamb, T., Richter, T., & Green, M. C. (2015). The Transportation Scale-Short Form (TS-SF). *Media Psychology*, 18(2), 243–266. <https://doi.org/10.1080/15213269.2014.987400>
- Boas, T. C., Christenson, D. P., & Glick, D. M. (2020). Recruiting large online samples in the United States and India: Facebook, Mechanical Turk and Qualtrics. *Political Science Research and Methods*, 8(2), 232–250. <https://doi.org/10.1017/psrm.2018.28>
- Brashier, N. M., & Marsh, E. J. (2020). Judging truth. *Annual Review of Psychology*, 71(1), 499–515. <https://doi.org/10.1146/annurev-psych-010419-050807>
- Budd, K. M., & Mancini, C. (2016). Crime control theater: Public (mis-) perceptions of the effectiveness of sex offender residence restrictions. *Psychology, Public Policy, and Law*, 22(4), 362–374. <https://doi.org/10.1037/law0000083>

- Busselle, R., & Bilandzic, H. (2008). Fictionality and perceived realism in experiencing stories: A model of narrative comprehension and engagement. *Communication Theory*, 18(2), 255–280. <https://doi.org/10.1111/j.1468-2885.2008.00322.x>
- Busselle, R., & Bilandzic, H. (2009). Measuring narrative engagement. *Media Psychology*, 12(4), 321–347. <https://doi.org/10.1080/15213260.903287259>
- Campbell, D. S., & Newheiser, A.-K. (2019). Must the show go on? The (in)ability of counterevidence to change attitudes toward crime control theater policies. *Law and Human Behavior*, 43(6), 568–584. <https://doi.org/10.1037/lhb0000338>
- Chaiken, S. (1987). The heuristic model of persuasion. In *Social influence: The Ontario Symposium* (Vol. 5, pp. 3–39).
- Chan, M. P. S., Jones, C. R., Hall Jamieson, K., & Albarracín, D. (2017). Debunking: A meta-analysis of the psychological efficacy of messages countering misinformation. *Psychological Science*, 28(11), 1531–1546.
- Chi, M. T. H. (2008). Three types of conceptual change: Belief revision, mental model transformation, and categorical shift. In S. Vosniadou (Ed.), *International handbook of research on conceptual change* (pp. 61–82). Routledge.
- Cohen, J. (2001). Defining identification: A theoretical look at the identification of audiences with media characters. *Mass Communication & Society*, 4(3), 245–264. https://doi.org/10.1207/S15327825MC50403_01
- Cook, G. (2021). Changing the public's Crime Control Theater attitudes. [online] OSF. Available at <https://osf.io/3dr2j/>
- Cook, J., Ecker, U., & Lewandowsky, S. (2015). Misinformation and how to correct. In R. Scott & S. Kosslyn (Eds.), *Emerging trends in the social and behavioral sciences: An interdisciplinary, searchable, and linkable resource* (pp. 1–17). John Wiley and Sons. <https://doi.org/10.1002/9781118900772>
- Dal Cin, S., Zanna, M., & Fong, G. (2004). Narrative persuasion and overcoming resistance. In E. S. Knowles & J. A. Linn (Eds.), *Resistance and persuasion* (pp. 175–191). Erlbaum.
- DeVault, A., Miller, M. K., & Griffin, T. (2016). Crime control theater: Past, present, and future. *Psychology, Public Policy, and Law*, 22(4), 341–348. <https://doi.org/10.1037/law0000099>
- Dole, J. A., & Sinatra, G. M. (1998). Reconceptualizing change in the cognitive construction of knowledge. *Educational Psychologist*, 33(2-3), 109–128. <https://doi.org/10.1080/00461520.1998.9653294>
- Duwe, G., Donnay, W., & Tewksbury, R. (2008). Does residential proximity matter? A geographic analysis of sex offense recidivism. *Criminal Justice and Behavior*, 35(4), 484–504. <https://doi.org/10.1177/0093854807313690>
- Ecker, U. K., Lewandowsky, S., Swire, B., & Chang, D. (2011). Correcting false information in memory: Manipulating the strength of misinformation encoding and its retraction. *Psychonomic Bulletin & Review*, 18(3), 570–578. <https://doi.org/10.3758/s13423-011-0065-1>
- Goode, E., & Ben-Yehuda, N. (1994). Moral panics: Culture, politics, and social construction. *Annual Review of Sociology*, 20(1), 149–171. <https://doi.org/10.1146/annurev.so.20.080194.001053>
- Green, M. C., & Brock, T. C. (2000). The role of transportation in the persuasiveness of public narratives. *Journal of Personality and Social Psychology*, 79(5), 701–721. <https://doi.org/10.1037/0022-3514.79.5.701>
