High-Profile Police Violence, Public Opinion, and Police Reform*

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Abstract

This paper provides the first systematic analysis of how high-profile police killings affect state policing legislation and public attitudes toward the police. Using a novel dataset on state-level policing laws, I find that these incidents generate large legislative responses, concentrated in Democratic-controlled legislatures and among incidents drawing greater media and protest attention. They also polarize public opinion: support for police falls among Independents and Democrats but remains unchanged among Republicans. Finally, I provide descriptive evidence that public opinion matters for legislative responses: Democratic legislators sponsor more policing bills when constituent support for police declines, while Republicans show no such response. *JEL Codes:* D72, H11, H76, J15, K14, K42, L82.

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1 Introduction

How do policymakers respond to high-profile events that capture intense public attention? A large literature shows that media coverage shapes public opinion and can affect short-term government action (DellaVigna & Kaplan, 2007; Djourelova, 2023; Durante & Zhuravskaya, 2018; Eisensee & Strömberg, 2007; Enikolopov et al., 2011; Snyder & Strömberg, 2010). Yet far less is known about whether media salience produces lasting policy change, and how policymakers translate public pressure into enacted law, one of the most permanent and consequential forms of policy response. In this paper, I study this question in the context of high-profile police violence, arguably one of the most publicly salient and politically consequential events in recent American history. Cases such as the 2014 fatal shooting of Michael Brown and the 2020 murder of George Floyd captured intense national attention, sparked sustained public debate over policing practices, and prompted millions of Americans to take to the streets (Buchanan et al., 2020; Reny & Newman, 2021).

Nonetheless, there exists little causal evidence on the effects of high-profile police violence on policing reform. Recent studies find that exposure to police violence mobilizes voters and increases public support for justice reform (Ang & Tebes, 2024; Morris & Shoub, 2024). However, whether these shifts in expressed preferences — and the protests these incidents spark — ultimately translate into meaningful changes in policing policy remains largely unresolved. Importantly, high-profile killings may create "policy windows" during which heightened media attention and constituent pressure make legislatures more receptive to change (Luca et al., 2020), but the magnitude and form of these policy responses remain an open empirical question.

This paper provides the first systematic empirical analysis of the causal effects of high-profile police violence on state-level policing regulations and public attitudes toward the police, and how those attitudes influence legislative responses.[1] I first use data on high-profile police killings from Premkumar (2025b) and construct a novel dataset of U.S. state-level policing-related legislation spanning 2005 to 2016. In this period, state legislatures introduced more than 20,000 policing-related bills, of which roughly 6,300 became laws. Over the same period, Premkumar identifies 79 high-profile incidents, including well-known cases such as Michael Brown and Freddie Gray.

I then exploit the plausibly exogenous timing of high-profile police killings to estimate their causal effects on policing legislation and public support for police using a difference-in-differences design. Specifically, I compare bills introduced and passed before and after these incidents in affected states relative to other states, and support for police in affected counties relative to other counties. Given that high-profile police killings are typically major policing scandals involving salient features of alleged police misconduct, such as the killing of unarmed civilians, the estimates

[1] Ang and Tebes (2024) find that police violence raises support for reform, but study all police killings in Los Angeles, while Ang et al. (2025) and Reny and Newman (2021) find reduced community trust but focus only on George Floyd.

are best understood as capturing the effect of a compound event comprising the killing, its victim and incident characteristics, and the media attention it generates, rather than the effect of police killings per se.

In the main legislative analysis, I find that high-profile police killings evoke large legislative responses. A single high-profile police killing is associated with a 17.2% increase in policing-related bills introduced and a 9.5% increase in bills passed within a state in the year following the incident. Event study analysis confirms that legislative activity in treated and control states follows similar trends prior to high-profile incidents, supporting the identifying assumption underlying the difference-in-differences design. The dynamic estimates further reveal that high-profile police killings generate persistent but not permanent shocks to state legislative activity, with effects strongest in the current and following legislative cycles.

The legislative responses are stronger for incidents that receive greater media attention and generate more protest activity, as measured by the number of news articles covering the incident, the volume of protest coverage, and whether the incident appears in national television news. Moreover, the responses are primarily driven by Democratic-controlled legislatures, while Republican-controlled legislatures show little response, consistent with well-documented partisan differences in support for police reform (Ekins, 2016; Gallup, 2020; Jones, 2013). Examining individual policy categories, I find that Democratic-controlled legislatures pass significantly more of both reform-oriented bills, such as those related to use of force, policing alternatives, and technology, and officer-supportive bills, such as those related to officer benefits.

To support the causal interpretation of these results, I conduct several exercises that provide evidence for the identifying assumption, rule out alternative explanations, and test the robustness of the main estimates. First, I conduct an event study of media attention to the police around the timing of high-profile incidents. I find no evidence of elevated media coverage of police violence, police reform, or policing in general prior to the incidents, while coverage of all three topics spikes sharply afterward. This pattern supports the assumption that the timing of these incidents is plausibly quasi-random, rather than driven by pre-existing media attention or demand for reform.

Second, I rule out that the legislative responses are driven by police violence more generally or by the Black Lives Matter (BLM) movement, the most likely confounder during the sample period. I find that total police killings have minimal effects on overall legislative activity, and that the main results remain highly robust when restricting the sample to the pre-BLM periods. Third, I show that the main results are highly robust across a wide range of alternative specifications, including alternative constructions of the dependent and independent variables, different functional forms, and estimators that are robust to heterogeneous treatment effects.

In the second part of the paper, I use data from the Cooperative Election Study (CES) to study how high-profile police killings affect public support for the police. Because the incidents

in Premkumar end in 2016, they overlap with only two CES waves in which the police funding question is available. I therefore extend the list of high-profile incidents through 2023 using national television news coverage as a proxy for high-profile status, covering some of the most salient cases in recent years, including the killings of George Floyd in 2020. Using the extended set of incidents and CES waves from 2014 to 2024, I conduct an event study of support for police funding and find no evidence of differential trends in support prior to the incidents. Following an incident, support for police funding declines by a lasting 5 percentage points, roughly 10% relative to the sample mean. Moreover, this decline reflects a partisan divide: support falls among Democratic and Independent respondents but not among Republicans.

Results using the original Premkumar incidents and CES waves from 2014 to 2016 display a similar pattern: overall support for police funding declines, driven mainly by Democratic and Independent respondents, while Republican respondents exhibit a marginally significant increase in support. In contrast, in both samples, high-profile police killings have no effect on support for other spending categories that Democrats typically favor, such as education and welfare, suggesting that the decline reflects a response specific to the police rather than a broader shift in political preferences.

In the final part of the paper, I provide descriptive evidence on how the aforementioned shifts in public support for the police relate to legislative activity. To do so, I collect data on the sponsors of policing-related bills passed in state legislatures, match each sponsor to their party affiliation and the county they represent, and construct a county-level panel of legislative activity spanning the 2014 to 2016 legislative cycles. I find that both high-profile police killings and declines in constituent support for the police are associated with greater legislative activity at the county level. Consistent with the partisan pattern in public opinion and with the larger responses in Democratic-controlled legislatures at the state level, this response is driven primarily by Democratic legislators. These patterns suggest that public opinion is an important channel through which high-profile police violence shapes legislative responses.

This paper contributes to several strands of literature in economics and political science. First, it adds to work examining the political consequences of high-profile crime-related incidents. Existing research largely focuses on mass shootings and school shootings, finding that such incidents mobilize voters (Hassell et al., 2020; Yousaf, 2021), increase interest group activity (Baldwin et al., 2026), and evoke legislative activity at state legislatures (Luca et al., 2020). Recent work on police killings finds that exposure to police violence increases voter participation and support for criminal justice reform (Ang & Tebes, 2024; Morris & Shoub, 2024), while highly salient incidents reduce community engagement with law enforcement (Ang et al., 2025; Reny & Newman, 2021). To my knowledge, this paper is the first to examine how high-profile police violence affects policing legislation and how shifts in public opinion following these incidents relate to legislative responses. Moreover, the

paper shows that high-profile police killing incidents have polarizing effects on public attitudes toward the police, contributing to the literature on the determinants of political polarization (Autor et al., 2020; Mian et al., 2014; Yousaf, 2021).

This paper also relates to the extensive literature on the economics of policing. Existing work examines how policing-related policies affect policing performance (Adger et al., 2026; Baicker & Jacobson, 2007; DeAngelo & Hansen, 2014; McCrary, 2007; Miller & Segal, 2019), how non-policy channels such as peer effects and political rallies affect police behavior (Grosjean et al., 2023; Holz et al., 2023; R. Rivera, 2025), and how high-profile police violence affects policing activity, showing that following negative media shocks or formal investigations, police officers tend to reduce proactive policing efforts to avoid additional scrutiny (Cheng & Long, 2022; Devi & Fryer, 2020; Premkumar, 2025b; R. G. Rivera & Ba, 2025; Shi, 2009). However, little attention has been paid to what motivates policymakers to implement policing regulations in the first place. This paper fills this gap by showing that high-profile police violence generates significant legislative responses at the state level.

Finally, this paper contributes to the broader literature on the political economy of mass media. Prior research documents that media coverage plays a crucial role in disciplining politicians and shaping government decisions (Besley & Burgess, 2002; Clinton & Enamorado, 2014; Djourelova & Durante, 2022; Durante & Zhuravskaya, 2018; Eisensee & Strömberg, 2007; Ferraz & Finan, 2008; Snyder & Strömberg, 2010). Research on media attention and policing also shows that media framing matters for how readers perceive police killings, and that police officers respond to local media attention on crime (Mastrorocco & Ornaghi, 2025; Moreno-Medina et al., 2025). This paper adds to this literature by studying how salient police killings that attract media coverage shape both public opinion and legislative responses.

The remainder of this paper is organized as follows. Section 2 describes the data and institutional background. Section 3 discusses the identification strategy. Section 4 presents the main results on legislative responses to high-profile police violence. Section 5 examines the effects on public opinion and how shifts in constituent attitudes shape legislator behavior. Section 6 concludes.

2 Background and Data

2.1 Background on Police Reform

There has been a surge in public demand for reforms to policing practices in the U.S. over the past two decades, sparked by a series of high-profile police killings that captured national attention.[2] These

[2]Notable incidents include the death of Oscar Grant in 2009, the killings of Michael Brown and Freddie Gray between 2014 and 2015, and the murder of George Floyd in 2020 (Buchanan et al., 2020; Garrett, 2023; Subramanian &

incidents, often involving unarmed racial minorities during seemingly routine police encounters, have intensified public scrutiny of police behavior and fueled demands for reforms in use of force, accountability, and policing standards.

While there have been some efforts to advance policing reform at the federal level, [3] frontline police officers are governed primarily by state and local law, making state legislatures and city councils the main engines of meaningful policing reform.[4] (Ray & Moore, 2024; Subramanian & Skrzypiec, 2017). Over the past decade, state-level policing regulation has been at the center of police reform efforts.[5] Given the importance of state-level policing regulations, research centers such as Duke Law's Wilson Center for Science and Justice (Garrett, 2023) and legislative organizations such as the National Conference of State Legislatures (NCSL, 2020) have begun building databases to track the introduction and enactment of state-level policing legislation since 2020. Moreover, reports from public policy organizations document substantial variation across states in both the volume and passage rate of policing-related legislation (Ray & Moore, 2024), and one purpose of this paper is to investigate this variation by linking high-profile police violence to legislative responses.

2.2 Data on Police Violence

2.2.1 Data on High-Profile Police Violence

This paper studies the effects of high-profile police violence on state-level policing regulations and public opinion toward the police. Data on high-profile police violence incidents are drawn from Premkumar (2025b), who defines high-profile incidents as those generating more than 1,000 news articles. Over the sample period of 2005 to 2016, there are 79 such incidents across the United States.[6] Table 1 summarizes the victim and incident characteristics of these incidents, along with the mean number of news articles covering incidents in each category. Consistent with common perceptions of police violence and with well-documented racial disparities in police use of force (Edwards et al., 2019; Fryer, 2019; Knox et al., 2020), the victims are predominantly male (86%)

Skrzypiec, 2017).

[3]For example, President Obama's Task Force on 21st Century Policing issued recommendations on training, investigations, data collection, and community engagement (President's Task Force on 21st Century Policing, 2015). Presidents Trump and Biden also issued executive orders addressing use-of-force standards, accountability, and police-community relations: Executive Order 13929 on Safe Policing for Safe Communities (The White House, 2020) and Executive Order 14074 on Advancing Effective, Accountable Policing (The White House, 2022).

[4]For example, the Chicago City Council expanded its prohibition on profiling to include national origin and gender identity in 2015 (SO2014-9752). Colorado enacted legislation limiting police use of chokeholds in 2016 (Colorado HB 1264).

[5]As the National Conference of State Legislatures notes, state law "can set the baseline for the standard of practice in states, creating a foundation for state regulatory agencies and local jurisdictions to expand upon" (NCSL, 2020).

[6]In his analysis of how high-profile police violence affects police behavior, Premkumar (2025b) uses 72 of these 79 incidents, excluding cases where the involved departments poorly reported data to the FBI.

and Black or Hispanic (84%). The vast majority were killed by gunshot (91%), and roughly 60% were unarmed at the time of the encounter and recorded on video. Panel A of Figure 1 shows a map of the locations of these incidents.

[Insert Table 1 here]

[Insert Figure 1 here]

What Makes an Incident High-Profile? To examine more formally what distinguishes high-profile incidents from the majority of police killings that receive little media attention, I estimate an incident-level linear probability model of high-profile status on victim and incident characteristics, as well as state-level political, crime, and media conditions.[7] Appendix Figure A1 reports the results. Consistent with Table 1, incidents involving Black victims, deaths by gunshot, unarmed victims, and video recordings are significantly more likely to become high-profile, while incidents involving fleeing, older, or male victims are less likely to. These effects are large relative to the base rate of 0.75%: a Black victim raises the probability of high-profile status by 1.9 percentage points, and an unarmed victim by 1.5 percentage points. In contrast, state political conditions, such as whether the state legislature is controlled by the Democratic Party, prior-year policing legislation and media coverage of policing, violent crime, and the overall level of police killings have no predictive power, suggesting that high-profile status is mainly driven by the ex ante characteristics of the victim and incident rather than by the state environment.

[Insert Appendix Figure A1 here]

To complement Premkumar's data, I collect three additional measures. First, I search individually for each incident's appearance in national television news across four major networks (CNN, NBC, ABC, and CBS) using data from the Vanderbilt Television News Archive (VTNA),[8] finding that 26 of the 79 high-profile incidents received national television coverage. Second, I construct a proxy for protest activity using news articles from LexisNexis Advance that mention "police," "protest," the name of the victim, and the location of the incident. Third, to provide supporting evidence for the quasi-random timing assumption of high-profile police killings, I use Factiva[9] to compile weekly state-level counts of news articles mentioning "police violence" and "police reform" as well as "police" as a measure of general police coverage into a state-by-week panel spanning 2005 to 2016.

[7]The analysis uses all police killings recorded in the Fatal Encounters database from 2011 to 2016, the period for which information on victims' armed status is available. The sample contains 9,600 incidents with non-missing victim and incident characteristics and covers 72 of the 79 high-profile incidents.

[8]The Vanderbilt Television News Archive is accessible online at https://tvnews.vanderbilt.edu. I also use the archive to construct the extended list of high-profile incidents through 2023, as described in Section 5.

[9]Factiva is a news database provided by Dow Jones, accessed through the University of Southern California library.

As placebo checks, I also compile counts of articles mentioning unrelated policy topics, including climate change, the minimum wage, and abortion rights.

2.2.2 Data on Non-High-Profile Police Violence

To contrast the effects of high-profile police killings, I additionally incorporate data on non-high-profile police violence as a placebo check for my main analysis, measured as the total number of police killings at the state-year level. Total police killings are collected from Fatal Encounters, which is recognized as the most comprehensive dataset on officer-involved fatalities available for the sample period (Cheng & Long, 2022; Moreno-Medina et al., 2025; Premkumar, 2025b).[10]

2.3 Data on Policing-Related Legislation

For the main outcome variable, I construct a comprehensive dataset on state-level policing-related legislation across all fifty states and the District of Columbia from 2005 to 2016 using the LexisNexis bill tracking service, following a similar data collection process to Garrett (2023) and NCSL (2020). Specifically, I search for all legislation mentioning "police" or "law enforcement" and collect information on bill introduction and passage dates and a synopsis of each bill's content.[11] Panel B of Figure 1 shows that state legislatures introduced more than 20,000 law-enforcement-related bills over this period, of which approximately 6,300 became laws. A salient feature of the data is the alternating peaks and troughs between odd and even years, reflecting the biennial structure of state legislative calendars (Luca et al., 2020).

[Insert Figure 1 here]

To classify bills by policy topic, I use the NCSL policing legislation database (NCSL, 2020) as labeled training data to build a topic classifier via supervised fine-tuning of a large language model, and apply the trained classifier to my 2005–2016 dataset. A detailed description of the training process is available in Appendix B. The classifier assigns each bill a binary indicator for each of the following categories: use of force, technology, policing standards, training and certification, police accountability and oversight, officer wellbeing and benefits, and policing alternatives, with unclassified bills coded as other. The model achieves an overall label-level accuracy of 93.89% and a weighted F1 score of 79%.[12] Summary statistics are reported in Table 2.

[Insert Table 2 here]

[10] The Fatal Encounters dataset is available online at https://fatalencounters.org.

[11] I do not include resolutions, ballot measures, executive orders, or draft bill requests in the analysis, as including the latter might result in double counting of legislation.

[12] The figures reported exclude the policing standards category, which is the worst-performing label due to its broad and overlapping definition. When including policing standards, the model achieves an accuracy of 92.66% and a weighted F1 score of 73.55%.

2.4 Data on Control Variables

The main baseline controls used in the legislative analysis include the party control of the state legislature and the party affiliation of the governor, with split legislature and Independent governor as the omitted groups. Following Luca et al. (2020), I also control for whether the legislature was in a regular session and whether the observation falls in the first year of the biennial legislative cycle. In the main specifications, I further include time-varying demographic and economic controls: the Black, Hispanic, and female shares of the population, age shares, the unemployment rate, log income per capita, the poverty rate, police officers employed per capita, and the log of total violent crime.[13]

Summary statistics for all variables used in the main legislative analysis are presented in Table 2.

