How Exposure to Violence Against LGBTQ+ People Motivates Mass Prosocial Responses Toward LGBTQ+ Group Members

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Abstract

We present a *Fickle Prosocial Violence Response Model* (FPVR) to explain how indirect exposure to civilian-perpetrated violence against marginalized minority groups motivates prosocial attitudes toward victimized groups. Although the mass public may not sympathize with marginalized groups, they may adopt prosocial attitudes toward marginalized groups subject to civilian-perpetrated violence if the violence is salient and perceptibly illegitimate. However, the adoption of prosocial attitudes may be fickle. We find evidence consistent with the model. Studies 1-3 show high-profile violence against LGBTQ+ people increases support for LGBTQ+ rights and reduces negative attitudes toward LGBTQ+ group members. But, the adoption of prosocial attitudes is short-term. Study 4 shows less salient violence against LGBTQ+ people may not engender prosocial attitudes at the outset. Our findings suggest violent events must be sufficiently salient to initially motivate prosocial beliefs. Nevertheless, salient civilian-perpetrated violence against marginalized groups may not *sustainably* motivate prosocial beliefs toward targeted groups.

Keywords: exposure to violence; political violence; prosocial attitudes; intergroup relations; LGBTQ+ politics

Word Count: 11987

1 Introduction

Since the Stonewall Uprising, there have been numerous instances of anti-LGBTQ+ violence in the US. Despite progress on LGBTQ+ rights (Flores, 2014), anti-LGBTQ+ violence and hate crimes have increased[1] while several states introduced a record number of anti-LGBT+ laws recently.[2] Perhaps the most prominent, recent, instance of anti-LGBTQ+ violence was the 2022 Club Q massacre, where a gunman killed 5 clubgoers at a Colorado Springs LGBTQ+ nightclub. These violent acts, while sympathy-inducing within media and amongst some political elites, may reflect durable heteronormative societal norms.[3] Therefore, an open question is whether *indirect* (i.e. media observation of violence) exposure to high-profile civilian-perpetrated violence against LGBTQ+ group members motivates introspection among the mass public, shifting attitudes prosocially toward LGBTQ+ people.

We synthesize several theoretical insights and present a *Fickle Prosocial Violence Response* (FPVR) model to explain how violence against marginalized groups may elicit prosocial attitudes toward targeted groups. Although the mass public may not strongly empathize with marginalized minority groups (Cikara et al., 2014), violence against marginalized groups may elicit prosocial attitudes if the violence is salient, perceptibly illegitimate, and the media and/or elites respond sympathetically (Iyengar, 1994; Birkland, 1998; Branscombe and Miron, 2004; Harth et al., 2008; Vossen et al., 2017). However, prosocial attitude adoption may be short-term. Social group attitudes are typically entrenched, even in light of salient events (Sears, 1993; Tuch and Weitzer, 1997; Kite et al., 2019). Immediate adoption of prosocial beliefs after violence may be counterbalanced by countervailing information in a discriminatory society (Vuletich and Payne, 2019). Elite messaging and pressure to support targeted groups may dissipate after an event loses salience (Downs, 1972), undercutting sustainable prosocial attitudinal shifts (Zaller, 1992; Birkland and Lawrence, 2009).

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[1]https://www.hrc.org/press-releases/new-fbi-hate-crimes-report-shows-increases-in-anti-lgbtq-attacks

[2]https://www.aclu.org/press-releases/over-120-bills-restricting-lgbtq-rights-introduced-nationwide-2023-so-far

[3]*Heteronormativity* is "privileging gender conformity, heterosexuality, and nuclear families over "deviant" forms of gender expression, sexuality, and family (Pollitt et al., 2021)"

We find evidence supporting the *FPVR* model by using several surveys and an unexpected-event-during-survey-design. Studies 1-3 demonstrate the public adopts prosocial attitudes toward LGBTQ+ community segments and their political rights shortly after civilian violence against LGBTQ+ group members (i.e. the Pulse massacre, Matthew Shepard's murder). However, *these attitudinal shifts do not persist.* Study 4 demonstrates the Club Q massacre had no effect on anti-gay, anti-trans, attitudes. Consistent with the model, we provide evidence the null effects at the outset are due to the less salient nature of the Club Q massacre vis-a-vis the Pulse massacre and Shepard's murder. We provide corroborating evidence by demonstrating less salient violent incidents against LGBTQ+ people outside those in Studies 1-4 largely do not motivate prosocial mass attitudes.

Our theory and evidence makes several contributions. First, the *FPVR* model helps explain *how* violence against marginalized groups motivates prosocial beliefs toward targeted groups among the mass public. Our model is important in light of several salient instances of civilian violence against marginalized groups in the US: Vincent Chin's 1982 murder, a Chinese man murdered due to anti-Japanese resentment; James Byrd's 1996 murder, a Texas Black man lynched by white supremacists; the 2015 Charleston Church massacre, where a white supremacist murdered 9 Black churchgoers; the 2015 Stanford sexual assault case (*People v. Turner)*, where a Stanford undergraduate man sexually assaulted a woman; the 2019 El Paso massacre, where a white supremacist killed 23 people, mostly Latinos, to counteract a "Hispanic invasion"; the 2021 Atlanta spa shooting, where 8 people, mostly Asian women, were killed; and the 2022 Buffalo massacre, where a white supremacist killed 10 Black people because he felt non-whites were "replacing" whites. We show these events may not serve as sustainable moments of reevaluation concerning the socio-political status of marginalized groups and may not motivate prosocial attitudes at the outset if they are insufficiently salient. Thus, our model and evidence may explain why these events have not led to societal adjustment of beliefs perpetuating social inequalities.

Second, our analysis extends prior research on violence against marginalized groups by

examining a different perpetrator type (civilian) and group (LGBTQ+). Prior research on violence and prosocial attitudes in the U.S. typically focuses on state (i.e. police) violence against Black people. This research often identifies prosocial responses to violence, but mixed evidence on effect sustainability (Tuch and Weitzer, 1997; Sigelman et al., 1997; Chudy and Jefferson, 2021; Reny and Newman, 2021). Civilian-perpetrated violence against LGBTQ+ group members may have theoretically distinct but important consequences. Civilian-perpetrated (instead of state-perpetrated) violence may be less likely to initially and/or sustainably motivate prosocial attitudes. The mass public may attribute state violence to systemic yet reformable institutional problems, motivating policy preferences benefiting targeted groups (Oskooii, 2016). Yet, civilian violence may be rationalized as a problem inherent to a troubled individual as opposed to the public's systemic aggregate queerphobia (Ott and Aoki, 2002), undercutting, at worse, initial introspection over one's own queerphobic beliefs post-violence, at best, sustained introspection in a heteronormative society consistently encouraging queerphobia.[4] Moreover, unlike racialized state violence, the violence we examine are not associated with subsequent mass protest, which may sustain event salience, facilitating long-lasting attitudinal shifts (Reny and Newman, 2021). Consistent with these theoretical perspectives (and the *FPVR* model), our evidence highlights similarities and contrasts in the prosocial consequences of different types of violence against different groups, paving the way for further work in assessing how contextual variation of violent events may differentially motivate mass attitudinal responses.

Third, our analysis contributes to the *Focusing Event* literature (Birkland, 1998). Prior research shows salient events shift mass attitudes, but briefly because of eventual salience loss (Sigelman et al., 1997; Birkland and Lawrence, 2009). Additionally, LGBTQ+ politics research demonstrates high-profile pro-LGBTQ+ court cases (Flores, 2015), Pride parades (Ayoub, Page, et al., 2021), and celebrities coming out (Miller et al., 2020), can motivate prosocial attitudes toward LGBTQ+ people. But, this research places little emphasis on

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[4]"Queer" denotes a gender/sexual identity that does not correspond to heterosexual notions of sexuality and gender.

effect sustainability; does not assess event salience variation at the outset; and does not focus on violence against LGBTQ+ people, which may *reflect*, instead of *undercut*, queerphobia. We provide new evidence consistent with *Focusing Event Theory* in an unexplored domain.

2   Violence and Prosociality

Preexisting theory and evidence demonstrate *direct* or *proximal* (i.e. via close social ties, like family, friends, acquaintances) violence exposure during inter-group conflict may motivate parochialism, encourage intra- but not inter-group altruism, and undercut emotional substrates facilitating inter-group prosocial behaviors and attitudes, including, positive evaluations of outgroups and support for their political rights (Rusch, 2014; Lupu and Peisakhin, 2017; Mironova and Whitt, 2018; Hadzic et al., 2020). Other evidence, building on *Post-Traumatic Growth* and *Altruism Born of Suffering Theory* (Staub and Vollhardt, 2010), shows inter-group violence can motivate prosocial, altruistic attitudes and behaviors toward outgroups (Bakke et al., 2009). Direct or proximal violence exposure may motivate inter-group prosociality since victimization generates a basis for empathy (Sirin et al., 2021).

Although prior work suggests direct or proximal exposure to inter-group, mostly inter-ethnic, violence motivates prosociality, it is less clear how one-sided[5] *indirect* exposure to violence against LGBTQ+ people influences prosocial attitudes toward LGBTQ+ group members among dominant groups or the mass public. Hereafter, we define prosocial attitudes as positive feelings toward LGBTQ+ group members and policies facilitating their rights.

One expectation is that indirect exposure to one-sided violence may *not* motivate prosocial beliefs. Insufficient media coverage and attention to violent events may not produce agenda-setting effects mobilizing prosocial mass attitudes (Birkland, 1998). Additionally, *Social Identity Theory* (SIT) implies dominant group members garner self-esteem from minority group marginalization (Tajfel and Turner, 1982). Thus, the mass public may garner psychic benefits from indirectly observing violence against minority groups (Cikara et al.,

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[5] "One-sided" refers to dominant group-perpetrated violence.

2014). Consistent with *Inter-group Emotions Theory* (IET), these dynamics may be exacerbated by the absence of direct experiences with analogous violence facilitating empathy (Sirin et al., 2021). Moreover, the social distance between modal mass public members and, for example, LGBTQ+ people, may generate an empathy gap,[6] undercutting the adoption of prosocial attitudes after indirect violence exposure (Cikara et al., 2014). Finally, if the violence is civilian-perpetrated, the violent event may be framed by the media as a problem inherent to a troubled individual instead of societal antipathy toward LGBTQ+ people (Iyengar, 1994; Ott and Aoki, 2002; Zahzah, 2019), which could undercut reflection concerning one's own antipathic beliefs among the mass public. Therefore, we may observe an empirical pattern consistent with Figure 1, Panel A, where indirect exposure to civilian violence against marginalized groups does not motivate mass prosocial attitudes toward targeted groups.

Another expectation is that, under some conditions, indirect exposure to violence against marginalized groups may motivate prosocial attitudes to ameliorate conditions concomitant with the violence. *Focusing Event Theory* implies salient violent incidents can mobilize mass attitudes (Birkland, 1998). These attitudes may be more likely to be mobilized prosocially if the media and elites express the violence is illegitimate and are sympathetic toward the targeted group (Zaller, 1992; Iyengar, 1994). Indeed, sympathetic messaging by partisan elites post-violence may help socially conservative co-partisans reconsider prejudicial attitudes (Harrison and Michelson, 2017). The media also has a powerful influence on LGBTQ+ mass attitudes. Positive LGBTQ+ media portrayals and parasocial LGBTQ+ contact motivates prosocial attitudes toward LGBTQ+ people (Ayoub and Garretson, 2017; Miller et al., 2020).

Likewise, alternative SIT and IET insights suggest if the mass public feels one-sided civilian violence against marginalized groups is illegitimate, it reflects poorly on their own stigmatizing beliefs, even if minority group marginalization otherwise facilitates self-esteem

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[6]For the Pulse massacre, this gap may be amplified by the predominantly Latinx victims.
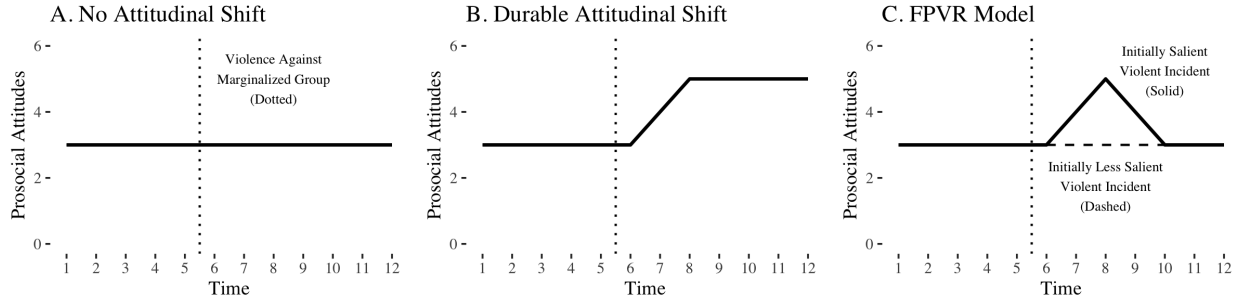
Figure 1: Stylized Expectations Concerning the Effect of Violence Against Marginalized Groups on Prosocial Attitudes. Horizontal lines denote prosocial attitudes toward marginalized groups (y-axis) over time (x-axis). The dotted line characterizes violence against a marginalized group.

(Harth et al., 2008). Dominant group or mass public members may emotionally regulate these psychic costs by reacting to violence against marginalized groups with sympathy and/or empathy (Branscombe and Miron, 2004), motivating the downstream adoption of prosocial attitudes toward marginalized groups (Harth et al., 2008; Stotzer, 2009).

Some prior research implies prosocial attitude adoption toward marginalized groups after violence exposure may be durable. The mass public has become increasingly inclusive toward LGBTQ+ community segments over several decades (Flores, 2014), suggesting the public may be durably receptive to sympathetic appeals after violence against LGBTQ+ group members. Indeed, Broockman and Kalla (2016) show a perspective-taking exercise can increase support for transgender anti-discrimination policies up to 3 months. Oskooii et al. (2021) show high-profile institutionalized discrimination against religious minorities can reduce mass support for policies negatively affecting targeted groups up to a year. Reny and Newman (2021) show anti-Black police violence can motivate prosocial attitudes toward Black people up to at least 100 days. Therefore, we might observe an empirical pattern consistent with Figure 1, Panel B, where the public adopts increasingly prosocial attitudes after indirect exposure to civilian violence against marginalized groups, and these attitudinal shifts are durable.

3   The Fickle Prosocial Violence Response (FPVR) Model

However, we develop and present a *Fickle Prosocial Violence Response* (FPVR) model, which posits perceptibly illegitimate salient civilian violence against marginalized groups can motivate prosocial attitudes toward targeted groups. But, *these attitudinal shifts may be fickle* given reductions in event salience, the dispositional qualities of social group attitudes, and countervailing information in an otherwise discriminatory society.

Issue-Attention Cycle Theory posits the public may react to dramatic events highlighting ignored social issues, like violence against LGBTQ+ group members, in an initially proactive manner. However, attitudinal shifts seeking to resolve a social ill may not be sustainable when it becomes clear resolving the problem is difficult (e.g. reevaluating queerphobic beliefs offering a privileged status) and the problem becomes less salient over time (Downs, 1972). Prior research implies sympathetic media and elite messaging after violence must persist to generate sustainable prosocial responses (Zaller, 1992). Without persistent sympathetic messaging, the masses may not be continually encouraged to adopt positive emotions toward targeted groups in addition to an understanding of the violence as illegitimate, producing a decay in prosocial attitudinal responses.

Moreover, prosocial attitudinal responses may be short-term impression management. Illegitimate violence rejected by society, media, and elites may motivate prosocial expressions toward the targeted group among the masses to save face (Harth et al., 2008), but may not result in long-term attitudinal shifts motivated by the difficult task of dismantling hierarchical social relations (Nguyen et al., 2021). Short-term impression management may not be capable of undermining predispositions toward marginalized groups rooted in pre-adult socialization (Sears, 1993; Kite et al., 2019). Long-term attitudinal shifts may also be undercut by countervailing pressure to adhere to queerphobic norms in an otherwise heteronormative society (Vuletich and Payne, 2019).

Framing theory may also help explain the potential absence of long-term prosocial attitudinal shifts. Story framing affects how the public assigns responsibility to an event and

preferred policy and societal responses. Media outlets may adopt episodic or thematic frames in their news coverage. Episodic frames emphasize event-centered information with attention toward an individual's actions (e.g. the violent perpetrator) whereas thematic frames emphasize broader problems (e.g. queerphobia) (Iyengar, 1994). Ott and Aoki (2002) and Zahzah (2019) posit media frames of prominent instances of violence against LGBTQ+ people, such as Matthew Shepard's murder and the Pulse massacre, often emphasize the perpetrator's gratuitous violence instead of societal heteronormativity. These episodic frames may allow mass public members to simply express prosocial attitudes toward LGBTQ+ to absolve oneself of short-term guilt but lose sight of reflecting over their quotidian role facilitating a heteronormative society in the long-term (Ott and Aoki, 2002), especially in light of countervailing information from a queerphobic society[7]

In summary, an observable implication of the theoretical synthesis informing the *FPVR* model is that indirect exposure to *salient* and *sympathetic* messaging from media and elites after violence against LGBTQ+ group members may encourage the adoption of prosocial attitudes toward LGBTQ+ community segments. But, the adoption of prosocial attitudes toward LGBTQ+ group members may not be long-lasting. Therefore, we may observe an empirical pattern consistent with the solid line on Figure 1, Panel C. **H1:** Indirect exposure to civilian violence against LGBTQ+ group members will initially increase prosocial attitudes toward LGBTQ+ group members. **H2:** But, indirect exposure to civilian violence against LGBTQ+ community segments will not produce sustainable increases in prosocial attitudes.

Prior evidence corroborates the *FPVR* model. Some evidence shows high-profile anti-Black police violence increased prosocial attitudes toward Black people, but these attitudes reverted to the pre-violence equilibrium shortly thereafter (Tuch and Weitzer, 1997; Chudy and Jefferson, 2021; Nguyen et al., 2021). Birkland and Lawrence (2009) demonstrate Columbine immediately increased gun control support, but only briefly.

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[7]Moreover, if the violence is a mass shooting, conservative outlets, like Fox News, may emphasize gun rights, reducing sustained discussion of violence against LGBTQ+ group members that may motivate long-term prosocial belief adoption (Cassino, 2016).

3.1 Individual-Level Heterogeneity

Shared Marginalization. *Group Empathy Theory* posits marginalized group members who possess similar discriminatory experiences support each other (Sirin et al., 2021). Cross-group support may be more likely if the discrimination a particular group experiences is perceptibly shared (Cortland et al., 2017). Members of other subjugated groups (e.g. non-whites, women), may perceive similarities between their experiences and those of LGBTQ+ group members, especially with regard to targeted violence. Indeed, the Introduction shows women and non-whites have been historically subject to targeted violence in a conceivably similar manner as LGBTQ+ people. Thus, group members discriminated against on other dimensions, like race and/or gender, may be more inclined to respond prosocially toward LGBTQ+ group members after exposure to violence against LGBTQ+ community segments.

Political Liberalism. Relative to conservatives and moderates, liberals are less socially conservative concerning sexuality and gender and are more acceptant of marginalized social groups. Indeed, liberals are more favorable toward LGBTQ+ community segments and pro-LGBTQ+ policies (Flores, 2014). Conservatives are more likely to adopt anti-LGBTQ+ beliefs in response to threatening anti-LGBTQ+ elite rhetoric while liberals are resistant to such rhetoric (Górska and Tausch, 2022). Relative to moderates and conservatives, liberals are also more inclined to respond prosocially toward marginalized groups in response to high-profile state violence against said groups (Reny and Newman, 2021). Therefore, liberals may be more likely than conservatives to adopt prosocial attitudes toward LGBTQ+ group members in response to violence against LGBTQ+ people.

Geographic Context. Individuals living in areas with a higher composition of LGBTQ+ people may be more likely to come into contact with LGBTQ+ group members and develop strong social ties with LGBTQ+ people (Tadlock et al., 2017). Harrison and Michelson (2019) identify consistent evidence contact with LGBTQ+ group members motivates prosociality toward different LGBTQ+ community segments. Given individuals living in areas with more LGBTQ+ people may be dispositionally favorable toward the LGBTQ+ com-
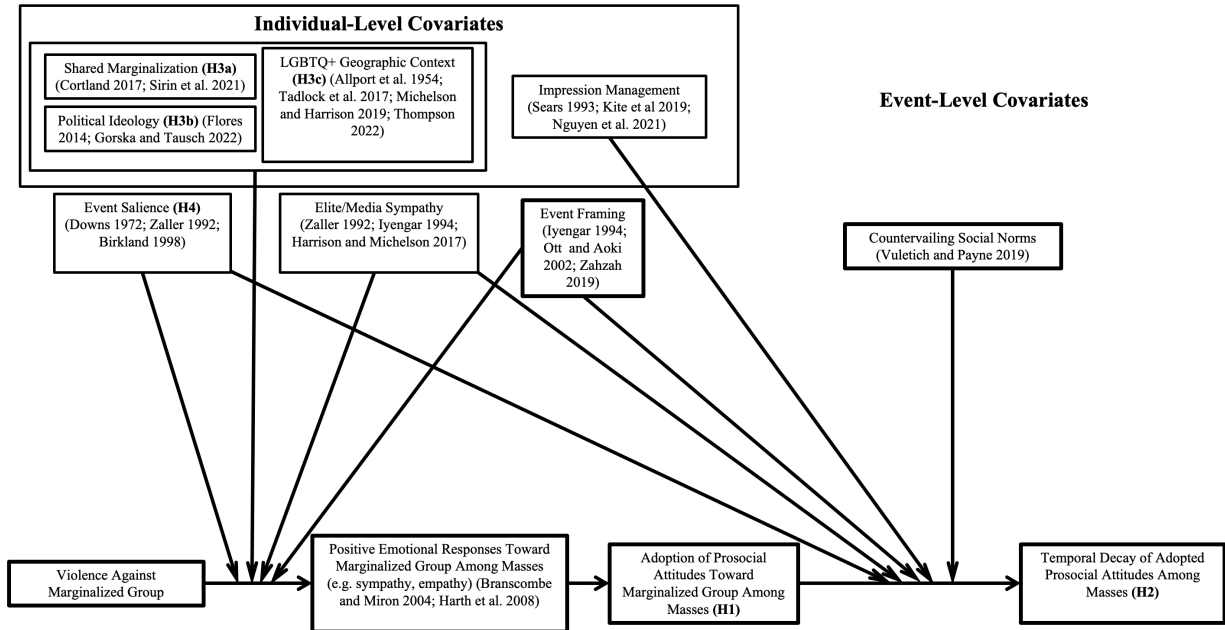
Figure 2: Fickle Prosocial Violence Response Model

munity (Thompson, 2022), they may be more inclined to adopt prosocial attitudes toward LGBTQ+ community segments after high-profile civilian violence against LGBTQ+ group members. Indeed, prior research shows individuals living in LGBTQ+ geographic contexts resist anti-LGBTQ+ elite rhetoric (Górska and Tausch, 2022).