- Griffin, T., & Miller, M. K. (2008). Child abduction, AMBER alert, and crime control theater. *Criminal Justice Review*, 33(2), 159–176. <https://doi.org/10.1177/0734016808316778>
- Guzzetti, B. J., Snyder, T. E., Glass, G. V., & Gamas, W. S. (1993). Promoting conceptual change in science: A comparative meta-analysis of instructional interventions from reading education and science education. *Reading Research Quarterly*, 28(2), 117–159. <https://doi.org/10.2307/747886>
- Hammond, M., Miller, M. K., & Griffin, T. (2010). Safe haven laws as crime control theater. *Child Abuse & Neglect*, 34(7), 545–552. <https://doi.org/10.1016/j.chiabu.2009.11.006>
- Hipp, J. R., Turner, S., & Jannetta, J. (2010). Are sex offenders moving into social disorganization? Analyzing the residential mobility of California parolees. *Journal of Research in Crime and Delinquency*, 47(4), 558–590. <https://doi.org/10.1177/0022427810381093>
- Hynd, C., & Alvermann, D. E. (1986). The role of refutation text in overcoming difficulty with science concepts. *Journal of Reading*, 29(5), 440–446.
- Jones, S. H., Johnson, M. L., & Campbell, B. D. (2015). Hot factors for a cold topic: Examining the role of task-value, attention allocation, and engagement on conceptual change. *Contemporary Educational Psychology*, 42(1), 62–70. <https://doi.org/10.1016/j.cedpsych.2015.04.004>
- Kahneman, D. (2011). *Thinking, fast and slow*. Farrar, Straus & Giroux.
- Kahneman, D., Slovic, P., & Tversky, A. (1982). *Judgment under uncertainty: Heuristics and biases*. Cambridge University Press; <https://doi.org/10.1017/CBO9780511809477>
- Kendeou, P., Smith, E. R., & O'Brien, E. J. (2013). Updating during reading comprehension: Why causality matters. *Journal of Experimental Psychology: Learning, Memory, and Cognition*, 39(3), 854–865. <https://doi.org/10.1037/a0029468>
- Kleban, H., & Jeglic, E. (2012). Dispelling the myths: Can psychoeducation change public attitudes towards sex offender? *Journal of Sexual Aggression*, 18(2), 179–193. <https://doi.org/10.1080/13552600.2011.552795>
- Knowles, E. S., & Linn, J. A. (2004). The importance of resistance to persuasion. In E. S. Knowles & J. A. Linn (Eds.), *Resistance and persuasion* (pp. 3–11). Erlbaum. <https://doi.org/10.4324/9781410609816>
- Kowalski, P., & Taylor, A. K. (2009). The effect of refuting misconceptions in the introductory psychology class. *Teaching of Psychology*, 36(3), 153–159. <https://doi.org/10.1080/00986280902959986>
- Krauss, D., & Scurich, N. (2014). The Impact of case factors on jurors' decisions in a sexual violent predator hearing. *Psychology, Public Policy, and Law*, 20(2), 135–145. <https://doi.org/10.1037/law0000007>
- Krauss, D., Cook, G. I., Song, E., & Umanath, S. (2021). The public's perception of crime control theater laws: It's complicated. *Psychology, Public Policy, and Law*, 27(3), 316–327. <https://doi.org/10.1037/law0000302>
- Kreuter, M. W., Green, M. C., Cappella, J. N., Slater, M. D., Wise, M. E., Storey, D., Clark, E. M., O'Keefe, D. J., Erwin, D. O., Holmes, K., Hinyard, L. J., Houston, T., & Woolley, S. (2007). Narrative communication in cancer prevention and control: A framework to guide research and application. *Annals of Behavioral Medicine*, 33(3), 221–235. <https://doi.org/10.1007/BF02879904>
- Lerner, J. S., Li, Y., Valdesolo, P., & Kassam, K. S. (2015). Emotion and decision making. *Annual Review of Psychology*, 66(1), 799–823. <https://doi.org/10.1146/annurev-psych-010213-115043>
- Levenson, J. S., & Cotter, L. P. (2005). The effect of Megan's Law on sex offender reintegration. *Journal of Contemporary Criminal Justice*, 21(1), 49–66. <https://doi.org/10.1177/1043986204271676>
- Levenson, J. S., Brannon, Y. N., Fortney, T., & Baker, J. (2007). Public perceptions about sex offenders and community protection policies. *Analyses of Social Issues and Public Policy (ASAP)*, 7, 137–161. <https://doi.org/10.1111/j.1530-2415.2007.00119.x>
- Lewandowsky, S., Ecker, U. K., Seifert, C. M., Schwarz, N., & Cook, J. (2012). Misinformation and its correction: Continued influence and successful debiasing. *Psychological Science in the Public Interest*, 13(3), 106–131. <https://doi.org/10.1177/1529100612451018>

