Generational Imprinting: How Political Events Shape Cohorts*

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May, 2026

Contemporary narratives of generational politics emphasize the idea that shared formative experiences produce shared political orientations across an age cohort. In this article, I advance a theory of cohortization to challenge that view, arguing that responses to large-scale political events produce polarization within cohorts as much as differentiation between them. I study the implications of this framework through the killing of George Floyd in 2020 and the ensuing Black Lives Matter protests, analyzing attitudes toward U.S. law enforcement among non-Hispanic White Americans across multiple national surveys spanning 2016 to 2024. I find that younger individuals became strongly unfavorable toward law enforcement in response to these events, while changes among older individuals were less substantial and more transitory. Crucially, this pattern was driven almost exclusively by young Democrats and Independents. The end result is not a coherent political generation but a generation internally divided along partisan lines.

This is my generation
This is my generation, baby
My, my, my, my generation

— The Who, "My Generation" (1965)

Generations figure prominently in contemporary politics. Often invoked to describe birth cohorts whose cultural orientations are said to bear the imprint of formative experiences (cf. Kertzer 1983), generation is presumed to define a coherent cultural collective, spanning from the one-dimensional men of the postwar era to the Woodstock youth and the yuppies of the eighties. This cultural cohesion ("coming of age" through shared experiences) led scholars to postulate historical generations (e.g., Elder 1974; Jennings and Niemi 2014), many of which were defined with reference to a major

*I thank Allison P. Anoll, Andrés Castro Araújo, Peter Bearman, Pablo Bello, Tyson H. Brown, Jon Green, Kieran Healy, Sunshine Hillygus, Christopher Johnston, Kevin Kiley, Omar Lizardo, Rachel Myrick, Craig Rawlings, Shikhar Singh, Daniel Stegmueller, Jessi Streib, and Stephen Vaisey for their helpful and frequently incisive comments on this project. I also thank conference and seminar participants at Columbia, Cornell, Duke, and Stanford for their insightful feedback on the many versions of this paper that, in retrospect, had to exist.
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political cleavage: the Vietnam Generation formed in opposition to the draft, and those politicized after 9/11 in the shadow of mass surveillance. What, if anything, unites these generations as political collectives, however, is a question that has rarely received clear theoretical specification.

This question has important consequences for our understanding of political culture. Generational narratives tend to emphasize cohesion within a cohort, typically focusing on the differences *between* birth cohorts. If it is exposure to a set of formative events that sets one cohort apart from another, the argument goes, then those formative events will translate into shared political orientations. Yet, the formation of a generation is a political process as much as a temporal one: political competition determines which experiences come to count as "formative" for a cohort, and within those cohorts, such experiences do not simply produce shared meaning, but *competing interpretations of those very events*, shaped by prior political dispositions. Generational formation, so understood, is a process of political differentiation *within* cohorts as much as a political differentiation *between* them.[1]

To explain this process of differentiation, I develop a micro-level model of generational imprinting. This model posits that when a salient political shock introduces a new problem into the information environment, political elites provide competing considerations to frame this problem. In response, people—seeking identity-congruence—accept considerations that align with their prior identities (Zaller 1992; Zaller and Feldman 1992). The durability of these considerations, however, depends on the composition of people's existing consideration sets: for individuals with few considerations (the young), these newly accepted considerations will result in persistent attitude changes, while the trajectories of individuals with many considerations (the old) will be less substantial and more transitory. In the end, these micro processes of personal attitude change produce, at the aggregate level, differences across age groups, leading to *generational differentiation* (Mannheim 1952).

A distinctive prediction of this model compared to impressionable years hypothesis (Krosnick and Alwin 1989; Sears and Funk 1999; Vaisey and Lizardo 2016) and previous accounts of generational learning (e.g., Bartels and Jackman 2014) is *within-cohort* differentiation as much as *between-cohort* differentiation. Since political learning is grounded in partisan-congenial acceptance (Zaller 1992) rather than pure age-based heterogeneity (Krosnick and Alwin 1989), I theorize that the imprinting process produces increased polarization within young birth cohorts rather than a coherent political collective. As such, the model allows us to look for young people who volunteer to fight in Vietnam, rather than purely focusing on the counter-mobilization against it as *the* generational response, or those young conservatives who supported the War on Terror, alongside their peers opposing it.

[1] As Mannheim (1952) highlights, a generation often hosts multiple "generation-units," and they constitute a generation at a higher level of abstraction "only in the sense of [units] fighting one another" (307).

To explore the predictions of this framework, I study the killing of George Floyd in 2020, as well as the ensuing Black Lives Matter (BLM) protests. Drawing on three national surveys spanning 2016 to 2024, I examine how White Americans changed their attitudes toward U.S. law enforcement, and show that younger individuals became strongly unfavorable toward the police in response to these events, compared to less substantial and more transitory changes among older individuals. Yet, I show that conclusions relying on such cohort differences provide an incomplete account: I find that age-based differentiation between younger and older Americans was almost exclusively driven by youth identifying as Democrats and Independents. This, I argue, led to within-cohort polarization, as much as a between-cohort differentiation. I show that the results do not stem from compositional changes in partisanship, differential shifts in media diet, or local trends, arguing for an account that emphasizes how differential political attention might have facilitated the imprinting process.

This article has several theoretical implications. First, I propose that *coming of age* in the same period does not necessarily lead to convergent political tastes. Instead, cohorts become relevant only when understood within the competitive struggle among parties and social movements to frame salient events. Thus, cohort effects are not—according to this account—self-sufficient drivers of aggregate political change. Second, I propose that the effect of elite messaging is not uniform across rank-and-file actors. Instead, they hinge on shared experiences, life-course timing, and sensitivity to change. As such, I reframe political socialization as a joint product of competition within politics and life-course sensitivity. In the end, this article provides a useful corrective to popular narratives about "generations" by theorizing that ideology structures how shared experiences are interpreted.

A Theory of Generational Imprinting

The problem of generations has long been a central issue in classical sociological theory (Mannheim 1952; Ryder 1965), with recent accounts in cultural sociology highlighting the role of cohortization as an engine of cultural change (Vaisey and Lizardo 2016). Scholars typically interpret generations as products of cohort effects, which emerge from the interaction of period effects (i.e., cultural and political processes shaping the range of experiences) and age effects (i.e., the differential responses to environmental stimuli across age groups). Hence, cohortization is often theorized as *period effects experienced at an early age*, that is, *differentiation in these early experiences* (Morgan and Lee 2024).[2]

[2]While some accounts in cultural sociology may be read as consistent with Ryder's age-by-period framework, which is the strategy I follow in this article, this attribution may not always be warranted. Accounts of cohortization in cultural sociology typically work with a somewhat schematic version of Ryder's framework, in which cohort differences are fixed early in life, rather than emerging dynamically at any point in the life course (Ryder 1965).

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 likely to change (Krosnick and Alwin 1989) and form identities (Jennings and Markus 1984), more willing to assign greater weight to new information (Achen 1992; Bartels and Jackman 2014), and more open to influence from socializing agents (Jennings and Niemi 1968) and political events (Sears and Valentino 1997). It also provides a mechanistic account of "fresh contacts" (Mannheim 1952), the classic idea that new cohorts carry "the impress of [these contacts] through life" (Ryder 1965:844)—a cultural development process commonly referred to as *generational imprinting*.

Yet while this imprinting process is often formulated through distinctive "historical markers" (Alwin, Cohen, and Newcomb 1991; Elder 1974; Mishler and Rose 2007), the theoretical mechanisms through which cohorts become politically differentiated are rather ambiguous.[3] The ambiguity lies primarily in treating formative events as sufficient indicators of generational imprinting, without explaining why a person changes in one direction rather than another. For instance, while the Great Depression (Elder 1974) and the Vietnam War (Jennings and Niemi 2014) are often treated as formative events for their respective cohorts, existing theories rarely clarify whether these events lead to more liberal or more conservative political orientations. In treating shared temporality as shared meaning-making, this framework fell short of explaining the direction of generational change—an issue that has frequently haunted the ambiguous use of the term *generations* (Kertzer 1983).[4]

The Dynamics of Generational Differentiation

The central reason why cohortization theories fail to predict the direction of opinion change is that these theories do not have a clear account of the information flow from political elites. This implies that while cohortization dynamics provide a *demand-side* account of who changes, they ultimately offer no *supply-side* mechanism of directional change. This results in an ambiguity in understanding why an actor forms evaluation A rather than evaluation B. It is thus imperative to proceed from the expectation that, when a new shock introduces a problem, the political environment will provide competing and most likely partisan considerations to frame it (Aldrich 1995; Zaller 1992).

[3] 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 the experiences of cohorts, which then produces cohort differentiation. Similarly, see Ghitza, Gelman and Auerbach (2023) for a comparable treatment. The problem is that this work quantifies cohort differences via backward tracing from aggregate data, without really unpacking the process of *cohortization* itself.

[4] 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).

According to this framework, the mechanism that generates directional opinionation is differential acceptance: when faced with competing considerations from political elites, people preferentially accept cues that align with their existing political dispositions. The partisan cue-taking studies provide ample evidence that evaluations of political objects, as different as policy preferences and personal values, are shaped by these elite information dynamics (Bisgaard and Slothuus 2018; Goren, Federico, and Kittilson 2009; Slothuus and Bisgaard 2020). Therefore, the process of partisan learning[5] is a powerful engine of differentiation, resolving the problem of directional change by positing that political identities predate the formation of specific evaluations across political issues:

> Premise 1: *When new political shocks arise, people's partisan dispositions determine which elite frames they internalize, producing systematic directional differentiation.*

What about the initial reception and durability of these new considerations? This is where theories on cohortization can extend Zaller (1992). Let me define the set of prior considerations a person has as their pre-existing "consideration set." One implication of the sensitive windows thesis is that we may understand different age groups as individuals whose pre-existing consideration sets vary in magnitude and complexity (Achen 1992; Bartels and Jackman 2014). According to this definition, younger individuals tend to have a smaller set, whereas older individuals tend to have a larger set of considerations. This indicates that, when a problem arises in the information environment, the influence a new consideration carries varies by age group (Anoll and Engelhardt 2023).

One theoretical implication of this framework is the distinction between *chronic* and *temporary* ideas. Among older individuals, the large consideration set implies that the new cue competes with many priors. This means that an older actor's initial reaction to an emerging issue consideration tends to be weaker. The heat of the event might generate a temporary update, though chronic ideas reassert once recency effects are washed away. Among younger individuals, these new considerations have almost no competition, however. New ideas may come to define how to think about that issue over time, changing temporary considerations to chronic baselines. This difference highlights two main trajectories of opinion behavior: differential response and differential retention. Hence:

> Premise 2: *The durability of political learning depends on the size of pre-existing consideration sets: younger people, having fewer priors, internalize new cues more durably, while older people, with stronger priors, experience weaker and more temporary change.*

These two premises help us understand individual mechanisms of attitude differentiation. I argue

[5]Since the theoretical framework posits congenial messages as the engine of directional opinionation, the *source* of these messages can be political parties, social or political movements, or a combination of both.

that *imprinting*, the extent to which a temporary reaction becomes a chronic baseline, requires that a new consideration be both identity-congruent (by Premise 1) and relatively powerful compared to existing priors (by Premise 2). When a new issue arises in the environment, individuals' evaluative updating is thus tied to the partisan supply of cues, as well as the prior stock of considerations. In this setting, identity-congruence provides the mechanism for directional changes, while differences in prior consideration sets supply the mechanism for cohort differentiation across age groups.

Taken together, this indicates that the processes of imprinting can fail for two reasons: *incongruence*, when new considerations conflict with pre-existing partisanship, and *saturation*, when congruent considerations enter into an already saturated field of consideration sets. Among older individuals, the failure thus reflects limited durability: even congruent new considerations fade quickly because their consideration set is already saturated. Among younger individuals, (1) congruent considerations face little resistance and might consolidate into chronic baselines given that younger individuals have sparse consideration sets, while (2) incongruent considerations initially register—due to, again, high receptivity—but decay over time as congruent signals become reinforced, which leads to a reversion to the partisan baseline among young actors with initially incongruent updates.

The Implications of Differentiation

Figure 1 provides a schematic representation of how four actors, defined via a 2×2 table of age and partisan groups, should change their political attitudes in response to an exogenous political event. Using these theoretical trajectories as our empirical benchmark, we can derive two basic hypotheses from this figure. Following the previous typology, the young refers to younger age groups and the old refers to older age groups.[6] The first hypothesis supplies the following prediction:

> Hypothesis 1. *The young change their attitudes more strongly in response to a political event compared to the old, with the magnitude varying by political identity.*

This hypothesis has a rather precise group-level quantity (Lundberg, Johnson, and Stewart 2021): $\mathbb{E}[Y_{post} - Y_{pre} \mid A, G]$, i.e., the difference in attitudes before and after a political event, stratified by age groups A (that is, young and old) and political groups G (that is, parties D and R).

The generalization of this expectation to multiple windows brings us to the question of trajectories, as shown in Figure 1, and the prediction of differential response across the young and the old:

[6] Throughout, "the young" refers to individuals in their impressionable years, typically operationalized as those aged younger than 25, while "the old" refers to older age groups outside this window.

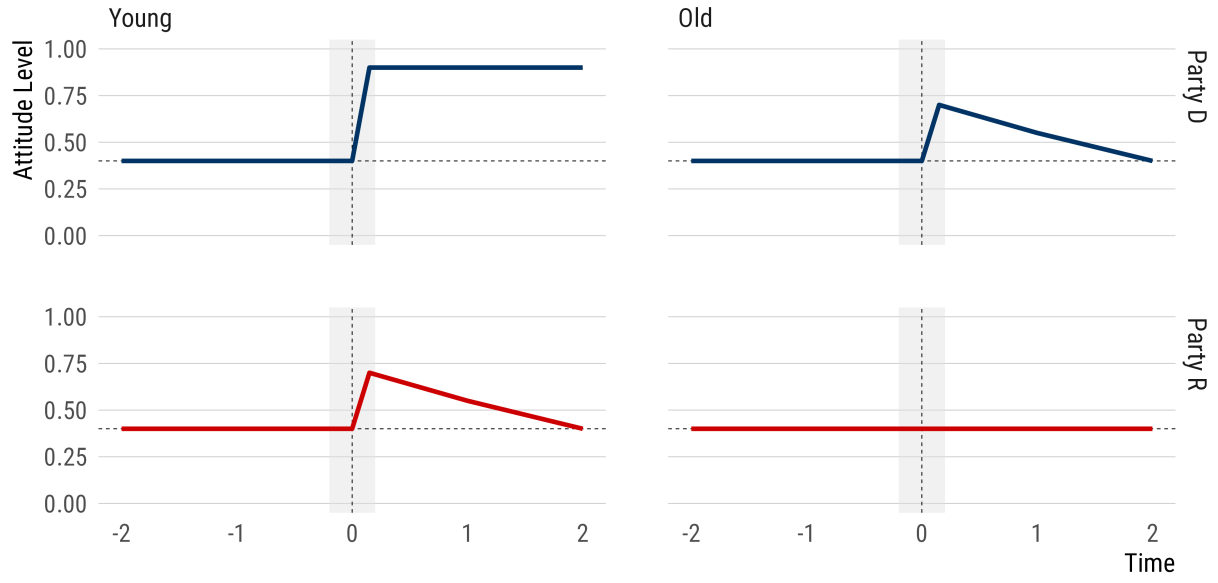

Figure 1: Theoretical Trajectories Across Age and Partisan Groups

Notes: The figure presents theoretical trajectories across four groups defined by age (young and old) and political identity (Party D and Party R), with the shaded area at time = 0 representing the event period.

> Hypothesis 2. *The durability of attitude changes will diverge across age and political identity. Among the old, the new considerations will lose accessibility, leading to a reversion to pre-event baselines. Among the young, congruent updates will endure, while incongruent considerations will lead to a reversion to the party line.*

The group-level quantity, this time, is $\mathbb{E}[Y_t - Y_{pre} \mid A, G]$, where the subscript t refers to each time window *after* the event, allowing us to trace the temporal trajectory of attitude updating.

Taken together, Hypotheses 1 and 2 have an important aggregate implication that has been largely overlooked in the generational politics scholarship. The standard expectation is that political events produce between-cohort differentiation, where younger and older cohorts diverge in their average attitudes, leading to the familiar argument of a political generation defined in opposition to older ones. That being said, the micro-dynamics outlined in this section suggest a more complex picture. Because updating is conditional on one's partisan identity (Premise 1), the young do not move as a bloc: congruent changers consolidate new considerations into chronic baselines, while incongruent changers revert to their prior baselines (Premise 2). The outcome of these opinionation dynamics is not a coherent political generation, but a cohort internally divided along prior ideological lines: put differently, *within*-cohort differentiation as much as a *between*-cohort differentiation.[7]

[7] In contrast to an approach that totalizes cohorts, this framework microfounds *cohort-units* (Mannheim 1952), clusters oriented toward the same (political) object, but from different, and increasingly antagonistic, perspectives.

The Empirical Setting

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 the Fall of 2016, long before the event, and runs through the Fall of 2024.

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). The scale of this mobilization positioned the George Floyd protests as a salient political event that swiftly re-centered public debate on police violence in the U.S., a chronic issue that had been ongoing for years (Edwards, Lee, and Esposito 2019).

The scale and salience of the George Floyd protests make this event an analytically powerful setting for investigating the opinion dynamics of cohortization. Since public attention is a scarce resource (Hilgartner and Bosk 1988) and political elites routinely reinforce identity-congenial signals (Zaller 1992), changes in the political information environment create opportunities for persistent change in attitudes, particularly when political mobilizations successfully shift the public agenda (Wasow 2020). The BLM mobilization in the summer of 2020 represents precisely such a moment.