[Insert Table 2 here]

2.5 Data on Public Attitudes toward the Police

In the second part of the paper, I study the effects of high-profile police violence on public attitudes toward the police using data from the Cooperative Election Study (CES), a biennial nationally representative survey (Kuriwaki, 2023; Schaffner et al., 2023). Specifically, I use the post-election waves from 2014 to 2024 for the event study analysis based on the extended set of high-profile incidents, and the 2014 and 2016 waves for the analysis based on the original Premkumar incidents. I measure public attitudes toward the police using the CES question on law enforcement funding, which asks respondents whether they support "greatly increasing," "slightly increasing," "maintaining," "slightly decreasing," or "greatly decreasing" financial allocations to law enforcement. I construct a binary indicator equal to one if the respondent selects "slightly increasing" or "greatly increasing," which serves as my proxy for support for the police. I also construct analogous binary indicators for support for welfare, education, transportation, and healthcare spending as placebo checks.[14]

2.6 Data on State Legislators

In the final part of the paper, I study how the shifts in public support for the police observed in the CES data relate to legislative activity. I collect data on winning state legislative candidates and their party affiliations from the State Legislative Election Returns Dataset compiled by Klarner

[13] Age, racial composition, and sex shares are drawn from the U.S. Census Bureau, as are income per capita and poverty rate. The unemployment rate is obtained from the Bureau of Labor Statistics (BLS). Data on violent crime and police officers employed per capita are collected from the Federal Bureau of Investigation (FBI) Uniform Crime Reports (UCR).

[14] Beginning in 2016, the CES also includes additional questions on attitudes toward the police, including support for increasing police presence, support for requiring police body cameras, whether respondents feel safe around the police (2016–2022), and ratings of local police (2016–2018). I use these questions to examine whether the results extend to other dimensions of attitudes toward the police.

(2018).[15] Using the bill sponsor information from the LexisNexis bill tracking service, I match each policing-related bill to its sponsoring legislator and, through the legislator-county link, to the county they represent.[16] I then construct a county-level panel of legislative activity covering the 2014 and 2016 legislative cycles.[17] Combined with the county-level changes in support for police funding derived from the CES data, I then examine how high-profile police killings and shifts in public attitudes relate to legislative responses.

3 Empirical Strategy

3.1 Effects of High-Profile Police Killing on Policing Legislation

This section describes the empirical strategy for the main legislative analysis; the empirical strategy for the public support analysis is introduced in Section 5. I implement a difference-in-differences (DiD) strategy that exploits the plausibly exogenous timing of high-profile police killings, comparing policing legislation before and after these incidents in treated states relative to other states. The dependent variables are state-year counts of policing-related bills introduced or enacted.[18] My preferred specification is a two-way fixed effects (TWFE) Poisson regression, which accounts for the count nature of the dependent variable (Wooldridge, 2010):

$$\mathbb{E}\left[y_{s,t} \mid \alpha_s, \lambda_t, PoliceKill_{s,t-1}, X_{s,t}\right] = \exp\left(\alpha_s + \lambda_t + \beta\, PoliceKill_{s,t-1} + \gamma' X_{s,t}\right), \tag{1}$$

where $y_{s,t}$ is the number of policing-related bills (or enacted laws) in state s and year t, α_s and λ_t are state and year fixed effects, $X_{s,t}$ contains baseline and time-varying controls, and $PoliceKill_{s,t-1}$ is either a binary indicator or count of high-profile killings in year $t-1$. I also show robustness to a linear OLS specification:

$$y_{s,t} = \alpha_s + \lambda_t + \beta\, PoliceKill_{s,t-1} + X'_{s,t}\gamma + \varepsilon_{s,t}. \tag{2}$$

In both specifications, robust standard errors are clustered at the state level.

Because treatment timing is staggered across states, TWFE estimates may be biased when treatment effects are heterogeneous across cohorts or over time (Baker et al., 2022; Callaway &

[15] The dataset is publicly available at https://www.klarnerpolitics.org/datasets-1.

[16] For bills introduced between 2004 and 2011, district boundaries come from the Census 2000-cycle State Legislative District Summary File; for 2012 through 2016, they come from the Census 2013 county–state legislative district relationship files, which reflect the districting plans in force for the 2012 elections.

[17] Each legislative cycle spans two years: the 2014 cycle covers 2013–2014 and the 2016 cycle covers 2015–2016.

[18] For bills passed, I assign the passage to the year of introduction rather than the year of enactment. For example, if a bill was introduced in 2005 but passed in 2006, it is counted as a passed bill in 2005. This ensures that the dependent variable more accurately captures the legislative response to high-profile incidents in the prior year.

Sant'Anna, 2021; de Chaisemartin & D'Haultfœuille, 2020; Goodman-Bacon, 2021). I therefore report results from two heterogeneity-robust estimators as robustness checks. First, I use a stacked difference-in-differences estimator (Baker et al., 2022; Cengiz et al., 2019), which constructs a separate dataset for each high-profile killing event using an event window from two years before to four years after the incident, compares the treated state only to states that remain untreated within that window, and stacks the event-specific datasets with cohort identifiers. Equations (1) and (2) are then augmented with cohort-by-state and cohort-by-year fixed effects. Second, I use the estimator of de Chaisemartin and D'Haultfœuille (2024), which accommodates non-binary and non-absorbing treatments that can switch on and off across periods.

Causal identification of the model hinges on the parallel trends assumption: absent high-profile police killings, treated and control states would have experienced similar trends in policing-related legislative activity. Importantly, the design does not require treated and control states to be comparable in levels. State fixed effects absorb time-invariant differences across states, such as baseline political culture or legislative capacity, while year fixed effects absorb common national shocks, including any nationwide legislative response to a particularly salient incident. Identification therefore comes from changes in legislative activity in treated states relative to contemporaneous changes in other states, after accounting for time-invariant state differences and common national shocks. The estimates thus capture the differential legislative response in states where high-profile killings occur, rather than the overall nationwide response to these incidents.

To provide support for the parallel trends assumption, I first aggregate each state's legislative activity into two-year biennia, which accounts for the substantial variation in legislation activities between odd- and even-numbered years and captures the full legislative cycle. I then conduct a distributed-lag event study following Freyaldenhoven (2025) and Freyaldenhoven et al. (2019), which accommodates overlapping, non-absorbing events of varying intensity.[19] Specifically, I estimate

$$\mathbb{E}\left[y_{s,b} \mid \alpha_s, \lambda_b, X_{s,b}\right] = \exp\left(\alpha_s + \lambda_b + \sum_{\substack{k=-3 \\ k\neq -1}}^{3} \beta_k \, \Delta PoliceKill_{s,b-k} + \gamma' X_{s,b}\right), \tag{3}$$

where b denotes the biennial legislative cycle, k indexes event time relative to a high-profile killing, $\Delta PoliceKill_{s,b-k}$ is the change in the number of high-profile killings, and $k = -1$ is omitted as the reference period. The endpoints $k = -3$ and $k = 3$ are binned, absorbing all biennia more than two periods before and after an incident. Under this specification, each β_k captures the cumulative effect of an additional high-profile killing as of event time k, relative to the omitted period. Coefficients on $k < 0$ test for pre-existing trends, while coefficients on $k \geq 0$ trace the dynamic legislative response.

[19] Distributed-lag event study designs are more commonly used in labor economics to study the dynamic effects of minimum wage changes, which similarly recur and vary in size across states (Dube et al., 2010; Leung, 2021).

I also estimate a linear OLS version of the distributed-lag model:

$$y_{s,b} = \alpha_s + \lambda_b + \sum_{\substack{k=-3 \\ k\neq -1}}^{3} \beta_k \, \Delta PoliceKill_{s,b-k} + \gamma' X_{s,b} + \varepsilon_{s,b}, \tag{4}$$

where all variables are defined as in Equation (3). As a further robustness check, I also report event study estimates from the heterogeneity-robust estimator of de Chaisemartin and D'Haultfœuille (2024), which accommodates designs where treatment timing varies across groups and treatment intensity may differ.[20]

4 Results

4.1 Effects of High-Profile Police Killings on Policing Legislation

4.1.1 Main Effects on Bill Introduction and Enactment

Table 3 presents the main results on the effects of high-profile police killings on policing-related bills introduced and passed in state legislatures. Panel A reports results for bill introductions, and Panel B reports results for bill enactments. Columns (1) through (4) report Poisson fixed-effects estimates from Equation (1). Column (1) includes state and year fixed effects with baseline political and institutional controls, Column (2) adds time-varying demographic and economic controls, and Column (3) further weights regressions by state population. Column (4) uses the same specification as Column (3) but aggregates bill counts to the biennial level, matching the specification used in the event study analysis. Columns (5) through (8) repeat the same structure using the linear OLS specification in Equation (2). My preferred specifications are reported in Columns (3) and (4).

[Insert Table 3 here]

Column (3) suggests that each high-profile police killing is associated with a 17.2% increase in policing-related bills introduced and a 9.5% increase in bills passed within a state in the year following the incident.[21] On average, this amounts to approximately 8.88 additional bills introduced (17.2% × 51.51 baseline mean) and 1.70 additional bills passed (9.5% × 17.79 baseline mean). The OLS estimates in Column (7) closely match these figures, implying 7.94 additional bills introduced and 1.93 additional bills passed. Column (4), which aggregates bill counts to the biennial level,

[20] As this estimator is restricted to linear specifications, it serves as a robustness check to my preferred Poisson specification. Estimates are implemented using the `did_multiplegt_dyn` Stata package of de Chaisemartin and D'Haultfœuille (2024).

[21] The percentage effects are computed from the Poisson coefficients as $100 \times [\exp(\hat{\beta}) - 1]$: $100 \times [\exp(0.159) - 1] = 17.2\%$ for bills introduced and $100 \times [\exp(0.091) - 1] = 9.5\%$ for bills passed.

yields broadly comparable estimates: each high-profile police killing is associated with a 13.4% increase in bills introduced and a 13.8% increase in bills passed over the following legislative cycle. The effect on bill passage is larger and more precisely estimated at the biennial level, consistent with enactment often taking longer than a single year to complete within a legislative cycle. Overall, these results provide strong evidence that high-profile police killings generate large legislative responses.

4.1.2 Dynamic Effects on Bill Introduction and Enactment

To investigate the dynamic effects of high-profile police killings on legislative activity, I estimate the event study specification in Equation (3) using the biennial bill counts from Column (4) of Table 3.[22] Panels A and B of Figure 2a present the Poisson event study estimates for bills introduced and bills passed, respectively. For robustness, the linear OLS estimates from Equation (4) and the heterogeneity-robust estimates of de Chaisemartin and D'Haultfœuille (2024) are presented in Appendix Figure A3a.

[Insert Figure 2a here]

[Insert Appendix Figure A3a here]

The Poisson estimates from Panels (a) and (b) of Figure 2a confirm that treated and control states follow similar trends in policing-related legislative activity during the two legislative cycles prior to high-profile incidents, providing support for the parallel trends assumption underlying the difference-in-differences design. Moreover, the legislative responses are strongest in the current and immediately following biennial cycle for bills introduced, with increases of 10.6% and 22.2%, respectively, and in the current and next two cycles for bills passed, with increases of 10.3%, 17.7%, and 27.3%, respectively. The effects then gradually fade, although the estimates for the final event-time bin are less precise, reflecting the smaller number of observations available at longer horizons, since most high-profile police killings in the sample occurred after 2012. These results suggest that high-profile police killings generate persistent but gradually fading shocks to state legislative activity, rather than representing a permanent shift in the level of policing-related legislation.

I obtain similar but slightly noisier results using the linear OLS specification and the heterogeneity-robust estimator of de Chaisemartin and D'Haultfœuille (2024), presented in Appendix Figure A3a. Across both alternative estimators, the treatment effects are similarly concentrated in the first two legislative cycles following the incident.

[22] Another advantage of the biennial outcome is that it reduces artificial gaps in treatment status. For example, if a state experiences high-profile killings in years t and $t+2$, the annual specification codes year $t+1$ as untreated, even though the state is in the midst of a sustained period of exposure. At the biennial level, the same state is instead treated in two consecutive legislative cycles, which yields a cleaner treatment path and improves the precision of the dynamic estimates.

4.2 Identification Checks, Alternative Explanations, and Robustness

In this section, I conduct several checks to assess the validity of the research design, examine alternative explanations, and test the robustness of the main results. Specifically, I first provide supporting evidence that the timing of high-profile incidents is plausibly quasi-random. I then examine whether the legislative responses reflect police violence more generally or are specific to high-profile incidents, and whether the main results are confounded by the Black Lives Matter movement. Finally, I show that the results are robust to a wide range of alternative specifications, including different constructions of the dependent and independent variables, alternative estimators, and different sets of controls.

4.2.1 Identification Checks

The main potential threat to identification is that high-profile incidents may occur in, or become salient because of, states already trending toward policing reform. In that case, legislative activity would have increased even absent the incident, or the incident's high-profile status would itself reflect pre-existing reform momentum. For example, if police-community relations were already deteriorating, or if there was pre-existing demand for police reform, media outlets might actively seek out incidents likely to make headlines. I address these concerns in several ways. First, the event study in Figure 2a shows no evidence of pre-existing trends in legislative activity; if anything, the pre-incident coefficients are slightly negative, moving in the opposite direction of the subsequent increase. Second, as shown in Appendix Figure A1, high-profile status is driven primarily by the ex ante characteristics of the victim and incident rather than by state-level political, crime, and media conditions, such as party control of the state legislature, the overall level of police violence, and prior media attention to the police.

Event Study of Media Attention Around High-Profile Incidents Next, I conduct an event study of media attention towards the police around the high-profile incidents. Using Factiva, I search for news articles mentioning "police violence," "police reform," or "police" in general, restricting results to each state separately, and compile the weekly count of articles mentioning these terms as a proxy for state-level media attention to the police. To test for pre-existing trends in media attention, I estimate event study regressions analogous to Equation (3), using the weekly count of news articles as the dependent variable, in a window of six weeks before and after each high-profile incident. Panels (a), (b), and (c) of Figure 3 show results for news articles on police violence, police reform, and the combined violence and reform coverage, respectively. Panel (d) shows results for general police coverage alongside a set of placebo topics: the minimum wage, abortion rights, and climate change. These placebo topics capture policy issues commonly associated with Democratic Party

priorities but are not directly related to police violence or policing reform.

[Insert Figure 3 here]

Figure 3 reveals a clear pattern. Prior to high-profile incidents, the coefficients for media coverage of police violence, police reform, and policing in general are close to zero, with no evidence of pre-existing trends. Immediately following an incident, coverage of all three topics spikes sharply, while coverage of the placebo topics shows no pre-trends and declines after the incident, consistent with high-profile incidents crowding out other news coverage (Eisensee & Strömberg, 2007). Taken together, these results suggest that the high-profile police killings used in the analysis are not driven by pre-existing media attention to policing or pre-existing tensions between local communities and law enforcement.

Randomization Test of Incident Timing As a further check on the quasi-random timing assumption, I test whether the high-profile incidents are disproportionately likely to occur while state legislatures are in session. If incidents were more likely to be politicized and become high-profile when legislatures are in session and reform momentum is building, one would expect high-profile incidents to cluster in active legislative sessions.

[Insert Appendix Figure A2 here]

To test this, I conduct a randomization exercise in which, for each incident, I hold the state and calendar year fixed and redraw the date uniformly within that state-year, repeating the procedure 200,000 times. Appendix Figure A2 reports the results. The observed number of incidents occurring during regular legislative sessions (41 of 79, or 51.9%) is nearly identical to the mean of the randomization distribution (41.16, or 52.1%) and falls well within the simulated 95% range of 35 to 48 incidents. Thus, there is no evidence that high-profile incidents cluster around active legislative sessions.

4.2.2 Alternative Explanations

The Black Lives Matter Movement The most likely confounder during the sample period (2005–2016) is the Black Lives Matter (BLM) movement. High-profile police killings, such as the killing of Michael Brown, helped elevate BLM to national prominence,[23] and if unobservable local BLM activity jointly drives both whether killings become high-profile and the demand for legislative reform, the causal interpretation of the main results would be jeopardized. To address this concern, Column (1) of Appendix Table A2 restricts the sample to the period before 2014, when the movement gained national prominence.

[23] See https://blacklivesmatter.com/our-history/ for a timeline of the Black Lives Matter movement.

[Insert Appendix Table A2 here]

The estimates remain highly significant for both bills introduced and bills passed, ruling out BLM activity as a potential explanation for the main results.

Non-High-Profile Police Violence I next examine whether the main legislative effects could be driven by police violence more generally, rather than by high-profile incidents specifically. To do so, I measure non-high-profile police violence as the total number of police killings in a state-year. Columns (2) through (4) of Appendix Table A2 include high-profile incidents and total police killings separately and jointly as explanatory variables.

Throughout the table, the effect of high-profile incidents remains positive and statistically significant, while total police killings have precisely estimated zero effects on both bills introduced and bills passed. Moreover, the high-profile estimates are unchanged when total police killings are included as a control (Column 2 versus Column 3). These results confirm that it is incidents receiving significant media attention, rather than police violence more broadly, that drive meaningful legislative responses.

4.2.3 Robustness Checks

Alternative Specifications Table 4 reports results from a wide range of alternative specifications. Columns (1) and (6) reproduce the preferred Poisson and OLS estimates from Columns (3) and (7) of Table 3. Column (2) excludes violent crime and police officers employed as controls, since Premkumar (2025b) finds that high-profile police killings have immediate effects on both, which could make them problematic controls. Columns (3) and (7) add state-specific linear time trends, absorbing any confounds that vary by state but evolve smoothly over time. Columns (4) and (8) use the stacked difference-in-differences estimator. Column (5) replaces the count of high-profile killings with a binary indicator and Column (9) reports the average total effect from the de Chaisemartin and D'Haultfœuille (2024) estimator on the biennial panel. Column (10) uses a log transformation of the outcome.

[Insert Table 4 here]

Across these specifications, the effect of high-profile police killings on bills introduced remains positive and statistically significant. The effect on bills passed is also robust in most specifications, although it is smaller when state-specific linear trends are included and imprecisely estimated when using a binary treatment indicator, suggesting that the passage response is driven primarily by the intensity of exposure rather than by the occurrence of any single incident. Overall, these results provide further support for the validity of the main findings.

Spillovers to Other States A related concern is that high-profile incidents may generate spillovers onto other states, so that some control states are potentially partially treated. Appendix Table A1 tests for this by additionally controlling for high-profile police killings in neighboring states and in states within the same Census division.

[Insert Appendix Table A1 here]

None of the spillover coefficients is statistically significant for either bills introduced or bills passed, while the main estimates remain positive, significant, and similar in magnitude to the baseline results. These results suggest that the control group is not substantially contaminated by spillovers from treated states.

4.3 Heterogeneity by Media Coverage, Protest Activity, and Political Party

As discussed in the introduction, high-profile police killings are best understood as compound events that combine the killing itself, the characteristics of the victim and incident, and the media coverage and protest activity the incident generates. In this section, I unpack this compound event by examining how the legislative response varies with media coverage and protest activity, and then examine how it varies with legislative party control. Table 5 reports the results, with Panel A showing results for bills introduced and Panel B for bills passed. Column (1) reports the baseline effects using the binary treatment indicator, while Columns (2) through (4) interact the treatment with media coverage, protest activity, and legislative party control, respectively.

[Insert Table 5 here]

Role of Media Coverage Premkumar defines high-profile incidents as those generating more than 1,000 news articles, but media attention varies substantially even among these incidents (Table 2). Column (2) of Table 5 interacts the treatment with the number of news articles covering each incident. For bills introduced, greater coverage amplifies the legislative response: each additional 1,000 articles is associated with an approximately 1% larger increase in bills introduced.