- Loewenstein, G. (1996). Out of control: Visceral influences on behavior. *Organizational Behavior and Human Decision Processes*, 65(3), 272–292. <https://doi.org/10.1006/obhd.1996.0028>
- Maria, K., & Johnson, J. M. (1990). Correcting misconceptions: Effect of type of text. *National Reading Conference Yearbook*, 39, 329–337.
- Moyer-Gusé, E. (2008). Toward a theory of entertainment persuasion: Explaining the persuasive effects of entertainment-education messages. *Communication Theory*, 18(3), 407–425. <https://doi.org/10.1111/j.1468-2885.2008.00328.x>
- Oppenheimer, D. M., Meyvis, T., & Davidenko, N. (2009). Instructional manipulation checks: Detecting satisficing to increase statistical power. *Journal of Experimental Social Psychology*, 45(4), 867–872. <https://doi.org/10.1016/j.jesp.2009.03.009>
- Oschatz, C., & Marker, C. (2020). Long-term persuasive effects in narrative communication research: A meta-analysis. *Journal of Communication*, 70(4), 473–496. <https://doi.org/10.1093/joc/fjaa017>
- Palmer, D. H. (2003). Investigating the relationship between refutational text and conceptual change. *Science Education*, 87(5), 663–684. <https://doi.org/10.1002/sce.1056>
- Petty, R. E., & Cacioppo, J. T. (1986). The elaboration likelihood model of persuasion. *Advances in Experimental Social Psychology*, 19(1), 123–205. [https://doi.org/10.1016/S0065-2601\(08\)60214-2](https://doi.org/10.1016/S0065-2601(08)60214-2)
- Prescott, J. J. (2016). Portmanteau ascendant: Post-release regulations and sex offender recidivism. *Connecticut Law Review*, 48(4), 1035–1078.
- Prescott, J. J., & Rockoff, J. E. (2011). Do sex offender registration and notification laws affect criminal behavior? *The Journal of Law & Economics*, 54(1), 161–206. <https://doi.org/10.1086/658485>
- Qualtrics Panels. (2020). Panel management software: On-demand insights with feedback from the right people, at the right time. <http://www.qualtrics.com/panel-management> and <https://www.qualtrics.com/research-services/online-sample/>
- Reddy, P., & Lantz, C. (2010). Myths, maths and madness. In D. Upton & A. Trapp (Eds.), *Teaching psychology in higher education* (pp. 54–81). Blackwell. <https://doi.org/10.1002/9781444320732.ch3>
- Rocklage, M. D., & Luttrell, A. (2021). Attitudes based on feelings: Fixed or fleeting? *Psychological Science*, 32(3), 364–380. <https://doi.org/10.1177/0956797620965532>
- Schutte, J., & Hosch, H. (1997). Gender differences in sexual assault verdicts: A meta-analysis. *Journal of Social Behavior and Personality*, 12(3), 759–772.
- Schwarz, N., Newman, E., & Leach, W. (2016). Making the truth stick & the myths fade: Lessons from cognitive psychology. *Behavioral Science & Policy*, 2(1), 85–95. <https://doi.org/10.1353/bsp.2016.0009>
- Sicafuse, L. L., & Miller, M. K. (2012). The effects of information processing and message quality on attitudes toward the AMBER alert system. *Applied Psychology in Criminal Justice*, 8(2), 69–86.
- Sinatra, G. M., & Broughton, S. H. (2011). Bridging reading comprehension and conceptual change in science education: The promise of refutation text. *Reading Research Quarterly*, 46(4), 374–393. <https://doi.org/10.1002/RRQ.005>
- Slater, M. D., & Rouner, D. (2002). Entertainment-education and elaboration likelihood: Understanding the processing of narrative persuasion. *Communication Theory*, 12(2), 173–191.
- Socia, K., & Harris, A. (2016). Evaluating public perceptions of the risk presented by registered sex offenders: Evidence of crime control theater? *Psychology, Public Policy, and Law*, 22(4), 375–385. <https://doi.org/10.1037/law0000081>
- Sunstein, C. (1996). On the expressive function of law. *University of Pennsylvania Law Review*, 144(5), 2021–2053. <https://doi.org/10.2307/3312647>
- Swire, B., & Ecker, U. K. (2018). Misinformation and its correction: Cognitive mechanisms and recommendations for mass communication. In B. Southwell, E. Thorson, & L. Sheble (Eds.), *Misinformation and mass audiences* (195–211), University of Texas Press.
- Swire, B., Ecker, U. K. H., & Lewandowsky, S. (2017). The role of familiarity in correcting inaccurate information. *Journal of Experimental Psychology: Learning, Memory, and Cognition*, 43(12), 1948–1961. <https://doi.org/10.1037/xlm0000422>
- Tewksbury, R., Jennings, W. G., & Zgoba, K. M. (2012). A longitudinal examination of sex offender recidivism prior to and following the implementation of SORN. *Behavioral Sciences & the Law*, 30(3), 308–328. <https://doi.org/10.1002/bsl.1009>
- Tippett, C. D. (2010). Refutation text in science education: A review of 2 decades of research. *International Journal of Science and Mathematics Education*, 8(6), 951–970. <https://doi.org/10.1007/s10763-010-9203-x>
- Vaughan, E. D. (1977). Misconceptions about psychology among introductory psychology students. *Teaching of Psychology*, 4(3), 138–141. https://doi.org/10.1207/s15328023top0403_9
- Vaughn, A. R., & Johnson, M. L. (2018). Communicating and enhancing teachers' attitudes and understanding of influenza using refutational text. *Vaccine*, 36(48), 7306–7315. <https://doi.org/10.1016/j.vaccine.2018.10.040>
- Wakefield, H. (2006). The vilification of sex offenders: Do laws targeting sex offenders increase recidivism and sexual violence? *The Journal of Sexual Offender Civil Commitment, Science and the Law*, 1(1), 141–149.
- Walter, N., & Murphy, S. T. (2018). How to unring the bell: A meta-analytic approach to correction of misinformation. *Communication Monographs*, 85(3), 423–441. <https://doi.org/10.1080/03637751.2018.1467564>
- Walter, N., & Tukachinsky, R. (2020). A meta-analytic examination of the continued influence of misinformation in the face of correction: How powerful is it, why does it happen, and how to stop it? *Communication Research*, 47(2), 155–177. <https://doi.org/10.1177/0093650219854600>
- Yelderman, L. A., Miller, M. K., Forsythe, S., & Sicafuse, L. L. (2018). Understanding crime control theater: Do sample type, gender, and emotions relate to support for crime control theater policies? *Criminal Justice Review*, 43(2), 147–173. <https://doi.org/10.1177/0734016817710695>
- Zgoba, K. M., & Mitchell, M. M. (2021). The effectiveness of sex offender registration and notification: A meta-analysis of 25 years of findings. *Journal of Experimental Criminology*. Advance online publication. <https://doi.org/10.1007/s11292-021-09480-z>
- Zgoba, K. M., Jennings, W. G., & Salerno, L. M. (2018). Megan's Law 20 years later: An empirical analysis and policy review. *Criminal Justice and Behavior*, 45(7), 1028–1046. <https://doi.org/10.1177/0093854818771409>

