Causal Inference in Contentious Politics

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Abstract

This essay-style dissertation uses quasi-experimental designs on large-scale data to study three episodes of contention in which a highly visible political event reshaped political behavior or expressed attitudes. The chapters cover gun politics in the United States, immigration politics in Germany, and group attitudes around the George Floyd protest wave. Read together, they argue that observable political shifts during such episodes most often come not from changes in what citizens believe but from already-held predispositions becoming politically consequential under specific organizational, electoral, or normative conditions.

The first chapter applies a staggered difference-in-differences design to address-level data on roughly 225,000 donations to the National Rifle Association and finds that fatal school shootings produce durable counter-mobilization among gun-rights supporters, helping explain why gun regulation in the United States remains elusive despite recurrent public outrage. The second, co-authored with Sascha Riaz, exploits the seasonality of German outdoor pools and finds that exposure to ethnic diversity in a politicized public space raises support for the far-right *Alternative für Deutschland* by heightening the salience of immigration rather than by changing underlying immigration attitudes. The third applies a regression discontinuity in time around the start of the nationally visible George Floyd protest wave to around 500,000 Nationscape interviews and finds that expressed favorability rose not only toward Black Americans but also toward other minority groups not directly centered in the protests, including Asians, Latinx individuals, Muslims, and the LGBTQ community, with shifts concentrated in Democratic-leaning counties.

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sharp minds gathered there, writing this dissertation would have been substantially more difficult. Essentially all of the major ideas in the chapters that follow were first pitched to someone over a warm meal in the Nuffield College dining hall. Just as importantly, many ideas that had were flawed or simply mediocre died right then and there, allowing me to fail often and early.

The chapters presented in this dissertation have, in one form or another, been presented to audiences at various places, including American University, the EUI, Humboldt University of Berlin, Nuffield College, NYU Abu Dhabi, the University of Oxford, the University of Southern California, UT Austin, and WZB Berlin. I thank all of these audiences for their helpful feedback and suggestions.

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Introduction

Political life is punctuated by episodes of contention. A fatal school shooting, a national protest wave, or a politicized intergroup encounter can disrupt the routine relationship between citizens and politics and reshape, often quickly, what people do, how they vote, and what they feel comfortable saying in public. Political-science scholarship has long had a name for this register of politics: *contentious politics* (McAdam et al., 2001; Tarrow, 2011). Most empirical research on public opinion and political behavior, however, has been built on data from routine moments instead, such as regularly fielded surveys, scheduled elections, and policy debates within stable issue areas. This dissertation studies citizen-level political behavior in three contentious episodes and asks what changes when the conditions of public life are no longer ordinary.

Contentious moments matter for democratic politics for at least three reasons. First, much of what is observed as large-scale movement in public opinion takes place during them. Substantial shifts in expressed attitudes, vote intention, and costly political behavior tend to cluster around events rather than to accumulate slowly between them (Birkland, 1998; Kingdon, 1984; Zaller, 1992; Druckman and Leeper, 2012). Second, the asymmetries that channel citizen preferences into politics—which constituencies mobilize, which issues acquire electoral weight, which views can be voiced without social cost—can shift quickly during such episodes, sometimes in ways that persist long after the event itself (Meyer and Staggenborg, 1996; Patashnik, 2023; Tankard and Paluck, 2016). Third, contentious moments are the register of politics in which large observable shifts in citizen behavior are most likely to be read by political actors as evidence about what citizens want (Wouters and Walgrave, 2017).

The empirical chapters take up three contentious episodes that differ in country, substance, and form. Chapter 1 studies how fatal school shootings in the United States activate organized counter-mobilization by gun-rights supporters, in ways that help explain why public outrage about gun violence routinely fails to produce gun regulation. Chapter 2 turns to Germany, where the far-right has

turned a mundane public space, public swimming pools, into a recurring electoral cue for the *Alternative für Deutschland*. Chapter 3 returns to the United States to examine the protest wave that followed the murder of George Floyd, asking whether it shifted expressed attitudes not only toward Black Americans but toward several adjacent marginalized groups not directly named by the event. The three episodes look somewhat different on the surface. However, taken together, they show that political behavior during contention is shaped less by the formation of new preferences than by the specific organizational, electoral, and normative conditions under which existing political material acquires force.

The dissertation defends three claims that the chapters together support. First, apparent shifts in public opinion during contentious moments more often reflect existing political material—organized identity, latent issue weight, prevailing normative environments around expression—becoming politically consequential than the formation of new preferences. Second, the same event can travel through different mechanisms, depending on the surrounding political infrastructure: organized mobilization, electoral salience, or shifts in normative permission. Third, the conditions under which contention turns predispositions into political voice are unevenly distributed across constituencies, attitudes, and local environments, producing inequalities in democratic representation that originate upstream of the moment of policy response.

Citizens during contentious moments

The dissertation builds on three bodies of research that have studied citizens during contentious moments, even where they have not used the same vocabulary. Each chapter sits primarily within one of them. Read together, they sketch a more general picture of how political behavior changes during episodes of contention.

The first is the contentious-politics tradition itself, which has long emphasized that political conflict outside the institutional routine is not just an aggregation of individual choices but a process structured by organizational infrastructures, repertoires of action, and political opportunities (Tarrow, 2011; McAdam et al., 2001; Jenkins, 1983). Within this tradition, movement and counter-movement scholarship has shown that such episodes typically activate both sides of a political

contest at once, and that asymmetries in organizational capacity shape whose response endures and at what cost (Meyer and Staggenborg, 1996; Patashnik, 2023). Adjacent work on focusing events has shown that rare, high-salience events restructure political agendas and create temporary policy windows (Birkland, 1998; Kingdon, 1984; Baumgartner and Jones, 1993; Jones and Baumgartner, 2005; Birkland and Schwaeble, 2019). American gun politics is the paradigm case for this strand. A tightly organized minority has long sustained policy positions that diffuse survey majorities oppose (Goss, 2006; Lacombe, 2021). Chapter 1 sits in this literature.

The second is research on public opinion under threat, salience, and shock. Citizens hold many views but do not weight them equally in every choice or at every moment. Salience and accessibility shape which considerations enter political evaluation (McCombs and Shaw, 1972; Iyengar and Kinder, 1987; Zaller, 1992), and contentious moments frequently operate by raising the salience of an existing concern without altering the concern itself (Stenner, 2005; Marcus et al., 2000; Albertson and Gadarian, 2015; Mutz, 2018; Chong and Druckman, 2007). In the immigration politics literature in particular, the salience of immigration is a documented driver of far-right support, often more strongly than any short-run movement in immigration attitudes themselves, which tend to be stable over the life course (Ivarsflaten, 2008; Dennison, 2020; Kriesi et al., 2012; Kustov et al., 2021). The contact-theory literature has likewise moved toward a view in which the political context surrounding intergroup exposure determines whether contact reduces or galvanizes prejudice (Allport, 1954; Pettigrew and Tropp, 2006; Paluck et al., 2019; Hopkins, 2010; Enos, 2014; Dinas et al., 2019; Hangartner et al., 2019; Dancygier, 2010, 2017, 2024). Chapter 2 sits at the intersection of these literatures, in a setting where everyday contact has been turned into a recurring contentious cue by political actors.

The third is research on public norms and expression during high-salience events. Citizens do not voice attitudes in a vacuum. They infer from their environment what others believe and what can be said without social cost (Noelle-Neumann, 1984; Kuran, 1995; Bicchieri, 2006). Visible public signals from elites, institutions, protests, and the media can rapidly alter perceived norms, sometimes producing fast changes in expressed attitudes even when private views are slower

to move (Tankard and Paluck, 2016, 2017; Arias, 2019; Clayton et al., 2021; Alvarez-Benjumea, 2022; Bursztyn et al., 2020; Valentim, 2024). Studies of major protest waves in particular have documented their capacity to move the political environment around groups and issues, with consequences for downstream opinion and behavior (Gillion, 2012; Wasow, 2020; Reny and Newman, 2021; Holbein and Hassell, 2023). Chapter 3 sits in this literature.

Argument

The argument the dissertation defends has three parts.

First, citizens engaging with politics during contentious moments often appear to change their minds. The empirical record of the chapters, however, is more consistent with a related but distinct family of readings. Contentious moments convert what citizens already bring to them, whether organized identity, latent issue weight, or established normative environments around expression, into observable political behavior, without requiring corresponding movement in underlying preferences (Zaller, 1992; Kustov et al., 2021; Sears and Funk, 1999). In Chapter 1, NRA donations rise after school shootings because gun ownership is, for gun-rights supporters, a long-standing political identity rather than a newly formed view. In Chapter 2, AfD support rises during the outdoor pool season because the season raises the electoral weight of immigration attitudes that respondents already held. Underlying immigration attitudes themselves do not move. In Chapter 3, expressed favorability rises across several marginalized groups in the days after Floyd's murder. The chapter does not directly observe private beliefs, but the joint pattern—a shift across five different groups at the same cutoff, concentrated where the local environment reinforced the national signal—is more plausibly read as a change in the normative environment around expression than as a simultaneous change in private beliefs about five distinct groups within a single week. Reading these patterns as preference change is the natural first move. In each case, the chapter's empirical strategy is designed to make the alternative reading visible and at least as defensible.

Second, contentious moments do not all travel through the same mechanism. The chapters illustrate three different routes. School shootings activate gun-rights

supporters by raising the perceived threat of new regulation; a standing organizational infrastructure already in place absorbs that response and converts it into costly, sustained participation. Politicized intergroup encounters in public pools raise the electoral salience of immigration in localities where ethnic diversity makes the cue meaningful. The Floyd protest wave operates through changes in the normative environment around expression, mediated by the local context in which the national signal is received. These routes differ in the cost of the response, in its temporal profile, and in the populations among which the response is concentrated. The dissertation does not test the three routes against one another in a single design. Each chapter is best understood through the route most relevant to its case, and the three are offered as a lens for reading the chapters together rather than as a competition between them.

Third, contentious moments reshape political voice unevenly. Where the structures that channel contention into political consequence favor one side of a contest, through organizational density, ownership of a salient issue, or alignment with prevailing public norms, comparable distributions of underlying preference can produce sharply unequal political voice. The cases examined here are not a representative sample of contemporary contention. They do, however, suggest that the inequalities the responsiveness literature documents downstream may have important antecedents upstream, in the differential capacity of constituencies, attitudes, and local environments to convert contentious moments into political force.

Contributions

The dissertation makes three distinct contributions.

The first is substantive. Inequality in democratic voice begins upstream of the moment of policy responsiveness, in the differential capacity of constituencies to convert episodes of contention into observable political consequence. The chapters offer three concrete instances: a constituency that converts threat from gun control legislation into durable, organized political pressure (Chapter 1); a stable anti-immigrant attitude that becomes electorally consequential when a politicized public setting brings the immigration issue to the surface (Chapter 2); a public-

expression environment that responds quickly to a national signal in some places and not others (Chapter 3). In each case, similarly distributed preferences yield very different distributions of political consequence because the conditions of contention favor one side of the contest.

The second is conceptual. The dissertation bridges the contentious-politics tradition with empirical research on public opinion and political behavior. Most public-opinion work has studied citizens during routine times. Most contentious-politics work has studied movement-level dynamics rather than citizen-level response. By examining citizen-level political behavior in these moments using quasi-experimental designs and large-scale data, the dissertation contributes to both literatures and suggests how each could be more attentive to the other. The chapters share an interpretive move. What looks like rapid opinion change during a contentious moment is more often the conversion of existing political material into observable behavior under the specific organizational, electoral, or normative conditions the moment makes available. Reading these shifts as preference change misclassifies the kind of change that has occurred, and therefore mispredicts who is moving, for how long, and to what political end.

The third is empirical. The chapters pair quasi-experimental designs with large-scale administrative or panel data (address-level donation records, a high-frequency German voter panel, and weekly American survey waves) to identify the political consequences of these episodes at temporal and geographic resolutions fine enough to distinguish among readings. None of these designs are necessarily methodologically novel by themselves. What is new, however, is bringing them to bear, in combination, on a class of questions about political behavior during contention that have most often been treated descriptively. In that regard, the designs share a structural feature. Each exploits a feature of the episode (its date, location, or recurring cycle) to compare the same underlying material under different environmental conditions. That feature is what licenses the dissertation's reading of each chapter, as existing political material becoming politically consequential under specific conditions rather than as a movement in underlying preference, and what makes the cases comparable across the dissertation.

The dissertation is also explicit about what it specifically does not claim. It does not adjudicate among the three routes in a single research design. It also neither measures long-run preference change nor does it measure downstream policy outcomes. It also does not argue that contention is normatively desirable or undesirable in itself. Contention is a recurring feature of democratic politics. The unequal distribution of the conditions under which contention turns dispositions into political consequence is a politically consequential fact that the dissertation aims to name precisely rather than to evaluate.

Research design and scope

This is an essay-style dissertation. The three empirical chapters are self-contained papers, each written to contribute to its own substantive literature. The cases (gun politics in the United States, immigration politics in Germany, and racial and group attitudes in the United States) were not selected for cross-national comparison. They were selected because each offers a setting in which a contentious episode has a clear date or recurring cycle, the relevant dispositions or normative environments plausibly predate the episode, and a quasi-experimental design can credibly recover the effect of the episode on political behavior. That structure is what licenses the dissertation's reading of each case. The cost of the strategy is that domain-specific institutions, histories, and prior literatures are developed within the chapters rather than synthesized across them.

Each chapter applies a quasi-experimental design matched to the episode it studies. Chapter 1 uses a staggered difference-in-differences estimator around the locations and timing of fatal school shootings (Callaway and Sant'Anna, 2021). Chapter 2, for its main result, uses a difference-in-differences design that exploits the seasonality of German outdoor pools. Chapter 3 uses a regression discontinuity in time around the moment at which the public reaction to George Floyd's murder became nationally visible. In each case, the design exploits a feature of the episode that allows the kind of before-after or treated-untreated comparison the dissertation's reading requires.

These designs are well suited to the dissertation's central claims, but they also define the scope of inference. The strongest causal claims concern the existence, di-

rection, and short-run timing of shifts in donations, vote intentions, and expressed favorability within each chapter's observation window. Claims about the mechanism through which these shifts operate are supported by theory and auxiliary evidence (patterns across subgroups, secondary outcomes, placebo cases, and contexts in which the episode is absent), but the chapters do not, and by design cannot, separately identify every step in the causal chain. The aim is to show that the observed patterns are best read as existing political material becoming politically consequential during these moments, not to manipulate every link in the causal chain experimentally.

Three studies of contentious politics

The three empirical chapters take up three episodes of contention that differ in the form contention takes. Chapter 1 studies an event of violence, a fatal school shooting that triggers organized counter-mobilization. Chapter 2 studies a recurring cycle, the politicization of a mundane intergroup setting during a seasonal cue. Chapter 3 studies the paradigmatic episode, a national protest wave following a high-salience act of violence. Across the three, the form of contention varies, but the analytical question is the same: what happens to political behavior when the moment arrives, and how should it be read?

Chapter 1: School Shootings Increase NRA Donations

The puzzle that motivates the first chapter is among the most-studied in American politics: why gun regulation in the United States remains elusive despite recurring tragedies and majority public support for stricter laws. School shootings reliably shift average public opinion toward stricter gun regulation (Newman and Hartman, 2019; Hartman and Newman, 2019; García-Montoya et al., 2022) and prompt the introduction of new gun-control bills in state and federal legislatures (Schildkraut and Hernandez, 2014; Watson, 2022). The standard reading of these patterns treats school shootings as focusing events that open temporary policy windows for legislative change (Birkland, 1998; Kingdon, 1984; Baumgartner and Jones, 1993). Nonetheless, in the vast majority of cases, substantive policy reform does not follow, and state gun law occasionally moves in the opposite direction

after high-profile events (Luca et al., 2020; Goss, 2006). The windows open, in other words, but tend to close again with little to show for the opening.

The chapter focuses on a side of this contest that has received less attention. Gun-rights supporters in the United States are an unusually mobilizable constituency. For a substantial share of American gun owners, gun ownership is a core political identity rather than a hobby or security choice (Lacombe, 2021), and the National Rifle Association maintains a dense organizational infrastructure of state affiliates, magazines, and direct-mail networks that converts moments of perceived threat into political action (Spitzer, 2020; Lacombe, 2021; Goss, 2006). Gun-control supporters have not assembled a comparable infrastructure. The contentious-politics literature on movement and counter-movement suggests that, in such configurations, the same external shock that mobilizes one side can mobilize the other more durably, and the better-organized side can extract more political consequence from the same event (Meyer and Staggenborg, 1996; Patashnik, 2023). The chapter asks whether, and how durably, school shootings activate this gun-rights counter-mobilization, and what that implies for the legislative aftermath of focusing events.

The empirical analysis combines address-level data on roughly 225,000 individual donations to the NRA's Political Victory Fund between 2000 and 2022, obtained from the Federal Election Commission, with county-level data on the locations of fatal school shootings. Using a staggered difference-in-differences design (Callaway and Sant'Anna, 2021), the chapter estimates the effect of a county's first fatal school shooting on real donation amounts and unique donor counts at the county-year level. Identification rests on the assumption that the precise location and timing of fatal school shootings are plausibly as-if-random conditional on county and year fixed effects (García-Montoya et al., 2022). The chapter probes this assumption through alternative shooting datasets, alternative control groups, county-specific time trends, and alternative staggered estimators. Standard errors are clustered by county throughout.

The chapter finds that donations to the NRA rise by approximately 30% on the intensive margin and 40% on the extensive margin in the years following a fatal school shooting. These increases persist over several years rather than fading with the news cycle. Effects are larger in states with permissive baseline regulation,

where the marginal threat from new restrictions is greater, but do not scale strictly with fatality counts, consistent with even relatively small incidents being able to raise the local threat of new regulation. A placebo using donations to the anti-abortion National Right to Life Committee shows that the pattern is not a generic post-tragedy surge in conservative giving. A parallel analysis of donations to the Giffords PAC finds no comparable mobilization on the gun-control side. Crucially, a complementary analysis of NRA advertising on Meta finds no spike in spending or impressions in affected states in the weeks after a shooting, consistent with the existing infrastructure doing the work rather than a localized post-shooting fundraising surge. Supplementary analyses of Cooperative Election Study data indicate that gun owners also increase non-electoral political participation more generally in the immediate aftermath of local shootings, suggesting that the donation response is part of a broader bundle of participation rather than a narrow fundraising artifact.

Taken together, the findings document a single dynamic. A constituency for whom gun ownership is a stable political identity converts an episode of violence into costly, sustained participation through a standing organization. The chapter's contribution is to show that counter-mobilization following focusing events can be durable, that durability depends on standing organizational infrastructure rather than on episodic fundraising, and that the asymmetry between mobilization and counter-mobilization in the gun politics case is large enough to help explain why public windows of opportunity for gun regulation routinely close without substantive change. In a slightly modified form, this chapter appeared in *Science Advances* 9(51). It has also been featured in *Science* 382(6677), and was selected as *Science Advances* Editors' Pick in 2024.

Chapter 2: Racial Threat in Public Social Spaces

Whether and how intergroup exposure shapes political attitudes is one of the longest-running questions in the political behavior literature. Classical contact theory holds that, under the right conditions, repeated interaction across group lines can reduce prejudice (Allport, 1954; Pettigrew and Tropp, 2006; Paluck et al., 2019). A more recent literature has shown that the political effects of intergroup exposure are highly contingent on the context in which exposure occurs (Hop-

kins, 2010; Enos, 2014; Dinas et al., 2019; Hangartner et al., 2019; Dancygier, 2010, 2017, 2024). Where exposure occurs in settings that political actors have already framed as threatening, exposure appears to galvanize rather than mute out-group prejudice. The contingency has reopened the question of how, where, and when intergroup contact moves political behavior, and has shifted attention from the existence of exposure effects to the political conditions under which they emerge.

Chapter 2, co-authored with Sascha Riaz, studies the political consequences of ethnic diversity in German public swimming pools, a public social space that far-right political actors have turned into a recurring site of contention. Public pools are highly frequented social spaces in Germany that bring together a representative cross-section of native and immigrant populations, and that have, in recent years, been the subject of an explicit far-right campaign linking diversity to public safety. The far-right *Alternative für Deutschland* (AfD) campaigns on diversity in pools as a threat to public safety, and the corresponding narrative is reproduced in local and national media coverage of pool-related incidents. The setting is not classical contentious politics in the sense of street protest or organized movement, but it is contentious in a different and increasingly common sense. A mundane public setting has been turned into a recurring site of political conflict by repeated political and media framing. The chapter asks whether repeated exposure to ethnic diversity in this politicized setting raises support for the AfD, and through which mechanism.

The theoretical argument is that exposure in politicized public spaces need not move underlying immigration attitudes, which prior research has shown to be highly stable over the life course (Kustov et al., 2021). Rather, exposure raises the perceived salience of immigration relative to other political issues, so that voters with pre-existing anti-immigrant attitudes attach greater weight to those attitudes when choosing between parties (Kriesi et al., 2012; Dennison, 2020; Ivarsflaten, 2008; Fitzgerald et al., 2012; Ward, 2019). The chapter therefore sits at the intersection of the contact-theory literature on the political conditions of exposure and the spatial-voting literature on the electoral consequences of issue-salience shifts for far-right support.

The empirical analysis combines the geolocation of all 4,669 public swimming pools in Germany with a high-frequency panel survey of about 240,000 eligible

voters interviewed between January 2017 and December 2018. Using a difference-in-differences design that exploits the seasonality of outdoor pools, the chapter compares changes in far-right support during the outdoor pool season among respondents in localities with an outdoor pool to respondents in localities without one. Identification relies on changes within the same respondent over time and accommodates all time-invariant differences across localities and all time-varying shocks that affect both groups equally. The parallel-trends assumption is probed through pre-trend tests, a placebo using indoor pools (which are open year-round), checks for the presence of other seasonal public spaces, and an external-validity test against precinct-level federal election returns in Berlin and Hamburg.

The chapter finds that AfD support rises by approximately 0.8 percentage points in ethnically diverse localities during the outdoor pool season and recedes after the season ends. The effect is concentrated in high-diversity localities. We find no comparable effect in low-diversity localities or for indoor pools. A parallel mechanism analysis on a separate panel survey of more than 30,000 respondents shows that exposure raises the perceived salience of immigration relative to economic issues without shifting underlying immigration attitudes themselves. This is the result that the salience interpretation predicts and the preference-change interpretation cannot accommodate. A novel survey of more than 3,000 respondents fielded during the 2024 outdoor pool season corroborates the political narrative linking pools to threat perceptions. Most respondents have never personally witnessed a crime in a pool, yet the large majority have heard about such incidents in the media, and respondents who have are significantly more likely to express threat perceptions.

Taken together, the findings document a different dynamic from Chapter 1. A stable anti-immigrant disposition acquires greater weight in vote choice when a politicized public setting brings the immigration issue to the surface, without underlying immigration attitudes themselves moving. The chapter's contribution is to identify a class of intergroup-exposure effects that operate through issue salience rather than through preference change; to show that the political effects of intergroup contact depend strongly on whether the relevant setting has been politicized, a moderating role that helps organize the apparent disagreement between earlier studies finding prejudice-reducing and prejudice-galvanizing effects

of exposure; and to provide a quasi-experimental estimate at a scale that covers an entire major European economy. A version of this chapter is forthcoming in the *Journal of Politics*.

Chapter 3: Public Norms and Cross-Group Spillovers

When a highly visible contentious episode shifts public discussion about prejudice against one group, do its effects stop at that group, or do they spill over to expressed attitudes toward other marginalized groups? Most work on the political effects of high-salience violence and protest has studied one group at a time (Reny and Newman, 2021; Gillion, 2012; Wasow, 2020; Holbein and Hassell, 2023). The third chapter asks whether the public reaction to George Floyd's murder in May 2020—one of the largest protest waves in recent American history (Buchanan et al., 2020; Smith et al., 2020)—shifted expressed attitudes toward minority groups well beyond the one directly at the center of the event.

The premise of the chapter is that public arguments about prejudice and the legitimacy of public expression do not address single groups in isolation. Citizens infer from their social environment what others believe and what can be said without social cost (Noelle-Neumann, 1984; Kuran, 1995; Bicchieri, 2006), and visible public signals from elites, institutions, protests, and the media can rapidly alter perceived norms, sometimes producing fast changes in expressed attitudes even when private views are slower to move (Tankard and Paluck, 2016, 2017; Arias, 2019; Clayton et al., 2021; Alvarez-Benjumea, 2022; Bursztyn et al., 2020; Valentim, 2024). When a public signal is unusually visible and framed in general terms rather than as a single-incident grievance, it can be read as a signal about the acceptability of prejudice more broadly, and therefore travel across group boundaries (Nolan, 2011). The public response to Floyd's murder was unusually broad. The protests were geographically widespread, drew wide public support, and were accompanied by public condemnations of racism from companies, universities, and major public-facing institutions. The cross-group framing was also made explicit at the time by protest organizers and adjacent identity-group organizations.

I examine these questions using Nationscape, a large repeated cross-section fielded in weekly waves between July 2019 and February 2021 (Tausanovitch et al., 2019), with roughly 500,000 interviews overall. The empirical design is a regres-

sion discontinuity in time around May 28, 2020, the date at which public attention to the protest wave rose sharply across Google search, television news, and newspaper data. I estimate the discontinuity in expressed group favorability at the cutoff using local linear regressions, triangular kernels, MSE-optimal bandwidths, and robust bias-corrected inference. The choice of cutoff and identification strategy follow recent work that treats Floyd's murder as an unexpected regression-discontinuity-in-time shock for attitudes toward Black Americans and the police (Reny and Newman, 2021). Going beyond that direct-effect analysis, I extend the design to five group-favorability outcomes: Black Americans, Asians, Latinx individuals, Muslims, and the LGBTQ community.

Expressed favorability rises at the cutoff toward all five groups, with discontinuities ranging from 0.05 to 0.12 points on a four-point scale. Placebo outcomes (favorability toward Whites and Socialists, ideology more generally, and demographic variables that should not change at the cutoff) show no comparable movement, which argues against a generalized positive mood, a leftward ideological shift, or compositional change in the survey sample around the cutoff. The pattern is concentrated in Democratic-leaning counties, and within those counties the shift among Republican respondents is at least as large as among Democrats. This contextual heterogeneity is consistent with the local environment reinforcing the national signal where the signal is socially legible, and it pushes against an alternative reading in which only White liberals shifted in response to the salience of White-perpetrated violence. The estimates are stable across alternative cutoff dates within the May 25–28 window, alternative bandwidths from half to twice the optimal value, leave-one-state-out checks, and a placebo cutoff at the date of the *Bostock v. Clayton County* decision.

The chapter is explicit that the design is reduced-form. It cannot adjudicate among the closely related channels through which a broad public signal might operate: updated perceptions of what others consider acceptable, increased accessibility of anti-prejudice considerations at the moment of survey response, or the framing of marginalized groups as objects of a common anti-discrimination norm. The chapter does not claim that private beliefs durably changed. The outcomes are explicit survey evaluations, and the result speaks to short-run expressed favorability rather than to deeper preference change. What the design does establish is that

expressed favorability moved across several group boundaries at the same cutoff and in the same direction, and that the pattern is shaped by the local environment in the way a public-norm reading would expect.

