Generational Imprints: How Political Events Shape Cohorts*

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This article investigates generational imprinting, the process by which salient shocks to the political information environment produce differential attitude change across age groups. Focusing on the killing of George Floyd and the subsequent Black Lives Matter protests as a quasi-natural experiment, I analyze attitudes toward U.S. law enforcement among non-Hispanic White Americans using three survey studies that collectively span the period from 2016 to 2022. Analyses show that (1) changes in attitudes toward law enforcement were greater among younger age groups than older ones; (2) Democrats and Independents drove these changes, with largely stable trajectories among Republicans; (3) these effects persisted among younger individuals, while fading among older individuals; and (4) political attentiveness amplified cohort-based polarization. Findings indicate that cohort differentiation, in combination with political field dynamics, may drive aggregate political change in response to political shocks.

Introduction

Change in political culture is typically gradual. In conventional accounts, the engine of this change is *cohort replacement*, a process in which successive cohorts pass through formative years, settle into relatively durable worldviews, and gradually replace older cohorts. As a result, aggregate political change mostly resides in the replacement of old cohorts by the new ones.

At the heart of cohort replacement theory is the notion that formative experiences in early life shape people's long-term political orientations. The formative experiences scholars point to, however, are often analytically distal. While historical or structural factors are typically invoked to contextualize cohorts—chronicling, for instance, the *Children of Great Depression* (Elder 1999)—it remains unclear how these contextual processes meaningfully differentiate cohorts across diverse political divides. One way to address this limitation is to leverage exogenous and high-salience shocks that saturate the political information environment. These shocks align exposure timing across the population,

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providing an analytically valuable window for investigating opinionation dynamics across cohorts in natural settings with clear analytic designs (Sears and Valentino 1997).

In this article, I use the case of the killing of George Floyd on 2020 in Minneapolis, Minnesota, and the subsequent Black Lives Matter (BLM) protests, to examine when, how, and for whom political attitudes change. Focusing on how non-Hispanic White Americans changed their attitudes toward U.S. law enforcement in response to this event, I find that (1) change in attitudes was greater among younger individuals (ages 18-24) than the older ones (ages 25+); (2) this was concentrated among Democrats and Independents, with largely stable trajectories for Republicans; (3) these effects were persistent for at least half a year among younger age grouups while fading among older age groups; and (4) political attentiveness amplified these cohort-based differences.

I propose two opinionation dynamics—*cohort differentiation* and *political sorting*—and examine their interactions to understand long-lasting generational imprints on political orientations. Integrating these dynamics into a unified framework, I offer observable implications, which clarify *who* changes in a particular time period and the *direction* of this change. Following classical sociological accounts (Mannheim 1952; Ryder 1965), I argue that an analytically more useful conceptualization of cohorts requires a disaggregation: rather than adopting a strategy that totalizes cohorts and attributes the explanatory power to diverging political circumstances, we can look at "cohort-units" within each cohort: clusters oriented toward similar interpretive frameworks (Mannheim 1952).

These findings contribute to several subfields in sociology. First, I suggest that the mere fact of being formed in the same historical period of time does not necessarily lead to convergent political tastes; cohorts become meaningful only when they are mediated by the cultural logics of the political field. Thus, cohort effects are not—according to this account—self-sufficient drivers of aggregate political change. Second, I show that public opinion dynamics cannot be reduced to elite messaging alone; instead, they hinge on shared experiences, life-course timing, and sensitivity to change. Therefore, I reframe event-led political socialization as a joint product of life-course sensitivity and field-level political competition. Third, I provide a useful corrective to popular narratives about "generations" by conceptualizing the field of politics as a structuring force of shared experiences.

The article proceeds as follows. In the first section, I propose a theoretical framework that combines the dynamics of cohort differentiation and political sorting. Next, I introduce the empirical setting, detail the data sources, outline the main empirical estimands and propose an identification strategy. I then present the findings. In the final section, I discuss the implications of these results for cultural and political sociology, as well as the sociology of aging, socialization, and the life-course.

Theoretical Framework

I bring together two dynamics to explain the process of *opinionation*, i.e., the development of stable evaluations of political objects: cohort differentiation and political sorting. The former allows me to identify *who* forms stable attitudes at any given time while the latter specifies its *direction*. Applied to the case of attitudes toward law enforcement, these dynamics explain how salient political events have differential effects across age groups and prior political dispositions.

The Dynamics of Cohort Differentiation

The question of cohort differentiation, or "cohortization," has long been central in classical sociology (Mannheim 1952; Ryder 1965), with recent accounts in cultural sociology highlighting its role in social and cultural change (Vaisey and Lizardo 2016). Scholars argue that cohort differentiation arises from the interaction of "period effects," i.e., social and political processes shaping the range of potential experiences an individual may have; and "age effects," i.e., the differential responses to environmental stimuli across age groups. Thus, "cohort effects" are often understood as *period effects experienced at a particular age* or *differentiation in these experiences* (Morgan and Lee 2024).

This formulation has important advantages for explaining political change. It fits well with classical socialization paradigms that propose sensitive windows early in life, during which individuals are more susceptible to attitude formation (Alwin and Krosnick 1991; Krosnick and Alwin 1989), and that socializing agents, especially the family (Jennings and Niemi 1968), assume a significant role. It also provides a general account of "fresh contacts," to use a phrase from Mannheim (1952), that allow young cohorts to form new evaluations. The notion that these cohorts carry "the impress of [these contacts] through life" (Ryder 1965:844) is what we may call generational imprinting.[1]

What are the observable implications of cohort differentiation in explaining the dynamics of opinionation? Panel A in Figure 1 presents three opinionation dynamics in a context where there is an exogenous shock to the political information environment, and individual trajectories of political evaluations may depend on one's age group, defined as "the Young" and "the Old."

The first two processes are age-agnostic. On the one hand, people may update their attitudes when presented with new information, regardless of their age or life-stage, and these changes may persist (*Persistent Updating*). On the other hand, people may have prior dispositions that reproduce similar judgments of political objects, and when prompted, they might temporarily update, only to return to their baselines after a certain window of time (*Transient Shock*). The third process—which I call *Differential Response*—advocates separate dynamics for the Young and the Old, thereby introducing cohort differentiation to the opinionation dynamics. In this particular setting, persistent updating is the prevalent process among the Young, suggesting recalibration of attitudes, while the transient shocks are more prevalent among the Old, suggesting the dominant role of dispositions.

[1] See Bartels and Jackman (2014) for a succinct formal treatment that brings Bayesian and generational learning together. In their theory, cohort differentiation endogenously emerges from the interaction of age and period-specific shocks, and the main assumption generating this prediction is the notion that people of different ages possibly attach different *weights* to exogenous political shocks, or proposals, from the environment. The differences in the composition of these proposals lead to a differentiation in cohort experiences, which in turn produce political differentiation.

The primary mechanisms ensuring differential response may vary across models: while a Bayesian learning process may imply that, with age, each new experience will have diminishing weights for the updating process (Bartels and Jackman 2014), it is also plausible to argue that the structuration of the life-course, allowing more exploration in adolescence and early adulthood, may be the main mechanism (Dannefer 1984). Nonetheless, the expectations that the individual-level opinionation dynamics will differ across age groups still emerge across a variety of theoretical models.

This framework is powerful for explaining *who* changes at any given time, though it cannot predict the *direction* of this change—an issue that has frequently haunted[2] the uses of the term "generation" (Kertzer 1983). What, then, is the main driver for directional opinionation? Following Mannheim (1952), I argue that the solution lies in the disaggregation of the cohort. In contrast to an approach that totalizes cohorts, we can analytically look at "cohort-units," clusters oriented toward the same (political) object, but from different, and increasingly antagonistic, perspectives. In contemporary political conditions, these cohort-units are classified, almost naturally, by political parties.

The Dynamics of Political Sorting

To understand directional opinionation, we need to postulate an external domain to which individuals are oriented. When it comes to political opinionation, this domain is the *field of politics* (Martin 2014): political parties and their positions vis-à-vis one another constitute the political domain as a structuration engine, and the communications from the political parties often determine how partisans form evaluative judgments of political objects. When prompted with new information, it is thus very likely that people form competing—partisan—political evaluations (Zaller 1992).

The scholarly literature offers several reasons why this account is well supported by empirical evidence. The structure of political positions in the population seems to be grounded in one's ideological identity, from which most political views are derived (Boutyline and Vaisey 2017). In a similar vein, the partisan cue-taking literature provides ample evidence that evaluations of political objects, as different as policy proposals and personal values, are shaped by elite party dynamics (Bisgaard and Slothuus 2018; Goren, Federico, and Kittilson 2009; Slothuus and Bisgaard 2020). It is possible that this orientation increased over time, too, with people and their views being sorted into their political identities much more extensively (Baldassarri and Gelman 2008; DellaPosta 2020).[3]

Understood within the context of cohortization, the political sorting dynamic implies two empirical expectations. For the Old, the attachment to parties and prior partisan dispositions will shape how individuals react to the new information. For the Young, two concurrent processes are plausible: a

[2]As Ryder (1965:850) puts, "the entry of fresh cohorts into the political stream represents a potentiality for change, but without specification of its *content* or *direction*" (emphases added). Similar issues are raised in Mannheim (1952).

[3]The drivers of this sorting is not particularly consequential for the present account. While "social sorting" is proposed as the main mechanism (Rawlings 2022), the recent evidence suggests that it may be less about demographic sorting and more about the re-organization of the U.S. political field around strong ideological fault lines (Konicki 2025). In both cases, partisan frames still dominate the processes of updating, regardless of how the sorting was achieved.

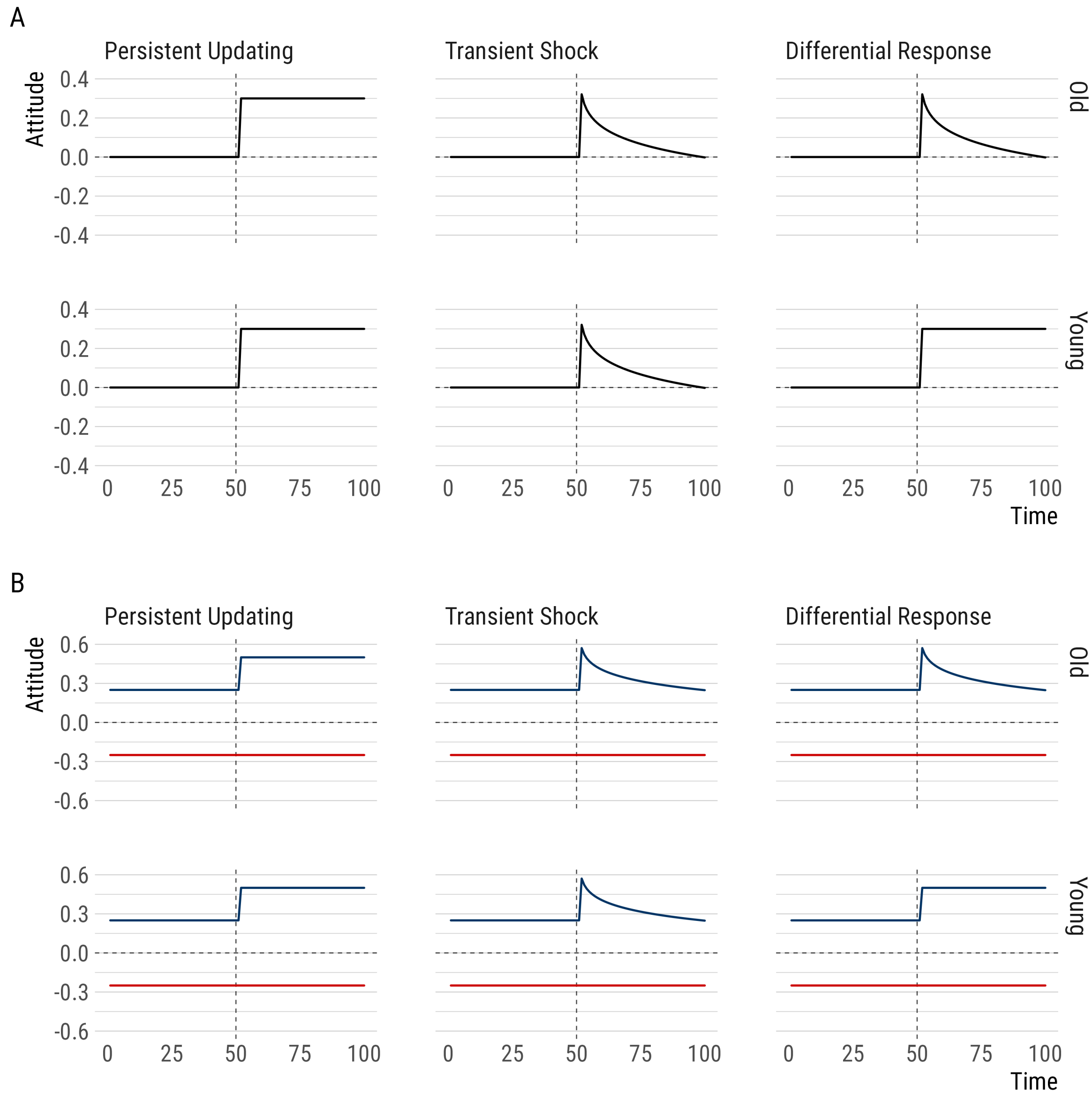

Figure 1: Theoretical Expectations on Individual-Level Attitude Trajectories
Notes: The x-axis shows the time window before and after the event (centralized at $t = 50$), with each tick representing a time period. The y-axis shows one's evaluative judgments on the pertinent political object. Panel A presents dynamics at the population. Panel B presents dynamics across two subgroups, Political Group D and Political Group R.

differential update to the initial position based on partisan cues, or a differential partisan switching following attitude update.[4] In both cases, the empirical patterns are observationally equivalent: the evaluations of the Young will increasingly diverge from each other; put differently, cohort units will become oriented to the same object while their evaluations become strikingly different.

[4]If there are cohort effects, it follows that the Young is also more susceptible to potential partisan realignment.

Panel B in Figure 1 introduces these dynamics into individual-level trajectories in political attitudes. We now have two groups, Political Group *D* and Political Group *R*, with prior dispositions toward the object, though with small differences. When the new information comes to light, the Group *D* evaluates it as favorable (due to elite messaging or protest frames), and updates their attitudes. For Group *R*, the new information does not challenge their prior predispositions; hence, their attitudes do not update in response to this event, and we see a flat trajectory.[5]

Tying the Opinionation Dynamics Together

The dynamics of cohort differentiation and political sorting lead to several theoretical conclusions. While cohort differentiation provides an account for how political shocks affect younger individuals more strongly than older ones—thus addressing the *who* question—the political sorting clarifies the *direction* of change. That is, when changes in the political information environment prompt the younger cohorts to form and rework their attitudes, political sorting channels these developments into the political field, orienting positions in specific directions. Unifying these basic dynamics, we expect that young individuals not only change more strongly, but also maintain their new positions, in part because elite-level signaling anchors these evaluations to a particular partisan line.