In summary, **H3a-c:** indirect exposure to civilian violence against LGBTQ+ group members will be more likely to motivate prosocial attitudes toward LGBTQ+ group members among: **a)** non-whites and women relative to whites and men; **b)** liberals relative to moderates and conservatives; **c)** individuals living in geographic contexts with more LGBTQ+ people relative to those living in contexts with less LGBTQ+ people.

3.2 Event-Level Salience Heterogeneity

The *FPVR* model implies violent events must be sufficiently *salient* (i.e. covered by media and paid attention to by the public), to generate attitudinal shifts toward targeted groups (Downs, 1972; Zaller, 1992; Birkland, 1998). Indeed, prior studies demonstrating mass at-

titudinal shifts after US violent events are analyzing high-profile events (Tuch and Weitzer, 1997; Sigelman et al., 1997; Birkland and Lawrence, 2009; Reny and Newman, 2021). Moreover, prior research informing the $FPVR$ model's assumptions suggests attitudinal shifts decay with reduced salience (Tuch and Weitzer, 1997; Birkland and Lawrence, 2009; Chudy and Jefferson, 2021; Nguyen et al., 2021). Importantly, salience *is not binary.* Violent Event A may be more salient than Violent Event B, but less salient than Violent Event C, such that Event A does not sufficiently influence mass attitudes like Event C does. Thus, we may expect to observe an empirical pattern consistent with the dashed line on Figure 1, Panel C.

H4: Initially more salient instances of civilian violence against LGBTQ+ group members will be more likely to motivate prosocial attitudes toward LGBTQ+ people than initially less salient instances of civilian violence against LGBTQ+ group members.

4   Event 1: The Pulse Massacre

Studies 1-2 evaluate the consequences of the Pulse massacre. The massacre occurred on June 12, 2016 at the Pulse LGBTQ+ nightclub in Orlando, Florida. The massacre was perpetrated by Omar Mateen, an ISIS allegiant. Mateen killed 49 and injured 53 clubgoers with a semi-automatic rifle.[8] After taking hostages, Mateen was killed by the police. During the massacre, Pulse was hosting "Latin Night." 80% of victims were Latinx.[9]

The nation reacted sympathetically post-massacre. Republican Florida Governor Rick Scott expressed support for those affected while instituting a state of emergency. The Obama administration expressed condolences and ordered federal assistance to the police investigation and the community. In a press conference, Obama described the massacre as an "act of hate." Many on social media, including 2016 presidential election candidates, congresspeople, political figures, foreign leaders, and celebrities expressed condolences.

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[8]https://www.cnn.com/2016/06/12/us/orlando-shooter-omar-mateen/index.html

[9]The massacre's victims spanned the LGBTQ+ spectrum, but gay men may have been centered in the media post-massacre (Ramirez et al., 2018). Although this might mean the massacre was not interpreted as violence against a broader LGBTQ+ community, this is not a shortcoming with our analysis, but with how society interprets the massacre.

The massacre was salient. 90% of adults indicated they were closely following the incident immediately post-massacre (Figure A1). A survey during the massacre (June 10-26) suggests the public was aware of the shooting since it expressed more concerns about terrorism and gun violence post-massacre (Figure A4). Media coverage of topics related to Pulse, LGBTQ issues, and terrorism discontinuously increased post-massacre (Figure A2). Google searches related to Pulse, LGBTQ issues, and terrorism peak when the massacre occurs (Figure A3). Media coverage and Google searches related to these topics were either declining or limited pre-massacre, suggesting anticipatory effects do not drive attitudinal shifts toward LGBTQ+ issues or people post-massacre. However, coverage and searches decline to their pre-incident levels by July, implying fleeting salience.

The massacre was not simply interpreted as a terror attack, but targeted, illegitimate, anti-LGBTQ+ violence.[10] 70-85% of adults believed the shooting was a hate crime (Figure A5).[11]

Therefore, consistent with the *FPVR* model, the mass public may respond prosocially to the perceptibly illegitimate Pulse massacre given the event's salience and concomitant sympathetic response from both the media and elites. But, given reduced media coverage and attention to the event over time, attitudinal responses may be short-lived.

4.1   Study 1: TAPS

4.1.1   Data and Design

Study 1 uses The American Panel Survey (TAPS, Wave 55), to assess if exposure to violence against LGBTQ+ people motivates support for policies benefiting LGBTQ+ community segments. TAPS is a monthly online survey administered by the Weidenbaum Center, with

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[10]Omar Mateen was not explicitly motivated by anti-LGBTQ attitudes. Mateen randomly targeted nightclubs to inflict mass casualties (see: `https://www.nbcnews.com/feature/nbc-out/what-really-happened-night-pulse-n882571`). However, the mass public *perceived* the massacre as an anti-LGBTQ+ hate crime regardless of Mateen's motive (Figure A5).

[11]See Online Dataverse Supplementary Material (DSM) Sections 1.2 and 1.6 for details on Figure A5 data.

national probability sampling conducted by GfK/Knowledge Networks.

The outcome is same-sex marriage support (*SSM support*). SSM is an important LGBTQ+ rights dimension and it implicates multiple LGBTQ+ community segments. Gay, lesbian, and bisexual people who want to marry a same-sex partner benefit from legalized SSM. Transgender people who have not changed their "legal" gender but seek to marry their partner in heterosexual romantic relationships, in addition to transgender people in same-gender relationships, would benefit from legalized SSM.[12] SSM approval is near-unanimous among LGBTQ+ people. 60% of LGBTQ+ people say SSM should be a priority even if it takes attention from other issues.[13] TAPS asks respondents if they "generally support or oppose same-sex marriage," with an option to indicate "no opinion."[14] We measure *SSM support* as an indicator equal to 1 if the respondent indicates they support SSM and 0 otherwise.

The independent variable is being interviewed after the Pulse massacre (*post-Pulse*). TAPS was fielded between 06/08/2016-07/08/2016. Pulse occurs on 06/12/2016, so we implement an unexpected-event-during-survey-design (UESD) with TAPS comparing *SSM support* for respondents interviewed pre- and post-Pulse (Muñoz et al., 2020). *Post-Pulse* is a binary indicator equal to 1 if a respondent is interviewed after 06/12/2016. Since we cannot be certain respondents perceived the massacre, the *post-Pulse* coefficient is an "intent-to-treat" (ITT) effect. However, Figures A1-A4 suggest the public was attentive to the massacre. Moreover, TAPS respondents are more likely to believe ISIS is an important issue *post-Pulse* (Figure B6), suggesting they "received the treatment" since the massacre's perpetrator pledged fealty to ISIS. If **H1** is supported, the *post-Pulse* coefficient would be *positive*.

In the absence of internal attention checks, we truncate our sample to those who completed the survey in a "reasonable duration" to account for online survey respondent inattentiveness, which may produce low quality responses attenuating associations of interest.

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[12]https://transequality.org/issues/resources/marriage-equality-and-transgender-people

[13]https://www.pewresearch.org/social-trends/2013/06/13/a-survey-of-lgbt-americans/

[14]See DSM Section 2.1 for outcome measurement details.

See DSM Section 2.4.1 for more details and evidence truncation does not affect our results or TAPS' representativeness. After truncation, TAPS contains $N = 1142$ respondents, 677 (59%) interviewed before Pulse and 465 after (41%).

We demonstrate the *post-Pulse* coefficient is insulated from bias by validating UESD identification assumptions. The first assumption is ignorability. "Treatment" should be independent of potential outcomes conditional on random sampling. Thus, respondents interviewed pre and *post-Pulse* should be compositionally similar. Figure 3, Panel A supports the assumption. Respondents interviewed *post-Pulse* are compositionally similar to respondents interviewed pre-Pulse across 20 baseline covariates except age (see DSM Section 2.2 for baseline covariate measurement), a finding consistent with multiple testing.

Excludability is another UESD identification assumption: differences between respondents interviewed pre- and *post-Pulse* should be the sole consequence of the massacre. The "treatment" is not just the massacre, but collateral media attention. However, outside the massacre, there are no punctuated moments of media attention over LGBTQ+ issues or violence against LGBTQ+ people during the month TAPS was fielded (June, Figures A2 and A3), suggesting the absence of simultaneous events motivating pro-LGBTQ+ attitudes.

Additionally, it is unlikely preexisting *SSM support* time trends are driving the result. We subset TAPS to the pre-Pulse period and assess the placebo "effect" of being interviewed after the median pre-treatment date and find null results (DSM Table 89).

4.1.2   Results

Consistent with **H1**, respondents interviewed *post-Pulse* are 12 and 9 percentage points more likely to support SSM without and with covariate adjustment ($p < 0.05$, Figure 3, Panel B). These coefficients are 20-24% of the outcome standard deviation.

Our results are robust. Our findings are likely not driven by secular dynamics outside the massacre. Falsification tests on treatment-irrelevant outcomes such as support for increasing taxes, common core, a citizenship pathway, abortion, the Keystone pipeline, ACA repeal and
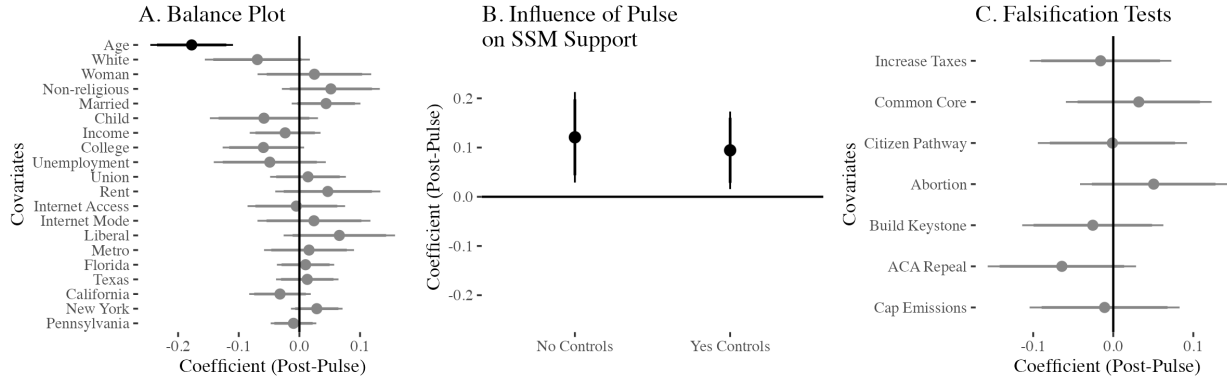
14

Figure 3: *SSM Support* Increases *post-Pulse*. Panel A displays respondent covariate balance pre- and *post-Pulse*. Panel B characterizes the *post-Pulse* effect on *SSM support* with and without covariate adjustment. Panel C displays falsification tests characterizing the unadjusted *post-Pulse* effect on LGBTQ+-irrelevant outcomes. Black coefficients are statistically significant, grey otherwise. Estimates use population weights. All covariates scaled between 0-1. 95% CIs displayed from HC2 robust SEs. See DSM Tables 91, 92, and 93 for regression tables characterizing the coefficients.

emission caps are null (Figure 3, Panel C). These tests suggest chance age imbalance does not implicate balance on policy preferences.[15] Given the close association between socially conservative religious beliefs like abortion restrictionism and SSM opposition (Uecker and Froese, 2019), the null effect of *post-Pulse* on abortion support on Figure 3 Panel C suggests our results are not driven by secular shifts in social conservatism or religiosity.[16] The results are not driven by outcome item non-response since non-response is balanced pre- and post-Pulse (DSM Table 88). The results are not driven by seasonal trends, Pulse's influence is unique to 2016. 3 surveys fielded in June 2012, 2013, and 2017 show the influence of being interviewed after the massacre's calendar day on *SSM support* is null (Figure B7), suggesting no secular dynamics intrinsic to the month of June that could explain our findings (e.g. Pride Month). Our findings are robust to smaller bandwidths less susceptible to secular temporal trends (Figure B8). Finally, given we are deriving intent-to-treat coefficients, we

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[15]Age imbalance may not induce bias. Age is unrelated to *SSM support* in TAPS, so it does not explain *joint* treatment and outcome variation (DSM Table 92).

[16]SSM and abortion support are only moderately correlated ($\rho = 0.52$), suggesting *SSM support* is explained by other factors, like the Pulse massacre, independent of dispositional religiosity or social conservatism. Religiosity is constant pre- and *post-Pulse* (Figure 3, Panel A), further suggesting religiosity does not drive our results.
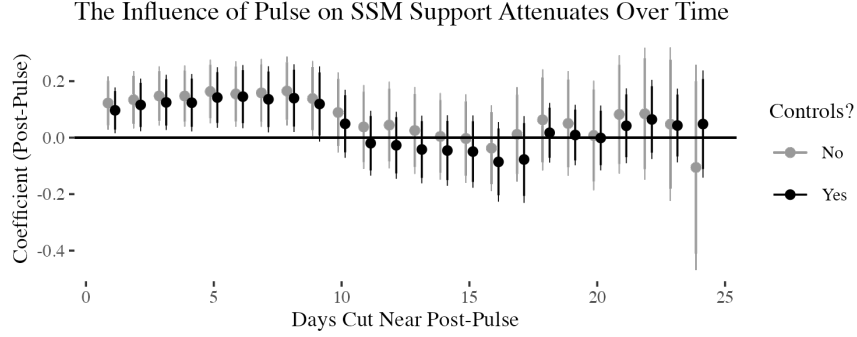
The Influence of Pulse on SSM Support Attenuates Over Time

Figure 4: The Influence of Pulse on SSM Support Attenuates Over Time. X-axis is days cut from moment of Pulse massacre after the massacre (with days after intact). Y-axis is the *post-Pulse* coefficient. 95% CIs from robust SEs. See DSM Table 94 for regression table characterizing reported coefficients in this figure. See DSM Section 2.10.5 for control covariate coefficients.

test if *post-Pulse* is heterogeneous by political interest or news consumption. We do not find heterogeneity (Section B.5). This is not concerning since 90% of the public was following the shooting (Figure A1), suggesting high treatment reception regardless of dispositional political or media interest.

4.1.3 Temporal Persistence

We test **H2** by assessing if the influence of Pulse on *SSM support* is temporally durable. We remove observations in the days immediately *post-Pulse* but not after those days, and re-analyze the influence of being surveyed *post-Pulse*. The logic is that respondents interviewed immediately *post-Pulse* may be the most susceptible to shifting attitudes toward LGBTQ+ community segments. Removing them may help us evaluate attitudinal decay by comparing respondents interviewed just before and some days after Pulse. After removing respondents interviewed between 1-10 days *post-Pulse,* the influence of being interviewed *post-Pulse* on *SSM support* is null (Figure 4).[17] Therefore, temporal attenuation is quick relative to prior studies demonstrating attitudinal shifts lasting several months to a year (Broockman and Kalla, 2016; Oskooii et al., 2021). Consistent with **H2**, the initial *SSM support* increase

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[17]1/20 covariates are imbalanced after cutting 2, 4, 6, 14, 16, 21, 22 days *post-Pulse* (DSM Table 65), suggesting Figure 4's results are not driven by imbalance.

post-Pulse was not durable.

4.1.4   Individual-Level Heterogeneity

We test **H3a-c** by assessing if the *post-Pulse* coefficient is larger among: a) non-whites relative to whites and women relative to men; b) liberals relative to moderates and conservatives; and c) individuals living in states with a higher proportion of LGBT-identifying people and counties with a higher density of same-sex couples relative to individuals who live in areas with less LGBT-identifying people and same-sex couples.[18] Inconsistent with **H3a-c**, *post-Pulse* does not appear heterogeneous by marginalized group membership, liberalism, and LGBTQ+ geographic context (Table B1). These findings suggest the massacre had a *largely homogeneous initial influence on mass attitudes.*

4.2   Study 2: PI S-IAT Data

4.2.1   Data and Design

Study 2 examines if the public adopts positive attitudes toward LGBTQ+ community segments *post-Pulse.* We use Project Implicit (PI) data on US respondents self-selecting into and completing an internet survey in 2016 asking questions on their explicit and implicit attitudes toward gay people via PI's Sexuality Implicit Association Test (S-IAT, $N = 43,950$).[19] On average, 175 U.S. respondents completed the PI S-IAT survey daily during 2016.[20] For information on S-IAT sample composition and representativeness, see DSM Section 3.1.

The outcomes are the S-IAT *D-score*, *straight bias*, and *heterocentrism.* The S-IAT calcu-

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[18]We use 2016 Gallup data to identify the proportion of each state's population identifying as "lesbian, gay, bisexual or transgender." (see: `https://news.gallup.com/poll/201731/lgbt-identification-rises.aspx`) We use 2010 Census data to identify same-sex couple density (the number of same-sex couple households per 1000 households in a county, see: `https://williamsinstitute.law.ucla.edu/visualization/lgbt-stats/`). We merge these state and county-level covariates to the TAPS data by using respondent zipcode information.

[19]Data available here: `https://osf.io/yjqmw/`. See `https://implicit.harvard.edu/implicit/education.html` for Project Implicit information.

[20]We exclude respondents interviewed after 09/08/2016 due to order effects since the S-IAT measurement changes from 188 to 200 trials by cutting a task block at that moment.

lates normalized averages of how quickly respondents associate negative/positive attributes to gay/straight people relative to negative/positive attributes to straight/gay people in the form of a *D-score*. The *D-score* ranges from -2-2. Higher values suggest implicit bias against gay people (i.e. associating negative attributes to gay people) (Greenwald and Lai, 2020).[21]

Given indirect measurement, the *D-score* may be less influenced by impression management to be perceived as pro-gay post-massacre (Greenwald and Lai, 2020). Therefore, we can assess relatively quick, negative, emotional responses (i.e. System 2 responses) to gay people in addition to more deliberate evaluations of gay people (i.e. System 1 responses) (Greenwald and Lai, 2020). Although the IAT is not insulated from introspection, the modest correlation between the *D-score* and explicit bias suggests the IAT measures attitudes that are difficult to manipulate. Therefore, the *D-score* is valuable since we can demonstrate even temporary prosocial attitudinal shifts may not be impression management. The *D-score* is well-established and associated with objective covariates characterizing subordination (Ratliff and Smith, 2021).

Heterocentrism and *straight bias* are explicit anti-gay bias measures. *Heterocentrism* is the difference in 10-point feeling thermometers for straight and gay men. *Straight bias* is a 7 point measure from "I strongly prefer gay to straight people" to "I strongly prefer straight to gay people." The *D-score*, *straight bias*, and *heterocentrism* are rescaled between 0-1.

Although *heterocentrism* is explicitly about gay men, and *straight bias* is implicitly about gay men, the *D-score* captures attitudes toward gay men and lesbians. In effect, the *D-score* implicates gay men, lesbians, and bisexuals (and transgender people in same-gender relationships). Moreover, even if our Study 2 outcomes are limited when it comes to measuring attitudes toward the broader LGBTQ+ community (e.g. transgender people), attitudes toward gay people are correlated with attitudes toward transgender people (Norton and Herek, 2013), which may be pronounced given the massacre affected transgender people.[22]

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[21]See DSM Section 3.3 for more *D-score* measurement details.

[22]https://www.advocate.com/crime/2016/6/17/pulse-survivor-stop-being-shady-and-messy-just-love-one-another-video

Therefore, our Study 2 outcomes implicate large LGBTQ+ community segments. Given the outcomes characterize negative attitudes, if **H1** is supported, *post-Pulse* should be *negative*.

We use a UESD with the S-IAT to evaluate how anti-gay attitudes shifted *post-Pulse*. Given the large number of individuals taking the S-IAT daily, we estimate the influence of taking the S-IAT *post-Pulse* using respondents taking the S-IAT 5-50 days pre- and post-massacre in addition to the full 2016 sample between January-September.