Taken together, the findings document a third dynamic, distinct from the first two. A public signal during such an episode alters the expressive environment within which group attitudes are voiced, and the effect spills beyond the group directly at the center of the event. The chapter's contribution is to show that the consequences of a major protest wave reach attitudes well beyond the focal group; that these spillovers travel furthest where the local environment reinforces the national signal; and that work on the political consequences of high-salience violence and protest must therefore consider outcomes beyond the focal group as part of its standard analytical object.

The remainder of the dissertation proceeds as follows. Chapter 1 examines school shootings and NRA donations in the United States. Chapter 2 examines the political effects of intergroup encounter in German public pools. Chapter 3 examines cross-group spillovers in the aftermath of the George Floyd protest wave. The conclusion draws the three chapters together and reflects on what they imply for the study of contentious politics and democratic representation. The end of the dissertation also contains appendices for all three empirical chapters.

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Chapter 1

School Shootings Increase NRA Donations*

Tobias Roemer†



Abstract

The United States has experienced a tragic increase in school shootings in recent years. Despite substantial media attention being paid to such incidents and widespread support for stricter gun-control measures, substantial legislative action remains absent, however. To make sense of this puzzle, this chapter focuses on the mobilizing responses of gun-rights supporters in the aftermath of such events. Using granular data from 225,000 donations to the NRA's political action committee in a difference-in-differences design, this chapter provides causal estimates showing substantial increases in both donation amounts and donor numbers after school shootings. These results provide insight into the counter-mobilizing responses of the pro-gun political right after shootings. In contrast to the typically transient nature of gun-control movements, these counter-mobilizing responses are notably durable. The results of this chapter have important implications for our understanding of the complexities of, and obstacles to, gun regulation in the United States.

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1 Introduction

The US has experienced a tragic increase in school shootings in recent years (Teasley, 2018; Kalesan et al., 2016). Between 1995 and 2021, shootings on campuses of US schools have resulted in over 300 deaths and touched on the lives of more than 240,000 students (Levine and McKnight, 2021; Rossin-Slater et al., 2020), thrusting the issue into the forefront of public concern and sparking strong debates about the need for stricter gun regulation in the US (Fox et al., 2022; Brock and Routon, 2020). Policy proposals are often aimed at reducing access to firearms and increasing background checks for potential gun owners (Brock and Routon, 2020; Hartman and Newman, 2019).

Previous scholarship on the consequences of school (and mass) shootings has found that citizens, on average, tend to become more left-wing in their attitudes toward gun regulation and voting behavior after shootings (Hartman and Newman, 2019; Newman and Hartman, 2019; Yousaf, 2021; García-Montoya et al., 2022; Olzak, 2023; Frey and Kirk, 2021, for an alternative view, see Rogowski and Tucker, 2019; Hassell et al., 2020). On the legislative side, these tragic events often catalyze the introduction of new gun-control bills (Schildkraut and Hernandez, 2014; Watson, 2022). Nonetheless, in the vast majority of cases, gun regulation remains either unchanged (Schildkraut and Hernandez, 2014; Goss, 2006; Luca et al., 2017; Cook and Donohue, 2017) or becomes even less restrictive (Luca et al., 2020) in the aftermath of shootings. This limited legislative action on gun regulation raises a critical question: Why does the United States continue to struggle with implementing meaningful gun-control measures despite recurring tragedies and public support for such measures?

This chapter emphasizes a different perspective on the political dynamics in the aftermath of school shootings that helps reconcile these findings. Using data on donations to the National Rifle Association's (NRA) political action committee (PAC), I examine how gun-rights supporters respond to school shootings, and the salient gun regulation discourse often attached to those (Brock and Routon, 2020; Newman and Hartman, 2019). Donations are an important marker for political mobilization responses on the ground and tend to go hand in hand with other (less costly) forms of political participation (Heersink et al., 2022, 2021; Bailey et al.,

2023; Steinberg, 2015; Grumbach, 2020; Panagopoulos and Bergan, 2007; Brown Jr et al., 1980). Understanding donation behavior thus provides a unique insight into the degree to which pro-regulation movements are faced with opposition from the gun-rights side.

Using address-level data from 225,000 individual donations in a difference-in-differences design, this chapter provides causal evidence suggesting that donations to the NRA surge in counties that experience school shootings. In stark contrast to the typically transient nature of pro-gun regulation movements (Goss, 2006; Spitzer, 2020; Laschever and Meyer, 2021), this uptick in political participation endures over multiple years, remaining elevated over comparatively long periods of time. These effects are largest in states where gun regulation is weakest, adding to the political difficulties of enacting gun regulation in states with the greatest potential benefits in terms of reducing firearm-related deaths (Kalesan et al., 2016; Luca et al., 2017; Cook and Donohue, 2017; Santaella-Tenorio et al., 2016). Donations to gun-control groups in affected regions do not seem to specifically respond to such events.

The results presented in this chapter add to the understanding of the political consequences of school shootings as well as the complexities of enacting gun-control legislation in the contemporary United States. Events such as school shootings are often seen as potential catalysts for change (Frey and Kirk, 2021), opening rare "windows of opportunity" for gun-control legislation (Frey and Kirk, 2021; Sharkey and Shen, 2021). However, as this chapter suggests, such efforts tend to be met with counter-mobilizing responses by citizens opposed to gun regulation and who backlash against proposals for stricter laws. In this sense, understanding how these different mobilization dynamics play out over longer time frames that are relevant for policymaking (Druckman and Leeper, 2012) is crucial for understanding the struggle gun-control advocates face in the United States.

This chapter also speaks to the dissertation's broader argument about political behavior during episodes of contention. Gun-rights supporters' commitments to Second Amendment rights predate any particular shooting; what changes after a salient event is the political environment around those commitments—the perceived threat of new regulation and the activity of an organizational infrastructure well placed to channel that threat into participation. The dynamics studied here

are accordingly an instance of mobilization through an existing organizational structure: a stable predisposition produces a costly and durable political response because a trigger reaches it through a dense recruitment network.

2 Counter-mobilization in gun politics

The political dynamics of gun regulation in the United States are an ongoing contest between mobilization and counter-mobilization (Meyer and Staggenborg, 1996; Mayer and Bert, 2017; Laschever and Meyer, 2021). Both supporters and opponents of stricter regulation have organized over decades into competing networks of interest groups, social movement organizations, and engaged constituencies that respond to one another's gains and losses (Goss, 2006; Spitzer, 2020; Andrews and Edwards, 2004). Salient incidents of gun violence, such as school shootings, are external shocks that reshape this contest by raising the public salience of the issue and opening windows of legislative opportunity (Birkland, 1998; Kingdon, 1995; Frey and Kirk, 2021; Sharkey and Shen, 2021). Whether such windows close with substantive policy change depends as much on the strength and durability of the response from gun-rights supporters as on the size of the initial pro-regulation push. To make sense of the puzzle of repeated public outrage producing little legislative change, this chapter focuses on the counter-mobilization side and asks how forcefully gun-rights supporters react when their preferred policy status quo comes under threat.

Focusing events and the mobilization phase

Existing scholarship on the political consequences of school shootings has predominantly focused on the mobilization phase of this contest. School shootings reliably increase media coverage of gun violence (Schildkraut and Muschert, 2014; Fox et al., 2021), push average public opinion toward stricter gun regulation (Newman and Hartman, 2019; Hartman and Newman, 2019; Jose et al., 2021), prompt protest mobilization in affected and adjacent areas (Sato and Haselswerdt, 2022; Olzak, 2023), and shift voting behavior in ways that benefit gun-control proponents (García-Montoya et al., 2022; Yousaf, 2021). On the legislative side, they produce a surge in the introduction of new gun-control bills (Schildkraut and

Hernandez, 2014; Watson, 2022). Taken together, these patterns are consistent with the classic conception of focusing events as moments that elevate an issue on the public agenda and create temporary policy windows (Birkland, 1998; Kingdon, 1995).

Less attention has gone to what happens on the other side of the contest, and how it shapes legislative outcomes that play out over months and years rather than days (Druckman and Leeper, 2012). The same event that mobilizes gun-control advocates also unsettles a constituency for whom the resulting policy push is a direct threat to a core political identity. Movement scholars have long argued that mobilization and counter-mobilization unfold as paired processes, with each side's moves shaping the other's (Meyer and Staggenborg, 1996; Mayer and Bert, 2017; Laschever and Meyer, 2021). Understanding why gun regulation stalls in the United States requires studying both halves.

Gun owners as a mobilizable constituency

Gun-rights supporters are unusually well suited to engage in counter-mobilization. For a substantial share of American gun owners, gun ownership is more than a hobby or security choice; it is a core political identity (Lacombe et al., 2019; Lacombe, 2019, 2021; Joslyn et al., 2017; Joslyn, 2020; Carlson, 2015). Survey evidence indicates that gun owners are unusually politically engaged, that they prioritize gun policy more than non-owners do, and that they tend to evaluate candidates and parties heavily on this single dimension (Parker et al., 2017; Middlewood et al., 2019; Musa, 2016). In aggregate, issue-specific minorities of this kind can routinely outweigh more diffuse majorities in the legislative process (Bouton et al., 2021; Schutten et al., 2022). As Bouton et al. (2021) put it, single-minded constituencies of this sort tend to "dominate an apathetic majority that favors tighter regulations." This combination of identity centrality, asymmetric salience, and an existing organizational infrastructure (discussed below) is the configuration that movement scholars associate with rapid and durable counter-mobilization (Klandermans, 1984; Andrews and Edwards, 2004; Hertel-Fernandez, 2019).

Salient shootings sharpen this dynamic. By drawing attention to gun violence and pushing concrete legislative proposals onto the public agenda, school shootings raise the perceived likelihood that new restrictions on gun ownership will be

enacted (Steidley and Colen, 2017; Conley, 2019). For many gun-rights supporters, the result is a credible threat to Second Amendment rights rather than a technical debate over public safety (Dawson, 2019; Lee et al., 2022). Gun-rights supporters respond to such threats through a coherent bundle of behaviors: preemptive gun purchases (Porfiri et al., 2019; Brock and Routon, 2020), increased information-seeking about gun rights and pending legislation (Semenza and Bernau, 2022), and rising membership in gun-rights organizations (Olzak, 2023). These responses are shaped by the surrounding political discourse, which frames new proposals as threats to a politicized identity.

The NRA and the infrastructure of counter-mobilization

Gun-rights counter-mobilization can keep up with—and often outlast—gun-control mobilization because it does not have to be assembled from scratch each time gun policy returns to the agenda. The National Rifle Association (NRA) provides a permanent, well-resourced infrastructure that converts diffuse threat perceptions into concrete political action (Spitzer, 2020; Lacombe, 2021; Vizzard, 2000; Reich and Barth, 2017). Historically, the NRA has cultivated a dense network of state affiliates, training programs, magazines, and direct-mail operations that allow it to communicate with members at low marginal cost when policy is at stake (Shaiko and Wallace, 1998; Oldham et al., 2016; Dawson, 2019). Spitzer (2020) reports that the NRA spends more on internal communication than any other interest group in US politics, consistent with a strategy where the binding constraint is the speed and scale of grassroots mobilization, not necessarily money resources. Veteran observers have echoed this assessment, attributing the NRA's electoral and legislative successes largely to its "real grassroots prowess" rather than to campaign spending alone (Shaiko and Wallace, 1998, 155).

Two episodes illustrate how this infrastructure converts triggering events into political pressure. After the 1994 federal assault weapons ban, an NRA-organized constituent backlash is widely credited with helping prevent renewal of the ban a decade later (Vizzard, 2000; Higham and Horwitz, 2013). More recently, after Senator Pat Toomey's support for universal background checks in the aftermath of the 2012 Sandy Hook shooting, gun-rights supporters at hundreds of Pennsylvania polling stations wrote "2A"—shorthand for the Second Amendment—on their bal-

lots, in a coordinated signal of intensity that legislators report as one of the most consequential forms of constituent feedback (Gutowski, 2021). As an NRA aide quoted in Gutowski (2021) described it, the most damaging form of backlash is "the fact that your office gets overrun with people calling and writing letters … It's a level of motivation and organization that shows that people feel strongly about the issue."

Field-experimental evidence shows that mobilized constituent contact with legislators can shift their support for specific bills (Bergan and Cole, 2015), and that legislators draw inferences about district preferences disproportionately from copartisan contact (Broockman and Ryan, 2016; Walgrave and Soontjens, 2023). In a polarized environment where serious gun-control proposals are typically sponsored by Democratic legislators, even modest shifts in the volume of constituent communication from gun-rights supporters in Republican-leaning districts can shift legislators' calculations on gun regulation.

From mobilization on the ground to observable donations

Many of the behaviors that constitute counter-mobilization on the ground—phone calls, e-mails, attendance at town halls, conversations with neighbors—are difficult to observe systematically across counties and over time. Donations to political action committees, by contrast, leave a public administrative trace. Donating is part of a tightly correlated bundle of political activities, including contacting legislators, attending events, and recruiting other contributors (Steinberg, 2015; Panagopoulos and Bergan, 2007; Brown Jr et al., 1980; Heersink et al., 2021, 2022; Bouton et al., 2022; Lowry, 2013; Grumbach, 2020; Bailey et al., 2023). As one of the costlier forms of political behavior (Margetts et al., 2011; Schafer, 2021; Marien et al., 2010), donating tracks underlying intensity of issue engagement beyond fleeting attentional spikes.

This chapter therefore treats changes in donations to the NRA's Political Victory Fund as an observable proxy for broader counter-mobilization on the ground. Three predictions follow from the framework above. First, in the aftermath of a fatal school shooting, donations from gun-rights supporters in affected counties should rise on both the extensive margin (more donors) and the intensive margin (larger total amounts), driven by the same threat-induced response visible in

the gun-purchasing and information-seeking literatures. Second, this response is expected to be comparatively durable. Whereas gun-control mobilization has historically struggled to sustain itself once the immediate news cycle moves on (Spitzer, 2020; Goss, 2006; Laschever and Meyer, 2021; Schildkraut and Muschert, 2014), gun-rights supporters benefit from an organizational infrastructure and an identity-based commitment that should keep them engaged over multiple years (Lacombe, 2021; Joslyn, 2020). In a policy process that itself unfolds over months and years (Druckman and Leeper, 2012; Baumgartner et al., 2009; Shapiro, 2011), the asymmetry in staying power matters at least as much as the size of the initial response. Third, the strength of counter-mobilization should depend on how much is perceived to be at stake. In states with already restrictive gun regulation, the marginal threat from a new proposal is smaller and the politically activated constituency also tends to be smaller (Reich and Barth, 2017; Kalesan et al., 2016; Conley, 2019). In contrast, in states with permissive baselines, the same proposal looks more consequential, and the mobilizable constituency is larger. The counter-mobilization response should therefore be stronger in less restrictive states. By the same logic, while the most fatal incidents receive disproportionate national media coverage (Schildkraut and Elsass, 2016; Fox et al., 2021; Schildkraut and Muschert, 2014), even shootings with relatively few fatalities can credibly raise the local threat of new gun regulation when they generate sustained local discourse, so the counter-mobilization effect should not scale strictly with fatality counts. The remainder of this chapter examines those predictions empirically.

3 Materials and methods

Donations data

The most important PAC for citizens concerned with gun rights is the NRA's Political Victory Fund (NRA-PVF), which uses its collected funds to endorse "gun-friendly" candidates and counter candidates perceived as a threat to Second Amendment rights (Spitzer, 2020; Reich and Barth, 2017). Data on NRA-PVF donations (and all placebos) between 2000 and 2022 are obtained through the Federal Election Commission (FEC). The FEC supplies data on individuals who have donated at least $200 to a PAC in a given election cycle, containing the full

name and address of a person, job title, receipt date, amount, and recipient for each donor and donation.

These data have been used extensively for research on political mobilization (Lowry, 2013; Perez-Truglia, 2018; Culberson et al., 2019), donation behavior (Barber et al., 2017; Heerwig, 2018; Bonica, 2014), and the consequences of campaign contributions, e.g., polarization or policy outcomes (Kujala, 2020; Ansolabehere et al., 2003; Kolodny and Dwyre, 2018). Since this chapter is focused on the mobilization of individuals, only donations from individuals are retained, leaving around 225,000 unique donations for the entire period. The provided addresses are used to geocode the county for each individual donation. Thanks to the relative completeness of the information on contributor addresses, 99.91% of addresses are successfully geocoded to the county level. Donations emanate from 2,984 counties across the country.

Two separate variables are constructed: (i) real donations in a county in a given year and (ii) the number of individual donors in a county in a given year. For the former, donations are deflated by the consumer price index (CPI) with base year 2022 and aggregated to the county-year level. For the latter, the available personal information is used to identify the number of unique donors who have donated in a given county year.

School shooting data

Data on school shootings come from a validated and well-referenced list by Wikipedia that has been used extensively for research on the political consequences of school shootings (García-Montoya et al., 2022; Hassell et al., 2020; Preti, 2008). For the analysis, only county years between 2000 and 2022 are retained in which at least one event with at least one fatality occurred. The reason for this is that the likelihood of heightened discourse over the issue of gun violence is most likely to occur when a shooting did lead to fatalities (Semenza and Bernau, 2022; Fox et al., 2021). The staggered difference-in-differences estimator used in this analysis (see below) allows the consideration of the effect of the first shooting that occurred in a county during the sample period. After the first shooting, a county is coded as treated and does not return to the control group. The full school shooting dataset contains 131 school shootings occurring in 40 different states, all of which are

displayed on the map in Figure 1. The final dataset for the analysis is obtained by merging county-level donations with the event-level data on school shootings. Estimations using alternative data sources (Sections S2 and S9) and fatality cutoffs (Section S8) are discussed in the Supplementary Materials.

Figure 1: Map of US school shootings.



Notes: This map shows the locations of all school shootings between 2000 and 2022 that are considered in the main analysis.

Statistical analysis

To estimate the causal effect of school shootings on donation mobilizing behavior, a staggered difference-in-differences design is used, employing the robust estimator put forward by Callaway and Sant'Anna (Callaway and Sant'Anna, 2021). The identification assumption is that treated and control units would have behaved similarly in the absence of treatment, using the fact that the exact location and timing of school shootings are largely as-if random (Yousaf, 2021; García-Montoya et al., 2022; Blair and Schwieit, 2014; Madfis, 2017). The control units in my main estimation are all units that are never "treated," i.e., that do not experience a school shooting over the entire period. In Sections S3.2 and S3.3, alternative criteria for inclusion in the control group are considered and show that the main results are

robust to such alternative definitions of control units. For all estimations, standard errors (SEs) are clustered on the county level. More formally, the analysis proceeds in two steps. First, it estimates group-time average treatment effects on the treated (ATTs) of the form

$$ATT(g,t) = \mathbb{E}[Y_t(g) - Y_t(0)|G = g] \tag{1}$$

where g represents the year of first treatment, and t represents year. For example, $ATT(2003, 2006)$ is the treatment effect in the year 2006 for units treated first in 2003, i.e., the dynamic effect for this treatment group after 3 years. Group-time ATTs of this form are defined as the expectation of the difference between outcome Y in group g at time t—i.e., $Y_t(g)$—and the outcome in the control group at time t—i.e., $Y_t(0)$. In a second step, Callaway and Sant'Anna's respective dynamic aggregation procedure is used to turn these estimates into an event-study format that resembles the output of the standard multiperiod two-way fixed effects (TWFE) difference-in-differences model.

$$\theta_{es}(e) = \sum_{g \in \mathcal{G}} \mathbf{1}\{g + e \leq \mathcal{T}\} \mathbb{P}(G = g \mid G + e \leq \mathcal{T})\, ATT(g, g + e). \tag{2}$$

Here, the individual effect parameter $\theta_{es}(e)$ indicates the causal effect of the treatment after e periods, averaged over all treatment groups that were exposed to at least e periods of treatment. For a lengthier discussion of aggregation procedures, see Callaway and Sant'Anna (2021).

All analyses are performed in levels rather than logs, despite the fact that being able to interpret the results as percentage changes would generally be desirable. The reason for this is that I cannot consistently estimate percentage-interpretable ATTs of log-like transformations in situations where I have nontrivial amounts of zero values (Chen and Roth, 2023). This applies to this context because there exist counties with no donations in a given year. Instead, the level results are contextualized by being set in relation to the pretreatment baseline.

Heterogeneous treatment effects

To examine treatment effect heterogeneity, the treatment group is split into (i) counties within states that average 1 (unrestrictive) on Giffords' "Annual Gun Law Scorecard" (Giffords Law Center to Prevent Gun Violence, 2022) and counties within states that average higher than 1 (restrictive) and (ii) below three/three or more fatalities. Three fatalities are a commonly used threshold for classifying shootings as mass shootings (Follman et al., 2023; Stanford Geospatial Center, 2016), allowing the comparison of the effect of school shootings to school mass shootings.

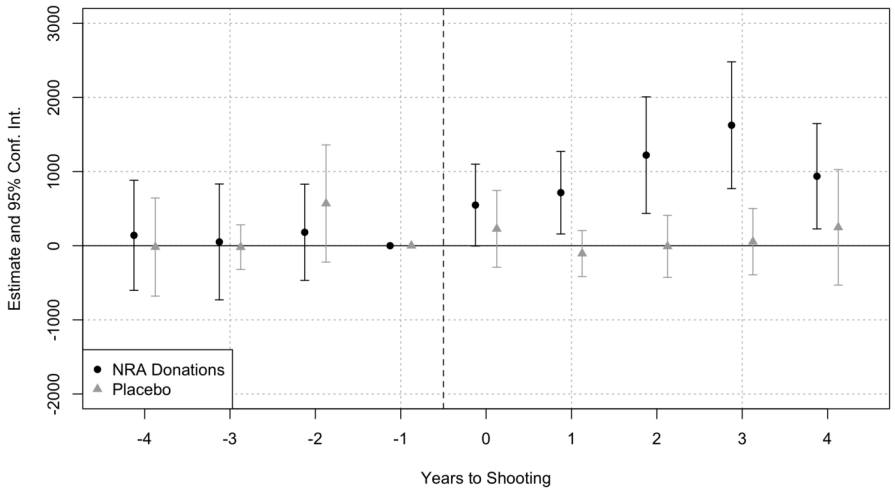
4 Results

Figure 2 reports event-study estimates of the causal effect of school shootings on donation amounts (top) and the number of individual donors (bottom) in affected counties. Consistent with the first prediction of the framework above, the results provide evidence for substantial mobilization by gun-rights supporters in the years following a school shooting, on both the intensive and extensive margins of donation behavior. Starting in the year of the shooting (t_0), I observe notable and persistent surges in donations in treated counties relative to the untreated control group.

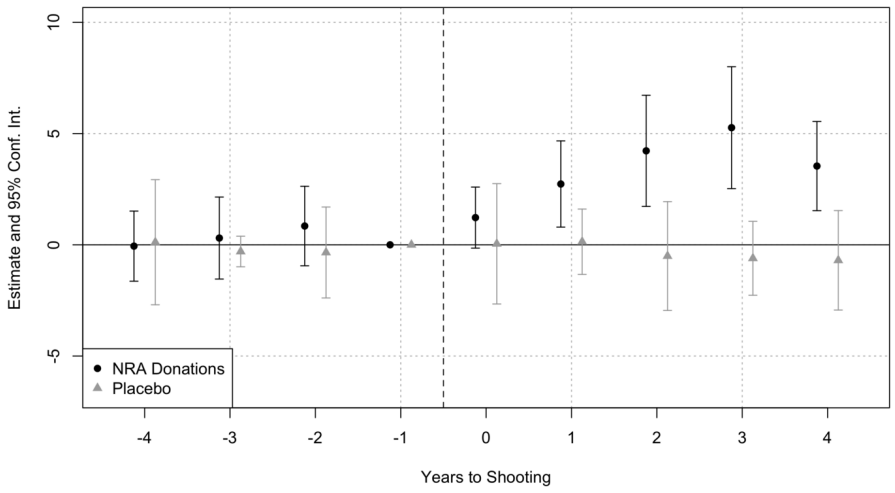
On the basis of the main specification, it is estimated that donations increase on average by \$970, $p < 0.001$, 95% confidence interval (CI) [\$517, \$1,424] per annum in the years following a shooting; this is an average increase of around 30% over the pretreatment baseline for each year. The estimated total dynamic effect is \$4,852, which is comparable to an additional 1.5 years of pre-treatment donations. These increases in donated sums are not merely the result of greater donations by existing donors, as is shown in the bottom panel of Figure 2. The number of individual contributors increases at its peak by around 5, $p < 0.001$, 95% CI [2.61, 7.48], representing a 40% increase over the baseline. School shootings incentivize individuals who have previously not been contributors to start donating money. There are 10 counties in which there were no active donors pre-shooting but where a group of new donors emerged, starting to donate money from the year of the shooting onward. This effect, too, is remarkably durable.

Figure 2: School shootings increase NRA donations.

(a) Donation amounts.



(b) Number of donors.



Notes: These figures display event study estimates for the effect of school shootings on donation amounts (top) and the number of individual donors (bottom), using a staggered difference-in-differences design. Treatment is defined as occurrence of a school shooting. The relevant time variable is year. The placebo is donations to the anti-abortion National Right to Life Committee (NRLC) PAC. The bars represent 95% confidence intervals. SEs are clustered at the county level.

In general, these effects are consistent with previous findings that suggest that gun-rights supporters are a very politically active constituency that is comparatively easy to mobilize (Goss, 2006; Grossmann, 2020; Joslyn et al., 2017; Lacombe, 2019; Middlewood et al., 2019; Parker et al., 2017). Moreover, these findings are consistent with studies finding that gun-rights supporters respond to threats of gun regulation by, e.g., preemptively purchasing guns (Porfiri et al., 2019), gathering information about gun rights (Semenza and Bernau, 2022), or joining gun-rights organizations (Olzak, 2023), especially after incidents that receive considerable media attention. The empirical pattern also aligns with anecdotal evidence suggesting that gun-rights supporters mobilize by contacting legislators when perceiving gun rights under threat (Gutowski, 2021).

There is reason to believe that the effect estimates presented here are likely conservative, underestimating the true treatment effects school shootings have on NRA donations. In general, direct contributions to the NRA (rather than the PAC) are unobservable, and FEC rules on campaign finance mean that even for the NRA-PVF, I can only observe individuals who surpass the itemization threshold of $200 (Alvarez et al., 2020; Bouton et al., 2022) over the course of an electoral cycle. Given that name disclosure is generally considered a cost (La Raja, 2014) and that a large fraction of NRA-PVF donations come from small donors (see, e.g., Federal Election Commission, 2018), relying on itemized donations poses a conservative test. The fact that clear patterns of surges in donations are visible in the data nonetheless should be considered strong evidence for the notion that such dynamics exist.