Note that this account is silent on the precise cognitive and social mechanisms that generate these dynamics. Cohort differentiation, for instance, may result from developmental processes that regulate Bayesian learning, or the notion that early life is structured to facilitate exploration. Similarly, political sorting may be shaped by cues from partisan elites (Zaller 1992), or by frames circulating through media in response to protests (Wasow 2020). Nor do I exclude the possibility that the partisan differences in post-shock recalibration are rooted in motivated cognition (Guay and Johnston 2022; Kunda 1990). At this stage, the framework proposes *descriptive* predictions on change and its directions, rather than fully adjudicating among these various micro-mechanisms.

I now propose three expectations derived from these considerations. Following with the previous typology, the Young refers to younger age groups while the Old refers to older age groups. Again, these expectations consider a shock to the political information environment, rather than a gradual development. Figure 2 proposes a Directed Acyclic Graph (DAG) that incorporates the necessary elements for these expectations. The first expectation interacts cohort differentiation and political sorting, and suggests that attitude updating depends on age group and political identity:

Expectation 1. *When a political shock changes the political information environment, the Young will update their attitudes more strongly after the event, and this reaction diverges along political identity.*

This expectation has a rather precise unit-specific quantity (Lundberg, Johnson, and Stewart 2021): the difference in attitudes before and after a political shock, $\hat{Y}_i$, stratified by age groups A and po-

[5] An alternative account can suggest that the Political Group *R* moves in the opposite direction, intensifying polarization. This is particularly likely among the Young, but possibly dependent on the specific political event.

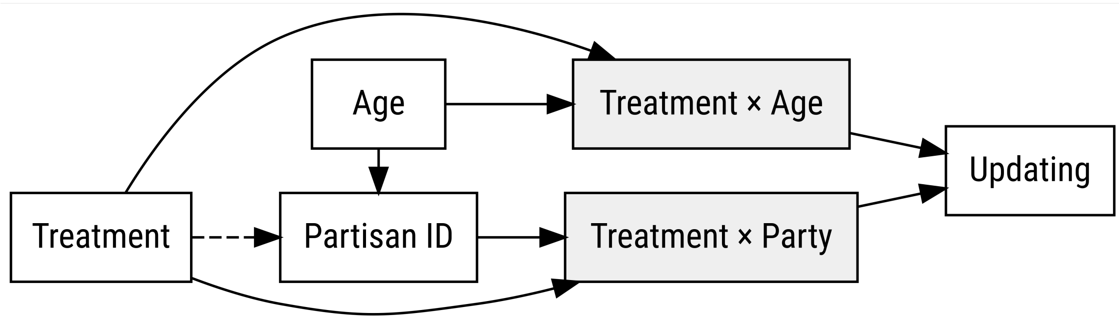

Figure 2: Directed Acyclic Graph for the Theoretical Framework
Notes: Directed Acyclic Graph presenting the proposed relationships. Shaded nodes represent interaction terms. Solid lines represent proposed causal mechanisms. Dashed lines represent potential causal mechanisms.

litical groups G, or $\hat{Y}_i(A, G)$. These groups are, of course, *pseudo-treatments*; the causal mechanisms in question depend on the specific micro-mechanisms proposed, as discussed above.

The generalization of this expectation to multiple windows brings us to the question of trajectories, discussed in Figure 1, and the prediction of differential response across the Young and the Old:

Expectation 2. *Attitude changes after the political shock will endure among the Young over time, while the Old's attitudes will revert, after a suitable time window, to their pre-event baselines.*

The unit-specific quantity, this time, is $\hat{Y}_{it}(A, G)$, where the subscript t refers to each time window *after* the event. Once again, I expect the change in each window to be stratified across age groups A and political groups G. Another—related—quantification is the differential slopes after the event: while the updating dynamic predicts a flat slope, the transience predicts a downward change.

These expectations cover the main opinionation dynamics. There is, however, one additional corollary: the polarization I expect after the event may be more pronounced among people with higher political exposure (Zaller 1992). Considering that the theoretical framework understands the political domain as an external structuration engine, it follows that people with an increased orientation toward the field should be more attuned to opinionation dynamics. This moderation effect can result, at least in principle, from two basic sources: the visibility of the political object, which affects the extent to which people are susceptible to exposure, and inter-individual differences in political attention, which regulates the extent to which people select into the political domain. Hence:

Expectation 3. *The dynamics of differential response will be stronger if* (A) *the political event is more visible and* (B) *the individual is politically more attentive, both of which increases one's political exposure.*

The Empirical Setting

The Empirical Case and Study Design

I apply this framework to the killing of George Floyd on May 25, 2020, and the ensuing Black Lives Matter (BLM) protests. I focus in particular on how non-Hispanic White Americans changed their attitudes toward U.S. law enforcement in response to this event, whether this change differed across age groups and party identities, and whether it persisted or decayed over time. The study window starts as early as 2016, long before the event, and reaches toward the Fall of 2022.

The Setting. On May 25, 2020, in Minneapolis, Minnesota, a 46-year-old Black man, George Floyd, was killed during an arrest when a White police officer pressed his knee on Floyd's neck for approximately 9 minutes 29 seconds—a scene recorded by bystanders, and rapidly disseminated online. Within the next few days, protests erupted nationally in the U.S., as well as over 60 countries, under the banner of BLM. Survey-based crowd counts place total U.S. participation between 15 million and 26 million, making the BLM mobilization the largest sustained protest movement in the U.S. history (Buchanan, Bui, and Patel 2020). Scholars emphasize that the scale of this response was unparalleled, positioning the George Floyd protests as a salient political shock that rapidly re-centered public debate on police violence in the U.S. (Edwards, Lee, and Esposito 2019).

The scale and salience of the George Floyd protests make this event an analytically powerful setting for examining cohort differentiation and political sorting. Since public attention is a scarce resource (Hilgartner and Bosk 1988) and political elites routinely reinforce identity-congenial signals (Zaller 1992), exogenous shocks to the information environment create opportunities for persistent change in attitudes. The killing of George Floyd and the ensuing protests represent precisely such a shock. The previous research indeed showed that the Floyd protests led to measurable changes in attitudes (Gethin and Pons 2024); however, these studies often reported that these attitude changes were not durable (Reny and Newman 2021). None of the scholarly literature acknowledged the fact that the event may have catalyzed *pre-adult socialization* (see Sears and Valentino 1997).

The Target Population. I focus analytically on non-Hispanic White Americans, as the dynamics of political opinionation in response to racialized police violence likely differ across racial groups. For racial minorities, particularly Black Americans, encounters with law enforcement are often shaped by direct or vicarious experiences of discrimination, surveillance, and harm, meaning that minority attitudes are more likely to reflect long-standing structural marginalization that inform prior beliefs and expectations (Jefferson, Neuner, and Pasek 2021). Conversely, among non-Hispanic Whites—who are less likely to experience routine police violence and more likely to benefit from institutional trust—an event like the killing of George Floyd may function as a discrete political shock. That is, the event more plausibly serves as a *socializing incident* than an identity reinforcement one. In this vein, recent research showed that the BLM substantially changed the socialization priorities among non-Hispanic Whites in the U.S. (Anoll, Engelhardt, and Israel-Trummel 2024).

A related reason to this focus is the fact that the non-Hispanic Whites have a higher internal political heterogeneity, compared to substantial levels of partisan sorting among racial minorities, making the former more suitable for an analysis of political sorting. Focusing on non-Hispanic Whites thus allows for a sharper empirical test of cohort differentiation and political sorting dynamics, while also isolating attitudinal updating from patterns of racialized institutional trust.

The Target Quantity. I focus on attitudes toward law enforcement to evaluate the main theoretical expectations—a core political debate in contemporary U.S. politics. I conceptualize these attitudes as evaluative judgments that can reflect long-standing dispositions among individuals, capturing related constructs about "legal cynicism" (Kirk and Papachristos 2011) and institutional trust (Ben-Menachem and Torrats-Espinosa 2024). These attitudes prove especially significant in moments of salience—when the legitimacy of law enforcement becomes a salient issue of public debate—such as during the aftermath of George Floyd's killing and the ensuing BLM protests.

The central empirical estimand is the age and partisan stratified change in average attitudes toward law enforcement between the last pre-event time window and each post-event time window, as well as their aggregation, taking the event point as the central quasi-exogenous treatment.

The Empirical Scope Conditions. While the empirical setting is analytically powerful, it has several scope conditions. First, the salience of this event is also its weakness, limiting the generalizability of opinionation dynamics to cases of atypical political mobilization. Second, the event occurred amid overlapping crises, such as the COVID-19 pandemic and the 2020 election cycle, which potentially introduce confounding shocks to the political information environment. While the analyses below are temporally granular, these confounding events present challenges to the main empirical design. Finally, the design depends on assumptions about differential responsiveness across groups, rather than having a "clean" separation of exposure. All that being said, the empirical setting provides a salient and politically pertinent moment to evaluate the main expectations.

Data Sources

To evaluate these expectations, I use data from three large-scale surveys of American adults: *Democracy Fund + UCLA Nationscape* (Tausanovitch and Vavreck 2021—henceforth, simply *Nationscape*), *Cooperative Congressional Election Study* (Schaffner, Ansolabehere, and Shih 2023, or simply CCES), and the *American National Election Study* (American National Election Studies 2021—ANES).

Nationscape. The Nationscape is a repeated cross-sectional survey, fielded from mid-2019 through the end of 2020, on a large non-probability sample from *Lucid* (now called *Cint*). Surveying roughly 6,250 participants each week over an extended period of time, Nationscape provides detailed data on 312,954 non-Hispanic Whites before and after the killing of George Floyd, and their attitudes toward law enforcement. To increase statistical power, I pooled these weekly installments into 4-week windows and used post-stratification weights to bolster the representativeness of the sample.

CCES. To further extend the observation window and offer a complementary assessment, I employ data from the 2016, 2020, and 2022 waves of the CCES, a large-scale survey program administered by YouGov with each wave featuring more than 40,000 non-Hispanic White participants. The study uses a matched random sample methodology to recruit U.S. participants, complementing it with poststratification weights from the U.S. Census to adjust for sample imbalances. The CCES allows me to (1) replicate, albeit partially, the main results in the Nationscape and (2) extend the 6-month observation window after the killing of George Floyd in the Nationscape up to two years.

ANES. Analyses with repeated cross-section surveys help us understand trajectories across *groups*, but they do not provide direct evidence within *individuals*. Hence, I complement the Nationscape and CCES analyses with panel data from the 2016 and 2020 waves of the ANES. The panel design is particularly well-suited to assess within-person change, though the sample is smaller ($N = 1,912$), limiting precision in subgroup analyses. Nonetheless, the ANES provides a probability-based gold standard, and its panel component is a powerful complement to the cross-sectional analyses. Due to its two-wave structure and sample size, I use the ANES only to evaluate Expectation 1.

In addition to these survey sources, I use crowd-sourced data from the *Crowd Counting Consortium* (CCC)—a project of Harvard Kennedy School and the University of Connecticut—that compiles publicly available data on demonstrations in the United States. Following prior work (Gethin and Pons 2024) as well as suggestions from the CCC team, I filtered for demonstrations protesting issues related to *race* and *racism* within the first three months after the killing of George Floyd, matching this information to the Nationscape at the congressional district level. This data source allows me to test, alongside Nationscape, Expectation 3A. To do so, I leverage the intensity of protests in one's congressional district as a proxy for their—potential—local exposure to BLM mobilization.

Supplemental Materials provide more details about these data sources, as well as decisions related to data processing. Tables S1 through S6 present descriptive statistics for each data source.

Measurement

I operationalize *attitudes toward law enforcement* using three separate survey questions, each featured in one data source: (1) the favorability of the police in Nationscape, (2) one's feelings of safety with the police in CCES, and (3) police thermometer in ANES.[6] The measurement strategy in (1) and (3)

[6]The survey questions are as follows: (1) "Here are the names of some groups that are in the news from time to time. How favorable is your impression of each group or haven't you heard enough to say? — The Police," with response options *very favorable* = 0, *somewhat favorable* = 0.25, *somewhat unfavorable* = 0.75 and *very unfavorable* = 1 ("haven't heard enough" option being the midpoint = 0.50), (2) "Do the police make you feel," with response options *mostly safe* = 0, *somewhat safe* = 1/3, *somewhat unsafe* = 2/3, and *mostly unsafe* = 1, and (3) "I'd like to get your feelings toward some of our political leaders and other people who are in the news these days. I'll read the name of a person and I'd like you to rate that person using something we call the feeling thermometer. Ratings between 50 degrees and 100 degrees mean that you feel favorable and warm toward the person. Ratings between 0 degrees and 50 degrees mean that you don't feel favorable toward the person and that you don't care too much for that person. You would rate the person at the 50 degree mark if you don't feel particularly warm or cold toward the person. How would you rate: 'the police,' " with response options ranging from 0 (very cold) and 100 (very warm).

is relatively straightforward; each capturing evaluative judgments on "the police" as an institution. Of course, the question from CCES taps a more personalized dimension of one's interactions with police officers, and it may not necessarily reflect a politicized response.[7] Despite these differences in emphasis and wording, however, it still taps into an underlying evaluative disposition toward law enforcement, allowing me to provide partial, though converging, evidence on expectations.[8]

The main variation of interest, for all analyses, is *respondent age at the time of the killing of George Floyd*. This presents two significant challenges. First, while "age" is a rather straightforward measure, age effects in cross-sectional surveys may conflate age, period and cohort processes (Fosse and Winship 2019). The fact that the empirical setting involves an event study design means that I cannot reliably distinguish people being in different developmental stages ("age effects") and the fact that people were born in different time periods ("cohort effects"). Second, there is a trade-off between precision in age categories, on the one hand, and statistical efficiency, on the other. I present all analyses with qualitative age categories, while also performing analyses with age splines when pertinent.

Main Moderators. As I expect differential response across partisan lines, I use a three-point party identification—Democrat, Independent, and Republican—in the analyses. To operationalize *political attention*, I generate a general attention scale using two constructs, political interest and political knowledge, the details of which are presented in the Supplemental Materials. To capture a person's exposure to protests, or *protest intensity*, I calculate the number of protests at the congressional district level using CCC, and match the resulting counts to respondents' locations. In both cases, I use equal quartiles to construct ordinal response categories to avoid functional form concerns.

Adjustment Variables. In addition to party, political attention, and protest intensity, all of which serve as moderators, I statistically adjust for one's sex—measured simply as *male* and *female*—and geographic location—measured as congressional district and county—to adjust for compositional differences across gender and region lines. With the risk of having a "bad control" (Cinelli, Forney, and Pearl 2022), I also adjust for education, considering that it may tap into parental socioeconomic status and account for platform effects, given that online samples skew to highly educated.

Identification Strategy

Let Y_{it} be the observed attitude toward law enforcement for individual i and time t. Let A indicate whether individual i is "the Young" ($A = 1$) or "the Old" ($A = 0$), and let P indicate whether the period is post-event ($P = 1$) or not ($P = 0$). The empirical estimand of interest is the differential

[7]That said, Figure S10 in the Supplemental Materials indicate that the responses to this item in 2016 (before the event) sharply diverge by partisan identification, suggesting that the measure was already politicized.