To complement this measure, Appendix Figure A5 separates high-profile incidents by whether they received national television news coverage, using data from the Vanderbilt Television News Archive (Durante & Zhuravskaya, 2018; Eisensee & Strömberg, 2007).

[Insert Appendix Figure A5 here]

Incidents covered on national television generate a response in bills introduced roughly four times as large as other high-profile incidents. For bills passed, the point estimate for nationally

televised incidents is also larger, though imprecisely estimated. Taken together, these results suggest that legislative responses, particularly bill introductions, are stronger for incidents that receive greater media attention.

Role of Protest Activity A distinctive feature of high-profile police killings is that they frequently spark large-scale protest activity (Buchanan et al., 2020). To examine whether protest activity amplifies legislative responses, I construct a proxy for protest activity using news articles collected from LexisNexis Advance that mention the term "police", "protest", the name of the victim, and the location of the incident. Column (3) of Table 5 reports the results.

Consistent with the view that protest activity generates greater political pressure on policymakers, Column (3) shows that incidents associated with more protest activity generate larger legislative responses. Specifically, each additional 1,000 news articles covering protest activity is associated with a 7.8% increase in bills introduced relative to the baseline incident effect. The effect on bills passed is positive but not statistically significant (0.033, s.e. 0.030), suggesting that passing legislation requires a higher degree of political consensus and is more difficult to achieve than bill introduction.

Role of Political Party Recent research shows that views on policing are deeply divided along partisan lines, with Republicans exhibiting substantially stronger support for the police than Democrats (Eckhouse, 2019; Ekins, 2016; Jones, 2013; Parker & Hurst, 2021). In this section, I examine the differential legislative responses to high-profile police killings across Democratic and Republican controlled state legislatures. Column (4) of Table 5 interacts the binary treatment indicator with indicators for Democratic and Republican legislative control, with split legislature serving as the omitted reference group.

To make the comparison clearer, Panels (a) and (b) of Figure 4a plot the total effect of a high-profile police killing for each type of legislature, combining the baseline and interaction coefficients. The legislative response is concentrated in Democratic-controlled legislatures. Following a high-profile incident, Democratic-controlled legislatures introduce 61.1% more and pass 24.7% more policing-related bills, while the corresponding effects for Republican-controlled legislatures are small and statistically indistinguishable from zero for both outcomes.

[Insert Figure 4a here]

These results indicate that the legislative responses to high-profile police violence are driven primarily by states with Democratic-controlled legislatures. In the next section, I further explore this heterogeneity by individual bill category.

4.4 Legislative Responses by Bill Type and Political Party

Main Effects Having established that high-profile police violence evokes large legislative responses at the state level, I now examine specifically what types of policing-related bills are being passed. To do so, I employ the policing legislation dataset from the National Conference of State Legislatures (NCSL, 2020) as training data for a policing bill topic classifier, which I use to categorize bills in my 2005–2016 dataset into specific policy areas. The details of the training and classification process are documented in Appendix B.

Using the labels obtained from the NCSL database, I classify bills into the following categories: use of force, technology, policing standards, training and certification, police accountability and oversight, officer wellbeing and benefits, and policing alternatives.[24] Bills that do not fit into any of these categories are coded as other. Since the classifier assigns a binary indicator for each category independently, a single bill can belong to multiple categories simultaneously. I then aggregate the number of bills belonging to each category to the state-year level as the dependent variable.

Figures 2a and 2b present the Poisson event study estimates by bill category, with robustness checks using the OLS and de Chaisemartin and D'Haultfœuille (2024) estimators reported in Appendix Figures A3a and A3b. Across most bill categories, the pre-incident coefficients remain close to zero, consistent with the parallel trends assumption.

[Insert Figures 2a and 2b here]

The event study estimates reveal that the legislative response unfolds differently across bill categories. Bills related to use of force increase in the current and following legislative cycle, while bills related to policing standards, officer benefits, and policing alternatives increase more gradually, with effects emerging or strengthening in later cycles. In contrast, the estimates for technology, training, accountability, and other bills are generally small and imprecise. The OLS and de Chaisemartin and D'Haultfœuille (2024) estimates are qualitatively similar for use of force and policing standards, but less consistent for officer benefits and policing alternatives, so the dynamic results for these two categories should be interpreted with caution.

Heterogeneity by Political Party As in the aggregate analysis, the legislative responses by bill category vary substantially with party control. Appendix Table A3 reports the static effects of high-profile police killings on the passage of each bill category, first reporting baseline effects and then adding interactions with legislative party control. To visualize the effects, Figures 4a and 4b plot the implied total effect of a high-profile police killing on each bill category by legislative party control.

[24] See Ba et al. (2025) and Ba et al. (2024) for recent research on policing alternatives.

[Insert Figures 4a and 4b here]

Consistent with the aggregate results, the responses for most bill categories are concentrated in Democratic-controlled legislatures, which pass significantly more bills related to use of force, policing alternatives, technology, and officer benefits following a high-profile incident. In contrast, the corresponding effects in Republican-controlled and split legislatures are mostly small and statistically insignificant. The main exception is police training, for which Democratic-controlled legislatures pass significantly fewer bills following an incident.

4.4.1 Additional Analysis on Policing Legislation

This section provides additional analyses of the main legislative results. Appendix Figure A4 examines the sensitivity of the legislative response to the definition of high-profile status by varying the minimum number of news articles required for an incident to be classified as high-profile. Appendix Figure A6 and Appendix Table A4 compare legislative responses to one versus multiple high-profile police killings within a state-year. Finally, Appendix Table A5 conducts a reverse causality check at the state-year level, examining whether prior legislative activity predicts the number of high-profile police killings in a state. This complements the incident-level analysis in Appendix Figure A1, which examines which police killings become high-profile, by testing whether states with more prior policing legislation experience more or fewer high-profile incidents.

5 Effects on Public Support for Police and Legislative Response

5.1 Effects on Public Support for Police

In this section, I investigate the effects of high-profile police violence on public support for the police. Recent research by Ang et al. (2025) and Reny and Newman (2021) shows that the high-profile killing of George Floyd reduced community engagement with and favorability toward law enforcement. This paper aims to provide a more comprehensive picture by examining a broader set of high-profile incidents spanning more than a decade. To do so, I use data from the Cooperative Election Study (CES), a repeated cross-section survey with 50,000 to 60,000 respondents per wave in election years. The CES has been widely used in prior research to study public attitudes on a range of topics, including views on immigration (Djourelova, 2023) and perceptions of the police (Moreno-Medina et al., 2025). As introduced in the Data section, I use respondents' support for police funding as a proxy for public support for the police. [25]

[25] Beginning in 2016, the CES also includes additional questions on attitudes toward the police, including support for increasing police presence, support for requiring police body cameras, whether respondents feel safe around the police

In this analysis, I combine the original Premkumar incidents with an extended list of high-profile incidents constructed from the Vanderbilt Television News Archive. Because the Premkumar incidents end in 2016, they overlap with only two CES waves in which the police funding question is available. The extended list allows me to conduct a more thorough event study covering CES waves from 2014 to 2024. Specifically, I search for every police killing recorded in the Fatal Encounters database from 2005 to 2022 in national television news coverage, using appearance on national television as a proxy for high-profile status.[26] This proxy is well suited to the setting: as shown in Appendix Figure A5, incidents that received national television coverage generate substantially larger legislative responses than other high-profile incidents, suggesting that they represent an even more salient group among high-profile cases.

Combining the two sources yields 129 high-profile incidents between 2005 to 2022, including 25 incidents occurring after 2016. Notable examples include the killings of Stephon Clark and George Floyd, which are among the most salient police killings in recent history.

5.1.1 Empirical Strategy

I estimate the effects of high-profile police killings on support for the police using two complementary specifications. The first is a respondent-level difference-in-differences regression:

$$\begin{aligned} Support_{i,c,t} = \alpha_c + \lambda_t + \beta\, PoliceKill_{c,t} + \beta_D\, PoliceKill_{c,t} \times Democrat_i \\ + \beta_R\, PoliceKill_{c,t} \times Republican_i + X'_{i,c,t}\gamma + \varepsilon_{i,c,t}, \end{aligned} \tag{5}$$

where $Support_{i,c,t}$ is a binary indicator equal to one if respondent i in county c and survey wave t supports increasing police funding, and α_c and λ_t are county and year fixed effects. $PoliceKill_{c,t}$ is an indicator equal to one in survey waves after county c first experiences a high-profile police killing. Treated counties are those first treated within the sample period, and control counties are those never treated. $X_{i,c,t}$ is a vector of individual-level controls.[27] The coefficient β captures the effect for Independents (the omitted group), while β_D and β_R measure the differential effects for Democrats and Republicans relative to Independents. All regressions use CES survey weights, and standard errors are clustered at the county level. I estimate this specification both for the original

(2016–2022), and ratings of local police (2016–2018). I use these questions to examine whether the results extend to other dimensions of attitudes toward the police.

[26] For each incident, I search Vanderbilt segment titles and descriptions using the victim's name, "police," and the location of the incident, covering the period from the incident date through 180 days afterward. I then manually verify that the matched segments concern the police killing in question, and classify an incident as high-profile only if it appears in at least two verified segments, to reduce noise from incidental mentions.

[27] Individual-level controls include gender, race and ethnicity (White, Black, Hispanic, Asian), marital status, voter registration status, educational attainment, age and age squared, homeownership, internet access at home, household income, party affiliation (Democrat, Republican), media consumption, and employment status (full-time, part-time, unemployed, retired).

Premkumar incidents using the 2014 and 2016 waves and for the extended set of incidents using the 2014–2024 waves

To examine dynamics and test for pre-existing trends, I collapse the extended sample to a county-by-wave panel and estimate the following event study specification:

$$\bar{Y}_{c,t} = \alpha_c + \lambda_t + \sum_{\substack{k=-3 \\ k\neq -1}}^{2} \beta_k \, \Delta PoliceKill_{c,t-k} + \varepsilon_{c,t}, \tag{6}$$

where $\bar{Y}_{c,t}$ is the share of respondents in county c and survey wave t who support increasing police funding, α_c and λ_t are county and wave fixed effects, and $\Delta PoliceKill_{c,t-k}$ is the change in treatment status k waves earlier, with $k = -1$ omitted as the reference period. Coefficients on $k < -1$ test for pre-existing trends, while coefficients on $k \geq 0$ capture the dynamic effects following the incident. I estimate Equation (6) using the heterogeneity-robust estimator of de Chaisemartin and D'Haultfœuille (2024), comparing treated counties with never-treated counties. Counties are weighted by their number of respondents, and standard errors are clustered at the county level.

In both the respondent-level and county-level analyses, I drop counties that experienced a high-profile police killing before the 2014 CES wave, so that previously treated counties never serve as controls for newly treated ones.

5.1.2 Effects on Public Support for Police: Results

Table 6 reports the results from Equation (5), and Figure 6 presents the event study estimates from Equation (6).

[Insert Table 6 here]

[Insert Figure 6 here]

Columns (1) through (4) of Table 6 report results using the original Premkumar incidents and the 2014 and 2016 CES waves, while Columns (5) through (8) use the extended set of incidents and the 2014–2024 waves. Columns (1) and (5) report the baseline specification with county and year fixed effects, and Columns (2) and (6) add interactions between the treatment and respondents' party affiliation. Columns (3), (4), (7), and (8) repeat these specifications with state-by-year fixed effects in place of year fixed effects.

Results from both samples are highly consistent: high-profile police killings reduce support for police funding by about 5 percentage points (Columns 1 and 5), roughly a 10% decline relative to the pre-treatment mean. More strikingly, the effects diverge sharply by party. Panels (a) and (b) of Figure 5 plot the total effect for each partisan group in the original and extended samples, respectively, combining the baseline and interaction coefficients.

[Insert Figure 5 here]

In both samples, support for police funding declines significantly among Democrats and Independents. Among Democrats, support falls by 6.6 percentage points in the original sample and 6.1 percentage points in the extended sample, corresponding to declines of 14.2% and 12.8% relative to their pre-treatment means. Among Independents, support falls by 9.6 and 7.1 percentage points, respectively. Among Republicans, by contrast, support remains unchanged in the extended sample and shows a marginally significant increase of 3.7 percentage points, or 6.2%, in the original sample.

Panel (a) of Figure 6 plots the event study estimates for support for police funding using the extended sample. The pre-treatment coefficients are close to zero and statistically insignificant. Following an incident, support for police funding declines immediately and remains significantly lower in subsequent waves, with the decline growing from about 4.5 percentage points in the first post-treatment wave to about 7.5 percentage points two waves later.

Taken together, these results provide strong evidence that high-profile police killings produce a persistent decline in public support for the police. The effects, however, vary substantially by political affiliation: the decline is concentrated among Democrats and Independents, while support among Republicans remains unchanged or even increases.

Placebo Checks: Other Spending Categories I next examine the effects of high-profile police killings on support for other spending categories as placebo checks. The CES asks the same funding question for welfare, healthcare, education, and transportation. If high-profile police killings affect attitudes specifically toward the police, rather than reflecting a broader shift in spending preferences, one would expect no significant effects on these other categories. Appendix Table A6 reports results for the original sample, and Panels (b) through (e) of Figure 6 plot the event study estimates for the extended sample.

[Insert Appendix Table A6 here]

Across these outcomes, I find no systematic evidence that high-profile police killings affect support for other spending categories, including welfare, healthcare, and education, which Democrats typically favor. These results suggest that the decline in support for police funding reflects a response specific to the police rather than a general shift toward more liberal spending preferences.

Other Policing-Related Questions in the CES I also examine other questions on attitudes toward the police that the CES includes beginning in 2016: support for increasing police presence, support for requiring police body cameras, whether respondents feel safe around the police (all 2016–2022), and ratings of local police (2016–2018). Appendix Table A7 reports the results, and Panels (c) through (f) of Figure 5 plot the estimates by party.

[Insert Appendix Table A7 here]

Although the pooled effects are generally small and imprecise, the estimates display a partisan divergence similar to that for police funding. For example, Democrats significantly reduce their support for increasing police presence following a high-profile incident, while Republican support does not decline. Similarly, Republicans rate their local police more favorably and report feeling safer around the police relative to Independents. Because these questions are available only from 2016 onward, treated counties in this analysis are limited to those first experiencing a high-profile incident after 2016, as all counties treated by the original Premkumar incidents are dropped as previously treated. Since these results are based on a small number of treated counties, they should be interpreted as suggestive.

5.2 Public Support and Legislative Action: Descriptive Evidence

This paper has established two main results so far. First, high-profile police killings lead to significantly more policing-related bills being introduced and passed in state legislatures. Second, high-profile police violence reduces public support for the police, as measured by support for police funding. In this section, I provide descriptive evidence on how shifts in public support for the police following high-profile incidents relate to legislative responses, examining whether legislators sponsor more policing-related bills in counties where support for the police declines.

5.2.1 Empirical Strategy

Using the legislator data described in Data Section, I match each policing-related bill to its sponsoring legislator and, through the legislator-county link, to the county they represent. I then construct a county-level panel of legislative activity covering the 2014 and 2016 legislative cycles.[28]

I then estimate the following Poisson specification:

$$\mathbb{E}\left[y_{c,t} \mid \alpha_c, \lambda_t, X_{c,t}\right] = \exp\Big(\alpha_c + \lambda_t + \beta_1\, PoliceKill_{c,t} + \beta_2\, SupportDecline_{c,t} + \beta_3\, PoliceKill_{c,t} \times SupportDecline_{c,t} + \gamma' X_{c,t}\Big), \tag{7}$$

where $y_{c,t}$ is the number of policing-related bills sponsored by legislators representing county c that were passed by the state legislature in legislative cycle t, α_c and λ_t are county and cycle fixed effects, and $X_{c,t}$ contains a set of time-varying controls. $PoliceKill_{c,t}$ is an indicator for whether county c experienced a high-profile police killing between the 2014 and 2016 CES waves, defined using the union of the original Premkumar incidents and the extended set of incidents over this

[28] Because legislative districts often span multiple counties, each bill is allocated equally across all counties that its sponsor's district overlaps. A bill with multiple sponsors from the same district is counted only once for that district.

period,[29] and $SupportDecline_{c,t}$ is the decline in county-level support for police funding between the two waves, measured as the share of respondents supporting increased police funding in 2014 minus the corresponding share in 2016.

The main coefficients of interest are β_1 and β_2, which capture how legislative activity responds to high-profile police killings and to declines in public support for the police, respectively. I also examine the interaction term β_3, which captures whether the legislative response to high-profile police killings is larger in counties where support for the police declines more. Standard errors are clustered at the county level.

5.2.2 Results

Table 7 reports the baseline results from Equation (7). To examine whether legislators of different parties respond differently, I then extend Equation (7) by stacking the county-cycle panel by sponsor party, so that each observation counts the bills passed that were sponsored by Democratic or Republican legislators representing a given county. I additionally include county-by-party and cycle-by-party fixed effects. The results are reported in Table 8.

[Insert Table 7 here]

[Insert Table 8 here]

Columns (1) through (3) of Table 7 show that both high-profile police killings and declines in public support for the police are associated with greater legislative activity at the county level. A high-profile killing is associated with a 24.2% increase in policing-related bills passed (Column 1), while a 10-percentage-point decline in support for police funding is associated with an 8.2% increase (Column 2). Both estimates remain similar in magnitude and statistically significant when included jointly (Column 3). The interaction between the two, added in Column (4), is not statistically significant.[30] Importantly, the coefficient on support decline is positive and significant across all specifications, indicating that counties experiencing larger declines in support for the police see more legislative activity.

As a falsification check, Appendix Table A8 uses the declines in support for welfare, healthcare, education, and transportation spending as explanatory variables in separate regressions. None of these placebo measures is significantly associated with policing legislation, suggesting that legislative activity responds specifically to shifts in attitudes toward the police rather than to broader changes in spending preferences.

[29] As in the public opinion analysis, counties that experienced a high-profile police killing before the 2014 CES wave are dropped, so that previously treated counties do not serve as controls.

[30] Because the decline in support is itself partly a consequence of high-profile police killings, the interaction term is noisy and imprecisely estimated.

[Insert Appendix Table A8 here]

Table 8 examines whether these patterns differ by the party of the bill sponsor. Democratic-sponsored legislation increases following both high-profile killings and declines in support: a high-profile killing is associated with a 28.1% increase in bills passed, and a 10-percentage-point decline in support with a 14.1% increase. By contrast, Republican-sponsored legislation shows no significant response to either. The difference between parties is statistically significant at the 10% level for high-profile killings, though not for support decline.

Overall, these results align with the findings from the previous sections: Democrats and Independents reduce their support for the police following high-profile incidents, Democratic-controlled legislatures pass more policing-related bills at the state level, and Democratic legislators make greater legislative efforts in counties where support for the police declines. These patterns suggest that public opinion is an important channel through which high-profile police violence shapes legislative responses.