We validate the UESD ignorability identification assumption. Unlike Study 1, respondents are not sampled, but self-select, into the S-IAT. Therefore, sample composition may shift due to external events or secular trends. We expect respondents surveyed shortly pre- and post-massacre will be compositionally similar. However, respondents may be increasingly dissimilar in samples including respondents taking the survey well before or after the massacre. Figure C10 verifies our expectation. For 5-20 day bandwidth samples (Panels A-D), there is statistical imbalance on respondent characteristics pre- and post-Pulse on 1-2/12 baseline covariates. For 25-50 day bandwidth samples, there is imbalance on 3-7 covariates (Panels E-J). Given the 15 and 20-day bandwidth samples are only imbalanced on race, we prioritize interpreting the influence of *post-Pulse* on anti-gay attitudes using these samples. These findings suggest our coefficient estimates, particularly for the 15 and 20-day bandwidth samples, are relatively insulated from omitted variable bias.[23]

4.2.2   Results

Figure 5 displays *post-Pulse* ITT coefficients where the outcome is the *D-score*, *straight bias*, and *heterocentrism*. In the 15 and 20-day sample bandwidth estimates, respondents surveyed *post-Pulse* have a lower *D-score* (-0.01, $p < 0.10$) and *heterocentrism* (-0.01, $p < 0.01$), equivalent to 7% and 8% of the respective outcome standard deviations pre-Pulse. Although small, these coefficients are reasonable, likely underestimated, and substantively important vis-a-vis the target population (see DSM Section 3.4).

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[23]Importantly, like Study 1, religiosity is constant pre- and *post-Pulse*, suggesting socially conservative trends are not driving our results.
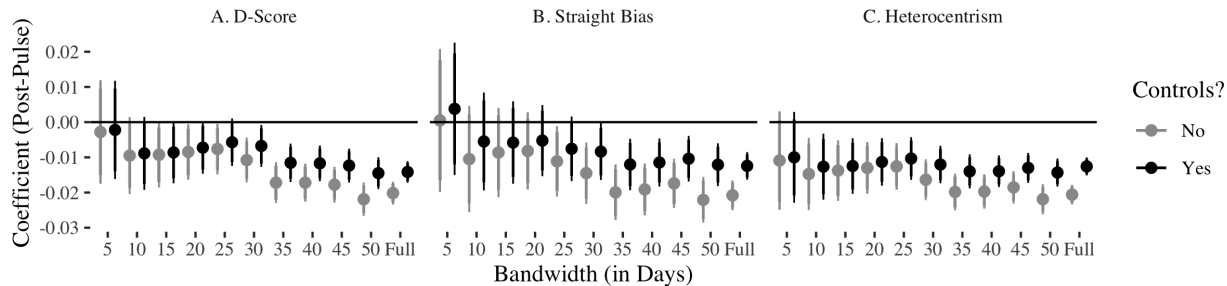
Figure 5: Influence of *post-Pulse* on Anti-Gay Attitudes. The x-axis is the sample bandwidth. The y-axis is the *post-Pulse* coefficient. All covariates rescaled between 0-1. 95% CIs displayed from robust SEs. See DSM Sections 3.5.2 and 3.5.3 for corresponding regression tables.

The massacre does not appear to statistically reduce *straight bias* except in sample bandwidths with higher covariate imbalance (e.g. 25-50 days). Given *straight bias* is highly explicit, the absence of a reliable shift in *straight bias post-Pulse* may be a function of impression management on part of respondents disposed against LGBTQ+ whose attitudes may otherwise shift in favor of LGBTQ+ through indirect bias measurement (Greenwald, McGhee, et al., 1998). In sum, we find additional support for **H1** in Study 2.

We conduct several robustness checks. Preexisting time trends are not driving our results (Section C.3). We rule out if systematic temporal trends near June motivate prosocial attitudes toward gay people other than the massacre (Section C.4). We rule out if our findings are due to a secular attitudinal trend in favor of marginalized groups (Section C.5). We also rule out if respondent self-selection generates sorting bias (Section C.7).

4.2.3 Temporal Persistence

We assess if the *D-score* and *heterocentrism* decrease is sustainable. Consistent with **H2**, descriptive statistics suggest anti-gay attitudes decreased *post-Pulse*, but rebounded to pre-Pulse levels around August (Figure C9). We conduct a formal test of the sustainability of attitudinal shifts *post-Pulse* and compare S-IAT respondents surveyed 15 days pre-Pulse to those surveyed 15 days after 1-72 days *post-Pulse* (leaving at least 15 days up to the end of the post-treatment sample in the 2016 S-IAT data). This exercise allows us to compare
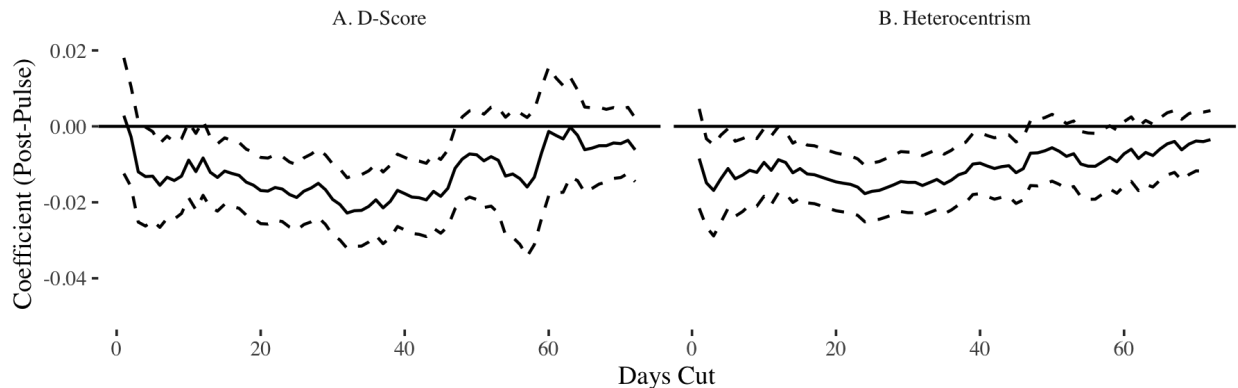
A. D-Score   B. Heterocentrism

Figure 6: The Influence of *Post-Pulse* on Reducing Anti-Gay Attitudes Attenuates Over Time. X-axis is days cut from moment of Pulse massacre after the massacre (with 15 days after kept intact). Y-axis is the *post-Pulse* coefficient. All estimates from models adjusting for controls. 95% CIs from robust SEs. See DSM Tables 123-130 for tables characterizing the displayed coefficients. See DSM Sections 3.5.8 and 3.5.9 for control coefficients.

individuals surveyed prior to Pulse to those surveyed some time away from Pulse at multiple time intervals. Respondents in time intervals that cut more days *post-Pulse* are temporally further from the massacre and potentially more subject to attitudinal decay in pro-gay beliefs. Figure 6 demonstrates the *D-score* and *heterocentrism* decrease was sustained up to 40 days *post-Pulse*. However, after 40 days, *post-Pulse* attenuates toward 0.[24] Although attitudinal shifts last 40 days, these shifts are still much shorter than prior studies demonstrating long-term attitudinal shifts toward marginalized groups after external stimuli (Broockman and Kalla, 2016; Oskooii et al., 2021). Consistent with **H2**, Study 2 suggests the massacre motivated prosocial beliefs, but not durably.

4.2.4   Individual-Level Heterogeneity

We test **H3a-c** by assessing if the *post-Pulse* coefficient is larger among non-whites, women, liberals, and individuals living in geographic contexts with more LGBTQ+ people.[25] In-

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[24]After cutting 40 days *post-Pulse*, there is covariate imbalance, but this does not invalidate Figure 6. After covariate adjustment, the *post-Pulse* coefficients attenuate toward zero, suggesting temporal attenuation occurred *earlier* than our results suggest (DSM Section 3.6).

[25]Geographic context is measured like Study 1. We use respondent county data in the S-IAT to merge in information on LGBTQ+ geographic context.

consistent with **H3a-c**, we find the massacre's influence is homogeneous. *Post-pulse* is not stronger for non-whites, women, liberals, or respondents in geographic contexts with more LGBTQ+ people (Tables C4-C5).

4.3   Mitigating Bundled Treatment Concerns

It is unclear if respondents adopted prosocial beliefs toward LGBTQ+ community segments because the Pulse massacre was a terror attack or attack against Latinxs instead of perceptibly anti-LGBTQ+ violence. We mitigate these concerns with several tests and evidence outlined in detail in DSM Section 1.8. We summarize these tests and evidence here. First, other terror attacks do not motivate pro-LGBTQ+ beliefs and Pulse did not motivate antipathy toward groups stereotypically associated with radical Islamic organizations, mitigating concerns our results are driven by the massacre being a terror attack. Second, other attacks against Latinxs do not motivate pro-LGBTQ+ beliefs and Pulse did not motivate positive attitudes toward Latinxs, mitigating concerns our results are driven by the massacre being violence against Latinxs. Third, we show the public was disproportionately attentive to LGBT topics *post-Pulse* relative to terrorism- and/or Latino-related topics, implying the public primarily perceived the event as anti-LGBTQ+ violence.

Our tests do not entirely mitigate the bundled treatment problem. Our results may be due to the *combination* of circumstances associated with Pulse. Therefore, we conceptually replicate Studies 1-2 by assessing the influence of instances of violence against LGBTQ+ group member(s) in Studies 3-4 that were not terror attacks nor attacks against non-whites.

5   Event 2: Matthew Shepard's Murder

Studies 1-2 may not be externally valid. The Pulse massacre is a unique instance of violence against LGBTQ+ people. It is the deadliest instance of violence against LGBTQ+, is the second deadliest mass shooting, has predominantly Latinx victims, was ISIS-inspired
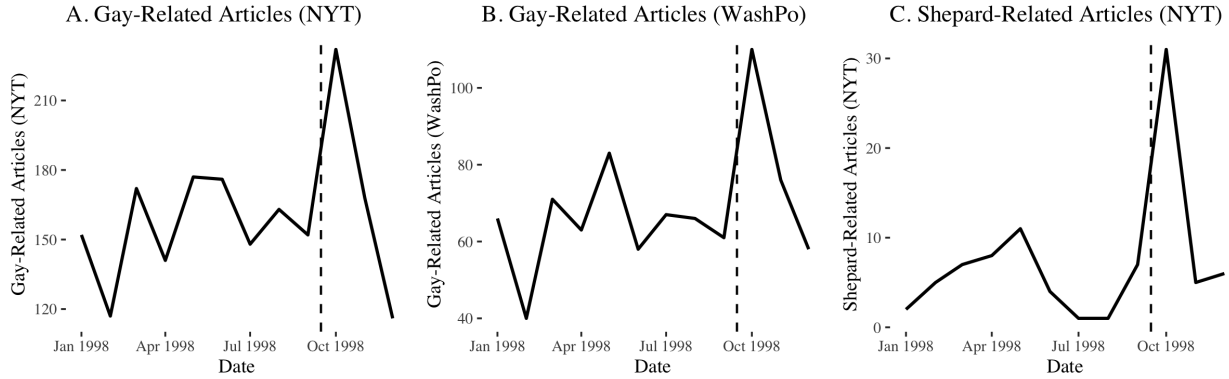
A. Gay-Related Articles (NYT) B. Gay-Related Articles (WashPo) C. Shepard-Related Articles (NYT)

Figure 7: Media Coverage of Gay-Related Content in 1998. Panels A/B display the number of NYT/Washington Post gay-related articles (y-axis) by month (x-axis). Panel C displays the number of gay-related articles related to Shepard or anti-gay violence by month. Dashed vertical line denotes the period Shepard is murdered.

terrorism, and occurred after seminal gay rights victories (e.g. same-sex marriage). Therefore, it may be prudent to assess if a distinct instance of violence against LGBTQ+ group member(s) also motivates prosocial beliefs. Consequently, we examine how the murder of Matthew Shepard, a white gay Wyoming college student, by two white men, influenced beliefs toward homosexuality during a more homophobic temporal context.

On October 6, 1998, Shepard was brutally beaten by Aaron McKinney and Russell Henderson. The incident was heavily covered by national media (Loffreda, 2001). Shepard died six days later on October 12. The murder was salient and the nation reacted sympathetically. A bipartisan group of Congresspeople condemned the murder and expressed condolences. A vigil was held outside the US capitol on October 15, where thousands of people, including current and former Congresspeople and celebrities, paid respects to Shepard. Advocates note Shepard's murder engendered a "seismic shift in attitudes towards the LGBTQ community."[26] Indeed, a decade later, Congress passed the Matthew Shepard and James Byrd Hate Crimes Prevention Act, which expanded the power to prosecute sexuality hate crimes.

On the month of Shepard's murder, the number of gay-related news articles was 150%

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[26]https://www.nbcnews.com/feature/nbc-out/two-decades-after-matthew-shepard-s-death-lgbtq-community-still-n919401

(NYT) and 172% (WashPo) of the Jan-Sep 1998 average (Figure 7).[27] Consistent with the *FPVR* model, media attention to Shepard's murder was immediately intense but quickly declined, suggesting attitudinal responses may be short-lived.

5.1   Study 3

5.1.1   Data and Design

To evaluate if Shepard's murder decreased anti-gay attitudes, we identify surveys with similar items characterizing attitudes toward gay people shortly before and after Shepard's murder.[28] We identify two representative CNN telephone polls asking respondents if they believe homosexuality is "morally wrong" (*moral wrong*) 4 months before and 2 days after Shepard's death (CNN Jun. 1998, $N = 1016$; CNN Oct. 1998, $N = 1036$).[29] We stack these datasets and identify overlapping controls from each survey.[30] We then compare respondents interviewed after Shepard's murder (*post-Shepard*) to those before to assess if anti-gay violence exposure decreased the belief homosexuality is morally wrong, consistent with **H1**. We focus on surveys with the *moral wrong* outcome for 3 reasons. First, it is asked on three surveys after Shepard's murder (in 1998, 2001, 2004), allowing an assessment of long-term attitudinal shifts. Second, there are multiple pre-Shepard surveys with the same item, allowing placebo tests to rule out if *post-Shepard* effects are due to secular progressive attitudinal trends concerning homosexuality's morality. Third, *moral wrong* implicates large LGBTQ+ community segments. Lesbian, gay, bisexual, and transgender people may all partake in "homosexual" behavior. Given the outcome characterizes a negative attitude toward LGBTQ+ community segments, the *post-Shepard* coefficient would be *negative* if **H1** is supported.

Our approach has shortcomings we assuage. First, given the absence of auxiliary data

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[27]See DSM Section 4.1 for details on media data.

[28]We use the search terms "homosexuality" OR "homosexual" OR "gay" in Roper iPoll between 1996-2000 to identify gay-related items around Shepard's murder.

[29]We found two other items that could serve as potential candidates for assessing the influence of Shepard's murder on LGBTQ+ attitudes. We do not use them for various reasons that we outline in Section D.1.

[30]See DSM Section 4.2 for more sampling methodology details.

on attention to the murder, we cannot be certain respondents "received the treatment." Therefore, we interpret *post-Shepard* as an ITT effect. However, Figure 7 suggests the murder received significant media attention such that it might shift mass attitudes.

Second, given possible differences in sampling between the two surveys, our statistical conclusions may be due to sample composition. Balance tests between the two surveys demonstrate limited baseline covariate imbalance (Figure 8, Panel A), suggesting sample composition may not drive our results.

Third, unlike Studies 1-2, we cannot assess an immediate effect of anti-gay violence exposure even though the two surveys were fielded near Shepard's murder. There are four months between the surveys with the *moral wrong* outcome (Jun.-Oct. 1998). Therefore, our *post-Shepard* estimates may be due to intervening factors or secular progressive time trends. However, there is no anti-gay violence with the level of media coverage Shepard's murder garnered in between the field periods (Figure 7). Crowdsourced evidence suggests the last prominent instance of anti-LGBTQ+ violence prior to Shepard's murder was not between June-October 1998, but on February 1997 (the Otherside Lounge Bombing).[31] Indeed, between June-September 1998, there were *zero* New York Times articles related to anti-gay hate crimes. Conversely, on the month of Shepard's murder (October 1998), there were 17 NYT articles related to anti-gay hate crimes (Figure D15). Two other intervening factors in 1998 may explain our results: 1) President Clinton signing an executive order against sexual orientation discrimination and 2) Tammy Baldwin's House election (the first lesbian congressperson). We provide evidence these events are unlikely explaining our *post-Shepard* coefficient estimates (Section D.3).

Moreover, we rule out if our results are due to secular outcome time trends by conducting a temporal placebo test and demonstrating *moral wrong* levels do not change between Apr. 1997-Jun. 1998 (Figure 8, Panel B).[32] These results suggest prominent pre-study events, such as Ellen DeGeneres' televised coming out in April 1997, are not driving our results.

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[31]https://en.wikipedia.org/wiki/History_of_violence_against_LGBT_people_in_the_United_States
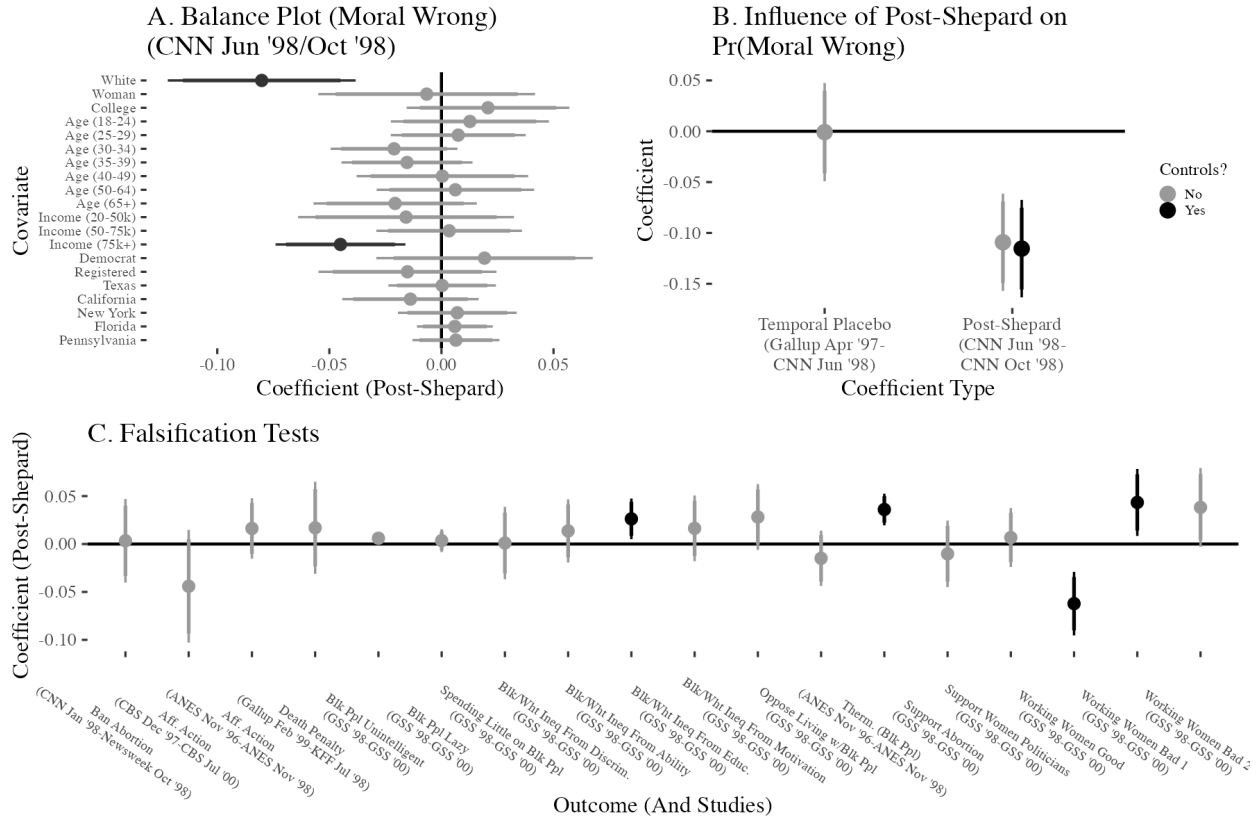[32]See DSM Section 4.2 for more temporal placebo test details.

Figure 8: Respondents Interviewed *Post-Shepard* Were Less Likely To Believe Homosexuality is Morally Wrong. Panel A characterizes balance between respondents interviewed pre- and post-Shepard's murder. Black coefficients are significant, grey otherwise. Panel B characterizes a) the influence of being interviewed on June 1998 relative to April 1997 on the belief homosexuality is morally wrong (temporal placebo) and b) the influence of being interviewed *post-Shepard* on *moral wrong*. Panel C characterizes falsification tests assessing the influence of *post-Shepard* on non-LGBTQ+ group attitudes. 95% CIs displayed from robust SEs. See DSM Tables 176-179 for regression tables on balance tests, the temporal placebo, the *post-Shepard* coefficient estimates, and falsification tests.

Despite Study 3's shortcomings, we believe the design provides sufficient complementary evidence to Studies 1–2 along with suggestive evidence our theory generalizes beyond Pulse.

5.1.2 Results

Consistent with **H1**, Figure 8, Panel B shows respondents interviewed *post-Shepard* were 12 percentage points less likely to report homosexuality is morally wrong with or without covariate adjustment, 24% of the outcome standard deviation ($p < 0.001$).

We conduct falsification tests on outcomes related to non-LGBTQ+ marginalized groups to rule out secular supportive trends toward marginalized groups driving our results (Figure 8, Panel C).[33] Only 4/18 outcomes are significant and the *post-Shepard* coefficient is not consistently in support of non-LGBTQ+ groups, suggesting no systematic secular trend driving our results (see DSM Section 4.6 for more details). Like Study 1, the null effects of *post-Shepard* on abortion support suggest our results are not driven by secular shifts in social conservatism and/or religiosity.