[8]While I am not aware of validity checks concerning police favorability and feelings of safety, the thermometer questions in the ANES received widespread attention. Tyler and Iyengar (2024), for instance, showed that thermometer questions on the Democratic and Republican parties are robust to concerns about selection bias and priming effects. Since the authors found mode effects, however, I analyze the ANES data with multiple modes to ensure validity.

effect of the killing of George Floyd on attitudes toward law enforcement across A, or:

$$[\mathbb{E}(Y_{it}|A = 1, P = 1) - \mathbb{E}(Y_{it}|A = 1, P = 0)] - [\mathbb{E}(Y_{it}|A = 0, P = 1) - \mathbb{E}(Y_{it}|A = 0, P = 0)] \quad (1)$$

Intuitively, this captures whether the changes in attitudes toward law enforcement are larger among younger individuals than it is among older individuals. It is a difference-in-differences setup, with empirical interest in the final differenced quantity. I propose this estimand, alongside a variety of descriptive quantities, to capture the theoretical estimand described above.

I make two additions to this estimand. First, I estimate subgroup-specific difference-in-differences, where the *treated* group is defined as being "young" ($A = 1$) *and* having a specific partisan identity, captured with G (e.g., G = Republican). Second, I allow the basic difference-in-differences estimate to vary across pre and post windows to see heterogeneous trajectories over time. The first addition allows me to test Expectation 1, the second addition allows me to test Expectation 2. Expectations 3A and 3B also extend naturally: I add a third quantity of variation, E, capturing exposure.

Identification Assumptions. I rely on three identification assumptions:

(1) I assume that, absent the event, the average change in attitudes across the post-event window would have been identical between the younger and older groups (*parallel trends*).

(2) I assume that there are no compositional changes that differentially affect younger and older groups in the study window (*stable composition*).

(3) I assume that there are no additional group-specific shocks (*alternative shocks*).

The parallel trends assumption is fundamentally untestable, though strikingly stable pre-event trajectories below suggest that the trends would likely be parallel.[9] To the extent that the Nationscape, CCES, and ANES are high in coverage, it is also possible that the event may have led to differential sample inclusion or attrition, though the descriptive analyses show that there are no strong compositional changes in the period (e.g., Reny and Newman 2021). The third assumption is, however, *naturally* wrong: the historical period saw several other cases of police violence (such as the shooting of Jacob Blake—see Ben-Menachem and Torrats-Espinosa 2024), and the elections in November may have generated differential responses to the police. That said, the *falsity* of this assumption is its *strength*: the event created a political chain—starting with the BLM protests—leading to salient political discussions. More importantly, the Nationscape data is granular enough to see the initial shock and decay dynamics before other salient events come into the picture.

Statistical Strategy. I implement all difference-in-differences analyses with a classic two-way fixed effects (TWFE) framework. More particularly, I estimate:

$$y_{it} = \alpha_i + \theta_t + \beta T_{it} + \gamma_i + \epsilon_{it} \quad (2)$$

[9] Table S7 in the Supplemental Materials formally tests this assumption, showing no pre-trends across models.

where α_i denotes fixed effects for age categories, θ_t denotes fixed effects for time, γ_i denotes adjustment variables, and the coefficient β captures the classic difference-in-differences estimate.

To test Expectation 1, I modify this equation by including treatment interactions with party identity, while I include a third set of interaction variables—protest intensity and political attention—to test Expectations 3A and 3B. To test Expectation 2, I modify the TWFE setup in Equation (2) by adding leads and lags around event time $t = 0$, and estimate separate effects at each time window.

A Note on Treatment Status of Age. The potential outcomes setup aims to clarify basic assumptions; however, the term *effects of age* has a rather ambiguous language, given that the true causal process refers to differential updating, approximated by a specific biological year of age. Since there is no magical age (Miller 1956), all discrete cut-offs delineating the Young and the Old would necessarily be imperfect. I settled on age 25, categorizing people aged 24 or younger as *the Young* and aged 25 and older as *the Old*, considering previous work that suggests age 25 marks a key point in people's dispositional development. In the Supplemental Materials, however, I demonstrate analyses that test how sensitive this choice is to alternative age specifications or functional form assumptions.[10]

Findings

I present the empirical analyses in three sections. First, I explore Expectation 1 and Expectation 2 using the Nationscape and CCES data, showing that (a) change in attitudes toward law enforcement in response to the killing of George Floyd was higher among younger individuals than older ones, (b) the effects were concentrated among Democrats and Independents, with Republicans showing mostly stable trajectories, and (c) these effects were persistent for at least half a year—and up to two years—among younger individuals, while fading among the older ones. Second, I analyze longitudinal panel data to explore whether the level of change was indeed higher among younger people, controlling for all time-invariant characteristics. Third, I evaluate two mechanisms: protest intensity in local environments (Expectation 3A) and inter-individual differences in political attention (Expectation 3B). Table 1 presents a general overview of the analysis plan.

Table 1: The Overview of the Analysis Plan

Analyses	Longitudinal	Expectation (1)	Expectation (2)	Expectation (3A)	Expectation (3B)
Nationscape	-	+	+	+	+
CCES	-	+	+	-	-
ANES	+	+	-	-	-

[10] Figure S6 leverages a moving window approach to estimate difference-in-differences models in Nationscape and CCES by systematically varying the age cut-offs, showing a monotonic decrease in estimates, as expected. Figure S11 uses an age spline to model first-differences in ANES, corroborating the main findings.

The Trajectory of Attitudes Toward Law Enforcement

Figure 3 presents the weighted averages of unfavorable attitudes toward the police before and after the killing of George Floyd. I show the results by cross-classifying age and party groups to account for the considerable heterogeneity in pre-event positions, the initial response to the event, and subsequent trajectories. These estimates provide strong evidence for two theoretical expectations.

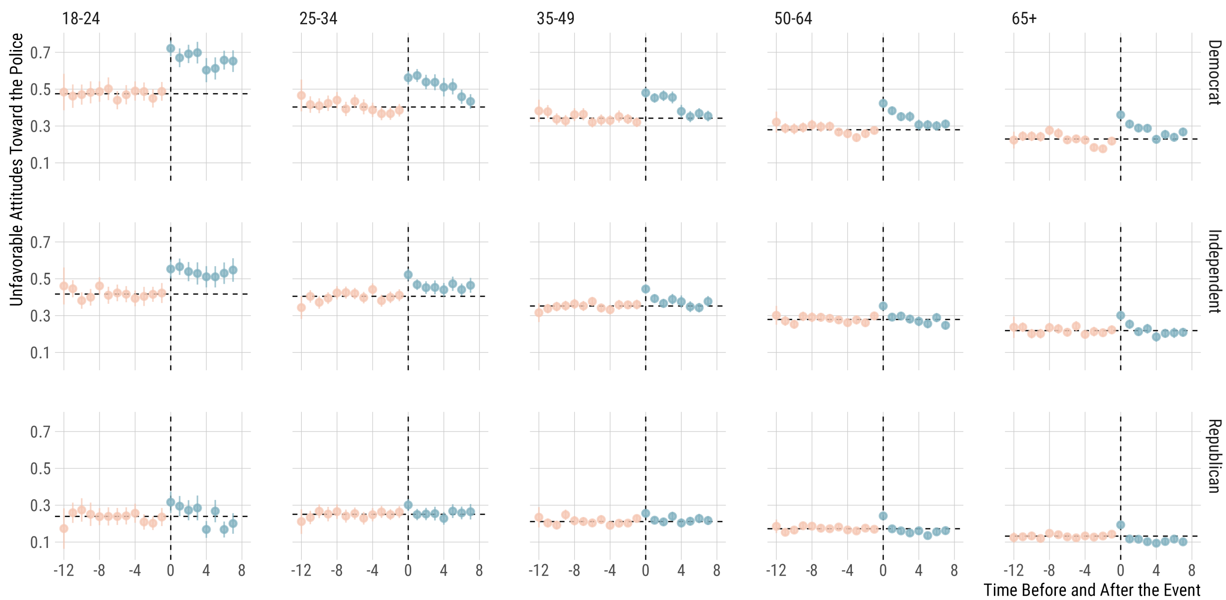

Figure 3: The Trajectory of Unfavorable Attitudes Toward the Police

Notes: The x-axis shows the time window before and after the killing of George Floyd (centralized at $t = 0$), with each tick representing 4-week windows in Nationscape 2019-2020 data file. The y-axis shows the unfavorable attitudes toward the police, normalized between 0 and 1. Each point is a weighted average for a particular age group, party group, and time. The horizontal dashed lines represent pre-event averages for each age and party group.

Expectation 1. The response to the killing of George Floyd was stronger among younger individuals compared to the older individuals. Averaging across all parties, people between the ages of 18 and 24 changed their position by 0.12 on a 0–1 scale (a 0.36 standard deviation change), compared to 0.06 for ages 25-34 (0.18 SD), 0.03 for ages 35-49 (0.10 SD), and 0.02 (0.05 SD) for ages 50-64 and 65+. This, of course, masks substantial partisan heterogeneity: Democrats and Independents aged 18-24 experienced a change of 0.19 (0.58 SD) and 0.12 (0.36 SD), respectively, whereas Republicans experienced a mere 0.01 (0.03 SD) level of change. The results suggest that Republicans may have shifted to the *opposite* direction over the period. I unpack this with DID specifications below.

Expectation 2. The initial changes in attitudes toward the police persisted among younger individuals, but faded for older individuals. Once again, when averaging across parties and both pre-event and post-event windows, the average negative post-event slope is roughly 0.06 steeper for people aged 25-34, 0.09 for ages 35-49, and 0.10 for ages 50-64 and 65+. More intuitively, the findings in

Figure 3 clearly show that the initial changes in Democrats and Independents aged 18-24 persisted over the period—which spans a little more than half a year—while faded for older individuals over the same time window, suggesting age-based differences in people's long-term trajectories.[11]

Table 2: Two-Way Fixed Effects Models on Unfavorable Attitudes Toward the Police

	(1)	(2)	(3)	(4)	(5)
Treatment	9.03	9.04	9.04	9.35	21.35
	(0.81)	(0.77)	(0.77)	(0.81)	(1.11)
Treatment x Independent					-12.13
					(1.43)
Treatment x Republican					-27.26
					(1.44)
Adjustment	-	+	+	+	+
Matched Sample	-	-	-	+	-
Standard Error	Robust	Robust	Clustered	Robust	Robust
N	312954	312954	312954	124085	312954
R2	0.07	0.13	0.13	0.13	0.13

Notes: Two-way fixed effects estimates using the Nationscape data file. Model 1 presents unadjusted estimates. Model 2 adjusts for gender, party identification, education, and their interactions, as well as congressional district fixed effects. Model 3 presents (2) with standard errors clustered at the age group and time level. Model 4 uses a matched sample, where respondents are matched on all covariates within each time window using a variable-ratio nearest neighbor matching. Model 5 presents the same estimates where the treatment indicator is interacted with party identification. The outcome variable is multipled by 100—to range from 0 to 100—to increase legibility.

Table 2 evaluates these findings more formally in a difference-in-differences context, where respondents aged 18-24 make up the "treatment" group and respondents aged 25 and older make up the "control" group. Across all specifications, the differences between groups increased by an average of 0.09 on a 0–1 scale after the event—corresponding to an effect size of $d = 0.27$. Figure 4 presents the findings from a dynamic specification, suggesting that this polarization in attitudes toward law enforcement persisted at least over half a year following the event in the Nationscape data.

Note how the Young Republicans update in the *opposite* direction to have increased favorability of the police. As noted above, this may result from Republican-identifying individuals changing their views in the study window, possibly as a backlash to the BLM mobilization; or from compositional

[11] These findings are highly robust to alternative decisions: the unweighted analyses present similar results (Figure S1), as do the ones where *have not heard enough* responses are dropped rather than being coded as midpoint (Figure S2). The same applies to analyses that use a binary outcome rather than the current categorical measure (Figure S3). In Figure S4, I present the same estimates at the weekly level, rather than 4-week aggregations. While—naturally—less precise, the smoothed linear trajectories clearly show the same findings as described in the main article. Figure S5 shows that using ideological identity rather than partisan identity provides converging evidence.

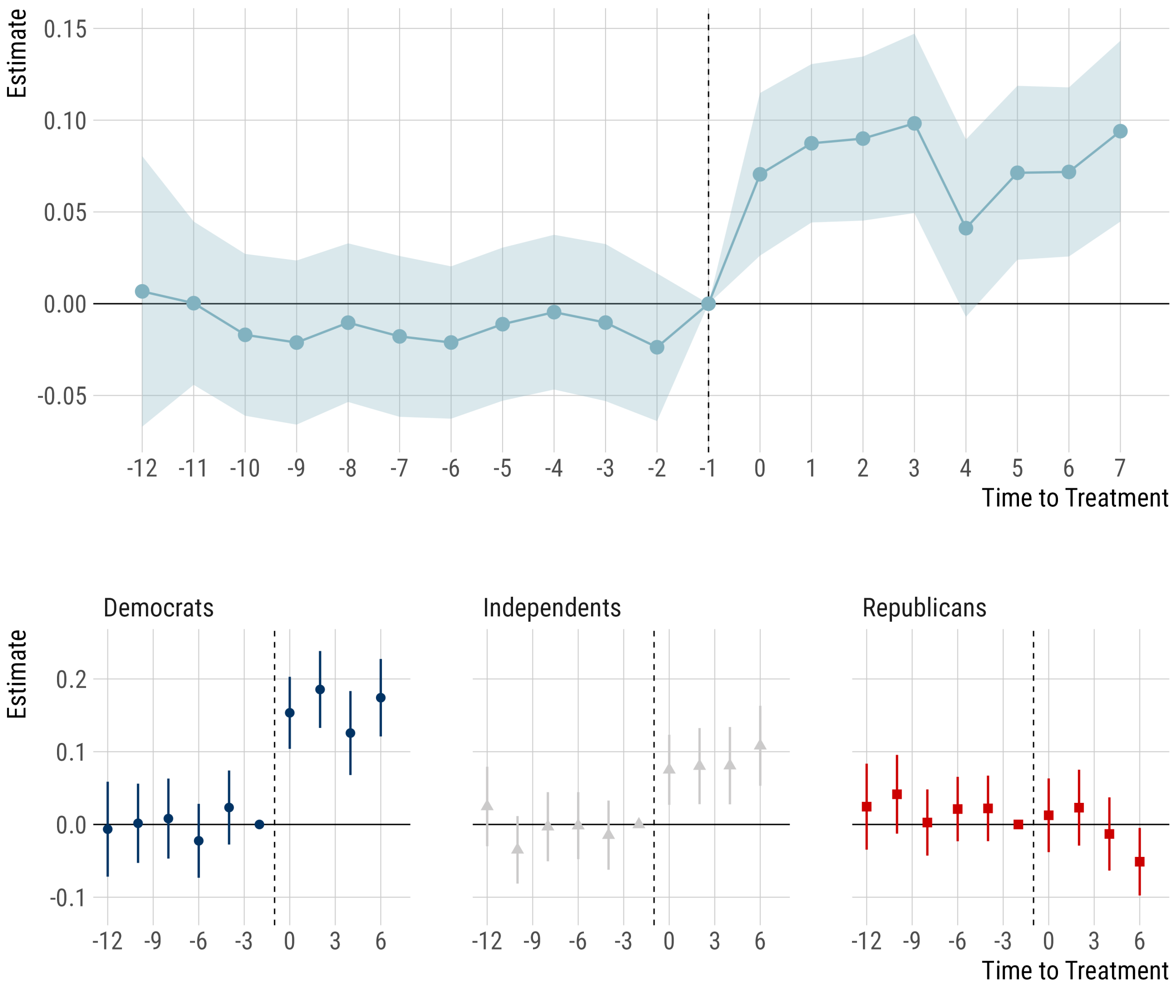

Figure 4: Dynamic TWFE on Unfavorable Attitudes Toward the Police

Notes: The figure presents dynamic TWFE estimates that measure the diverging trajectories of the treated (18-24 year-old respondents) and control (age 25+) groups in Nationscape 2019-2020 data file. The model in the top panel adjusts for gender, party identification, education, and congressional district. The models in the bottom panel perform the same estimations, each subsetting one party identifier in the U.S. To stabilize estimates, time windows in the bottom panel are binned, such that the dynamic TWFE for each panel is estimated for two time periods at each step.

changes where people with less favorability to the police leaving the Republican identification. In both cases, however, we would see an increased polarization, as documented in Figure 4.