6 Conclusion

In this paper, I study how policymakers respond to events that receive high media and public attention in the context of high-profile police violence, which is one of the most defining political issues of the twenty-first century in the United States. To answer this question, I draw on high-profile police killing incidents from 2005 to 2016 from Premkumar (2025b), construct a novel dataset on state-level policing legislation, and use data from the Cooperative Election Study to measure shifts in public attitudes toward the police and their role in shaping legislative responses.

I find high-profile police violence leads to large legislative responses: a single high-profile killing is associated with a 17.2% increase in policing-related bills introduced and a 9.5% increase in bills passed in the following year. These responses are stronger for incidents that receive greater media attention and generate more protest activity, and are driven primarily by Democratic-controlled legislatures, which pass more bills related to use of force, technology, and policing alternatives, as well as officer-supportive bills related to officer benefits. In contrast, Republican-controlled legislatures show little response.

High-profile police killings also reduce public support for the police. Support for police funding declines by about 5 percentage points following an incident, with the decline concentrated among Democrats and Independents, while support among Republicans remains largely unchanged. These incidents have no systematic effects on support for other spending categories, suggesting a response specific to the police.

Finally, descriptive evidence links these two sets of findings: Democratic legislators make greater legislative efforts in counties where support for the police declines, while Republican legislators

show no such response. These patterns suggest that public opinion is an important channel through which high-profile police violence shapes legislative responses, and that media salience plays a crucial role in translating public concern into policy action.

While these results reveal important dynamics in state-level policing reform, an important avenue for future research is to examine legislative responses at more local levels of government, such as city councils and county boards, where policing policy is also frequently shaped and where mayors and local officials may face more direct and immediate constituent pressure following high-profile incidents. Another promising direction is to examine the effectiveness of the legislation enacted in response to these incidents, and in particular whether it reduces police misconduct and use of force.

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7 Tables

Table 1
Victim and Incident Characteristics of High-Profile Police Violence

Category	Share	Mean news articles	Number
Panel A: Gender			
Female	13%	2,022	10
Male	86%	16,999	68
Trans/Other	1%	3,870	1
Panel B: Race/Ethnicity			
Black/Hispanic	84%	17,402	66
Other	16%	2,426	13
Panel C: Cause of Death			
Gunshot	91%	12,688	72
Beaten	3%	3,230	2
Medical emergency	3%	126,855	2
Restrained	1%	2,790	1
Tasered	1%	1,610	1
Drug	1%	1,960	1
Panel D: Armed Status			
Allegedly armed	39%	7,632	31
Unarmed	61%	19,655	48
Panel E: Recorded on Video			
Recorded on video	59%	14,819	47
Not recorded on video	41%	15,111	32

Notes: The table reports summary statistics on victim and incident characteristics for the high-profile police violence incidents in the Premkumar (2025a) sample. Share is the percentage of incidents in the category. Mean news articles is the mean number of news search results associated with incidents in that category. Number is the corresponding count of incidents. Race/ethnicity is collapsed to Black/Hispanic versus Other. Categories within each panel are mutually exclusive and sum to the total number of high-profile incidents in the sample.

Table 2
Summary Statistics

	Mean	SD	p10	p50	p90	p95	p99
Policing Legislation							
Bills introduced: All	41.11	43.87	5	27	98	136	219
Bills passed: All	12.54	14.62	1	8	26	45	71
Bills passed: Use of force	0.12	0.43	0	0	0	1	2
Bills passed: Technology	0.48	0.96	0	0	2	2	4
Bills passed: Standards	2.01	2.51	0	1	5	7	12
Bills passed: Training & certification	1.03	1.57	0	0	3	4	8
Bills passed: Accountability & oversight	0.80	1.31	0	0	2	3	6
Bills passed: Officer benefits	3.17	4.11	0	2	8	12	20
Bills passed: Policing alternatives	0.21	0.53	0	0	1	1	3
Bills passed: Other	5.20	7.80	0	3	12	18	43
Police Violence							
High-profile police killings (Premkumar)	0.13	0.56	0	0	0	1	2
Total police killings (count)	27.90	39.08	2	19	51	89	230
Media and Protest Coverage							
News articles (Premkumar)	1928.17	20518.44	0	0	0	2300	19030
National TV news	0.04	0.21	0	0	0	0	1
Protest news articles	6.63	58.07	0	0	0	8.70	159.30
Political Controls							
Democratic legislature	0.39	0.49	0	0	1	1	1
Republican legislature	0.43	0.50	0	0	1	1	1
Split legislature	0.16	0.37	0	0	1	1	1
Democratic governor	0.50	0.50	0	1	1	1	1
Republican governor	0.56	0.50	0	1	1	1	1
Institutional Controls							
Regular session	0.96	0.19	1	1	1	1	1
First year of biennium	0.50	0.50	0	0.50	1	1	1
Demographic and Economic Controls							
Black share of population	0.12	0.11	0.01	0.09	0.29	0.33	0.52
Hispanic share of population	0.11	0.10	0.03	0.08	0.24	0.36	0.46
Female share of population	0.51	0.01	0.50	0.51	0.52	0.52	0.53
Unemployment rate	6.17	2.16	3.70	5.80	9.40	10.30	11.70
Income per capita (log)	11.64	0.16	11.45	11.63	11.86	11.91	12.07
Violent crime rate (log)	9.43	1.24	7.60	9.64	10.82	11.50	12.02
Poverty rate	13.20	3.41	9.30	12.70	17.90	19.50	22.10
Officers per capita	9.64	1.07	8.10	9.70	11.04	11.51	11.95
Teenager and adult share (13–61)	0.66	0.02	0.64	0.66	0.68	0.69	0.73
Elderly share (62+)	0.17	0.02	0.14	0.17	0.20	0.21	0.23

Notes: Sample includes 612 state-year observations from 2005 to 2016. The Policing Legislation panel shows the number of policing-related bills introduced and passed in total, as well as bills passed by individual policy category. High-profile police killings are drawn from Premkumar (2025a). Total police killings are the total number of police killings in a given state-year. News articles are the number of news articles covering the high-profile police violence incident from Premkumar (2025a); National TV news is the number of national television news segments covering the incident; Protest news is the number of news articles covering associated protest activity. Democratic and Republican legislature are indicators for party control of the state legislature. Republican governor is an indicator for Republican governors. Regular session indicates whether the legislature convened a regular session to consider bills; some state legislatures only meet every other year. First year of biennium is an indicator for the first year of the two-year legislative biennium. Income per capita and violent crime rate are measured in log terms; officers employed is measured in per capita terms; all other controls are measured in percentage terms.

Table 3
Effect of High-Profile Police Killings on Policing Legislation

	Poisson				Linear (OLS)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Bills Introduced								
HP Killing (Count)	0.159***	0.180***	0.159***	0.126***	7.287**	7.748**	7.938**	12.394***
	(0.054)	(0.061)	(0.059)	(0.022)	(2.734)	(3.062)	(3.154)	(2.900)
Panel B: Bills Passed								
HP Killing (Count)	0.073	0.086**	0.091**	0.129***	1.348*	1.389*	1.927***	4.609**
	(0.044)	(0.043)	(0.036)	(0.040)	(0.789)	(0.811)	(0.500)	(1.746)
Baseline mean	51.51 (Bills Introduced)				17.79 (Bills Passed)			
Specifications								
State and year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time-varying controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Population weights	No	No	Yes	Yes	No	No	Yes	Yes
Biennial analysis	No	No	No	Yes	No	No	No	Yes
Observations	612	612	612	306	612	612	612	306

Notes: Robust standard errors clustered at the state level are reported in parentheses. All specifications include state and year fixed effects and baseline political and institutional controls. Columns (1) and (5) report the baseline annual, unweighted specifications. Columns (2) and (6) additionally include time-varying demographic, economic, crime, and police-employment controls. Columns (3) and (7) additionally apply population weights. Columns (4) and (8) report the corresponding population-weighted biennial specifications. Columns (1)–(4) report Poisson fixed-effects estimates; coefficients can be converted to percentage changes in the expected count as $100 \times [\exp(\hat{\beta}) - 1]$. Columns (5)–(8) report OLS estimates, for which coefficients represent the change in the number of bills associated with one additional high-profile police killing. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 4
Effect of High-Profile Police Killings: Robustness Checks

	Poisson					Linear (OLS)				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: Bills Introduced										
HP Killing (Count)	0.159***	0.161***	0.102**	0.136***		7.938**	4.903*	6.222***	11.746**	0.099***
	(0.059)	(0.059)	(0.047)	(0.028)		(3.154)	(2.452)	(1.575)	(5.563)	(0.035)
HP Killing (Binary)					0.271**					
					(0.115)					
Panel B: Bills Passed										
HP Killing (Count)	0.091**	0.093**	0.039**	0.122***		1.927***	0.884**	2.050***	3.940**	0.084***
	(0.036)	(0.040)	(0.020)	(0.020)		(0.500)	(0.341)	(0.508)	(1.686)	(0.027)
HP Killing (Binary)					0.052					
					(0.055)					
Specifications										
No crime/officer controls	No	Yes	No	No	No	No	No	No	No	No
State linear time trend	No	No	Yes	No	No	No	Yes	No	No	No
Stacked DiD estimator	No	No	No	Yes	No	No	No	Yes	No	No
dCDH estimator	No	No	No	No	No	No	No	No	Yes	No
Log outcome	No	No	No	No	No	No	No	No	No	Yes
Observations	612	612	612	2188	612	612	612	2188	–	612

Notes: Standard errors clustered at the state level are in parentheses. Columns (1) and (6) are the reference specifications from Table 2. Column (2) excludes violent-crime and officer-employment controls. Columns (3) and (7) add state-specific linear trends. Columns (4) and (8) use stacked DiD with a $[-2, 4]$ event window and cohorts defined by lagged HP police violence. Column (5) uses a binary indicator for at least one lagged HP police killing. Column (9) reports the average total effect from the de Chaisemartin & D'Haultfœuille (2024) estimator on the biennium-collapsed 2005–2016 panel. Column (10) uses a log transformation of the outcome. Poisson coefficients imply $100 \times [\exp(\hat{\beta}) - 1]$ percent changes in the expected count. The baseline mean is 51.51 for bills introduced and 17.79 for bills passed. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5
The Role of Media Coverage, Protest Activity, and Political Party

	(1) Baseline	(2) × Media	(3) × Protest	(4) × Party
Panel A: Bills Introduced				
HP Killing (Binary)	0.271**	0.251**	0.229*	−0.261*
	(0.115)	(0.119)	(0.125)	(0.144)
HP Killing × Media coverage		0.001***		
		(0.000)		
HP Killing × Protest activity			0.075**	
			(0.030)	
HP Killing × Dem. legislature				0.738***
				(0.206)
HP Killing × Rep. legislature				0.299*
				(0.169)
Panel B: Bills Passed				
HP Killing (Binary)	0.052	0.058	0.038	−0.333***
	(0.055)	(0.055)	(0.057)	(0.127)
HP Killing × Media coverage		−0.001		
		(0.001)		
HP Killing × Protest activity			0.033	
			(0.030)	
HP Killing × Dem. legislature				0.554***
				(0.166)
HP Killing × Rep. legislature				0.222
				(0.168)
Baseline mean	51.51 (Bills Introduced)		17.79 (Bills Passed)	
Observations	612	612	612	612

Notes: Robust standard errors clustered at the state level are in parentheses. All specifications report Poisson fixed-effects estimates, are weighted by state population, and include state and year fixed effects, baseline controls, and time-varying controls. The treatment variable is a binary indicator for a high-profile police killing in the prior year. Column (2) interacts the treatment with lagged local media coverage. Column (3) interacts the treatment with lagged protest activity. Column (4) interacts the treatment with indicators for Democratic and Republican legislature control; the omitted category is split legislature. For interpretation, a Poisson coefficient $\hat{\beta}$ implies a proportional change in the expected count equal to $100 \times [\exp(\hat{\beta}) - 1]$ percent. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6
High-Profile Police Killings and Support for Increasing Law Enforcement Spending

	CES 2014–2016				CES 2014–2024			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
HP Killing	−0.050***	−0.096***	−0.041**	−0.089***	−0.051***	−0.071***	−0.052***	−0.073***
	(0.015)	(0.022)	(0.018)	(0.024)	(0.014)	(0.019)	(0.013)	(0.017)
Democrat	−0.017**	−0.017**	−0.016**	−0.017**	−0.055***	−0.056***	−0.055***	−0.056***
	(0.007)	(0.007)	(0.007)	(0.007)	(0.005)	(0.005)	(0.005)	(0.005)
Republican	0.135***	0.131***	0.135***	0.131***	0.183***	0.178***	0.184***	0.179***
	(0.007)	(0.007)	(0.007)	(0.007)	(0.004)	(0.005)	(0.004)	(0.005)
HP Killing × Democrat		0.030		0.033		0.010		0.010
		(0.021)		(0.022)		(0.016)		(0.016)
HP Killing × Republican		0.133***		0.135***		0.063***		0.062***
		(0.030)		(0.030)		(0.016)		(0.016)
Baseline means	(Total: 0.481	Democrat: 0.465		Republican: 0.600)	(Total: 0.492	Democrat: 0.476		Republican: 0.599)
Party interactions	No	Yes	No	Yes	No	Yes	No	Yes
Year fixed effects	Yes	Yes	No	No	Yes	Yes	No	No
State × year fixed effects	No	No	Yes	Yes	No	No	Yes	Yes
Observations	70,793	70,793	70,793	70,793	183,569	183,569	183,569	183,569
Counties	2,370	2,370	2,370	2,370	2,748	2,748	2,748	2,748

Notes: Standard errors clustered at the county level are in parentheses. All specifications are estimated using OLS, include county fixed effects, and use CES survey weights. Columns (1)–(2) and (5)–(6) include year fixed effects; Columns (3)–(4) and (7)–(8) include state-by-year fixed effects. Independent voters are the omitted party group. The interaction terms report differences for Democrats and Republicans relative to Independents. Columns (1)–(4) use the original 2014–2016 Premkumar sample. Columns (5)–(8) use the extended 2014–2024 CES sample and an expanded set of high-profile police-killing incidents through 2022 collected by the author; treatment is restricted to incidents occurring after the 2014 survey wave, and counties already treated as of the 2014 wave are dropped. Baseline means are survey-weighted pretreatment means of the outcome among respondents in treated counties, reported for all respondents and separately for Democrats and Republicans. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 7
High-Profile Police Killings, Public Support, and Policing Legislation

	(1) HP Killing	(2) Support Decline	(3) Both	(4) Interaction
HP Killing	0.217*		0.202*	0.084
	(0.115)		(0.116)	(0.129)
Support Decline		0.079**	0.072*	0.090**
		(0.039)	(0.040)	(0.039)
HP Killing × Support Decline				−0.156
				(0.124)
Baseline mean		3.5		
HP killing included	Yes	No	Yes	Yes
Support decline included	No	Yes	Yes	Yes
Interaction included	No	No	No	Yes
County fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Observations	1,492	1,492	1,492	1,492
County clusters	746	746	746	746

Notes: Standard errors clustered at the county level are in parentheses. The dependent variable is the number of policing bills passed in the county. All specifications report Poisson estimates, include county and year fixed effects and demographic and economic controls, and use population weights. The high-profile incident measure combines the original Premkumar incidents with the extended set of high-profile police-killing incidents collected by the author. Support Decline is the decrease in county-level support for increasing law-enforcement spending between the 2014 and 2016 CES surveys, measured in 10-percentage-point units. Treatment is restricted to incidents occurring after the 2014 survey wave; counties already treated as of the 2014 wave are dropped, and the sample requires at least five CES respondents in both survey waves. Poisson coefficients are reported as log incidence-rate ratios. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 8
High-Profile Police Killings, Public Support, and Policing Legislation by Party

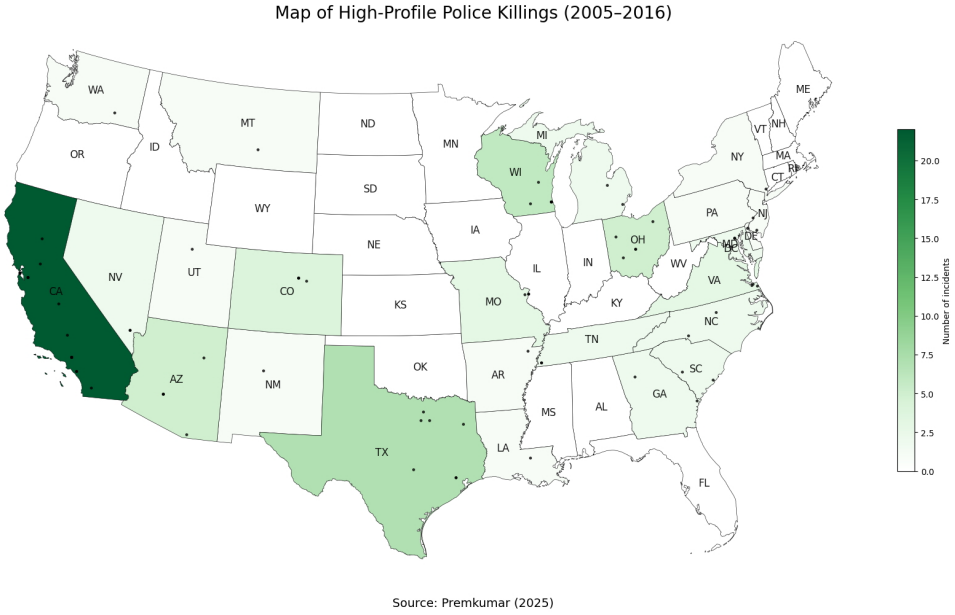
	(1) Democrat	(2) Republican	(3) Difference
HP Killing	0.248*	−0.320	−0.568*
	(0.135)	(0.257)	(0.295)
Support Decline	0.132**	0.060	−0.073
	(0.061)	(0.045)	(0.071)
HP Killing × Support Decline	−0.089	−0.177	−0.088
	(0.139)	(0.155)	(0.222)
Baseline mean	2.15 (Democrat)	1.37 (Republican)	
County fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
County × party fixed effects	Yes	Yes	Yes
Year × party fixed effects	Yes	Yes	Yes
Observations	1,824	1,824	1,824
County clusters	743	743	743

Notes: Standard errors clustered at the county level are in parentheses. The dependent variable is the number of policing bills passed. All estimates come from a single pooled Poisson regression with county, year, county-by-party, and year-by-party fixed effects, population weights, demographic and economic controls. Column (1) reports the estimated coefficients for Democrats. Column (2) reports the implied coefficients for Republicans, calculated as the sum of the Democratic coefficient and the corresponding party interaction. Column (3) reports the Republican-minus-Democratic difference. Support Decline is the decrease in county-level support for increasing law-enforcement spending between the 2014 and 2016 CES surveys, measured in 10-percentage-point units. The high-profile incident measure combines the original Premkumar incidents with the extended set of high-profile police-killing incidents collected by the author. Baseline means are 2.15 bills for Democrats and 1.37 bills for Republicans. Coefficients are reported as log incidence-rate ratios. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