5.1.3 Temporal Persistence

To assess the persistence of attitudinal shifts *post-Shepard*, we identify 6 surveys between 1978-2004 where the *moral wrong* item was asked,[34] allowing us to evaluate trends in the public's belief homosexuality is morally wrong pre- and *post-Shepard*. We do not use the CNN June 1998 poll on Figure 9 in our assessment of temporal persistence (see DSM Section 4.5 for details as to why).

Figure 9 displays event study estimates comparing *moral wrong* levels in 5 surveys between 1978-2004 to a survey fielded prior to Shepard's murder in 1994. From 1978-1994, belief in *moral wrong* is remarkably stable. Respondents surveyed in 1994 are not statistically distinct from respondents surveyed in 1992 or 1978. Consistent with our initial temporal placebo test, these findings suggest an absence of progressive attitudinal trends toward gay people prior to Shepard's murder. However, in October 1998, immediately after Shepard's murder, there is a statistically distinguishable decrease in *moral wrong*. But, the mass public's beliefs in *moral wrong* reverse to pre-murder levels by 2001 and 2004. Consistent with **H2**, our results suggest Shepard's murder motivated a decrease in negative beliefs concerning "homosexuals," but this decrease was not sustainable.

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[33]See DSM Section 4.7 for more falsification test outcome details.
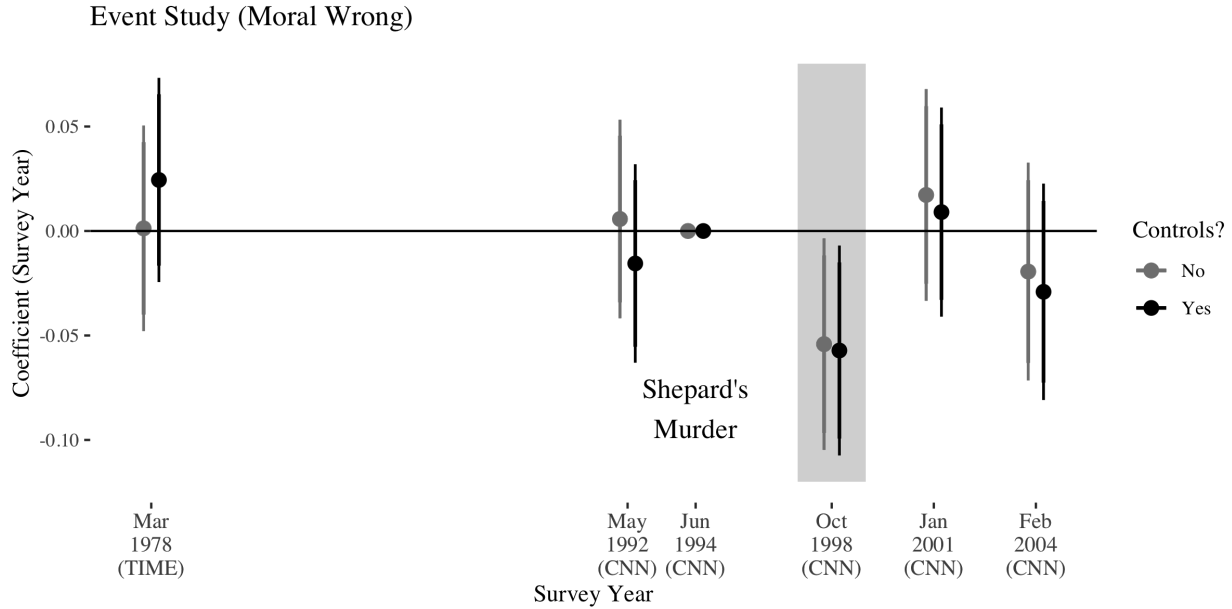[34]See DSM Section 4.4 for details on the 6 surveys.

Figure 9: Belief in *Moral Wrong* is Stable Between 1978-2004 With the Exception of the Moment Shepard Was Murdered. Reference study is the 1994 CNN poll. Color denotes the inclusion/exclusion of controls (age, education, gender, partisanship, race). Shaded estimate denotes Shepard's murder (Oct. 1998). All estimates use survey weights. All covariates scaled between 0-1. See DSM Table 183 for a corresponding regression table. 95% CIs displayed derived from robust SEs.

5.1.4 Individual-Level Heterogeneity

We test **H3a-b** by assessing if the *post-Shepard* coefficient is stronger among a) non-whites and women and b) Democrats.[35] Given the absence of a) county-level geographic data in the two 1998 CNN polls and b) state-level LGBT population information in the 1990s, we cannot test **H3c**. We find some evidence consistent with **H3a** (Table D7). Although there is no *post-Shepard* heterogeneity by gender, non-whites are less likely to believe homosexuality is morally wrong relative to whites *post-Shepard*. Whites are 7 percentage points less likely to believe homosexuality is morally wrong *post-Shepard* whereas non-whites are 22 percentage points less likely, 44% of the pre-Shepard outcome standard deviation. Likewise, we find evidence supporting **H3b** (Table D7). The *post-Shepard* effect appears driven by Democrats. Democrats are 22 percentage points less likely to believe homosexuality is morally wrong

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[35]Data on liberalism is unavailable in the 1998 CNN polls, but Democratic partisanship is an appropriate proxy given its' strong association with liberalism.

post-Shepard, whereas non-Democrats are 2 percentage points less likely.

6   Event 3: The Club Q Massacre

Study 4 mitigates two shortcomings with Studies 1-3. First, Studies 1-3 all analyze initially highly salient events (i.e. high media coverage, attention). However, consistent with **H4** and the *FPVR* model, relatively initially less salient violent events may be less likely to motivate prosocial attitudes toward LGBTQ+ community segments. Study 4 allows us to evaluate the consequences of indirect exposure to a putatively high-profile, but relatively initially less salient, instance of violence against LGBTQ+ group members: the 2022 Club Q massacre. Consequently, Study 4 allows us to test **H4** and broader *FPVR* model implications related to initial event salience. Second, the outcomes in Studies 1-3 do not explicitly reference broader LGBTQ+ segments beyond gays and lesbians (e.g. transgender people). Conversely, Study 4 not only examines the same Study 2 outcomes using the 2022 PI S-IAT survey, but additional outcomes characterizing negative attitudes toward transgender people in the 2022 PI Transgender Implicit Association Test (PI T-IAT) survey.[36] Therefore, Study 4 allows us to examine the consequences of violence against LGBTQ+ group members on mass attitudes explicitly related to transgender people, a small, politicized, population (Lewis et al., 2022).

On November 19, 2022, in Colorado Springs, CO, Anderson Aldrich entered an LGBTQ+ nightclub, Club Q, and killed five clubgoers, including two trans people, while injuring 25 others with an AR-15-style rifle.[37] Aldrich was eventually incapacitated by clubgoers and apprehended by police. Evidence suggests the violence was bias-motivated. Aldrich pleaded "no contest" in court to two hate crime charges.[38]

The media and some elites reacted sympathetically to the violence. President Biden and Transportation Secretary Buttigieg immediately expressed condolences.[39] However, un-

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[36]PI started collecting transgender attitude data in 2020 (https://osf.io/fb29q/).

[37]https://www.cnn.com/2022/11/20/us/colorado-springs-shooting-gay-nightclub

[38]https://www.pbs.org/newshour/nation/club-q-shooter-who-killed-5-gets-life-in-prison

[39]https://www.denver7.com/news/local-news/we-are-devastated-officials-react-to-deadly-mass-shooting-at-club-q-in-colorado-springs

like the Pulse massacre and Shepard's murder, the elite response was relatively polarized. Buttigieg blamed the shooting on growing Republican anti-LGBTQ+ rhetoric.[40] Tucker Carlson and several right-wing commentators blamed the violence on purported "grooming" activity from LGBTQ+ people.[41] Republican politicians who expressed condolences were criticized for simultaneously engaging in anti-LGBTQ+ rhetoric.[42] LGBTQ+ advocates noted a rise in queerphobic posts across social media platforms post-shooting.[43]

Moreover, relative to Shepard's murder and the Pulse massacre, the Club Q massacre was less salient. First, there were less NYT articles related to the Club Q massacre two months after the event relative to Shepard's murder and the Pulse massacre (Figure E18). Second, regression discontinuity-in-time estimates suggest that although online articles on topics related to mass shootings, the LGBT community, and hate crimes discontinuously increased after Club Q, there were more online articles on topics related to mass shootings and the LGBT community after Pulse (Figures E19-E20, Table E8). Third, Google search data demonstrates there was more attention to mass shootings, LGBT people, and LGBT hate crimes immediately during Pulse relative to immediately during the Club Q massacre (Figure E21). Therefore, consistent with the $FPVR$ model and **H4**, although Club Q was relatively high-profile, its' lower-profile status vis-a-vis Pulse and Shepard's murder suggests it may be less likely to initially shift mass attitudes.

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[40]https://www.yahoo.com/video/pete-buttigieg-says-political-attacks-145452238.html

[41]https://www.nbcnews.com/tech/internet/right-wing-influencers-media-double-anti-lgbtq-rhetoric-wake-colorado-rcna58371

[42]https://www.durangoherald.com/articles/lauren-boebert-defends-her-past-anti-lgbtq-and-anti-trans-tweets/

[43]See: https://www.isdglobal.org/digital_dispatches/groomer-discourse-intensifies-and-neo-nazis-celebrate-in-wake-of-colorado-springs-attack/ and see: https://apnews.com/article/technology-shootings-business-social-media-colorado-75a3c597a60dca0f116d5deb6a6c1a6b

6.1 Study 4

6.1.1 Data and Design

We use data on U.S. respondents self-selecting into the 2022 PI S-IAT ($N = 184,824$, 506 daily average respondents) and T-IAT ($N = 85,303$, 233 daily average respondents) surveys. See DSM Section 5.1 for information on S-IAT and T-IAT sample composition and representativeness.

The S-IAT outcomes are the same as Study 2's (anti-gay *D-score*, *heterocentrism*, *straight bias*). The three T-IAT outcomes are similar but slightly different. The anti-trans *D-score* is measured by assessing the speed by which respondents associate negative/positive attributes (words) to images of trans/cis celebrities. Higher values suggest respondents associated negative/positive attributes to trans people faster/slower than cis people. *Ciscentrism* measures relative warmth toward cisgender people vis-a-vis trans people. *Cis bias* is a 7-point scale measuring preferences for cisgender relative to trans people. See DSM Section 5.2 for more T-IAT outcome measurement details. Prior research finds the T-IAT outcomes are correlated with anti-trans policy preferences (Axt et al., 2021). All outcomes are rescaled between 0-1.

The main independent variable is *post-Club Q*, an indicator equal to 1 if a respondent self-selects into the S-IAT or T-IAT after November 19, 2022. The *post-Club Q* coefficients will be *negative* if prosocial attitudes increase *post-Club Q*.

We implement another UESD, estimating the influence of *post-Club Q* 5-40 days in 5-day intervals post-massacre.[44] We assess covariate balance for these bandwidth samples between respondents taking the S-IAT/T-IAT pre- and *post-Club Q* (Figures E22-E23).[45] Covariate imbalance increases as sample bandwidth increases, likely due to unobservable secular trends. Therefore, we primarily interpret the 20- and 15-day bandwidth samples in the S-IAT and T-IAT respectively, where there is the least imbalance (4/12 and 1/12 covariates imbalanced respectively).

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[44]There are no data after 40 days *post-Club Q* since the 2022 surveys end on December 2022.
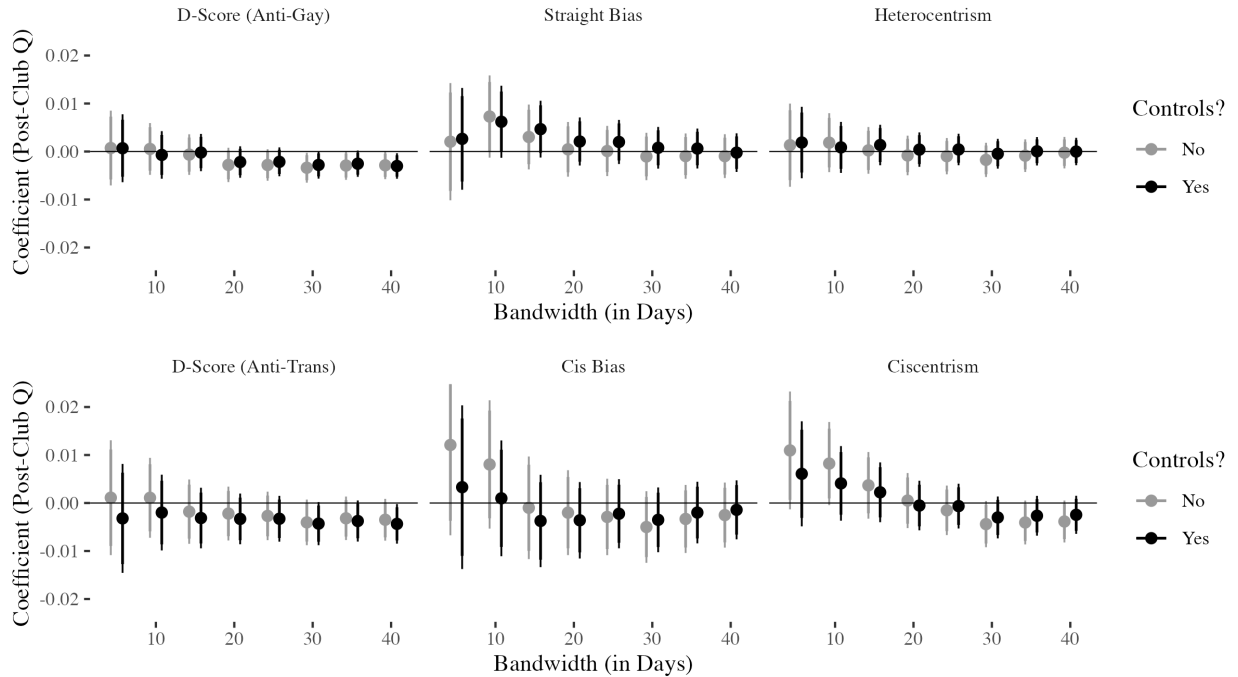[45]Baseline control covariates are measured like Study 2.

Figure 10: Influence of *post-Club Q* on Anti-Gay, Anti-Trans Attitudes. The x-axis is the bandwidth sample (1-40 days). The y-axis is the *post-Club Q* coefficient. Panels characterize different outcomes. The top/bottom 3 panels characterize estimates from the 2022 PI S-IAT/T-IAT data. Black coefficients are from models adjusting for controls, grey otherwise. 95% CIs displayed from HC2 robust SEs. See DSM Tables 185-186 for regression tables characterizing these estimates.

6.1.2 Results

The *post-Club Q* coefficient is null across all outcomes in the S-IAT/T-IAT 20/15-day bandwidth samples (Figure 10). Although *post-Club Q* coefficients in larger bandwidth samples suggest a decrease in the anti-trans and anti-gay *D-score* (e.g. the 40-day bandwidth samples), these estimates should be viewed skeptically given they possess high covariate imbalance and are more likely to be perturbed by unobservable secular trends (Figures E22-E23). Consistent with the *FPVR* model and **H4**, less salient violent events like Club Q do not motivate attitudinal shifts like more salient events (e.g. Pulse or Shepard's murder).

6.1.3 Individual-Level Heterogeneity

We test **H3a-c** and assess if the *post-Club Q* coefficient is larger among a) non-whites and women, b) liberals, and c) individuals living in geographic contexts with more LGBTQ+ group members using the 20- and 15-day bandwidth samples for the S-IAT and T-IAT. We find limited heterogeneity across these characteristics (see Tables E9-E10). The only statistically significant heterogeneity we identify is that the *post-Club Q* coefficient is negative and stronger among women for the *Cis Bias* outcome (Table E10). However, we do not identify heterogeneity by gender in the S-IAT data or the other two T-IAT outcomes. Therefore, we interpret the influence of *post-Club Q* as largely homogeneous.

6.2 Evidence From Less Salient Violent Events

A limitation with Study 4 is that, although the Club Q massacre was less salient than Pulse and Shepard's murder, the null results may be due to the arguably more polarized temporal context given the recent rise of Republican anti-LGBTQ+ rhetoric and policies. Anti-LGBTQ+ laws implemented in Republican states (Figure E24) and right-wing anti-LGBTQ+ protests have increased in the past few years (Figure E25). Indeed, prior research shows LGBTQ+ mass attitudes may entrench in polarized contexts (Lewis et al., 2022). The *FPVR* model also corroborates this limitation, since sympathetic responses by bipartisan elites may be necessary to motivate prosocial mass attitudes (Figure 2).

To circumvent this limitation, we use crowdsourced data on less salient violent incidents against LGBTQ+ people between 2010-2022 and evaluate the influence of these events on prosocial attitudes.[46] We demonstrate the incidents outside of those in Studies 1-4 are significantly less salient (Figure F26). We identify 3570, 442, and 358 NYT article hits related to the Pulse massacre, Shepard's murder, and the Club Q massacre respectively (Figure F26, Panel C). Conversely, the next most salient violent incident against LGBTQ+ group

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[46]https://en.wikipedia.org/wiki/History_of_violence_against_LGBT_people_in_the_United_States

members between 2010-2022 was Mark Carson's May 2013 murder with 30 hits (Figure F26, Panel B). Consistent with **H4**, other less salient violent incidents against LGBTQ+ group members outside those in Studies 1-4 have largely null effects on mass attitudes toward gay people (Figure F27). The few significant effects are not consistently in the same substantive direction, implying a random, unsystematic, causal process.

7   Limitations and Additional Robustness Checks

Our analyses have limitations. First, one issue with our analytic approach is that we use several distinct outcomes across different time periods while assuming they measure the same concept (i.e. prosocial LGBTQ+ attitudes). We believe this is an advantage since prosocial attitudes towards LGBTQ+ group members are *multidimensional* and not correlated with each other 1-to-1 (Flores, 2014). Moreover, queerness is *fluid* and is simultaneously defined and expressed differently over time (Lewis et al., 2022). Therefore, although the meaning of our outcomes may shift over time, our theory may continue to apply across temporal domains. In sum, our empirical approach helps demonstrate our theory is justifiable and broadly applicable by showing high-profile violence against LGBTQ+ people influences distinct prosocial attitudinal dimensions (e.g. policy preferences *and* affective attitudes toward *distinct* LGBTQ+ community segments) similarly across temporal domains.

Nevertheless, our outcomes capture the same concept. If our outcomes are measuring the same concept despite differences in measurement and temporal domain across studies, they should 1) be highly correlated consistently with each other *across several time periods*, and 2) have similar correlates over time. We show these criteria are met in Section 7.

Second, although we provide evidence respondents likely perceived and responded to violence against LGBTQ+ people in a manner consistent with the *FPVR* model, we cannot be certain respondents "received the treatment." Future research should use designs encouraging stronger treatment reception (e.g. survey experiments) to assess if our analyses

underestimate effects and/or temporal persistence. However, unlike designs offering stronger treatment reception, a (tragic) advantage of our design(s) is that they derive effects based on "real-world," externally valid events.

Third, our evidence has not tested all mechanisms consistent with the *FPVR* model's assumptions. Our design is advantageous in that we can assess the effects of violence on prosocial attitudes in an uncontrolled environment with plausible identification assumptions, undercutting demand effects or external invalidity. But, our data were not directly collected to test our hypotheses, making mechanism tests difficult. To the extent we can provide evidence for mechanisms outlined in the *FPVR* model (Figure 2), we show a) initial salience is necessary to motivate prosocial attitudes at the outset, b) declines in salience over time are concomitant with decay in prosocial attitudinal shifts, and c) there is limited support shared marginalization, ideology, and LGBTQ+ geographic context consistently moderates the initial adoption of prosocial attitudes.

Future research should test other *FPVR* model mechanisms (Figure 2). Psychological insights are promising. Violence exposure's influence on prosocial beliefs and their sustainability may be mediated through positive emotional responses toward marginalized groups (e.g. empathy, sympathy, anger, guilt) (Branscombe and Miron, 2004; Harth et al., 2008). Additionally, future research should assess how media frames condition the public's attitudinal responses. During Shepard's murder and Pulse, the media and elites framed the victims sympathetically (instead of unsympathetically). Concomitantly, prior research suggests the media used episodic frames focusing on perpetrator motivations instead of thematic frames emphasizing societal queerphobia (Ott and Aoki, 2002; Zahzah, 2019). It may be prudent to evaluate if framing differences condition prosocial responses and their temporal durability.

Fourth, another limitation is that we only focus on *indirect* exposure to *high-profile* violence. *Direct observation* of smaller-scale quotidian violence against LGBTQ+ group members (e.g. observing hate crimes, assault, verbal abuse) may have a stronger, durable influence on prosocial beliefs. Future research should explore how different violence exposure

types motivate prosocial beliefs.

Fifth, another limitation is that we only explore attitudinal shifts, not behavior. See DSM Section 1.7 for reasoning and evidence the lack of behavioral emphasis may not be a shortcoming.

8   Conclusion

We present a *Fickle Prosocial Violence Response* model to explain how indirect exposure to civilian violence against marginalized groups may influence prosocial attitudes toward targeted groups. Across four studies and three events, we provide evidence supporting the model and show indirect civilian violence against LGBTQ+ group members increases prosocial attitudes toward LGBTQ+ community segments. However, these prosocial responses are not temporally sustainable and less salient events do not motivate prosociality at the outset. Our core contribution is that we repeatedly demonstrate indirect exposure to salient civilian violence against marginalized groups may not sustainably undercut negative attitudes toward these groups. The *FPVR* model provides a general framework that can be tested and theoretically built upon in domains outside anti-LGBTQ+ violence, such as violence against other marginalized groups (e.g. non-whites, immigrants, women).