Is this pattern specific to mobilization around gun regulation, or do I observe a general spike in political participation within the wider (conservative) population around a larger set of political issues? It is imaginable that salient and traumatic events such as school shootings intensify general political discourse and participation (Bateson, 2012; Schüller, 2015; Holbein and Hassell, 2023; Reny and Newman, 2021), leading to similar upticks across many issue areas that are largely orthogonal to the issue of gun regulation. To consider this possibility, changes in donation behavior to the anti-abortion National Right to Life Committee (NRLC) PAC after school shootings are examined; abortion being another highly contentious issue in contemporary US politics that is orthogonal to gun control. Replicating the main analysis using donations to the NRLC as outcome, the gray triangles in Figure 2

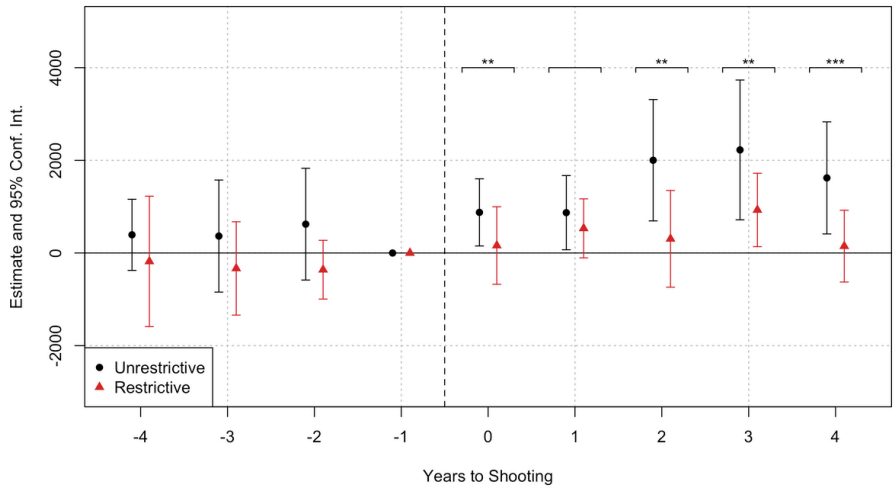
demonstrate that this is not the case: Political mobilization around this issue does not seem to respond to school shootings.

Two heterogeneity tests follow from the argument above. If counter-mobilization is shaped by the political conditions surrounding an existing constituency rather than by the magnitude of the triggering event, two patterns should hold. First, effects should be stronger in states with permissive baseline regulation, where new restrictions would be more consequential and the politically activated constituency is denser (Reich and Barth, 2017; Kalesan et al., 2016; Conley, 2019). Second, effects should not scale strictly with the number of fatalities, since even lower-fatality shootings can credibly raise the local threat of new regulation (Luca et al., 2017; Semenza and Bernau, 2022), despite the most fatal events drawing disproportionate national coverage (Schildkraut and Elsass, 2016; Fox et al., 2021). A pattern in the opposite direction on either cut—larger effects in already-restrictive states, or effects tightly scaling with fatality counts—would be hard to reconcile with the argument. Figure 3 shows that treatment effects are indeed larger in states with less restrictive gun laws, i.e., in states in which there is more to lose politically, but do not substantially increase with the number of fatalities. Both results are consistent with the view that counter-mobilization tracks the political conditions surrounding the constituency rather than the size of the triggering shock.

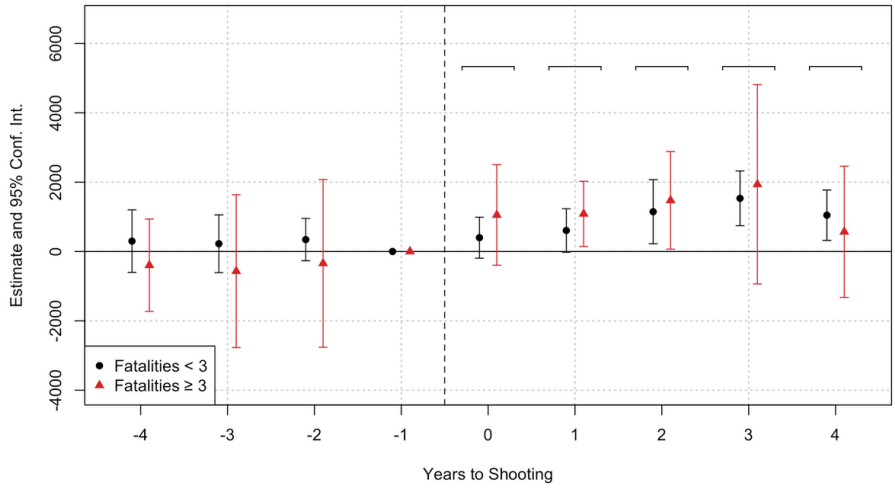
A natural question that emerges is whether these changes in donation behavior are the result of the NRA successfully mobilizing its support base in affected areas or whether this pattern is the result of an independent, bottom-up mobilization effort by citizens concerned with gun rights. As the main gun-rights organization in the US, the NRA has historically been actively encouraging its members to voice their concerns to political representatives and engage in grassroots activism when serious proposals for gun regulation emerge (Oldham et al., 2016). In this light, one might think that the NRA intensifies fundraising activity in affected areas, i.e., in areas in which threat perceptions are largest. To speak to this question, geocoded data on advertisement spending by the NRA on Meta (Facebook and Instagram) is analyzed. The data are obtained through Meta's Ad Library API and contain information on the amount spent on advertisements by the NRA, as well as the number of impressions these ads have received. The unit of observation is the ad-

Figure 3: Effect of school shootings depends on gun law restrictiveness but not fatality count.

(a) Heterogeneity by gun law restrictiveness.



(b) Heterogeneity by fatality count.



Notes: These figures display event study estimates for the effect of school shootings on donation amounts by state gun law restrictiveness (top) and the number of fatalities (bottom), using a staggered difference-in-differences design. Treatment is defined as occurrence of a school shooting. The relevant time variable is year. The bars represent 95% confidence intervals. All SEs are clustered at the county level. Stars above coefficients indicate significance level of coefficient difference obtained using bootstrap ($n = 500$). Gun law restrictiveness data on state-level come from Giffords' "Gun Law Restrictiveness Scorecard." Observations are split into scores equal to 1 (unrestrictive) and above 1 (restrictive). $^{***}p < 0.01$, $^{**}p < 0.05$, and $^{*}p < 0.1$.

state-day level and data are available for the years 2018 to 2023. For the analysis, data are aggregated to the state-week level, i.e., all ad spending and impressions for each week and state are collapsed into one number, independently of how many different ads were run that week.[1] If the NRA is indeed intensifying fundraising efforts in affected areas, I would expect to see increases in advertisement spending and user impressions (views) in states in which school shootings occurred.

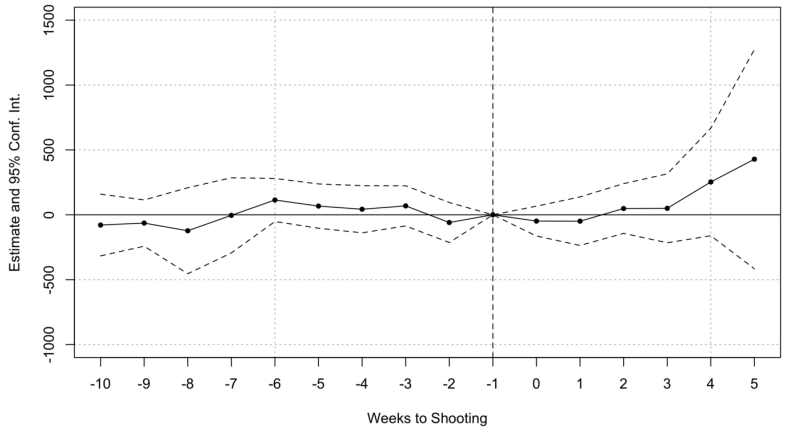
Using the same staggered difference-in-differences design as above, and defining time relative to treatment in weeks since the shooting, Figure 4 indicates that this is not the case. NRA advertisement spending and user impressions do not increase in states in which school shootings occurred in the weeks after the incident. This suggests that the observed mobilization pattern is not driven by new bursts of paid advertising in affected areas. The pattern is consistent with the framework above, in which the NRA's standing infrastructure of state affiliates, magazines, and direct-mail communication (Shaiko and Wallace, 1998; Spitzer, 2020) channels threat perceptions into political action without requiring localized advertising surges. The caveat that heavily localized effects might be washed out in my state-level data and that other fundraising channels that I did not observe exist remains, however.

Having shown how NRA donations respond to school shootings, and to make sense of how to interpret this in the greater question of gun control, one might ask how this compares to changes in pro-gun-control mobilization. To this end, the main analysis is repeated with donation data from the Giffords PAC. Giffords is one of the leading gun-control groups in the US. For this analysis, data on donations for the years 2013 to 2022 are obtained from the FEC. Other than the dependent variable, the performed analysis is identical to the main analysis. The results of this analysis are shown in Figure 5. In particular, it does not appear that gun-control donations respond in a similar fashion. There seems to be neither an effect on donations nor on donor numbers, with relatively tight confidence intervals. Hence, at least on the pecuniary dimension, there appears to be an asymmetry in

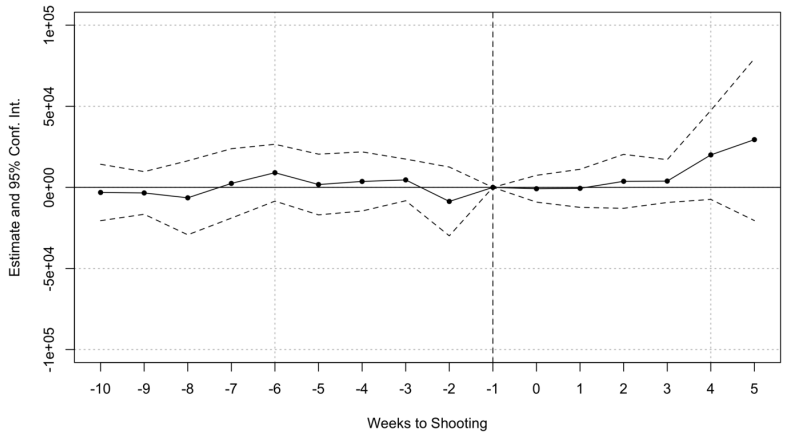
[1]Weeks are being used as the relevant time unit because the data are available for only a limited number of years and the variables are measured only at the state level. Estimating effects over longer time horizons would therefore risk capturing compounded state-level treatment effects driven by other state-level factors that also influence spending and impressions. On a theoretical level, one might expect fundraising activities to increase soon after the event when outrage (and threat) is largest. Analyses using months as time period are noisier but yield identical results.

Figure 4: NRA Meta advertisement after school shootings.

(a) Spending.



(b) Impressions.



Notes: This figure displays event study estimates for the effect of school shootings on NRA advertisement spending (top) and user impressions (bottom) on Meta, using a staggered difference-in-differences design. Treatment is defined as occurrence of a school shooting. The level of treatment is state. The relevant time variable is week. The bands represent 95% confidence intervals. All standard errors are clustered at the state level.

mobilization after shootings, in line with the second prediction of the framework above, suggesting that engagement on the gun-rights side is more durable than the typically more diffuse pro-regulation push.

Robustness checks

A series of robustness checks are performed to scrutinize the robustness of the main estimates. Pah et al. (2017) constructed a school shooting database on the basis of various data sources, aiming to be as comprehensive as possible up to 2013. Using this alternative data source yields equivalent results (Section S2.2). Similarly, García-Montoya et al. (2022) provide a dataset of "rampage shootings," applying stricter definitions of what constitutes a school shooting (Schildkraut and Elsass, 2016; Newman et al., 2008). The results from this estimation yield similar effect estimates as in the main text (Section S2.1). Mass shootings are another highly salient form of gun violence that should elicit similar responses from gun-rights supporters (Frey and Kirk, 2021; Rogowski and Tucker, 2019). Coding treatment as the occurrence of a mass shooting on the basis of data by Mother Jones (Follman et al., 2023), this analysis finds identical effects on NRA donations (Section S2.3).

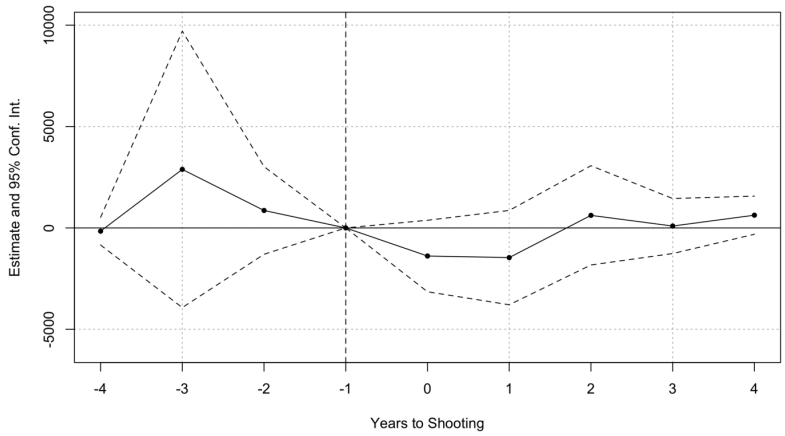
In contrast, unsuccessfully attempted school shootings, or school shootings that did not result in fatalities, generally result in lower attitude shifts and less media coverage (Semenza and Bernau, 2022; Fox et al., 2021; Schildkraut and Elsass, 2016). Consequently, they pose a lower threat to gun rights and should elicit more nuanced responses in terms of NRA donations. Using data from the K-12 school shootings dataset (Riedman, 2023), it is shown that this is indeed the case. Such events do not have significant effects on NRA donations. For validation purposes, using the same data, the same analysis shows that treatment effects are identical to the results in the main text when focusing on school shootings with fatalities.

Limited spillover effects into adjacent counties exist, implying that the control group is contaminated with partially treated units (Section S4). This implies that results presented in the main text are likely biased toward zero (Butts, 2021).

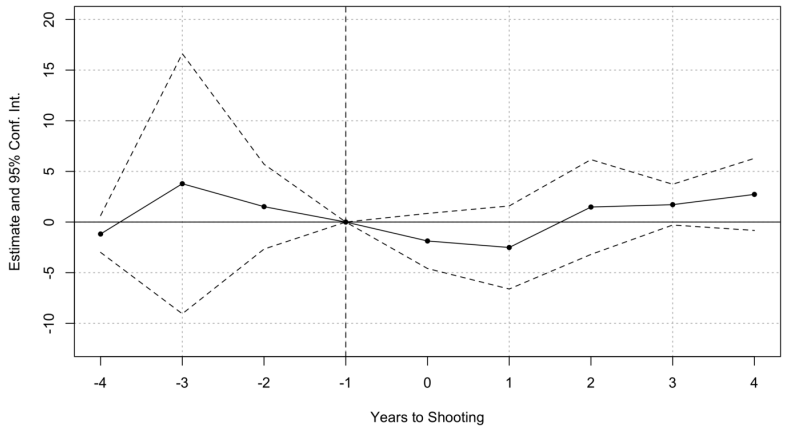
The central identification assumption is that treated and untreated units would have evolved in parallel in the absence of treatment. While this assumption is not directly testable, additional analyses in which this assumption is potentially more likely to hold are performed. Following Hassell and Holbein (2024), models that

Figure 5: School shootings and gun-control PAC donations.

(a) Donation amounts.



(b) Number of donors.



Notes: This figure displays event study estimates for the effect of school shootings on donation amounts to the Giffords PAC, using a staggered difference-in-differences design. Treatment is defined as occurrence of a school shooting. The relevant time variable is year. The bands represent 95% confidence intervals. All standard errors are clustered at the county level.

allow for heterogeneous county-level time trends are estimated, accounting for the possibility that counties with a higher propensity for school shootings are trending in different ways (Section S3.1). In this model, the ATT estimates the deviation from the individual time trend caused by the shooting. Despite the fact that this strategy tends to impose attenuation bias in staggered settings (Goodman-Bacon, 2021), allowing for such trends produces similar empirical patterns. In addition, models that restrict the control group to directly adjacent (Section S3.2) and to not-yet-treated (Section S3.3) counties are estimated. Directly adjacent counties are likely more similar in terms of demographic structure or local economy (García-Montoya et al., 2022). The presence of spillover effects renders this comparison a conservative test for the empirical pattern (Butts, 2021). Restricting the control group to not-yet-treated units means only making comparisons with counties where school shootings eventually occur, comparing counties that are in expectation more alike. All of these analyses support the notion that school shootings increase NRA donations.

All results are highly insensitive to the exclusion of any particular state, as leave-one-out analyses suggest (Section S5). Results are not driven by single, high-saliency events such as Sandy Hook 2012 or Parkland 2018. Results do not depend on the particular choice of estimator (Section S6). Re-estimating the main results using the alternative staggered estimators by Borusyak et al. (2021), De Chaisemartin and d'Haultfoeuille (2020), and Sun and Abraham (2021) produces largely identical estimates.

5 Discussion

The results of this chapter lead to three conclusions. First, school shootings have important effects on political participation on the political right. The causal estimates of this chapter demonstrate that gun-rights supporters respond to school shootings by increasing contributions to political organizations committed to upholding Second Amendment rights. These effects are sizable, with donations increasing by around 30% and donor numbers increasing by about 40% in affected counties as compared to the pretreatment baseline. Studies focusing solely on aggregate shifts in the general population miss such mobilization dynamics by

highly motivated subgroups with a vested interest in preventing policy change (Heersink et al., 2021).

It is worth noting that behaviors such as donating money usually do not stand in isolation but often go hand-in-hand with other forms of (less costly) participation, such as contacting legislators or protesting (see analysis in Section S7). In this light—and considering the fact that gun owners are a highly politically active (Musa, 2016) and easily mobilizable constituency (Goss, 2006)—such increases in donations might be indicative of more general political mobilization on the ground. A supplementary analysis of Cooperative Election Study data in Section S9 supports this notion, showing that gun owners increase different forms of non-electoral political participation immediately after school shootings in their local area.

Direct contact between constituents and legislators can have substantial effects on their respective perceptions of constituency public opinion (Walgrave and Soontjens, 2023) and influence support for bills (Bergan and Cole, 2015). Thus, such mobilization during times that are often seen as policy windows for gun regulation might help explain why gun regulation remains elusive despite widespread public support. While such behaviors are often difficult to observe directly and over time, donations provide a unique opportunity to learn about such dynamics.

Second, the impact of school shootings on donations and donor counts is remarkably durable. Even multiple years after the incident, both donations and donor counts remain at elevated levels. Insofar as this pattern is indicative of citizens remaining actively committed to the cause of protecting gun rights, this highlights another difficulty in the struggle to enact stricter gun-control laws. The time frame over which legislation is passed is medium- to long-term (Druckman and Leeper, 2012). Thus, while windows of opportunity might open the door for the introduction of new bill proposals (Schildkraut and Hernandez, 2014; Watson, 2022; Druckman and Leeper, 2012; Shapiro, 2011), making sure that these bills eventually pass requires that gun-control advocates collectively mobilize and remain mobilized over longer periods of time (Laschever and Meyer, 2021; Baumgartner et al., 2009). However, gun-control movements have historically struggled to keep such momentum up (Goss, 2006; Spitzer, 2020; Laschever and Meyer, 2021), and saliency usually wanes as the issue-attention cycle brings other issues to the

forefront (Luca et al., 2020; Schildkraut and Muschert, 2014). On the contrary, for many gun owners, gun ownership is a fundamental part of identity (Lacombe, 2019, 2021), and Second Amendment rights are one of the most important policy issues (Goss, 2006; Joslyn, 2020). As Bouton et al. (2021) write, these gun-rights supporters tend to "dominate an apathetic majority that favors tighter regulations." To bring about policy change, gun-control groups and their supporters have to remain mobilized over the long-term.

Last, the strength of these effects is most pronounced in states with weaker gun regulation. These states typically experience higher rates of firearm-related deaths and are most likely to benefit from gun-control legislation (Luca et al., 2017; Cook and Donohue, 2017; Santaella-Tenorio et al., 2016). For instance, Kalesan et al. (2016) estimate that the introduction of universal background checks, as already existent in many states and supported by 92% of citizens in 2022 (Gallup, 2022), could reduce the rate of firearm-related deaths from 10.35 to 4.46 per 100,000 people in states without such a policy. The fact that such states also witness the greatest backlashes against gun control further adds to the intricate challenges involved in enacting comprehensive gun regulation measures to reduce shootings.

The pattern of heterogeneous effects also speaks to the broader question of when counter-mobilization is most pronounced. The fact that effects are larger in less restrictive states, and do not scale strictly with fatality counts, is consistent with a view that emphasizes the conditions for counter-mobilization rather than the size of the triggering event alone. Where the marginal threat from new regulation is greater and the mobilizable constituency is denser, the same kind of event produces a larger and more durable response; where either condition is weaker, the response attenuates even when the event is comparable in fatality count. The heterogeneity does not point to a different attitude in different states. It points to differences in how readily an existing organized constituency can be reached by a triggering event—a feature of the political environment rather than of underlying preferences (Conley, 2019; Reich and Barth, 2017). More broadly, the chapter illustrates one of the routes by which contentious moments reshape political behavior, through the organizational conditions surrounding a stable constituency rather than through shifts in what citizens want.

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Chapter 2

Racial Threat in Public Social Spaces*

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Abstract

We examine the electoral effects of exposure to ethnic diversity in public social spaces. We study the case of public swimming pools in Germany, where intergroup relations have become increasingly politicized in recent years. Our empirical analysis combines (i) information on the geolocation of all public pools in Germany, (ii) a high-frequency panel survey of more than 240,000 voters, and (iii) a novel survey on threat perceptions in public pools. Using a difference-in-differences design that leverages the seasonality of outdoor swimming pools for causal identification, we estimate that exposure to ethnic diversity in public pools increases far-right support by about 0.8 percentage points. Additional panel analyses suggest that these results are driven by an increase in the issue salience of immigration relative to taxes and redistribution. Our findings lend credence to the racial threat hypothesis in politicized public social spaces that do not meet Allport's conditions for positive intergroup contact.

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1 Introduction

Public social spaces are vital to the fabric of democratic societies, fostering interactions across different social and demographic groups and reflecting the evolving mosaic of society (Parkinson, 2012). As ethnic diversity is rising across industrialized democracies, public spaces increasingly facilitate repeated, everyday encounters not only between different social and economic groups but also across ethnic lines.[1] This chapter hones in on the political ramifications of this secular trend. Specifically, we examine the effects of intergroup encounters and exposure to ethnic diversity in a public social space that has become increasingly politicized in recent years: swimming pools.

Both historically and today, public swimming pools have constituted social spaces where broader cleavages and conflicts between different social, religious, and ethnic groups have manifested (Carlucci et al., 2020). In the United States, for example, public pools have mirrored racial segregation policies and social hierarchies throughout the 20th century, with many cities divesting from public pools or closing them altogether in the wake of desegregation mandates stemming from the Civil Rights Act (Wiltse, 2007; McGhee, 2022). Today, religious swimwear and gender-specific swimming hours have become politically contentious issues in many countries, including France (Ganley and Picheta, 2022), Denmark (The Local, 2017), and Israel (NYT, 2023).

This chapter examines the link between exposure to ethnic diversity in public swimming pools and support for far-right parties. We argue that exposure to ethnic diversity in public social spaces such as swimming pools can galvanize racial threat perceptions among native voters (Blalock, 1967; Claassen and McLaren, 2021; Kustov et al., 2021). Highly frequented public social spaces facilitate repeated encounters between the local native and immigrant population and thereby directly expose native voters to a snapshot of ethnic diversity at the

[1] Distinction between race, ethnicity, and citizenship: our argument focuses on exposure to ethnic diversity, which goes beyond the concept of race in that it also captures cultural characteristics such as language and religion (Bulatao and Anderson, 2004). However, much of the related literature studies the US context and primarily focuses on the concept of race. Accordingly, our title, abstract, and theory section refer to terms coined in this literature, in particular the 'racial threat hypothesis' (Blalock, 1967). Our empirical strategy proxies ethnicity through citizenship (see Section 4.1 for details).

local level. Repeated intergroup encounters in public social spaces can thus elevate native voters' attention to immigration, multiculturalism, and demographic change as key political issues. Exposure to diversity in public social spaces, in other words, can increase the issue salience of immigration among native voters. Following standard spatial voting models, we argue that raising the issue salience of immigration benefits far-right parties, as voters with anti-immigrant preferences become more likely to vote for far-right parties regardless of their positions on other political issues (Ivarsflaten, 2008; De Vries et al., 2013; Dennison, 2020). This can be particularly beneficial for far-right parties such as the German *Alternative für Deutschland*, which combines anti-immigrant rhetoric with economic policies that run diametrically opposed to most of their constituents' interests (FAZ, 2023). In sum, we argue that repeated exposure to local ethnic diversity in public social spaces increases the perceived issue salience of immigration among native voters to the benefit of far-right parties.

We test this argument in the context of public swimming pools in Germany—a setting in which intergroup relations have become increasingly politicized in recent years. To provide descriptive evidence on threat perceptions related to public pools, we conducted a novel survey of over 3,000 respondents during the 2024 outdoor pool season. We find that the far-right *Alternative für Deutschland* (AfD) and extensive media coverage of out-group crime in public pools have established a political narrative linking diversity in public pools to violent crime. As a result, diversity in public pools is perceived as threatening and dangerous by a sizable share of the German electorate. While violent crimes committed by immigrants in public swimming pools are extremely rare, concerns about the safety of public pools are widespread (Zeit, 2023).

We combine multiple data sources to empirically examine the political effects of exposure to ethnic diversity in public swimming pools. First, we collected information on the precise geolocation of all public swimming pools in Germany. We then link the local presence of public pools to highly granular information on local far-right support. To do this, we draw on high-frequency panel survey data covering about 240,000 eligible voters surveyed between January 2017 and December 2018. To causally identify the effect of out-group exposure in public pools on political preferences, we leverage the seasonality of outdoor swimming

pools in a difference-in-differences design. In a nutshell, we compare changes in electoral support for the far-right among respondents who live in localities with outdoor swimming pools to respondents living in localities without outdoor swimming pools before and after the onset of the outdoor pool season. We estimate specifications at varying levels of local ethnic heterogeneity, proxied through the county-level share of non-EU foreigners. Our difference-in-differences approach accommodates (i) level differences in far-right support between localities with and without outdoor pools and (ii) time-specific, nationwide shocks to far-right support (e.g., general seasonal trends). We conducted a series of additional statistical tests for the identifying assumptions of the difference-in-differences approach in our setting. Most importantly, we demonstrate parallel trends prior to the outdoor pool season. Between October and April, electoral support for the far-right evolves in parallel in localities with and without outdoor pools.

Across a series of specifications, we find a sizable, statistically significant positive effect of exposure to ethnic diversity in public swimming pools on far-right support. In ethnically diverse localities, the far-right AfD gains about 0.8 percentage points during the outdoor pool season. These gains only materialize in high-diversity localities and come at the expense of parties across the political spectrum. In ethnically homogeneous localities, we do not find differential changes in electoral support between localities with and without outdoor pools during the summer outdoor pool season. To establish the external validity of our results, we conducted a supplementary analysis of precinct-level electoral returns in Germany's two largest cities, Berlin and Hamburg. In line with our survey-based results, we find a positive, statistically significant cross-sectional relationship between the local presence of public outdoor swimming pools and support for the far-right in federal elections. Taken together, we find strong evidence that exposure to local ethnic diversity in public swimming pools benefits far-right parties.