While the Nationscape provides a targeted time window to evaluate the main expectations, it does not rely on a probability sample, raising the question of whether these findings could be replicated in alternative surveys. I evaluated this possibility by analyzing three waves of survey data from the CCES. These surveys allow me to investigate whether differential trajectories across age and party groups emerge in a different context, while also enabling to explore whether the effects persist until two years later. Of course, the CCES is not as time-intensive as the Nationscape, and this broader time window limits the ability to make precise causal statements about the underlying process. It also features a different outcome—while the Nationscape measures one's "favorability" toward the police, the CCES measures whether the focal person "feels unsafe" around the American police.

Expectations 1 and 2. Figure 5 presents dynamic difference-in-differences estimates, where people aged 18-24 in 2020 make up the treatment group and people aged 25 and older in 2020 make up the control group. The killing of George Floyd and the subsequent BLM protests resulted in an average polarization of 0.10 on a 0–1 scale. This corresponds to an effect size of $d = 0.36$—relatively close to the estimate I recovered from the Nationscape. Moreover, this difference remained constant in two consecutive years. The bottom panel presents the same estimates broken down by party, showing relatively similar patterns to the ones observed in the Nationscape.[12]

First-Difference Analyses in the Panel Context

Analyses that use repeated cross-section surveys help us understand trajectories across groups, but they cannot evaluate changes at the individual level. To address this problem, I used data from the panel component of the 2016 and 2020 ANES surveys. The police officer thermometer question in 2020 was asked in the survey's post-election field, which was conducted between November 8, 2020 and January 4, 2021—roughly five and a half months after the killing of George Floyd. Therefore, it stands as a reasonable test of whether the effects observed in the Nationscape and the CCES may be observed when tracking the same set of individuals across time. Due to ANES's short window (only two waves) and sample size ($N = 1,912$), these analyses can only evaluate Expectation 1.

Expectation 1. Figure 6 presents the central findings, revealing a clear pattern: individuals between the ages of 18 and 29[13] on 2020 changed much more strongly than individuals aged 30 and older. To put the results in comparative context, the "treatment" group changed by 16.9 in a 0–100 scale, compared to 3.7 (aged 30-39), 2.2 (aged 40-64), and 3.2 (aged 65+). The polarization between the younger and older individuals—specified in a 2 x 2 difference-in-differences context—corresponds to an effect size of $d = 0.66$, suggesting powerful effects among younger individuals.[14] Considering

[12] The fact that we observe changes among Republicans in the Nationscape but not in CCES may result from differences in the main outcome of interest: while the former measures participants' favorability toward the police, the latter is about whether they feel unsafe around the U.S. police. Although interpretations of question wording effects remain speculative, the analyses nevertheless indicate a small but persistent effect size among Republicans in Nationscape.

[13] Since the original 2016 panel sample was necessarily restrictive in its age range—only 53 respondents were aged 18–24 in 2020 due to being sampled in 2016—I expanded the age range from 18–24 to 18–29 for these analyses.

[14] In supplemental analyses, I show that (a) the use of age splines rather than age categories replicates the same finding (Figure S11), (b) the patterns are robust to the removal of "outliers"—defined as change scores outside the 5% and 95% quantiles—or robust regression (Figure S12), and (c) the mode effects are inconsequential (Figure S13). Placebo analyses that look at ANES 2020-2022 study show that change between 2020 and 2022—each fielded after the event—shows no differences across age (Figure S15). Since the "treatment" group contains 155 respondents, I do not have sufficient power to explore within-party trajectories, though Figure S14 provides a compelling evidence that partisan differences—even with small samples—point toward the same predictions as we saw in Nationscape and CCES. One exception to this pattern is *negative* effects found for Republicans, compared to null or positive expectations. I discuss this in more detail in the Supplemental Materials, presenting several directional analyses.

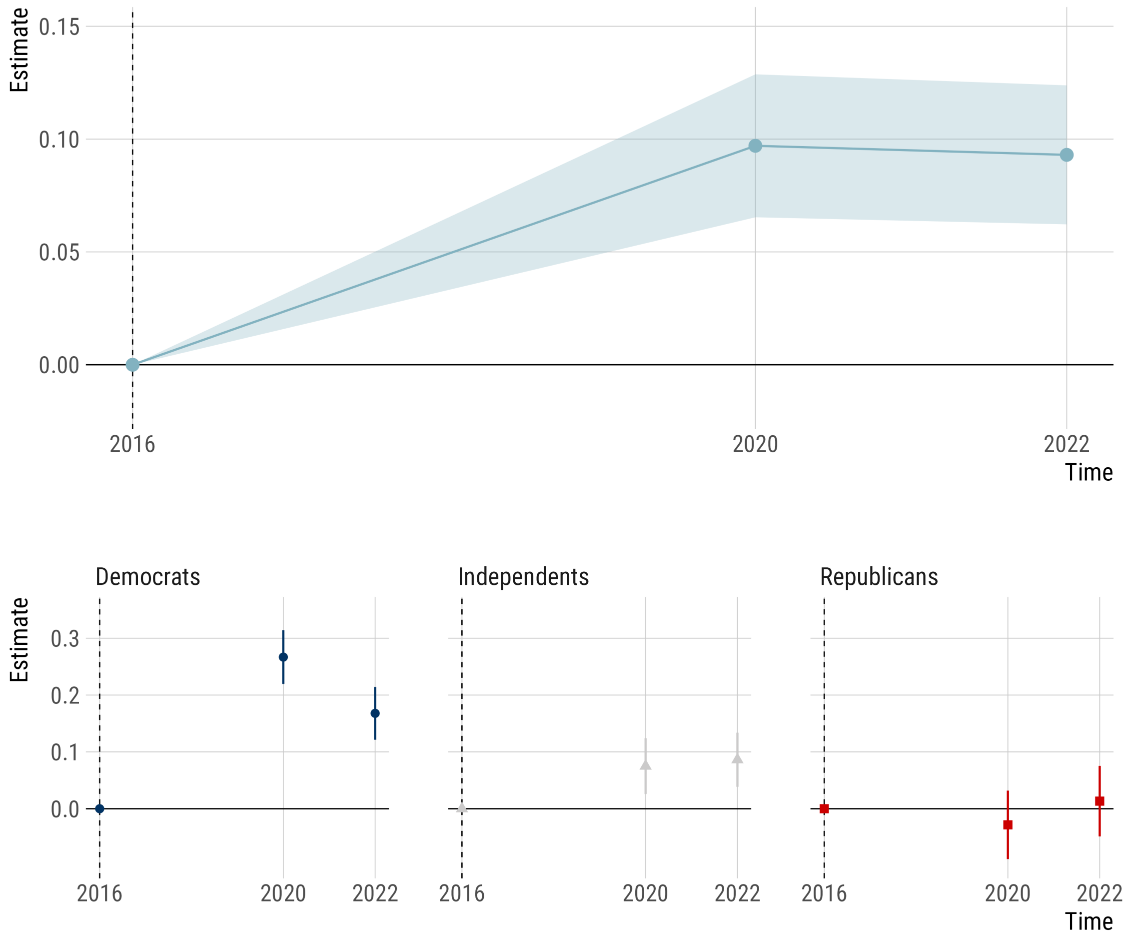

Figure 5: Dynamic TWFE on "Feeling Unsafe" with the Police

Notes: The figure presents dynamic TWFE estimates that measure the diverging trajectories of the treated (18-24 year-old respondents) and control (age 25+) groups in the Cooperative Congressional Election Study's 2016-2022 data file. The model in the top panel adjusts for gender, party identification, education, and county. The models in the bottom panel perform the same estimations, each subsetting one party identifier in the U.S. as the main treated group.

that the panel structure resolves potential compositional changes that would violate stable composition assumption, this finding provides a strong confirmation of the main expectation.

The Moderating Role of Political Exposure

What mechanisms explain these two findings? In Expectations 3A and 3B, I proposed that exposure to political messaging, combined with young people's greater propensity to update, might explain differential age effects. I now examine two such pathways: the first involves the intensity of protests in one's local environment and the second involves individual differences in political attentiveness.

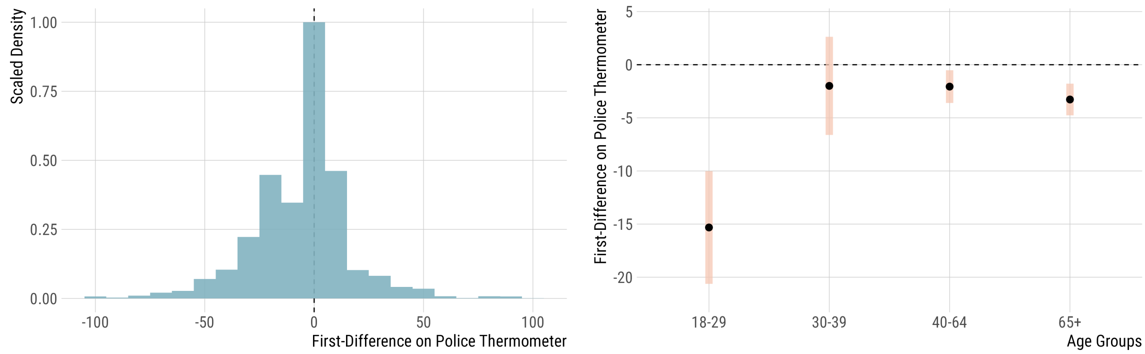

Figure 6: Panel Estimates of Change in Police Thermometer
Notes: The left panel presents the distribution of change scores between 2016 and 2020 among 1,912 ANES respondents on police thermometer, which ranges from 0 (cold) to 100 (warm). The right panel shows estimates across age groups. The model adjusts for survey mode and party identification, with robust standard errors.

In each case, greater exposure to political messaging should lead to stronger attitude changes, that is in turn moderated by one's political affiliation at the partisan level. In each analysis, I fit a TWFE model where the treatment status is interacted with party and the variable of interest, allowing me to inspect multiple "treatment effects." Figure 7 presents the findings in two panels.

Expectation 3A. One's proximity to high protest areas do not lead to consistent effects on differential updating. Moving from the lowest protest intensity districts to the highest protest intensity ones changes the TWFE estimates by 0.01 ($p > 0.05$) for Democrats, 0.02 ($p > 0.05$) for Independents, and -0.02 ($p > 0.05$) for Republicans, suggesting no effects. These findings strongly reject Expectation 3A: an increased opportunity to be exposed to political events via local influences does not seem to be affecting differential updating in a consistent direction. Across all cases, the differential updating is mainly driven by partisan identity and not by proximity to political protests.[15]

Expectation 3B. The analyses suggest that inter-individual differences in political attention is a strong moderator of differential updating. The change in political attention leads to monotonic increases: moving from the lowest attention score to the highest attention score leads to an increase of 0.17 ($p < 0.05$) for Democrats and an increase of 0.20 ($p < 0.05$) for Independents, with no change among Republicans (-0.03, $p > 0.05$).[16] The polarization between Democrats and Republicans is at 0.15 if political attention is at the lowest level and at 0.35 if attention is at the highest level.

In sum, the analyses suggest that political events may serve as catalysts for political differentiation when (1) individuals are more susceptible to political updating—i.e., when they live through their formative windows—and (2) this propensity to change is reinforced by strong political messaging. That said, this moderating effect works not via local interactions, but general political attention.

[15]Figures S7 and S8 show alternative specifications for the same pattern, the former using logged protests and the latter using 10 categories rather than 4. Each provides nearly identical findings.
[16]Figure S9 shows that political attention does not differentially change in response to the event.