8 Figures

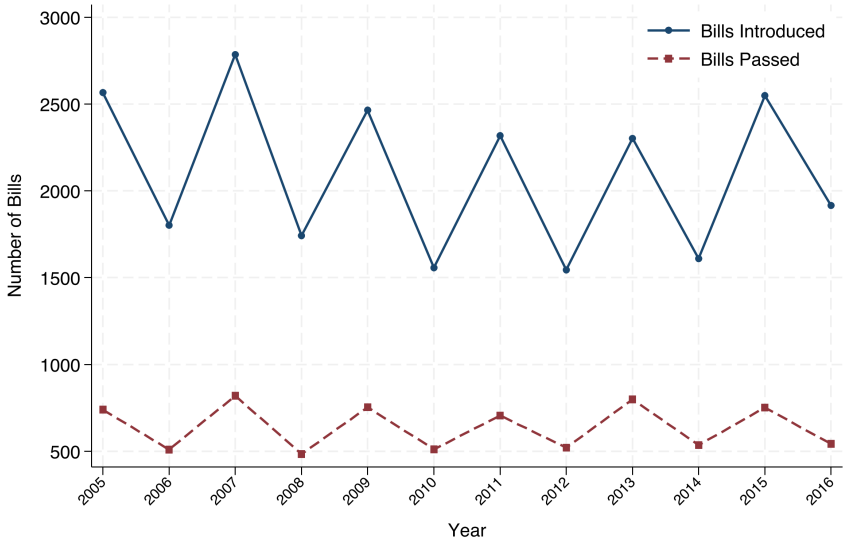
Figure 1
High-Profile Police Killings and Policing Legislation, 2005–2016

(a) High-Profile Police Killings



Source: Premkumar (2025)

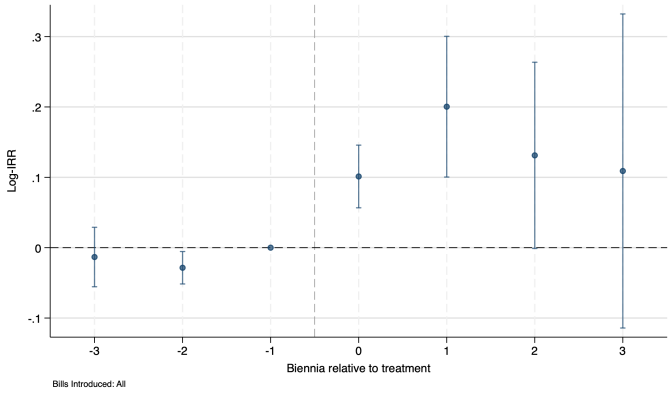
(b) Police Bills Introduced and Passed



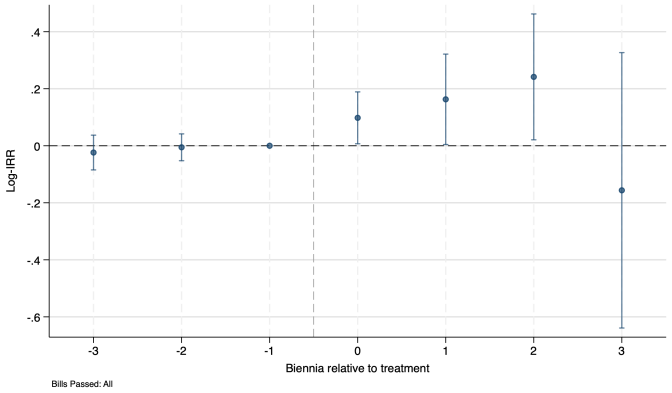
Notes: Panel (a) maps the locations of 79 high-profile police killings between 2005 and 2016. Panel (b) plots the total number of policing-related bills introduced and passed in state legislatures from 2005 to 2016. The alternating peaks in odd years and troughs in even years reflect the biennial structure of state legislative calendars.

Figure 2a
Dynamic Effect of High-Profile Police Killings on Policing Legislation

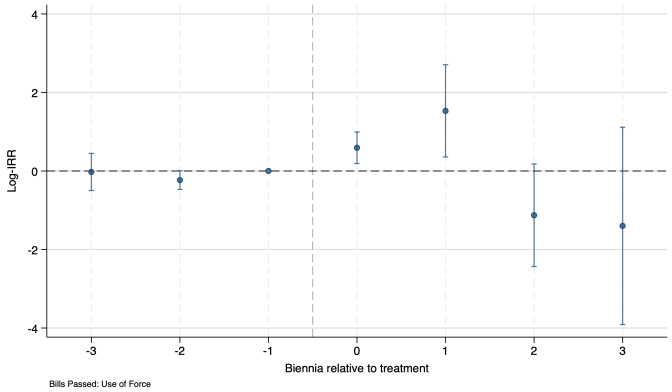
(a) Bills Introduced: All



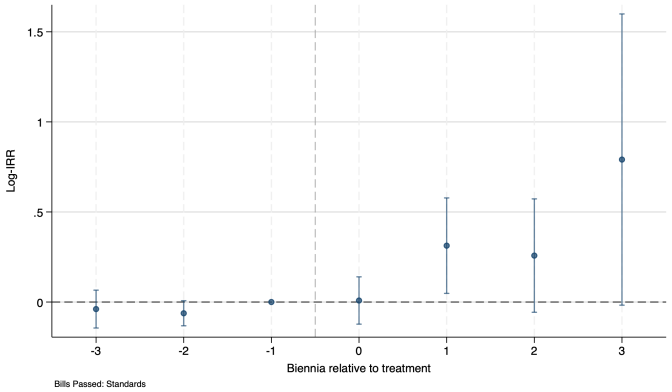
(b) Bills Passed: All



(c) Use of Force

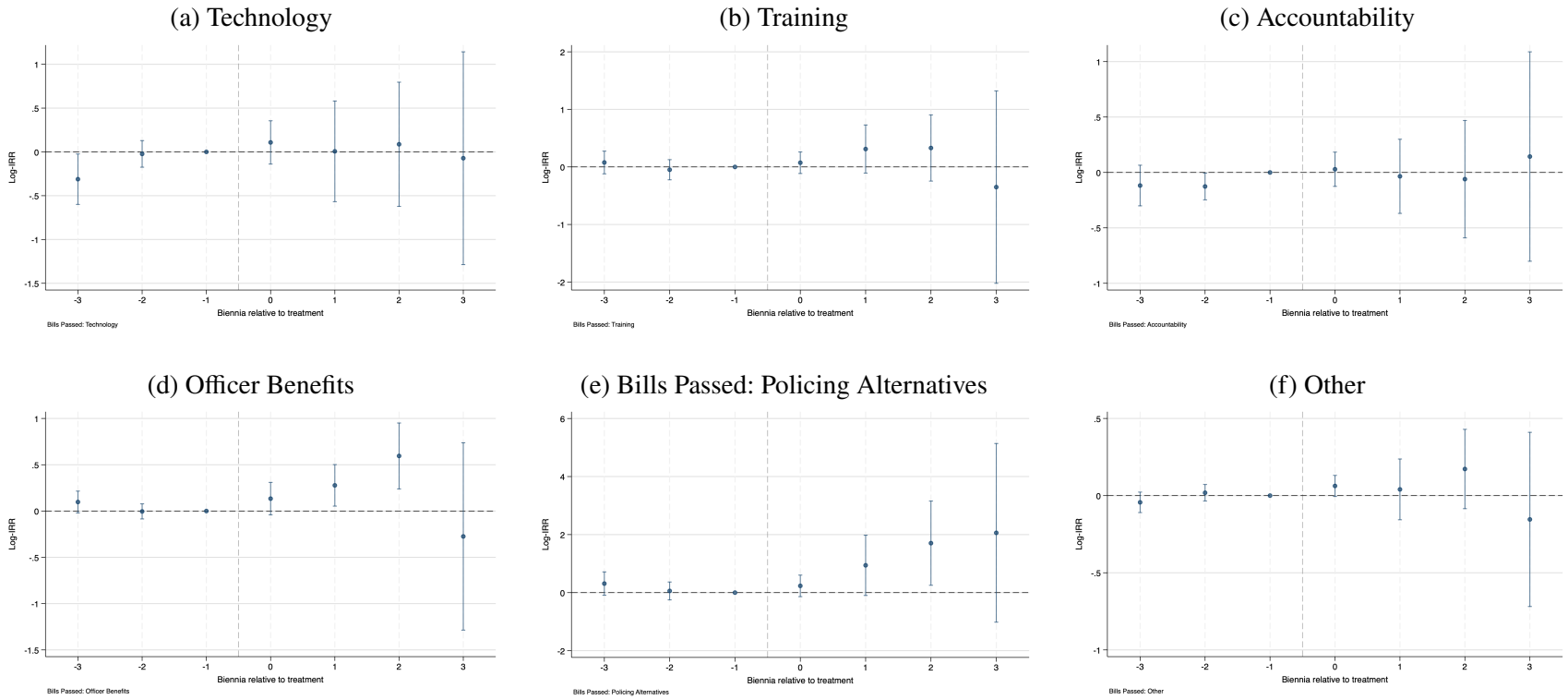


(d) Standards



Notes: Each panel plots event-study coefficients from Poisson regressions of biennial state-level bill counts on event-time dummies relative to the biennium of a high-profile police killing. The treatment variable is the count of high-profile police killings. Event-time $t = -1$ is the omitted reference period. All specifications include state and biennium fixed effects, baseline and time-varying controls, and are population weighted. Standard errors are clustered at the state level. Bars represent 95% confidence intervals.

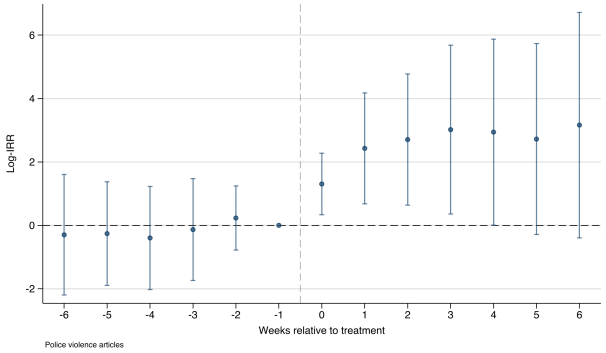
Figure 2b
Dynamic Effect of High-Profile Police Killings on Policing Legislation (continued)



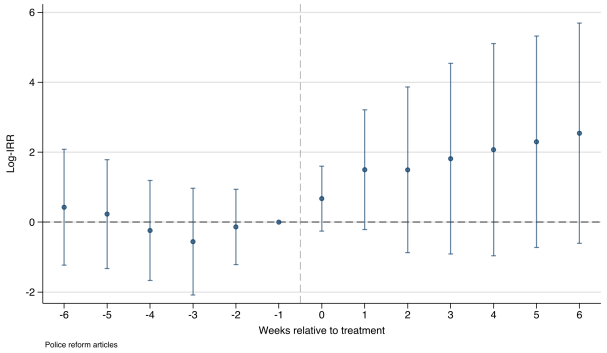
Notes: Each panel plots event-study coefficients from Poisson regressions of biennial state-level bill counts on event-time dummies relative to the biennium of a high-profile police killing, using a symmetric window of ±2 biennia. The treatment variable is the count of high-profile police killings. Event-time $t = -1$ is the omitted reference period. All specifications include state and biennium fixed effects, baseline controls and time-varying controls, and are population weighted. Standard errors are clustered at the state level. Bars represent 95% confidence intervals.

Figure 3
Event Study of Media Attention Around High-Profile Police Killings

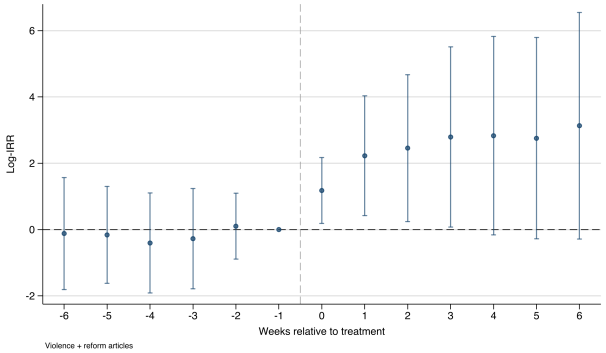
(a) News Articles on Police Violence



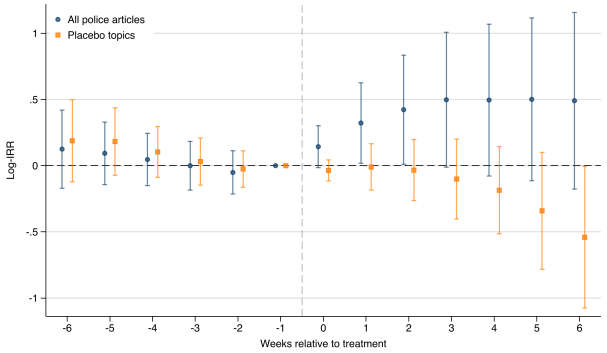
(b) News Articles on Police Reform



(c) Violence+Reform Coverage



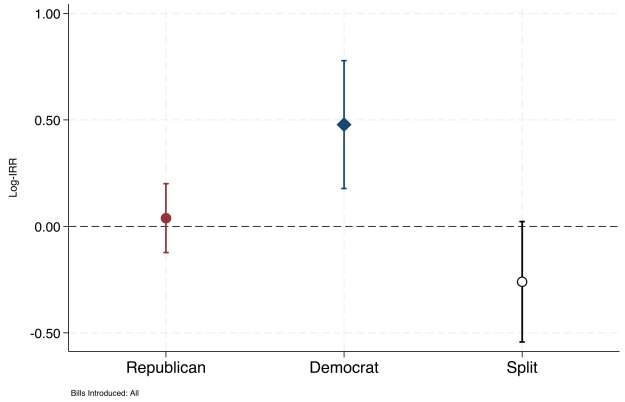
(d) General Police Coverage and Placebo Topics



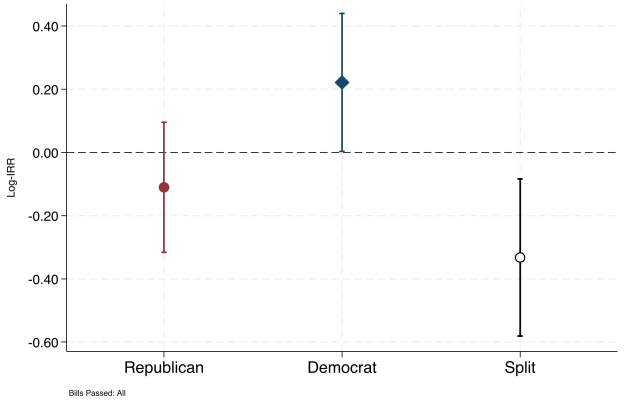
Notes: Each panel plots event-study coefficients from Poisson regressions of weekly state-level news article counts on event-time indicators relative to the week of a high-profile police killing, using a symmetric window of ±6 weeks. The treatment variable is an indicator for the occurrence of a high-profile police killing in a given state-week. Event time $t = -1$ is the omitted reference period. Panel (a) reports weekly counts of articles covering police violence; Panel (b) reports articles covering police reform; and Panel (c) reports the combined count of police-violence and police-reform articles. Panel (d) compares all articles concerning police with articles mentioning at least one of three placebo policy topics: climate change, the minimum wage, or abortion rights. All specifications include state and week fixed effects and control for log population. Standard errors are clustered at the state level. Bars represent 95% confidence intervals.

Figure 4a
Legislative Responses to High-Profile Police Killings by Party Control

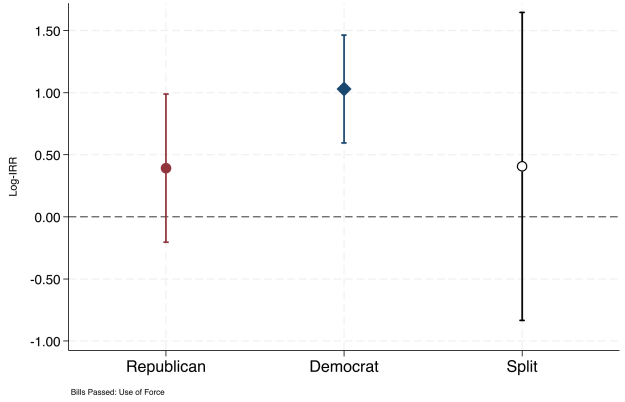
(a) Bills Introduced: All



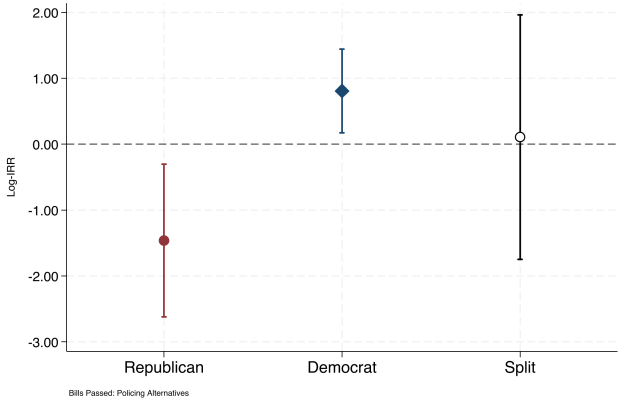
(b) Bills Passed: All



(c) Bills Passed: Use of Force



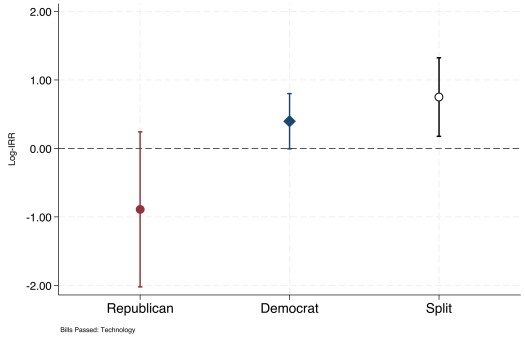
(d) Bills Passed: Policing Alternatives



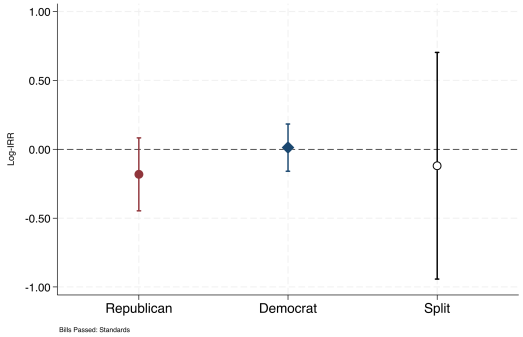
Notes: Each panel reports the estimated effect of a high-profile police killing on policing legislation by party control of the state legislature, with 95% confidence intervals. Estimates are total effects from Poisson interaction specifications that include state and year fixed effects, baseline controls, and time-varying controls. Regressions are weighted by state population, and standard errors are clustered at the state level. Legislature control means that one party controls both legislative chambers; split control is the omitted category.