To examine the mechanisms driving our results, we draw on an additional panel survey of more than 30,000 eligible voters surveyed between 2016 and 2019 (GLES, 2023). We use these data to test a direct empirical implication of our theoretical framework. Specifically, we examine whether the shifts in electoral behavior we observe are accompanied by shifts in underlying preferences or changes in the perceived salience of different political issues (Fournier et al., 2003). In line with

our theoretical framework, we find that exposure to ethnic diversity in public pools increases far-right support by heightening the salience of immigration relative to other issues, particularly taxes and redistribution. Exposure to diversity in public social spaces, in other words, results in a shift in the relative salience of cultural issues (2^{nd} dimension politics) and economic concerns (1^{st} dimension politics) to the benefit of the far-right. In line with prior research, which has highlighted the stability of immigration attitudes (Kustov et al., 2021), we do not find evidence for changes in underlying preferences.

Our study contributes to prior research on intergroup relations and immigration attitudes in multiple ways. First, we demonstrate a new mechanism that links out-group exposure to changes in political behavior. While most prior research has focused on how intergroup contact shapes attitudes and preferences, our study demonstrates that out-group exposure can alter electoral outcomes even when underlying preferences remain unchanged. In our setting, we find that exposure to ethnic diversity increases the perceived salience of the immigration issue at the expense of the economic dimension of politics. Our study thus offers a novel perspective on previous null findings in the literature (e.g., Elwert et al., 2023). Specifically, our findings suggest that out-group exposure can affect political behavior even when underlying preferences and attitudes remain unchanged. This mechanism is particularly relevant in the context of immigration, where preferences tend to be highly stable over the life course (Kustov et al., 2021). Future research should pay equal attention to changes in issue preferences and salience, as the latter appears more malleable in adult voters and thus more likely to precipitate political changes over the short- and medium-term (Sears and Funk, 1999; San Miguel et al., 2002; Prior, 2010).

Second, our findings shed new light on the contextual conditions in which out-group exposure triggers racial threat dynamics. Specifically, our findings align with and extend prior evidence presented by Hopkins (2010) on how nationwide public narratives can shape the political effects of demographic changes and out-group exposure. In the setting we study, the far-right has successfully manufactured a public narrative in which diversity in public pools is linked to violent crime. Our findings demonstrate that in a highly politicized public social space where immigrants are construed as threatening, repeated exposure to out-groups

can trigger racial threat dynamics at the local level. More generally, our results underscore the crucial moderating role of political actors in shaping natives' interpretations of intergroup interactions (Green et al., 1998). In addition, our results align with prior research that has established misperceptions about immigrant crime as one of the most potent drivers of xenophobia in advanced democracies (Ajzenman et al., 2023).

Third, we build on and extend prior research on the political effects of everyday intergroup encounters in multiethnic democracies. Prior work on intergroup contact has mainly studied comparatively favorable settings, which has contributed to a growing consensus in the literature that "contact typically reduces prejudice" (Pettigrew and Tropp, 2006; Paluck et al., 2019). Exposure to successful, high-status out-groups such as doctors (Weiss, 2021), students (Corno et al., 2022), and scientists (Unan, 2023), for example, has been shown to shift out-group attitudes in a positive direction. This is particularly true when contact occurs under favorable contextual conditions, i.e., when groups cooperate in pursuit of a common goal (Allport, 1954) such as, for example, in the workplace (Andersson and Dehdari, 2021). However, virtually all European democracies are characterized by persistent economic and social inequality between ethnic groups, such that non-European immigrants tend to be underrepresented in high-status, well-paid occupations (Heath and Brinbaum, 2014; Ingwersen and Thomsen, 2021). Going beyond prior work, we study the effects of exposure to out-groups in settings that bring together a much larger, more representative share of the local native and immigrant population, spanning various social and demographic groups. In contrast to the positive effects of intergroup contact under favorable conditions, our findings suggest that repeated 'casual contact' without cooperation can galvanize rather than mute out-group prejudice. We demonstrate that these negative effects not only materialize when exposure is short-term and fleeting (Enos, 2014; Hangartner et al., 2019; Dinas et al., 2019) but in settings where intergroup encounters occur on a regular, repeated basis. Contrary to Enos (2014), we do not find evidence that negative reactions diminish with repeated exposure over longer periods of time.

Fourth, our study advances the research frontier with respect to external and internal validity. Most prior work on intergroup contact and out-group exposure

falls into one of two categories: first, a large and growing number of field experiments have greatly improved our understanding of the effects of intergroup contact by providing credible causal estimates under experimental conditions. However, these studies often only cover a small subset of the relevant population, for example, individual cities (Enos, 2014; Broockman and Kalla, 2016; Scacco and Warren, 2018; Condra and Linardi, 2019; Mousa, 2020) or specific demographic groups, most prominently college students (Paluck et al., 2019). To what extent these results can be generalized to a broader set of geographies and demographic groups remains unclear. Conversely, while some observational studies use countrywide or cross-national data (Pettigrew, 1997), the internal validity of such studies is often limited due to strong identification assumptions ('selection on observables'). Our study brings together a high degree of both internal and external validity in that our analysis (i) covers the entirety of Europe's largest economy over a time span of two years and (ii) uses a quasi-experimental approach to causally identify the political effects of repeated out-group exposure.

Finally, our study provides new insights into the drivers of far-right support in regions characterized by high ethnic diversity. A large literature on the German case has focused on explaining the higher levels of far-right support and xenophobia in East Germany, where ethnic diversity is comparatively low (e.g., Pesthy et al., 2021; Arzheimer, 2023). However, the far-right likewise enjoys substantial electoral support in West Germany. In fact, due to its overall larger population, West Germany accounted for about two-thirds of all AfD votes in the 2021 federal election. Our findings advance our understanding of the nationwide appeal of the far-right and underscore the importance of heterogeneous drivers of far-right support across diverse geographic contexts.

2 Theoretical framework

Our theoretical framework links exposure to diversity in public social spaces to far-right electoral support. We posit a multi-step argument.

First, immigrant-native encounters in public social spaces can increase the perceived salience of immigration. Highly frequented social spaces such as public swimming pools bring together a large share of the local native and immigrant

population and thereby directly expose native voters to a snapshot of ethnic diversity at the local level. This is particularly meaningful in settings where exposure to diversity in other aspects of native voters' everyday life is limited, for example, when housing and labor markets are segmented along ethnic lines (Charles, 2003; Elbers, 2021; Hopkins, 2010). In high-diversity localities, repeated intergroup encounters in public social spaces can elevate native voters' attention to immigration, multiculturalism, and demographic change as critical political issues. We provide descriptive evidence in support of this piece of our argument in Table S5 in the SI, where we show that pool visitorship is associated with higher estimates of diversity at the local level.

In settings where a large share of the electorate holds anti-immigrant attitudes, increased immigration salience benefits far-right parties. This step of our argument draws on the intuition provided by standard spatial voting models, where voters choose between parties based on the proximity of their own preferences to the positions of the parties on various issues (Downs, 1957; Enelow and Hinich, 1984; Fournier et al., 2003; Hatton, 2021). Following this framework, raising the issue salience of immigration effectively increases the weight that voters attach to their immigration attitudes when they choose between different party platforms. Conversely, the relative weight of concerns about other policy issues—notably taxes, transfers, and economic concerns—is reduced. Exposure to diversity, in other words, results in a shift in the relative salience of cultural issues (2nd dimension politics) and economic concerns (1st dimension politics) (Kriesi et al., 2012). The higher the salience of immigration, the more likely voters become to embrace parties that align with their immigration preferences, regardless of their positions on other issues.

Drawing on prior research, we argue that raising the issue salience of immigration generally benefits far-right parties due to two mutually reinforcing mechanisms. First, anti-immigrant attitudes are generally more widespread than electoral support for far-right parties. In a representative survey from 2023, for example, about two-thirds of Germans expressed the view that immigration primarily has negative effects on Germany (ARD, 2023). In settings where anti-immigrant attitudes are widespread, raising the issue salience of immigration will benefit far-right parties, as a higher share of the electorate will express their anti-immigrant

preferences at the ballot box by voting for the far-right. Second, far-right parties, in many cases, combine xenophobic, anti-immigrant positions with economic policies that run diametrically opposed to most of their constituents' interests (Hacker and Pierson, 2020). For the 2021 federal election, the German AfD, for example, ran on an anti-immigrant platform combined with tax policies that would have disproportionately benefited high-income voters (FAZ, 2023). To the extent that economic policy positions that run counter to the preferences of most voters are de-emphasized, far-right party platforms become comparatively more appealing.

This argument builds on the 'galvanizing' perspective in prior research on anti-immigrant attitudes and support for the radical right (Claassen and McLaren, 2021). Cues of cultural and racial threat—such as exposure to ethnic diversity in public social spaces in our setting—can galvanize voters with preexisting anti-immigrant preferences (Blalock, 1967; Kustov et al., 2021). In line with this argument, prior research has established immigration salience as a key predictor of far-right success and electoral support in a variety of contexts (Ivarsflaten, 2008; De Vries et al., 2013; Dennison, 2020). Far-right parties benefit as voters with pre-existing racial stereotypes and anti-immigrant preferences attach greater weight to these preferences when choosing between different party platforms and, therefore, become more likely to cast their ballots for far-right parties and candidates. While shifts in immigration preferences rather than salience are an alternative, plausible mechanism that may link intergroup encounters in public social spaces to far-right support, prior research suggests that immigration attitudes are highly stable over the life course (Kustov et al., 2021; Claassen and McLaren, 2021). Against this background, we do not expect exposure to diversity in public social spaces to lead to persistent shifts in the underlying preferences of voters.

Prior research has shown that politicization at the national level can amplify the dynamics we outline above. Hopkins (2010), for example, demonstrates how the effects of demographic shifts on immigration attitudes are moderated by nationwide trends in public discourse and politicization. Similarly, Green et al. (1998) contend that the link between material deprivation and xenophobia is moderated by political (xenophobic) campaigns. Building on this line of research, we argue that exposure to diversity is particularly likely to trigger anti-immigrant sentiment

in spaces where diversity is perceived as threatening. In the context we study, intergroup relations in public pools have become increasingly politicized in recent years. The far-right has created a public narrative linking diversity in public pools to violent crime—a narrative that is frequently replicated in the local and national news media. As a result, a sizable share of the German electorate perceives diversity in public pools as potentially threatening and dangerous (see Sections 3 and S2). We elaborate on the politicization of public swimming pools in the German context in Sections 3 and S2.

Finally, we note that in a small number of cases, native voters may be directly exposed to social norm violations attributed to immigrants in public social spaces, which may increase support for far-right parties. In a highly frequented, dense public social space such as public pools, social norm violations ranging from minor deviant behavior to criminal offenses will generally occur (Groff and McCord, 2012). In the setting we study, violent crimes are extremely rare, and the majority of offenders are German citizens (see Section 3). However, prior research in social psychology has revealed strong in-group biases in how norm violations are perceived and sanctioned. For the same norm violation, out-group offenders are generally sanctioned more harshly than in-group offenders (Bernhard et al., 2006). Crucially, the sanctioning of norm violations attributed to individual out-group offenders is often not limited to the original offender but can translate into heightened prejudice and animosity against the out-group as a whole that receives 'collective blame' (Lickel et al., 2006). We consider this an alternative mechanism that may link the presence of public pools to far-right support in a small subset of cases.[2]

3 Context: public pools in Germany

Public pools in Germany attract large and frequent visitorship across social and ethnic groups, particularly during the summer outdoor pool season. According to a recent survey, 67% of Germans at least occasionally visit public pools in their

[2]Table S6 shows that voters who self-report having witnessed a crime in a public pool are more likely to support the far-right AfD. However, this effect pertains to only a small minority of respondents: while the majority have heard about crimes in public pools through the news media, only 18% self-report to have personally witnessed such incidents (see Table S2). Direct exposure to social norm violations is thus likely not the main mechanism driving our results.

free time (Statista, 2021b). Visitorship is slightly higher among women than men and highest among families and young adults (Statista, 2021c,a). In contrast to the United States, where following the desegregation of public spaces, including pools, Whites turned to private pools and swim clubs (Wiltse, 2007; McGhee, 2022), private pools are still relatively uncommon in Germany. The total number of private pools per capita is about 1 per 100 residents in Germany (BSW, 2020).

Figure 1: AfD campaign ad.

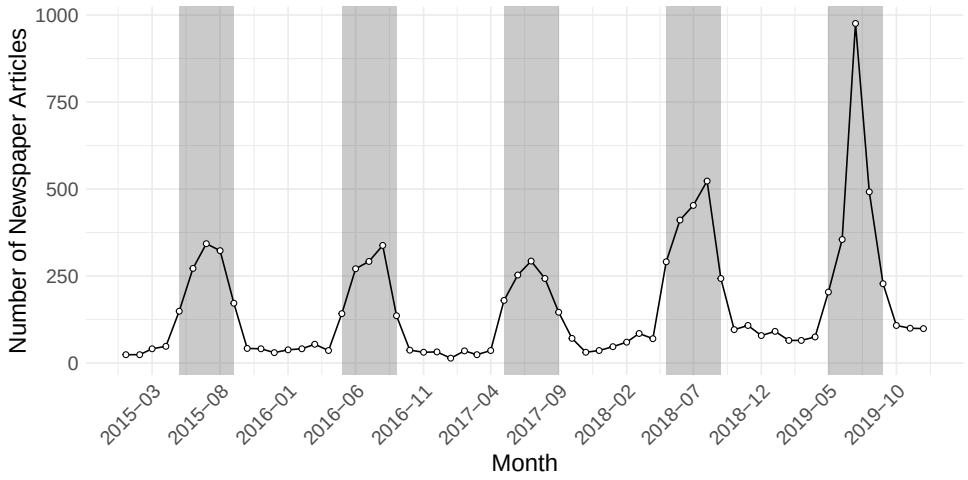


Notes: English translation: "Protect outdoor pools with deportations." AfD campaign ad was retrieved from (Tagesschau, 2023).

In recent years, intergroup relations in public pools have become an increasingly salient political issue in Germany. The AfD, Germany's main far-right party, campaigns on the notion that diversity in public pools constitutes a threat to public safety (see, e.g., Figure 1). A key component of the AfD's political messaging is allegations of frequent sexual assaults in public pools committed by foreign offenders. Crime in public pools is portrayed as pervasive and increasing over time. The narrative of increasing diversity in swimming pools as a threat to public safety is spread through political messaging both online and offline (see, e.g., AfD, 2023a). In 2019, for example, the party's chairwoman proclaimed that swimming pool riots by "aggressive migrants" are a symptom of "partial state failure" (AfD, 2019). In 2023, the AfD introduced a newsfeed that features alleged incidents of violence in

public pools involving foreign perpetrators (AfD, 2023d). The party also spreads news about incidents involving "foreign looking" suspects through its account on the platform X (formerly known as Twitter) (see, e.g., AfD, 2023b,c).[3] This narrative is frequently replicated in the local and national news media, which frequently reports about violent crime in public pools. It thus contributes to the perception that this constitutes a systemic and increasingly worsening issue (Tagesschau, 2023; Spiegel, 2023). Figure 2 illustrates trends in media coverage of violence in public outdoor pools between 2015 and 2019. Media coverage of violence in public outdoor pools is frequent and concentrated during the outdoor pool season (May – September).

Figure 2: Media coverage of violence in German outdoor pools.



Notes: The figure shows the monthly number of newspaper articles in Germany discussing violence in public pools. To identify these articles, we use *Genios* newspaper archive and search for the mention of "Freibad" (outdoor pool) in conjunction with one of the following keywords: "Ausschreitung" (disturbances), "Prügelei" (brawl), "Gewalt" (violence), *or* "Randale" (riot). The outdoor pool season (May – September) is shaded in gray.

As a result of the politicization of public swimming pools in the public discourse, a sizable share of the German electorate perceives diversity in public pools as negative and threatening. In a recent survey, 58% of Germans reported worries about safety in public swimming pools (Zeit, 2023). To examine threat perceptions linked to diversity in public swimming pools more systematically, we fielded a representative survey during the outdoor pool season in 2024. We find that the vast

[3]The AfD account has 231,000 followers on the platform X as of January 2024.

majority of voters have never witnessed any crime in public pools (82%). However, more than 70% of respondents indicate that they have heard about such crimes in the news media (Table S2). On average, respondents overestimate the share of violent crimes in public pools committed by foreign offenders by a factor of around two (Figure S6). About one-third of respondents indicate that they "increasingly feel like a stranger in their own land" and that they "feel uncomfortable because of many foreigners" in public pools (Table S3). These threat perceptions appear to be linked to politicization by the far-right, as respondents who indicated that they have "heard about" crimes in public pools in the news media exhibit significantly higher levels of threat perceptions (Table S4). Finally, we show that merely priming people to think about their personal experiences in public pools increases far-right support in high-diversity localities (see Figure S7). We discuss this survey evidence in more detail in Section S2 in the SI.

Crucially, there is a striking mismatch between perceptions and reality regarding the frequency of violent crime in public pools. Public safety concerns are widespread, although these incidents are generally rare (Spiegel, 2023). While nationwide data on the frequency of crimes in public pools are not available, we can estimate their frequency based on data published individually by 15 out of 16 German state police units for the year 2022.[4] In these states, accounting for around 99% of the total German population and representing 95% of all public pools, a total of 9,398 crimes in public pools have been recorded for the year 2022. Out of these cases, most are related to non-violent crimes (76%). Most offenders are German citizens, who account for about 69% of all reported crimes in states where information on suspects' nationality is available. This number is slightly higher than the share of Germans among offenders for all violent crimes in Germany (61%). Based on these data, we estimate the total number of violent crimes committed by foreigners in German public pools to fall between 700 and 900. This implies that at least 80% of public pools do not experience any violent crimes committed by foreigners in a given pool season. It is important to note that this is a lower bound estimate and does *not* imply that 20% of pools do experience violent out-

[4]See SI S1.1 for details on the underlying calculations and back-of-the-envelope estimation. We note that data are available for fewer states prior to the pandemic (2019). The number of violent crimes in public pools has not increased in recent years but is relatively stable over time.

group crime. Given the spatial and temporal clustering of crime events, the share of public pools that experience them is likely substantially lower.

4 Data and empirical strategy

4.1 Data sources

Below, we describe the data sources we use to measure (1) the presence of public swimming pools, (2) electoral support for the far-right, and (3) ethnic diversity. We present summary statistics for our data in Table S36 in the SI.

Pool locations: We collected information on the precise geolocation of all public swimming pools in Germany. We obtained these data from *Bäderleben*, a public research project led by researchers at *Hochschule Koblenz*. In total, we observe the precise location of 4,669 public pools, of which 3,164 are outdoor pools (*Freibad*), and 1,505 are indoor pools (*Hallenbad*). We provide more details on the swimming pool data in Section S3.1 in the SI.

Vote intentions: To measure support for the far-right AfD, we draw on a large-scale panel survey collected by the survey company *Civey*. Respondents are asked which party they would vote for if there were a federal election next Sunday ("Sonntagsfrage"). We observe more than one million responses from about 240,000 unique survey respondents interviewed between January 2017 and December 2018. For each respondent, we also observe the interview date and zip-code region. Zip-codes are small geographic units that roughly correspond to municipalities in size. Germany has about 8,000 zip-code regions with an average size of about 43 square kilometers. We provide additional details on how Civey recruits and interviews survey respondents in Section S7 in the SI.[5] The survey data's temporal and spatial granularity allows us to closely trace political attitudes in localities with and without outdoor pools at the local level over time. To examine the external validity of

[5]Because our empirical strategy relies on changes in political preferences within the same individual over time (i.e., including respondent fixed effects), we discard all respondents for whom only a single survey response is observed during our study period.

the survey-based results, we additionally use precinct-level electoral returns in a supplementary analysis (see Sections 5.1 and S5 for more details).

Local ethnic heterogeneity: Because data on race or ethnicity are not collected in Germany, we use citizenship as a proxy. Specifically, we measure the share of non-EU immigrants (foreigners) at the county level.[6] We match each zip-code region to its corresponding county and translate the continuous foreigner share variable to a binary indicator for respondents in localities with 'high' non-EU immigrant shares. For our main specification, we use the 75th percentile value in the survey sample as the cutoff. We thus compare respondents in the upper quartile w.r.t. local diversity to respondents in localities with low non-EU immigrant shares. In Figure 4, we present the results for varying cutoffs for the 'high' immigrant share subset.

We intentionally use data collected before the large-scale influx of predominantly female, White Ukrainian (non-EU) refugees into Germany beginning in early 2022. Prior research indicates that native Germans hold substantially more accepting attitudes toward Ukrainian/female refugees compared to male immigrants from Muslim-majority or African countries (Ward, 2019; Moise et al., 2024). We use county-level data because more granular information on the spatial distribution of foreigners is only available for the year 2011 when the last census prior to the Ukrainian influx was conducted. We do not use these data for our main analysis because (i) the foreign population was substantially smaller and (ii) the composition of the foreign population was different, with a much lower share of non-EU foreigners (in particular refugees) in 2011. However, using the 2011 census data, we show that foreigner shares at the county- and zip-code level are strongly correlated (see Figure S4). We also note that our measure does not cover 2nd generation immigrants with German citizenship. Data on the share of the local population with a 'migration background' (at least one foreign-born parent) are not available for all counties. However, we can show that within the subset of localities for which data are available, the share of non-EU foreigners and the

[6]We use data for the year 2021. In Table S21, we demonstrate that our results remain unchanged when using data for the year 2018 instead of 2021. We do not use these data for our main analyses because it is missing for a small number of counties (primarily in the state of Saarland). As of 2018, about six million non-EU foreigners resided in Germany. The five largest countries of origin are (1) Turkey, (2) Syria, (3) Afghanistan, (4) Russia, and (5) Iraq.

share of the local population with a migration background are very highly correlated ($r = 0.88$, see Figure S2). Using 2011 census data that are available for a subset of large municipalities, we show that this correlation is similarly high at the municipality level ($r = 0.91$, see Figure S3).

4.2 Estimation

Our goal is to estimate the causal effect of exposure to ethnic diversity in public swimming pools on far-right electoral support. To do this, we leverage the seasonality of outdoor swimming pools for causal identification in a difference-in-differences design. In a nutshell, we compare changes in electoral support for the far-right among respondents living in localities with outdoor swimming pools ('treated' group) to respondents living in localities without outdoor swimming pools ('control' group) before and after the onset of the outdoor pool season. To do this, we estimate standard OLS difference-in-differences specifications of the following form:

$$Y_{i,t} = \alpha_i + \gamma_t + \tau \text{Pool season}_t \times \text{Outdoor pool}_k + \varepsilon_{i,t} \quad (1)$$

where $Y_{i,t}$ is a binary indicator that equals one if respondent i interviewed at time t in zip-code region k indicates a vote intention for the AfD. Outdoor pool$_k$ is a binary indicator for the presence of an outdoor pool in the zip-code of a given survey respondent. Pool season$_t$ is a binary indicator that equals one if the interview date t falls in the outdoor pool season that ranges from May to September.[7] Our main quantity of interest is τ, which identifies the differential change in far-right support during the outdoor pool season in zip-code regions with outdoor pools relative to localities without outdoor pools. As noted in Section 4.1, we estimate this specification separately for localities with high and low levels of ethnic diversity. Following our theoretical framework, we only expect statistically significant effects when local ethnic diversity is sufficiently high.

α_i and γ_t indicate respondent and time (interview-day) fixed effects. We note that because we include respondent fixed effects in our specification, we leverage changes in electoral preferences within the same individual over time. Over-time

[7] The vast majority of outdoor pools open in May for the summer season and close in September. We demonstrate this in Figure S5 in the SI, where we visualize information on outdoor pool opening/closing dates. This information is available for 57% of pools in our dataset.

changes in the composition of the survey sample are therefore not a concern in our empirical setup. We do not estimate the non-interacted coefficients for pool season and outdoor pool because they are absorbed by (perfectly collinear with) the respondent and time fixed effects. For our main specifications, we cluster standard errors at the level of respondents as we observe repeated observations of the same individuals in a panel. Our results remain unchanged when we use standard errors clustered at the zip-code (see Table S8) or county level (see Table S9).

4.3 Causal identification

The key identifying assumption underlying our difference-in-differences design is parallel trends in localities with and without outdoor swimming pools if outdoor swimming pools had remained closed during the outdoor pool season. While we cannot directly test this assumption, we demonstrate parallel trends in the treated and control groups prior to the outdoor pool season in Figure 3. We do not find statistically significant differences in the trends in far-right support between localities with and without outdoor pools before the onset of the outdoor pool season. Between October and April, electoral support for the far-right evolves in parallel in localities with and without outdoor pools.

To further scrutinize our identification strategy, we devised a placebo test using indoor pools as a placebo treatment. Indoor pools are open throughout the year and do not attract seasonal visitorship to the same degree as outdoor pools. In addition, indoor pools are often less recreational and more focused on activities like athletic training, swimming lessons, and structured programs. Consequently, indoor pools tend to be less crowded and attract different demographics and social groups than outdoor pools. If our identifying assumptions hold, voter preferences in localities with only indoor public pools should not follow a similar seasonal pattern as in localities with outdoor pools.

We test this prediction in Table 1. Here, we estimate similar specifications as for our main results (Table 2) but define the presence of an indoor swimming pool as the 'treatment' variable relative to the control group without any public swimming pools. Localities with outdoor swimming pools are excluded from this analysis. We find statistically insignificant point estimates of small magnitude in both ethnically heterogeneous and ethnically homogeneous localities. In other

Table 1: Indoor pools placebo test.

Non-EU Immigrant share	AfD Support (in p.p.) Full sample (1)	High (2)	Low (3)
Indoor pool × Pool season	0.005	0.203	-0.095
	(0.151)	(0.291)	(0.178)
R^2	0.894	0.893	0.893
Observations	685,334	206,517	478,817
Respondent fixed effects	✓	✓	✓
Interview date fixed effects	✓	✓	✓

Notes: Results from OLS regressions where AfD support (in p.p.) is the dependent variable. The placebo treatment is defined as the presence of a public indoor pool in a given zip-code. We exclude zip-codes with outdoor pools from this analysis. Standard errors are clustered by respondent. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1.

words, we do not find evidence that the mere presence of public pools is correlated with other seasonal determinants of political preferences.

Crucially, our identification strategy does not require that outdoor pool presence is as-if-randomly assigned across municipalities. The difference-in-differences design accommodates all time-invariant differences between the treated and control groups, i.e., between localities with and without outdoor swimming pools. The fact that outdoor pool presence is correlated with population density, for example, may result in level differences in far-right support between localities with and without outdoor pools but should not lead to differential *changes* between the two groups during the outdoor pool season. Similarly, the difference-in-differences approach accommodates all time-specific factors or shocks that equally affect both groups, such as generally higher or lower levels of far-right support during the summer.