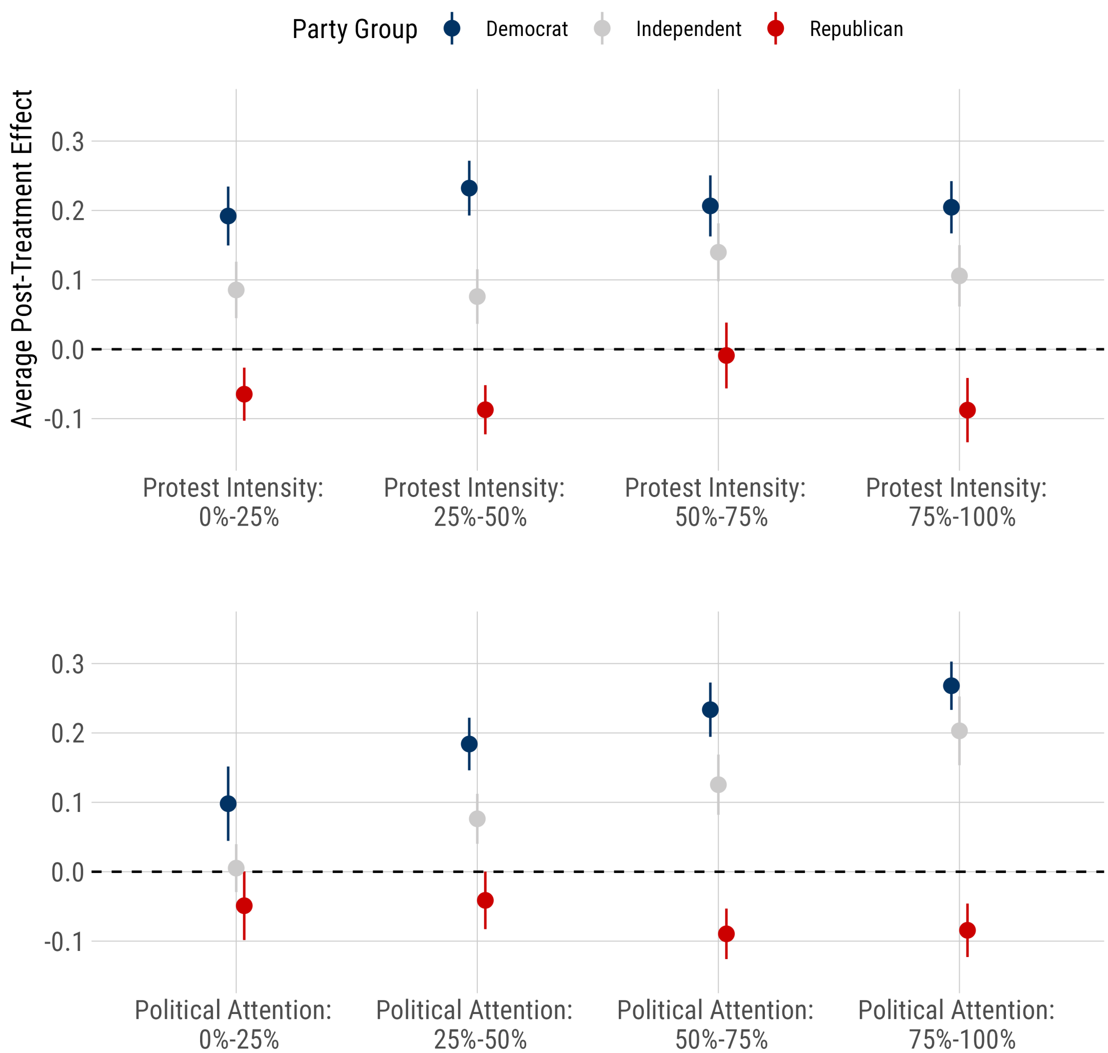

Figure 7: The Differential Post-Treatment Effects by Party and Political Exposure

Notes: The figure presents the average post-treatment effects—averaged across all post-treatment periods—from a model with treatment status, partisanship, and exposure, with unit and time fixed effects. The models adjust for gender, education, and their interactions (as well as congressional fixed effects for the second panel). Robust standard errors. Each category in the x-axes are constructed from quartiles in their respective distributions.

Discussion and Conclusions

The findings presented above highlight three central conclusions. First, the killing of George Floyd and the subsequent BLM mobilization caused a substantial increase in non-Hispanic White Americans' negative evaluations of U.S. law enforcement, driven by changes among the youngest cohorts, while older cohorts showed far smaller and far more transient attitude changes. These trajectories, however, were strongly conditioned by partisanship: while the youth identifying as Democrat and Independent moved against the police, the Republican youth exhibited no change at first, and later, a slight shift in the opposite direction, generating an increase in cohort-level polarization. Finally, individual-level exposure to elite political discourse—measured as political attention—amplified these cohort and partisan trajectories whereas spatial proximity to protests did not.

Taken together, the evidence presented in this article supports a model of generational imprinting in which cohort differentiation and political sorting serve as complementary engines of aggregate political change. Put differently, exogenous shocks to the political information environment change political orientations unequally, depending on people's temporal and political locations. The article thus offers a generalized framework for understanding differential political change.

Theoretical and Empirical Limitations

There are, however, several limitations of this article that should temper the broader implications. First, while the article attempts evidentiary triangulation and exploits granular time windows, the fact that the empirical analyses do not rely on a "clean" design raises inferential uncertainties. This has several consequences. On the one hand, the use of age groups as "pseudo-treatments" is natural given the main expectations. On the other hand, there are no shared standards for modeling age, addressing the notorious age-period-cohort problem, or determining an appropriate age window. Consequently, the treatment assignment (respondent age) is not precisely specified.

Second, the difference-in-differences strategy used in the article relies on untestable assumptions of parallel trends and differential compositional change. While the pre-treatment period shows stable and flat trajectories, bolstering confidence in post-treatment parallel trends, there are unmeasured shocks after the event—including, for instance, the pandemic and the 2020 elections—which may have differentially affected the cohort groups. Similarly, differential survey participation may have biased the unobserved characteristics of age and partisan groups. In particular, changes in partisan cohorts might have been consequential in introducing certain compositional biases.

There are also several measurement problems, particularly with the measures of political attention and local protest exposure. The former is based on political interest and a narrow set of knowledge questions, which together offer an imperfect measure of media exposure, political engagement, and factual knowledge. The latter approximates protest volume, but the strategy for calculating spatial proximity is relatively coarse. These measurement constraints, while not daunting, highlight that the moderating effects may understate the full impact of political exposure.

The killing of George Floyd and the ensuing BLM mobilization constitute an unusually visible and racially charged episode of political mobilization in U.S. history. Whether smaller-scale shocks and non-racialized issues would produce comparable findings remains to be established. Accordingly, the findings apply to a relatively narrow class of political stimuli. While the strongly salient nature of the event provides a useful boundary condition, at the level of individual variation, there might be a variety of unmeasured interactions that shape the observed outcomes.

Implications for Theory and Research

In the domain of cultural sociology, these findings support the view that shared experiences do not necessarily lead to convergent political tastes, since these experiences are mediated by the political field. Instead, cohorts are differentiated into cohort-units—clusters within cohorts oriented toward the same political object through a variety of interpretive standpoints. I proposed that parties serve as interpretive scaffolding for these units, given the cultural logics of the political field. The sorting process thus channels shared experiences and leads to cohort differentiation.

While this framework provides directional hypotheses for cultural sociology, it also highlights that public opinion dynamics cannot be reduced to elite messaging alone (Zaller 1992), considering the importance of shared experiences and life-course timing for change and stability (Kiley and Vaisey 2020; Vaisey and Lizardo 2016). In this sense, the findings encourage a synthesis of developmental and structural models to locate the processes of political socialization and political learning within the prevailing institutional diversity of contemporary political circumstances.

In the domain of political sociology, the findings indicate a potential microfoundation for affective polarization (Rawlings 2022). Variation in response trajectories suggests that polarization may not be exclusively rooted in elite messaging or social mobilization. Instead, the polarization dynamics may be seeded at different points in the life-course. The null effect of local protest density, coupled with the strong moderating role of political attention, highlights not only the fact that the principal channel of political influence seems increasingly "national" rather than "local," but also the notion that environmental proposals have differential imprinting across cohorts.

As a consequence, this article proposes a general template for analyzing aggregate political change by articulating (1) a framework for cohort-based social learning and (2) an account that integrates developmental perspectives and political sorting. Analyzing cohort turnover and its differentiation requires an understanding of intersecting developmental and institutional forces: the former helps us understand the agents of change, while the latter informs our predictions about its trajectory.

Data Note

Source files to reproduce all analyses are presented, temporarily, at GitHub. Note that this article is a work in progress. Accordingly, the repository will be moved to an OSF folder later.

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Supplemental Materials for

Generational Imprints: How Political Events Shape Cohorts

Turgut Keskintürk

April, 2025

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1 Supplemental Materials A: Data Sources

This section provides details about the data sources used in the analyses: *Democracy Fund + UCLA Nationscape*, *Cooperative Congressional Election Study*, and the *American National Election Study*.

1.1 The Nationscape

The Democracy Fund + UCLA Nationscape data (Tausanovitch and Vavreck 2021, henceforth, simply *Nationscape*) is a weekly cross-section survey, fielded between mid-2019 and the end of 2020 by the Democracy Fund Voter Study Group and UCLA, on a sample of American adults from Lucid, a market research platform that provides survey participants. Nationscape aimed to complete 6,250 survey interviews each week, of which roughly 67.4% identify as non-Hispanic White. To increase statistical power, I pooled these weekly installments into 4-week windows, centered on the window surrounding George Floyd's killing, with an average of 16,000 participants in every window. This allowed me to investigate 313,440 non-Hispanic Whites in the U.S. across an extended period.

Table S1: Descriptive Statistics for Nationscape

Characteristic	**N = 312954**
Age Groups	
18-24	9.0%
25-34	15.6%
35-49	23.4%
50-64	26.6%
65+	25.4%
Gender	
Female	49.5%
Male	50.5%
Educational Attainment	
High School or Less	28.1%
Some College	37.5%
College	21.4%
Post-Graduate Degree	13.1%
Party Identification	
Democrat	28.3%
Independent	34.9%
Republican	36.8%
Political Attention	0.60 (0.31)

Nationscape consists of a non-probability convenience sample recruited from Lucid. However, the research team (a) recruited the participants in each week with demographic quotas on age, gender, race, ethnicity, region, income, and education; (b) calculated post-stratification weights using the

American Community Survey's (ACS) 2017 adult population estimates, and (c) performed a series of benchmarking tests to evaluate the representativeness of the sample (Tausanovitch et al. 2021). I conducted all analyses with post-stratification weights. Table S1 provides a descriptive summary of pertinent respondent characteristics, while Table S2 breaks them down by the age groups.

Table S2: Descriptive Statistics by Age Group for Nationscape

Characteristic	**18-24**	**25-34**	**35-49**	**50-64**	**65+**
Gender					
Female	47.4%	46.4%	46.1%	55.7%	48.7%
Male	52.6%	53.6%	53.9%	44.3%	51.3%
Educational Attainment					
High School or Less	50.2%	32.4%	25.3%	26.0%	22.3%
Some College	38.2%	33.5%	34.4%	39.9%	40.0%
College	9.1%	23.4%	22.7%	22.6%	22.0%
Post-Graduate Degree	2.5%	10.8%	17.6%	11.6%	15.6%
Party Identification					
Democrat	31.7%	27.5%	27.2%	26.6%	30.6%
Independent	39.9%	41.9%	36.9%	33.0%	28.9%
Republican	28.4%	30.6%	35.9%	40.4%	40.5%
Political Attention	0.49 (0.29)	0.47 (0.29)	0.54 (0.30)	0.63 (0.30)	0.73 (0.27)

The original fielding window of the Nationscape was between July 18, 2019 and December 31, 2020. The researchers added four additions to these weekly installments: three concurrent waves fielded in April 2020, July 2020 and January 2021, and one additional wave conducted immediately after the U.S. Capitol attack. I excluded both this final wave and the last concurrent wave from analyses due to the Capitol event. The inclusion of these waves suggests that favorability toward the police may have slightly increased after the U.S. Capitol attack. However, with only two waves of data, this effect remains speculative and I do not consider it in the article. I also dropped the first concurrent wave, as it did not include the question that serves as the main dependent variable.[1]

To calculate protest intensity at the congressional district level, I used data from the Crowd Counting Consortium, a joint crowd-sourcing project of Harvard Kennedy School and the University of Connecticut, which collects publicly available protest data in the United States. To do so, I filtered for gatherings protesting topics related to "race" and "racism" within the first three months after the killing of George Floyd, yielding a total of 1,382 counties with such information (matching similar procedures in independent analyses, see Gethin and Pons 2024). I aggregated this data to the congressional district level, and matched the resulting protest vector with the Nationscape data.

Out of 437 congressional districts observed in the Nationscape, only 5 did not have BLM protests, but this number varies widely, ranging from 0 to 425, with an interquartile range of 20 to 61. As may

[1] In addition to these constraints, I dropped roughly 3,000 respondents due to missing data in covariates; however, this group represents only 1% of the observations within the survey window.

be expected, the exposure to BLM protests was slightly higher among participants with Democratic self-identification, given the geographical distribution of the protests and partisan lines.

I operationalize one's political attention using two constructs: *political interest* and *political knowledge*. The first construct relies on a survey question that asks participants how closely they follow "what's going on" in government, a measure ranging from 0 ("hardly at all") to 1 ("most of the time"). The second construct is measured with two knowledge questions: the first asks how many years are in a full U.S. Senate term, and the other asks for the name of the Chief Justice of the U.S. Supreme Court. This measure also ranges from 0 (no correct answers) to 1 (both answers correct). In the final step, I calculate the average of these two constructs to have an overall political attention measure.[2]

1.2 Cooperative Congressional Election Study

To supplement the Nationscape analyses and extend the time windows to 2022, I conducted analyses with the 2016, 2020, and 2022 waves of the Cooperative Congressional Election Study (CCES). These waves include a question that asks whether the police makes people feel safe, with response options ranging from "mostly safe" to "mostly unsafe." Table S3 provides a descriptive summary of pertinent respondent characteristics, while Table S4 breaks them down by the age groups.

Table S3: Descriptive Statistics for CCES

Characteristic	**2016**	**2020**	**2022**
Age Groups			
18-24	8.9%	8.7%	8.6%
25-34	17.5%	15.7%	15.2%
35-49	20.7%	20.6%	21.8%
50-64	31.8%	28.8%	28.4%
65+	21.1%	26.1%	25.9%
Gender			
Female	51.7%	51.1%	51.6%
Male	48.3%	48.9%	48.4%
Educational Attainment			
High School or Less	39.9%	34.6%	33.9%
Some College	32.0%	31.2%	28.2%
College	18.2%	21.5%	24.1%
Post-Graduate Degree	9.9%	12.7%	13.8%
Party Identification			
Democrat	28.3%	26.8%	27.1%
Independent	39.6%	37.2%	38.2%
Republican	32.1%	36.0%	34.6%

[2]This measurement of *political attention* is also intended to capture an overall *exposure propensity*. An alternative account can conceptualize it as "political awareness," à la Zaller (1992).

Table S4: Descriptive Statistics by Age Group for CCES

Characteristic	<24	25-34	35-49	50-64	65+
Gender					
Female	46.0%	52.2%	49.9%	51.0%	54.6%
Male	54.0%	47.8%	50.1%	49.0%	45.4%
Educational Attainment					
High School or Less	42.5%	28.9%	30.4%	35.7%	44.1%
Some College	42.0%	32.2%	30.9%	29.8%	26.7%
College	13.8%	28.4%	24.5%	21.2%	16.2%
Post-Graduate Degree	1.7%	10.5%	14.2%	13.3%	13.0%
Party Identification					
Democrat	27.5%	29.5%	28.5%	25.8%	27.2%
Independent	46.7%	43.4%	41.1%	37.0%	32.2%
Republican	25.8%	27.1%	30.4%	37.2%	40.6%

1.3 American National Election Study

Analyses that use repeated cross-section surveys help us understand trajectories across *groups*, but they do not provide evidence within *individuals*, which is the central claim of this article. To provide such evidence, I use survey data from the longitudinal panel component of the 2016 and 2020 waves of the American National Election Study. Between 2016 and 2020, ANES surveyed the same 1,912[3] non-Hispanic White individuals, allowing for an evaluation of whether change scores in the police thermometer measure from 2016 to 2020 differ between younger and older cohorts. Table S5 shows descriptive statistics for pertinent measures[4], and Table S6 breaks them down by survey mode.

Table S5: Descriptive Statistics for ANES

Characteristic	N = 1912
Age Groups	
18-29	12.8%
30-39	14.8%
40-64	45.6%
65+	26.9%
Party Identification	
Democrat	33.1%
Independent	5.4%
Republican	40.9%
Switcher	20.6%

[3]A 9.1% reduction from 2,105 individuals due to missing data in age, party or police thermometer.