Of course, the murder of George Floyd was not, in any sense, an isolated event. The years preceding 2020 presented a succession of nationally prominent police violence victims, including, among others, Trayvon Martin, Eric Garner, Tamir Rice, Freddie Gray, Philando Castile, and Breonna Taylor. Such instances already established competing partisan frameworks for interpreting police conduct among the American public. What distinguished the Floyd case, however, was the sheer scale and salience of the ensuing BLM movement, which unfolded precisely at the intersection of an election year, a pandemic that virtually facilitated political engagement, and agenda-seeding mobilization (Wasow 2020). The result was a moment of *concentrated salience*, one that compressed into months the kind of political opinionation dynamics that might otherwise unfold across decades.[8]

[8]There is now a sizable scholarly literature on the influence of the killing of George Floyd and the BLM. Existing studies have typically asked whether protest exposure and proximity produced attitude changes among the U.S. public (for prominent examples, see Gethin and Pons 2024; Reny and Newman 2021). This research often reported that these

In addition to its analytic importance, the Floyd case arguably presents the first high-salience mobilization with sufficient social science infrastructure to observe cohortization dynamics in real time. Many comparable cases, including the Vietnam War or civil rights era mobilizations, lack the fine-grained temporal measurement, both cross-sectional and panel, that accompanies this study. That being said, the Floyd case, for all its salience, cannot resolve the question of life-course persistence that the age-period-cohort (APC) literature may rightly demand; it will not do so, presumably, for many decades. Nonetheless, this article accepts this trade-off deliberately: short-to-medium term opinionation dynamics, after all, constitute a micro-foundation for such long-term processes.

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 racial marginalization that informs prior beliefs[9] 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 potentially serves as a *socializing incident*[10] rather than an *identity reinforcement* one.[11]

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 may 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. Attitudes toward law enforcement, in short, are an ideal construct for observing the opinionation dynamics: they are evaluative, politically legible, and sensitive to the kind of salient event this study examines.

attitude changes were rather small (Gethin and Pons 2024) or not durable (Reny and Newman 2021). None, however, considered the idea that the event may have catalyzed a cohortization process among young Americans.

[9] See Anoll and Engelhardt (2023) for a similar argument on how unique histories with U.S. institutions may "produce divergent foundations for integrating new information in the form of direct experiences into political views" (1150). Using the same terminology I have used above, minorities in the U.S. may have a larger consideration set.

[10] 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).

[11] The non-Hispanic Whites also have a higher internal political heterogeneity, compared to substantial levels of partisan sorting among ethnoracial minorities, making the former more appropriate for an analysis of partisan differentiation. According to the two-party share estimates of Cooperative Congressional Election Study, for instance, almost 90% of Black Americans in 2020 voted for the Democratic Party. This share is less than 43% for White Americans.

Analytic Strategy

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 2025), or simply ANES. I complement these data with *Crowd Counting Consortium* (CCC) and *Gallup Poll Social Series* (GPSS) for additional analyses.

Data Sources

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 improve 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 CCES 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 findings from the Nationscape and (2) extend the post-event period from the six months following the killing of George Floyd in Nationscape 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 with panel data from the 2016, 2020, and 2024 waves of the ANES. The panel design is particularly well-suited to assess within-person change, though the sample is smaller ($N = 1,507$), limiting precision in subgroup analyses. Nonetheless, the ANES provides a probability-based gold standard, and because the same individuals are observed over time, the ANES panel also helps rule out compositional changes as an unobserved driver of the observed generational differences.

Since these surveys offer distinct benefits, I use them all to triangulate the central expectations. The ANES provides the highest-quality sample, with the voting-eligible adults as its target population, and a panel design that tracks the same individuals across the study window. That said, its modest

sample size limits the precision in subgroup analyses. The CCES, with its matched samples, offers large N surveys across multiple waves, enabling more granular subgroup analyses than the ANES. That said, it is cross-sectional and its temporal resolution is coarse. The Nationscape, while not a probability sample, supplies weekly observations granular enough to characterize the immediate response to the event at a temporal resolution that neither the ANES nor the CCES can match.

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 and CCES at the county level. I leverage the intensity of protests in one's local environment as a proxy for their probable direct exposures to BLM mobilization, measured as the count of protests recorded in the CCC data.

To test changes in differential attention to political issues in the U.S., I use data from the *Gallup Poll Social Series*, which features the *most important problems* (MIPs) questions in 2020 and 2021 monthly surveys. I similarly use time-series estimates from the ANES in 2016 and 2020 to complement these scores. In both cases, I rely on the coding of open-ended data provided by each organization.

Supplemental Materials provide more details about these data sources, as well as decisions related to data processing. Tables S1 through S8 provide descriptive statistics across all data sources.

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) the police thermometer rating in ANES.[12] Across all surveys, I recoded the items so that higher values indicate more unfavorable attitudes toward law enforcement.

[12]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* and *somewhat safe* equal to 0, and *somewhat unsafe* and *mostly unsafe* equal to 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 recoded, so higher values represent colder feelings.

The measurement strategy in Nationscape and ANES 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 might not necessarily reflect a politicized response.[13] Despite these differences in emphasis and wording, however, it still captures an underlying evaluative disposition toward U.S. law enforcement, allowing me to present partial, albeit converging, evidence on the main expectations across a longer time window.[14]

Identification Strategy

The treatment, for all analyses, is *exposure to the killing of George Floyd and the BLM mobilization*. Since this exposure was effectively population-wide, the analyses focus on differential responses across individuals rather than on the average effect of the treatment itself. Let me start by noting that the theory predicts heterogeneity along two dimensions: *age*, which I tie to the size of one's pre-existing consideration sets, and *partisan identity*, which determines which political frames are internalized. I investigate differential trajectories in attitudes toward law enforcement before and after the event, stratified along both dimensions. This strategy generates conditional differences, which are, under the identification assumptions, interpreted as the differential causal effects of the exposure.

Following prior research that identifies age 25 as a significant point in dispositional development (Ghitza et al. 2023; Krosnick and Alwin 1989), I use age 25 as the operational cutoff in Nationscape, classifying respondents aged 24 and younger as the young and those aged 25 and older as the old. For the CCES and ANES, I extend the cutoff to age 29 for statistical precision in subgroup analyses. Sensitivity analyses that use alternative cutoffs are reported in the Supplemental Materials.

Let Y_{it} denote the attitude of individual i at time t, $A \in \{0, 1\}$ the age group, with 0 = old and 1 = young, G one's partisan identity, and $P \in \{0, 1\}$ whether the time is pre-event or post-event.

The first estimand is the within-group change in attitudes:

$$\mathbb{E}[Y_{it}|A, G, P = 1] - \mathbb{E}[Y_{it}|A, G, P = 0] \tag{1}$$

Equation (1) shows the quantity Hypotheses 1 and 2 predict to vary across age and partisan groups.

[13] That said, Table S13 in the Supplemental Materials indicates that the responses to this item in 2016 (before the event) sharply diverged by partisan identification, suggesting that the underlying construct was already politicized.

[14] 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. However, since the authors found mode effects, the analyses using the ANES always adjust for survey mode.

To test differential updating across age groups, I estimate the cross-group difference with:

$$(\mathbb{E}[Y_{it}|A=1,G,P=1]-\mathbb{E}[Y_{it}|A=1,G,P=0])-(\mathbb{E}[Y_{it}|A=0,G,P=1]-\mathbb{E}[Y_{it}|A=0,G,P=0]) \tag{2}$$

Equation (2) is a difference-in-differences (DID) quantity capturing polarization between younger and older individuals within a partisan group. To examine the trajectories predicted in Hypothesis 2, I estimate the dynamic analog of Equation (1) for each event window t, rather than pooling over the full post-window. Under the identification assumptions below, Equation (1) identifies the effect of the event for each stratum, defined via age and partisan identities, while Equation (2) identifies the differential effects of the event (i.e., between-cohort polarization) within partisan group G.

Of course, while Equations (1) and (2) invoke causal interpretations, the within-stratum change in Equation (1) confounds the treatment effect of the event with any contemporaneous trend or shock affecting that group. The differential update in Equation (2), in contrast, differences out shocks and trends common to both age groups within a partisan group, leaving the age-differential treatment effect under parallel trends. Existing studies have estimated the analog of Equation (1) through a regression-discontinuity-in-time design (Reny and Newman 2021) and a simple-difference design (Gethin and Pons 2024), establishing the local treatment effect of the event ("did attitudes change in response to the event?"). The analyses here, instead, focus on the heterogeneity question presented in Equation (2), asking "did age groups within each partisan group G change differentially?".

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*).

While these assumptions are standard, parallel trends assumption (PTA) between the young and the old requires more care to defend. First, I show that pre-event trajectories across age and partisan groups are essentially flat before the event and the pre-trend tests presented in Table S12 show no statistically significant differences.[15] As a further investigation, Supplemental Materials K presents

[15]One might object that the micromodel for differential updating itself predicts that the PTA across age groups should not hold. This critique, however, confuses volatility and secular change: while younger individuals are theorized to respond more strongly to new information (and being more volatile), there is no reason to assume that they will have different secular trajectories absent the event. To present further evidence that this is indeed the case, I conducted a

a variety of exploratory analyses that look at the difference in trajectories between the young and the old on a series of political issues during the same window, indicating no general developmental divergence between younger and older Americans before or after the killing of George Floyd.

I evaluate assumptions (2) and (3) empirically, examining compositional changes, age-differential media exposure, and exposure variation in BLM protest intensity as alternative mechanisms.

Statistical Strategy

I implement all difference-in-differences analyses using a dynamic event study model:

$$y_{iat} = \theta_a + \delta_t + \sum_{\tau=-q, \tau\neq-1}^{m} \beta_\tau D_{a\tau} + \gamma' X_{iat} + \epsilon_{iat} \tag{3}$$

where a indexes age group (the young and the old), τ indexes the event-time relative to the killing of George Floyd at time $t*$, θ_a are age group fixed effects, δ_t are time fixed effects, $D_{a\tau}$ is an indicator equal to one for individuals in the treated age group observed at event-time τ and zero otherwise, β_τ captures the differential mean between the age groups, and X_{iat} includes an adjustment set.

I adjust for one's sex (measured simply as *male* and *female*), level of education, geographic location, measured as congressional district and county, as well as political attention. The exclusion of these adjustments does not alter the findings, and the stability in pre-trends holds with or without these adjustments. Since the analyses rely on repeated cross-section data in Nationscape and CCES with a small number of treatment groups, standard cluster-robust estimators are unreliable. I therefore report heteroskedasticity-robust standard errors. All models are estimated using survey weights.

Findings

I present the analyses in three sections. First, I explore the basic expectations using the Nationscape, CCES, and ANES, showing that (a) changes in attitudes toward law enforcement in response to the killing of George Floyd were higher among younger individuals than older ones, (b) this difference was, at first, unevenly distributed yet directionally similar across major political wings, but after a period of time, (c) changes remained persistent only among young Democrats and Independents—

placebo analysis that looks at ANES 2020-2022 Social Media Study. I found that attitude changes between 2020 and 2022—each fielded after the event—show no directional differences across age, even though younger individuals are on average more volatile in their responses between periods (see Figure S8 for the full results).

ranging from half a year in Nationscape to four years in ANES—while quickly fading among older individuals and the Republicans. Next, I examine three alternative explanations that may confound the basic model: compositional changes in partisan identification, age-segmented media exposure, and differential changes in people's protest exposure. Once I rule these explanations out, I explore an alternative process in the next section, demonstrating that differential changes in the salience of race relations and policing can account for the proposed cohortization dynamics. Figure 2 provides a general overview of the study timeline, as well as the data sources used in the analyses.

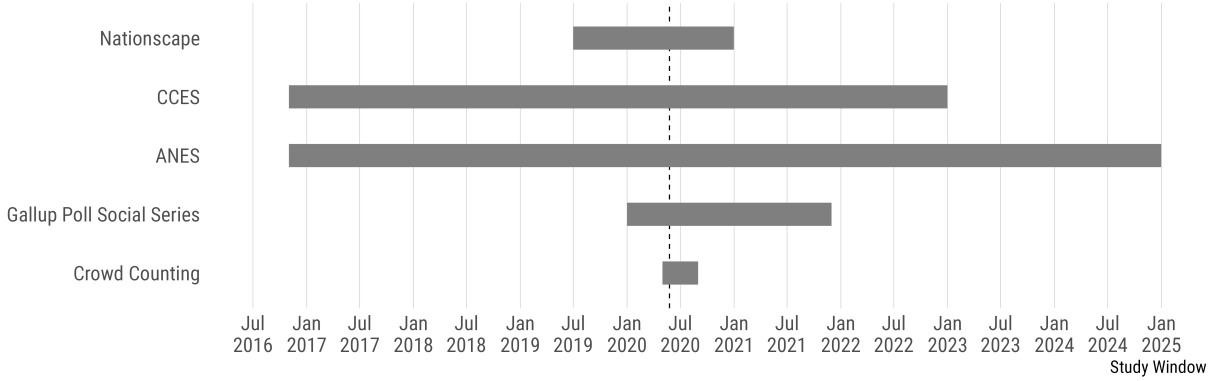

Figure 2: The Study Timeline and the Data Sources Used

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, people's initial response to the event, and subsequent trajectories. These estimates provide strong evidence for the hypotheses.

The response to the killing of George Floyd and the BLM 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 SD 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 heterogeneities: Democrats and Independents aged 18-24 experienced an average attitude change of 0.19 (0.56 SD) and 0.12 (0.36 SD), respectively, whereas Republicans experienced a mere 0.01 (0.04 SD) level of change which was not significant at $p < 0.05$. Therefore, consistent with Hypothesis 1, the mean changes in attitudes toward law enforcement were stronger among the young, with the magnitude of the updates varying by political identities.

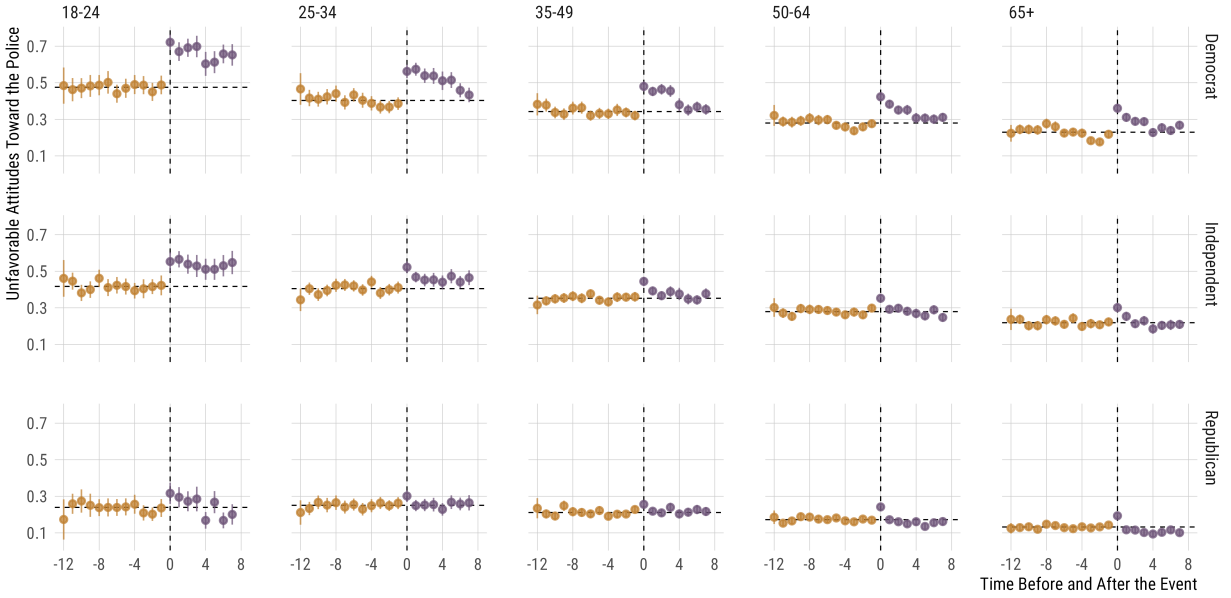

Figure 3: The Trajectory of Unfavorable Attitudes Toward the Police

Notes: The x-axis shows the time window before and after the killing of George Floyd (centralized at time = 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. See Table S22 in the Supplemental Materials for the full set of estimates, alongside their standard errors and sample sizes.

Consistent with Hypothesis 2, the initial changes in attitudes toward the police persisted among younger Democrats, while faded for older individuals and younger Republicans. Figure 3 reveals that Democrats and Independents aged 18–24 maintained their unfavorable evaluations for more than half a year, whereas comparable effects among older cohorts dissipated over the same period. Young Republicans, on the other hand, moved in the direction of more negativity toward the police (changing by 0.07, significant at $p < 0.001$) in the initial window, followed by a backlash movement (-0.06, $p < 0.01$). Consistent with Hypothesis 2, which claims incongruent considerations should fade as partisan signals reassert, this initial change among Republicans was temporary.[16]

To put these polarization dynamics in context, Figure 4 presents the findings from a set of dynamic difference-in-differences specifications. In doing so, the top panel investigates differences between the young and the old across the full sample while the bottom panel explores these dynamics *within*

[16]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 here. Figure S5 shows that using ideological identity rather than partisan identity provides similar age-based trajectories.

each party. The results suggest that the polarization in attitudes between the younger and the older individuals toward law enforcement persisted for at least half a year subsequent to the event among Democrats and Independents. Among Republicans, however, these initial updates quickly washed away—the young falling in line with the old individuals in their perceptions of police.[17]

Triangulating Cohortization Dynamics

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 study, while also extending the time to see whether the effects persist 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 measure on *the feelings of safety around the U.S. police*.

Cohortization Dynamics in CCES. Figure 5 presents dynamic difference-in-differences estimates, where people aged 18-29 in 2020 make up the "treatment group" and people aged 30 and older in 2020 make up the "control group." The killing of George Floyd and the subsequent BLM movement resulted in an average polarization of 11% on attitudes toward the police. While I cannot investigate the reception or retention dynamics in detail, I find that the differences remained largely constant two years later. The bottom panel presents the same estimates broken down by parties, showing relatively similar patterns of change I have observed among the Nationscape sample.