We note that the presence of other public spaces that might have seasonal patterns in visitorship (e.g., public squares, shopping streets) and likewise affect far-right support could bias our effect estimates if their presence is correlated with the presence of public pools. We address this concern in Section S4.2 in the SI,

where we show that (i) we do not find higher levels of far-right support during the summer in localities without outdoor pools, (ii) our results hold when allowing for differential trends by population density and (iii) our results remain unchanged when we control for the presence of natural pools at seas and lakes.

For additional background information, we examine the cross-sectional predictors of outdoor pool presence in Figure S8. We analyze the predictors of outdoor pool presence relative to two baselines, namely relative to (i) localities without any public pools and (ii) localities with an indoor but no outdoor pool. In Tables S11 and S12, we show that we obtain similar results regardless of which of the two control groups we use for our analysis. We find that outdoor pools positively correlate with the presence of other public amenities (e.g., hospitals) and population density but negatively correlate with income and employment. We stress that the differences between localities with and without outdoor pools (particularly when compared to those with indoor pools) are of small substantive magnitude at less than 0.05 standard deviations.

5 Results

We present our main results in Table 2. We find a positive, statistically significant effect of exposure to ethnic diversity in public swimming pools on electoral support for the far-right. The effect magnitude of about 0.8 percentage points is meaningful relative to the overall level of electoral support for the AfD (12.6% in the 2017 federal election). We note that we obtain higher effect point estimates ($\approx$ 1.5 p.p.) when we render the survey sample more representative of the German population using weighting (see Table S23). The electoral gains for the far-right come at the expense of parties across the political spectrum, except the Social Democrats (see Table S22 in the SI). In line with our theoretical framework, we only find statistically significant effects in high-diversity localities (column 2 in Table 2). We find negative, statistically insignificant point estimates of small magnitude in low-diversity localities (column 3). We note that the difference between the effect estimates in the high- and low-diversity subsets is statistically significant ($p < 0.01$).[8] When we disaggregate the results by gender, we find substantially

[8]P-values were obtained using 250 bootstraps clustered by survey respondent. In all bootstrap iterations, the difference in effect estimates between the high- and low-diversity subsets is positive.

stronger effects among women than men (see Table S24). This finding aligns with prior research on gender differences in fears of crime (Cops and Pleysier, 2011), but may also be attributable to the explicitly gendered political campaign by the far-right, which links out-group norm violations in public pools to sexual assaults against native women (see Section 3).

Table 2: Main results.

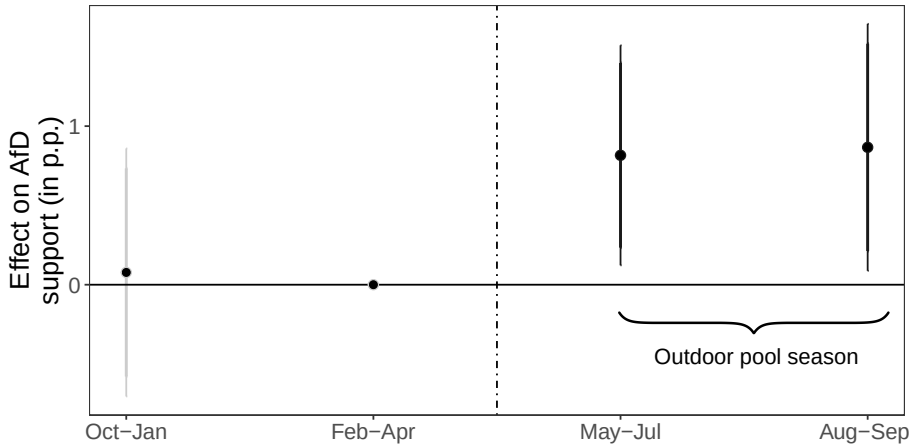
	AfD Support (in p.p.)		
Non-EU Immigrant share	Full sample	High	Low
	(1)	(2)	(3)
Outdoor pool × Pool season	0.024	0.793***	-0.144
	(0.109)	(0.283)	(0.122)
R^2	0.892	0.892	0.892
Observations	1,160,202	256,085	904,117
Respondent fixed effects	✓	✓	✓
Interview date fixed effects	✓	✓	✓

Notes: Results from OLS regressions where AfD support (in p.p.) is the dependent variable. The treatment is defined as the presence of a public outdoor pool in a given zip-code. Pool season is a dummy variable that equals one for survey responses recorded between May and September. Standard errors are clustered by respondent. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1.

In Figure 3, we zoom in on high-diversity localities using an event-study specification. As noted previously, we observe parallel trends between localities with and without outdoor pools prior to the outdoor pool season. Political preferences in high-diversity localities with and without outdoor pools diverge from May onwards, following the onset of the outdoor pool season that facilitates repeated intergroup encounters at the local level. In line with the results presented in Table 2, we observe a differential increase in far-right support in localities with outdoor pools, which lasts until the end of the outdoor pool season in September. The effects we observe persist throughout the pool season but dissipate quickly after outdoor pools close in October. This aligns with our theoretical framework that posits temporary shifts in the underlying salience of immigration (rather than long-lasting changes in underlying preferences) as the key mechanism driving these

results. Prior research suggests that issue salience is subject to strong temporal fluctuations within relatively short intervals, whereas preferences tend to be more stable over time (Kustov et al., 2021). We revisit this point and empirically examine the mechanisms underlying our results in Section 5.2.

Figure 3: Event-study estimates.

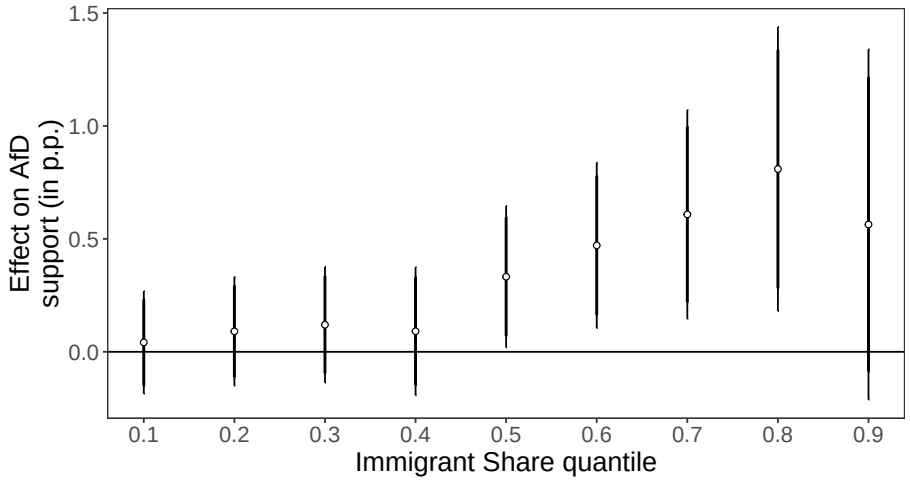


Notes: The figure shows treatment effect estimates from event-study difference-in-differences specifications. Error bars indicate 90- and 95% confidence intervals. The sample includes zip-codes in the upper quartile with respect to local ethnic diversity (non-EU immigrant share).

Next, we test a direct empirical implication of our theoretical framework: to the extent that our empirical strategy identifies the effects of exposure to ethnic diversity in public swimming pools, we would expect the strongest effects in the most ethnically diverse localities. We test this prediction through two additional tests: first, in Figure 4 we show treatment effect estimates at varying cutoffs for the 'high' diversity subset, showing that our effect estimates increase as the local share of non-EU foreigners increases. Second, we show in Table S25 that our results are specifically driven by localities with a high share of non-European foreigners, whereas we find statistically insignificant, negative point estimates for localities that only feature a high share of predominantly White, European foreigners. While these estimates should be interpreted with caution, as the local share of EU and non-EU foreigners is highly correlated across geographies, they align with prior research that has shown that Europeans hold particularly negative attitudes to-

ward immigrants from Muslim-majority and African countries (Bansak et al., 2016). Taken together, we interpret these findings as supporting evidence that our results are driven by repeated exposure to out-groups in public swimming pools.

Figure 4: Heterogeneity by local ethnic diversity.



Notes: The figure shows treatment effect estimates for varying quantile thresholds defining the 'high' immigrant share subset of respondents. We use data on the county-level share of non-EU immigrants. For each cutoff, we estimate an analogous specification as for column 2 in Table 2. Error bars indicate 90- and 95% confidence intervals.

To further scrutinize our results, we conducted a series of additional robustness checks and tests. Specifically, we (i) estimate a range of alternative specifications, including clustering standard errors at different levels, splitting the analysis by year, and allowing for seasonal trends by region; (ii) address concerns about spillovers by narrowing down the sample, and (iii) demonstrate that seasonal concerns about objective crime rates do not explain our findings. We discuss these tests in detail in Section S4 in the SI.

5.1 External validity

A potential concern about our empirical analysis is that the *Civey* sample is not representative of the overall German population (see Table S36). We note that because we leverage over-time changes at the individual level in a panel setup, this should only affect the external but not internal validity of our results. However,

it might be the case that the sample average treatment effect we identify is not a good proxy for the average treatment effect in the German population. To address this concern, we repeat our main analysis using survey weights that render the weighted sample representative for the joint population distribution of (1) age, (2) gender, (3) education, and (4) state. We present results using survey weights in Table S23 in the SI. Our substantive conclusions remain unchanged when we use the weighted sample. In fact, we find larger effect estimates in the weighted sample (about 1.5 p.p.). This is likely because women are underrepresented in the raw sample, yet we observe stronger effects among women (see Table S24). This suggests that our main analysis, if anything, underestimates the effects of out-group exposure in public pools for the German population overall.

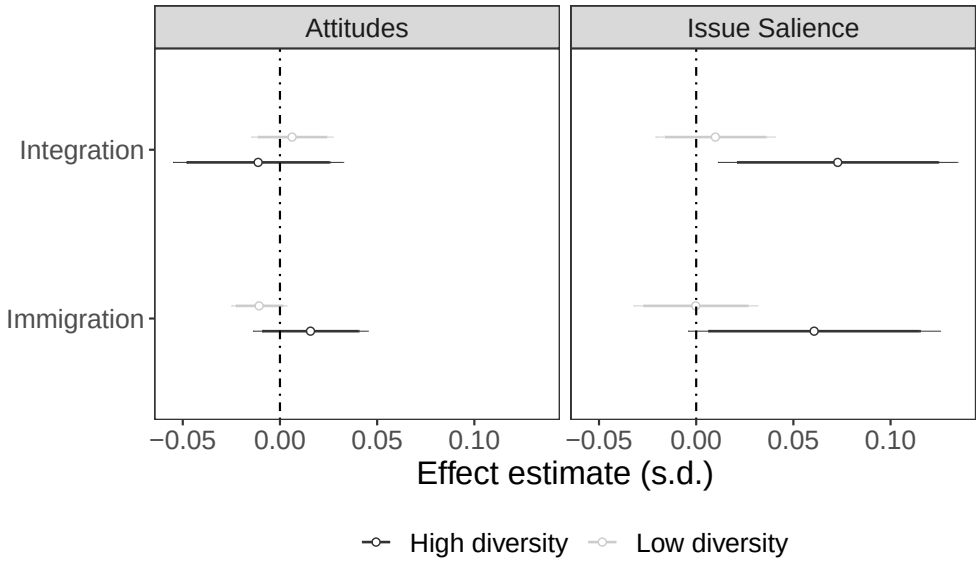
We further corroborate the external validity of our survey results through a supplementary analysis of electoral returns from the 2017 federal election, held in late September at the end of the outdoor pool season. Focusing on Berlin and Hamburg—two cities where diversity is high and thus "most likely cases"—we analyze the relationship between the local presence of outdoor pools and far-right electoral support. We conduct a cross-sectional matching analysis comparing precincts with outdoor pools to similar precincts without pools (see Section S5 for details). We find a statistically significant, positive association between the local presence of outdoor pools and far-right electoral support in both cities (see Table S29). We interpret these results as supporting evidence that the effects we find in our survey data extend to actual electoral behavior. However, it should be noted that this analysis is based on cross-sectional data and thus requires substantially stronger assumptions for causal identification than our main analysis.

5.2 Mechanisms: shifts in attitudes or issue salience?

Next, we examine the mechanisms driving the connection between exposure to diversity in public swimming pools and far-right voting. Specifically, we examine whether the shifts in far-right support we observe are driven by individual-level changes in (i) immigration attitudes or (ii) the perceived salience of the immigration issue, i.e., the weight that voters place on immigration when choosing between different party platforms (see Section 2). We draw on another panel survey for this

analysis, the German Longitudinal Election Study (GLES, 2023). We estimate analogous difference-in-differences specifications as for our main results for four main outcome variables: (i) issue salience of immigration, (ii) issue salience of foreigner integration, (iii) attitudes toward integration (multiculturalism vs. assimilation) and (iv) immigration attitudes. We provide additional details on the data source, survey items, and estimation in Section S6 in the SI.

Figure 5: Effects on issue salience and immigration attitudes.



Notes: Results from OLS regressions. We use panel survey data from the German Longitudinal Election Study (GLES). All outcome variables are standardized. See Table S30 for the original survey items and scales. The treatment is defined as the interaction between the presence of an outdoor public pool and the pool season. Standard errors are clustered by respondent. Results in tabular form are provided in Tables S31 and S33 in the SI. Error bars indicate 90- and 95% confidence intervals.

We present the results in Figure 5. In line with our theoretical framework, exposure to ethnic diversity in public pools increases the salience of the immigration issue but does not shift immigration attitudes (black estimates). At the same time, the relative weight of other issue areas, particularly taxes and redistribution, decreases (see Table S32 in the SI). This relative increase in the importance of cultural over economic concerns benefits far-right parties such as the *Alternative für Deutschland*, which, similar to other right-wing populist parties, runs on an

economic platform that disproportionately benefits the rich (Hacker and Pierson, 2020; FAZ, 2023). More generally, these results align with a large body of prior research highlighting immigration salience as a key driver of far-right support (Ivarsflaten, 2008; De Vries et al., 2013; Dennison, 2020) and the stability of immigration attitudes (Kustov et al., 2021).

As expected, we do not find statistically significant effects for any outcome variable in low-diversity localities (light gray placebo estimates). To further scrutinize the validity of these results, we conducted two additional placebo tests. First, similar to the results presented in Table 1, we demonstrate null effects for a placebo treatment using the presence of indoor pools (Table S34). Second, we leverage the onset of the COVID-19 pandemic, which resulted in drastically reduced visitorship of public pools. We demonstrate null results in the survey years 2020/2021 in Table S35.

6 Discussion

This chapter provides evidence that exposure to ethnic diversity in public social spaces can galvanize racial threat perceptions among native voters and thereby render them more likely to support far-right parties. Our findings show that outgroup exposure can alter electoral outcomes even when underlying preferences remain unchanged, introducing a new mechanism linking intergroup encounters to political behavior. Overall, our results lend credence to the racial threat hypothesis (Blalock, 1967) in settings that do not meet Allport's 1954 conditions for positive intergroup contact.

An important limitation of our study is that we cannot directly observe intergroup encounters in public swimming pools. Instead, we proxy exposure to ethnic diversity using aggregate-level measures, specifically the regional proportion of non-EU foreigners. While this approach is admittedly coarse, we consider it the best viable alternative in the context we study. Individual-level data on pool attendance, which would allow us to measure ethnic diversity at the pool level more precisely, is not systematically collected nationwide. In addition, Germany does not collect data on ethnicity for historical reasons—neither at the local nor national level—limiting our ability to precisely measure ethnicity. Consequently, we rely

on non-EU citizenship as a proxy. While we regard this approach as the best available option, we acknowledge that it may only approximate the ethnic composition of public pools and may introduce some degree of measurement error.

Further research is necessary to understand the scope conditions of the effects we find, particularly whether they depend on the politicized nature of the public space we study. Our findings provide suggestive evidence that strategic politicization by the far-right—a previously understudied moderating factor—may shape the effects of intergroup interactions. The politicized nature of the public space we study may explain why our results differ from Enos (2014), who finds suggestive evidence that the negative effects of out-group exposure in public spaces diminish with repeated exposure over time. In contrast, our findings align with prior research showing that intergroup dynamics can be moderated by public discourse and politicization (Green et al., 1998; Hopkins, 2010). Future research should explore whether similar dynamics extend to other public spaces. In many countries, including Germany, public parks may exhibit similar dynamics because they (i) similarly facilitate repeated exposure to diversity in a public social space, (ii) are linked to out-group crime in the news media, and consequently (iii) diversity in parks is perceived as similarly threatening as in public pools (see Table S3). We do not expect effects of similar magnitude for non-politicized spaces such as supermarkets, where diversity is perceived as less threatening, and exposure is thus less likely to trigger anti-immigrant sentiment. Although examining additional public spaces is beyond the scope of this chapter, it represents a promising avenue for future research.

Crucially, our findings should not be misinterpreted as advocating for divestment from public pools or ethnic segregation in public social spaces. On the contrary, they highlight the need for more research on effective policy solutions to foster positive social interactions between ethnic groups in these spaces. First and foremost, however, improving intergroup relations in public spaces will require a reduction of negative racial stereotypes among natives, without which false narratives about allegedly pervasive immigrant crime in public spaces could not resonate with large shares of the European electorate.

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Chapter 3

Public Norms and Cross-Group Spillovers*

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Abstract

When a highly visible political event shifts public discussion about one form of prejudice, do its effects spill over to attitudes toward other marginalized groups? I argue that public shocks can act as norm signals about the acceptability of prejudice more generally, with effects extending beyond the group directly affected. Drawing on around 500,000 interviews from Nationscape, I use a regression discontinuity in time around the May 2020 protest wave following the murder of George Floyd. Expressed favorability rises sharply at the cutoff not only toward Black Americans but also toward Asians, Latinx individuals, Muslims, and the LGBTQ community. The shifts are concentrated in Democratic-leaning counties, including among Republican respondents in those areas, consistent with local context reinforcing the national signal. The results suggest that political shocks can move expressed group attitudes through cross-cutting norm channels rather than only through group-specific information.

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1 Introduction

When a highly visible political event shifts public discussion about one form of prejudice, do its effects stop at the group directly affected, or do they spill over to expressed attitudes toward other marginalized groups? Most work on the political effects of high-salience violence and protest studies one group at a time (Walker et al., 2020; Reny and Newman, 2021; Roman and Thompson, 2025; Crabtree and Yadon, 2024; Crabtree and Roman, 2024). The premise of this chapter is that public conflict over group inclusion is rarely organized around fully isolated attitudes. Public arguments about prejudice and the legitimacy of exclusion shape the climate in which group attitudes are expressed (Kuran, 1995; Mendelberg, 2001; Tankard and Paluck, 2017; Arias, 2019; Bursztyn et al., 2020). A public shock that changes how citizens understand the norm governing one form of prejudice may also change how they evaluate adjacent forms.

The murder of George Floyd in May 2020 provides a useful setting for studying this possibility. Floyd's murder triggered one of the largest protest waves in recent American history and generated demonstrations beyond the United States (Buchanan et al., 2020; Smith et al., 2020). The protests also affected attitudes toward police and perceived anti-Black discrimination, especially among low-prejudice and politically liberal Americans (Reny and Newman, 2021). While much of the existing work on the protests focuses on the group directly affected by the event, the public reaction to Floyd's murder was framed in broader moral and political terms: mass protest and public support for racial-justice mobilization, alongside visible statements by companies and higher-education institutions about racism and racial inequality (Parker et al., 2020; Long and McCarthy, 2020; Anderson and McClain, 2020; Casellas Connors and McCoy, 2022; Toraif et al., 2023; Brown et al., 2024).

This chapter argues that the reaction to Floyd's murder could generate spillovers across adjacent group attitudes. The claim concerns the normative and interpretive context in which citizens express attitudes toward a wider set of marginalized groups, rather than durable change in deep private preferences (Kuran, 1995; Alvarez-Benjumea, 2023; Valentim, 2024). The public response to

the original event matters as much as the event itself: when that response is highly visible and sufficiently validated by influential public actors, citizens may update their beliefs about the norm governing intergroup attitudes more generally. The claim is closest in spirit to Bursztyn et al. (2020) on elite norm-shifting and to Mendelberg (2001) on the norm of equality. The empirical question is whether such a signal can travel across group boundaries when it operates from below rather than from elite messengers.

I use *spillover* throughout this chapter to label an empirical pattern of coordinated movement in expressed favorability across several marginalized groups not directly named by the event, rather than a particular mechanism for how that movement is produced.[1]

Two mechanisms are consistent with such a pattern. The first is strict propagation from the focal group to adjacent ones, and the second is a common inclusive frame that moves several group evaluations together once the public reaction is nationally visible. The design cannot distinguish between these mechanisms, but documenting the spillover pattern does not depend on doing so.

The chapter makes three contributions. First, it documents the pattern itself. A public shock organized around one marginalized group shifted expressed favorability toward several other marginalized identity groups not directly named by the event. Most prior work on the political effects of high-salience violence and protest studies one group at a time and is therefore not positioned to recover this pattern (Walker et al., 2020; Reny and Newman, 2021; Roman and Thompson, 2025; Crabtree and Yadon, 2024; Crabtree and Roman, 2024). Second, it shows that the pattern is concentrated where the local partisan environment was likely to validate the national signal, including among Republican respondents in Democratic-leaning areas, suggesting that the same national event lands very differently in different local contexts (Hopkins, 2010; Paluck and Shepherd, 2012). Third, it connects work on racial violence and protest to research on cross-group attitude generalization (Pettigrew, 2009; Tesler, 2012, 2016), by showing that public signals can shift ex-

[1]This is the same pattern-vs-mechanism distinction used in the literature on issue and racial spillover (Tesler, 2012, 2016), where the term names a generalization across attitudes while the channel through which that generalization is produced (analogy, norm extension, perceived linked fate, or a pre-existing cross-group frame) is a separate empirical question.

pressed evaluations beyond the domain of the original event, and by clarifying which kinds of political change the evidence does and does not support.

The chapter forms part of a broader dissertation on political behavior during episodes of contention. In the framing of the dissertation, the relevant existing political material is the normative environment around intergroup expression, and the relevant moderator is the local partisan context in which respondents encounter the national signal.

I examine these questions using Nationscape, a large-scale public opinion survey fielded in weekly waves that includes around 500,000 interviews between July 2019 and February 2021 (Democracy Fund Voter Study Group, 2023). Its scale and the availability of interview dates allow me to trace short-run changes in expressed group attitudes around May 28, 2020, when public attention to the protests rose sharply across search, television, and newspaper data. I exploit the unexpected timing of the protest wave using a regression discontinuity in time. The design follows Reny and Newman (2021), who use a similar approach to test for direct effects on attitudes toward Black Americans and the police. Going beyond their direct-effect analysis, I estimate the discontinuity in expressed favorability for five outcomes (Black Americans, Asians, Latinx individuals, Muslims, and the LGBTQ community), test placebo outcomes (Whites and Socialists), and compare the discontinuity across local partisan environments and respondent partisanship.

Expressed favorability increases toward Black Americans and toward several other minority groups in the immediate aftermath of the Floyd protests, with larger effects in Democratic-leaning areas for both Democratic and Republican respondents. The pattern is consistent with the protest wave operating as a broader public signal about acceptable prejudice, rather than only as new information about anti-Black discrimination. The rest of this chapter is structured as follows: Section 2 develops the theoretical argument. Section 3 describes the data and research design. Section 4 presents the results. Section 5 reports a series of robustness checks. Lastly, Section 6 concludes.

2 Theoretical framework

Attitudes toward minority groups depend on private beliefs and on beliefs about what others consider acceptable (Bicchieri, 2017; Kuran, 1995; Tankard and Paluck, 2016). This is especially true for attitudes toward stigmatized groups, where expression is social and potentially sanctionable (Ivarsflaten et al., 2010; Blinder et al., 2019; Alvarez-Benjumea and Valentim, 2024). Citizens rarely observe the full distribution of opinion around them directly; instead, they infer it from public signals such as elite statements, media coverage, and institutional decisions (Tankard and Paluck, 2017; Arias, 2019; Clayton et al., 2021). These signals may contain both descriptive information, about what many others appear to think, and injunctive information, about what kinds of expression will be approved or sanctioned. Public events can therefore matter politically by altering what citizens think others expect them to say and do, even when underlying preferences do not change (Kuran, 1995; Mendelberg, 2001; Bursztyn et al., 2020; Valentim, 2024).

The same logic is especially relevant for prejudice because prejudiced expression is normatively regulated. The same underlying disposition can be more or less readily expressed depending on context, and repeated public cues about what is legitimate can over time reshape private judgments (Sears and Henry, 2003; Bursztyn et al., 2020). I therefore do not treat social norms and attitudes as wholly separate domains. Expressed group favorability is politically meaningful in its own right. In particular, expressed favorability is what citizens reveal in surveys, and it shapes how groups are evaluated in everyday interaction.

Public shocks often carry more than one kind of information. A large protest wave conveys both that a grievance exists and that many people are willing to condemn it publicly and attach political weight to doing so. Protest in that sense works as a coordination signal, telling citizens how broad and how visible support for a particular public stance is (Lohmann, 1994; Arias, 2019). When the signal is strong enough, citizens may update their beliefs about the norm governing public expression even when private views do not move. This argument extends the elite-driven norm-shifting logic of Bursztyn et al. (2020) to a mass-level event, and it parallels the role of perceived norms documented by Tankard and Paluck (2017) and Paluck and Shepherd (2012).

Locally contained events typically register as relevant only to that specific incident or community, while a large national protest wave creates common knowledge that many others are attending to the same political claim. Framing matters separately: a reaction couched as a response to one victim or one institution should have narrower effects than one framed around racism and discrimination more generally. As discussed below, the reaction to Floyd's murder was framed in general terms rather than as a single-incident grievance (Parker et al., 2020; Long and McCarthy, 2020).

The argument also draws on a separate strand of research on the racialization of political reasoning. Tesler (2012, 2016) shows that reasoning about race, once politicized, can spill into adjacent policy and group evaluations. Pettigrew (2009) documents that group attitudes are linked rather than fully separate. The implication is that, when a national event makes one form of group prejudice politically focal, evaluations of other marginalized groups can move in the same direction without requiring direct exposure to those groups.

The reaction to Floyd's murder constituted this kind of signal. The protests were unusually large and geographically broad (Buchanan et al., 2020; Smith et al., 2020), and they generated repeated public condemnation of anti-Black racism from protesters and public-facing institutions. Polling at the time found wide public support for the protests, and many Americans reported seeing companies and organizations issue statements about race or racial inequality (Parker et al., 2020; Long and McCarthy, 2020; Anderson and McClain, 2020). Universities responded in similar ways (Casellas Connors and McCoy, 2022; Toraif et al., 2023; Brown et al., 2024). Many citizens were therefore exposed both to information about discrimination against Black Americans and to a highly visible public response that made anti-racist expression more socially validated.