Interestingly, we find limited individual-level heterogeneity in Studies 1-2 and 4,[47] and some evidence non-whites and Democrats are more likely to adopt prosocial attitudes after Shepard's murder in Study 3. The absence of heterogeneous effects in Studies 1-2 are not necessarily surprising. The *Parallel Publics* thesis posits salient events can generate common information exposure and therefore homogeneous attitudinal responses across population subgroups (Page and Shapiro, 2010). Relatedly, there was mainstream agreement among media and elites the Pulse massacre was tragic and reflected illegitimate behavior. Thus, messaging associated with the massacre was not a "group cue" that could motivate prosocial

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[47]An alternative hypothesis to **H3b** is political moderates may be more likely to adopt prosocial attitudes post-violence given their attitudes are less crystallized. We find no evidence supporting this hypothesis (Tables B1, C5, D7).

responses among some subgroups but not others (Zaller, 1992). Indeed, the effect homogeneity we identify is consistent with prior evidence showing *SSM support* moves in parallel over time across partisan and social subgroups (Coppock, 2023). Study 3's individual-level heterogeneity may be a function of temporal context. Relative to 2016, racial violence was salient in 1998. James Byrd was murdered 4 months before Shepard's murder. The Rampart LAPD scandal was also underway (involving the police beating of Ishmael Jimenez). Therefore, non-whites may have been primed to adopt prosocial attitudes toward groups facing conceivably analogous violence. Likewise, the mass public was less acceptant toward LGBTQ+ people in the 1990s. Therefore, socially conservative Republicans and independents may have been resistant to sympathetic messaging after Shepard's murder relative to liberal Democrats. Finally, Study 4's limited heterogeneity may be due to Club Q's limited salience vis-a-vis Pulse and Shepard's murder.

What would generate durable effects? The *FPVR* model suggests sustained media attention may motivate sustained attitudinal shifts (Figure 2). Disturbingly, salient violent event *recurrence* may facilitate sustainable prosocial shifts. Additionally, the *FPVR* model posits elites play a role in making violent incidents salient. Therefore, elites who continue to strategically amplify issues related to a specific event long after occurrence may sustain attitudinal shifts (Zaller, 1992; Birkland, 1998). The masses may also play a role in facilitating continued event salience. Reny and Newman (2021) show prosocial attitudinal responses to anti-Black violence are relatively durable if the violence is concomitant with (a very large and sustained) social protest. Moreover, perhaps *direct* or *proximal*, as opposed to *indirect*, violence exposure is necessary to durably shift mass attitudes, consistent with prior work (Lupu and Peisakhin, 2017; Mironova and Whitt, 2018; Hadzic et al., 2020). The *FPVR* model could also be extended by evaluating effect sustainability conditional on victim or perpetrator characteristics (e.g. state- vs. civilian-perpetrated), and the scale of violence. We leave it to future research to continue to develop new theoretical insights, extend the *FPVR* model, and assess possibilities for durable effects.

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Supplemental Information

A   Pulse Context

A.1   Demonstrating Pulse Was Salient

Figure A1: Survey Data Demonstrate the Pulse Massacre Was Salient. Panels A and B display how closely respondents were following the Pulse shooting in a June 2016 CBS and Kaiser poll respectively. Panel B compares attention to Pulse (x-axis) relative to other issues (y-axis). All estimates are population weighted. 95% CIs displayed from 1000 bootstrap simulations. See DSM Section 1.2 for more details on Figure A1 polls.

A.2   Media Coverage Over Time

Figure A2: Media Coverage of Topics Related to the Pulse Massacre Over Time.
Panels A, C, and E display the count of Pulse-, LGBTQ-, and terrorism-related stories between January-October 2016. Panels B, D, and F display the ratio of Pulse-, LGBTQ-, and terrorism-related stories relative to the total number of stories in digital news. Loess models fit on each side of the moment the massacre occurs. Annotations denote RDiT estimates for the effect of Pulse on the article count and ratio using MSE optimal bandwidth selection (Calonico et al., 2015) (running variable degree = 1). See DSM Section 1.3 for more details on Figure A2 data.

3

A.3 Search Behavior Over Time

A. Pulse Nightclub Shooting

Pulse Massacre
(06/12)

(y-axis: Mean Google Trends Hits; values 0, 25, 50, 75, 100)
(x-axis: Date; Jan, Apr, Jul, Oct)

B. LGBTQ+ Related

Pacquiao
Anti-Gay
Remarks
(02/16)

NC Blocks Anti-LGBTQ
Discrim. (03/24)

Colombia
Legalizes
SSM (04/08)

China Court
Rejects
SSM (04/13)

Texas Court
Sustains
SSM (04/15)

Pulse Massacre
(06/12)

(y-axis: Mean Google Trends Hits; values 20, 40, 60, 80)
(x-axis: Date; Jan, Apr, Jul, Oct)

C. Terrorism Related

Brussels Attack
(03/22)

Pulse Massacre
(06/12)

Ataturk Attack
(06/28)

Nice Attack
(07/14)

(y-axis: Mean Google Trends Hits; values 20, 40, 60, 80)
(x-axis: Date; Jan, Apr, Jul, Oct)

Figure A3: Search Behavior From Google Trends Demonstrates the Pulse Massacre Was Salient and Unexpected. Panels A, B, and C display the average search intensity for Pulse massacre-, LGBTQ-, and terrorism-related terms between January-October 2016. Vertical lines and annotations denote key events related to respective topics. See DSM Section 1.5 for more details on Figure A3 data.
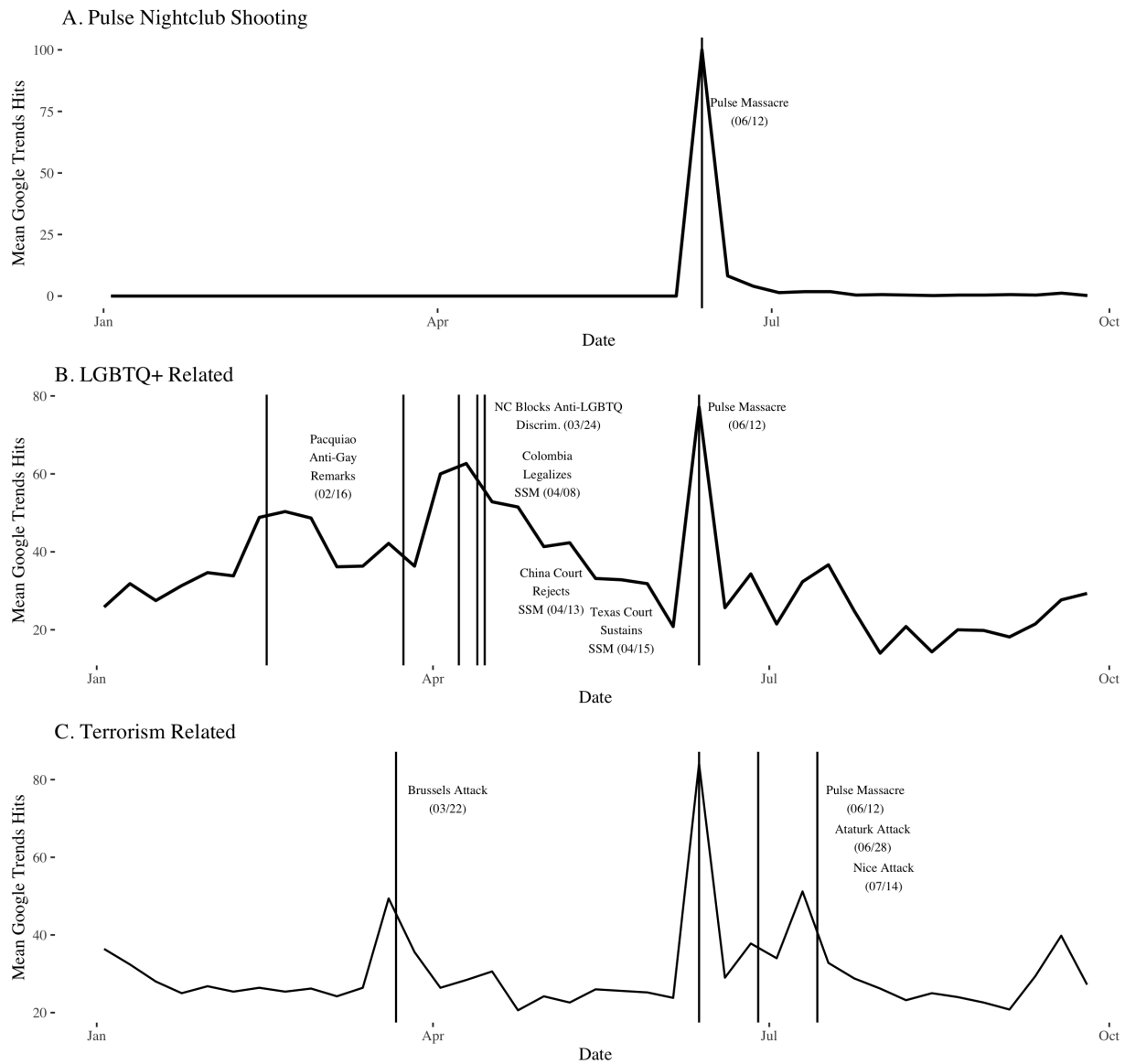
4

A.4 Demonstrating Public Perceived Pulse

Figure A4: The Pulse Massacre Was Perceived by the Mass Public. Panels A-E characterize predicted values of belief country is less safe since 9/11, terror attacks are likely in the future, international terrorism is a critical threat, worry about terrorism, and worry about gun violence respectively. Panel F characterizes the the influence of Pulse (x-axis) on the aforementioned outcomes (y-axis) adjusting and not for imbalanced covariates (black = with controls, grey otherwise). All covariates rescaled between 0-1. 95% CIs displayed derived from HC2 robust standard errors. Data are from the Chicago Council on Global Affairs Survey (June 10-26). See DSM Section 1.4 for more details on Chicago Council data. See also Table SM 1 in the Online Supplementary Material.

A.5 Demonstrating Public Perceived Massacre as Hate Crime

Figure A5: The Pulse Massacre Was Perceived as Targeted Anti-LGBTQ+ Violence. Panels A and B display beliefs the public felt the shooting was an anti-LGBTQ+ hate crime in a June 2016 CBS poll (Panel A) and July 2016 AP poll (Panel B). All estimates are population weighted.

B Study 1: TAPS

B.1 Manipulation Check

Figure B6: Belief ISIS = Most Important Issue Increases After Pulse. Estimates use survey weights to ensure representativeness. All covariates scaled between 0-1. 95% CIs displayed derived from HC2 robust standard errors. See DSM Table 90 for regression table characterizing *post-Pulse* and control coefficients.

B.2 Temporal Placebo Tests

Figure B7: The Effect of Pulse is Unique to 2016. The x-axis is the survey at use. The y-axis is the coefficient for a binary indicator if the respondent was interviewed the calendar day after the Pulse massacre in 2012, 2013, 2016, and 2017 respectively. The outcome for all studies/models is support for same sex marriage. Color denotes the inclusion/exclusion of adjustment for baseline covariates between respondents interviewed before and after the calendar day of the Pulse massacre. All covariates rescaled between 0-1. 95% CIs displayed from HC2 robust standard errors. See DSM Table 87 for regression tables characterizing these post-Pulse (and control) coefficient estimates.

B.3 Alternative Bandwidths

Figure B8: The Effect of Pulse is Robust to Alternate Bandwidths. The x-axis is the bandwidth (in days) for the pre and post Pulse period. The y-axis is the coefficient for a binary indicator if the respondent was interviewed after the Pulse nightclub shooting. Color denotes the inclusion/exclusion of control covariates adjusting for covariate imbalance between respondents interviewed before and after the Pulse nightclub shooting. Annotations denote sample size for each estimate in addition to the number of imbalanced covariates. All covariates re-scaled between 0-1. 95% CIs displayed from HC2 robust standard errors. See DSM Table 106 for regression tables characterizing the post-Pulse and control coefficients.
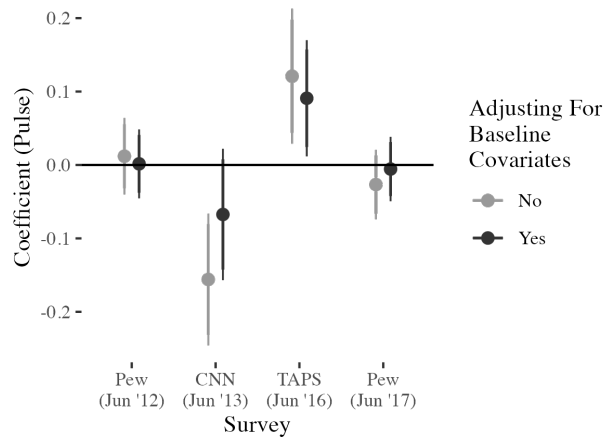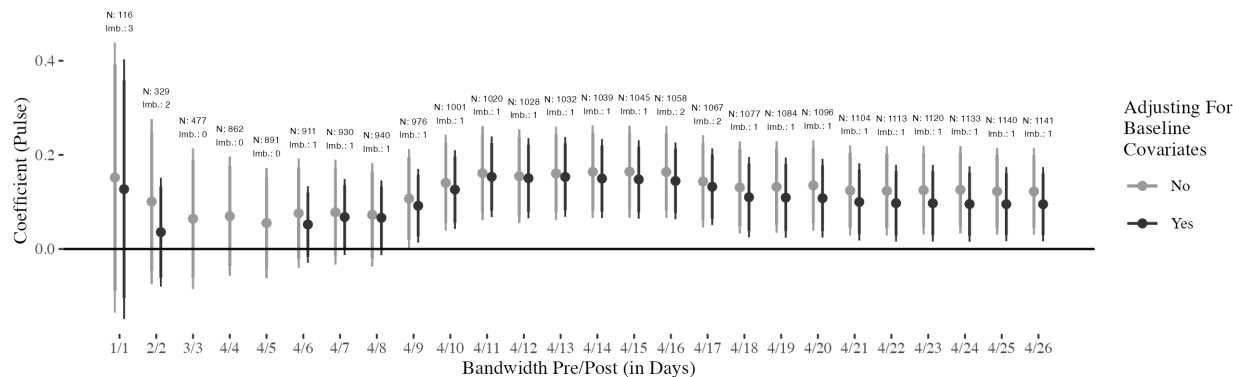
B.4 Evaluating Individual-Level Heterogeneity

Table B1: Assessing Heterogenous Influence of *Post-Pulse* (Study 1)

	SSM Support					
	(1)	(2)	(3)	(4)	(5)	(6)
Post-Pulse	0.08†	0.14**	0.10†	0.12**	0.11	0.13†
	(0.04)	(0.05)	(0.05)	(0.04)	(0.12)	(0.07)
Post-Pulse x Non-White	0.07					
	(0.10)					
Post-Pulse x Woman		−0.08				
		(0.08)				
Post-Pulse x Liberal			−0.01			
			(0.08)			
Post-Pulse x Moderate				−0.05		
				(0.12)		
Post-Pulse x % LGBTQ (State)					−0.09	
					(0.58)	
Post-Pulse x SS Couple Density						−0.19
						(0.34)
Non-White	−0.24***					
	(0.07)					
Woman	0.05	0.09†	0.05	0.08*	0.05	0.05
	(0.04)	(0.05)	(0.04)	(0.04)	(0.04)	(0.04)
Liberal	0.38***	0.38***	0.38***		0.38***	0.38***
	(0.04)	(0.04)	(0.05)		(0.04)	(0.04)
Moderate				−0.05		
				(0.07)		
% LGBT (State)					0.31	
					(0.40)	
SS Couple Per Capita (County)						0.22
						(0.17)
R²	0.35	0.36	0.35	0.24	0.36	0.36
Num. obs.	1132	1132	1132	1132	1132	1132
N Clusters					50	585

Note: ***$p < 0.001$; **$p < 0.01$; *$p < 0.05$. All models adjust for age, white (if not assessing heterogeneity by non-white), woman, religiosity, marital status, parental status, income, college education, unemployed status, union member, renter status, internet access, internet mode, liberal, metropolitan residence and Florida, Texas, California, New York, and Pennsylvania residence. HC2 robust SEs in parentheses but clustered at state and county-level for Models 4-5.

B.5 Heterogeneity By Political Interest and Media Consumption

B.5.1 Measurement of Political Interest and Media Consumption

Political Interest: How interested would you say you are in politics and current affairs? 1) very interested, 2) somewhat interested, 3) not very interested, 4) not at all interested. Coded as a binary indicator equal to 1 if respondent puts "very interested," 0 otherwise (45% say "very interested," 55% say otherwise).

News Consumption: How frequently do you pay attention to news about national and international issues? 1) every day, 2) several times a week, 3) once a week, 4) several times a month, 5) once a month, 6) less often, 7) never. Coded as a binary indicator equal to 1 if respondent puts "every day," 0 otherwise (59% say "every day", 41% say otherwise).

Interest Scale: The *interest scale* is an additive index from 0-2 of the news consumption and *political interest* measures discussed above (0 = 32% of the sample, 1 = 32% of the sample, 2 = 36% of the sample).

B.5.2 Results

Table B2: Evaluating Heterogenous Influence of Post-Pulse Conditional on Political Interest and News Consumption

	SSM Support					
	(1)	(2)	(3)	(4)	(5)	(6)
Post-Pulse x Political Interest	0.01	0.04				
	(0.09)	(0.08)				
Post-Pulse x News Consumption			−0.11	−0.01		
			(0.09)	(0.08)		
Post-Pulse x Interest Scale					−0.03	0.01
					(0.06)	(0.05)
Controls	N	Y	N	Y	N	Y
R^2	0.02	0.36	0.02	0.35	0.02	0.35
N	1134	1132	1134	1132	1134	1132

Note: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$. Models alternate between excluding/including control covariates. This table only presents the interaction between the *post-pulse* indicator and *political interest*, *news consumption*, and the *interest scale*. HC2 robust standard errors in parentheses. All covariates are scaled between 0-1. All estimates are population-weighted.

Here we assess the heterogenous influence of being interviewed *post-Pulse* on *SSM support* among TAPS respondents conditional on political interest and news consumption. We conduct this test to assess if those who are attuned to media and politics are differentially more likely to support the rights of segments of the LGBTQ+ community in response to exposure to violence against LGBTQ+ (Reny and Newman, 2021).

Table B2 demonstrates that the influence of being interviewed *post-Pulse* on *SSM support* is not heterogeneous with respect to *political interest*, *news consumption* levels, or the *interest scale*.

We do not think the absence of heterogeneity poses a problem for the validity of our results. Consistent with prior research, the *political interest* and *news consumption* measures capture a *general* disposition towards consuming media and politics that is relatively stable (Prior, 2010). But that general disposition may be abrogated in the context of high-salience events. This is to say, even those segments of the mass public who do not necessarily pay attention to salient political/media events may have internalized information about the Pulse massacre. This is corroborated by our evidence on Figure A1, which demonstrates that 86% (Kaiser Poll, June 15-21, 2016) to 89% (CBS News Poll, June 13-14, 2016) of the mass public was closely following the shooting. Therefore, nearly all of the mass public was closely following the Pulse massacre, implying high levels of potential treatment reception regardless of one's generalized *political interest* or level of *news consumption*.

C   Study 2: PI S-IAT

C.1   Anti-Gay Attitudes Over Time

Figure C9: Anti-Gay Attitudes (y-axis) Over Time (x-axis, in days) Between 2016-01-01 and 2016-09-07. Dashed vertical line is the moment the Pulse nightclub massacre occurred. Loess models are fit on each side of the moment Pulse occurred. All covariates re-scaled between 0-1.

C.2   Balance Tests

Figure C10: Balance on IAT Taker Composition Before and After the Massacre. Each panel characterizes covariate balance for different bandwidths (see plot title, with sample size). The x-axis is the *post-Pulse* coefficient derived from separate regression models regressing a baseline covariate (y-axis) on *post-Pulse*. Black coefficients are statistically significant, grey otherwise. See Section **??** for regression tables characterizing these balance plots.

10

C.3 Temporal Placebo Tests

Here, we show preexisting time trends are not driving our results. We estimate the influence of taking the PI S-IAT 15 and 20 days pre-Pulse relative to 16-30 and 21-40 days pre-Pulse on the *D-score* and *heterocentrism*. We also estimate the influence of taking the PI S-IAT after (2016-03-07 to 2016-06-11) relative to before (2016-01-01 to 2016-03-06) the median pre-treatment date. These placebo estimates are null, suggesting secular pro-gay time trends do not explain our findings (Figure C11).

Figure C11: Comparing True *post-Pulse* Coefficient to Placebo Coefficients To Rule Out Pre-Treatment Temporal Trends That Motivate Pro-Gay Attitudes. The x-axis is the type of estimate. True (15 days) is the true *post-Pulse* coefficient using a 15-day bandwidth. True (20 days) is the same with a 20-day bandwidth. Placebo 1 estimates the influence of taking the IAT in the 15 days prior to the Pulse massacre relative to the 16-30 days prior to the Pulse massacre. Placebo 2 estimates the influence of taking the IAT in the 20 days prior to the Pulse massacre relative to the 21-40 days prior to the Pulse massacre. Placebo 3 estimates the influence of taking the IAT after the median pre-treatment day (2016-03-07 to 2016-06-12) relative to the days before the median pre-treatment day (2016-01-01 to 2016-03-06). The y-axis is the coefficient. The left/right panel characterizes the influence of the true and placebo coefficients on the *D-score* and *heterocentrism*. Estimates are not covariate-adjusted. See DSM Table 174 for regression tables characterizing these estimates. 95% CIs displayed from HC2 robust SEs.