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 components of the 2016, 2020, and 2024 ANES surveys. The police 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 three waves) and sample size ($N = 1,507$), this analysis is highly conservative.

Cohortization Dynamics in ANES. Table 1 presents the central findings, revealing a clear pattern:

[17] In Figure S6, I leverage a moving window approach to estimate these difference-in-differences models across different age cut-offs, showing a monotonic decrease in estimates, as expected. These models show that, when it comes to the Nationscape data, the most intense polarization happens for 18-23, stabilizing around the age cut-off 22-27.

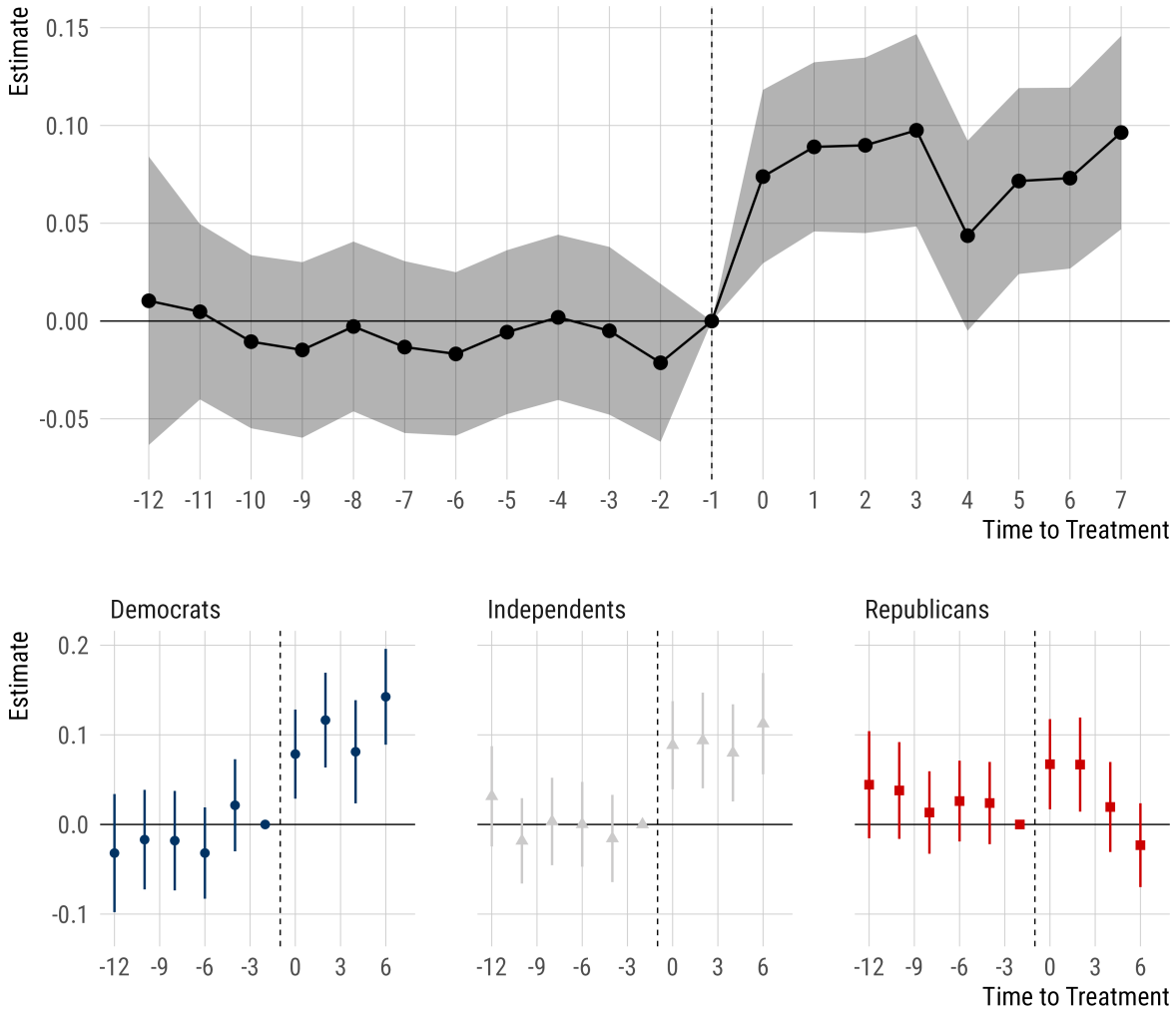

Figure 4: Dynamic Difference-in-Differences on Unfavorable Attitudes Toward the Police

Notes: The figure presents dynamic DID 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 sex, party identification, education, political attention, and 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 DID for each panel is estimated for two time periods at each step. The DID models underlying these figures are presented in Tables S9, S10, and S11 in the Supplemental Materials. Results from the tests of parallel trends in pre-treatment periods, showing no differential pre-trends, are presented in Table S12.

individuals between the ages of 18 and 29 in 2020 changed more strongly than individuals aged 30 and older, and this effect is exclusively concentrated among Democrats rather than Republicans. While estimating these models, I restricted the ANES sample to participants who were consistent partisans in the study window ($N = 1,113$). Looking at the coefficients, we see that polarization between younger and older individuals was 15 points on a 0–100 scale (0.67 SD) in 2020, and roughly

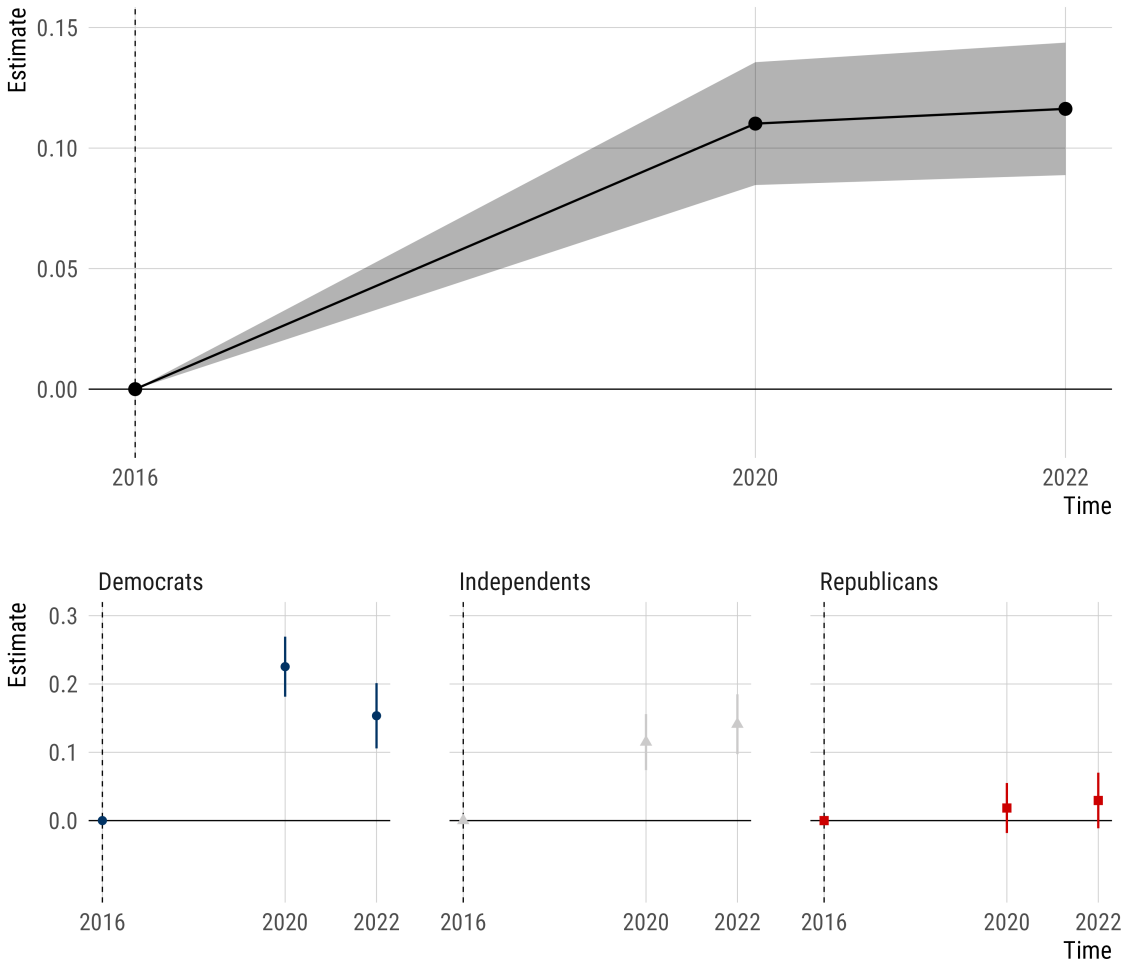

Figure 5: Dynamic Difference-in-Differences on "Feeling Unsafe" with the Police

Notes: The figure presents dynamic DID estimates that measure the diverging trajectories of the treated (18-29 year-old respondents) and control (age 30+) groups in the Cooperative Congressional Election Study's 2016-2022 data file. The model in the top panel adjusts for one's sex, party identification, education, political attention, and district. The models in the bottom panel perform the same estimations, each subsetting one party identifier in the U.S. as the main treated group. The DID models underlying these figures are presented in Table S14 in the Supplemental Materials.

11 points of this change was still visible 4 years later in 2024. Among Republicans, there was no significant change.[18] Considering that the panel structure resolves potential compositional changes that would violate stable composition assumption, this finding provides a significant confirmation of the main expectations.[19] I unpack the behaviors of the remaining individuals below.

[18] Due to low sample sizes, I could not incorporate consistent Independents as a separate group. Analyses that consider differential partisan movement over time, and their effects on these estimates, are presented below.

[19] In supplemental analyses, I present alternative estimates that use splines rather than qualitative age categories, which result in the same substantive findings (see Figure S7 and Table S15 for details).

Table 1: Dynamic Difference-in-Differences Models Estimating Police Thermometer in ANES

	Democrats	Republicans
Treatment: 2020	15.00**	7.86
	(4.62)	(4.47)
Treatment: 2024	11.06*	2.28
	(4.35)	(4.57)
Survey Mode in 2016: Web	1.39	-2.29
	(2.51)	(2.06)
Observations	1572	1767
Unique N	524	589

Notes: The DID models include respondent and survey-wave fixed effects. The interaction terms Treatment: 2020 and Treatment: 2024 capture differential changes in attitudes for respondents aged 29 or younger in 2020 relative to older respondents. The event baseline is set to 2016. Because the 2016 wave included both Web and Face-to-Face respondents, the effect of survey mode was identified independently from the survey year, despite the later waves being Web-only.
* $p < 0.05$, ** $p < 0.01$

Adjudicating Alternative Processes

There are several alternative processes that can independently generate the patterns I attribute to cohortization. In this section, I examine three candidates to evaluate their competing merits.

Compositional Partisan Change. The findings described so far, with the exception of ANES, cannot distinguish two scenarios. First, individuals' update trajectories may have depended on their initial partisan dispositions. This would support the general model. Second, however, it is also plausible to think that individuals update their initial partisan identities to reflect their new attitudes. This would imply that compositional changes in party identification generate the central findings.

To evaluate this possibility, I conducted two analyses. Since Nationscape and CCES are time-series cross-sectional datasets, we cannot observe the same individuals over time. However, both datasets included participants' vote recalls from 2016. We may, in principle, operationalize these responses as the *prior* partisan orientations of participants. Of course, even assuming that recall bias is negligible, this approach cannot properly disentangle partisan switching from unstable partisanship. It can, however, assess the extent to which the results hinge on alternative specifications. This constitutes the first analysis. The second analysis involves the remaining $N = 394$ individuals in ANES to see how changing partisan trajectories are reflected in changing attitudes toward the police. While the number of young individuals is very low in this group, limiting age-based analyses, it may still give us an understanding of how attitudes on the police change alongside partisan identities.

Figure 6 presents the results. Both the top and the middle panels show that, by 2016 vote recall, the

results are highly stable and in the expected direction.[20] Note that previous vote choice, particularly among those who voted, means that these age groups are older (that is, "the young" becomes much smaller when I condition on having voted in 2016), leading to relatively imprecise and attenuated estimates. However, the patterns still imply that the estimates are largely comparable.

Looking at the ANES, we find several patterns. With one exception, none of the trajectories shows movement in the police thermometer. Destabilizing Democrats—those who became Independents or Republicans in 2020 and 2024—however, show *increased* favorability of the police in this period, suggesting that those leaving the Democratic party after the event may have contributed to changes in attitudes. While sample size does not allow me to disaggregate this group by cohort, we might quantify their potential effects on overall estimates by including this group first among consistent Democrats, then among consistent Republicans. In doing so, I estimated successive TWFE models, showing that the average coefficient among Democrats declines from 12.9 to 11.6 (a change of 10%) with their inclusion, while the coefficient on Republicans changes from 4.9 to 5.2 (a change of 6%). This indicates that, overall, compositional changes do not affect the main conclusions.

Age-Segmented Media Exposure. An alternative explanation might propose that differential media consumption between younger and older individuals may drive these basic findings. Rather than reflecting a process of cohortization, the trajectories we observe thus simply result from *differential changes* in people's media consumption[21], independent of any age-based mechanisms.

To evaluate this possibility, I turn to Nationscape and CCES. The former featured several questions about where participants "might have heard news about politics," including responses about social media, local newspapers, CNN, MSNBC, Fox News, Network News, Local TV, Telemundo, NPR, AM Talk Radio, and national newspapers such as The New York Times. The latter featured a more limited set of channels (TV, newspapers, radio, as well as social media), though it included political social media behaviors that provide a profile of consumption (posting a comment on politics, watching or reading stories related to politics, following political events, and forwarding posts or events to other people). For each dataset, I used nearest neighbor matching with 3 units to match respondents within each unique time period using these media items, alongside gender, education, and partisan identity. To control for differential political attention, I interacted each media variable with participant political attention score. In the final step, I estimated within-party models to see whether the findings hold for these matched samples compared to the main samples.[22]

[20] I purposefully excluded "partisan switchers" (Democrats who were Trump voters in 2016 and Republicans who were Clinton voters in 2016) as their numbers were very small, resulting in large and highly imprecise estimates.

[21] I estimated DID models to see whether there are differential consumption changes after the event between the younger and older individuals. While substantively small, there were indeed several changes, most notably for social media.

[22] I matched 3 "control" units to each "treatment" unit, though using less or more leads to the same findings. I also used

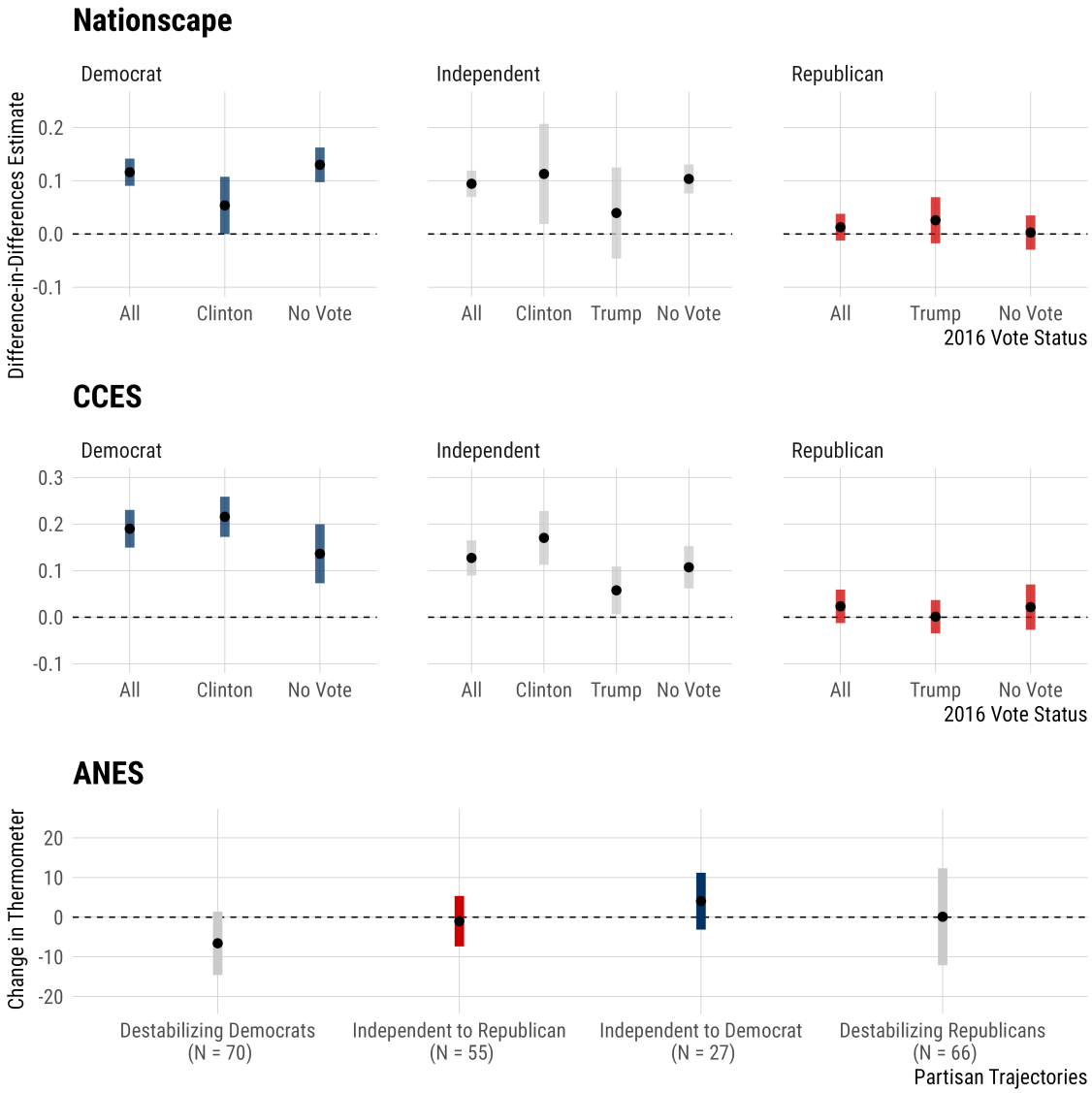

Figure 6: Compositional Changes in Party Identification

Notes: The top panel presents difference-in-differences estimates of attitudinal polarization between the young and the old across political parties and 2016 vote recall in the Nationscape. The middle panel presents difference-in-differences estimates of attitudinal polarization between the young and the old across political parties and 2016 vote recall in the CCES. The bottom panel presents estimates of change from the ANES, defined as the update from pre-event attitudes to the average of post-event attitudes (estimated, once again, via fixed effects models), across four groups: *Destabilizing Democrats*, those who switched from being a Democrat to being an Independent or Republican after the event; *Independent to Republican*, those who switched from being an Independent to being a Republican after the event; *Independent to Democrat*, those who switched from being an Independent to being a Democrat after the event, and *Destabilizing Republicans*, those who switched from being a Republican to being an Independent or a Democrat after the event. The models underlying these figures are presented in Tables S16 and S17 in the Supplemental Materials. The full set of coefficients representing all 24 models presented in the figure can be accessed via the open replication package.

the same specifications I used in the matching process while estimating the regression models.