Figure 4b
Legislative Responses to High-Profile Police Killings by Party Control (continued)

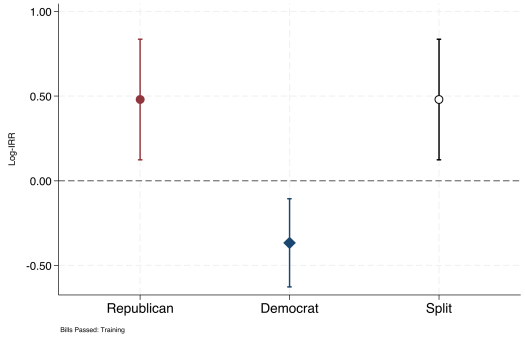
(e) Bills Passed: Technology



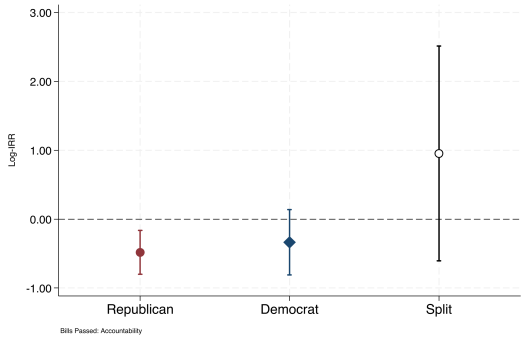
(f) Bills Passed: Standards



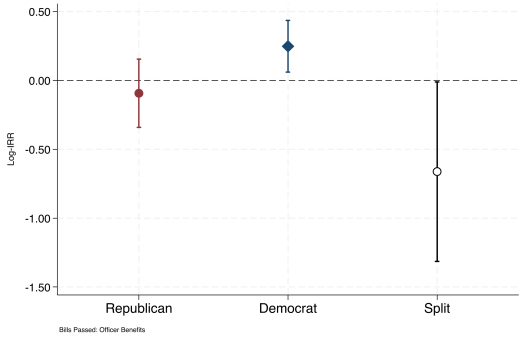
(g) Bills Passed: Training



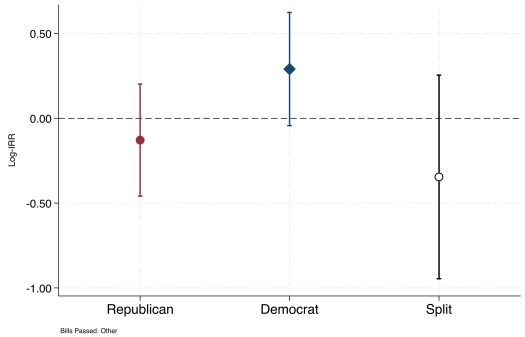
(h) Bills Passed: Accountability



(i) Bills Passed: Officer Benefits

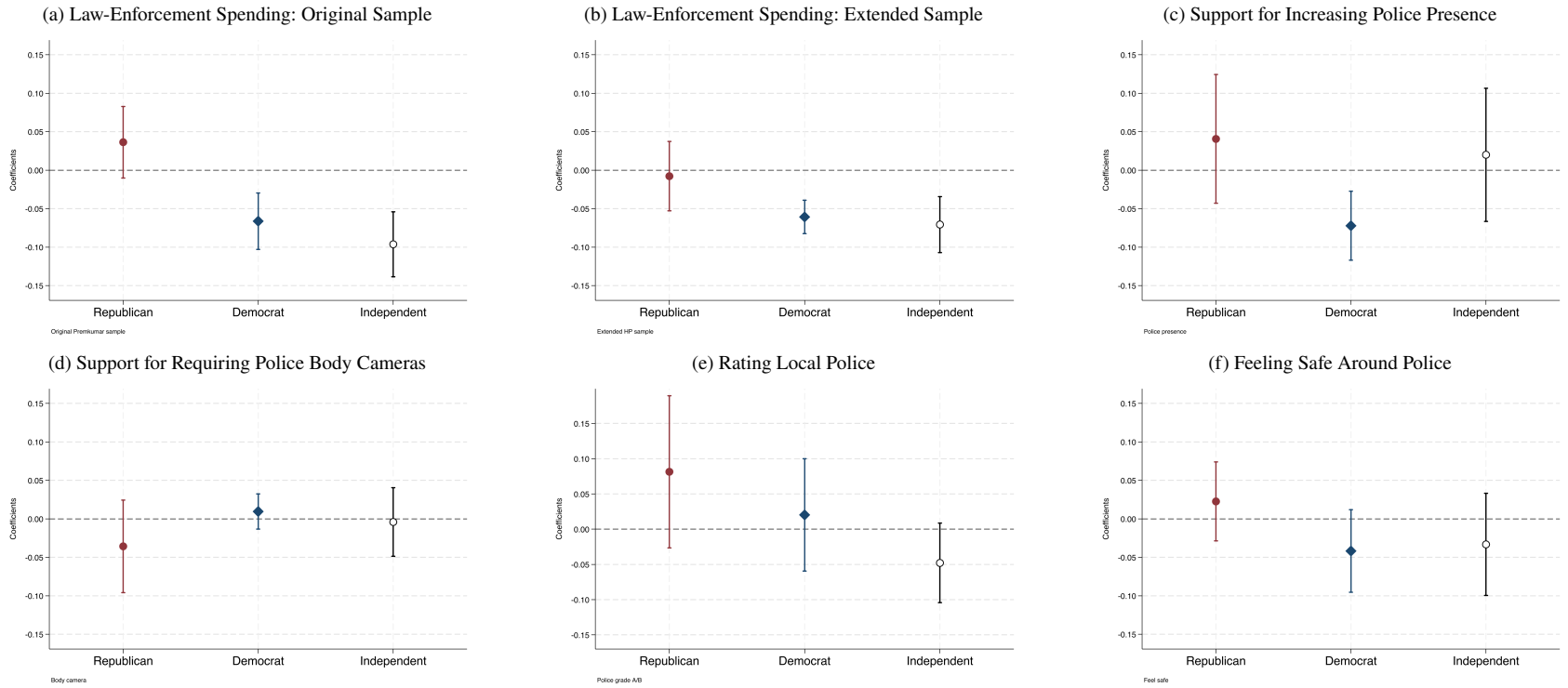


(j) Bills Passed: Other



Notes: Each panel reports the estimated effect of a high-profile police killing on policing legislation by party control of the state legislature, with 95% confidence intervals. Estimates are total effects from Poisson interaction specifications that include state and year fixed effects, baseline controls, and time-varying controls. Regressions are weighted by state population, and standard errors are clustered at the state level. Legislature control means that one party controls both legislative chambers; split control is the omitted category.

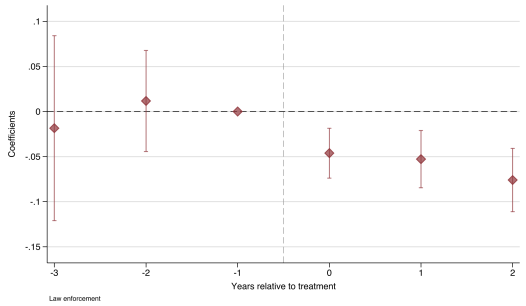
Figure 5
Effects of High-Profile Police Killings on Support for Police by Party



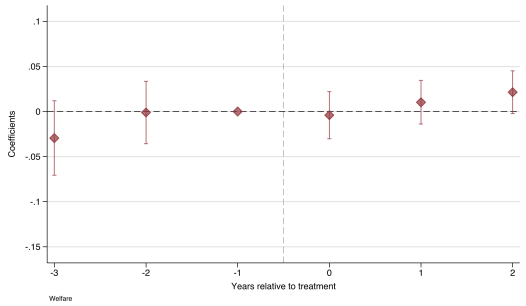
Notes: Each panel reports the estimated effect of a high-profile police killing on the stated outcome by respondent party identification, with 95% confidence intervals. Estimates are total effects calculated from OLS interaction specifications. Treatment is restricted to counties first treated within each panel's CES window; counties treated before that window are dropped. All specifications include county and year fixed effects, use CES survey weights, and cluster standard errors at the county level. Panel (a) uses the original Premkumar incident sample and the 2014 and 2016 CES waves. Panel (b) uses the 2014, 2016, 2018, 2020, 2022, and 2024 CES waves and the expanded set of high-profile police-killing incidents through 2022 collected by the author. Panels (c), (d), and (f) use the 2016, 2020, and 2022 CES waves; Panel (e) uses the 2016 and 2018 waves. Panel (c) measures support for increasing the number of police on the street by 10 percent; Panel (d), support for requiring police body cameras; Panel (e) respondents' ratings of their local police; and Panel (f), whether police make respondents feel mostly or somewhat safe.

Figure 6
Event-Study Effects of High-Profile Police Killings on Support for Government Spending

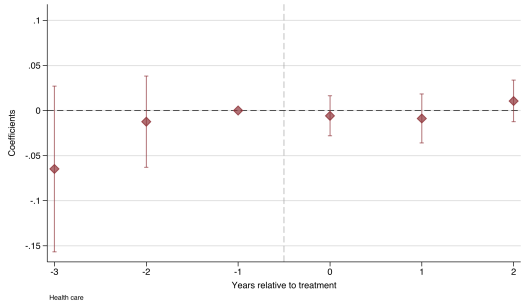
(a) Law-Enforcement Spending



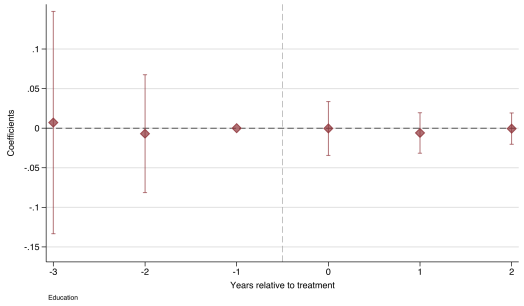
(b) Welfare Spending



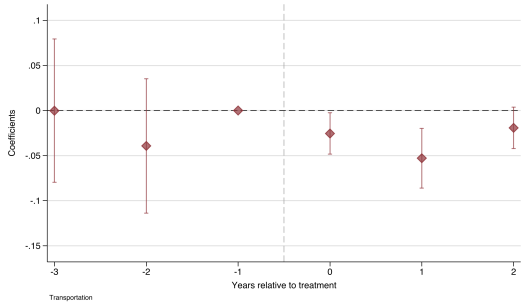
(c) Health-Care Spending



(d) Education Spending



(e) Transportation Spending



Notes: Each panel plots event-study estimates of the effect of a high-profile police killing on public support for increased government spending, using the de Chaisemartin and D'Haultfoeuille (2024) estimator. The dependent variable is the county-year share of respondents supporting increased government spending in the stated category, constructed from the 2014–2024 CES waves. Incidents are drawn from the extended sample through 2022 collected by the author. Treatment is restricted to incidents occurring after the 2014 survey wave; counties already treated as of the 2014 wave are dropped. All specifications include county and year fixed effects, and are weighted by the number of CES respondents. Standard errors are clustered at the county level. Bars represent 95% confidence intervals.

High-Profile Police Violence, Public Opinion, and Police Reform

Online Appendix

Jinlin Li

October 21, 2026

A Appendix A: Additional Figures and Tables **2**

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B Appendix B: Training the Policing Bill Topic Classifier **17**

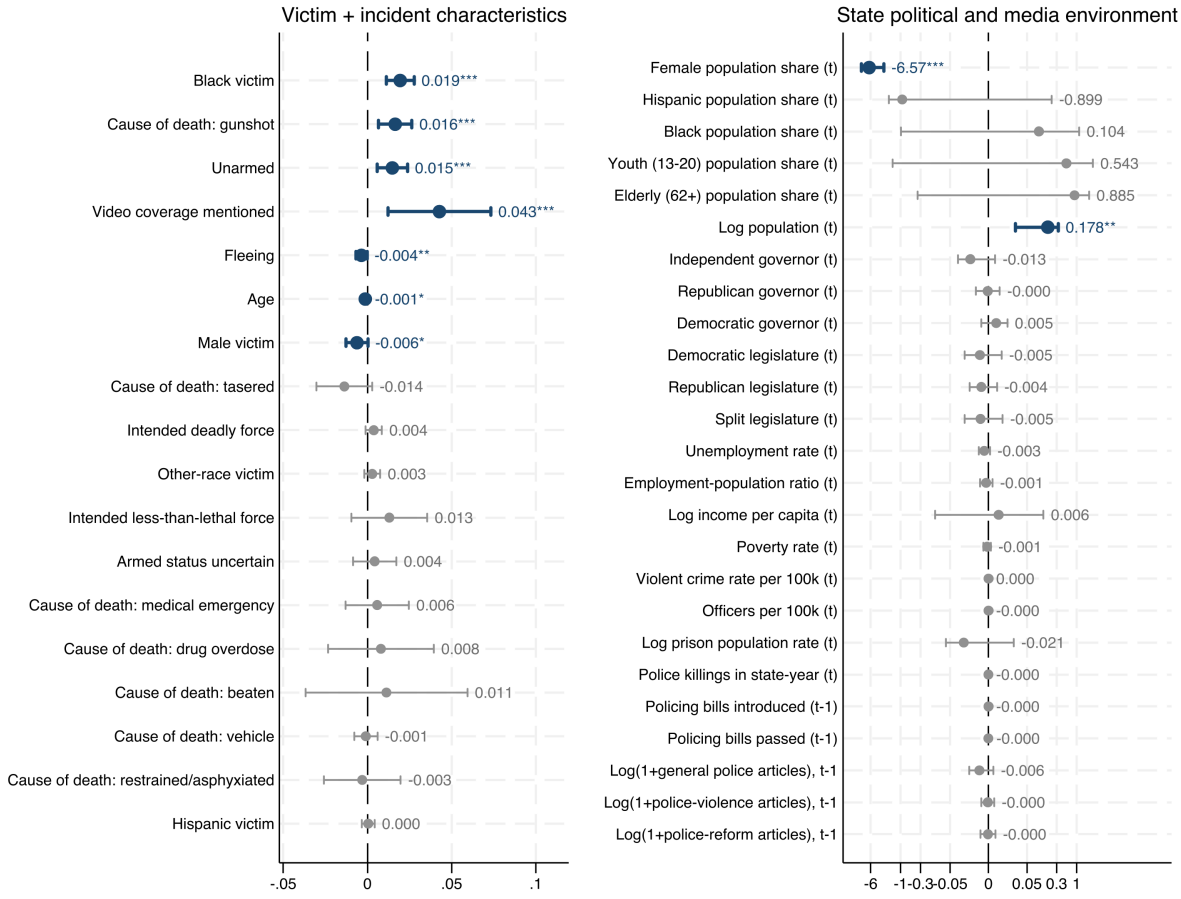
Table B1: Distribution of Bill Topics in the NCSL Database 17

Table B2: Policing Bill Topic Classifier: Per-Label Performance 19

A Appendix A: Additional Figures and Tables

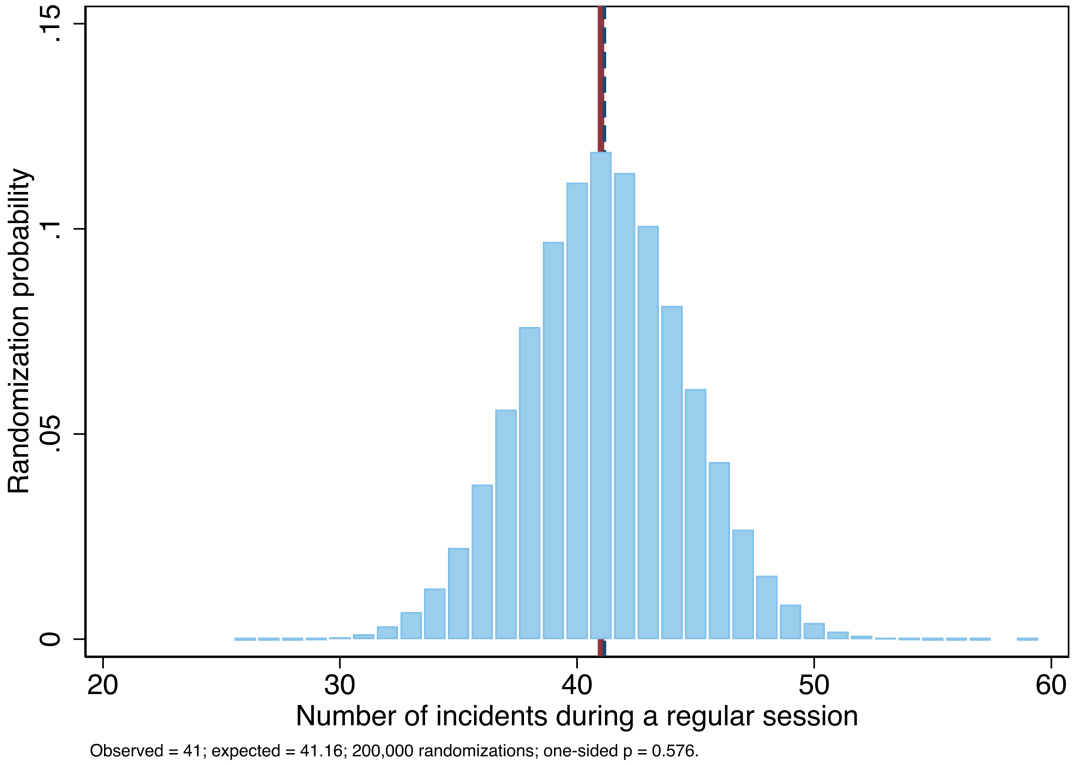
A.1 Figures

Figure A1
Predictors of High-Profile Status among Police-Killing Incidents



Notes: Coefficients and 95% confidence intervals from a single linear probability model. The sample is the 9,600 Fatal Encounters incidents from 2011–2016 with non-missing victim and incident characteristics; coverage of armed status, alleged weapon and fleeing begins in 2011. The dependent variable is a binary indicator for high-profile status, equal to one for an audited match to Premkumar's high-profile sample (72 of 9,600 incidents). All estimates include state and year fixed effects, with standard errors clustered by state. Panel A shows victim and incident characteristics on a linear scale, ordered by significance; Panel B shows the state environment on a symmetric-log scale, with untransformed coefficients plotted throughout. Omitted categories are White victims, allegedly armed incidents, and "other" cause of death. Age is measured in decades. Video coverage is proxied by whether video, footage, bodycam, dashcam or surveillance is mentioned in the incident description or source URL, so it reflects reported rather than verified recording. "Police killings in state-year" excludes the focal incident. State characteristics are measured at t; lagged legislation and media volumes at $t-1$. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

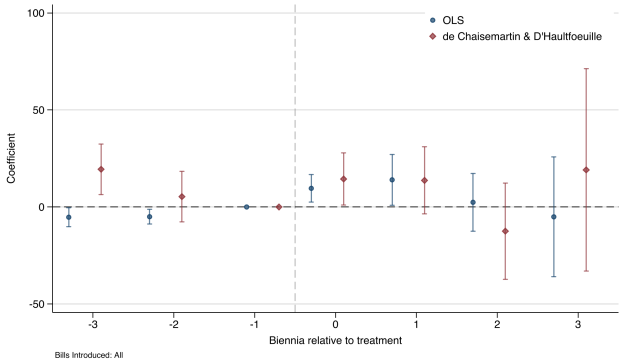
Figure A2
Randomization Test of Incident Timing During Legislative Sessions



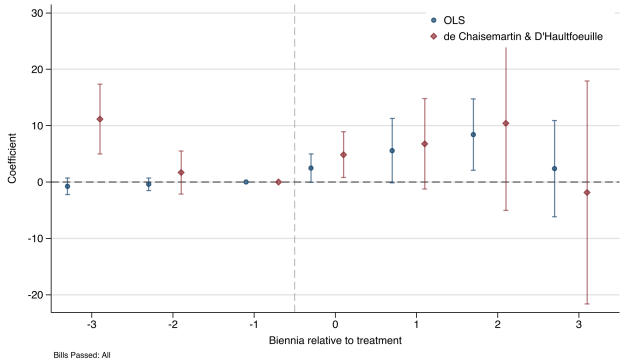
Notes: Randomization distribution of the number of high-profile incidents that would fall during a regular legislative session under random timing. For each of the 79 incidents, the incident's state and calendar year are held fixed and a date is redrawn uniformly from that same state-year; the procedure is repeated 200,000 times. Holding the state-year fixed accounts for differences in session length and schedule across states, such as California's long sessions and Texas's biennial calendar, and for the concentration of incidents in 2014–2016. The 79 incidents span 52 distinct state-years. The solid vertical line marks the observed count (41 of 79, or 51.9%); the dashed line marks the mean of the randomization distribution (41.16, or 52.1%). The simulated 95% range is 35 to 48 incidents; the one-sided p-value for an excess of in-session incidents is 0.576 and the two-sided p-value is 1.000. Session dates are regular sessions only, from the published convening date through adjournment inclusive, taken from NCSL legislative session calendars; special sessions are excluded and intra-session recesses are not removed, so "in session" means within the formal convene-to-adjourn span rather than a floor-vote day.