The public response was also framed in cross-group terms. Protesters interviewed by the *Washington Post* described the moment in terms of linked fate across racial, ethnic, and gender-identity lines, with one queer and trans Asian-American organizer in Minneapolis telling the paper that "all of our liberation is tied together" (Vongkiatkajorn et al., 2020). Adjacent identity-group organizations made the same point publicly: more than 100 LGBTQ organizations signed an open letter joining #BlackLivesMatter during Pride Month (Morrison, 2020), the Brooklyn Lib-

eration rally for Black trans lives drew an estimated 15,000 attendees (CBS News, 2020), and the Strike for Black Lives joined racial-justice organizations with major labor unions across more than 25 cities (Service Employees International Union, 2020). Black Lives Matter co-founder Opal Tometi reinforced the same theme in mid-2020, emphasizing that the movement had always affirmed queer, trans, and immigrant communities (Joseph, 2020). This material provides qualitative evidence for the claim that the cross-group frame was present in the public discussion at the time.

If citizens respond to that signal at the level of a broader posture against prejudice, the effects need not stop with attitudes toward Black Americans. A strong public cue condemning one form of exclusion can spill over to other groups understood as plausible objects of the same norm. Citizens infer something general about the social acceptability of prejudice and apply it when evaluating adjacent groups (Pettigrew, 2009; Tesler, 2016).

As defined in the introduction, spillover refers to coordinated movement in expressed favorability across several marginalized groups in response to an event organized around one. Two mechanisms are consistent with this pattern. Under the first, a response to the group at the center of the event propagates to evaluations of adjacent groups through cognitive linkage, perceived norm extension, or analogy (Pettigrew, 2009; Tesler, 2012, 2016). Under the second, the public reaction to Floyd's murder was framed in cross-group terms once it became nationally visible: protests, adjacent identity-group organizations, and accompanying public statements made anti-prejudice and inclusive frames simultaneously available across several identity categories. Here, the response operates through a common inclusive frame that connects several marginalized groups in citizens' attention at the same time, rather than strictly following a group-specific reaction.

These mechanisms are alternative explanations for the spillover pattern, and they need not be mutually exclusive. A cross-group frame can be the channel through which a strict spillover occurs, and repeated cross-group association can over time turn a group-specific reaction into a broader inclusive norm. The design does not distinguish between these mechanisms. The analysis shows that a single national event produced a coordinated shift in expressed evaluations across sev-

eral adjacent group attitudes and identifies the local conditions that strengthen or weaken that shift.

The argument does not predict that every disliked or politically contested group benefits from the signal. Generalization is most likely for marginalized identity groups that are commonly understood as objects of anti-discrimination norms, such as those defined by race, ethnicity, religion, sexuality, or gender identity (Ivarsflaten et al., 2010; Blinder et al., 2019; Tankard and Paluck, 2017; Roman and Thompson, 2025). The empirical analysis therefore focuses on Black Americans, Asians, Latinx individuals, Muslims, and the LGBTQ community, with favorability toward Whites and Socialists reserved for placebo analysis. Whites are not a marginalized out-group in the same sense, and Socialists are primarily an ideological rather than an identity category.[2] I chose these five groups because they are commonly understood as objects of anti-discrimination norms and because Nationscape measured favorability toward them before and after May 2020.

Effects need not be uniform across groups. Spillovers should be stronger where the indirectly affected group is already legible as a marginalized identity group and where the local environment reinforces rather than contests the national signal. They should be weaker where a competing threat frame is simultaneously active for that group. Larger effects are also more likely where baseline favorability is lower or norms are less settled. Smaller effects on particular groups need not imply weaker theoretical relevance, since they may reflect ceiling effects or stronger prior crystallization. The argument requires only that at least some adjacent identity-group attitudes move in the same direction when the public signal is sufficiently broad.

The argument also does not require that the event produced a lasting shift toward more liberal private attitudes. The relevant change may lie in expressed attitudes, i.e. in what citizens feel comfortable saying when asked to evaluate different groups. This matters because public expression helps structure everyday interaction and political behavior, and norms often move more quickly than underlying preferences (Kuran, 1995; Alvarez-Benjumea, 2023; Bursztyn et al., 2020;

[2]Attitudes toward Socialists are not free of stigma. A general leftward affective shift would also move favorability toward Socialists, while an anti-prejudice signal tied to marginalized identity groups should not.

Valentim, 2024). A sufficiently strong public signal can therefore produce short-run improvements in expressed favorability even outside the domain that initially triggered the signal.

This interpretation lies between two stronger claims. It is not simply that respondents hid prejudice from an interviewer, especially because online survey responses are relatively private,[3] nor is it that durable private preferences changed overnight. The mechanism could operate through several closely related channels: (i) citizens updating what they take others to consider acceptable, (ii) anti-prejudice considerations becoming more cognitively available when respondents answer favorability questions, or (iii) several marginalized groups coming to be read as objects of a common anti-discrimination norm. Existing work treats these as closely connected, with perceived norms shaping expression in the short run and, over time, sometimes feeding back into private attitudes (Bicchieri, 2017; Tankard and Paluck, 2016, 2017; Alvarez-Benjumea, 2023; Valentim, 2024). The design cannot distinguish among these proximate channels, so I do not claim to identify which ones drive an effect. It can test whether expressed favorability moves in the joint pattern implied by a broad public signal about acceptable prejudice.

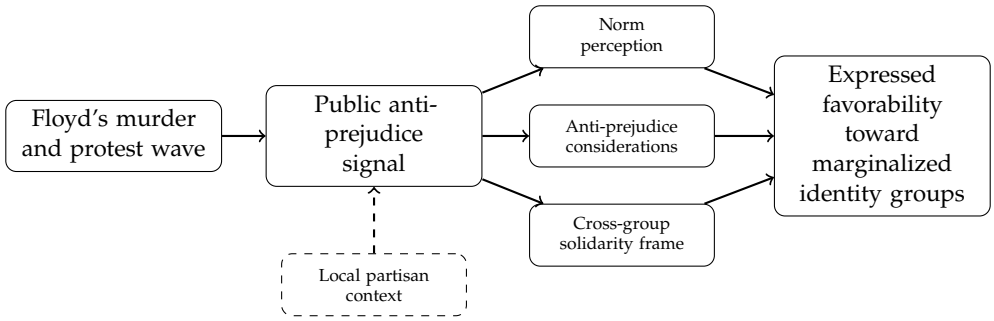
The argument also implies contextual heterogeneity. The effects of national events depend on whether the local environment reinforces or dampens the signal. Local social referents shape perceived collective norms, and elite and media signals affect how citizens understand norm boundaries (Paluck and Shepherd, 2012; Tankard and Paluck, 2016; Arias, 2019; Clayton et al., 2021; Hopkins, 2010). Ordinary social ecology—peer discussion, local protest presence, workplace and school statements, yard signs—can convert a national event into a locally credible norm. In the United States, views on race are strongly structured by partisanship, and responses to the Floyd protests varied accordingly (Westwood and Peterson, 2022; Mason, 2018; Reny and Newman, 2021). Respondents in Democratic-leaning areas are more likely to encounter local environments that validate the anti-racist signal, raising the cost of expressing exclusionary views. In Republican-leaning areas, counter-signals about protest, disorder, or partisan overreach may mute the

[3]The point here is not that an anonymous online survey recreates interpersonal sanctioning. It is rather that explicit survey answers remain acts of group evaluation, and salient public norms can affect which considerations respondents bring to the question even when no interviewer is present.

national cue or produce backlash. The same national event should therefore generate larger shifts in expressed attitudes in Democratic than in Republican areas, even conditional on respondents' own partisanship.

Figure 1 summarizes the argument. Floyd's murder and the protest wave generate a public signal about acceptable prejudice that may operate through several closely related proximate channels, with local context reinforcing or dampening it before it appears in expressed group evaluations. The empirical analysis tests the predicted pattern in expressed favorability.

Figure 1: Conceptual framework.



Notes: The figure presents the theoretical mechanism. The empirical analysis tests the prediction that expressed favorability shifts when the protest wave becomes nationally salient, conditional on local partisan context. The three boxes in the middle column depict candidate channels through which the public signal can produce the spillover pattern; the analysis does not separately identify them.

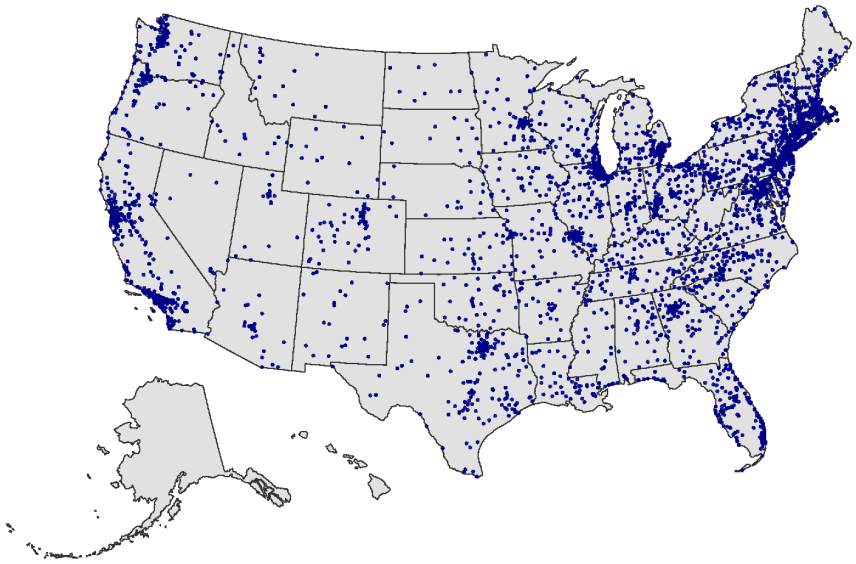
Two empirical expectations follow. First, expressed favorability after the protest wave should increase toward Black Americans and toward at least some other minority groups. Second, these shifts should be larger in Democratic local environments, where the new signal is more likely to be reinforced. This should be true for both Republicans and Democrats living in Democratic-leaning areas.

3 Data and methods

I study the aftermath of the murder of George Floyd, who was killed by a White police officer in Minneapolis on May 25, 2020. The killing triggered one of the largest protest waves in recent US history, with related demonstrations beyond the United States (Buchanan et al., 2020; Smith et al., 2020). Figure 2 shows the spatial

distribution of Black Lives Matter protests in the aftermath of the murder in the United States. The map shows that citizens did not observe only a single highly salient act of violence as many were also exposed to an unusually large and visible public response.

Figure 2: Black Lives Matter protests after George Floyd's murder.



Notes: The figure displays the locations of Black Lives Matter protests that occurred in the aftermath of George Floyd's murder in the United States. Data are from the Armed Conflict Location & Event Data Project and include events where "BLM: Black Lives Matter" was listed as an associated actor (Raleigh et al., 2010; Armed Conflict Location & Event Data Project, 2024).

The murder and the protests that followed also generated an unusually visible public discussion about racism and anti-racism. Figure 3 shows this across three kinds of attention: Google searches, television news coverage, and newspaper discussion.[4] Regardless of the measure, attention rises sharply around May 28, 2020, several days after the murder itself when the news of the murder and the following protests became salient on the national level (Reny and Newman, 2021). This pattern matters for the choice of cutoff date below since the relevant treatment

[4]These series are used as contextual evidence of aggregate public attention rather than as measures of individual-level exposure and they differ in both coverage and units. Specifically, Google Trends captures relative search interest, GDELT captures monitored television news, and Media Cloud captures the author-defined national outlet sample.

is not the murder in isolation, but the moment at which the public reaction to it became nationally visible.

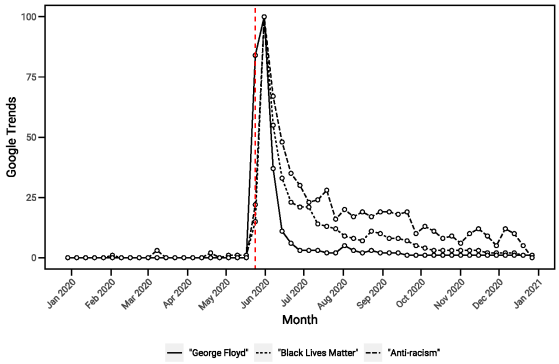
I draw on Nationscape, a large repeated cross-section fielded in weekly waves between July 2019 and February 2021 (Democracy Fund Voter Study Group, 2023; Tausanovitch et al., 2019). The survey interviewed roughly 6,000 respondents per week and contains just under 500,000 observations overall. Because the data include interview dates and respondent zip-codes, they make it possible to study short-run shifts in expressed group attitudes around an unexpected national event in specific areas. Appendix Table S2 reports summary statistics.

The main outcomes are favorability toward Black Americans, Asians, Latinx individuals, Muslims, and the LGBTQ community. Nationscape asks respondents how favorable or unfavorable their impression of each group is. I reverse the original coding so that higher values indicate greater favorability and analyze the outcomes on the resulting four-point scale. These outcomes are explicit survey evaluations, so the estimates should be interpreted as changes in expressed favorability rather than as direct measures of private affect or implicit prejudice. The same survey battery also includes favorability toward Whites and Socialists, which I later use as placebo outcomes to examine whether there has been a more general shift in favorability toward all groups.

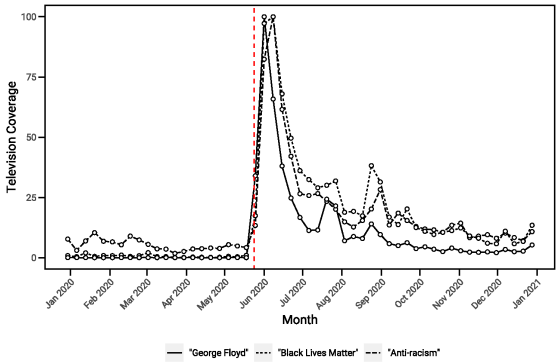
The empirical design is a regression discontinuity in time around May 28, 2020. The choice of date follows directly from the public-attention series in Figure 3, which show a sharp rise around that date across search, television, and newspaper data, several days after the murder itself. The design also follows recent work that treats Floyd's murder and the ensuing protests as an unexpected regression-discontinuity-in-time shock. Reny and Newman (2021) use this approach to test for direct effects on attitudes toward Black Americans and the police. I extend their analysis to test for spillovers onto other marginalized groups and to compare the discontinuity across local partisan environments. I use interview date as the running variable and estimate the discontinuity in expressed group favorability at the start of the protest wave. All models are local linear regressions with triangular kernels, MSE-optimal bandwidths, survey weights, and robust bias-corrected inference implemented with `rdrobust` (Cattaneo et al., 2019; Calonico et al., 2014, 2017, 2020). The MSE-optimal bandwidths are selected separately for each out-

Figure 3: Public attention after George Floyd's murder.

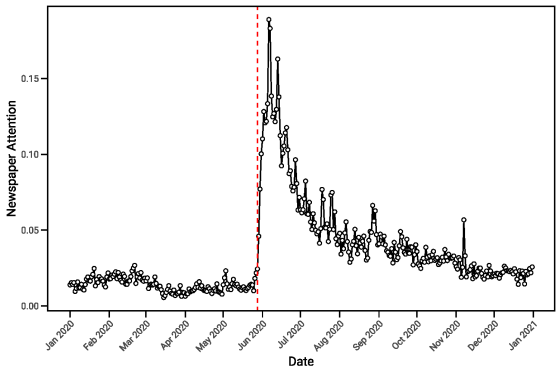
(a) Google Trends for related search terms.



(b) TV reporting on related topics.



(c) Newspaper attention to discrimination and racism.



Notes: The figure displays trends in public and media attention related to George Floyd's murder in 2020. Panel (a) shows Google search interest for "George Floyd," "Black Lives Matter," and "Anti-racism." Panel (b) shows television news coverage of the same topics, based on GDELT Television API data (Leetaru, 2017). Panel (c) shows the share of articles in a sample of 247 national news outlets that mention "Racism" or "Discrimination," based on Media Cloud data (Roberts et al., 2021). The vertical dashed line indicates the date of George Floyd's murder, May 25, 2020.

come and span several weeks on either side of the cutoff. The corresponding values are reported alongside each estimate, and Appendix Figure S5 shows that the estimates are stable when the bandwidth is varied from one-half to twice the optimal value.

Formally, for respondent i and outcome group g, let t_i denote the respondent's interview date and let c denote the cutoff date. The local linear RD specification can be written as

$$Y_{i,g} = \alpha_g + \tau_g \text{Post}_i + \beta_{1,g}(t_i - c) + \beta_{2,g}\text{Post}_i(t_i - c) + \varepsilon_{i,g}, \tag{1}$$

where $Y_{i,g}$ is the expressed favorability outcome for group g and Post_i is an indicator equal to one for respondents interviewed on or after the cutoff date. The interaction term allows the slope in interview date to differ on either side of the cutoff. The coefficient of interest, τ_g, captures the local discontinuity in expressed favorability for group g at the cutoff:

$$\tau_g = \lim_{t_i \downarrow c} \mathbb{E}[Y_{i,g} \mid t_i] - \lim_{t_i \uparrow c} \mathbb{E}[Y_{i,g} \mid t_i], \tag{2}$$

where the first term is the limit approaching the cutoff from the post-cutoff side and the second term is the limit approaching from the pre-cutoff side.

This estimand is a local average treatment effect with two implicit restrictions worth stating directly. First, it speaks to respondents interviewed in the days around the cutoff, not to the population at large. Second, it pools the effect of the murder, the protest wave, and the broader public response into a single reduced-form estimate. I therefore interpret τ_g as the short-run shift in how respondents reported evaluating each group around the moment at which the public reaction became nationally visible. It should not be read as the average effect of Floyd's murder on all Americans, or as a direct estimate of durable private attitude change.

I use Nationscape's survey weights throughout. Lucid constructs the weights to reproduce population marginals in pre- and post-cutoff samples, so the discontinuity inherits the representativeness of the weighted means. The unweighted estimates are reported in Appendix Table S7 and remain positive for all five outcomes.

Identification requires that, absent the protest wave, potential outcomes evolve smoothly through the cutoff. In unexpected-event settings, that assumption is most plausible in a narrow window around the event (Muñoz et al., 2020; Hausman and Rapson, 2018). The unexpected timing of the murder reduces concerns about anticipatory sorting, but it does not by itself rule out changes in survey fielding, response patterns, or contemporaneous shocks. Section 5 therefore gives particular attention to covariate balance, placebo outcomes, alternative cutoffs, and the density of observations around the cutoff. Appendix Figure S6 shows that the estimates are similar when I move the cutoff to May 25, 26, or 27, which is what one would expect if the results capture the broader shock created by the murder and the protests rather than an arbitrary date choice.

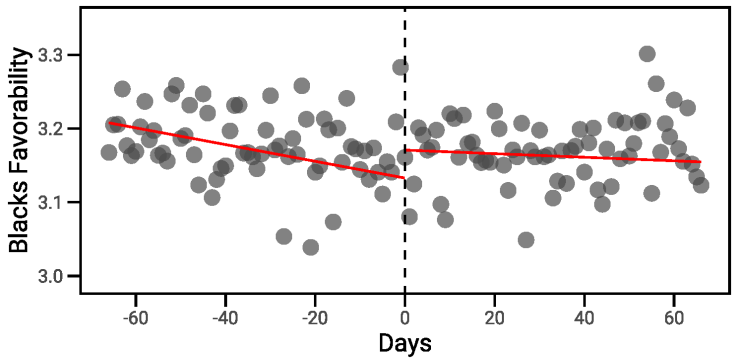
To study contextual heterogeneity, I classify respondents by the partisan leaning of their county using the 2016 presidential vote margin and then split the sample by respondents' own partisanship (MIT Election Data and Science Lab, 2018). Democratic-leaning counties are those with a positive Democratic presidential vote margin. For clarity, I summarize the five favorability outcomes in a single index (the average favorability toward all groups) and estimate the same RDiT separately within each subgroup. County vote share is an imperfect proxy for the local normative environment because it also captures urbanicity, racial composition, education, media markets, and exposure to protest. The test is therefore not a direct measurement of local norms. It asks whether the same national shock is larger in places where anti-racist signals were more likely to be socially reinforced (Hopkins, 2010; Paluck and Shepherd, 2012; Mason, 2018).

4 Results

Figure 4 shows a clear upward jump in favorability toward Black Americans at the cutoff. The corresponding RDiT estimate in Table 1 is 0.05 points on the four-point scale.

Figure 5 repeats the same exercise for Asians, Latinx individuals, Muslims, and the LGBTQ community. In each case, the discontinuity is positive. Table 1 confirms the same pattern statistically. The estimated jumps are 0.08 for Asians, 0.05 for Latinx individuals, 0.12 for Muslims, and 0.10 for the LGBTQ community,

Figure 4: Favorability toward Black Americans around May 28, 2020.

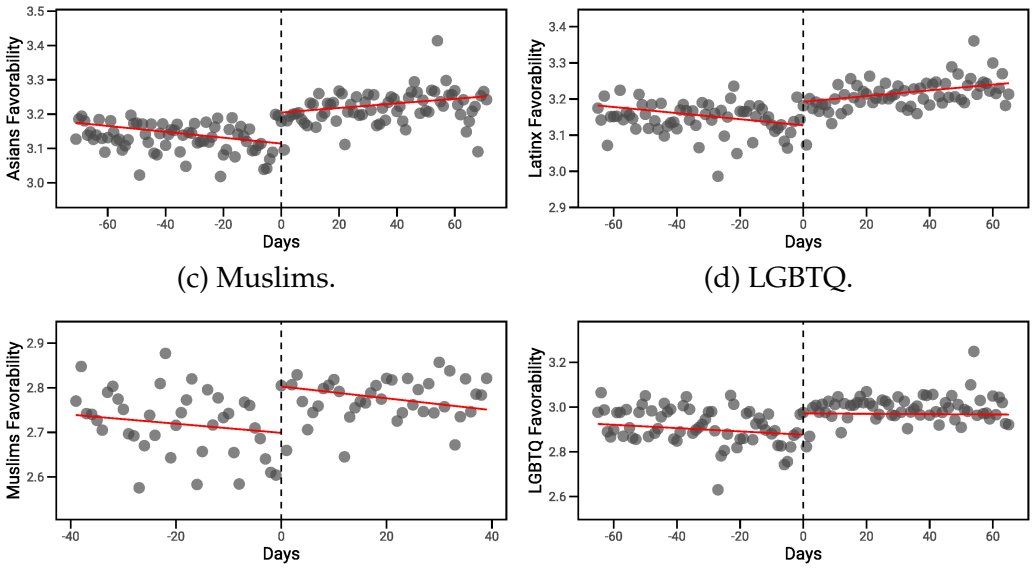


Notes: The figure shows binned means and local linear fits on either side of the May 28, 2020 cutoff. Estimates use a triangular kernel and an MSE-optimal bandwidth (Calonico et al., 2020). The corresponding coefficient is reported in Table 1.

Figure 5: Favorability toward other minority groups around May 28, 2020.

(a) Asians.

(b) Latinx.



(c) Muslims.

(d) LGBTQ.

Notes: Each panel shows binned means and local linear fits on either side of the May 28, 2020 cutoff. Estimates use a triangular kernel and MSE-optimal bandwidths (Calonico et al., 2020). Table 1 reports the corresponding coefficients.

and each estimate is distinguishable from zero under the reported RDiT inference. Relative to pre-treatment dispersion, these estimates correspond to shifts of roughly 0.06 to 0.12 standard deviations. On a four-point scale these are modest individual-level changes, roughly 2% to 4% of the scale range. As short-run average shifts around a national political shock, appearing across several distinct outcomes at the same cutoff, they are substantively meaningful. The joint pattern of positive discontinuities is the primary evidence for spillovers. Even after applying the Benjamini–Hochberg procedure to account for multiple testing, all coefficients remain statistically distinguishable from zero (Benjamini and Hochberg, 1995).

The estimate for Black Americans (0.05) is at the lower end of the five estimates, matching that for Latinx individuals. As anticipated in Section 2, smaller effects need not imply weaker theoretical relevance. Two explanations are possible, and they need not be mutually exclusive. First, there could be ceiling and crystallization effects. Attitudes toward Black Americans are anchored by a long-standing norm of racial equality (Mendelberg, 2001) and deeply structured by symbolic-racial considerations and partisanship (Sears and Henry, 2003; Tesler, 2012, 2016; Westwood and Peterson, 2022). Public attention to anti-Black discrimination had also moved through repeated cycles of high-salience violence and protest since at least the mid-2010s (Walker et al., 2020; Reny and Newman, 2021), leaving baseline favorability higher and priors firmer than for adjacent groups, and less room for expressed evaluations to shift at a single cutoff. Second, there could be countermobilization on the focal group. Because the protests named Black Americans directly, partisan counter-signals about the protests themselves were concentrated on this outcome, attenuating the pooled estimate even when the positive component is large (Hopkins, 2010; Druckman et al., 2013; Reny and Newman, 2021). Adjacent groups not directly affected were less exposed to a competing counter-frame. The design cannot distinguish between these explanations.

Figure 6 reports estimates for a summary index of the five favorability outcomes by respondent partisanship and county partisan leaning. The effects are concentrated in Democratic areas. Within those areas, the estimates are positive for both Democrats and Republicans, and the point estimate is larger among Republicans. In Republican areas, by contrast, the estimates are small and imprecise. The estimates for Republicans living in Democratic-leaning areas are especially

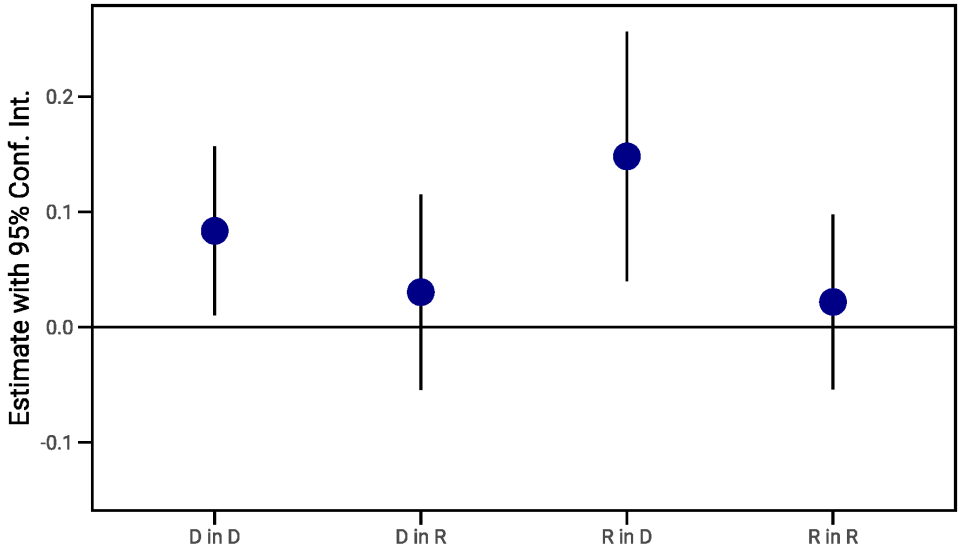
Table 1: Regression discontinuity estimates.