Appendix

Learning and Memory Questions

Sex Offender Registry Videos

1. In the United States, how many people are listed on sex offender registries?
 - a. Fewer than 600,000
 - b. 600,000
 - c. 800,000
 - d. More than 800,000
2. Crimes that require registration include
 - a. Offenses involving an additional party
 - b. Only felony offenses
 - c. Only dangerous offenses
 - d. All offenses
3. How long do individuals stay on the registry?
 - a. 5 years or less
 - b. 10 years or less
 - c. 20 years or less
 - d. Decades to a lifetime
4. Having sexual relations with an underage partner resulted in which level of child molestation?
 - a. Level 1
 - b. Level 2
 - c. Level 3
 - d. Level 4
5. Registration places which of the following restrictions on sex offenders?
 - a. Ability to be removed from the registry
 - b. Ability to move to another city
 - c. Ability to use public transit
 - d. Ability to freely use the internet
6. If offenders move to a different state, which of the following are they required to do?
 - a. Wear tracking devices
 - b. Take polygraph tests
 - c. Register within the state
 - d. Both b and c
7. Why do politicians not want to be viewed as “soft on crime”?
 - a. They make laws; they do not enforce them
 - b. They want to be re-elected
 - c. They want to be seen as allies to law enforcement
 - d. They want to keep the public’s trust
8. Sex offender registration laws apply to which population?
 - a. Only male offenders
 - b. Only dangerous male offenders
 - c. Only dangerous male and female offenders
 - d. All male and female offenders
9. Which of the following can be a result of passing the strictest laws for sex offenders?
 - a. Deterring future offenders
 - b. Keeping the public safe
 - c. Ignoring the constitutional rights of offenders
 - d. Classifying offenders as 2nd class citizens
10. The media and legislatures are exposed to what kind of the following evidence to show the effectiveness of sex offender laws?
 - a. Little to no evidence
 - b. Evidence from expert researchers in the field
 - c. Evidence from lawmakers
 - d. Both b and c