Figure A3a
Dynamic Effect of High-Profile Police Killings on Policing Legislation: Robustness

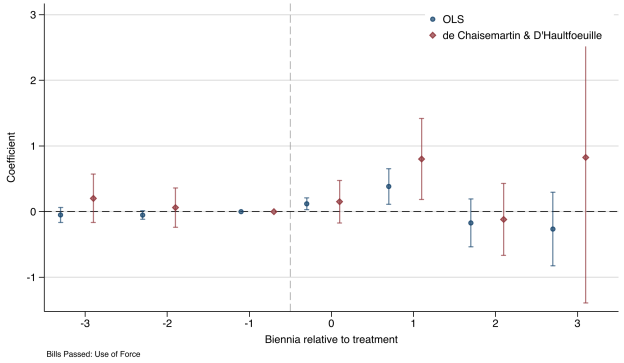
(a) Bills Introduced: All



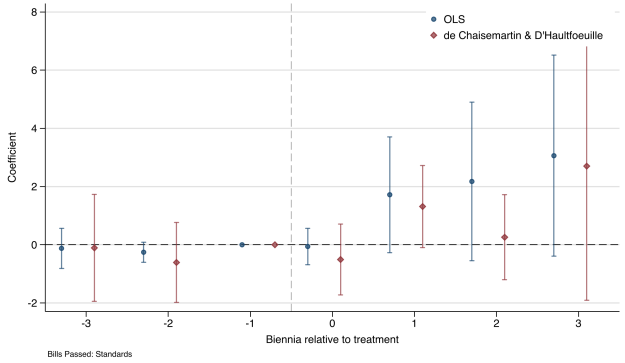
(b) Bills Passed: All



(c) Bills Passed: Use of Force



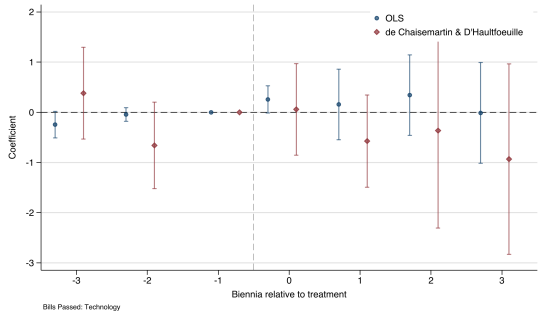
(d) Bills Passed: Standards



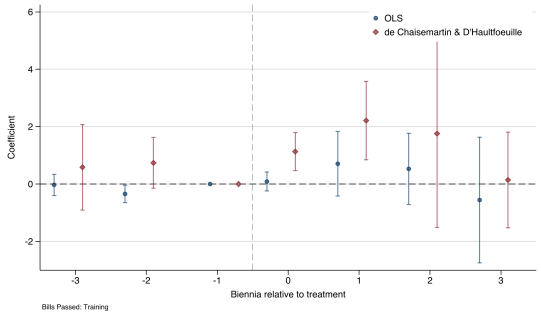
Notes: Each panel plots event-study coefficients from OLS (circles) and de Chaisemartin & D'Haultfœuille (2024) (diamonds) estimators of biennial state-level bill counts on event-time dummies relative to the biennium of a high-profile police killing. The treatment variable is the count of high-profile police killings. All specifications include state and biennium fixed effects, baseline controls and time-varying controls, and are population weighted. Standard errors are clustered at the state level. Bars represent 95% confidence intervals.

Figure A3b
Dynamic Effect of High-Profile Police Killings on Policing Legislation: Robustness

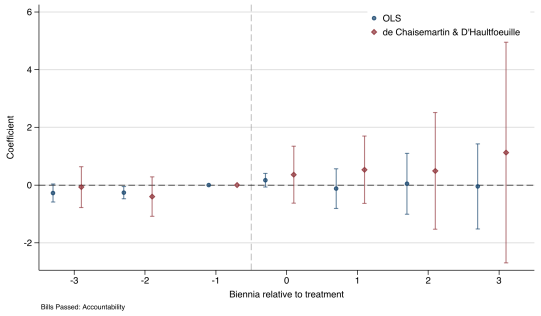
(a) Bills Passed: Technology



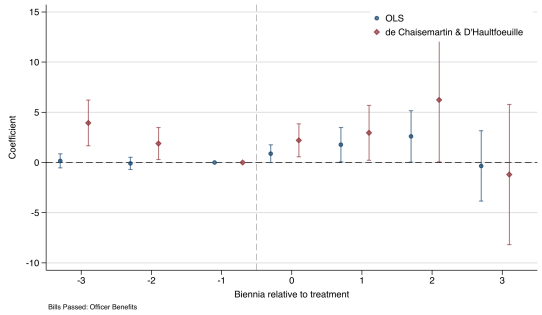
(b) Bills Passed: Training



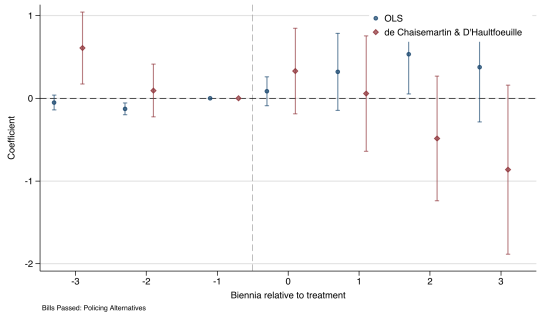
(c) Bills Passed: Accountability



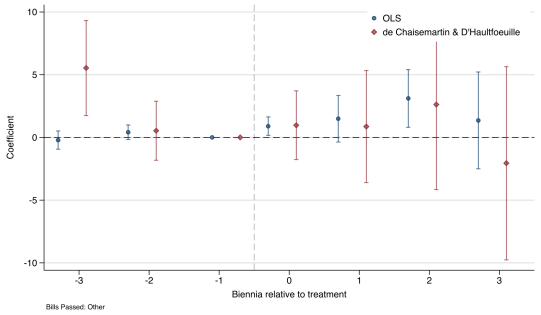
(d) Bills Passed: Officer Benefits



(e) Bills Passed: Policing Alternatives

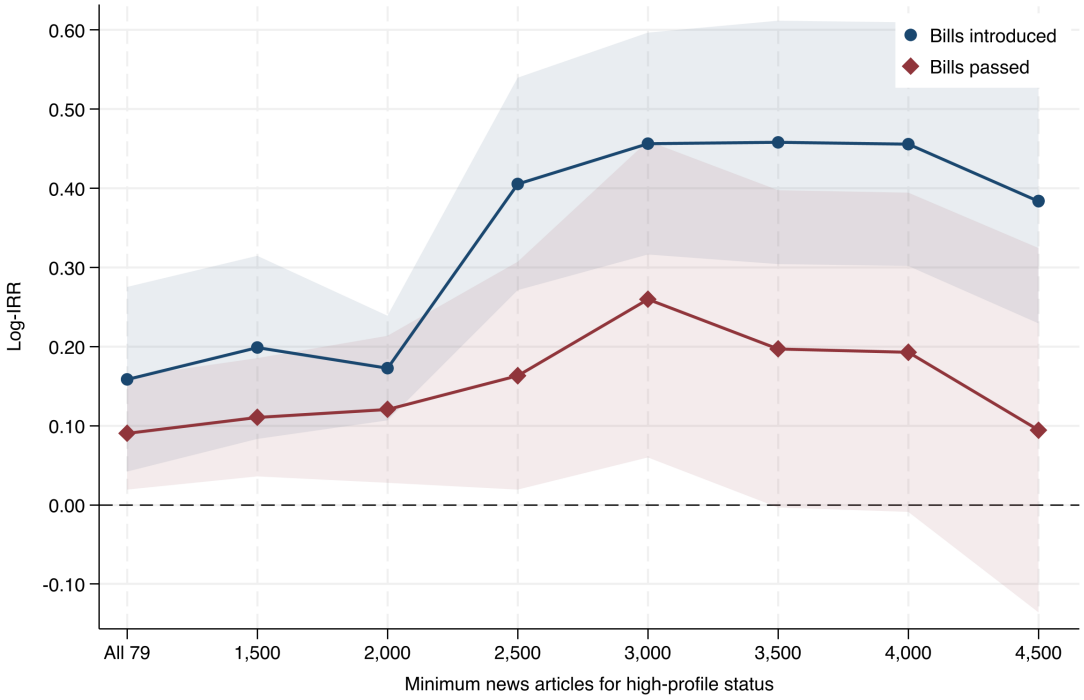


(f) Bills Passed: Other



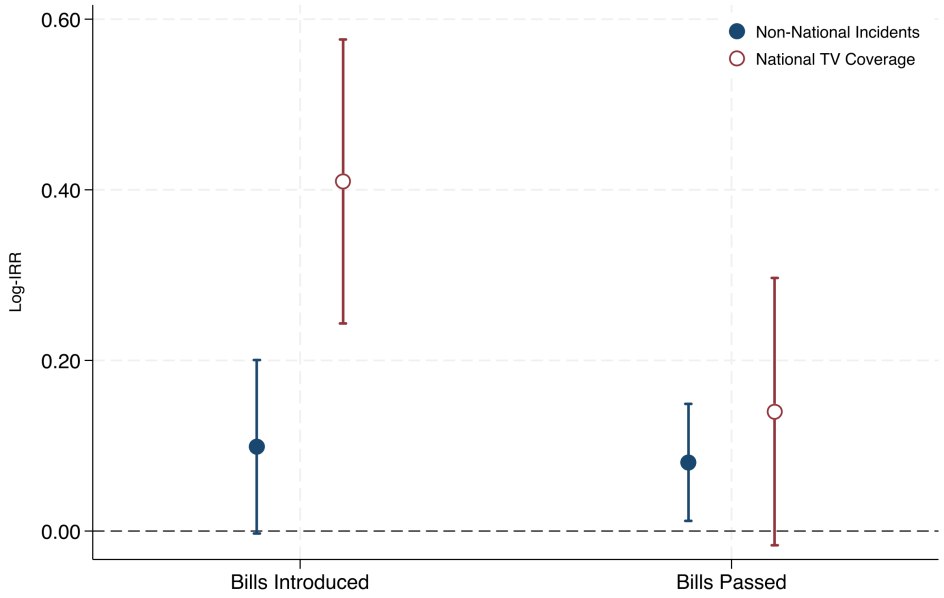
Notes: Each panel plots event-study coefficients from OLS (circles) and de Chaisemartin & D'Haultfœuille (2024) (diamonds) estimators of biennial state-level bill counts on event-time dummies relative to the biennium of a high-profile police killing. The treatment variable is the count of high-profile police killings. All specifications include state and biennium fixed effects, baseline controls and time-varying controls, and are population weighted. Standard errors are clustered at the state level. Bars represent 95% confidence intervals.

Figure A4
Sensitivity of the Legislative Response to the High-Profile Definition



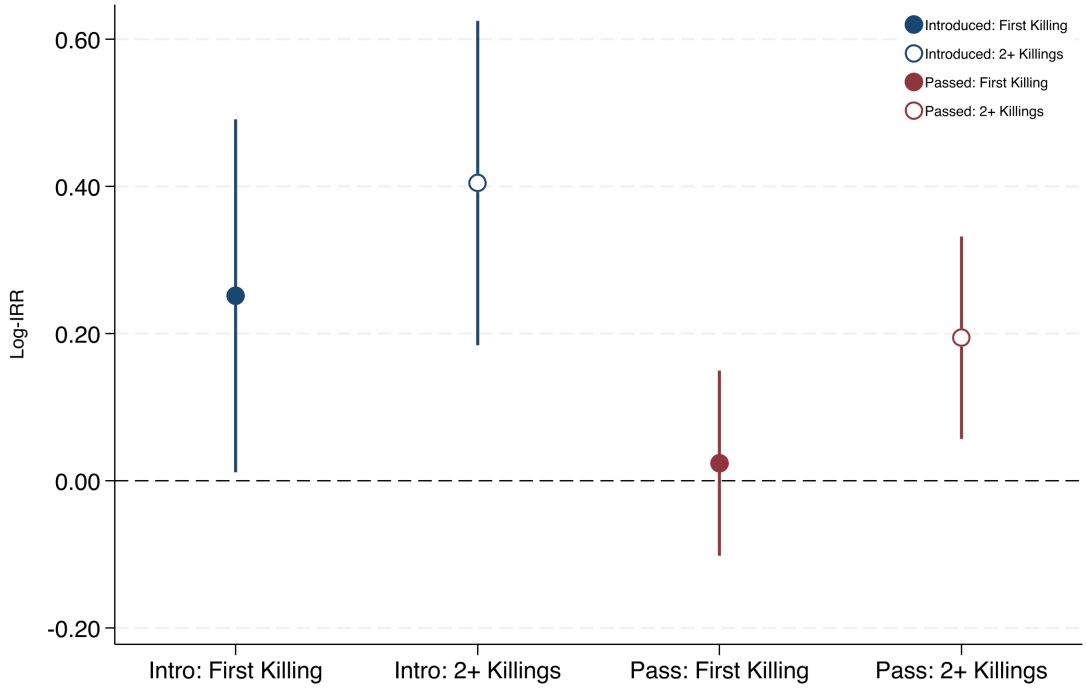
Notes: Each point plots the coefficient on the count of high-profile police killings, with shaded 95% confidence bands, from a separate re-estimation of the main specification. Moving right along the horizontal axis, an incident counts as high profile only if it attracted at least the stated number of news articles; the leftmost point retains all 79 incidents and reproduces the baseline estimate. News counts are from the Premkumar data and range from 1,010 to 400,000 articles, with a median of 2,400, so thresholds below 1,010 are non-binding. Each re-estimation rebuilds the treatment variable under the new definition and leaves everything else unchanged: Poisson with state and year fixed effects, population weights, the full control set, and standard errors clustered by state. The estimation sample is fixed at 612 state-years throughout.

Figure A5
Legislative Responses by National Television Coverage



Notes: Each point reports a coefficient estimate from a Poisson regression, with bars representing 95% confidence intervals. High-profile police killings are divided into two mutually exclusive categories according to whether the incident appeared on national television news: "National TV Coverage" and "Non-National Incidents." The lagged counts of incidents in both categories are entered jointly in the same regression for each outcome. The dependent variables are the counts of policing-related bills introduced and passed in state legislatures. All specifications include state and year fixed effects, baseline controls, and time-varying controls, and are weighted by state population. Robust standard errors are clustered at the state level. National television coverage is identified using the Vanderbilt Television News Archive. Poisson coefficients are reported on the log scale; a coefficient $\hat{\beta}$ implies a percentage change in the expected count equal to $100 \times [\exp(\hat{\beta}) - 1]$.

Figure A6
Legislative Responses to One versus Multiple High-Profile Police Killings



Notes: This figure reports the preferred Poisson fixed-effects estimates. "One killing" is an indicator equal to one if the lagged count of high-profile police killings equals one, while "Multiple killings" is an indicator equal to one if the lagged count is two or more. The omitted category is no high-profile police killing in the prior year. The estimates assess whether legislative responses are concentrated among state-years with a single incident or whether multiple incidents generate additional policy responses. Poisson coefficients are reported on the log scale; a coefficient $\hat{\beta}$ implies a percentage change in the expected count equal to $100 \times [\exp(\hat{\beta}) - 1]$. Bars report 95% confidence intervals.