	Blacks	**Asians**	**Latinx**	**Muslims**	**LGBTQ**
LATE	0.05*	0.08***	0.05**	0.12***	0.10***
	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)
BW est. (h)	66.05	71.90	100.61	39.86	65.69
BW bias (b)	115.29	111.68	157.24	77.62	104.83
Total no. obs.	406,058	368,414	383,872	374,213	399,215
Effective no. obs.	99,882	97,056	139,305	54,010	95,315
Left of cutoff	47,388	47,748	69,052	26,027	45,890
Right of cutoff	52,494	49,308	70,253	27,983	49,425

Notes: The table reports RDiT estimates for favorability toward each group at the May 28, 2020 cutoff. Each column corresponds to a different group. Triangular kernel and MSE-optimal bandwidths (Calonico et al., 2020); robust bias-corrected standard errors in parentheses. Significance levels: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

informative. They suggest that the pattern is not reducible to Democratic respondents becoming more favorable after a Democratic-aligned protest wave. They also weigh against an alternative reading in which only White liberals shifted in response to the salience of White-perpetrated violence. The same national event appears to matter most where the surrounding environment was likely to reinforce the anti-racist signal (Hopkins, 2010; Mason, 2018). This is consistent with the contextual reading of the argument, though it should be read as suggestive rather than direct evidence on the mechanism.

Figure 6: Effects by respondent partisanship and county partisan leaning.



Notes: The figure shows RDiT estimates for a summary index of favorability toward Black Americans, Asians, Latinx individuals, Muslims, and the LGBTQ community. The x-axis corresponds to combinations of respondents' own self-identified partisanship and the partisan leaning of the county in which they live. Counties are classified as Democrat-leaning if they had a positive Democratic two-party vote margin in the 2016 presidential election. Estimates use a triangular kernel and MSE-optimal bandwidths (Calonico et al., 2020). Bars indicate 95% confidence intervals based on robust bias-corrected standard errors.

5 Robustness

The main pattern is robust to several alternative checks. The most important threats are that the estimates reflect compositional changes in who answered the survey, a general leftward or pro-Democratic mood shift, survey demand after intense media coverage, or other events occurring at roughly the same time. The checks below help distinguish the spillover interpretation from these alternatives.

A first concern is compositional change. If the mix of respondents changed sharply around May 28, the discontinuities could reflect who entered the survey rather than how expressed evaluations changed. I do not find comparable discontinuities in respondent characteristics. Appendix Figure S4 shows null effects when demographic variables are used as outcomes, and Appendix Table S3 shows close balance around the cutoff. The results also remain visible when the sample is restricted to White respondents only (Appendix Table S8). These checks do not prove that the repeated cross-sections are identical, but they reduce the concern that the main estimates are driven by abrupt observable compositional shifts.

A second concern is that the estimates simply capture a generalized positive mood, a leftward ideological movement, or an increase in pro-Democratic affect after the protests. The placebo outcomes speak against this interpretation. Table S9 shows no change in favorability toward Whites or Socialists, which suggests that the discontinuities are not a general increase in positive affect toward all groups. Nor do the data suggest a general leftward ideological shift. When ideology itself is used as the outcome, the estimate is small and statistically insignificant (Appendix Table S10). This matters because a broad ideological shift could also generate correlated movement across several minority-group outcomes without requiring a change in perceived anti-prejudice norms.

A third concern is that the results are artifacts of specification choices. The estimates are very similar when I move the cutoff between May 25 and May 28 (Appendix Figure S6), vary the bandwidth from one-half to twice the optimal choice (Appendix Figure S5), use continuous rather than discrete time (Appendix Table S5), or include covariates (Appendix Table S6). They are also not driven by any single state, as leave-one-state-out estimates show (Appendix Figure S7). These checks are especially important for a regression discontinuity in time design,

where estimates can otherwise be sensitive to bandwidth choice, functional form, or idiosyncratic events in particular places.

Two further checks help interpret the design. First, I repeat the analysis using every Thursday in the survey period as a placebo cutoff,[5] provided that there are at least 30 days of observations on either side. The actual May 28 cutoff is consistently among the largest and most precisely estimated discontinuities (Appendix Figure S10). Second, the unweighted estimates remain positive for all five outcomes and remain statistically distinguishable from zero for Asians, Latinx individuals, Muslims, and the LGBTQ community, although the estimate for Black Americans attenuates (Appendix Table S7). The attenuation in the unweighted Black estimate is consistent with the weights doing actual work in reproducing population means, and it reinforces the importance of treating individual coefficients cautiously while leaving the broader spillover pattern intact.

A fourth concern is that one of the group-specific results reflects a separate contemporaneous event. This is most plausible for the LGBTQ outcome because the Supreme Court decided *Bostock v. Clayton County* in June 2020. The evidence does not support this interpretation. Using June 15, 2020 as a placebo cutoff yields a null estimate (Appendix Table S12). More generally, the fact that the main discontinuities appear across multiple minority-group outcomes at the same cutoff, and that the estimated effects in the main specification are unusually large as compared to other weeks on the same weekday, makes it less likely that the pattern is driven by separate group-specific news events, though this possibility cannot be ruled out for every outcome with equal certainty.

One issue deserves direct mention. The density test around the cutoff rejects continuity of the running variable (Appendix Figure S3; Cattaneo et al., 2020). The substantive concern this test proxies for is observable composition, and that is balanced. Covariates are balanced, placebo outcomes do not move, and the fluctuation in interview counts around the cutoff is not unusual relative to the rest of the series (Appendix Figure S2). The imbalance also goes in the direction of fewer post-cutoff interviews, which is difficult to reconcile with strategic survey response. At this sample size the density test rejects for trivial deviations from continuity, and so the rejection itself is largely uninformative about the substantive

[5]May 28, 2020 was a Thursday.

concern. For the purposes of this chapter, I do not view it as fatal, but mention it for the sake of completeness regardless.

6 Conclusion

This chapter has examined whether the public reaction to George Floyd's murder generated cross-group spillovers in expressed attitudes toward marginalized minority groups. Using a regression discontinuity in time around the start of the nationally visible protest wave, I find that expressed favorability rises toward Black Americans as well as toward Asians, Latinx individuals, Muslims, and the LGBTQ community, i.e., toward minority groups not directly related to the event. The shifts are concentrated in Democratic-leaning counties, including among Republican respondents in those areas, which is what one would expect if the local environment reinforced the national signal.

Taken together, these results are consistent with public reactions to high-salience events spilling across group boundaries. A national shock and the protest wave that followed it likely altered the public context around prejudice, and that shift appeared across several adjacent group attitudes rather than only the one directly at the center of the event. This contributes to research that treats public events as capable of moving what citizens feel comfortable expressing without necessarily moving deep private beliefs (Kuran, 1995; Bursztyn et al., 2020; Valentim, 2024), and to work on attitude generalization that has typically focused on contact or persuasion within a specific domain (Pettigrew, 2009; Tesler, 2016). It matters politically because expressed attitudes shape what individuals are willing to say in front of one another, and what elites are rewarded for saying in public.

While the evidence is consistent with the theory, important caveats remain. First, the outcomes are explicit survey evaluations, so the estimates capture short-run shifts in expressed favorability rather than directly measuring private affect or durable preference change. The data cannot establish whether respondents' underlying attitudes changed.

Second, I infer the role of a broader public signal from the joint pattern of outcomes and from the contextual heterogeneity, rather than measuring perceived

norms directly. As discussed in Section 2, the design does not distinguish between strict propagation from the focal group to adjacent ones and a common inclusive frame that moves several group evaluations together. Both mechanisms are consistent with the data and may operate together. The chapter documents this generalized response and its concentration in environments that reinforced the signal. It does not distinguish among the proximate channels through which the broader signal operates; this remains a question for further research.

Finally, I use county partisanship as a proxy for local context in the heterogeneity analysis. While this is a useful proxy for distinguishing areas with more conservative and more liberal environments, it is also correlated with other factors, such as urbanicity, racial composition, education, and income.

These limits also point toward useful next steps. Designs that combine large-N survey data with explicit norm-perception items would be better positioned to test the inferred mechanism directly, and panel data spanning the relevant period would help separate short-run expression from longer-run change. The findings also contribute to the dissertation's broader argument about political behavior during episodes of contention by showing that the normative consequences of a contentious moment can travel across group boundaries. In particular, a public shock organized around one marginalized group can reach attitudes toward several others, but only where the local environment translates the national signal into a credible local norm. Recognizing this widens the set of outcomes worth examining after a salient public event, and shifts attention from group-specific information to the broader public arguments that events provoke.

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Conclusion

The dissertation began with a question about how political behavior changes when public life is disrupted by an episode of contention. The three empirical chapters answered it in settings that look nothing alike—a fatal school shooting in a US county, a recurring seasonal cue in German public pools, and a national protest wave following a high-salience act of violence. Most public-opinion research has studied citizens during quieter periods; this dissertation studied them during contention.

The empirical chapters were written so that each contributes to its own substantive literature and stands on its own findings. Read together, they support a more general claim about contention and the politics behind it. Political behavior during contentious moments is most often best understood not as evidence that citizens have changed their minds but as evidence that predispositions citizens already held have become newly consequential, channeled into observable behavior by the organizational, electoral, or normative conditions the moment makes available.

What the chapters show together

Each chapter recovers a different route through which contention reshapes political behavior. Chapter 1 documents an organizational route. Donations to the National Rifle Association rise sharply in the years following a fatal school shooting in a county, with effects concentrated in states where the marginal threat from new regulation is greatest. The same incidents that draw public attention to the case for gun regulation reliably recruit the constituency most invested in resisting it. A standing infrastructure already in place absorbs that response and channels it into costly, sustained participation. The asymmetry between mobilization and counter-mobilization in this case is part of why the policy windows that open after focusing events so often close without substantive change.

Chapter 2 documents an electoral-salience route. Support for the *Alternative für Deutschland* rises in ethnically diverse German localities during the outdoor pool season and recedes once the season ends, with no comparable effect for indoor

pools or in low-diversity localities. The mechanism is a shift in issue salience rather than in underlying preferences. Exposure to ethnic diversity in a public setting that far-right actors have politicized raises the weight respondents place on immigration in their vote choice, and parallel panel evidence shows that underlying immigration attitudes themselves do not move. A politicized setting brings a stable disposition to the surface and gives it electoral force in localities where ethnic diversity makes the cue meaningful.

Chapter 3 documents a normative-expression route. Around the moment at which the public reaction to George Floyd's murder became nationally visible, expressed favorability rises toward five different marginalized groups—Black Americans, Asians, Latinx individuals, Muslims, and the LGBTQ community—with the largest shifts in Democratic-leaning counties and, within those counties, among Republican respondents at least as much as among Democrats. The joint pattern across groups and contexts is more plausibly read as a change in the normative environment around the expression of group attitudes than as a simultaneous change in private beliefs about five different groups within a single week.

The three episodes look very different on the surface and travel through different mechanisms. They share a structural feature, however. In each, a stable orientation predates the moment, an event of contention renders it newly relevant, and an environmental feature—organizational infrastructure, ethnic diversity in the local population, or the political color of the locality—determines what kind of political consequence the moment produces.

Implications

The dissertation contributes to the study of contentious politics by examining citizens during contention at a resolution and scale not typical of the field. Work in the contentious-politics tradition has long emphasized that political conflict outside the institutional routine is structured by organizational infrastructures, political opportunities, and repertoires of action, but its empirical attention has more often fallen on movements than on the citizens whose participation, vote choices, and public expression those movements mobilize. The chapters here pair quasi-experimental designs with large-scale administrative or panel data to re-

cover citizen-level political behavior during these three episodes. They suggest that the contentious-politics framework travels productively to questions that adjacent literatures on public opinion, party competition, and group attitudes have typically taken up without it.

Across the three chapters, what looks like rapid opinion change during a contentious moment is most often a stable predisposition becoming politically consequential under the specific conditions the moment makes available. Reading these shifts as preference change misclassifies the kind of change that has occurred, and therefore mispredicts who is moving, for how long, and to what political end. The implication for related literatures—on focusing events, on intergroup contact and far-right support, and on the political consequences of high-salience violence and protest—is that the appropriate analytical object during contention is often not whether preferences shifted but which preferences acquired new political force, and through which channel.

A broader implication runs to research on democratic representation. Existing accounts of unequal responsiveness typically focus on whose voices are heard once they have been raised. The chapters here suggest that some of the most consequential inequalities sit upstream of that step, in the differential capacity of constituencies, attitudes, and local environments to convert such moments into political voice in the first place. Similarly distributed preferences yield very different distributions of political consequence when the conditions of contention favor one side of the contest: a tightly organized constituency benefits more from such a moment than a diffuse one, an issue already politicized at the national level benefits more from a local cue than one that is not, and a national normative signal travels furthest where the local environment already validates it. Political imbalances can emerge from comparable distributions of underlying opinion when the conditions under which contention turns predispositions into political voice are themselves unequally available.

Limitations and directions

The dissertation is best understood as three carefully identified case studies rather than as a unified controlled comparison. School shootings, pool seasons, and a na-

tionally salient protest wave are very different objects, and the cases were selected because each admitted a credible quasi-experimental design rather than because they form a representative sample of contemporary contention. The reading of each chapter through one of three routes is in this sense offered more as an organizing lens than necessarily as a competitive test among them. The dissertation also does not adjudicate among the routes in a single research design, nor does it argue that they are mutually exclusive in general.

Each chapter's identification strategy is best suited to short- to medium-run effects in the vicinity of the moment, and each relies on a particular outcome—contributions to a single organization, vote intention in a single panel, and feeling-thermometer ratings of named groups in a particular survey. These measures are appropriate for the questions being asked and provide the granularity required for credible identification, but they are necessarily partial. Tracking the longer-run trajectory of these effects—whether the organized counter-mobilization documented in Chapter 1 eventually attenuates, how repeated salience cycles of the kind studied in Chapter 2 accumulate, and whether the post-Floyd expressive shifts persisted beyond the protest wave itself—would require complementary designs. So would triangulation against adjacent outcomes, and complementary qualitative evidence on the texture of intergroup contact in pools and on the local reception of a national signal after Floyd.

A natural direction for future research is to map the limits of the three routes documented here: episodes in which counter-mobilization fails to materialize, salience cycles that do not translate into electoral consequence, and high-salience signals that fail to spill across group boundaries.

Closing

The three chapters describe contentious moments in which something visibly moved and something else, perhaps more important, did not. Donations rose without changes in stated gun policy preferences. Vote shares shifted without movement in underlying immigration attitudes. Expressed favorability rose without obvious change in private group sentiment. Importantly, none of these is a finding about citizens being fickle or political behavior being shallow.

Rather, each should be read as a finding about the conditions under which durable orientations reach public life: when the organizational, electoral, or normative conditions of a contentious moment favor a particular constituency, attitude, or local environment, behavior and expression can move quickly, even while the underlying orientations remain largely where they were. Now and in the future, contention is a recurring feature of democratic politics; the unequal conditions under which it turns predispositions into political voice are a politically consequential fact that the dissertation has tried to name precisely. The chapters are offered in that spirit.

Appendix to Chapter 1

S1 Placebo tests

In the main analysis, I use donations to the Pro-Life National Right to Life Committee PAC as a placebo test for the alternative theory of a general mobilization effect of school shootings. In this section, I provide examinations of donations to additional non-gun-related PACs in response to school shootings to illustrate that the mobilization is unique to the issue of gun regulation. The panels in Figure S1 provide event study estimates for the Club for Growth (a), Planned Parenthood (b), and Sierra Club (c) PACs, using data from 2003 to 2022. I include left-leaning PACs to show that I find no effects on either side of the political spectrum.

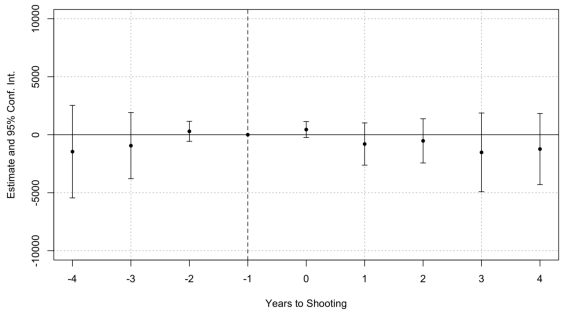
S2 Alternative data sources

S2.1 Rampage school shooting data

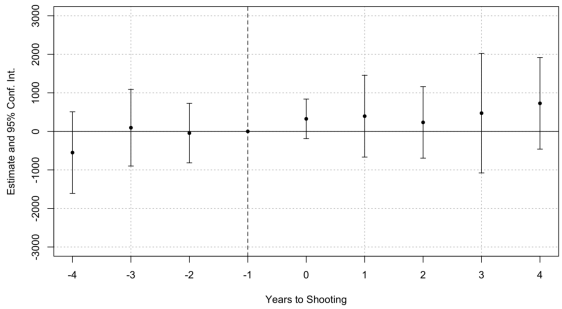
Not all school shootings can necessarily be classified as rampage shootings in the strictest sense. To examine whether defining treatment in this way affects treatment effect estimates, I utilize rampage school shooting data collected by García-Montoya et al. (2022). In their taxonomy, rampage school shootings are defined as shootings that "take place on a school-related public stage before an audience; involve multiple victims, some of whom are shot simply for their symbolic significance or at random; involve one or more shooters who are students or former students of the school" (Newman et al., 2008), and are not related to "gang violence or targeted militant or terroristic activity" (Schildkraut and Elsass, 2016).

Figure S1: School shootings and PAC donations: placebos.

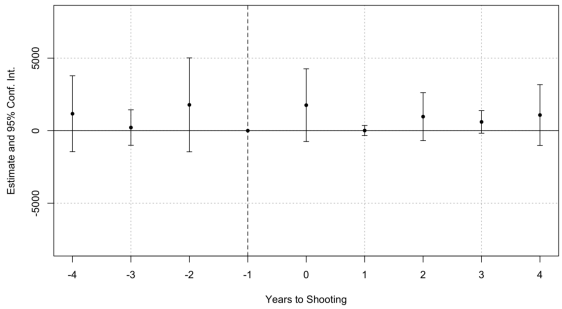
(a) Club for Growth.



(b) Planned Parenthood.



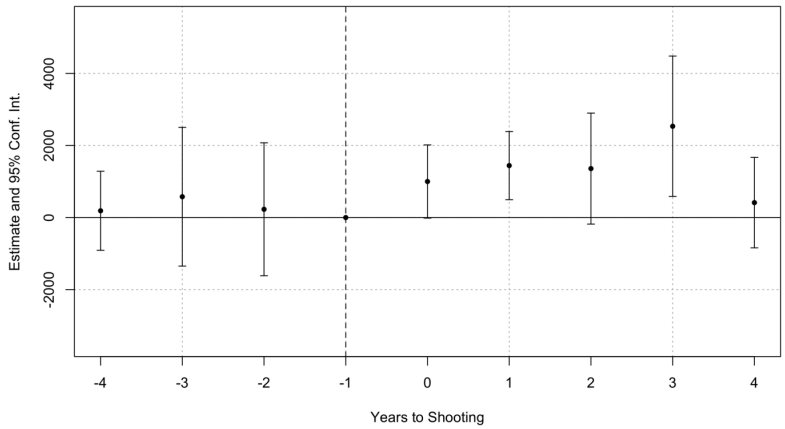
(c) Sierra Club.



Notes: These figures display event study estimates for the effect of school shootings on donation amounts to the Club for Growth (a), Planned Parenthood (b), and Sierra Club (c) PACs, using a staggered difference-in-differences design. Treatment is defined as occurrence of a school shooting. The relevant time variable is year. The bars represent 95% confidence intervals. All standard errors are clustered at the county level.

This approach could potentially influence the results in either direction. If the main analysis incorrectly includes school shootings that have minimal or no impact due to their non-rampage nature, effect estimates in the main analysis would be biased toward zero. Conversely, if all types of shootings have similar effects, but some are excluded, my control group would contain more "treated" units, which would also bias the results toward zero. In both scenarios, this exercise proves valuable for comparison and determining whether specific event types contribute to the overall pattern. As shown in Figure S2, effect estimates in this analysis are very similar.

Figure S2: Effect of rampage school shootings on NRA donations.

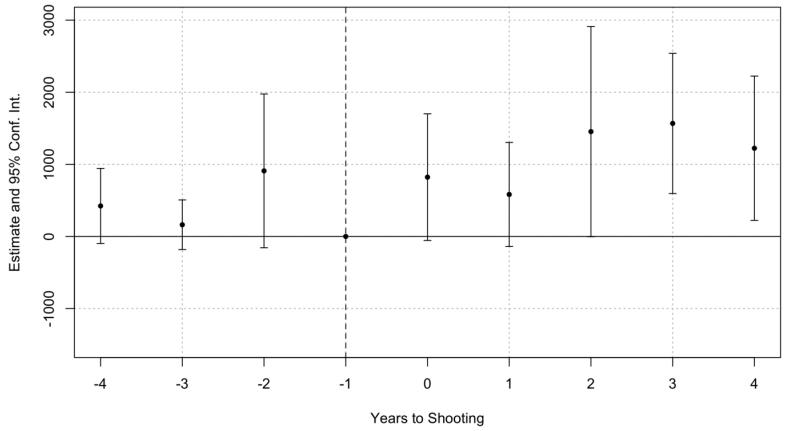


Notes: This figure displays event study estimates for the effect of rampage school shootings on donation amounts to the NRA-PVF, using a staggered difference-in-differences design. Treatment is defined as occurrence of a rampage school shooting, coded as per García-Montoya et al. (2022). The relevant time variable is year. The bars represent 95% confidence intervals. All standard errors are clustered at the county level.

S2.2 Pah et al. school shooting data

Pah et al. (2017) triangulate multiple data sources for school shootings to construct a dataset that is as comprehensive as possible, covering the period 1990 to 2013. I match school shootings coded in their data with NRA donations and re-estimate the causal effect of school shootings. The results of this analysis are displayed in Figure S3.

Figure S3: Results using Pah et al. data.



Notes: This figure displays event study estimates for the effect of school shootings on donation amounts to the NRA-PVF, using a staggered difference-in-differences design. Treatment is defined as occurrence of a school shooting, coded on the basis of data by Pah et al. (2017). The relevant time variable is year. The bars represent 95% confidence intervals. All standard errors are clustered at the county level.

S2.3 Mass shootings

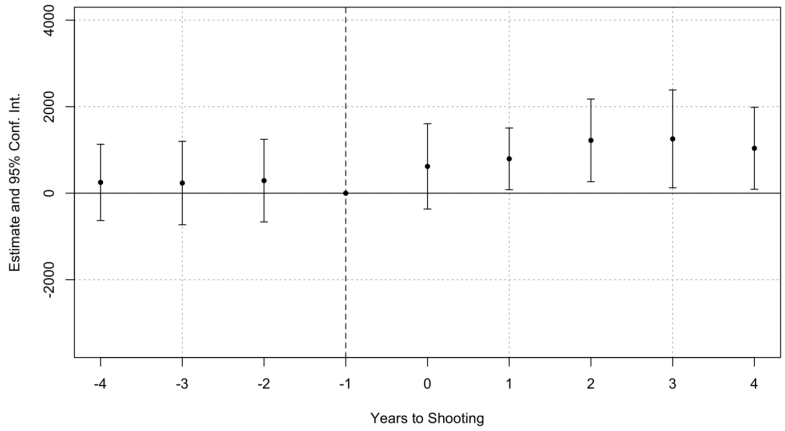
In the literature, scholars often focus on different subsets of salient violent events (see, e.g., Newman and Hartman, 2019; Frey and Kirk, 2021; Rogowski and Tucker, 2019; Hassell et al., 2020; Watson, 2022; Porfiri et al., 2019). To address potential concerns through selection of event type (school, rather than mass, shootings), I re-estimate the main effects using Mother Jones' US Mass Shootings dataset (2023). Estimates are displayed in Figure S4.

S3 Parallel trends assumption

S3.1 Heterogeneous time trends

Following Hassell and Holbein (2020), I re-estimate the main model while allowing for heterogeneous time trends by county. Doing so accounts for the possibility that treated and untreated units were already a priori trending in different directions. In such a model, the ATT estimates deviations from the respective individual trend. To allow for such trends, I use Sun and Abraham's (2021) staggered difference-in-

Figure S4: Mass shootings and NRA donations.



Notes: This figure displays event study estimates for the effect of mass shootings on donation amounts to the NRA-PVF, using a staggered difference-in-differences design. Treatment is defined as occurrence of a mass shooting, coded on the basis of data by Mother Jones (2023). The relevant time variable is year. The bars represent 95% confidence intervals. All standard errors are clustered at the county level.

differences estimator, which enables me to estimate such a model. Pursuing this strategy likely comes at the price of downward-biasing effect estimates (Goodman-Bacon, 2021). Results are displayed in Figure S5.

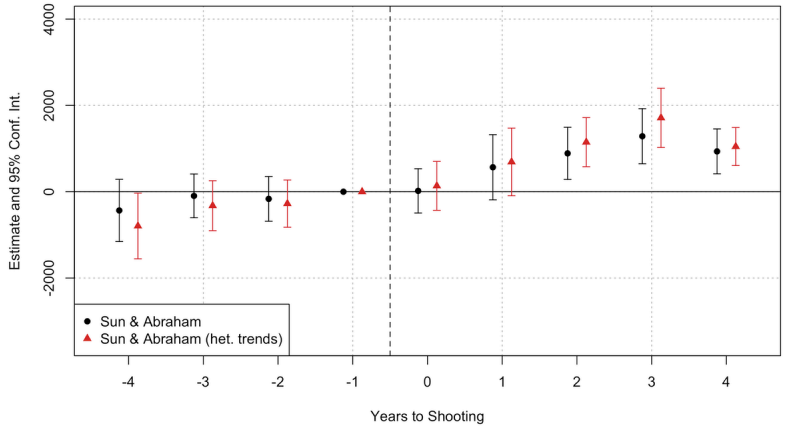
S3.2 Restricting control group to adjacent counties

In Figure S6 I present effect estimates for restricting the control group to directly adjacent counties, i.e., counties that are likely more similar in, e.g., economic or demographic structure to treated counties (García-Montoya et al., 2022). Data on adjacency come from the US Census' county adjacency file. The file can be found here: `https://www.census.gov/geographies/reference-files/2010/geo/county-adjacency.html`

S3.3 Restricting control group to not-yet-treated counties

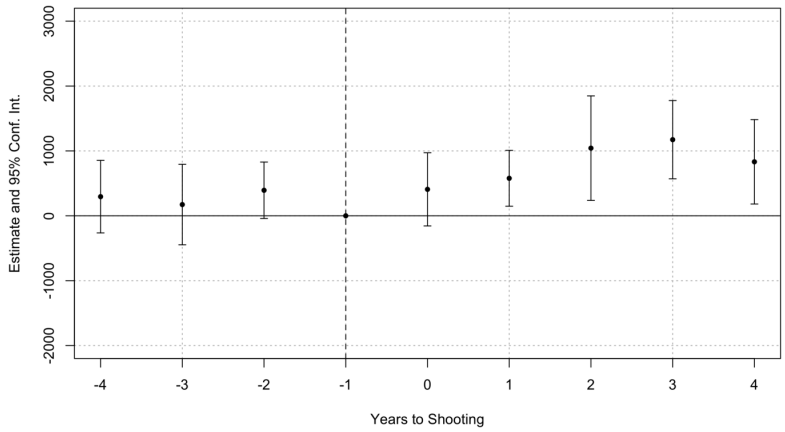
Figure S7 displays effect estimates for analyses using only not-yet-treated units as the control group. Power in this analysis is lower because the total size of the control group is significantly reduced.