[4]While constructing partisanship, the leaners are coded in their respective parties. For descriptive purposes, I present the distribution of party trajectories between 2016 and 2020, including *switchers*—those reporting different parties.

Table S6: Descriptive Statistics by Survey Mode for ANES

Characteristic	**FTF = 433**	**WEB = 1479**
Age Groups		
18-29	13.7%	12.5%
30-39	13.0%	15.4%
40-64	44.9%	45.8%
65+	28.4%	26.3%
Party Identification		
Democrat	34.6%	32.6%
Independent	4.5%	5.8%
Republican	39.8%	41.2%
Switcher	21.1%	20.4%

2 Supplemental Materials B: Alternative Specifications for Nationscape Trajectories

The main descriptive trends represent weighted average scores of unfavorable attitudes toward the police on a 0–1 scale. This item is constructed from four central response categories (very favorable = 0, somewhat favorable = 0.25, somewhat unfavorable = 0.75 and very unfavorable = 1), with "haven't heard enough" option being the midpoint (0.50). Here, I present three alternative specifications of this analysis. Figure S1 shows that the unweighted estimates have the same patterns as weighted estimates. Figure S2 shows that dropping "haven't heard enough" responses rather than coding them as the midpoint does not change the results. Finally, Figure S3 shows that binarizing this variable as 0 ("favorable") and 1 ("unfavorable") does not alter the substantive patterns.

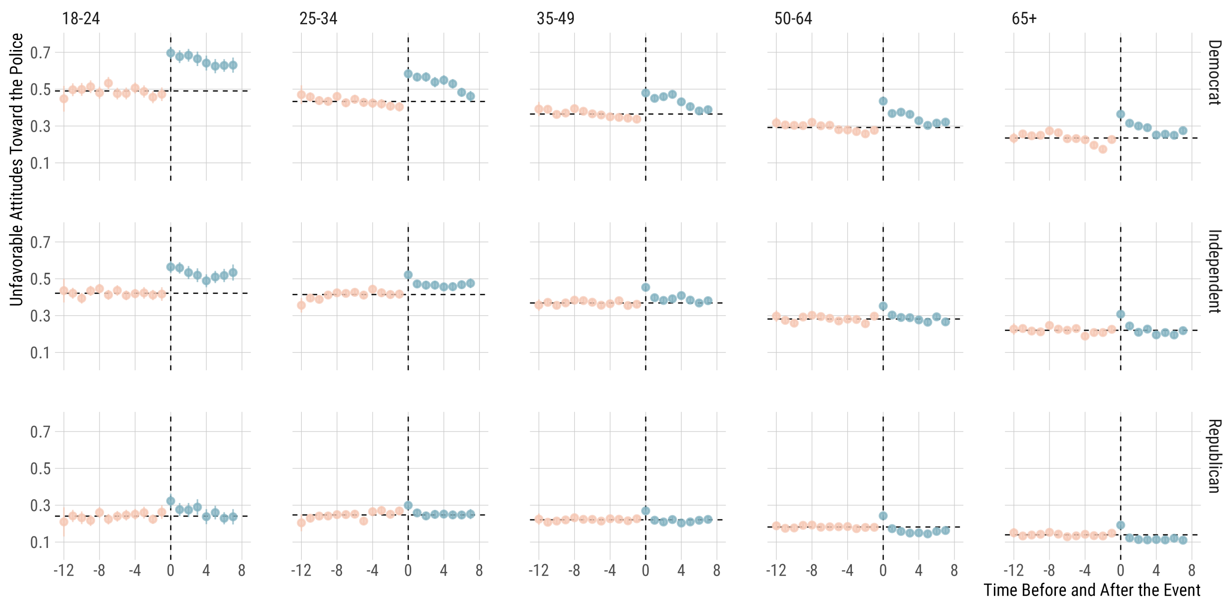

Figure S1: The Trajectory of Unfavorable Attitudes Toward the Police (Unweighted)

Notes: The x-axis shows the time window before and after the killing of George Floyd, centralized at $t = 0$, with each tick representing 4-week windows in Nationscape 2019-2020 data file. The y-axis shows the unfavorable attitudes toward the police, normalized between 0 and 1. The horizontal dashed lines represent pre-event averages.

Another researcher decision in the main article was to aggregate responses to 4-week installments. While each week is a stand-alone wave, I implemented this to increase statistical power and stability. In Figure S4, I present the same results using weekly waves: while the findings are more imprecise due to power issues, the general patterns still confirm the main expectations.

While the expectations involve political parties, it is plausible that people do not identify with their parties at the time of the survey because of the upcoming 2020 U.S. elections. In Figure S5, I present alternative analyses that instead use ideological identity. Similar results hold.

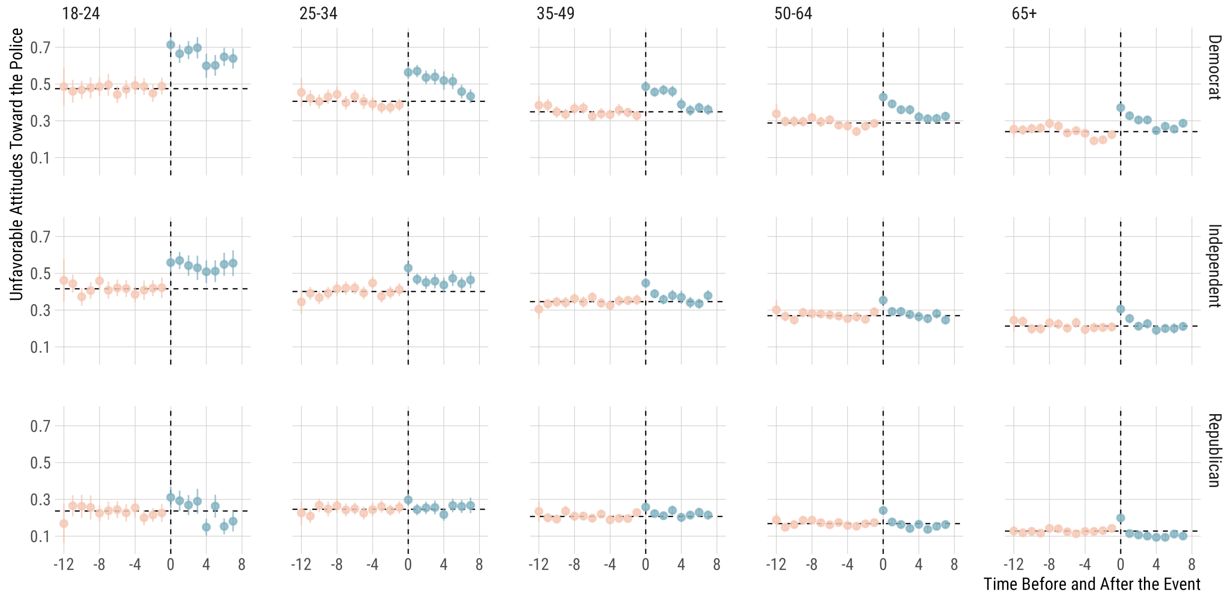

Figure S2: The Trajectory of Unfavorable Attitudes Toward the Police (Alternative Coding)
Notes: The x-axis shows the time window before and after the killing of George Floyd, centralized at $t = 0$, with each tick representing 4-week windows in Nationscape 2019-2020 data file. The y-axis shows the unfavorable attitudes toward the police, normalized between 0 and 1. The horizontal dashed lines represent pre-event averages.

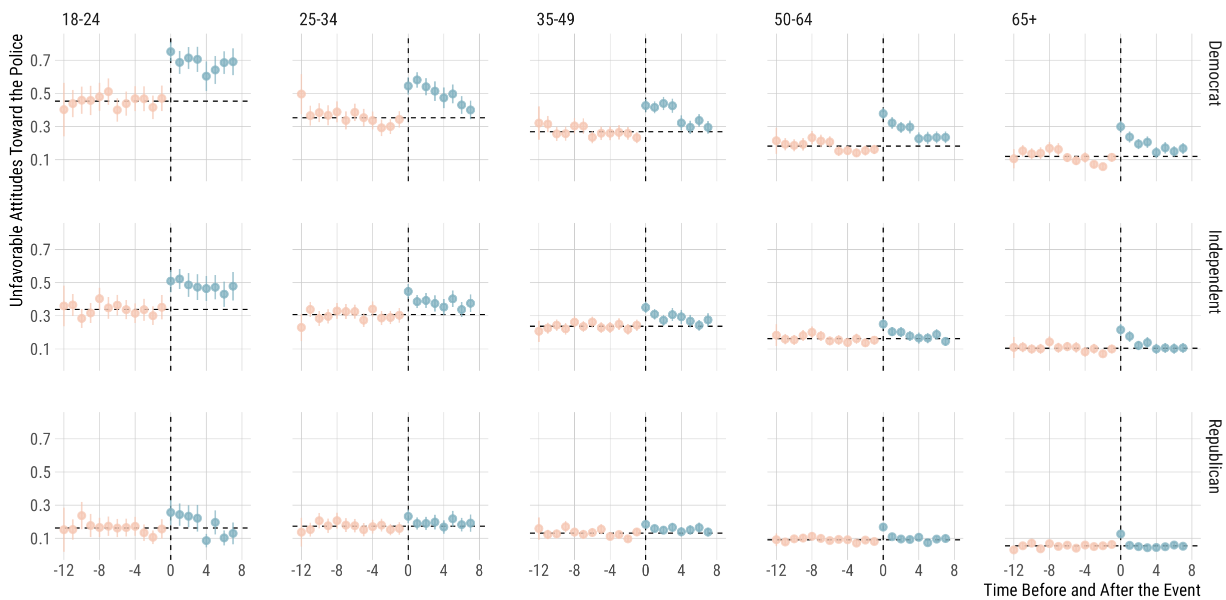

Figure S3: The Trajectory of Unfavorable Attitudes Toward the Police (Binary Outcome)
Notes: The x-axis shows the time window before and after the killing of George Floyd, centralized at $t = 0$, with each tick representing 4-week windows in Nationscape 2019-2020 data file. The y-axis shows the unfavorable attitudes toward the police, normalized between 0 and 1. The horizontal dashed lines represent pre-event averages.

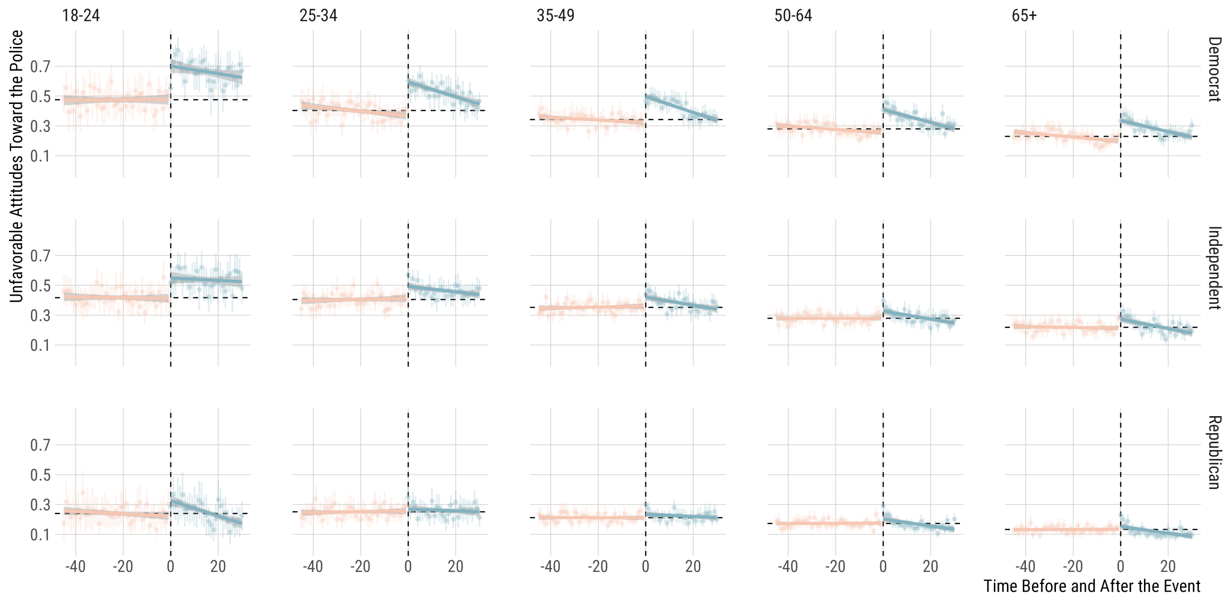

Figure S4: The Trajectory of Unfavorable Attitudes Toward the Police (Weekly Estimates)
Notes: The x-axis shows the time window before and after the killing of George Floyd, centralized at $t = 0$, with each tick representing 4-week windows in Nationscape 2019-2020 data file. The y-axis shows the unfavorable attitudes toward the police, normalized between 0 and 1. The horizontal dashed lines represent pre-event averages.

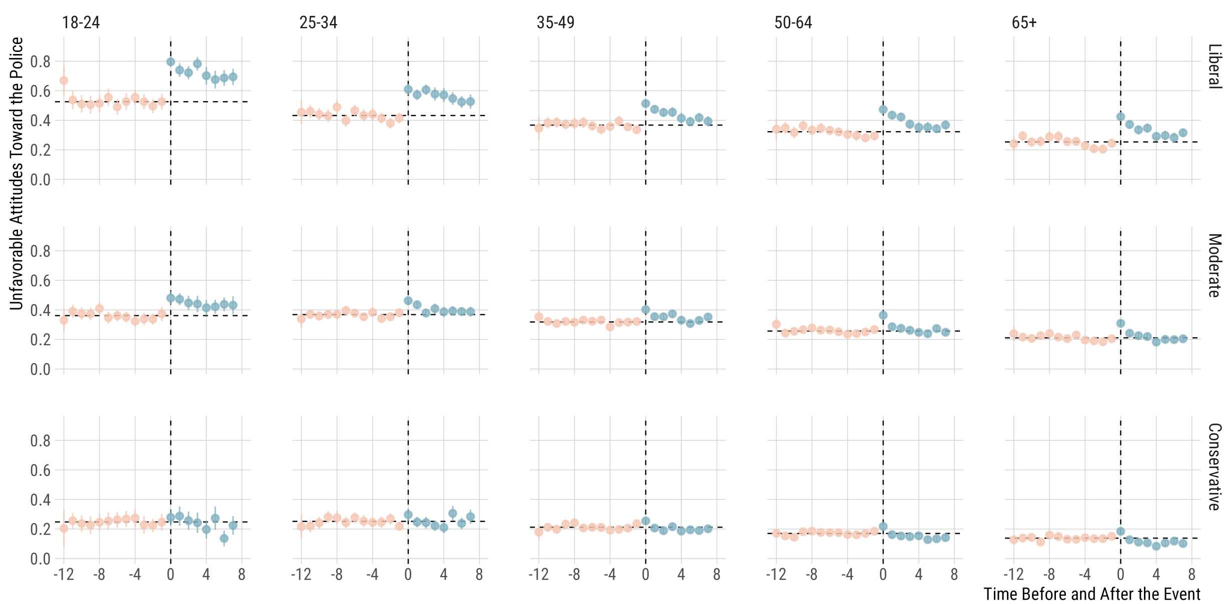

Figure S5: The Trajectory of Unfavorable Attitudes Toward the Police (Ideology)
Notes: The x-axis shows the time window before and after the killing of George Floyd, centralized at $t = 0$, with each tick representing 4-week windows in Nationscape 2019-2020 data file. The y-axis shows the unfavorable attitudes toward the police, normalized between 0 and 1. The horizontal dashed lines represent pre-event averages.