A.2 Tables

Table A1
Effect of High-Profile Police Killings: Spillover Analysis

	Bills Introduced		Bills Passed	
	(1) Neighbor	(2) Division	(3) Neighbor	(4) Division
Panel A: Count Treatment				
HP Killing (Count)	0.146**	0.169**	0.084**	0.088**
	(0.063)	(0.073)	(0.040)	(0.038)
Spillover: Neighboring States	−0.027		−0.010	
	(0.022)		(0.026)	
Spillover: Census Division		0.034		0.003
		(0.039)		(0.027)
Panel B: Binary Treatment				
HP Killing (Binary)	0.269**	0.286**	0.046	0.050
	(0.114)	(0.120)	(0.057)	(0.057)
Spillover: Neighboring States	0.011		−0.105	
	(0.091)		(0.071)	
Spillover: Census Division		0.126		0.023
		(0.086)		(0.052)
Observations	612	612	612	612

Notes: Robust standard errors clustered at the state level are in parentheses. All specifications report Poisson fixed-effects estimates, are weighted by state population, and include state and year fixed effects, baseline controls, and time-varying controls. Panel A uses the lagged count of high-profile police killings; Panel B uses a lagged indicator for the occurrence of at least one high-profile police killing. Columns (1) and (3) control for the corresponding lagged police-violence measure in contiguous neighboring states. Columns (2) and (4) control for the corresponding lagged police-violence measure in the same Census division. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A2
Non-High-Profile Police Violence and Pre-BLM Robustness

	(1) Pre-BLM	(2) Both	(3) HP Only	(4) Non-HP
Panel A: Bills Introduced				
HP Killing (Count)	0.149***	0.159***	0.159***	
	(0.026)	(0.060)	(0.059)	
Non-HP Killing	−0.002	0.000		0.000
	(0.003)	(0.002)		(0.002)
Panel B: Bills Passed				
HP Killing (Count)	0.251***	0.091**	0.091**	
	(0.048)	(0.036)	(0.036)	
Non-HP Killing	−0.003	−0.001		−0.001
	(0.003)	(0.002)		(0.003)
Observations	459	612	612	612

Notes: Robust standard errors clustered at the state level are in parentheses. All specifications report Poisson estimates, include baseline and time-varying controls, and are weighted by state population. Column (1) jointly includes lagged high-profile and total police killing counts and restricts the sample to the pre-BLM period (year < 2014). Columns (2)–(4) use the full 2005–2016 sample. Column (2) jointly includes both lagged police-violence measures. Column (3) includes only lagged high-profile police violence. Column (4) includes the lagged total count of police killings, without restricting the measure to high-profile incidents. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A3
Party Heterogeneity by Bill Category (Bills Introduced and Passed)

	All (Introduced)		All (Passed)		Force		Technology		Standards	
	Base	Party	Base	Party	Base	Party	Base	Party	Base	Party
HP Killing (Binary)	0.271**	−0.261*	0.052	−0.333***	0.695***	0.407	−0.123	0.750**	−0.068	−0.120
	(0.115)	(0.144)	(0.055)	(0.127)	(0.235)	(0.633)	(0.214)	(0.293)	(0.073)	(0.420)
HP Killing × Dem. leg.		0.738***		0.554***		0.623		−0.353		0.132
		(0.206)		(0.166)		(0.696)		(0.340)		(0.438)
HP Killing × Rep. leg.		0.299*		0.222		−0.015		−1.640**		−0.062
		(0.169)		(0.168)		(0.654)		(0.745)		(0.447)
Baseline mean	51.51		17.79		0.19		0.62		2.46	
Observations	612		612		492		540		612	

	Training		Accountability		Officer Benefits		Policing Alt.		Other	
	Base	Party	Base	Party	Base	Party	Base	Party	Base	Party
HP Killing (Binary)	−0.001	0.480***	−0.336**	0.952	0.046	−0.663**	0.037	0.108	0.075	−0.346
	(0.188)	(0.182)	(0.166)	(0.795)	(0.065)	(0.332)	(0.372)	(0.946)	(0.083)	(0.306)
HP Killing × Dem. leg.		−0.847***		−1.289		0.910**		0.700		0.635*
		(0.196)		(0.787)		(0.354)		(0.987)		(0.341)
HP Killing × Rep. leg.		0.000		−1.436*		0.569		−1.570		0.217
		(.)		(0.779)		(0.366)		(1.030)		(0.368)
Baseline mean	1.30		1.11		4.40		0.25		7.93	
Observations	600 / 595		576		612		444		612	

Notes: Robust standard errors clustered at the state level are in parentheses. All specifications report Poisson fixed-effects estimates, are weighted by state population, and include state and year fixed effects, baseline controls, and time-varying controls. The first pair of columns reports results for all bills introduced; the remaining column pairs report results for bills passed. The treatment variable is a binary indicator for a high-profile police killing in the prior year. Base columns include only the treatment indicator, while Party columns add interactions with indicators for Democratic and Republican legislature control; the omitted category is split legislature. Where the Base and Party specifications have different sample sizes, both observation counts are reported in Base/Party order. For interpretation, a Poisson coefficient $\hat{\beta}$ implies a proportional change in the expected count equal to $100 \times [\exp(\hat{\beta}) - 1]$ percent. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A4
Legislative Responses to One versus Multiple High-Profile Police Killings

	(1) All	(2) Force	(3) Tech.	(4) Standards	(5) Training	(6) Accountability	(7) Benefits	(8) Alt.	(9) Other
Panel A: Bills Introduced									
First killing	0.251**	0.696***	0.377**	0.094	0.142	−0.033	0.319**	0.361	0.267**
	(0.122)	(0.260)	(0.152)	(0.095)	(0.162)	(0.194)	(0.162)	(0.352)	(0.134)
Multiple killings	0.405***	0.687	0.065	0.540***	0.534***	0.133	0.600***	0.210	0.306**
	(0.112)	(0.620)	(0.209)	(0.117)	(0.202)	(0.220)	(0.144)	(0.270)	(0.132)
Baseline mean	51.51	0.52	2.05	7.53	3.27	3.22	14.39	0.60	21.31
Observations	612	492	612	612	612	612	612	540	612
Panel B: Bills Passed									
First killing	0.024	0.009	0.056	−0.207**	−0.097	−0.401**	−0.009	−0.026	0.081
	(0.064)	(0.439)	(0.200)	(0.093)	(0.219)	(0.178)	(0.068)	(0.411)	(0.082)
Multiple killings	0.195***	0.132	−1.263***	0.513***	0.468*	−0.165	0.327***	0.257	0.042
	(0.070)	(0.906)	(0.314)	(0.116)	(0.247)	(0.192)	(0.109)	(0.316)	(0.149)
Baseline mean	17.79	0.19	0.62	2.46	1.30	1.11	4.40	0.25	7.93
Observations	612	324	540	612	600	576	612	444	612

Notes: Robust standard errors clustered at the state level are in parentheses. All specifications report Poisson fixed-effects estimates, are weighted by state population, and include state and year fixed effects, baseline controls, and time-varying controls. The sample covers 2005–2016. "First killing" is an indicator equal to one when the lagged count of high-profile police killings equals one. "Multiple killings" is an indicator equal to one when the lagged count is two or more. The omitted category is no high-profile police killing in the prior year. A Poisson coefficient $\hat{\beta}$ implies a percentage change in the expected count equal to $100 \times [\exp(\hat{\beta}) - 1]$. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A5
Predicting Police Violence

	High-Profile Violence			Total Police Killings		
	(1)	(2)	(3)	(4)	(5)	(6)
L. Bills introduced	0.000			−0.016		
	(0.001)			(0.028)		
L. Bills passed (total)		−0.001			−0.178	
		(0.003)			(0.126)	
L. Passed: Use of force			0.044			0.200
			(0.097)			(3.104)
L. Passed: Technology			0.154*			−0.981
			(0.084)			(1.063)
L. Passed: Standards			−0.057*			0.519*
			(0.030)			(0.275)
L. Passed: Training			−0.072			−2.259*
			(0.048)			(1.206)
L. Passed: Accountability			0.034			1.107
			(0.071)			(1.414)
L. Passed: Benefits			0.008			0.039
			(0.011)			(0.228)
L. Passed: Alt. policing			0.021			0.216
			(0.089)			(2.219)
L. Passed: Other			−0.009			−0.094
			(0.009)			(0.137)
Log population	4.228	4.133	4.410	369.558***	357.212***	360.102***
	(5.561)	(5.577)	(6.105)	(117.320)	(104.790)	(115.753)
Republican governor	−0.263	−0.267	−0.342*	1.984	1.633	2.029
	(0.201)	(0.200)	(0.197)	(4.152)	(4.071)	(3.807)
Democrat governor	0.222	0.217	0.152	2.718	2.531	3.205
	(0.191)	(0.193)	(0.151)	(4.206)	(4.125)	(3.680)
Republican legislature	−0.332	−0.339	−0.347	8.675**	8.110*	7.606*
	(0.272)	(0.267)	(0.252)	(4.098)	(4.050)	(4.395)
Democrat legislature	−0.314	−0.320	−0.328	9.975	9.428	8.649
	(0.277)	(0.275)	(0.250)	(6.060)	(6.051)	(6.158)
Split legislature	−0.468	−0.472	−0.506*	10.570*	10.139*	9.213
	(0.322)	(0.320)	(0.301)	(5.668)	(5.603)	(5.811)
Black share	−72.102	−72.080	−71.188	418.912	413.345	361.407
	(46.388)	(46.449)	(43.450)	(393.378)	(385.695)	(374.611)
Hispanic share	31.415	31.277	22.814	−200.602	−199.414	−266.130
	(43.479)	(43.617)	(37.694)	(294.540)	(286.215)	(322.712)
Female share	231.836	234.067	213.689	609.226	836.626	521.658
	(232.617)	(232.648)	(232.807)	(1873.399)	(1836.107)	(1916.017)
Unemployment rate	0.117	0.118	0.111	0.114	0.049	0.734
	(0.105)	(0.103)	(0.104)	(1.490)	(1.516)	(1.701)
Violent crime index	0.000	0.000	0.000	−0.001***	−0.001***	−0.001***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Income per capita	12.335	12.362	11.674	62.793	65.849	62.778
	(8.784)	(8.781)	(8.148)	(78.029)	(81.013)	(77.448)
Poverty rate	0.056	0.056	0.057	−1.333*	−1.321*	−1.137*
	(0.067)	(0.067)	(0.065)	(0.788)	(0.788)	(0.669)
Officers per capita	0.230	0.230	0.172	−12.420*	−12.252*	−11.891*
	(0.422)	(0.420)	(0.428)	(6.873)	(6.251)	(6.426)
Age share (13–61)	124.354**	123.800**	115.920**	966.409	898.782	1032.014
	(47.746)	(48.136)	(46.108)	(821.695)	(790.321)	(772.962)
Age share (62+)	83.321*	83.097*	74.968*	−259.181	−292.033	−91.974
	(47.301)	(47.292)	(41.837)	(640.966)	(646.298)	(553.276)
Constant	−426.328**	−425.805**	−403.096**	−7359.903**	−7261.670**	−7223.697**
	(193.647)	(193.719)	(172.880)	(2957.310)	(2820.631)	(2872.008)
Observations	561	561	561	561	561	561
R-squared	0.439	0.439	0.475	0.547	0.552	0.571

Notes: Robust standard errors in parentheses. All specifications use OLS with state and year fixed effects. Cols (1)–(3) use the number of high-profile police violence as the dependent variable. Cols (4)–(6) use the total count of police killings as the dependent variable. Col (1) and (4): lagged total bills introduced. Col (2) and (5): lagged total bills passed. Col (3) and (6): lagged bills passed by category (use of force, technology, standards, training, accountability, benefits, alternative policing, other). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A6
Placebo Check: High-Profile Police Killings and Support for Other Spending Categories

	(1) Welfare	(2) Health Care	(3) Education	(4) Transportation
HP Killing	−0.020	−0.020	−0.009	−0.012
	(0.016)	(0.013)	(0.021)	(0.022)
Democrat	0.184***	0.256***	0.193***	0.084***
	(0.006)	(0.006)	(0.006)	(0.006)
Republican	−0.062***	−0.134***	−0.088***	−0.069***
	(0.005)	(0.007)	(0.007)	(0.007)
Baseline mean	0.278	0.515	0.666	0.555
County fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Observations	70,898	70,889	70,859	70,804
Counties	2,372	2,369	2,373	2,370

Notes: Standard errors clustered at the county level are in parentheses. All specifications are estimated using OLS, include county and year fixed effects, and use CES survey weights. The analysis uses the original Premkumar high-profile police-killing sample and CES data from 2014–2016. The dependent variables are indicators for supporting increased government spending in the stated category. Column (1) reports support for increased welfare spending; Column (2), health-care spending; Column (3), education spending; and Column (4), transportation spending. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A7
High-Profile Police Killings and Other Police Attitudes

	Police Presence		Body Cameras		Rating Local Police		Feeling Safe	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
HP Killing	−0.011	0.020	−0.008	−0.004	0.019	−0.048*	−0.021	−0.033
	(0.028)	(0.044)	(0.016)	(0.023)	(0.032)	(0.029)	(0.027)	(0.034)
Democrat	−0.110***	−0.108***	0.036***	0.036***	−0.019***	−0.020***	−0.030***	−0.030***
	(0.006)	(0.006)	(0.003)	(0.003)	(0.007)	(0.007)	(0.005)	(0.005)
Republican	0.215***	0.214***	−0.052***	−0.051***	0.100***	0.099***	0.100***	0.099***
	(0.005)	(0.005)	(0.004)	(0.004)	(0.007)	(0.007)	(0.004)	(0.004)
HP Killing × Democrat		−0.092**		0.014		0.068**		−0.009
		(0.041)		(0.019)		(0.035)		(0.020)
HP Killing × Republican		0.021		−0.032		0.129**		0.056**
		(0.046)		(0.037)		(0.062)		(0.022)
Baseline means	0.492 (Dem); 0.633 (Rep)		0.960 (Dem); 0.775 (Rep)		0.452 (Dem); 0.644 (Rep)		0.791 (Dem); 0.916 (Rep)	
Party interactions	No	Yes	No	Yes	No	Yes	No	Yes
County and year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	115,647	115,647	115,659	115,659	66,454	66,454	115,567	115,567
Counties	2,652	2,652	2,652	2,652	2,389	2,389	2,652	2,652

Notes: County-clustered standard errors are in parentheses. All specifications are estimated using OLS, include county and year fixed effects, and use CES survey weights. The outcomes are support for increasing police presence (Columns (1)–(2)), support for requiring police body cameras (Columns (3)–(4)), rating local police (Columns (5)–(6)), and feeling mostly or somewhat safe around police (Columns (7)–(8)). Police presence, body-camera support, and feelings of safety are observed in the 2016, 2020, and 2022 CES waves; police grades are observed in 2016 and 2018. Independent voters are the omitted party group. The HP Killings coefficient reports the effect for Independents, and the interaction terms report differences for Democrats and Republicans. Baseline means are survey-weighted pretreatment means among respondents in treated counties. The treatment combines the original Premkumar incidents with the expanded set of high-profile police-killing incidents through 2022 collected by the author; treated counties are those newly treated after 2016, and counties treated earlier are dropped. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A8
Public Support and Policing Legislation: Placebo Support Measures

	Dependent Variable: Policing Bills Passed				
	(1)	(2)	(3)	(4)	(5)
Support Decline (Law Enforcement)	0.079**				
	(0.039)				
Support Decline (Welfare)		−0.007			
		(0.042)			
Support Decline (Health Care)			0.017		
			(0.040)		
Support Decline (Education)				−0.004	
				(0.040)	
Support Decline (Transportation)					0.070
					(0.050)
County fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	1,492	1,494	1,492	1,488	1,486
County clusters	746	747	746	744	743

Notes: Standard errors clustered at the county level are in parentheses. The dependent variable in every column is the number of policing bills passed. Each column reports a separate Poisson regression using the decline in support for increasing government spending in the indicated category. Support decline is measured between the 2014 and 2016 CES surveys in 10-percentage-point units. All specifications include county and year fixed effects and demographic and economic controls, and use population weights. The sample requires at least five CES respondents in both survey waves. Coefficients are reported as log incidence-rate ratios. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

B Appendix B: Training the Policing Bill Topic Classifier

This appendix documents the process for training the Policing Bill Topic Classifier. Specifically, I perform a supervised fine-tuning task using GPT-4o Mini by OpenAI,[31] using the National Conference of State Legislatures (NCSL) policing legislation database as training data (NCSL, 2020). Table B1 reports the distribution of bill topics in the NCSL database used as training data.

Table B1
Distribution of Bill Topics in the NCSL Database

Topic	Count
Other Issues	2,094
Standards	1,833
Employment and Labor	1,671
Training	1,142
Officer Safety and Wellbeing	986
Use of Force	864
Technology	758
Oversight	709
Investigations and Discipline	567
Data and Transparency	551
Policing Alternatives and Collaboration	514
Certification	422
Total	12,111

Notes: The table reports the distribution of bill topics in the NCSL policing legislation database used as training data for the topic classifier. Only topics with more than 400 observations are included to ensure sufficient data for the fine-tuning process.

Since some bill topics serve similar legislative functions, I merge several categories prior to training the classifier. Specifically, I combine training and certification into a single *training and certification* category, and merge oversight, investigations and discipline, and data and transparency into a single *accountability and oversight* category. Similarly, I merge employment and labor with officer safety and wellbeing into a single *officer benefits and protections* category. Use of force, technology, and policing alternatives and collaboration are kept as separate categories. Bills that do

[31] I also experimented with the full GPT-4o model, but GPT-4o Mini yielded superior classification performance, likely due to its better calibration for structured classification tasks with limited training data.

not fit into any of these categories are classified as *other*. The final classification scheme therefore consists of eight categories: use of force, technology, policing standards, training and certification, accountability and oversight, officer benefits and protections, policing alternatives and collaboration, and others. I then use all bills classified into the seven non-other categories as labeled training data. Following standard practice in natural language processing and machine learning, I split the data into 80% training (6,941 bills), 10% validation (868 bills), and 10% test (868 bills).

Supervised Fine-Tuning Procedure I fine-tune GPT-4o Mini using a supervised learning approach in which each training example consists of three components: a system prompt, a user message, and an assistant response. The system prompt instructs the model to act as a classifier for state-level policing bills and to return a valid JSON object containing binary indicators (0 or 1) for each of the seven bill categories. The user message provides the bill title and summary as input. The assistant response provides the ground truth labels from the NCSL database as a JSON object. This is a multi-label classification task, meaning a single bill can belong to more than one category simultaneously, and the final output for each bill is a binary indicator for each of the seven bill categories. The training and validation sets are formatted in this way and provided to the OpenAI fine-tuning API to train the model.

Model Performance Table B2 reports the per-label performance of the fine-tuned GPT-4o Mini model on the held-out test set. Overall, the model achieves strong performance, with a weighted F1 score of 0.736 and a label-level accuracy of 0.927. The worst-performing category is policing standards, which has a relatively low F1 score of 0.538, likely reflecting its broad and overlapping definition with other categories. Excluding policing standards, the weighted F1 score improves to 0.791 and the label-level accuracy to 0.939. Compared to the pre-trained baseline models, which achieve weighted F1 scores of 0.597 and 0.583 for GPT-4o full and GPT-4o Mini respectively, the fine-tuned model represents a substantial improvement in classification performance.

I then apply the fine-tuned classifier to the full 2005–2016 legislation dataset to assign topic labels to each policing-related bill, enabling the analysis of legislative responses by bill category

presented in Section 4.

Table B2
Policing Bill Topic Classifier: Per-Label Performance

Label	Support	Accuracy	Precision	Recall	F1
Technology	92	0.972	0.915	0.815	0.862
Officer Benefits and Protections	264	0.878	0.797	0.803	0.800
Use of Force	76	0.964	0.846	0.724	0.780
Training and Certification	135	0.932	0.822	0.719	0.767
Policing Alternatives and Collaboration	53	0.968	0.805	0.623	0.702
Accountability and Oversight	142	0.907	0.740	0.662	0.699
Standards	148	0.865	0.648	0.459	0.538

Notes: The table reports per-label classification performance of the fine-tuned GPT-4o Mini model on the held-out test set of 868 bills. Support refers to the number of bills in each category in the test set.