Figure 7 presents the results. The left panel shows the standardized mean distances in unmatched and matched samples, suggesting that the matching was largely successful—with few exceptions—in generating comparable samples. The right panel shows the results from difference-in-differences analyses within groups, confirming that the findings remain largely stable when using the matched samples. While media consumption is surely endogenous in various ways, and it requires a much more in-depth analysis, these patterns provide greater confidence in the central findings.

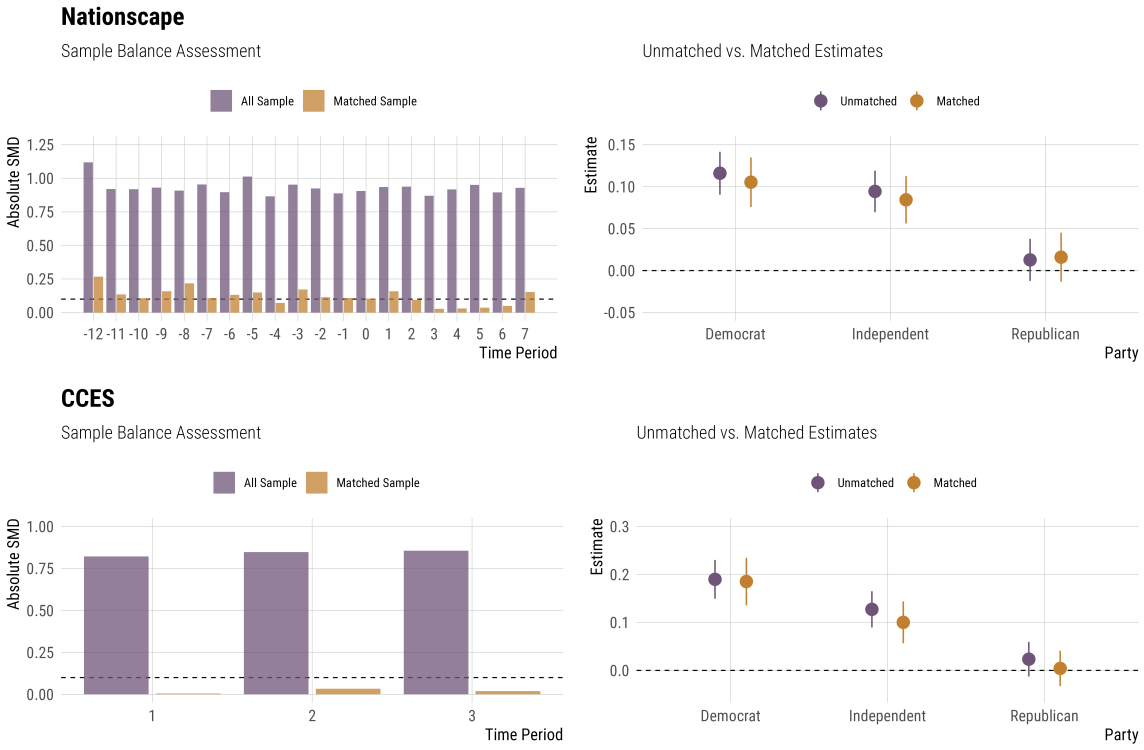

Figure 7: Difference-in-Differences Analyses on Matched Samples

Notes: The top panel presents the analyses for Nationscape, with the left figure showing standardized mean differences across matched and unmatched samples, while the right figure showing estimates from difference-in-differences analyses in the matched sample. Similarly, the bottom panel presents the analyses for the CCES using the same protocol. The models underlying these figures are presented in Tables S18 and S19 in the Supplemental Materials, while differences between younger and older individuals in media consumption are shown in Tables S3 and S6.

Exposure to BLM Mobilization. One final potential explanation is that the process of cohortization disguises differential environmental processes. Considering the age range of younger individuals, it might be the case that those who are in college also reside in predominantly college towns, which had more exposure to BLM. It might also be the case that the intensity of protests in a given region expose them to different considerations, which drive them to update their beliefs. Given that the BLM protests mainly occurred in Democratic areas, I may be picking up local effects.

In Figure 8, I show results from TWFE models with treatment-by-protest-intensity interactions, as measured in the Crowd Counting Consortium. The estimates show that proximity to high-protest districts does not lead to differential effects on within or between cohort differentiation.

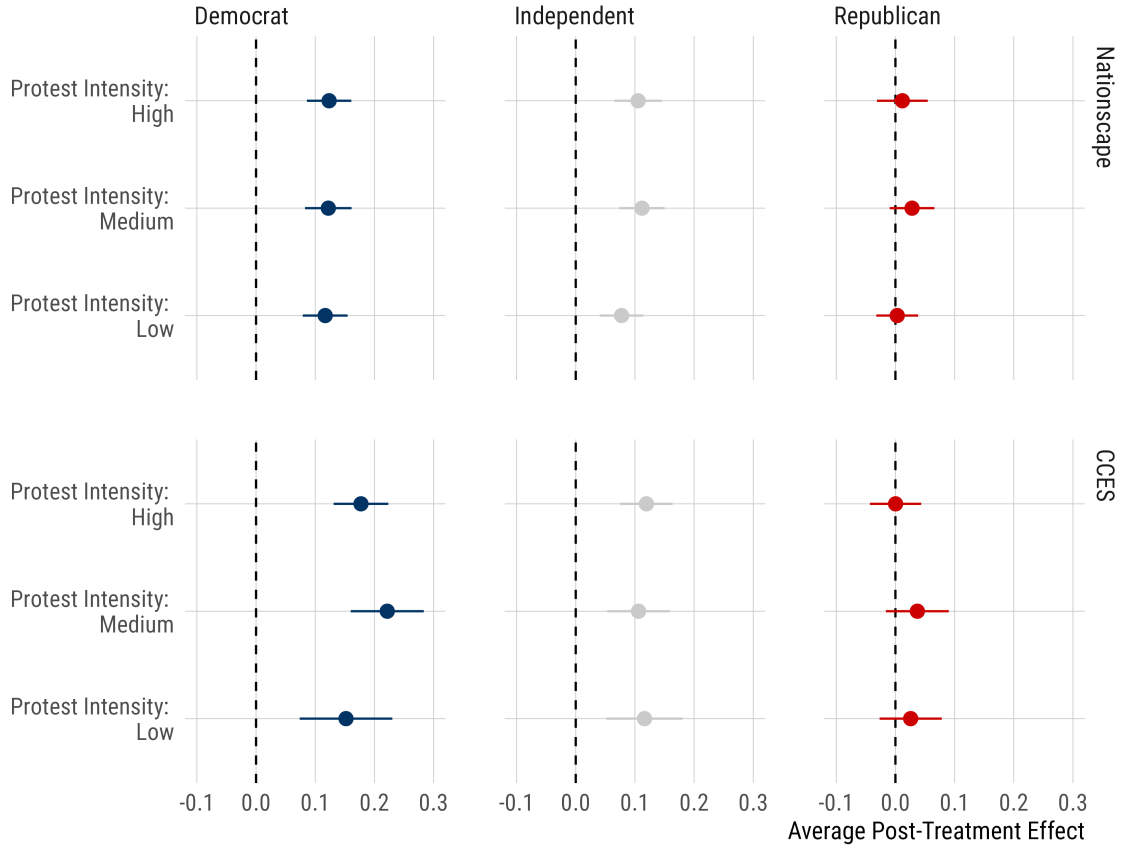

Figure 8: Differential Effects via Exposure to BLM Protests

Notes: The figure tests whether local exposure to protests moderates the main polarization effects within party groups, by presenting TWFE estimates with treatment-by-protest-intensity interactions, as measured in the CCC. The models underlying these figures are presented in the Tables S20 and S21 in the Supplemental Materials.

Political Attention as a Mechanism of Cohortization

While the life-course *timing* of an exogenous political shock is consequential for whether that shock will prove effective, it is also correct that cohorts must actually allocate some of their finite attention to the issues related to those shocks. Considering that attention is scarce and selective, this model implies that only a limited subset of issues will successfully imprint within any given cohort. In a world of many potential political issues, a young generation cannot deeply absorb them all; instead, they will strongly socialize into just those issues that happened to gain significant public attention

during their formative years. This implies that, with a limited *attention budget*, the issues in question should be able to overwhelm others; put differently, their attention share must increase.

I test one implication from this framework: when prompted to think about sociopolitical problems facing the United States, responses should be unevenly distributed across age and partisan groups, such that younger individuals identifying as Democrats and Independents should report problems related to race and policing more often spontaneously. I use the coded open-ended responses from Gallup Poll Social Series and the American National Election Studies for this analysis. In the former case, I treated responses saying "race relations" and "police brutality" in Gallup surveys as one of the three most important problems in the U.S. In the latter case, I coded "race relations" and "police issues" if participants mentioned them in their open-ended responses to ANES. While the Gallup survey ranges from January 2020 to November 2021, the ANES includes 2016 and 2020.

Figure 9 presents the results from the Gallup Poll Social Series, while Figure 10 presents the results from the ANES. Three empirical conclusions emerge from these findings. First, there was indeed a sizable attention gap between younger individuals and older individuals in the United States when it comes to perceiving issues related to race and policing as the most important problems. Second, this attention strongly increased from 2016 to 2020, much more so among younger individuals, in response to the killing of George Floyd. Third, while we know that attitudes persist among younger individuals, the attention allocated to race relations and policing steadily declined, suggesting that the imprinting process does not necessarily depend on sustained differential attention.

Discussion and Conclusions

Taken together, the evidence presented in this article supports a unified consideration-based model of generational imprinting. This framework provides a micro-foundation for cohort differentiation, arguing that an individual's response to an exogenous political shock is a joint function of partisan cue-taking (Premise 1) and the composition of the existing consideration sets (Premise 2). The joint operation of these two mechanisms produces not a coherent generational response but a generation internally divided, *within-cohort* polarization that the *between-cohort* framing obscures.

Theoretical and Empirical Limitations

There are, however, several limitations of this article that should temper the broader implications. First, the killing of George Floyd and the ensuing BLM mobilization constitute an unusually visible

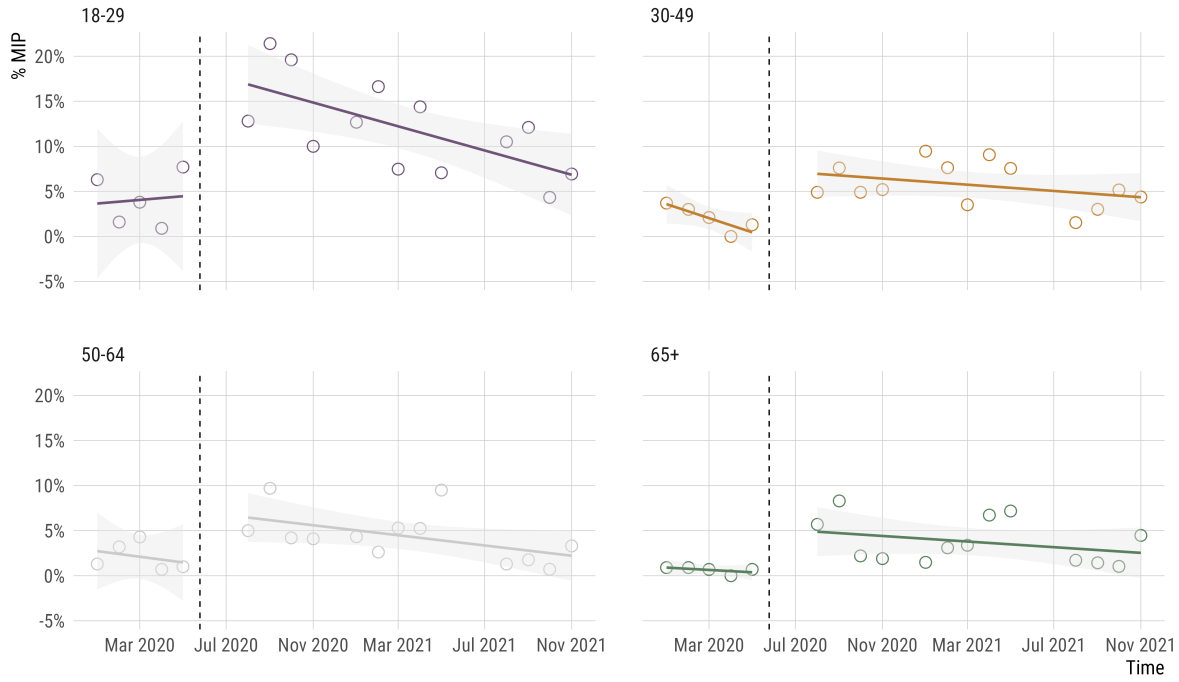

Figure 9: The Share of "Race Relations" and "Police Brutality" as MIP

Notes: The figure presents the percentage of respondents, stratified across age groups, who report "race relations" and "police brutality" as one of three "most important problems" in the United States based on the Gallup Poll Social Series.

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. While the strongly salient nature of the event provides a useful boundary condition, at the level of individual variation, there may be a variety of alternative mechanisms shaping differential response.

Second, the difference-in-differences strategy used in the article relies on untestable assumptions of parallel trends and stable compositional differences. 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 party groups. I relied on granular time windows with the Nationscape and a panel design with the ANES to offset these concerns, though the extent to which these attempts were successful depends on reasonable assumptions in the design.

Third, age effects in cross-sectional surveys conflate age, period, and cohort pathways (Fosse and Winship 2019), and the causal status of age remains contested in the literature (but see Kratz and Brüderl 2025). I conceptualize cohort effects as age-by-period interactions, but the single-event de-

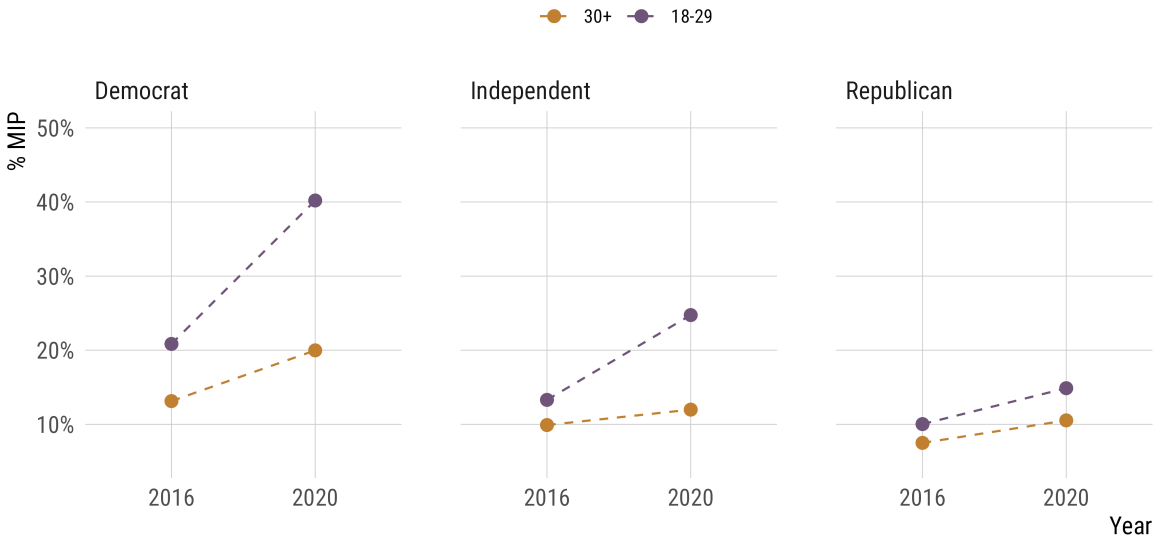

Figure 10: The Share of "Race Relations" and "Police Problems" as MIP

Notes: The figure presents the percentage of respondents, stratified across age groups, who report "race relations" and "police problems" as one of the "most important problems" in the United States, based on the ANES Time Series. The percentages demonstrate weighted averages across age and party groups.

sign used here cannot cleanly identify the sources of variation (Dinas and Stoker 2014). A stronger separation of these processes thus requires either repeated events or longer panel data, neither of which is available for the case at hand. Given that the killing of George Floyd and the ensuing BLM mobilization was a population-wide event, however, we have natural design limitations.