C.4 Prior and Post Year Temporal Placebo

Here, we attempt to rule out if systematic temporal trends near June motivate prosocial attitudes toward gay people other than the massacre. Thus, we assess the influence of placebo estimates comparing *D-score* and *heterocentrism* 15 and 20 days before and after June 12, the massacre calendar day, during the years 2010-2015 and 2017-2018. We find no consistent influence of these placebo estimates on the *D-score* and *heterocentrism* (Figure C12).

Figure C12: Temporal Placebo Tests Using IAT Data From Non-2016 Years. The x-axis is the IAT dataset at use (by year). The y-axis is the coefficient characterizing the influence of taking the IAT after June 12 (the calendar day of the Pulse nightclub shooting occurred). Panels A and B refer to estimates assessing the influence of the post-June 12th placebo on the *D-Score* and *Heterocentrism* outcomes. The top/bottom two panels are estimates using a 15/20 day bandwidth. 95% CIs displayed derived from HC2 robust standard errors. For regression tables characterizing these coefficients, see DSM Table 175

C.5 Falsification Tests on Treatment-Irrelevant Group Attitudes

Here, we demonstrate our findings may not be due to a secular attitudinal trend in favor of marginalized groups through several falsification tests assessing if attitudes toward Black people, Asians, the differently-abled, Arabs, darker-skin people, and women shifts *post-Pulse* using the 15 and 20-day bandwidth samples.[48] Across 28 statistical tests, only 3 are significant (Section C.5), suggesting our findings are not driven by secular liberal attitudinal trends toward marginalized groups.

Table C3: Falsification Test on Treatment-Irrelevant Group Attitudes

Post-Pulse Coef.	SE	p	N	Outcome	Dataset	Bandwidth
-0.000	0.005	0.949	11310.000	D-Score	Black/White IAT	15 days
-0.003	0.003	0.377	10960.000	White Bias	Black/White IAT	15 days
-0.006	0.003	0.043	11039.000	Ethnocentrism	Black/White IAT	15 days
0.012	0.015	0.434	1279.000	D-Score	Asian/European IAT	15 days
0.011	0.011	0.320	1234.000	White Bias	Asian/European IAT	15 days
0.006	0.014	0.670	1509.000	D-Score	Disabled/Abled IAT	15 days
-0.002	0.008	0.765	1484.000	Abled Bias	Disabled/Abled IAT	15 days
-0.009	0.009	0.319	1500.000	Abledcentrism	Disabled/Abled IAT	15 days
-0.013	0.013	0.327	1331.000	D-Score	Arab/Non-Arab IAT	15 days
-0.003	0.009	0.766	1267.000	Non-Arab Bias	Arab/Non-Arab IAT	15 days
-0.002	0.010	0.808	1310.000	Ethnocentrism	Arab/Non-Arab IAT	15 days
-0.014	0.009	0.145	3064.000	D-Score	Dark Skin/Light Skin IAT	15 days
-0.001	0.007	0.898	4550.000	D-Score	Man/Woman (Career) IAT	15 days
0.004	0.010	0.702	2339.000	D-Score	Man/Woman (Science) IAT	15 days
-0.003	0.004	0.429	15506.000	D-Score	Black/White IAT	20 days
-0.006	0.003	0.013	15037.000	White Bias	Black/White IAT	20 days
-0.008	0.003	0.004	15151.000	Ethnocentrism	Black/White IAT	20 days
0.008	0.013	0.518	1735.000	D-Score	Asian/European IAT	20 days
0.011	0.009	0.218	1670.000	White Bias	Asian/European IAT	20 days
0.010	0.012	0.399	1972.000	D-Score	Disabled/Abled IAT	20 days
0.005	0.007	0.481	1938.000	Abled Bias	Disabled/Abled IAT	20 days
-0.003	0.008	0.736	1959.000	Abledcentrism	Disabled/Abled IAT	20 days
0.005	0.012	0.638	1745.000	D-Score	Arab/Non-Arab IAT	20 days
0.005	0.008	0.532	1663.000	Non-Arab Bias	Arab/Non-Arab IAT	20 days
0.005	0.009	0.543	1717.000	Ethnocentrism	Arab/Non-Arab IAT	20 days
-0.009	0.008	0.249	4213.000	D-Score	Dark Skin/Light Skin IAT	20 days
-0.003	0.006	0.604	6624.000	D-Score	Man/Woman (Career) IAT	20 days
0.007	0.008	0.416	3371.000	D-Score	Man/Woman (Science) IAT	20 days

This table characterizes falsification tests assessing the influence of taking an IAT *post-Pulse* on groups that are potentially unrelated to LGBTQ+. Not all datasets include the respective *D-score*, *bias*, and dominant group-centrism outcomes (hence their missingness in some IAT datasets). HC2 robust SEs displayed.

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[48]Falsification test data comes from separate Project Implicit surveys co-currently available to take in addition to the anti-gay attitude survey.

C.6 Evaluating Individual-Level Heterogeneity

Table C4: Assessing Heterogenous Influence of *Post-Pulse* (Study 2, Part 1)

	D Score		Heterocentrism		D Score		Heterocentrism		D Score		Heterocentrism	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Post-Pulse	$-0.01^\dagger$	$-0.01^\dagger$	-0.02^{**}	-0.01^{**}	-0.02^*	-0.01	-0.02^{**}	-0.02^{**}	-0.01^*	-0.00	-0.01	-0.01
	(0.01)	(0.00)	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Post-Pulse x Non-White	0.01	0.00	0.01	0.00								
	(0.01)	(0.01)	(0.01)	(0.01)								
Post-Pulse x Woman					0.02	0.01	$0.02^\dagger$	0.01				
					(0.01)	(0.01)	(0.01)	(0.01)				
Post-Pulse x Liberal									0.01	-0.00	-0.00	-0.00
									(0.01)	(0.01)	(0.01)	(0.01)
Non-White	0.02^*	0.01^*	0.00	0.01								
	(0.01)	(0.01)	(0.01)	(0.01)								
Woman	-0.02^{***}	-0.02^{***}	-0.01	-0.01^*	-0.03^{***}	-0.02^{***}	-0.01^*	-0.01^*	-0.02^{***}	-0.02^{***}	-0.01	-0.01^*
	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)
Liberal	-0.07^{***}	-0.07^{***}	-0.08^{***}	-0.08^{***}	-0.07^{***}	-0.07^{***}	-0.08^{***}	-0.08^{***}	-0.07^{***}	-0.07^{***}	-0.08^{***}	-0.08^{***}
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)
Bandwidth	15 Days	20 Days	15 Days	20 Days	15 Days	20 Days	15 Days	20 Days	15 Days	20 Days	15 Days	20 Days
R^2	0.12	0.12	0.16	0.16	0.12	0.12	0.17	0.16	0.12	0.12	0.16	0.16
N	3638	4907	3645	4920	3638	4907	3645	4920	3638	4907	3645	4920

Note: $^{***}p < 0.001$; $^{**}p < 0.01$; $^*p < 0.05$. All models adjust for age, white (when not assessing heterogeneity by non-white), woman, college education, religious, metropolitan residence, ideology, California, Pennsylvania, Florida, and Illinois state residence. All covariates rescaled between 0-1. HC2 robust SEs in parentheses.

Table C5: Assessing Heterogenous Influence of *Post-Pulse* (Study 2, Part 2)

	D Score		Heterocentrism		D Score		Heterocentrism		D Score		Heterocentrism	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Post-Pulse	0.03	0.01	0.01	-0.01	-0.00	-0.00	-0.02^*	-0.02^{**}	-0.00	-0.01	-0.01^*	-0.01^{**}
	(0.02)	(0.01)	(0.03)	(0.03)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)
Post-Pulse x % LGBT (State)	-0.07	-0.04	-0.05	-0.00								
	(0.03)	(0.02)	(0.07)	(0.07)								
Post-Pulse x SS Couple Density (County)					-0.02	-0.02	0.05	0.04				
					(0.03)	(0.03)	(0.03)	(0.02)				
Post-Pulse x Moderate									-0.01	-0.00	-0.01	-0.00
									(0.01)	(0.01)	(0.01)	(0.01)
% LGBT (State)	-0.01	-0.02	-0.00	-0.02								
	(0.03)	(0.02)	(0.05)	(0.04)								
SS Couple Density (County)					-0.03	$-0.04^\dagger$	-0.09^{**}	-0.08^{**}				
					(0.03)	(0.02)	(0.02)	(0.02)				
Moderate									-0.02^*	-0.02^{**}	-0.05^{***}	-0.05^{***}
									(0.01)	(0.01)	(0.01)	(0.01)
R^2	0.12	0.12	0.17	0.16	0.12	0.12	0.17	0.16	0.11	0.12	0.18	0.17
Num. obs.	3638	4907	3645	4920	3638	4907	3645	4920	3638	4907	3645	4920
N Clusters	52	52	52	52	739	848	738	848				

$^{***}p < 0.001$; $^{**}p < 0.01$; $^*p < 0.05$; $^\dagger p < 0.1$

Note: $^{***}p < 0.001$; $^{**}p < 0.01$; $^*p < 0.05$. All models adjust for age, white, woman, college education, religious, metropolitan residence, ideology, California, Pennsylvania, Florida, and Illinois state residence. Models 1-4 adjust for an interaction between *post-pulse* and an indicator for state residence missingness. Models 5-8 adjust for an interaction between *post-pulse* and an indicator for county residence missingness. All covariates rescaled between 0-1. HC2 robust SEs in parentheses but clustered at state and county-level for Models 1-4 and 5-8 respectively.

C.7 Sorting Test

Given respondents self-select into the S-IAT, we may be concerned systematic sorting induces bias (e.g. pro-gay people taking the survey *post-Pulse*). We do not believe sorting is a concern. If more pro-gay individuals were taking the survey *post-Pulse*, *post-Pulse* respondents would be younger, more liberal, less religious, and more college-educated, but they are not (Figure C10, Panels C-D). Second, if sorting were operative, we may expect more respondents taking the S-IAT *post-Pulse*. We conduct a difference-in-means comparing the number of daily respondents *post-Pulse* relative to pre-Pulse, and do not statistically find more respondents took the S-IAT *post-Pulse* (Table C6).

Table C6: Effect of Pulse On Number of Project Implicit Sexuality IAT Survey Participants

	# Of Participants	
	(1)	(2)
Intercept	111.47***	118.60***
	(12.97)	(10.76)
Post-Pulse	22.00	10.60
	(17.21)	(14.09)
Bandwidth	15-day	20-day
R^2	0.06	0.01
Num. obs.	30	40

*** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$

D Study 3: Matthew Shepard

D.1 Alternative Outcomes

We found two other items that could serve as potential candidates for assessing the influence of Shepard's murder on attitudes toward LGBTQ+ group members, however, we do not use them for various reasons. One item measured support for *legal recognition* of "marriages between homosexuals." But, there is a 3-year interval between the two surveys including this outcome item (Gallup Mar. 1996, $N = 1008$, Gallup Feb. 1999, $N = 1054$), and there are no surveys with comparable items concerning legal recognition of marriages between homosexuals prior to the baseline time period to conduct temporal placebo tests. Nevertheless, respondents surveyed *post-Shepard* are more likely to support legally recognizing same-sex marriage, consistent with **H1** (see Section D.1.1 for details). Another item measures support for homosexuals serving in the armed forces using two surveys 7 months apart (*hire military*, Newsweek Jul. 1998, $N = 602$; Gallup Feb. 1999, $N = 1054$). Consistent with **H1**, we find respondents interviewed *post-Shepard* were more likely to support homosexuals

serving in the military (Figure D14, Panel B). However, these effects may be a function of a secular attitudinal trend in support of incorporating homosexuals in the military, perhaps buttressed by Bill Clinton's efforts to implement Don't Ask Don't Tell in the 1990s. We demonstrate this is the case by showing that support for hiring gay people in the military is on an increasing trend from 1977-1996 (Figure D14, Panel C). Conversely, the *moral wrong* outcome is remarkably stable prior to Shepard's murder, making it an ideal candidate for assessing attitudinal shifts *post-Shepard* and their temporal sustainability (Figure 9).

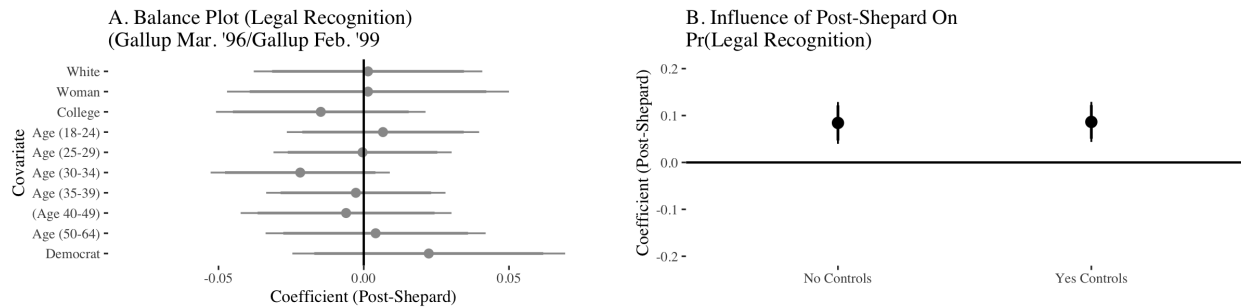### D.1.1 Alternative Outcome: Legal Recognition

Figure D13: Influence of Shepard's Murder on Support for Legal Recognition of Same-Sex Marriages. All estimates include population weights. All covariates are scaled between 0-1. 95% CIs displayed derived from HC2 robust standard errors. See DSM Table 180 for regression tables on coefficients characterizing Panel B.

Data are from two polls stacked together. The first poll is the Gallup March 1996 Politics Polls ($N = 1008$). It was fielded from March 15-17, 1996 and is a telephone survey. The second poll is the Gallup February 1999 Service Poll ($N = 1054$). It was fielded from February 8-9, 1999. The main outcome of interest for this analysis is *legal recognition.* Legal recognition is from a common item in these two polls that asks respondents if they "think marriages between homosexuals should or should not be recognized by the law as valid, with the same rights as traditional marriages." Respondents can choose to reply "should be valid" or "should not be recognized." The outcome is measured equal to 1 if the respondent replies with "should be valid," and 0 otherwise. Figure D13, Panel A displays covariate composition balance between the pre- (Gallup 1996) and *post-Shepard* (Gallup 1999) surveys. Panel B displays the influence of being interviewed in the *post-Shepard* survey on respondents reporting that they believe marriages between homosexual should be recognized by the law as valid. Respondents interviewed *post-Shepard* report a 8 percentage point increase in support for the belief homosexuals should have their marriages legally recognized.

D.1.2 Alternative Outcome: Hire Military

The two studies we use to assess if the belief homosexuals should be hired for the military increases after Shepard's murder are a Newsweek Jul. 30-31 1998 poll and a Gallup Feb. 8-9 1999 poll. Both are nationally representative adult telephone surveys ($N = 602$, $N = 1054$) and are population weighted to census demographic benchmarks.
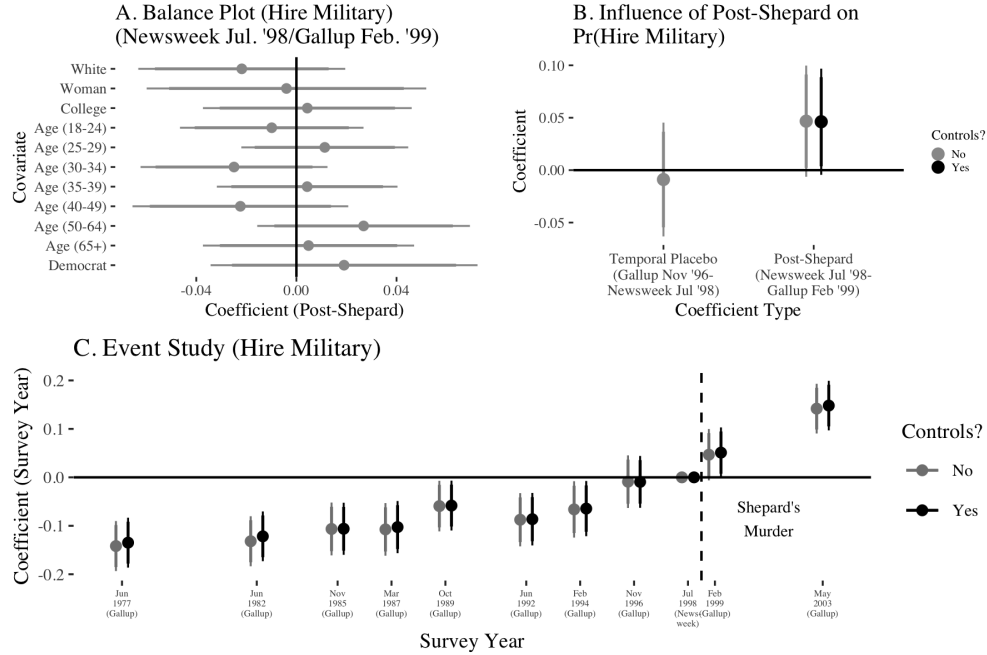
Figure D14: Influence of Shepard's Murder on Support for Hiring Gay People To Serve In The Military. Panel A displays covariate balance between the Newsweek Jul '98 and Gallup Feb '99 polls used to assess the influence of being interviewed *post-Shepard* on attitudes toward hiring gay people to serve in the military. Panel B displays a temporal placebo test assessing if mass attitudes on hiring gay people in the military shift between Nov '96 and Jul '98 in addition to coefficients with and without covariate adjustment that assess the influence of being interviewed *post-Shepard* on support for hiring gay people in the military. Panel C displays an event study assessing trends in support for hiring gay people in the military relative to a survey in Jul 1998 (hence no CIs for that survey estimate). All estimates include population weights. All covariates are scaled between 0-1. 95% CIs displayed derived from HC2 robust standard errors. See DSM Tables 181 and 182 for full regression tables characterizing the coefficients on Panels B and C

The outcome item of interest from the Newsweek poll is "Tell me if you think gays and lesbians should be hired as members of the armed forces" with response choices of 1) Should and 2) Should not. The outcome is binary, equal to 1 if the respondent indicates "Should." The outcome item of interest from the Gallup poll is "Do you think homosexuals should or should not be hired for the armed forces" with response choices of 1) Should and 2) Should not. The weights, outcome, and baseline covariates are then stacked amongst each other across the two polls, with respondents from the Gallup Feb. 1999 poll being defined as *post-Shepard* respondents (measured as a binary indicator equal to 1 if the respondent is from the October 1998 poll, 0 otherwise) and respondents from the Newsweek Jul. 1998 poll being defined as pre-Shepard respondents.

To conduct a temporal placebo test ruling out secular trends that may drive our finding that respondents interviewed after Shepard's murder were more likely to support hiring homosexuals in the military, we use a third survey, the Gallup Nov 21-24 1996 poll. The Gallup Nov 1996 poll is a nationally representative telephone survey ($N = 1003$) and is population weighted to census demographic benchmarks. The Gallup Nov 1996 poll includes an item asking respondents if they think "homosexuals should or should be hired for the armed forces" with responses 1) Should and 2) Should not. We then compare the average

level of support for whether homosexuals should be hired for the armed forces in the Gallup 1996 poll with the Newsweek 1998 poll.

Figure D14, Panel A, demonstrates that respondents interviewed before and after Shepard's murder are similar on demographic, socio-economic, and political covariates. Figure D14, Panel B demonstrates that respondents interviewed *post-Shepard* are more likely to support gay people serving in the armed forces by 5 percentage points ($p < 0.10$), equivalent to 9% of the outcome standard deviation. However, Figure D14, Panel C demonstrates that support for hiring gay people in the military is on an upward trend between 1977-1996, suggesting these results may be a function of a progressive secular trend in support of incorporating gay people in the military, perhaps the result of Bill Clinton's push for Don't Ask Don't Tell policies.

D.2 Assessing If Violence Against LGBTQ+ Community Segments Was Salient in 4 Months Between Surveys
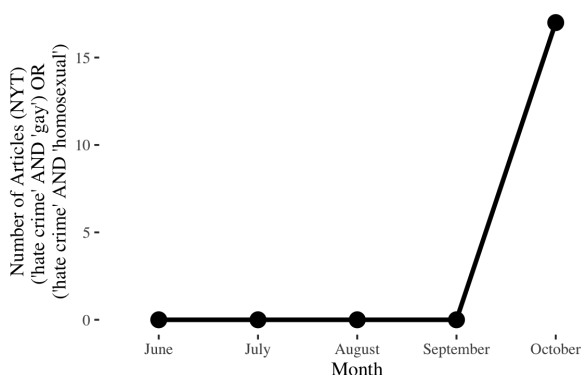

Figure D15: There Was No New York Times Coverage of Hate Crimes Related to Gay People In Between June-October 1998. The x-axis is the month of 1998, the y-axis is the count of articles identified in the New York Times Historic Database (ProQuest) that are related to the following search term: ("hate crime" AND "gay") OR ("hate crime" AND "homosexual")

D.3 Other Intervening Events

Two other intervening events outside of other instances of violence against LGBTQ+ community segments during 1998 may explain our *post-Shepard* coefficient. First, Clinton signed Executive Order 13087 on May 1998, which prohibited discrimination over sexual orientation in the Federal workforce. If this explains our results, then we would expect the temporal placebo coefficient to be negative and statistically significant given the post-placebo survey is fielded on June 1998, after the executive order. The placebo coefficient is 0 and insignificant, suggesting Clinton's order does not explain our results (Figure 8, Panel B). Indeed, Clinton's order was not nearly as salient as Shepard's murder. There was no NYT coverage of his order on May or June 1998, the moment the executive order was signed (Figure D16). Second, Tammy Baldwin's 1998 House election run (the first open lesbian elected to Congress). This is unlikely because Baldwin's run was significantly

less salient than Shepard's murder. There were only 2 NYT articles mentioning Baldwin during June-October 1998 but over 30 Shepard-related NYT articles on October 1998 (Figure D17).