3 Supplemental Materials C: Pre-Trend Tests for Nationscape Analyses

Table S7 presents pre-trend tests for the difference-in-differences specifications in the Nationscape, showing that there are no differences in trends before the event.

Table S7: Pre-Trend Tests for the Difference-in-Differences Analyses

Model	Estimate	Error	p-value
Full Model	-0.012	0.017	0.478
Democrats	-0.012	0.028	0.659
Independents	-0.003	0.029	0.903
Republicans	0.028	0.022	0.192

4 Supplemental Materials D: Age Cut-Off in Difference-in-Differences

As noted in the main article, the cut-off I used—age 24 and younger—to classify people as "young" and "adult" may be problematic. In Figure S6, I show analyses where I systematically varied these windows with 5-year increments, starting with ages 18-23 up until ages 30-35. As can be seen, the estimates monotonically decrease when age increases, corroborating the main findings.

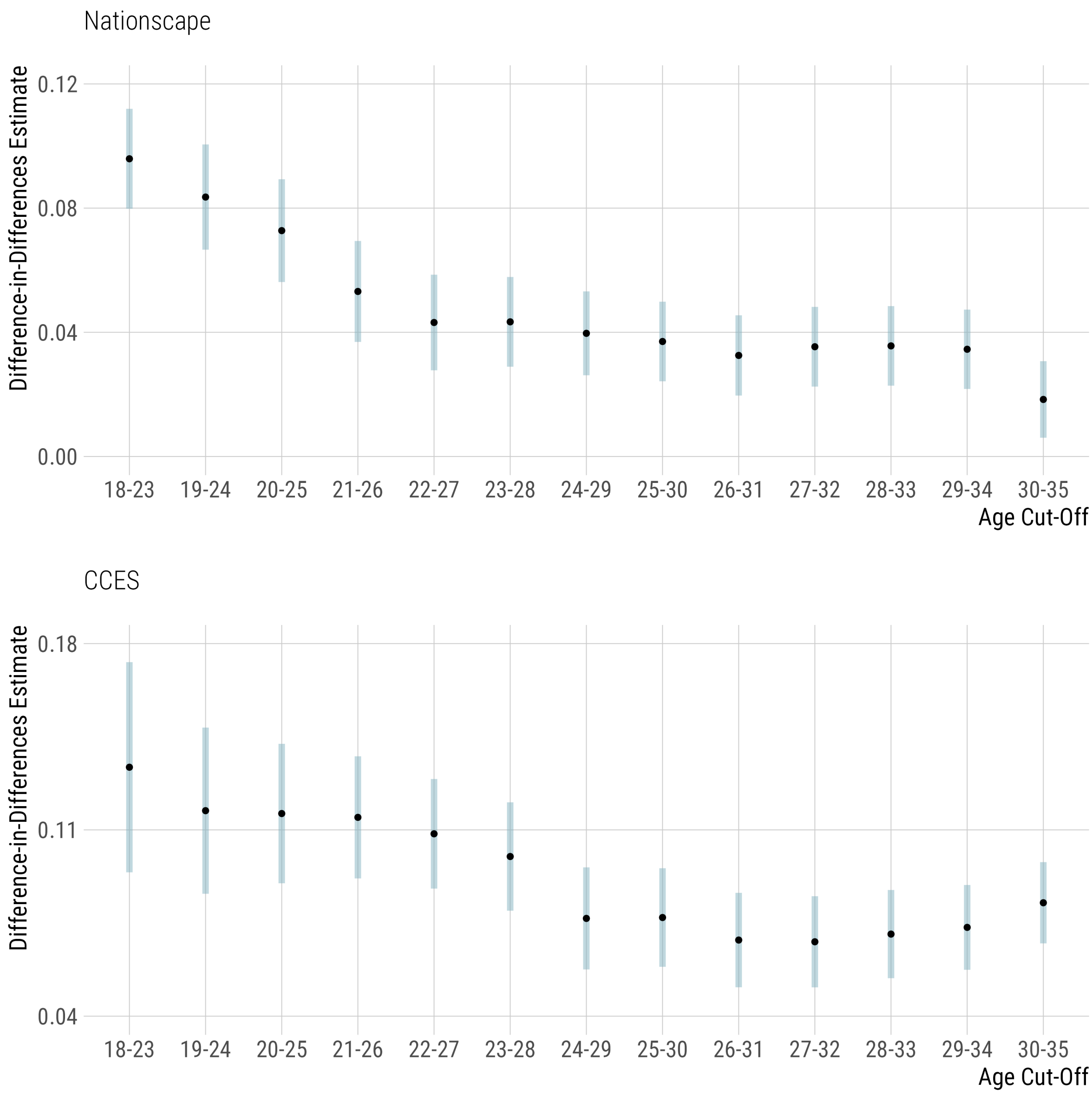

Figure S6: Varying Age Windows in Difference-in-Differences Analyses

Notes: The figure presents difference-in-differences estimates with varying "treatment" windows. The treatment group consists of individuals in a specific age band and the control group denotes people older than the specified window.

5 Supplemental Materials E: Alternative Specifications for Moderation Analyses on Political Exposure

In this section, I provide alternative analyses for political exposure mechanisms.

Expectation 3A. Figure S7 presents the same protest exposure effect, though it uses the logarithm of protest counts rather than raw counts while constructing the quartiles, showing identical estimates to the ones in the manuscript. Figure S8 breaks down the protest intensity into 10 equal categories, and performs the same estimations. Once again, the main results are unchanged.

Alternative specifications using different binning strategies (fixing width or fixing mass) produce similar findings, though their reliability depends on sample size considerations.

Expectation 3B. One potential concern with the political attention measure is that it may have moved in response to the event, polluting the estimates for the moderation analyses. Figure S9 presents the results from an analysis where I estimated a dynamic difference-in-differences model with political attention as the outcome variable. As shown, at least in terms of compositional differences, there were no differential changes in political attention throughout the study window.

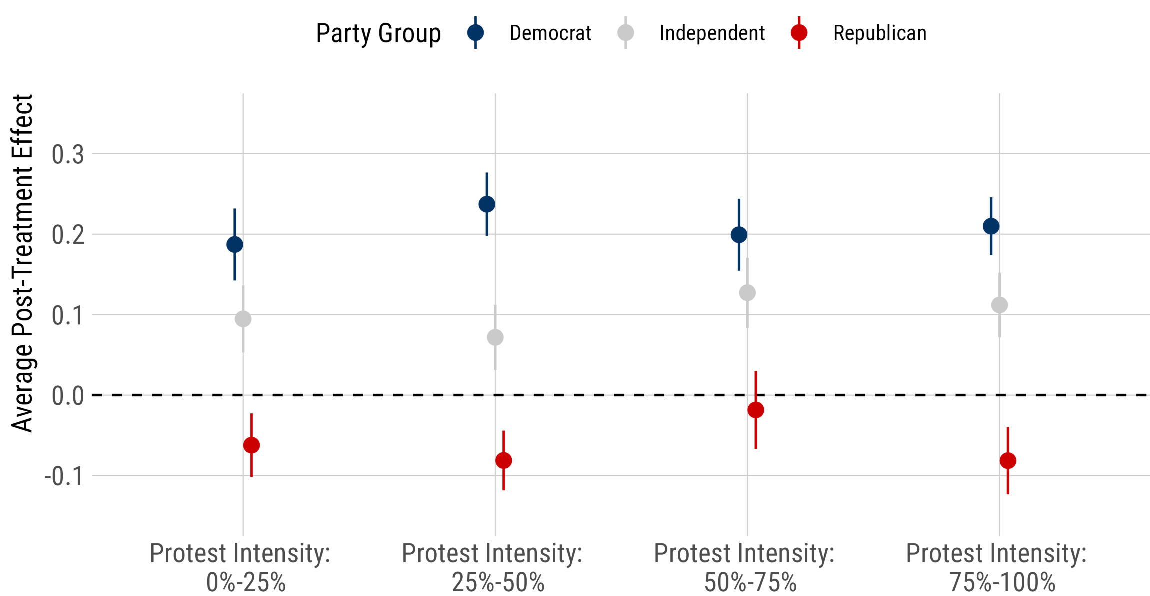

Figure S7: The Differential Post-Treatment Effects by Party and Protest Exposure
Notes: The figure presents the average post-treatment effects—averaged across all post-treatment periods—from a model with treatment status, partisanship, and exposure. The model adjust for gender, education, and their interaction. Robust standard errors. The x-axis represents quartiles in log distribution of protest counts.

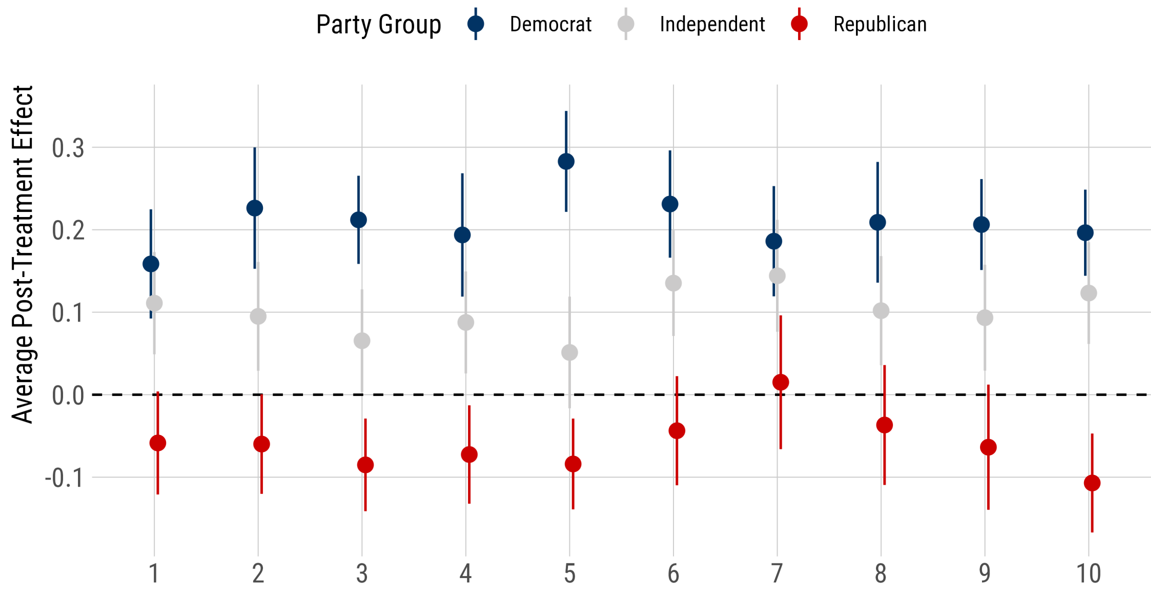

Figure S8: The Differential Post-Treatment Effects by Party and Protest Exposure
Notes: The figure presents the average post-treatment effects—averaged across all post-treatment periods—from a model with treatment status, partisanship, and exposure. The model adjust for gender, education, and their interaction. Robust standard errors. The x-axis represents 10 equal-sized bins of protest counts.

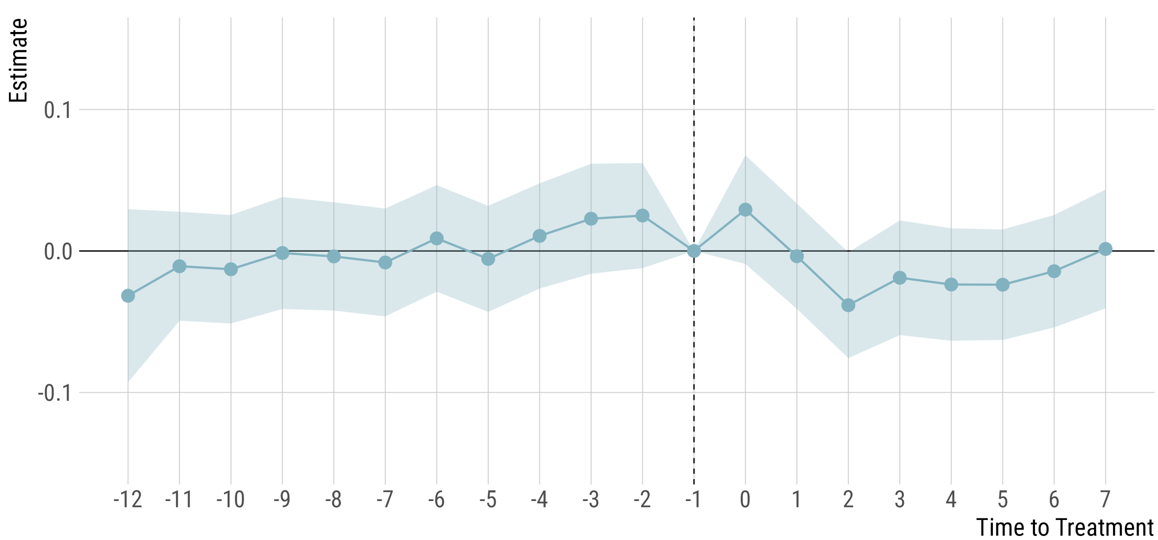

Figure S9: Change in Political Attention Across the Study Window
Notes: The figure presents the estimates from a dynamic difference-in-differences model quantifying the differences in political attention between the treatment and control groups. Robust standard errors.

6 Supplemental Materials F: "Feeling Unsafe" and Party Identification

Feelings of safety with the police taps into a personalized measurement. It is, however, plausible to ask whether there is any reason why the killing of George Floyd and the BLM mobilization would affect white people's feelings of safety, and their responses may not be politicized in the first place. Figure S10 shows the distribution of feelings of safety across party identity in 2016, before the event. It provides evidence that this question was *politicized* to begin with.