The article's model of generational imprinting assumes that individuals learn symbolic identities first, and align issue positions later (Boutyline and Vaisey 2017; Sears and Valentino 1997). Looking back at Figure 1, this means that a causal process from the treatment to a person's partisan identity was assumed not to exist. This is, naturally, an oversimplification. There are likely cases where the event caused a switch in people's affiliations, rather than the latter informing how they process the event. While the compositional trends I analyzed showed no effects contrary to the model, there may still be compositional changes that would produce observationally equivalent results. In this sense, a fully processual account needs a stronger design to investigate these possibilities.

A further limitation concerns the "Independents." The theoretical framework centered on partisan cue-taking and thus offers no clear prediction for individuals without strong partisan identification. I included Independents in the analyses for descriptive completeness, but their observed updates should be interpreted inductively rather than as a test of the identity-congruence mechanism. Future work might specify how low-information or weakly aligned actors fit into the model.

Implications

These findings support the view that shared experiences do not necessarily lead to convergent political attitudes, as these experiences are filtered through the political information environment. Instead, cohorts become differentiated into what Mannheim (1952) called cohort-units—subgroups within cohorts oriented toward the same political object through different interpretive standpoints. In contemporary U.S. politics, political parties provide the interpretive "scaffolding" for these units. The sorting process thus channels shared experiences, producing cohort differentiation rather than uniform convergence. Hence, one implication of this paper is to theorize generational polarization not as cohort replacement alone but as cohort-structured sorting within party coalitions.

One natural extension of this theoretical framework and findings is to examine the extent to which cohort replacement within the electorate may operate alongside or even overwrite elite repositioning. The long-term implications of these dynamics are that generational divergence *within* political camps may affect intraparty divergence on criminal justice, civil rights, and law enforcement policy. In this sense, the Floyd protests illustrate how event-driven cohortization may set the foundations for long-run evolution of generational political positions among the American electorate.

Ultimately, this article provides a useful corrective to classic narratives about generations. Instead of treating cohorts as passive recipients of "shared experiences," this article theorizes the political field as a structuring force that actively channels those experiences. Using this insight to develop a micro-level mechanism of generational differentiation, I argue that a more processual understanding of generational imprinting can provide a better explanation for cohort-led polarization.

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Supplemental Materials for

Generational Imprinting: How Political Events Shape Cohorts

Turgut Keskintürk

Table of Contents

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 312,954 non-Hispanic Whites in the U.S. across an extended period.

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 these down by the age groups.

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%
Ideological Identification	
Liberal	23.4%
Moderate	42.6%
Conservative	34.0%
Vote in 2016	
Trump	34.1%
Clinton	24.1%
Third Candidate	3.4%
No Vote/Recall	24.8%
Not Eligible	13.6%
Political Attention	0.60 (0.31)

Table S2: Descriptive Statistics by Age Group for Nationscape

Characteristic	**18-24** N = 24817	**25-34** N = 51872	**35-49** N = 85783	**50-64** N = 90433	**65+** N = 60049
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%
Ideological Identification					
Liberal	32.5%	27.7%	25.5%	19.7%	19.6%
Moderate	44.2%	48.5%	45.5%	42.1%	36.2%
Conservative	23.3%	23.8%	29.0%	38.1%	44.2%
Vote in 2016					
Trump	8.8%	23.3%	32.7%	38.9%	46.1%
Clinton	7.5%	17.8%	22.1%	26.0%	33.6%
Third Candidate	1.4%	3.3%	3.5%	3.7%	3.7%
No Vote/Recall	20.9%	42.9%	30.7%	22.8%	11.8%
Not Eligible	61.4%	12.6%	11.1%	8.6%	4.8%
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 survey question that serves as the main dependent variable.[1]

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 create an overall political attention item.

Table S3 shows the differences in media consumption channels between the "treated" and the "control" groups, used in the matching analyses between age groups in the main manuscript. Note that while I show the pooled averages, the matching procedure is applied separately to each period.

Table S3: Differential Media Consumption Profiles in Nationscape

Characteristic	**Age 24 or Younger** N = 24817	**Age 25 or Older** N = 288137
Social Media	87.5%	61.7%
Local Newspapers	26.2%	38.3%
CNN	45.7%	40.3%
MSNBC	22.4%	28.3%
Fox News	39.1%	42.2%
Network News	44.4%	61.6%
Local TV	41.2%	62.5%
Telemundo	5.8%	4.1%
NPR	13.5%	15.0%
AM Talk Radio	20.5%	19.4%
National Newspapers	36.4%	28.2%

[1] In addition to these constraints, I dropped roughly 4,400 respondents due to missing data in covariates; however, this group represents only 1.4% of the observations within the survey window.

1.2 Cooperative Congressional Election Study

To supplement the Nationscape analyses and extend the time coverage 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 S4 provides a descriptive summary of pertinent characteristics of participants while Table S5 breaks them down by the age groups.

Table S6 shows the differences in media consumption channels between the "treated" and the "control" groups, used in the matching analyses between age groups in the main manuscript. Note that while I show the pooled averages, the matching procedure is applied separately to each period.

Table S4: Descriptive Statistics for CCES

Characteristic	**2016** N = 44627	**2020** N = 43037	**2022** N = 40099
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+	21.2%	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%	38.3%
Republican	32.1%	36.0%	34.6%
Vote in 2016			
Clinton	29.9%	29.4%	24.8%
No Vote	26.8%	23.0%	31.4%
Third Candidate	4.1%	4.8%	6.2%
Trump	39.2%	42.7%	37.5%
Political Attention	0.80 (0.30)	0.77 (0.32)	0.73 (0.35)

Table S5: Descriptive Statistics by Age Group for CCES

Characteristic	24 or Younger N = 7386	25-34 N = 18340	35-49 N = 28497	50-64 N = 39504	65+ N = 34036
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%	32.1%	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%
Vote in 2016					
Clinton	13.4%	26.8%	30.1%	28.7%	30.7%
No Vote	65.5%	42.0%	31.0%	20.1%	12.4%
Third Candidate	4.7%	7.1%	6.7%	4.4%	3.1%
Trump	16.3%	24.0%	32.2%	46.8%	53.8%
Political Attention	0.59 (0.36)	0.65 (0.35)	0.73 (0.34)	0.81 (0.30)	0.87 (0.25)

1.3 The Crowd Counting Consortium

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 this protest vector with the Nationscape data. In the end, protest intensity refers to the number of protests in the three-month window in a region.

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 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.

For CCES, I use the same procedure, but—fortunately—CCES also included respondents' counties, which allowed me to implement the matching at the county level using the full CCC data.

1.4 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 component of the 2016, 2020, and 2024 waves of the American National Election Study. Between 2016 and 2024, ANES surveyed the same 1,507[2] non-Hispanic White individuals, allowing for an evaluation of whether change scores in the police thermometer measure from 2016 to 2024 differ between younger and older cohorts. Table S7 shows descriptive statistics for pertinent measures[3], and Table S8 breaks them down by partisanship.

1.5 American National Election Social Media Study

In the Supplemental Materials, I present analyses using the *Social Media Study* from the American National Election Study. This two-wave panel, collected during the 2020 presidential election and following the 2022 midterm election, draws on an online sample maintained by the National Opinion Research Center (NORC) at the University of Chicago. As with previous datasets, I restrict the sample to non-Hispanic White Americans, who made up 69.3% of the total participant pool.

The final analytic sample includes respondents who participated in both waves and provided valid responses to the police feeling thermometer, as well as their birth years (unique $N = 1,540$).

The first data collection of the police thermometer measure took place in the immediate aftermath of the 2020 presidential election (fielded between November 1, 2020, and January 1, 2021), while the second occurred following the 2022 midterm elections (fielded between November 9, 2022, and January 2, 2023). The data was thus collected after the killing of George Floyd.

Table S6: Differential Media Consumption Profiles in CCES

Characteristic	**Age 29 or Younger** N = 15974	**Age 30 or Older** N = 111789
TV	40.8%	66.4%
Newspaper	30.0%	41.0%
Radio	28.5%	32.5%
Social Media	90.1%	71.5%
Social Media Activities: Post	19.4%	23.2%
Social Media Activities: Comment	18.6%	29.4%
Social Media Activities: Story	60.3%	57.7%

[2]A 1.5% reduction from 1,531 individuals due to missing data in age, party, or police thermometer.

[3]While constructing partisanship, the leaners are coded in their respective parties. For descriptive purposes, I present the distribution of party trajectories in 2020, including those who switch their parties before or after.

Social Media Activities: Event	23.2%	22.9%
Social Media Activities: Forward	23.1%	24.9%

1.6 Measuring the "Most Important Problems"

To measure the "most important problems," I relied on two sources: cross-sectional datasets from the 2016 and 2020 American National Election Study, and the monthly waves from the Gallup Poll Social Series. For the former, I used the public files and open-ended response codes presented in the ANES cumulative data files. For the latter, I estimated weighted averages of open-ended response codes by age group using the raw Gallup Poll Social Series data files. Due to the proprietary nature of this data, the replication files only included the weighted estimates at the month and age-group level. In both datasets, I only used survey responses from non-Hispanic White participants.

Table S7: Descriptive Statistics for ANES

Characteristic	**N = 1507**
Age Groups	
18-29	12.9%
30-39	18.6%
40-64	45.1%
65+	23.3%
Party Identification at 2020	
Democrat	38.4%
Independent	12.2%
Republican	49.5%

Table S8: Descriptive Statistics by Partisanship for ANES

Characteristic	**Democrat = 626**	**Independent = 137**	**Republican = 744**
Age Groups			
18-29	13.8%	16.0%	11.5%
30-39	18.3%	19.0%	18.7%
40-64	44.5%	42.2%	46.4%
65+	23.4%	22.9%	23.4%

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.

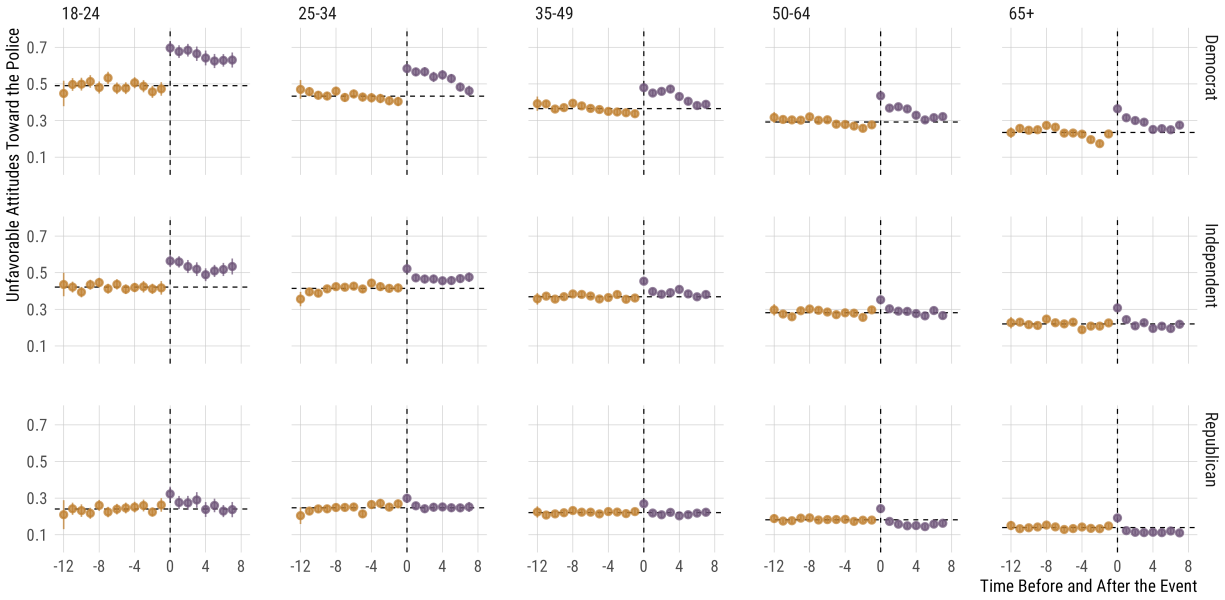

Figure S1: The Trajectory of Unfavorable Attitudes Toward the Police (Unweighted)
Notes: The x-axis shows the time window before and after the killing of George Floyd, centralized at $t = 0$, with each tick representing 4-week windows in Nationscape 2019-2020 data file. The y-axis shows the unfavorable 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. In Figure S4, I present the same results using weekly waves, confirming the expectations. In Figure S5, I present alternative analyses that instead use ideological identity. Similar results hold.

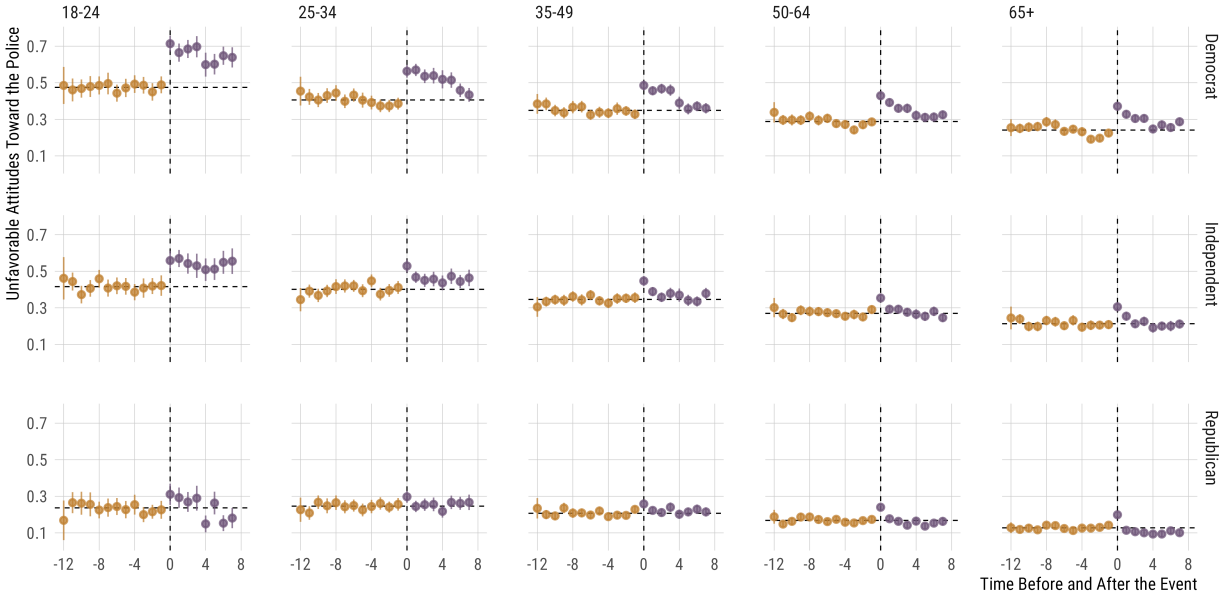

Figure S2: The Trajectory of Unfavorable Attitudes Toward the Police (Alternative Coding)

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.

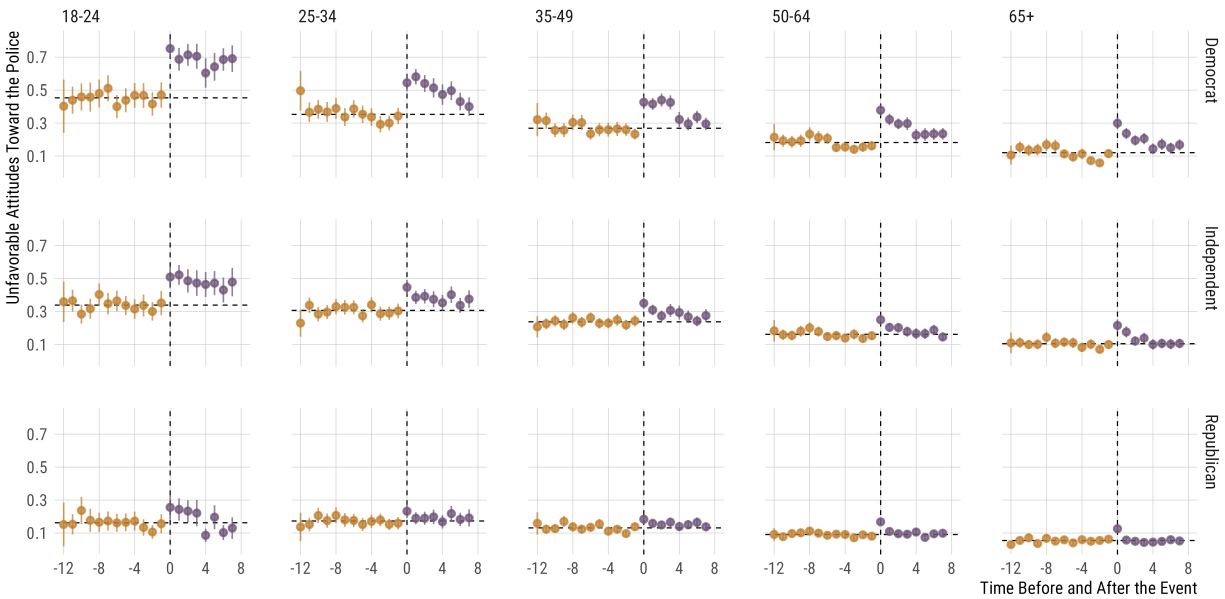

Figure S3: The Trajectory of Unfavorable Attitudes Toward the Police (Binary Outcome)

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.