D.3.1 Assessing If Clinton's Anti-Discrimination Executive Order Was Salient
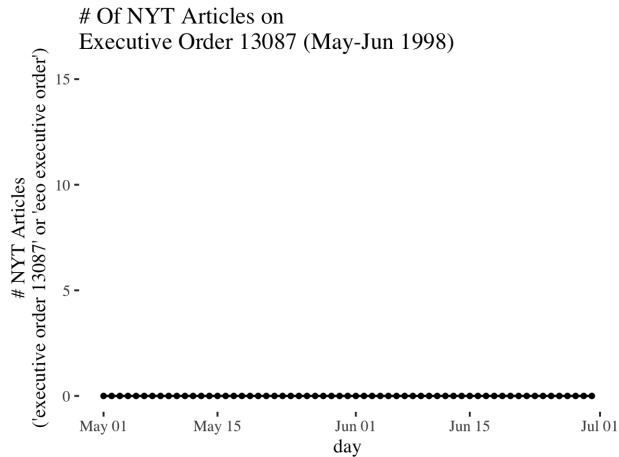

Figure D16: There Were No New York Times Articles Related to Executive Order 13087 Near The Moment It Was Signed. The x-axis is the day, the y-axis is the count of articles identified in the New York Times Historic Database (`rtimes` package) that are related to the following search terms: "executive order 13087" OR "eeo executive order."

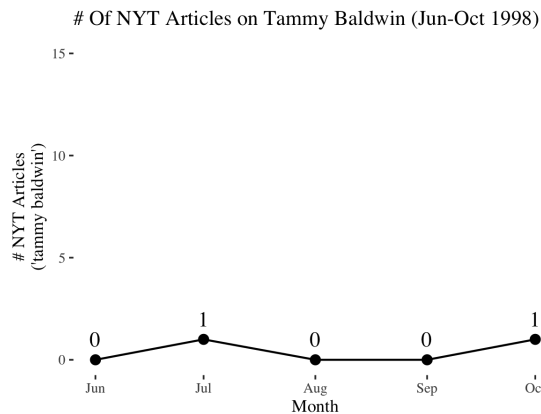### D.3.2 Assessing If Tammy Baldwin's Election Was Salient in 4 Months Between Surveys

Figure D17: There Were Only 2 New York Times Articles Related to Tammy Baldwin In Between June-October 1998. The x-axis is the month of 1998, the y-axis is the count of articles identified in the New York Times Historic Database (`rtimes` package) that are related to the following search term: "tammy baldwin." Annotations denote number of NYT articles for each specific month.

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D.4 Evaluating Individual-Level Heterogeneity

Table D7: Heterogenous Influence of *Post-Shepard* (Study 3)

| | Moral Wrong | | | |
	(1)	(2)	(3)	(4)
Post-Shepard	−0.07**	−0.10**	−0.02	−0.11***
	(0.03)	(0.04)	(0.03)	(0.03)
Post-Shepard x Non-White	−0.15*			
	(0.06)			
Post-Shepard x Woman		−0.02		
		(0.05)		
Post-Shepard x Democrat			−0.20***	
			(0.05)	
Post-Shepard x Independent				−0.06
				(0.07)
Non-White	0.10*			
	(0.04)			
Woman	−0.08*	−0.08*	−0.08*	−0.08*
	(0.03)	(0.03)	(0.03)	(0.03)
Democrat	0.06	0.06	0.06	0.07
	(0.03)	(0.03)	(0.04)	(0.04)
R^2	0.07	0.07	0.07	0.07
Num. obs.	2052	2052	2052	2052

***$p < 0.001$; **$p < 0.01$; *$p < 0.05$

Note: ***$p < 0.001$; **$p < 0.01$; *$p < 0.05$. All models adjust for age, white (if not assessing heterogeneity by non-white), woman, college education, partisanship, voter registration, and Florida, Texas, California, New York, and Pennsylvania residence. HC2 robust SEs in parentheses.

E Study 4: Club Q

E.1 Salience of Club Q Relative to Pulse and Shepard
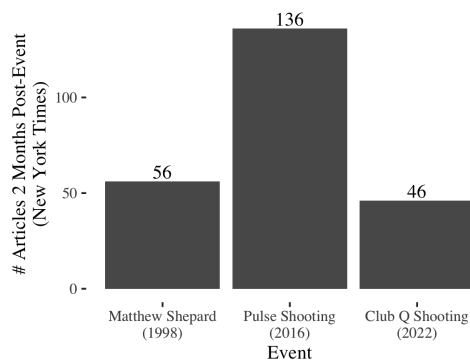
E.1.1 New York Times

Figure E18: Number of New York Times Articles Related to Matthew Shepard's Murder, the Pulse Massacre, and the Club Q Shooting In The Two Months After The Event(s). The x-axis is the respective event, the y-axis is the number of articles published in the New York Times in the two months after the incident. Data are from the ProQuest New York Times Historic Newspaper database. Search phrases for the respective incidents are: "matthew shepard AND (murder OR death OR killed)," "pulse AND shooting", and "club q AND shooting."
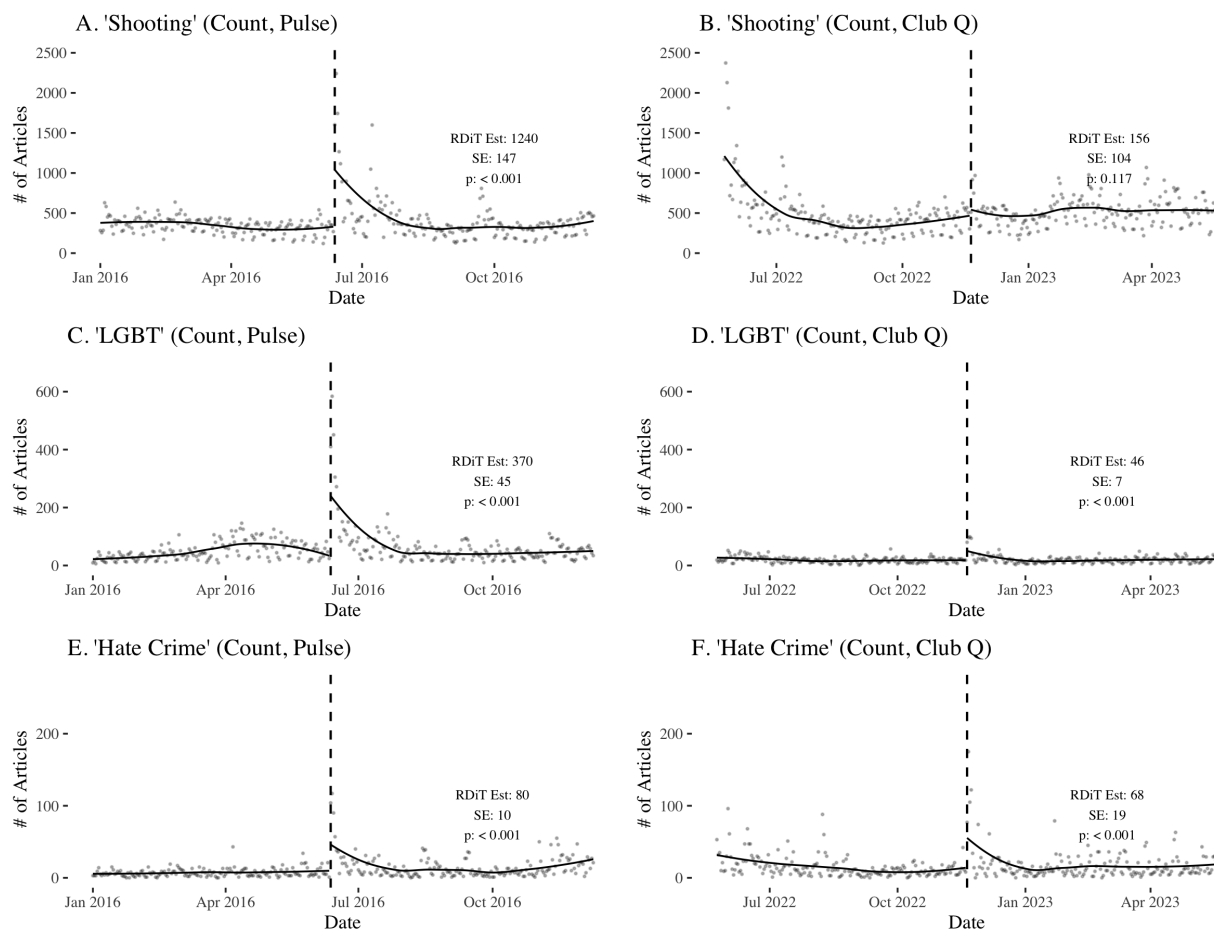
E.1.2 Mediacloud

Figure E19: Count of News Articles Related to Violence Against LGBTQ+ People Six Months Before and After the Pulse Massacre and Club Q Massacre. Panels A-B, C-D, and E-F characterize the count of news articles (y-axis) over time (x-axis) containing the phrases "shooting," "LGBT," and "hate crime" respectively. Panels A, C, E and B, D, F characterize the count of articles over time 6 months before and after the Pulse and Club Q massacres respectively. Dashed vertical line denotes the moment the respective massacres occurred. The dark line characterizes a loess model fit on each side of the moment the respective massacres occurred. Data are from Mediacloud, an open-source platform for media analysis (see: https://www.mediacloud.org/). Annotations denote regression discontinuity-in-time estimates characterizing the effect of the respective massacres on the count of articles related to specific phrases (polynomial degree = 1, kernel = uniform, using CCT optimal bandwidth selection, see Calonico et al. (2015)).
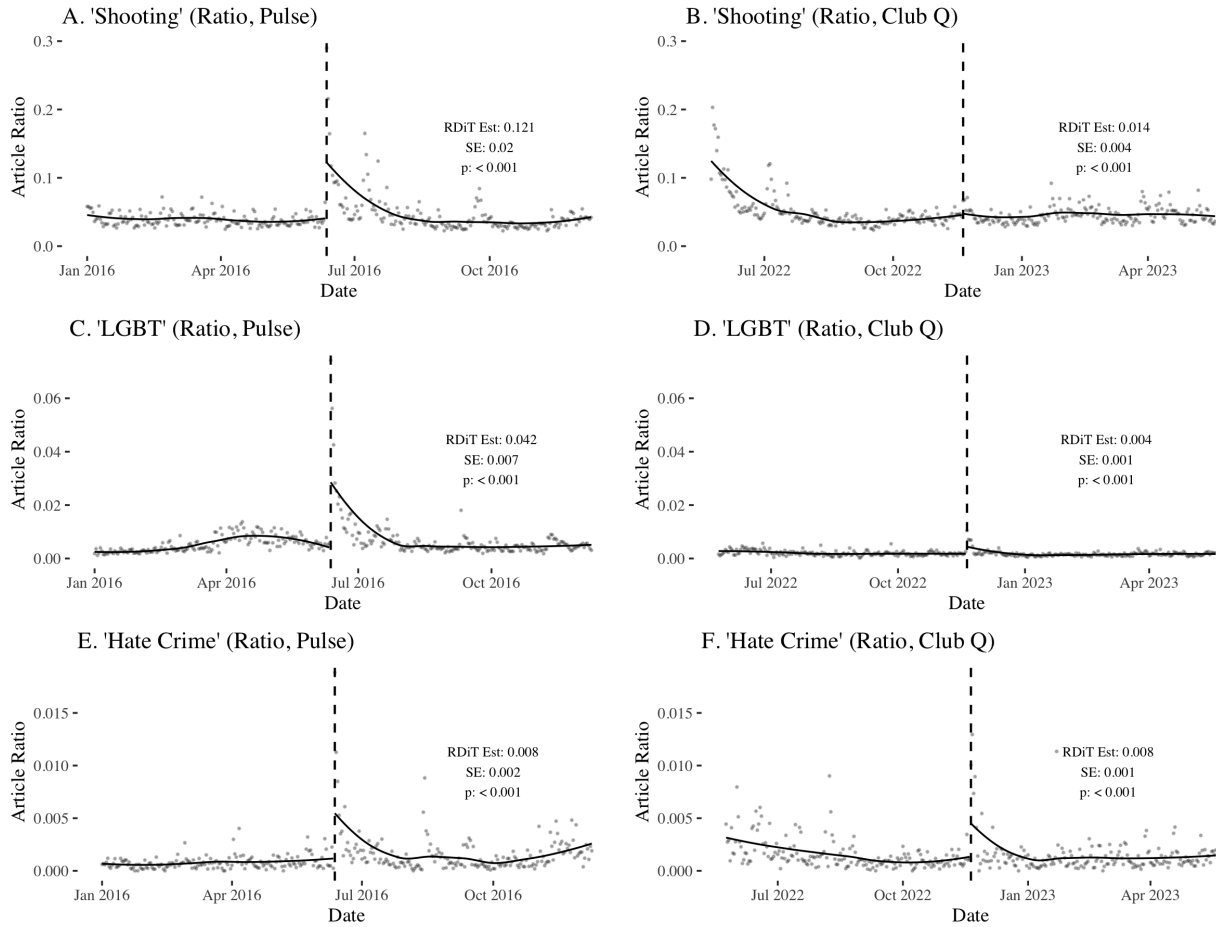
21

Figure E20: Ratio of News Articles Related to Violence Against LGBTQ+ People vis-a-vis All News Articles Six Months Before and After the Pulse Massacre and Club Q Massacre. Panels A-B, C-D, and E-F characterize the ratio of news articles (y-axis) over time (x-axis) containing the phrases "shooting," "LGBT," and "hate crime" over all news articles respectively. Panels A, C, E and B, D, F characterize the count of articles over time 6 months before and after the Pulse and Club Q massacres respectively. Dashed vertical line denotes the moment the respective massacres occurred. The dark line characterizes a loess model fit on each side of the moment the respective massacres occurred. Data are from Mediacloud, an open-source platform for media analysis (see: https://www.mediacloud.org/). Annotations denote regression discontinuity-in-time estimates characterizing the effect of the respective massacres on the count of articles related to specific phrases (polynomial degree = 1, kernel = uniform, using CCT optimal bandwidth selection, see Calonico et al. (2015)).

Table E8: Assessing Coefficient Differences Between *Post-Pulse* and *Post-Club Q* on Media Salience

Outcome	Topic	RDiT Coef. (Pulse)	RSE (Pulse)	RDiT Coef. (Club Q)	RSE (Club Q)	Coef. Difference	Difference t stat.	Difference p value
Count	Shooting	1240.232	147.036	155.632	103.825	1084.601	6.348	0.000
Count	LGBT	369.582	44.839	46.168	7.430	323.414	7.129	0.000
Count	Hate Crime	80.012	10.373	68.077	18.824	11.935	0.596	0.553
Ratio	Shooting	0.121	0.020	0.014	0.004	0.108	5.182	0.000
Ratio	LGBT	0.042	0.007	0.004	0.001	0.038	5.420	0.000
Ratio	Hate Crime	0.008	0.002	0.008	0.001	0.000	0.013	0.989

Note: All RDiT estimates use a uniform kernel and polynomial degree equal to 1 along with the optimal bandwidth selection mechanism by Calonico et al. (2015). Robust SEs displayed.
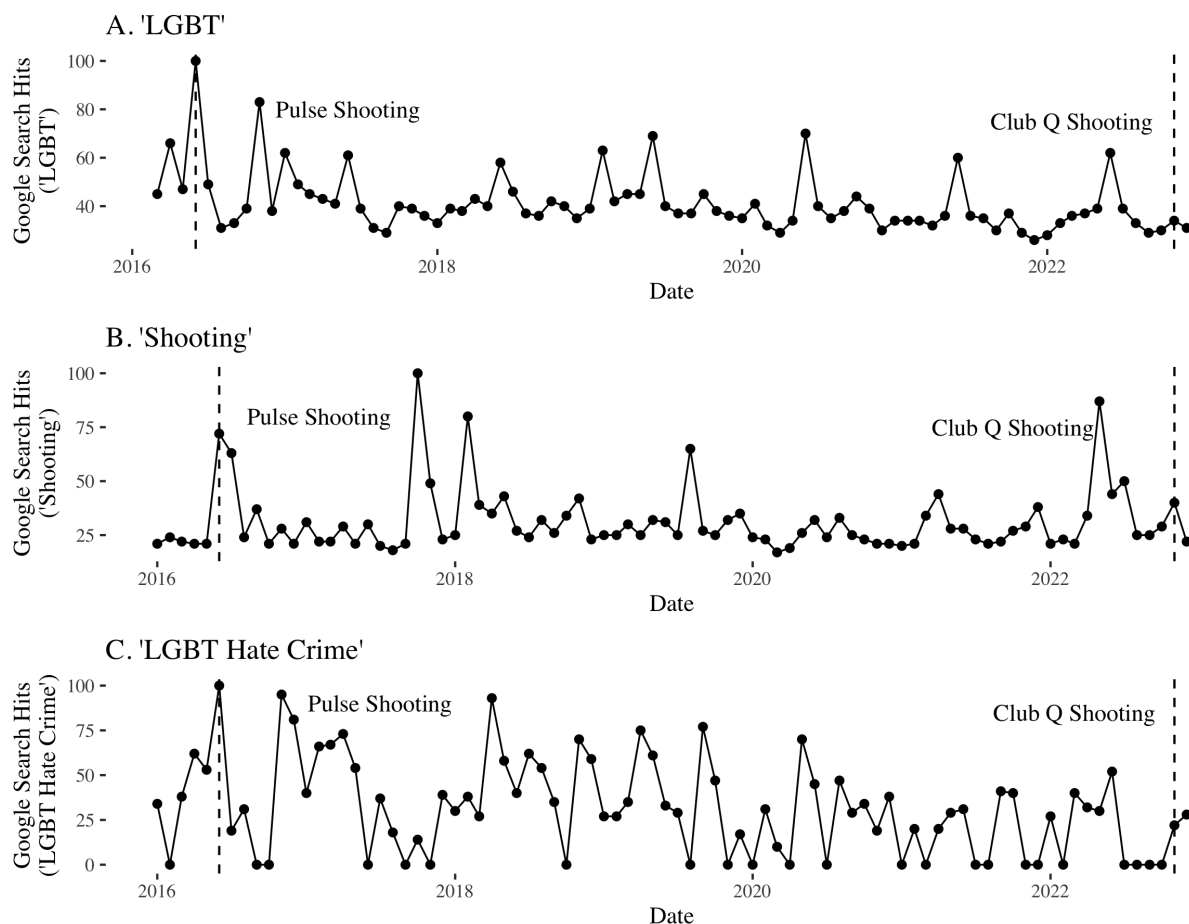
E.1.3   Google Trends

Figure E21: Google Search Intensity On Topics Related to LGBT, Hate Crimes, and Mass Shootings Over Time (2016-2022). The x-axis is month, the y-axis is the normalized search intensity for a particular search topic between 2016-2022. From left to right, dashed vertical lines denote the moment of the Pulse massacre and Club Q shooting. Panels A, B, and C characterize search intensity for the following search terms: "LGBT," "shooting," and "LGBT hate crime."

E.2 Balance Tests
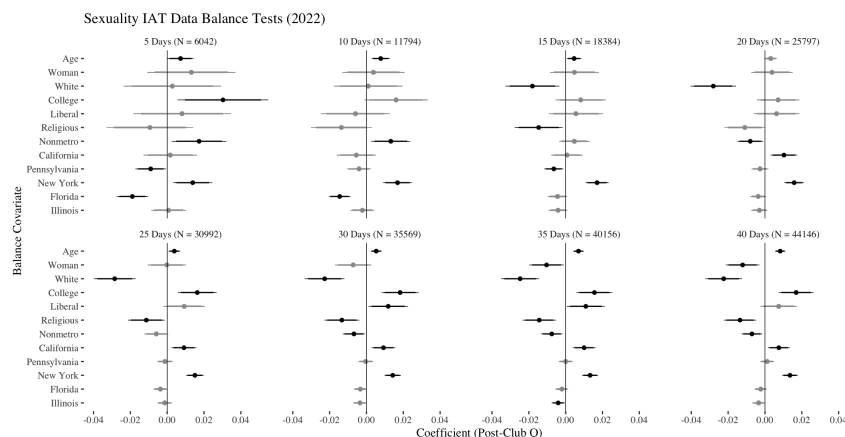
E.2.1 Project Implicit Sexuality IAT Data (2022)

Figure E22: Covariate Balance Between Project Implicit Sexuality IAT Survey-Takers Before and After Club Q Massacre. Each coefficient is from a separate model regressing a balance covariate (y-axis) on a binary indicator for taking the Sexuality IAT after the Club Q massacre (*post-Club Q*). Each panel characterizes the sample bandwidth at use (1-40 days from the Club Q massacre) and sample size. Statistically significant coefficients are black, grey otherwise. 95% CIs displayed derived from HC2 robust standard errors.