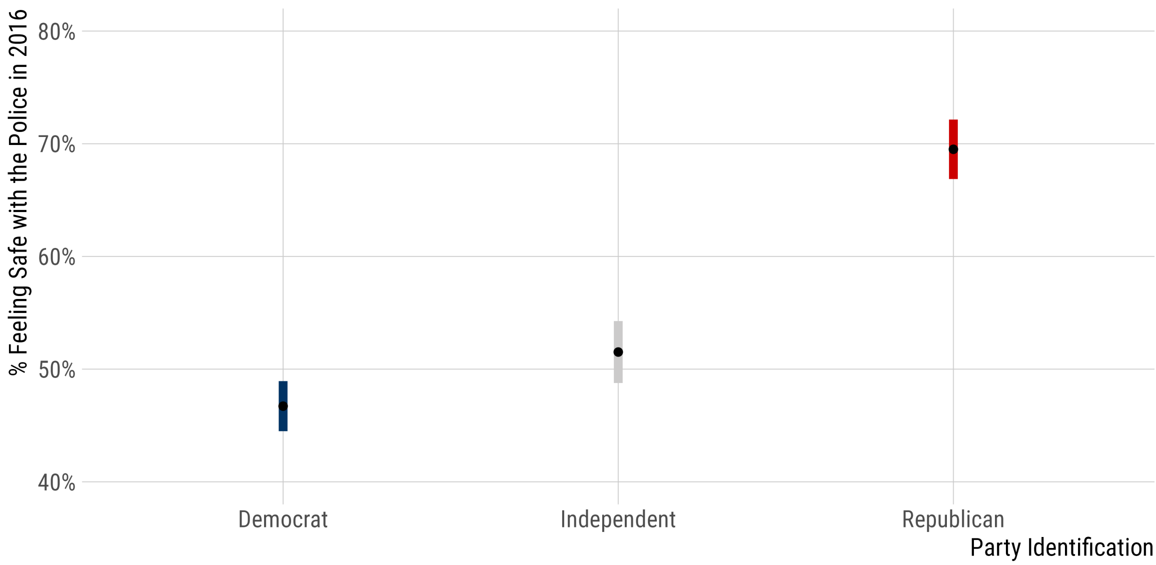

Figure S10: Percent Feeling Safe with the Police in 2016

Notes: The figure presents the estimates from a linear regression model estimating the binarized outcome of feeling safe in 2016 CCES. The model adjusts for age, gender, education, and county fixed effects, with robust standard errors.

7 Supplemental Materials G: Alternative Specifications for ANES Analyses

I implemented three alternative specifications:

- Figure S11 shows results from a model that uses an age spline with three degrees of freedom—determined via cross-validation—rather than a categorical age variable.[5]
- Figure S12 performs two analyses to assess outliers: the left panel shows differences in panel change when "outliers"—defined as 5% and 95% quantiles—are pruned while the right panel shows estimates from a robust regression model using iterated re-weighted least squares.
- Figure S13 replicates the analyses only among Web respondents, keeping all model specifications (except the adjustment for survey mode) exactly the same.

While the number of participants are very low across the parties (N for Democrats aged 18-29 is 77, N for Independents aged 18–29 is 22, and N for Republicans aged 18–29 is 59), I still conducted several heterogeneity analyses, the findings of which are presented in Figure S14. The estimates are rather imprecise. That said, the difference between Democrats and Republicans aged 18-29 is still statistically significant (difference = 13.6, with $p < 0.01$), confirming main predictions.

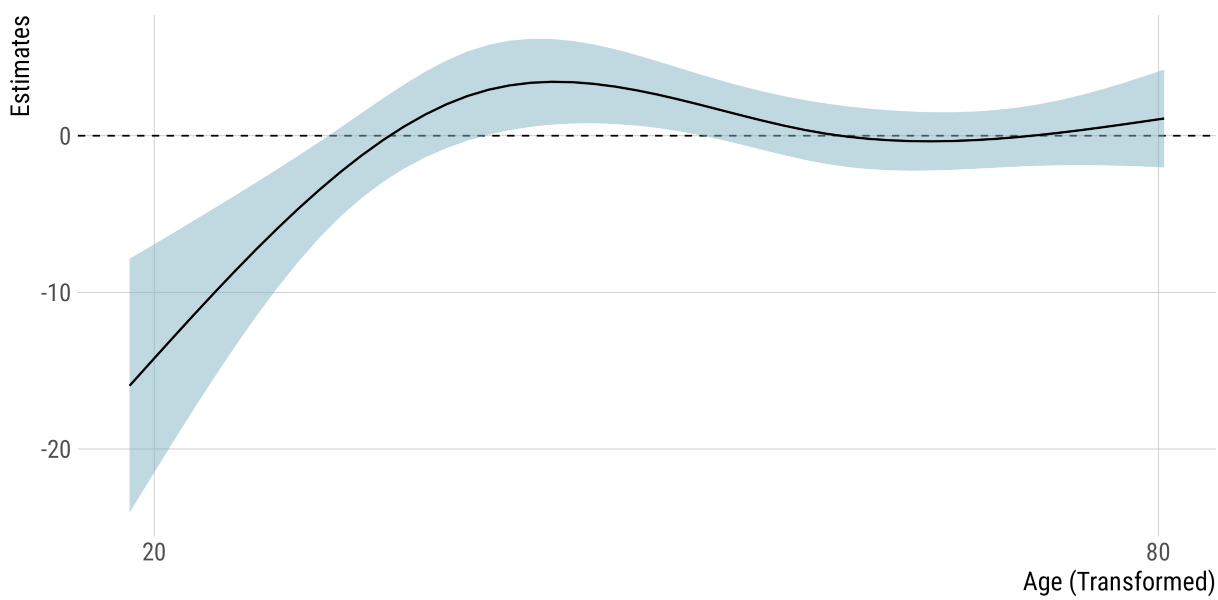

Figure S11: First-Differences in Police Thermometer, with Age Splines
Notes: The figure presents estimates of change in police thermometer in ANES 2016-2020 panel data, using an age-spline with 3 degrees of freedom, adjustment for survey mode, and party, with robust standard errors.

[5] I iteratively increased the degrees of freedom from $k = 1$ to $k = 10$ and selected the best-fitting model.

One curious pattern is the observation that, among people aged 18-29 who identify as Republican in both 2016 and 2020, we see a *decline* in police thermometer. While the finding is based on a few people, it suggests that there might be compositional effects in the repeated cross-section analyses. It is, however, an imprecise estimate, so I refrain from over-interpreting the result.

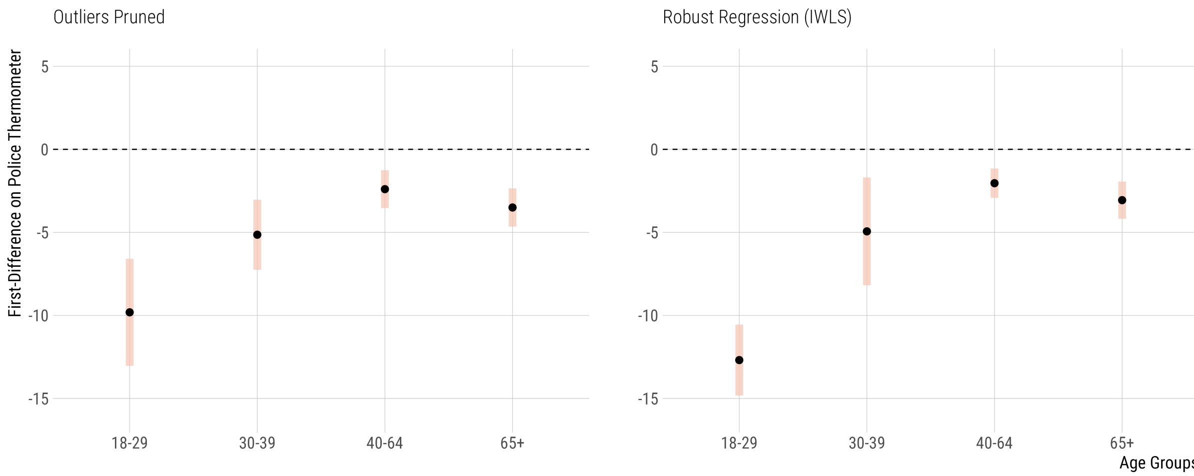

Figure S12: First-Differences in Police Thermometer, with Outlier Checks
Notes: The figure presents estimates of change in police thermometer in ANES 2016-2020 panel data, using two outlier checks. The left-panel presents results from an analysis where the outliers, defined as 5% and 95% quantiles, are pruned, while the right-panel presents results from a robust regression model with survey mode adjustment.

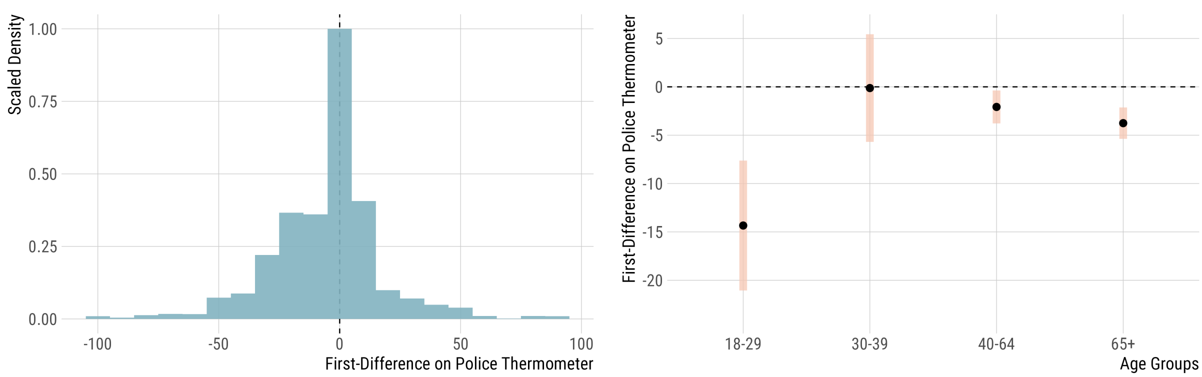

Figure S13: First-Differences in Police Thermometer Among Internet Respondents
Notes: The figure presents estimates of change in police thermometer in ANES 2016-2020 panel data among respondents who took both 2016 and 2020 surveys via online channels, rather than in-person, phone, or video channels.

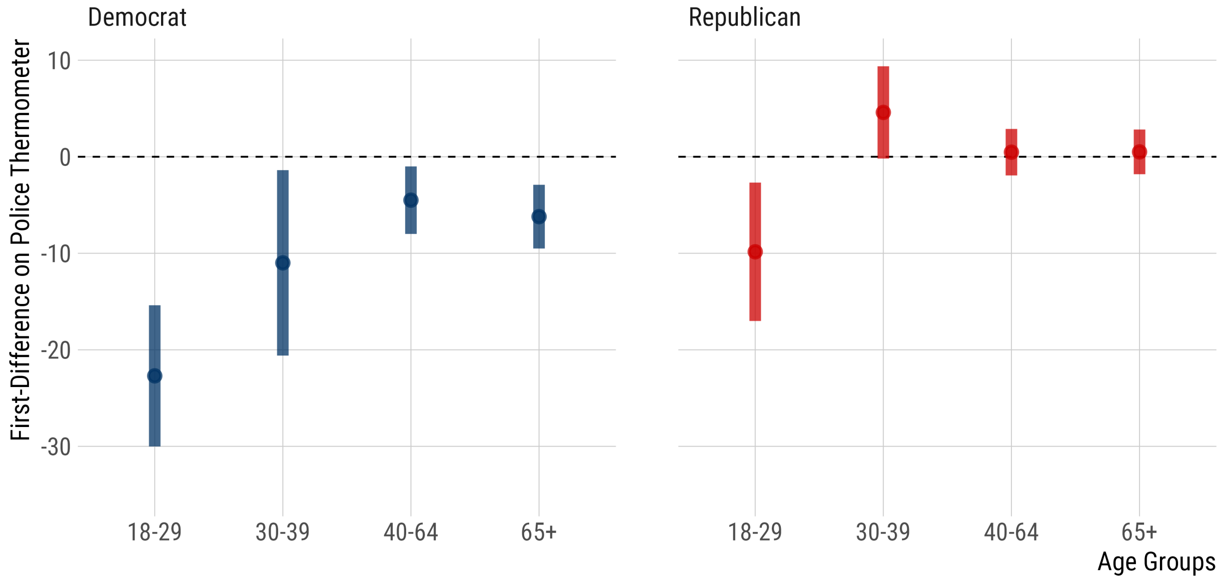

Figure S14: First-Differences in Police Thermometer by Party Identification

Notes: The figure presents estimates of change in police thermometer in ANES 2016-2020 panel data across 2020 party identification, while adjusting survey mode. The leaners are coded in their respective parties.

8 Supplemental Materials H: Placebo Analyses Using ANES 2020-2022

It is possible that the main findings stem less from the killing of George Floyd, and more from the observation that young individuals change more often than older individuals. If this is indeed the case, how can we distinguish the proposed causal process from a general "propensity to change?" I performed a placebo analysis using the 2020-2022 "Social Media Study" of the ANES, where both waves—2020 and 2022—were fielded after the killing of George Floyd. If the event was really consequential, we should expect the *level* of change to remain higher among young individuals, while the *direction* of change should show no differences, as there is no obvious reason why the (already strong) views of the police would directionally change. Figure S15 confirms this prediction.

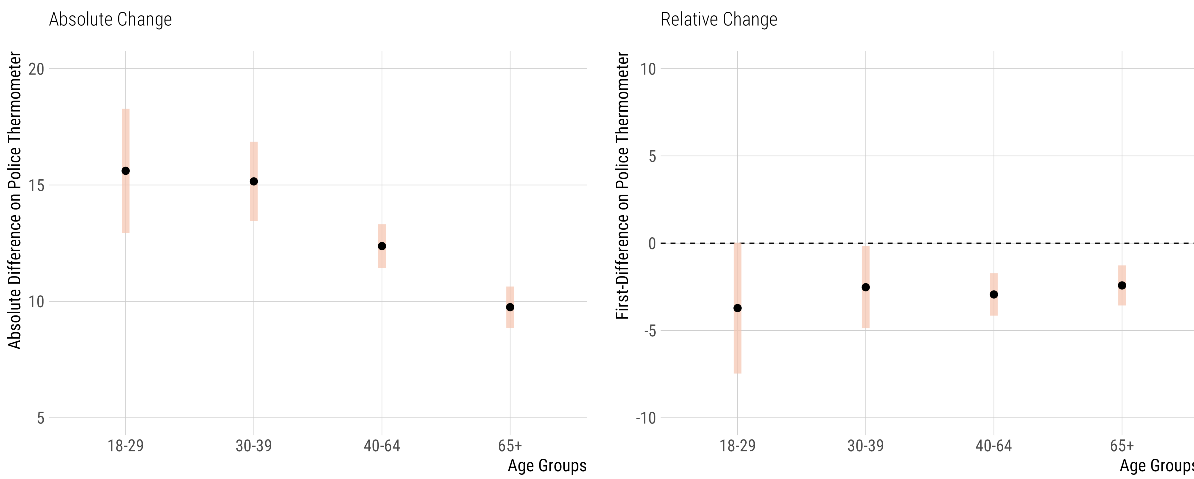

Figure S15: First-Differences in Police Thermometer in 2020-2022 Social Media Study

Notes: The figure presents estimates of change in police thermometer in ANES 2020-2022 panel data, where the left panel shows differences across absolute change while the right panel shows differences across directional change.

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