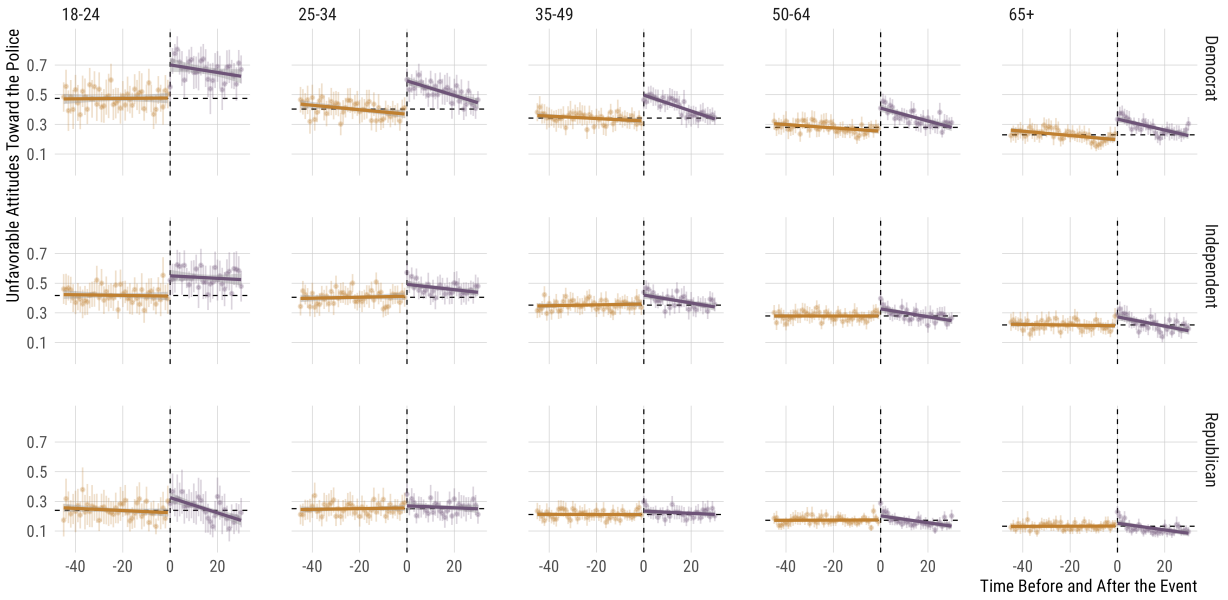

Figure S4: The Trajectory of Unfavorable Attitudes Toward the Police (Weekly Estimates)

Notes: The x-axis shows the time window before and after the killing of George Floyd, centralized at $t = 0$, with each tick representing 1-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.

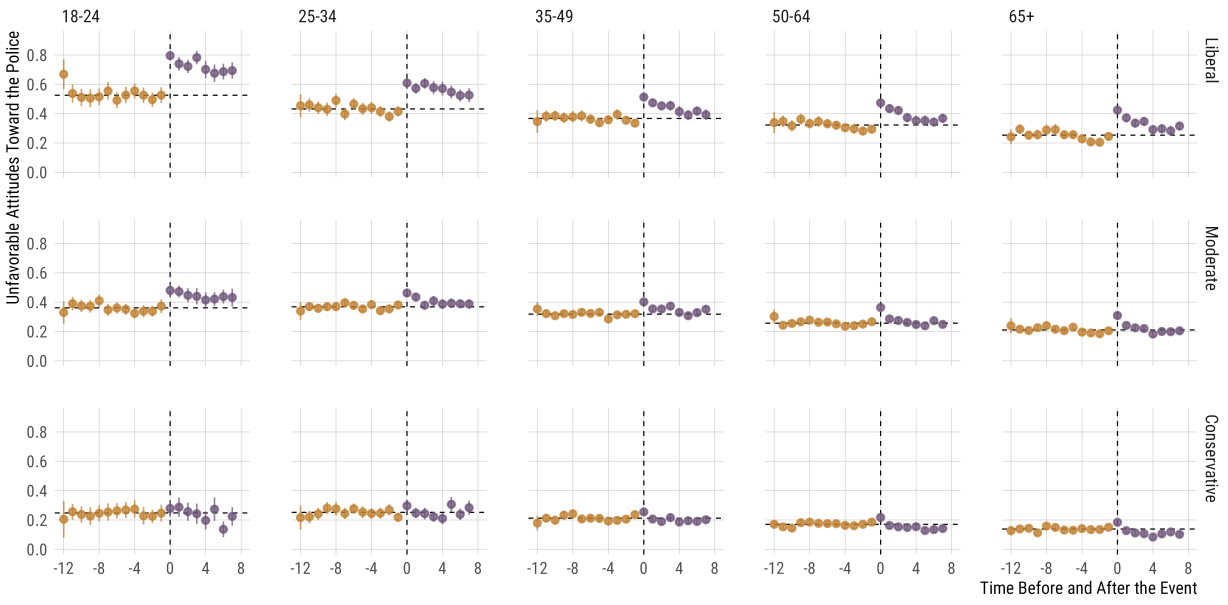

Figure S5: The Trajectory of Unfavorable Attitudes Toward the Police (Ideology)

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: Regression Analyses for Nationscape

This section presents the full tables for regression analyses using Nationscape:

- *Table S9*: difference-in-differences analyses estimating unfavorable attitudes toward the police as a function of treatment status and additional controls.
- *Table S10*: event study analyses, with leads and lags around the baseline at $t = -1$.
- *Table S11*: event study analyses within each party group, with binned periods.
- *Table S12*: analyses that investigate pre-trend parallel trends within the party groups.

Table S9: DID Models Estimating Unfavorable Attitudes Toward the Police

	Full Sample	Democrats	Independents	Republicans
Treatment	0.087***	0.116***	0.094***	0.013
	(0.008)	(0.013)	(0.013)	(0.013)
Gender	0.028***	0.011**	0.043***	0.033***
	(0.002)	(0.003)	(0.003)	(0.002)
Party: Independent	-0.037***			
	(0.002)			
Party: Republican	-0.164***			
	(0.002)			
Education: Some College	-0.015***	0.002	-0.027***	-0.016***
	(0.002)	(0.005)	(0.004)	(0.003)
Education: College	-0.026***	-0.003	-0.055***	-0.020***
	(0.003)	(0.005)	(0.005)	(0.004)
Education: Post-Graduate Degree	-0.023***	-0.012*	-0.041***	-0.012*
	(0.003)	(0.006)	(0.006)	(0.005)
Political Attention	-0.058***	0.063***	-0.071***	-0.134***
	(0.003)	(0.006)	(0.005)	(0.005)
N	312954	91448	104828	116675
R2	0.13	0.14	0.11	0.06

Notes: DID estimates using the Nationscape data files, where treatment denotes people who were between the ages of 18 and 24 during the study window. Columns represent the groups used in each model. Unit fixed effects in the TWFE specification include age group, time, and congressional district.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table S10: Dynamic DID Models Estimating Unfavorable Attitudes Toward the Police

	Full Sample
Time: -12	0.010
	(0.038)
Time: -11	0.005
	(0.023)
Time: -10	-0.011
	(0.023)
Time: -9	-0.015
	(0.023)
Time: -8	-0.003
	(0.022)
Time: -7	-0.013
	(0.022)
Time: -6	-0.017
	(0.021)
Time: -5	-0.006
	(0.021)
Time: -4	0.002
	(0.022)
Time: -3	-0.005
	(0.022)
Time: -2	-0.021
	(0.021)
Time: 0	0.074**
	(0.023)
Time: 1	0.089***
	(0.022)
Time: 2	0.090***
	(0.023)
Time: 3	0.098***
	(0.025)
Time: 4	0.044
	(0.025)
Time: 5	0.072**
	(0.024)
Time: 6	0.073**
	(0.024)
Time: 7	0.096***
	(0.025)
Gender	0.028***
	(0.002)
Party: Independent	-0.037***
	(0.002)
Party: Republican	-0.164***
	(0.002)
Education: Some College	-0.015***
	(0.002)

Education: College	-0.026***
	(0.003)
Education: Post-Graduate Degree	-0.023***
	(0.003)
Political Attention	-0.058***
	(0.003)
N	312954
R2	0.13

Notes: DID estimates using the Nationscape data files, where treatment denotes people who were between the ages of 18 and 24 during the study window. Columns represent the groups used in each model. Unit fixed effects in the TWFE specification include age group, time, and congressional district.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table S11: Dynamic DID Models Estimating Unfavorable Attitudes Toward the Police

	Democrats	Independents	Republicans
Time: -12:-11	-0.032	0.031	0.044
	(0.034)	(0.029)	(0.030)
Time: -10:-9	-0.017	-0.018	0.038
	(0.028)	(0.024)	(0.028)
Time: -8:-7	-0.018	0.003	0.013
	(0.028)	(0.025)	(0.023)
Time: -6:-5	-0.032	0.000	0.026
	(0.026)	(0.024)	(0.023)
Time: -4:-3	0.021	-0.016	0.024
	(0.026)	(0.025)	(0.023)
Time: 0:1	0.078**	0.088***	0.067**
	(0.025)	(0.025)	(0.026)
Time: 2:3	0.116***	0.094***	0.067*
	(0.027)	(0.027)	(0.027)
Time: 4:5	0.081**	0.080**	0.020
	(0.029)	(0.028)	(0.026)
Time: 6:7	0.142***	0.112***	-0.023
	(0.027)	(0.029)	(0.024)
Gender	0.011**	0.043***	0.033***
	(0.003)	(0.003)	(0.002)
Education: Some College	0.002	-0.027***	-0.016***
	(0.005)	(0.004)	(0.003)
Education: College	-0.003	-0.055***	-0.020***
	(0.005)	(0.005)	(0.004)
Education: Post-Graduate Degree	-0.012*	-0.041***	-0.012*
	(0.006)	(0.006)	(0.005)
Political Attention	0.063***	-0.071***	-0.134***
	(0.006)	(0.005)	(0.005)
N	91448	104828	116675
R2	0.14	0.11	0.07

Notes: DID estimates using the Nationscape data files, where treatment denotes people who were between the ages of 18 and 24 during the study window. Columns represent the groups used in each model. Unit fixed effects in the TWFE specification include age group, time, and congressional district.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table S12: Pre-Trend Tests for the Difference-in-Differences Analyses in Nationscape

Model	Estimate	Error	p
Full Model	-0.007	0.017	0.692
Democrats	-0.020	0.028	0.475
Independents	0.006	0.029	0.850
Republicans	0.005	0.028	0.848

4 Supplemental Materials D: Pre-Event Predictions of "Feeling Unsafe" in the CCES

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. Table S13 shows regression estimates from a model that provide clear evidence that this question was already *politicized* even before the event in 2016, showing clear differences across parties.

Table S13: Feeling Unsafe with the Police in 2016

	CCES in 2016
Party: Independent	0.000
	(0.005)
Party: Republican	-0.069***
	(0.005)
Gender	0.018***
	(0.004)
Education: Some College	-0.006
	(0.005)
Education: College	-0.025***
	(0.006)
Education: Post-Graduate Degree	-0.016*
	(0.007)
Political Attention	0.007
	(0.008)
N	44202
R2	0.13

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

5 Supplemental Materials E: Regression Analyses for CCES

Table S14 provides the difference-in-differences analyses estimating feelings of unsafety with the police as a function of treatment status and additional controls.

Table S14: Dynamic DID Models Estimating Feelings of Unsafety with the Police

	Full Sample	Democrats	Independents	Republicans
Treatment: 2020	0.110***	0.225***	0.115***	0.018
	(0.013)	(0.022)	(0.021)	(0.019)
Treatment: 2022	0.116***	0.153***	0.141***	0.029
	(0.014)	(0.024)	(0.022)	(0.021)
Gender	0.017***	-0.005	0.033***	0.023***
	(0.003)	(0.005)	(0.005)	(0.003)
Party: Independent	-0.034***			
	(0.004)			
Party: Republican	-0.142***			
	(0.003)			
Education: Some College	-0.018***	0.004	-0.027***	-0.019***
	(0.004)	(0.008)	(0.007)	(0.004)
Education: College	-0.036***	-0.013	-0.046***	-0.040***
	(0.004)	(0.008)	(0.007)	(0.004)
Education: Post-Graduate Degree	-0.026***	-0.006	-0.039***	-0.031***
	(0.004)	(0.008)	(0.008)	(0.006)
Political Attention	0.030***	0.100***	0.042***	-0.042***
	(0.005)	(0.011)	(0.009)	(0.007)
N	127566	41140	47496	37961
R2	0.14	0.18	0.17	0.16

Notes: DID estimates using the CCES data, where treatment denotes survey participants who were 29 or younger during the 2020 window. Columns represent the groups used in each model. Unit fixed effects in the TWFE specification include age group, time, and county fixed effects. The event baseline is set to 2016.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

6 Supplemental Materials F: Alternative Age Cut-Offs 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.

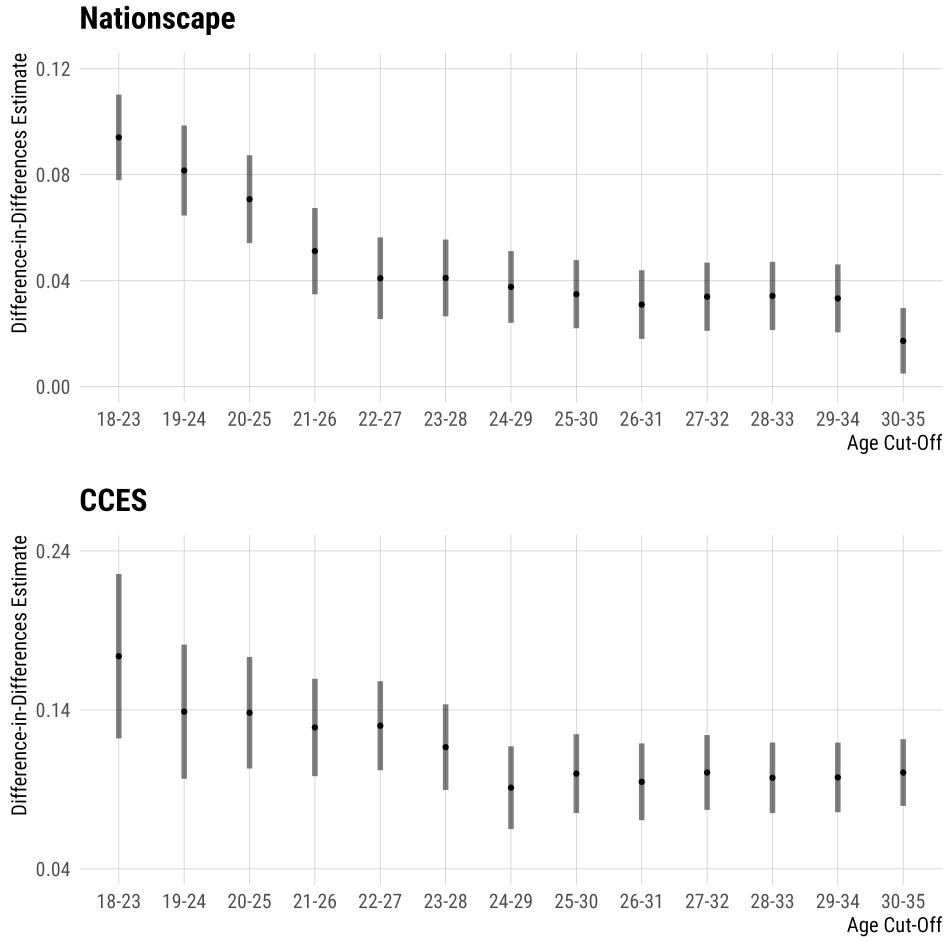

Figure S6: Varying Age Windows in Difference-in-Differences Analyses

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. All models (13 models for both the top and the bottom panel) adjust for gender, party identification, education, attention, and local district. To ease the interpretations, I omit the control variables from the figures.

7 Supplemental Materials G: Regression Analyses for ANES

Figure S7 presents results from a model estimated on the 2016 and 2020 waves of the ANES panel that examines changes in respondents' evaluations of the police between 2016 and 2020. To capture potential nonlinearities in age, I specified age as a spline with four degrees of freedom. The number of knots was selected through a simple cross-validation procedure: I fit models ranging from one to five degrees of freedom and compared out-of-sample prediction error, selecting the model with four degrees of freedom as the best balance of fit and parsimony. This specification provides greater flexibility than a categorical age variable, while still maintaining interpretability. The basic spline modeling that underlies this figure is presented in Table S15 with full trajectory interactions.

The figure presents predicted first-differences in police thermometer by age and partisan trajectory. Predictions are derived from the spline model, and averaged across the survey observations.

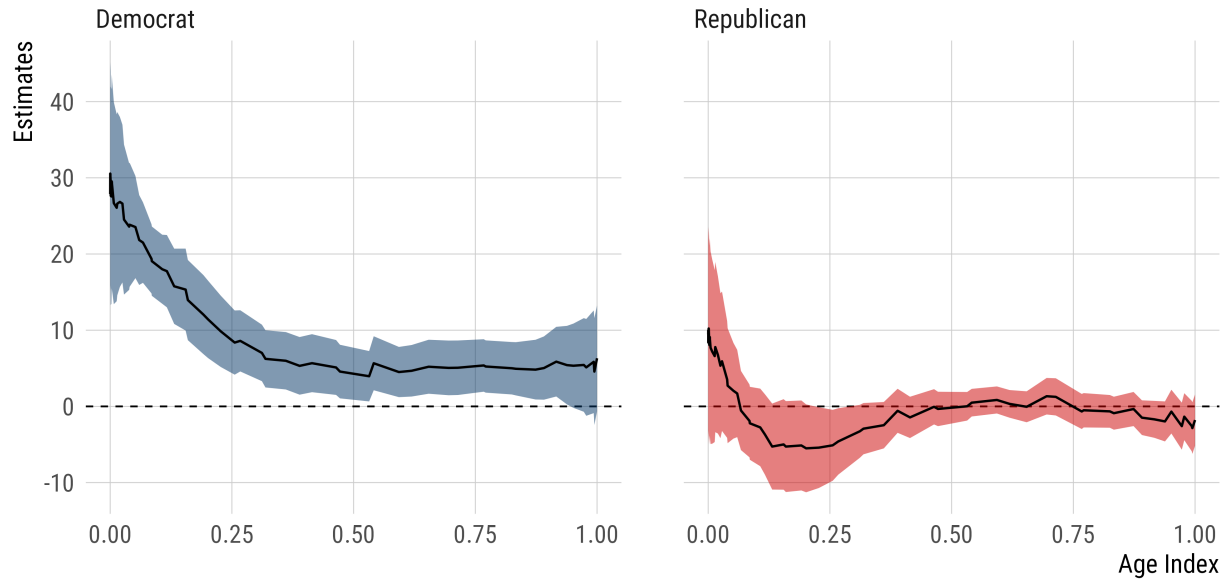

Figure S7: First-Differences in Police Thermometer, with Age Splines

Notes: The figure presents estimates of change in police thermometer in ANES 2016-2020 panel data, using an age-spline with 4 degrees of freedom, adjustment for survey mode, and robust standard errors. To help with the interpretation of spline estimates, the figure shows predictions of change scores calculated from this baseline model.