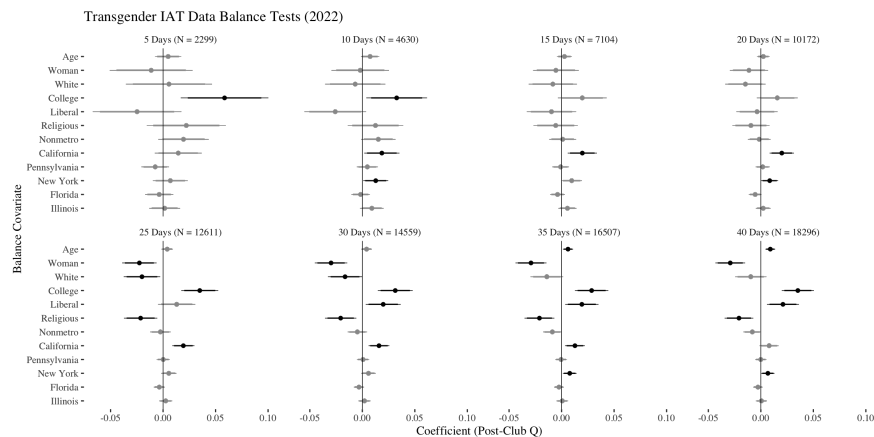### E.2.2 Project Implicit Transgender IAT Data (2022)

Figure E23: Covariate Balance Between Project Implicit Transgender IAT Survey-Takers Before and After Club Q Massacre. Each coefficient is from a separate model regressing a balance covariate (y-axis) on a binary indicator for taking the Transgender IAT after the Club Q massacre (*post-Club Q*). Each panel characterizes the sample bandwidth at use (1-40 days from the Club Q massacre) and sample size. Statistically significant coefficients are black, grey otherwise. 95% CIs displayed derived from HC2 robust standard errors.

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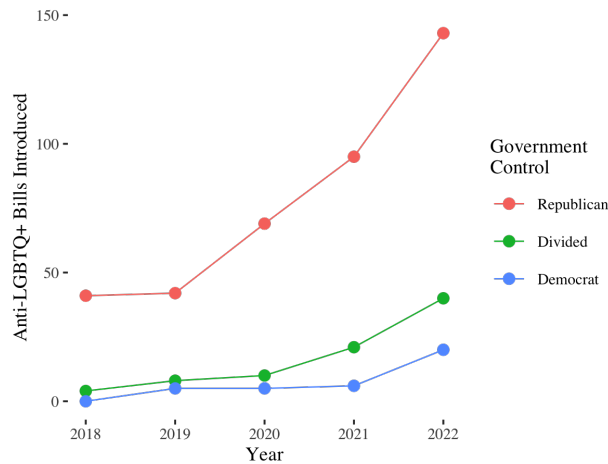## E.3 State-Level Anti-LGBTQ+ Bills Over Time By Partisan Control

Figure E24: Number of State-Level Anti-LGBTQ+ Bills Introduced Over Time By Partisan Control. X-axis is year, y-axis is the number of anti-LGBTQ+ bills introduced. Color denotes state government partisan control of governorship, upper, and lower house. Data on bill introductions are from the American Civil Liberties Union.

Data on partisan control of state government are from Grumbach (2022). Data on the introduction of anti-LGBTQ+ bills between 2018-2022 are from the American Civil Liberties Union (ACLU).[49] The data include the following types of bills: **1) Anti-Transgender Bills,** which include a) Single-sex facility restrictions; b) First amendment defense actions and religious exemptions; c) Restrictions on identification documents; d) Restrictions on health care/gender-affirming care; e) Restrictions on athletics. **2) Broader Anti-LGBTQ bills**, which include a) Religious exemption bills; b) Religious freedom restoration acts; c) First amendment defense acts; d) Health care access restrictions; e) Adoption and foster care restrictions; f) Marriage-related exemptions; g) Restrictions on schools and student organizations; h) Bills preempting local protections.

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[49]See https://www.aclu.org/past-legislation-affecting-lgbt-rights-across-country-2018, https://www.aclu.org/past-legislation-affecting-lgbt-rights-across-country-2019, https://www.aclu.org/past-legislation-affecting-lgbt-rights-across-country-2020, https://www.aclu.org/legislation-affecting-lgbtq-rights-across-country-2021, and https://www.aclu.org/legislation-affecting-lgbtq-rights-across-country-2022 for source data.

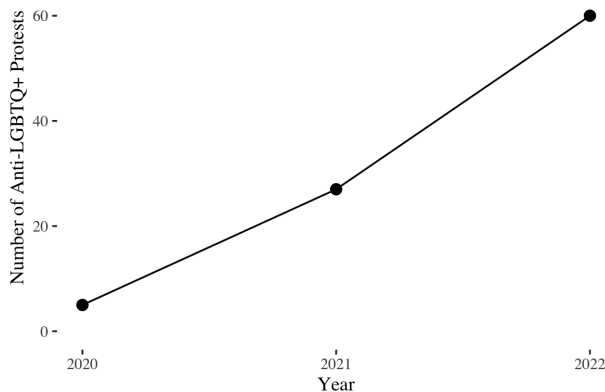# E.4 Anti-LGBTQ+ Right Wing Protests Over Time

Figure E25: Number of Right-Wing Anti-LGBTQ+ Protests Over Time (2020-2022). X-axis is year, y-axis is the number of anti-LGBTQ+ protests. Data are from ACLED (see: `https://acleddata.com/`, protest keyword = "anti-LGBT")

E.5 Evaluating Individual-Level Heterogeneity

E.5.1 Sexuality IAT

Table E9: Heterogeneous Influence of Club Q Massacre (S-IAT Dataset)

Interaction	Coefficient	SE	p-value	Dataset	Outcome	Bandwidth	N	R-Squared
Post-Club Q x Non-White	0.00	0.00	0.93	Sexuality IAT	D-Score (Anti-Gay)	20.00	24118	0.16
Post-Club Q x Woman	-0.00	0.00	0.71	Sexuality IAT	D-Score (Anti-Gay)	20.00	24118	0.16
Post-Club Q x Liberal	0.00	0.00	0.87	Sexuality IAT	D-Score (Anti-Gay)	20.00	24118	0.16
Post-Club Q x % LGBT (State)	0.00	0.00	0.59	Sexuality IAT	D-Score (Anti-Gay)	20.00	24118	0.17
Post-Club Q x SS Couple Density (County)	-0.00	0.00	0.68	Sexuality IAT	D-Score (Anti-Gay)	20.00	19057	0.17
Post-Club Q x Non-White	-0.01	0.01	0.26	Sexuality IAT	Straight Bias	20.00	24542	0.23
Post-Club Q x Woman	-0.01	0.01	0.08	Sexuality IAT	Straight Bias	20.00	24542	0.23
Post-Club Q x Liberal	-0.00	0.01	0.69	Sexuality IAT	Straight Bias	20.00	24542	0.23
Post-Club Q x % LGBT (State)	-0.00	0.00	0.78	Sexuality IAT	Straight Bias	20.00	24542	0.23
Post-Club Q x SS Couple Density (County)	-0.00	0.00	0.21	Sexuality IAT	Straight Bias	20.00	19492	0.23
Post-Club Q x Non-White	0.00	0.00	0.95	Sexuality IAT	Heterocentrism	20.00	24691	0.25
Post-Club Q x Woman	-0.01	0.00	0.15	Sexuality IAT	Heterocentrism	20.00	24691	0.25
Post-Club Q x Liberal	-0.00	0.00	0.49	Sexuality IAT	Heterocentrism	20.00	24691	0.25
Post-Club Q x % LGBT (State)	0.00	0.00	0.95	Sexuality IAT	Heterocentrism	20.00	24691	0.25
Post-Club Q x SS Couple Density (County)	-0.00	0.00	0.49	Sexuality IAT	Heterocentrism	20.00	19592	0.25

HC2 robust SEs reported. Each interaction coefficient is from a separate model.

E.5.2 Transgender IAT

Table E10: Heterogenous Influence of Club Q Massacre (T-IAT Dataset)

Interaction	Coefficient	SE	p-value	Dataset	Outcome	Bandwidth	N	R-Squared
Post-Club Q x Non-White	0.006	0.007	0.402	Transgender IAT	D-Score (Anti-Trans)	15.000	6185	0.116
Post-Club Q x Woman	-0.001	0.007	0.861	Transgender IAT	D-Score (Anti-Trans)	15.000	6185	0.116
Post-Club Q x Liberal	-0.008	0.007	0.197	Transgender IAT	D-Score (Anti-Trans)	15.000	6185	0.116
Post-Club Q x % LGBT (State)	-0.006	0.005	0.219	Transgender IAT	D-Score (Anti-Trans)	15.000	6185	0.117
Post-Club Q x SS Couple Density (County)	-0.002	0.001	0.072	Transgender IAT	D-Score (Anti-Trans)	15.000	4910	0.120
Post-Club Q x Non-White	-0.006	0.010	0.580	Transgender IAT	Cis Bias	15.000	6516	0.188
Post-Club Q x Woman	-0.024	0.011	0.035	Transgender IAT	Cis Bias	15.000	6516	0.189
Post-Club Q x Liberal	-0.007	0.010	0.512	Transgender IAT	Cis Bias	15.000	6516	0.188
Post-Club Q x % LGBT (State)	-0.007	0.006	0.216	Transgender IAT	Cis Bias	15.000	6516	0.190
Post-Club Q x SS Couple Density (County)	-0.001	0.002	0.624	Transgender IAT	Cis Bias	15.000	5179	0.183
Post-Club Q x Non-White	-0.009	0.007	0.163	Transgender IAT	Ciscentrism	15.000	6627	0.193
Post-Club Q x Woman	-0.008	0.008	0.287	Transgender IAT	Ciscentrism	15.000	6627	0.193
Post-Club Q x Liberal	-0.012	0.007	0.073	Transgender IAT	Ciscentrism	15.000	6627	0.193
Post-Club Q x % LGBT (State)	-0.004	0.004	0.276	Transgender IAT	Ciscentrism	15.000	6627	0.195
Post-Club Q x SS Couple Density (County)	0.000	0.001	0.825	Transgender IAT	Ciscentrism	15.000	5252	0.190

HC2 robust SEs reported. Each interaction coefficient is from a separate model.

F   Less Salient Violent Events

F.1   Salience: Search and Analysis Rules

Here, we assess the salience of several relatively prominent anti-LGBTQ+ violent events relative to the Pulse massacre, Matthew Shepard's murder, and the Club Q massacre between 2000-2022. The universe of events we assess is from this crowd-soruced list: `https://en.wikipedia.org/wiki/History_of_violence_against_LGBT_people_in_the_United_States`. To assess salience, we assess the number of search hits related to each event from the New York Times.

The Google search term we use to assess salience is: site:nytimes.com "[name of victim]" AND LGBT OR LGBTQ OR gay OR lesbian OR bisexual OR queer OR transgender OR trans OR homophobic OR transphobic AND attack OR assault OR murder OR kill OR killed OR killing OR death"

In cases where a particular place is attacked (e.g. Pulse, or Club Q), we replace "name of victim" with the place the attack occurred (e.g. "Pulse," "Club Q").

F.2 Salience of Less Salient Violent Events (2000-2022)

A. Salience of Violent Incidents Against
LGBTQ+ Group Members
(2000-2009)

B. Salience of Violent Incidents Against
LGBTQ+ Group Members
(2010-2022)

C. Salience of Shepard's Murder,
Pulse Massacre, Club Q

Matthew Shepard (1998-10-12) — 442 Hits

Pulse Massacre (2016-06-12) — 3570 Hits

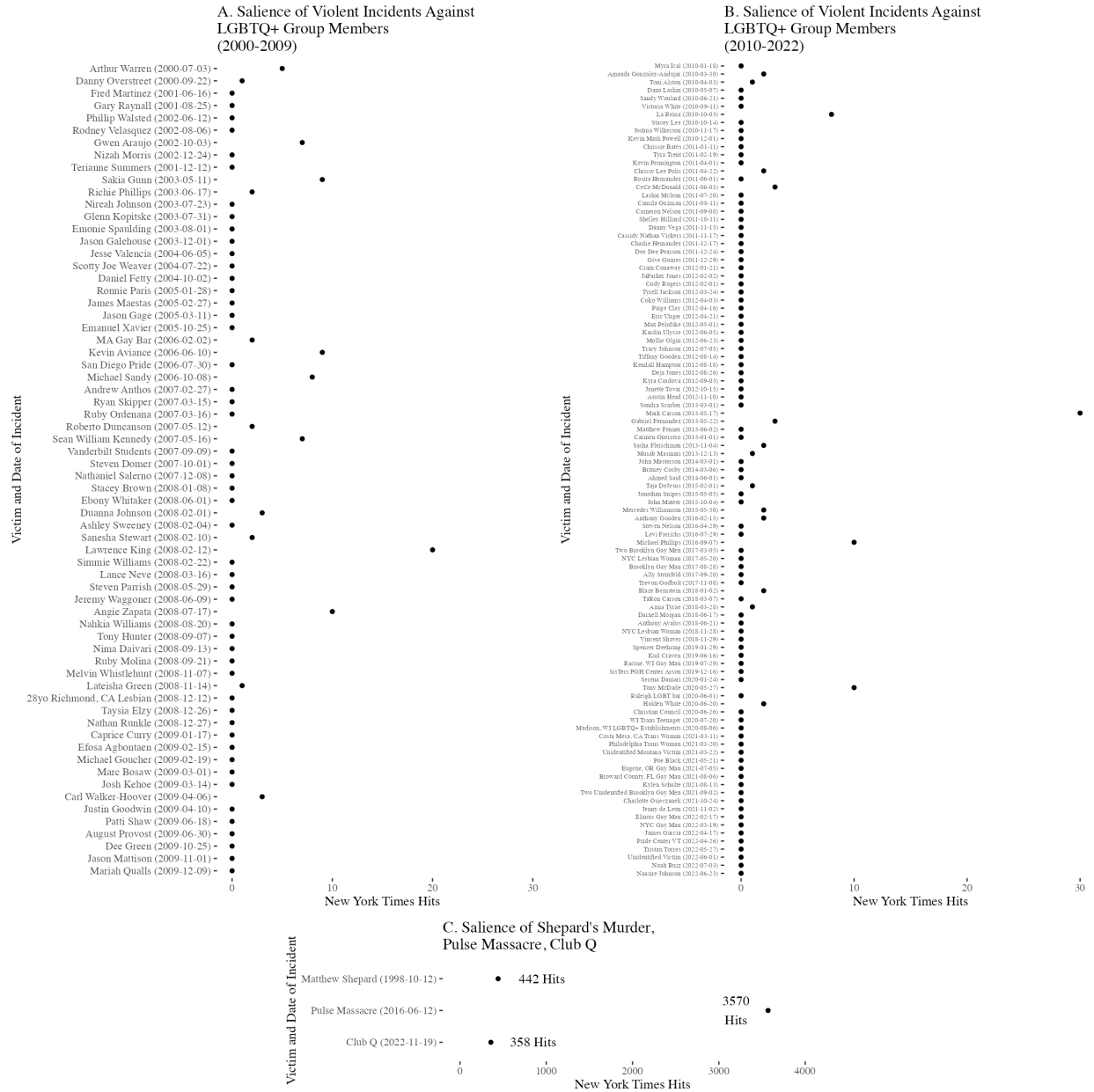Club Q (2022-11-19) — 358 Hits

Figure F26: Salience of Less Salient Violent Incidents Against LGBTQ+ Group Members Relative to the Pulse Massacre, Shepard's Murder, and the Club Q massacre. Panels A/B characterizes the salience (x-axis, number of NYT articles) of incidents (y-axis) from 2000-2009/2010-2022. Panel C characterizes the salience of Shepard's murder, the Pulse massacre, and the Club Q massacre. Annotations denote number of New York Times hits. See Section F.1 for information on measurement of violent incidents and salience.

Assessing Influence of Less Salient Violent Events on Prosocial Attitudes (2010-2022)
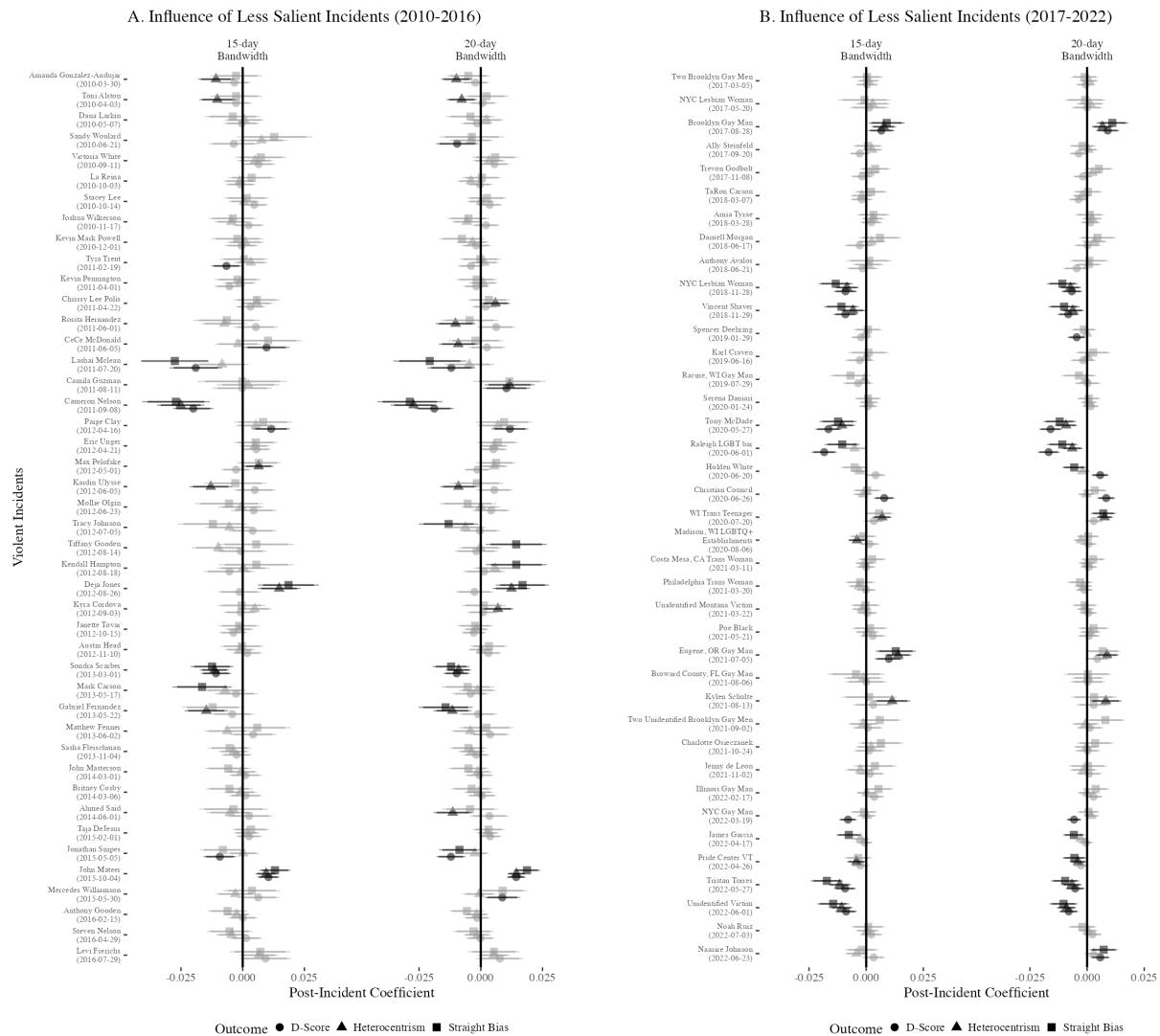

Figure F27: Influence of Less Salient Violent Incidents Against LGBTQ+ Group Members on Prosocial Attitudes Toward Gay People. Panels A/B characterize the influence of incidents on prosocial attitudes from 2010-2016/2017-2022. The x-axis is the post-incident coefficient, the y-axis is the name of victim and date of the respectively violent incident. Shape denotes outcome at use (*D-score, heterocentrism, straight bias*). Grey coefficients are statistically insignificant, black otherwise. Each panel contains two facets using data 15 days before and after the respective violent incident (left) and 20 days before and after the incident (right). 95% CIs displayed derived from HC2 robust SEs. See DSM Tables 187-363 for regression tables characterizing placebo and control coefficients displayed here.

In this analysis, we examine the influence of less salient violent incidents against LGBTQ+

group members on prosocial attitudes toward gay people between 2010-2022 (see Figure F26, see also `https://en.wikipedia.org/wiki/History_of_violence_against_LGBT_people_in_the_United_States`). Similar to Studies 2 and 4, we use Project Implicit Sexuality Implicit Association Test surveys on U.S. adults from 2010-2022 to conduct this analysis. In the analysis, we exclude less salient incidents where 1) there were days of missing data 15 and 20-days before and after the onset of a particular violent incident and 2) there were not 20 days of pre-treatment data for each respective yearly survey (e.g. if an incident occurred on January 7th in a particular year, where there is only 6 days of pre-treatment data for that particular year). Like Studies 2 and 4, We assess the effect of each incident on the *D-score*, *straight bias*, and *heterocentrism*.

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