(Appendix continues)

11. Which State's laws are featured in the video?
- Florida
 - Oklahoma
 - Texas
 - California
12. All sex offenders on their registry, regardless of crime, were permanently banned from which of the following spaces?
- Public parks
 - Libraries
 - Movie theaters
 - Theme parks
13. Which year did this ban occur?
- 2010
 - 2012
 - 2014
 - 2016
14. In the example given in the video, which sex offender crime requires registration?
- 17 y/o dating a 14 y/o
 - 19 y/o dating a 17 y/o
 - 19 y/o dating a 14 y/o
 - 21 y/o dating a 19 y/o
15. What is the highest level of risk designation?
- Level 1
 - Level 2
 - Level 3
 - Level 4
- b. under a Causeway or bridge
- c. in a halfway house
- d. in prison
2. What address is listed on the sex offenders' driver's license?
- Julia Tuttle Causeway
 - Homeless
 - Miami-Dade Sex offender Road
 - 350 Orange Road
3. What does the 1,000-foot housing restriction law prevent sex offenders from living near?
- hospital, subway stops, and grocery stores
 - movie theaters, hotels, and shopping malls
 - school, parks, and playgrounds
 - stoplights, fire hydrants, and bus stops
4. What times must sex offenders abide by the housing restrictions?
- 6 a.m.–7 p.m.
 - noon-midnight
 - all day
 - 6 p.m.–7 a.m.
5. According to a study, in Miami-Dade County, how many housing units are available for less than \$1000 a month and less than \$750 a month?
- 15 and 0
 - 10 and 10
 - 50 and 10
 - 0 and 20

Sex Offender Housing Restriction Videos

1. Where does the sex offender housing restriction lead sex offenders to live?
- in specific apartments
 - in a halfway house
 - in a halfway house
 - in prison
2. Who is responsible both for the adoption of strict housing restriction laws in Miami-Dade county and helping sex offenders find affordable housing?
- The Governor
 - A lobbyist

(Appendix continues)

- c. A sex offender commission
- d. The police
7. Owing to the overlapping housing restrictions laws, what spaces are available for sex offenders to reside?
- a. There are many areas for sex offenders to live
- b. There are absolutely no places for sex offender to live
- c. There are several million dollar neighborhoods, industrial parks, and the Causeway
- d. None of the above
8. What is the other primary factor in addition to the Miami-Dade county housing restriction laws that makes it difficult for sex offenders to find housing?
- a. each city in the county has their own housing restriction laws
- b. there are more playgrounds in other cities in the county
- c. there are more schools in other cities in the county
- d. housing costs are cheaper in other cities in the county
9. What organization has filed a lawsuit because the local housing restrictions leave nowhere for sex offenders to live?
- a. BBB (The Better Business Bureau)
- b. DHHS (Department of Health and Human Services)
- c. ACLU (American Civil Liberties Union)
- d. Greenpeace
10. How many sex offenders are living in a settlement with poor sanitation and living condition because of the sex offender housing restriction laws?
- a. fewer than 25
- b. 25–40
- c. 40—65
- d. more than 65
11. Which State's laws are featured in the video?
- a. Florida
- b. Oklahoma
- c. Texas
- d. California
12. What is the gender of the narrator for the video?
- a. male
- b. female
- c. both male and female voices are heard
- d. there is no narrator
13. Who molested the daughter of the individual responsible for creating the sex offender housing restriction laws?
- a. a stranger
- b. her nanny
- c. a postal worker
- d. her school teacher
14. How many cities have overlapping housing restrictions laws?
- a. just 1
- b. between 15-25
- c. between 26-50
- d. more than 50
15. The first housing restriction law adopted (in 2005) required that sex offenders live at least how far away from schools?
- a. 100 feet or more
- b. 500 feet or more
- c. 2500 feet or more
- d. 5000 feet or more

Received September 9, 2021
 Revision received November 29, 2021
 Accepted November 30, 2021 ■