Table S15: Spline Model Estimating First Differences

	(1)
ns(Age) = 1	-25.513***
	(7.374)
ns(Age) = 2	-19.034**
	(6.062)
ns(Age) = 3	-33.323
	(17.048)
ns(Age) = 4	-15.533***
	(4.659)
Party: Partisan Switchers	-26.278
	(14.967)
Party: Republican	-19.025
	(10.109)
Mode Effect: Web Survey	-3.118
	(1.656)
ns(Age) = 1 x Partisan Switchers	22.986
	(13.328)
ns(Age) = 2 x Partisan Switchers	30.784**
	(11.282)
ns(Age) = 3 x Partisan Switchers	28.948
	(36.013)
ns(Age) = 4 x Partisan Switchers	16.558*
	(7.730)
ns(Age) = 1 x Republican	16.597
	(10.060)
ns(Age) = 2 x Republican	16.122*
	(7.716)
ns(Age) = 3 x Republican	7.771
	(23.446)
ns(Age) = 4 x Republican	14.515*
	(5.630)
N	1507
R2	0.10

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

8 Supplemental Materials H: Placebo Analyses Using the ANES Social Media Study

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 ambivalence of response? 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 S8 confirms this prediction.

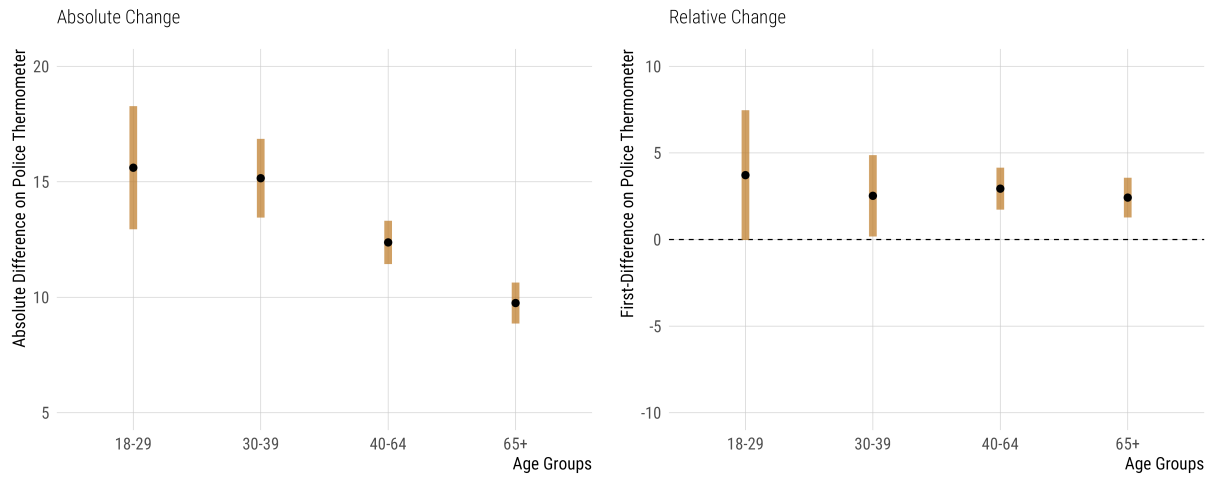

Figure S8: First-Differences in Police Thermometer in 2020-2022 Social Media Study

Notes: The figure presents estimates of change in police thermometer in ANES 2020-2022 panel data, where the left panel shows differences for absolute change while the right panel shows differences for directional change. To help with the interpretation of estimates, the figure shows predictions of change scores calculated from the baseline models, where I simply regressed the absolute or relative change scores on age group, with robust standard errors.

9 Supplemental Materials I: Regression Estimates for Alternative Explanations

This section provides regression estimates that adjudicate alternative processes.

- Table S16 shows DID estimates that examine partisan compositional change by reported 2016 vote. These models adjust for gender, party identification, education, attention, and districts or counties. For brevity, I only report the main DID estimates, omitting these controls.[4]
- Table S17 presents estimates from the ANES that look at partisan switching across time.
- Tables S18 and S19 provide regression estimates from models with matched samples.
- Tables S20 and S21 provide regression estimates that adjudicate local trends in protest exposure.

[4]The full set of coefficients from these models is presented in replication file path `output/C_claim/tablesA.csv`.

Table S16: DID Estimates Across 2016 Vote Choice for Nationscape and CCES

Data	Party ID	Vote in 2016	Estimate	95% CI (Lower)	95% CI (Upper)
Nationscape	Democrat	All	0.116	0.090	0.142
Nationscape	Democrat	Clinton	0.054	0.000	0.107
Nationscape	Democrat	No Vote	0.130	0.097	0.163
Nationscape	Independent	All	0.094	0.070	0.119
Nationscape	Independent	Clinton	0.113	0.019	0.207
Nationscape	Independent	Trump	0.039	-0.046	0.125
Nationscape	Independent	No Vote	0.103	0.076	0.131
Nationscape	Republican	All	0.013	-0.012	0.038
Nationscape	Republican	Trump	0.026	-0.018	0.069
Nationscape	Republican	No Vote	0.003	-0.029	0.035
CCES	Democrat	All	0.190	0.149	0.230
CCES	Democrat	Clinton	0.216	0.172	0.259
CCES	Democrat	No Vote	0.136	0.073	0.200
CCES	Independent	All	0.127	0.090	0.165
CCES	Independent	Clinton	0.170	0.113	0.228
CCES	Independent	Trump	0.058	0.007	0.109
CCES	Independent	No Vote	0.107	0.062	0.153
CCES	Republican	All	0.023	-0.013	0.059
CCES	Republican	Trump	0.001	-0.035	0.037
CCES	Republican	No Vote	0.022	-0.027	0.070

Table S17: DID Estimates Across 2016 Vote Choice for ANES

Trajectory	Estimate	95% CI (Lower)	95% CI (Upper)
Independent to Republican	-1.0	-7.4	5.4
Destabilizing Republicans	0.1	-12.1	12.3
Destabilizing Democrats	-6.6	-14.6	1.4
Independent to Democrat	4.0	-3.1	11.2

Table S18: DID Models Estimating Unfavorable Attitudes Toward the Police

	Democrats	Independents	Republicans
Treatment	0.105***	0.084***	0.016
	(0.015)	(0.014)	(0.015)
Gender	0.003	0.039***	0.043***
	(0.007)	(0.007)	(0.007)
Education: Some College	-0.008	-0.034***	-0.019**
	(0.008)	(0.007)	(0.007)
Education: College	-0.036***	-0.083***	-0.012
	(0.011)	(0.011)	(0.011)
Education: Post-Graduate Degree	-0.039*	-0.033	-0.043*
	(0.018)	(0.025)	(0.019)
Political Attention	0.094**	-0.092***	-0.160***
	(0.033)	(0.026)	(0.033)
Social Media	-0.039*	-0.051***	-0.062**
	(0.019)	(0.012)	(0.019)
Local Newspapers	-0.002	-0.007	0.018
	(0.021)	(0.017)	(0.020)
CNN	0.000	0.027	0.037*
	(0.018)	(0.016)	(0.019)
MSNBC	0.018	0.049*	0.042
	(0.024)	(0.021)	(0.026)
Fox News	0.014	-0.003	0.017
	(0.021)	(0.015)	(0.016)
Network News	0.003	-0.008	-0.033
	(0.019)	(0.015)	(0.017)
Local TV	-0.018	-0.011	-0.025
	(0.018)	(0.014)	(0.016)
Telemundo	-0.023	-0.073*	0.084
	(0.045)	(0.037)	(0.045)
NPR	0.031	0.009	0.039
	(0.027)	(0.027)	(0.036)
AM Talk Radio	0.002	0.010	-0.013
	(0.024)	(0.018)	(0.019)
National Newspapers	0.015	0.015	-0.022
	(0.021)	(0.018)	(0.020)
Attention x Social Media	0.050	0.146***	0.060
	(0.033)	(0.027)	(0.032)
Attention x Local Newspapers	-0.048	-0.013	-0.021
	(0.031)	(0.030)	(0.030)
Attention x CNN	-0.007	-0.024	-0.018
	(0.028)	(0.029)	(0.032)
Attention x MSNBC	-0.003	-0.016	-0.004
	(0.033)	(0.035)	(0.043)
Attention x Fox News	-0.135***	-0.128***	-0.092***
	(0.032)	(0.027)	(0.025)
Attention x Network News	-0.044	-0.018	0.068*
	(0.028)	(0.028)	(0.027)

Attention x Local TV	-0.017	-0.023	0.013
	(0.027)	(0.026)	(0.025)
Attention x Telemundo	-0.079	0.061	-0.113
	(0.073)	(0.070)	(0.076)
Attention x NPR	0.033	0.084*	-0.020
	(0.035)	(0.040)	(0.053)
Attention x AM Talk Radio	-0.054	-0.113***	-0.010
	(0.039)	(0.032)	(0.029)
Attention x National Newspapers	0.025	0.044	0.029
	(0.030)	(0.030)	(0.029)
N	24093	29601	23217
R2	0.23	0.14	0.12

Notes: DID estimates using the Nationscape data files, where treatment denotes people who were between the ages of 18 and 24 during the study window. Columns represent the groups used in each model. Unit fixed effects in the TWFE specification include age group, time, and congressional district.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table S19: DID Models Estimating Feelings of Unsafety with the Police

	Democrats	Independents	Republicans
Treatment	0.185***	0.100***	0.004
	(0.025)	(0.022)	(0.019)
Gender	-0.028*	0.034***	0.034***
	(0.011)	(0.010)	(0.007)
Education: Some College	-0.034*	-0.068***	-0.034***
	(0.016)	(0.011)	(0.010)
Education: College	-0.075***	-0.072***	-0.068***
	(0.017)	(0.014)	(0.011)
Education: Post-Graduate Degree	-0.100***	-0.112***	-0.077***
	(0.021)	(0.023)	(0.014)
Political Attention	0.196***	0.080*	-0.014
	(0.056)	(0.041)	(0.033)
TV	0.010	-0.020	0.064**
	(0.031)	(0.020)	(0.023)
Newspaper	-0.036	0.028	-0.006
	(0.035)	(0.026)	(0.027)
Radio	-0.008	0.010	-0.007
	(0.035)	(0.023)	(0.022)
SM	-0.022	-0.026	0.022
	(0.047)	(0.029)	(0.026)
SM Activities: Post	0.106	0.026	0.054
	(0.065)	(0.038)	(0.032)
SM Activities: Comment	0.012	-0.012	-0.012
	(0.054)	(0.039)	(0.037)
SM Activities: Story	0.033	-0.053*	-0.002
	(0.033)	(0.022)	(0.022)
SM Activities: Event	-0.045	0.002	-0.030
	(0.051)	(0.040)	(0.035)
SM Activities: Forward	0.125*	0.073	0.018
	(0.053)	(0.037)	(0.036)
Attention x TV	-0.155***	-0.102***	-0.069*
	(0.037)	(0.028)	(0.028)
Attention x Newspaper	0.060	0.005	0.012
	(0.042)	(0.033)	(0.032)
Attention x Radio	-0.007	-0.075*	-0.005
	(0.042)	(0.031)	(0.027)
Attention x SM	-0.017	-0.006	-0.032
	(0.061)	(0.044)	(0.035)
Attention x SM Activities: Post	-0.092	0.025	-0.027
	(0.073)	(0.047)	(0.039)
Attention x SM Activities: Comment	-0.010	0.071	0.048
	(0.061)	(0.048)	(0.044)
Attention x SM Activities: Story	0.012	0.075*	0.010
	(0.042)	(0.033)	(0.028)
Attention x SM Activities: Event	0.092	0.024	0.027
	(0.058)	(0.047)	(0.041)

Attention x SM Activities: Forward	-0.087	-0.040	-0.043
	(0.060)	(0.046)	(0.043)
N	13911	18205	10376
R2	0.27	0.28	0.33

Notes: DID estimates using the CCES data, where treatment denotes survey participants who were 29 or younger during the 2020 window. Columns represent the groups used in each model. Unit fixed effects in the TWFE specification include age group, time, and county
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table S20: DID Models Adjudicating Protest Exposure in Nationscape

	Democrats	Independents	Republicans
Treatment	0.117***	0.077***	0.003
	(0.019)	(0.019)	(0.018)
Medium Protest	-0.002	-0.006	-0.001
	(0.004)	(0.004)	(0.003)
High Protest	0.004	0.007	0.011***
	(0.004)	(0.004)	(0.003)
Treatment x Medium Protest	0.005	0.034	0.025
	(0.025)	(0.025)	(0.024)
Treatment x High Protest	0.007	0.028	0.009
	(0.024)	(0.025)	(0.026)
Gender	0.010**	0.043***	0.035***
	(0.003)	(0.003)	(0.003)
Education: Some College	0.004	-0.025***	-0.015***
	(0.005)	(0.004)	(0.003)
Education: College	-0.001	-0.053***	-0.017***
	(0.005)	(0.005)	(0.004)
Education: Post-Graduate Degree	-0.008	-0.035***	-0.007
	(0.006)	(0.006)	(0.005)
Political Attention	0.065***	-0.071***	-0.132***
	(0.006)	(0.006)	(0.005)
N	88634	102275	113345
R2	0.12	0.09	0.05

Notes: DID estimates using the Nationscape data files, where treatment denotes people who were between the ages of 18 and 24 during the study window. Columns represent the groups used in each model. Unit fixed effects in the TWFE specification include age group, time, and congressional district.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table S21: DID Models Adjudicating Protest Exposure in CCES

	Democrats	Independents	Republicans
Treatment	0.152***	0.116***	0.026
	(0.040)	(0.033)	(0.027)
Medium Protest	0.013	-0.001	0.003
	(0.009)	(0.008)	(0.005)
High Protest	0.017*	0.016*	0.002
	(0.008)	(0.007)	(0.004)
Treatment x Medium Protest	0.070	-0.010	0.011
	(0.044)	(0.034)	(0.025)
Treatment x High Protest	0.025	0.003	-0.026
	(0.039)	(0.031)	(0.020)
Gender	-0.005	0.028***	0.023***
	(0.006)	(0.005)	(0.004)
Education: Some College	0.006	-0.027***	-0.020***
	(0.008)	(0.007)	(0.005)
Education: College	-0.014	-0.045***	-0.042***
	(0.008)	(0.007)	(0.005)
Education: Post-Graduate Degree	-0.007	-0.039***	-0.031***
	(0.008)	(0.007)	(0.006)
Political Attention	0.094***	0.034***	-0.042***
	(0.011)	(0.009)	(0.008)
N	41565	47821	38239
R2	0.10	0.08	0.04

Notes: DID estimates using the CCES data, where treatment denotes survey participants who were 29 or younger during the 2020 window. Columns represent the groups used in each model. Unit fixed effects in the TWFE specification include age group, time, and county.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

10 Supplemental Materials J: The Trajectory of Unfavorable Attitudes in Nationscape

Table S22 provides the full estimates for weighted averages in the Nationscape.

Table S22: The Trajectory of Unfavorable Attitudes Toward the Police

Group	Age Group	N	Time	Average	SE of Average
Democrat	18-24	96	-12	0.484	0.050
Democrat	18-24	408	-11	0.462	0.032
Democrat	18-24	387	-10	0.471	0.028
Democrat	18-24	413	-9	0.483	0.031
Democrat	18-24	450	-8	0.487	0.028
Democrat	18-24	459	-7	0.502	0.031
Democrat	18-24	480	-6	0.440	0.025
Democrat	18-24	460	-5	0.470	0.027
Democrat	18-24	540	-4	0.490	0.027
Democrat	18-24	439	-3	0.487	0.025
Democrat	18-24	433	-2	0.450	0.025
Democrat	18-24	346	-1	0.488	0.026
Democrat	18-24	405	0	0.722	0.022
Democrat	18-24	387	1	0.670	0.026
Democrat	18-24	368	2	0.691	0.025
Democrat	18-24	308	3	0.698	0.030
Democrat	18-24	330	4	0.603	0.033
Democrat	18-24	350	5	0.613	0.031
Democrat	18-24	414	6	0.658	0.026
Democrat	18-24	272	7	0.652	0.030
Democrat	25-34	192	-12	0.466	0.044
Democrat	25-34	803	-11	0.416	0.023
Democrat	25-34	892	-10	0.410	0.021
Democrat	25-34	881	-9	0.423	0.021
Democrat	25-34	847	-8	0.440	0.022
Democrat	25-34	865	-7	0.393	0.020
Democrat	25-34	831	-6	0.434	0.019
Democrat	25-34	807	-5	0.404	0.021
Democrat	25-34	716	-4	0.388	0.020
Democrat	25-34	734	-3	0.367	0.018
Democrat	25-34	757	-2	0.367	0.017
Democrat	25-34	706	-1	0.386	0.017
Democrat	25-34	744	0	0.562	0.019
Democrat	25-34	968	1	0.574	0.018
Democrat	25-34	767	2	0.539	0.020
Democrat	25-34	686	3	0.537	0.022
Democrat	25-34	819	4	0.511	0.026
Democrat	25-34	822	5	0.515	0.022

Democrat	25-34	875	6	0.459	0.020
Democrat	25-34	636	7	0.433	0.020
Democrat	35-49	271	-12	0.382	0.031
Democrat	35-49	1176	-11	0.378	0.018
Democrat	35-49	1252	-10	0.337	0.016
Democrat	35-49	1472	-9	0.328	0.016
Democrat	35-49	1292	-8	0.362	0.018
Democrat	35-49	1207	-7	0.364	0.017
Democrat	35-49	1252	-6	0.320	0.014
Democrat	35-49	1207	-5	0.332	0.016
Democrat	35-49	1110	-4	0.330	0.015
Democrat	35-49	1056	-3	0.351	0.017
Democrat	35-49	1115	-2	0.338	0.014
Democrat	35-49	1189	-1	0.320	0.013
Democrat	35-49	1202	0	0.480	0.014
Democrat	35-49	1597	1	0.453	0.013
Democrat	35-49	1328	2	0.464	0.015
Democrat	35-49	1229	3	0.455	0.017
Democrat	35-49	1174	4	0.380	0.018
Democrat	35-49	1277	5	0.351	0.016
Democrat	35-49	1613	6	0.368	0.015
Democrat	35-49	1201	7	0.354	0.015
Democrat	50-64	407	-12	0.321	0.029
Democrat	50-64	1346	-11	0.288	0.014
Democrat	50-64	1238	-10	0.285	0.015
Democrat	50-64	1256	-9	0.292	0.014
Democrat	50-64	1240	-8	0.306	0.014
Democrat	50-64	1303	-7	0.296	0.014
Democrat	50-64	1331	-6	0.298	0.013
Democrat	50-64	1266	-5	0.267	0.011
Democrat	50-64	1288	-4	0.258	0.012
Democrat	50-64	1252	-3	0.237	0.012
Democrat	50-64	1190	-2	0.259	0.012
Democrat	50-64	1243	-1	0.276	0.011
Democrat	50-64	1434	0	0.423	0.013
Democrat	50-64	1563	1	0.383	0.012
Democrat	50-64	1517	2	0.351	0.012
Democrat	50-64	1374	3	0.351	0.014
Democrat	50-64	1234	4	0.307	0.014
Democrat	50-64	1309	5	0.306	0.013
Democrat	50-64	1348	6	0.301	0.013
Democrat	50-64	1250	7	0.311	0.013
Democrat	65+	281	-12	0.224	0.023
Democrat	65+	1011	-11	0.245	0.014
Democrat	65+	856	-10	0.245	0.014
Democrat	65+	882	-9	0.242	0.014
Democrat	65+	856	-8	0.276	0.014
Democrat	65+	822	-7	0.260	0.015

Democrat	65+	1000	-6	0.225	0.011
Democrat	65+	851	-5	0.230	0.011
Democrat	65+	779	-4	0.224	0.013
Democrat	65+	891	-3	0.183	0.010
Democrat	65+	753	-2	0.176	0.010
Democrat	65+	990	-1	0.218	0.011
Democrat	65+	1130	0	0.361	0.013
Democrat	65+	1239	1	0.311	0.012
Democrat	65+	1135	2	0.289	0.011
Democrat	65+	1250	3	0.287	0.012
Democrat	65+	1063	4	0.228	0.012
Democrat	65+	994	5	0.254	0.013
Democrat	65+	1013	6	0.239	0.012
Democrat	65+	951	7	0.268	0.012
Independent	18-24	132	-12	0.461	0.051
Independent	18-24	545	-11	0.446	0.024
Independent	18-24	573	-10	0.382	0.023
Independent	18-24	642	-9	0.400	0.023
Independent	18-24	711	-8	0.461	0.024
Independent	18-24	658	-7	0.411	0.024
Independent	18-24	595	-6	0.423	0.023
Independent	18-24	671	-5	0.416	0.022
Independent	18-24	678	-4	0.393	0.022
Independent	18-24	496	-3	0.404	0.026
Independent	18-24	557	-2	0.416	0.021
Independent	18-24	341	-1	0.423	0.028
Independent	18-24	452	0	0.553	0.026
Independent	18-24	559	1	0.565	0.023
Independent	18-24	409	2	0.539	0.027
Independent	18-24	387	3	0.529	0.031
Independent	18-24	430	4	0.511	0.029
Independent	18-24	463	5	0.511	0.029
Independent	18-24	421	6	0.530	0.030
Independent	18-24	258	7	0.548	0.033
Independent	25-34	271	-12	0.344	0.031
Independent	25-34	1147	-11	0.404	0.017
Independent	25-34	1111	-10	0.372	0.018
Independent	25-34	1204	-9	0.395	0.016
Independent	25-34	1268	-8	0.423	0.018
Independent	25-34	1278	-7	0.425	0.017
Independent	25-34	1151	-6	0.420	0.016
Independent	25-34	1052	-5	0.398	0.016
Independent	25-34	965	-4	0.442	0.017
Independent	25-34	927	-3	0.381	0.017
Independent	25-34	965	-2	0.399	0.016
Independent	25-34	778	-1	0.411	0.017
Independent	25-34	1035	0	0.522	0.017
Independent	25-34	1262	1	0.468	0.015

Independent	25-34	953	2	0.452	0.017
Independent	25-34	893	3	0.453	0.019
Independent	25-34	1119	4	0.440	0.019
Independent	25-34	981	5	0.473	0.020
Independent	25-34	1065	6	0.441	0.017
Independent	25-34	701	7	0.465	0.021
Independent	35-49	440	-12	0.316	0.026
Independent	35-49	1648	-11	0.338	0.013
Independent	35-49	1702	-10	0.349	0.013
Independent	35-49	1839	-9	0.354	0.014
Independent	35-49	1789	-8	0.364	0.014
Independent	35-49	1752	-7	0.352	0.014
Independent	35-49	1761	-6	0.377	0.013
Independent	35-49	1581	-5	0.342	0.013
Independent	35-49	1537	-4	0.332	0.013
Independent	35-49	1365	-3	0.359	0.014
Independent	35-49	1391	-2	0.358	0.012
Independent	35-49	1324	-1	0.360	0.014
Independent	35-49	1548	0	0.445	0.013
Independent	35-49	1808	1	0.392	0.012
Independent	35-49	1516	2	0.366	0.013
Independent	35-49	1350	3	0.389	0.015
Independent	35-49	1353	4	0.376	0.017
Independent	35-49	1350	5	0.348	0.015
Independent	35-49	1691	6	0.343	0.013
Independent	35-49	1187	7	0.377	0.015
Independent	50-64	386	-12	0.302	0.026
Independent	50-64	1518	-11	0.271	0.014
Independent	50-64	1509	-10	0.253	0.013
Independent	50-64	1403	-9	0.296	0.013
Independent	50-64	1474	-8	0.291	0.014
Independent	50-64	1577	-7	0.291	0.012
Independent	50-64	1629	-6	0.285	0.011
Independent	50-64	1428	-5	0.277	0.011
Independent	50-64	1422	-4	0.262	0.011
Independent	50-64	1411	-3	0.277	0.013
Independent	50-64	1351	-2	0.262	0.011
Independent	50-64	1307	-1	0.298	0.011
Independent	50-64	1483	0	0.352	0.012
Independent	50-64	1708	1	0.292	0.011
Independent	50-64	1573	2	0.298	0.012
Independent	50-64	1403	3	0.281	0.012
Independent	50-64	1323	4	0.269	0.014
Independent	50-64	1443	5	0.256	0.013
Independent	50-64	1448	6	0.289	0.013
Independent	50-64	1231	7	0.247	0.012
Independent	65+	251	-12	0.237	0.030
Independent	65+	1005	-11	0.237	0.013

Independent	65+	839	-10	0.202	0.013
Independent	65+	863	-9	0.202	0.013
Independent	65+	853	-8	0.235	0.013
Independent	65+	816	-7	0.228	0.013
Independent	65+	979	-6	0.210	0.012
Independent	65+	748	-5	0.243	0.014
Independent	65+	739	-4	0.198	0.012
Independent	65+	757	-3	0.213	0.013
Independent	65+	707	-2	0.207	0.011
Independent	65+	844	-1	0.223	0.012
Independent	65+	988	0	0.301	0.013
Independent	65+	1064	1	0.253	0.013
Independent	65+	974	2	0.213	0.012
Independent	65+	973	3	0.229	0.013
Independent	65+	859	4	0.185	0.014
Independent	65+	886	5	0.204	0.013
Independent	65+	805	6	0.207	0.014
Independent	65+	817	7	0.209	0.012
Republican	18-24	62	-12	0.173	0.056
Republican	18-24	369	-11	0.259	0.028
Republican	18-24	327	-10	0.274	0.032
Republican	18-24	404	-9	0.251	0.032
Republican	18-24	457	-8	0.238	0.024
Republican	18-24	480	-7	0.239	0.025
Republican	18-24	440	-6	0.239	0.024
Republican	18-24	462	-5	0.242	0.024
Republican	18-24	490	-4	0.257	0.026
Republican	18-24	387	-3	0.209	0.021
Republican	18-24	491	-2	0.202	0.019
Republican	18-24	295	-1	0.236	0.025
Republican	18-24	291	0	0.316	0.030
Republican	18-24	362	1	0.295	0.028
Republican	18-24	342	2	0.273	0.028
Republican	18-24	263	3	0.285	0.034
Republican	18-24	269	4	0.169	0.023
Republican	18-24	325	5	0.268	0.031
Republican	18-24	362	6	0.168	0.022
Republican	18-24	216	7	0.201	0.028
Republican	25-34	159	-12	0.211	0.034
Republican	25-34	860	-11	0.233	0.019
Republican	25-34	903	-10	0.267	0.019
Republican	25-34	914	-9	0.252	0.020
Republican	25-34	929	-8	0.265	0.020
Republican	25-34	984	-7	0.241	0.018
Republican	25-34	877	-6	0.255	0.017
Republican	25-34	827	-5	0.229	0.017
Republican	25-34	807	-4	0.248	0.018
Republican	25-34	798	-3	0.264	0.017

Republican	25-34	855	-2	0.249	0.015
Republican	25-34	754	-1	0.263	0.017
Republican	25-34	819	0	0.301	0.017
Republican	25-34	1012	1	0.249	0.016
Republican	25-34	851	2	0.252	0.017
Republican	25-34	788	3	0.254	0.019
Republican	25-34	928	4	0.228	0.017
Republican	25-34	887	5	0.268	0.019
Republican	25-34	900	6	0.258	0.018
Republican	25-34	546	7	0.265	0.021
Republican	35-49	361	-12	0.235	0.028
Republican	35-49	1487	-11	0.204	0.013
Republican	35-49	1506	-10	0.192	0.012
Republican	35-49	1833	-9	0.248	0.014
Republican	35-49	1624	-8	0.216	0.013
Republican	35-49	1724	-7	0.212	0.012
Republican	35-49	1672	-6	0.204	0.011
Republican	35-49	1613	-5	0.222	0.012
Republican	35-49	1636	-4	0.191	0.010
Republican	35-49	1558	-3	0.202	0.010
Republican	35-49	1602	-2	0.203	0.008
Republican	35-49	1647	-1	0.228	0.010
Republican	35-49	1691	0	0.256	0.011
Republican	35-49	2100	1	0.219	0.010
Republican	35-49	1772	2	0.210	0.011
Republican	35-49	1633	3	0.240	0.013
Republican	35-49	1654	4	0.204	0.013
Republican	35-49	1731	5	0.213	0.012
Republican	35-49	1659	6	0.228	0.013
Republican	35-49	1128	7	0.217	0.012
Republican	50-64	568	-12	0.186	0.019
Republican	50-64	1937	-11	0.153	0.009
Republican	50-64	1872	-10	0.165	0.010
Republican	50-64	1702	-9	0.189	0.011
Republican	50-64	1817	-8	0.186	0.011
Republican	50-64	1877	-7	0.175	0.011
Republican	50-64	2031	-6	0.172	0.009
Republican	50-64	1913	-5	0.182	0.009
Republican	50-64	2014	-4	0.166	0.009
Republican	50-64	1853	-3	0.160	0.008
Republican	50-64	1899	-2	0.176	0.008
Republican	50-64	1802	-1	0.170	0.008
Republican	50-64	2008	0	0.242	0.010
Republican	50-64	2233	1	0.172	0.008
Republican	50-64	2168	2	0.161	0.008
Republican	50-64	1963	3	0.149	0.009
Republican	50-64	1870	4	0.161	0.011
Republican	50-64	2038	5	0.135	0.008

Republican	50-64	1814	6	0.156	0.009
Republican	50-64	1638	7	0.162	0.011
Republican	65+	339	-12	0.125	0.014
Republican	65+	1354	-11	0.129	0.010
Republican	65+	1107	-10	0.133	0.011
Republican	65+	1164	-9	0.120	0.008
Republican	65+	1210	-8	0.147	0.010
Republican	65+	1199	-7	0.140	0.010
Republican	65+	1374	-6	0.128	0.009
Republican	65+	1215	-5	0.123	0.008
Republican	65+	1161	-4	0.133	0.009
Republican	65+	1179	-3	0.127	0.009
Republican	65+	1122	-2	0.133	0.009
Republican	65+	1272	-1	0.143	0.009
Republican	65+	1447	0	0.194	0.010
Republican	65+	1582	1	0.117	0.008
Republican	65+	1460	2	0.115	0.008
Republican	65+	1471	3	0.102	0.007
Republican	65+	1279	4	0.094	0.008
Republican	65+	1212	5	0.103	0.009
Republican	65+	1231	6	0.116	0.010
Republican	65+	1157	7	0.101	0.009

11 Supplemental Materials K: Event-Study Estimates Across Policy Attitudes

Figure S9 shows dynamic difference-in-differences models for 14 policy items. The results support the interpretation that the main findings reflect a specific response to the Floyd event rather than a general tendency for younger respondents to diverge from older respondents over this period.

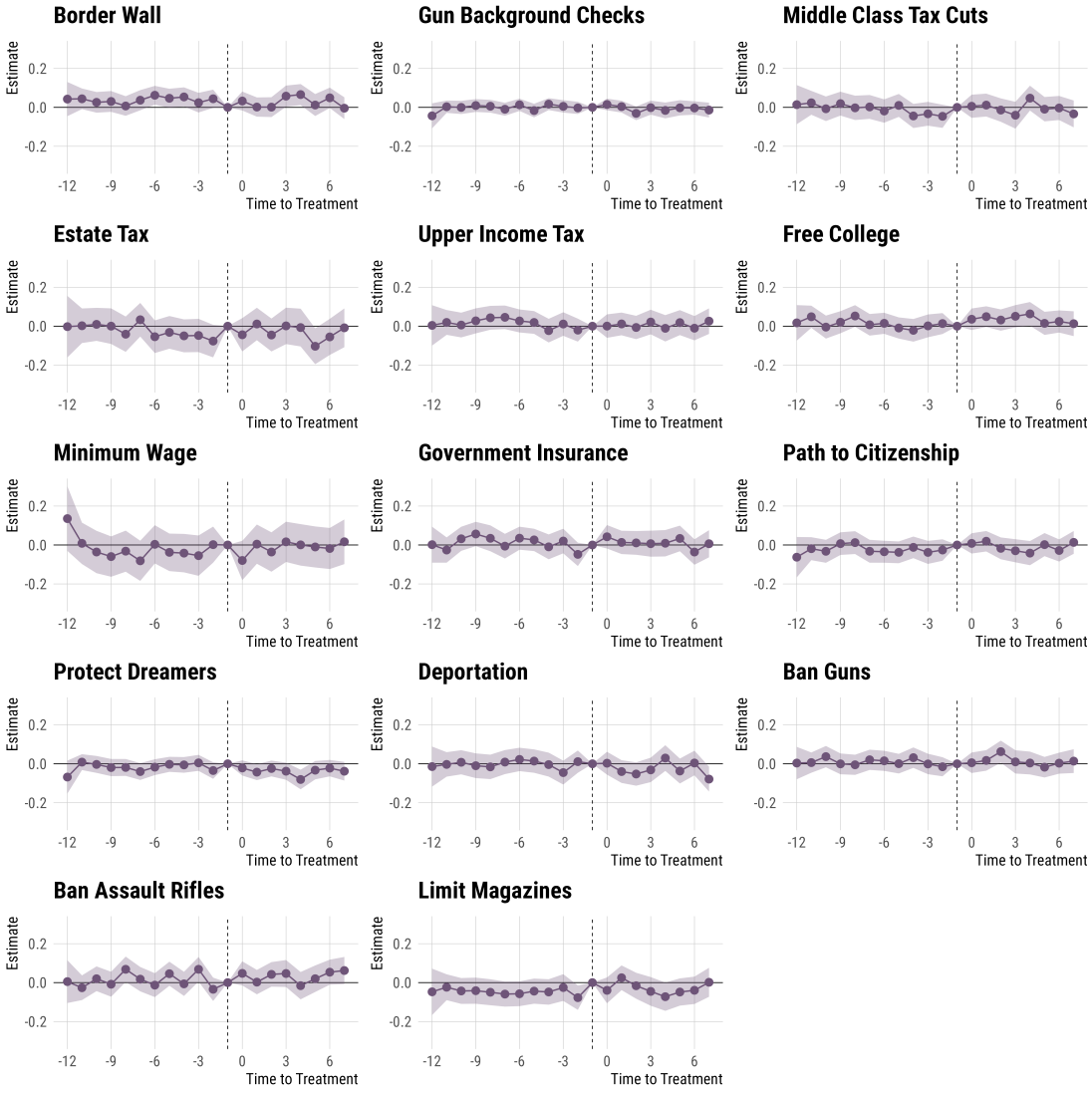

Figure S9: Event-Study Estimates Across Policy Attitudes in Nationscape

Notes: Each panel presents event-study estimates from a dynamic difference-in-differences model comparing 18–24-year-olds to older respondents, using the same specification as the main analyses. The dependent variable in each panel is a policy attitude item from the Nationscape 2019–2020 data file.

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