Figure S5: Allowing for heterogeneous time-trends.



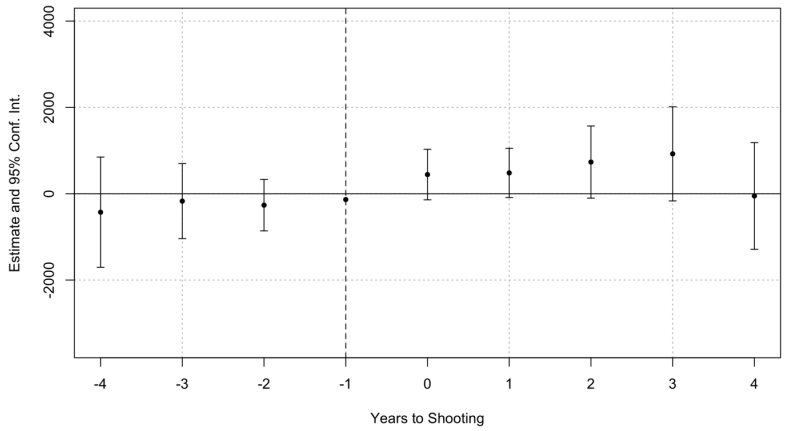
Notes: This figure displays event study estimates for the effect of school shootings on donation amounts to the NRA-PVF, using a staggered difference-in-differences design. This analysis uses the estimator by Sun and Abraham (2021). Linear time trends are allowed to vary by county. Treatment is defined as occurrence of a school shooting. The relevant time variable is year. The bars represent 95% confidence intervals. All standard errors are clustered at the county level.

Figure S6: School shootings and NRA donations: adjacent counties as control.



Notes: This figure displays event study estimates for the effect of school shootings on donation amounts to the NRA-PVF, using a staggered difference-in-differences design. Treatment is defined as occurrence of a school shooting. Control units are restricted to counties directly adjacent to treated counties. The relevant time variable is year. The bars represent 95% confidence intervals. All standard errors are clustered at the county level.

Figure S7: School shootings and NRA donations: not-yet-treated counties as control.



Notes: This figure displays event study estimates for the effect of school shootings on donation amounts to the NRA-PVF, using a staggered difference-in-differences design. Treatment is defined as occurrence of a school shooting. The control group is restricted to not-yet-treated units. The relevant time variable is year. The bars represent 95% confidence intervals. All standard errors are clustered at the county level.

S4 Spillover effects

In comparison to policies that are implemented in specific geographic regions, it is very much possible for the effect of school shootings to transcend boundaries. I examine the spatial specificity of the causal effect by re-estimating the main specification while coding adjacent counties additionally (panel a) and exclusively (panel b) as treated (see Figure S8). Information on adjacency is taken from the 'Adjacency File' supplied by the US Census Bureau. The file can be found here: `https://www.census.gov/geographies/reference-files/2010/geo/county-adjacency.html`

In doing so, I increase the number of treated counties from 131 to 694 in the former case. The estimates from this specification are lower than the main results, especially when I remove the counties in which the shooting originally occurred and focus solely on adjacent counties. However, the estimates remain positive and largely significant and are functionally equivalent in their pattern to the main results. This is in line with what I should expect given the relevance of proximity for the effects of such events on attitudes and behavior (Newman and Hartman,

2019; Sharkey and Shen, 2021; Marsh, 2022) and suggests that the (differential) mobilizing effects are highly localized in the affected areas; spillover effects are present but limited.

S5 Leave-one-out analysis

To ensure that my results are not driven by single states or events that have outsized effects on the overall result, I re-estimate the main difference-in-differences specification again, leaving out one state at a time (denoted on the x-axis). The outcome in this estimation is donation amounts. Figure S9 displays average ATTs over all post-treatment periods for each specification, including 95% confidence intervals. These estimated aggregated ATTs are highly unresponsive to the exclusion of any individual state.

S6 Alternative estimators

S6.1 Using alternative difference-in-differences estimators

To ensure that the results are not driven by the choice of the specific estimator, I re-estimate the results from the main figure using alternative staggered difference-in-differences estimators by Borusyak et al. (2021), De Chaisemartin and d'Haultfoeuille (2020), and Sun and Abraham (2021). Estimates from Callaway and Sant'Anna (2021) are displayed for comparison.

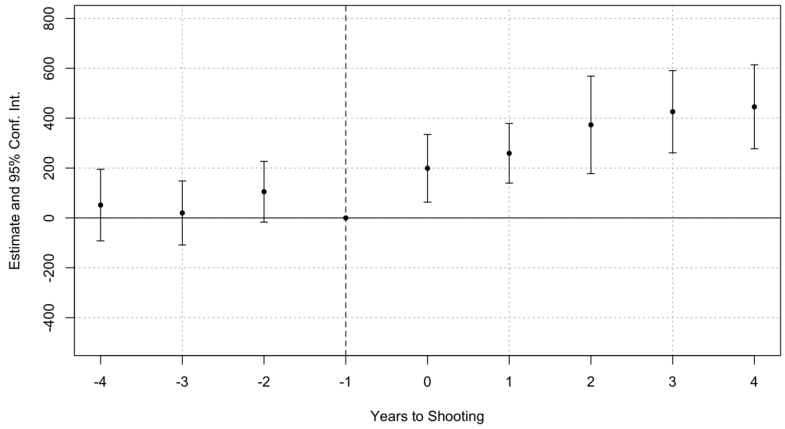
Estimates are very similar across all estimators. Effect estimates being larger when using the Borusyak et al. estimator is likely the result of this estimator making comparisons to the average of all pre-treatment periods (Roth et al., 2023).

S6.2 TWFE estimates

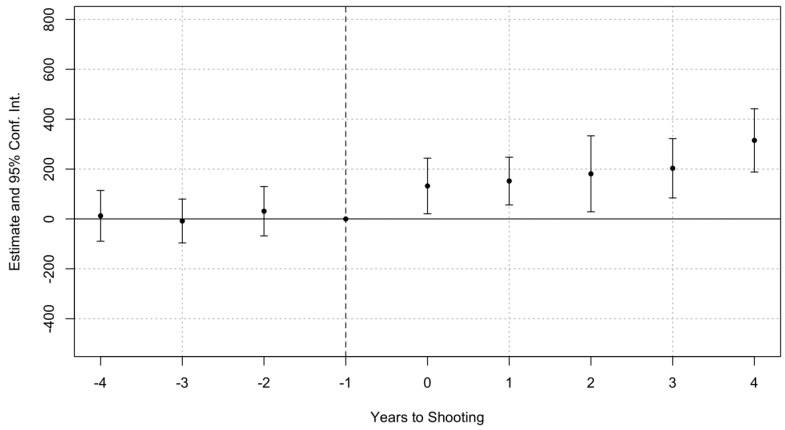
For reasons of completeness, I also report standard TWFE estimates. Despite the fact that this approach to estimating the ATT of a school shooting is potentially invalid in my scenario (Callaway and Sant'Anna, 2021; Goodman-Bacon, 2021; Borusyak et al., 2021), this method has for many years been the standard estimation

Figure S8: School shootings and NRA donations: spillover effects.

(a) Additionally treated.

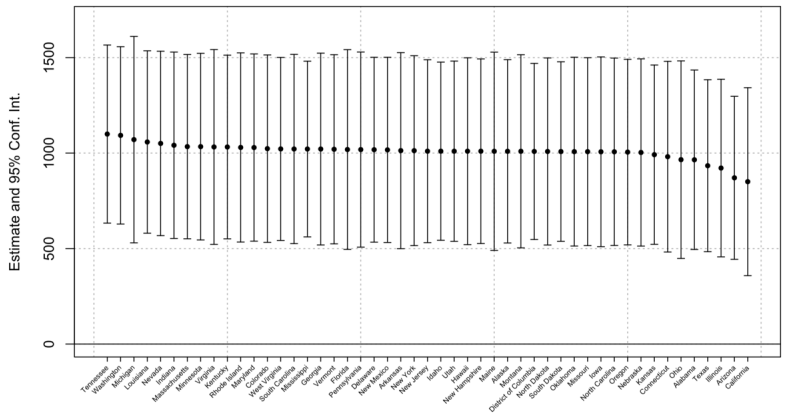


(b) Exclusively treated.



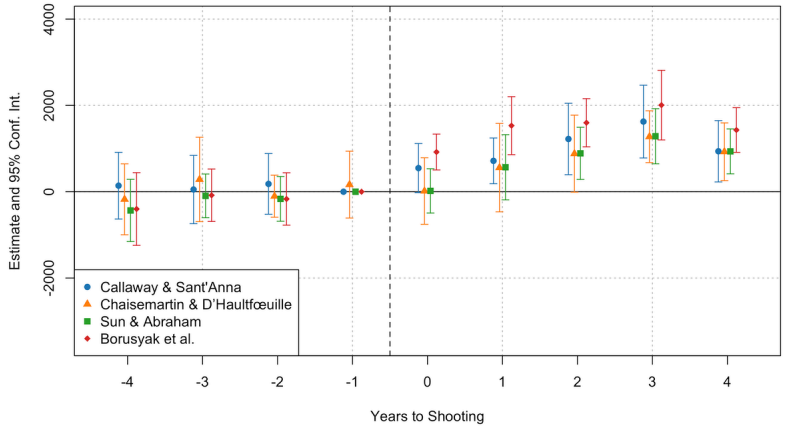
Notes: The event studies in this figure examine spillover effects of school shootings for NRA-PVF donations in adjacent counties. Adjacent counties are additionally (panel a) and exclusively (panel b) considered treated. Treatment is defined as occurrence of a school shooting. The relevant time variable is year. The bars represent 95% confidence intervals. All standard errors are clustered at the county level.

Figure S9: Effect sensitivity to specific states: leave-one-out analysis.



Notes: This figure displays time-averaged ATTs for the effect of school shootings on donations to the NRA-PVF. The labels on the x-axis indicate which state/district has been removed. Treatment is defined as occurrence of a school shooting. The relevant time variable is year. The bars represent 95% confidence intervals. All standard errors are clustered at the county level.

Figure S10: Alternative staggered estimators results.



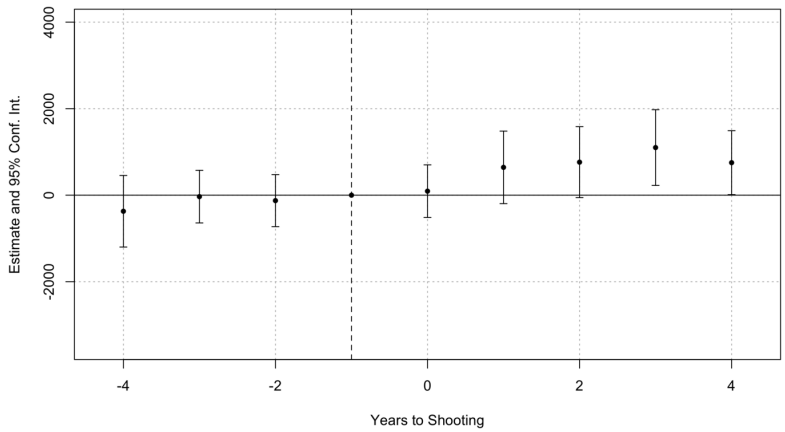
Notes: This figure displays event study estimates for the effect of school shootings on NRA-PVF donations using a range of different staggered difference-in-differences estimators. Treatment is defined as occurrence of a school shooting. The relevant time variable is year. The bars represent 95% confidence intervals. All standard errors are clustered at the county level.

strategy in the event study literature in Political Science (Liu et al., 2022). Hence, I provide such estimates for comparability. I estimate the following equation:

$$Y_{it} = \lambda_i + \delta_t + \sum_{\ell \neq 1} \beta_\ell \times D_{it}^{\ell} + \varepsilon_{it},$$

where i and t indicate county and year, respectively. The dependent variable, Y, is donations to the NRA-PVF at the county-year level. D is an indicator for unit i at time t being ℓ periods away from initial treatment. My coefficients of interest are the β coefficients, which represent the treatment effects for each lead/lag ℓ relative to treatment time. λ and δ are county and year fixed effects. For the estimation, I cluster standard errors on the county level. The results of this estimation are comparable to the results presented in the main figure of this chapter. This is what I should expect (Chiu et al., 2023).

Figure S11: TWFE estimates.



Notes: This figure displays event study estimates for the effect of school shootings on NRA-PVF donations using a TWFE model. Treatment is defined as occurrence of a school shooting. The relevant time variable is year. The bars represent 95% confidence intervals. All standard errors are clustered at the county level.

S7 Relationship between donations and other forms of non-electoral political participation

The table below examines the relationship between donations and other forms of non-electoral participation on individual level using data from the 2018, 2020, and 2022 CES waves. The main independent variable here is the answer to the question: "During the past year did you donate money to a candidate, campaign, or political organization?" coded in a binary way. The main dependent variables are answers to the questions "During the past year did you: attend local political meetings (Meetings), work for a candidate or campaign (Work), put up a political sign (Sign), attend a political protest, march or demonstration (Protest), contact a public official (Contact)?" These are yes or no questions.

I estimate the relationship between political donor status and these forms of participation using linear regressions of the form:

$$Y_i = \delta \, \text{Donate}_i + X_i^\top \beta + \lambda_{t(i)} + \varepsilon_i,$$

where Y_i denotes the outcome of interest for individual i. Donate_i is an indicator equal to one if respondent i reported having made a political donation in the previous 12 months. The coefficient δ captures the conditional association between donor status and the outcome. X_i is a vector of individual-level covariates, and $\lambda_{t(i)}$ denotes survey-wave fixed effects, where $t(i)$ is the wave in which individual i was observed. Standard errors are clustered at the ZIP-code level. Table S1 reports the results.

S8 Shootings without fatalities: K-12 school shooting data

The attentive reader might ask whether the results are different depending on whether I code treatment as a school shooting with or without fatalities. To address this question, I use data from the K-12 school shooting dataset (Riedman, 2023) that contains information on more than 2,000 US school shootings since 1970. Coding the treatment as an (unsuccessfully) attempted school shooting or shooting

Table S1: Donations and other forms of political participation: regression results.

	Meetings	Sign	Work	Protest	Contact
Donate	0.165***	0.274***	0.129***	0.173***	0.342***
	(0.003)	(0.003)	(0.002)	(0.003)	(0.003)
Indiv. covariates	✓	✓	✓	✓	✓
Year FEs:	✓	✓	✓	✓	✓
Num. Obs.	129,531	129,531	129,531	129,531	129,531
R^2	0.076	0.117	0.073	0.107	0.160

Notes: This table presents linear regression estimates for the relationship between donating money and different forms of political participation. The dependent variables are whether within the last year an individual attended local political meetings (Meetings), put up a political sign (Sign), worked for a candidate or campaign (Work), attended a political march, protest, or demonstration (Protest), or contacted a public official (Contact). All dependent variables are binary measures. Donate is a binary measure indicating whether a person stated that they contributed to a political candidate, campaign, or political organization in the last year. Individual level covariates include: age, gender, party identification, employment status, race, income. Standard errors are clustered by zip-code. Signif. Codes: ***: 0.001, **: 0.01, *: 0.05.

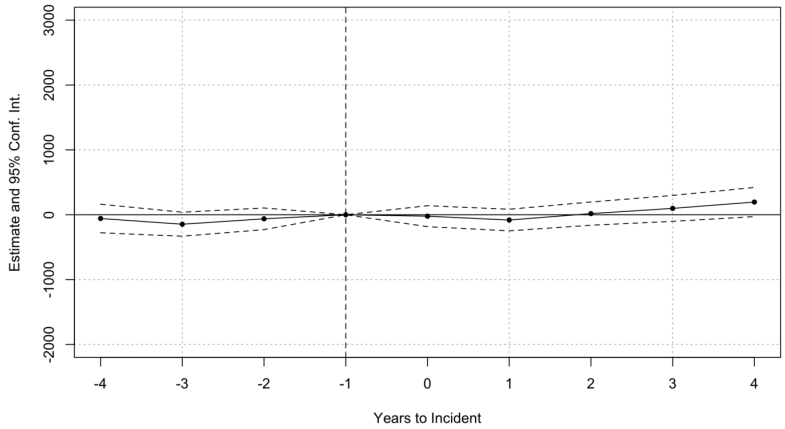
without fatalities in an otherwise identical analysis, I find no effects on donations by such events.

As a plausibility check, I demonstrate that I obtain largely identical results using this data if treatment is instead coded as a fatal school shooting.

S9 Other forms of non-electoral participation

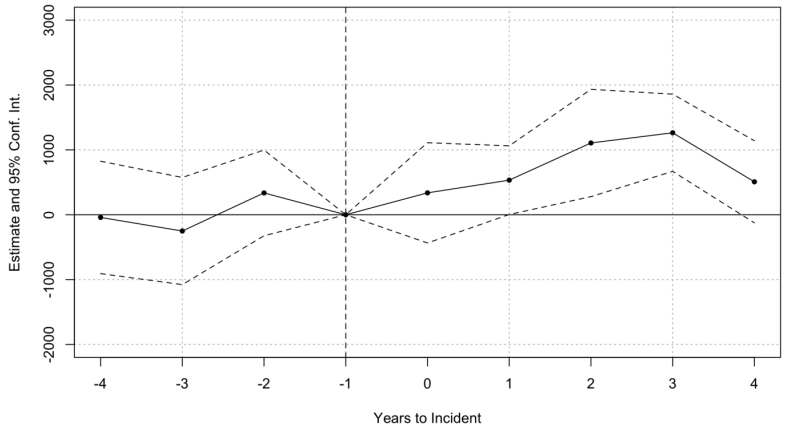
As an additional analysis, I estimate whether the likelihood for individuals to report having engaged in different forms of political participation over the last 12 months increases immediately after school shootings. Specifically, I estimate the effect of school shootings on reporting to have attended local political meetings (Meetings), put up a political sign (Sign), worked for a candidate or campaign (Work), attended a political march, protest, or demonstration (Protest), contacted a public official (Contact), or donated money to a political candidate, campaign, or political organization over the last year.

Figure S12: Effect of school shootings with no fatalities on NRA donations: K-12 school shootings data.



Notes: This figure displays event study estimates for the effect of school shootings without fatalities on donation amounts to the NRA-PVF, using a staggered difference-in-differences design. Treatment is defined as occurrence of an unsuccessfully attempted or non-fatal school shooting, using data from the K-12 school shootings database (Riedman, 2023). The relevant time variable is year. The bars represent 95% confidence intervals. All standard errors are clustered at the county level.

Figure S13: Effect of school shootings with fatalities on NRA donations: K-12 school shootings data.



Notes: This figure displays event study estimates for the effect of fatal school shootings on donation amounts to the NRA-PVF, using a staggered difference-in-differences design. Treatment is defined as a fatal school shooting, using data from the K-12 school shootings database (Riedman, 2023). The relevant time variable is year. The bars represent 95% confidence intervals. All standard errors are clustered at the county level.

To do this, I use data from the 2022 CES, which contains questions on these different forms of political participation (see also appendix Section S7), respondent zip-codes, and a range of individual-level covariates. This is the only wave that contains the relevant dependent variable together with information on gun ownership and during which at least one shooting occurred during the period during which the survey was fielded.

To estimate the effect of school shootings on reporting to have engaged in such activities, I follow Frey and Kirk (2021) and use an unexpected event during survey design (Muñoz et al., 2020). I compare responses by individuals in affected areas immediately before and after a shooting in the county occurred. During the field period of this survey (September 29th to November 8th, 2022), fatal school shootings occurred in 4 different counties. I restrict my analysis to counties with shootings and retain observations in the 14 days before and after a shooting. For each observation, I code whether the observation was recorded before or after the shooting.

To specifically zoom in on the effect of school shootings on political participation by gun owners, I use whether an individual lives in a gun-owning household as a proxy for attitudes toward gun rights. As compared to partisanship or attitudes toward gun control, this variable is less obviously post-treatment, as changes in gun ownership in response to a shooting over such a narrow window are unlikely. Similarly, the desirability bias imposed by that question is likely lower than that of alternative proxies that try to measure gun-rights attitudes. The identification assumption in my estimation is that observations around the cut-off do not differ in any meaningful way besides their exposure to treatment. The randomness in terms of school shootings (see data and method section in the main text) supports this assumption.

I estimate the following equation:

$$Y_i = \gamma \text{post}_i + \lambda \text{gun}_i + \delta \text{post}_i \times \text{gun}_i + X_i'\beta + \varepsilon_i,$$

where Y_i is the outcome of interest for individual i. The variable post_i indicates whether respondent i was interviewed before or after the shooting in their county, while gun_i is an indicator for the respondent living in a gun-owning household. The target estimates are γ and δ, where γ captures the effect of being interviewed

after the shooting for non-gun owners and δ captures the difference in effect size for gun owners. Standard errors are clustered at zip-code level.

Table S2: Political participation after school shootings: regression results.

	Meetings	Sign	Work	Protest	Contact	Donate
(Intercept)	0.095	0.185	-0.093	-0.031	-0.291+	-0.615***
	(0.119)	(0.140)	(0.087)	(0.082)	(0.173)	(0.160)
post	-0.036	-0.107*	-0.046	-0.034	0.021	-0.086
	(0.040)	(0.048)	(0.034)	(0.045)	(0.077)	(0.071)
gunown	-0.054	-0.067	-0.044	-0.080	0.000	-0.017
	(0.052)	(0.071)	(0.040)	(0.050)	(0.084)	(0.076)
post × gunown	0.152*	0.195*	0.137*	0.123*	0.020	0.184+
	(0.069)	(0.084)	(0.058)	(0.059)	(0.124)	(0.103)
Indiv. covariates	✓	✓	✓	✓	✓	✓
Num. Obs.	267	267	267	267	267	267
R^2	0.074	0.085	0.094	0.109	0.128	0.184

Notes: This table presents linear regression estimates for the effect of school shootings on different forms of non-electoral participation. This analysis compares individuals interviewed within 14 days before a school shooting in a given county to individuals interviewed within 14 days after. The dependent variables are whether within the last year an individual attended local political meetings (Meetings), put up a political sign (Sign), worked for a candidate or campaign (Work), attended a political march, protest, or demonstration (Protest), contacted a public official (Contact), or donated money to a political candidate, campaign, or political organization (Donate). All dependent variables are binary measures. Individual level covariates include: age, gender, party identification, employment status, race, income. Standard errors are clustered by zip-code. Signif. Codes: ***: 0.001, **: 0.01, *: 0.05, +: 0.1.

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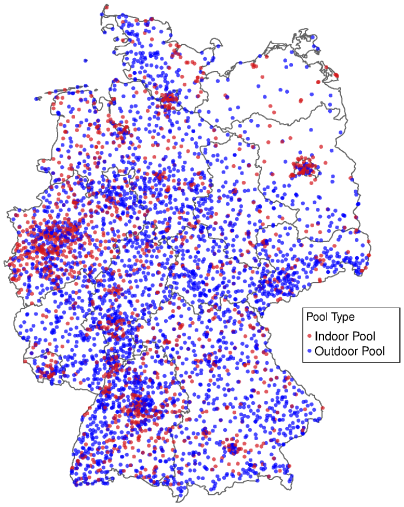
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Appendix to Chapter 2

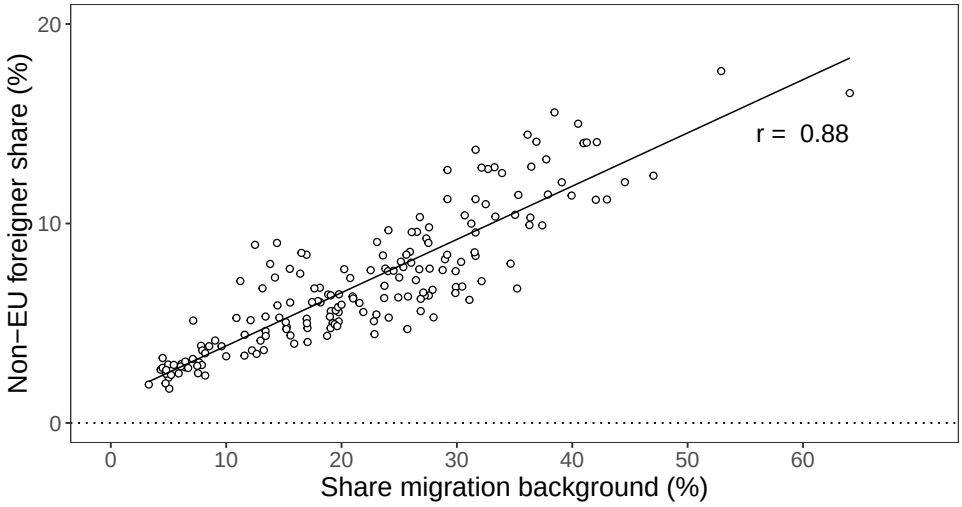
S1 Additional contextual information

Figure S1: Public pools across Germany.



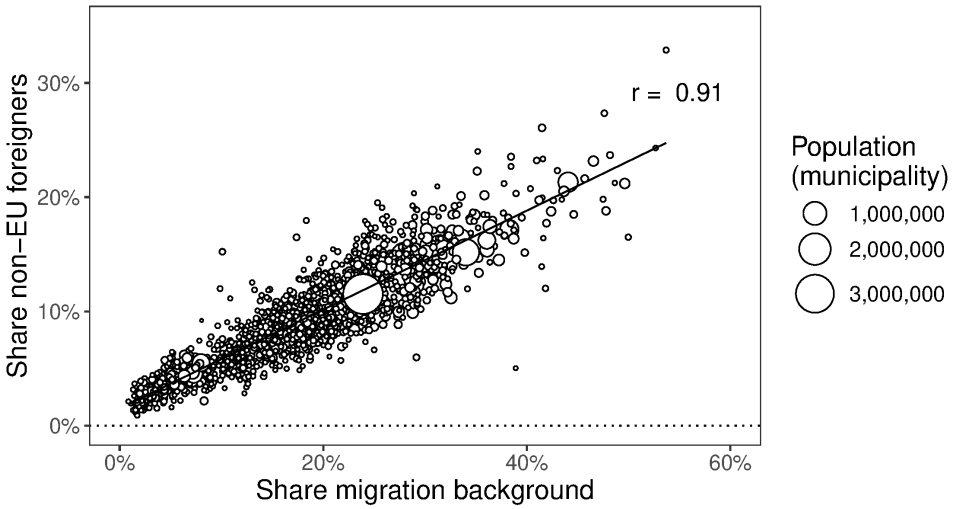
Notes: The figure shows the locations of public pools across Germany. Red dots indicate indoor pools ('Hallenbäder'), and outdoor pools ('Freibäder') are plotted as blue dots. Coordinate data were obtained from © GeoBasis-DE/BKG (2021), courtesy of the © Bundesinstitut für Sportwissenschaft.

Figure S2: Non-EU foreigner share and population share with a migration background, county level.



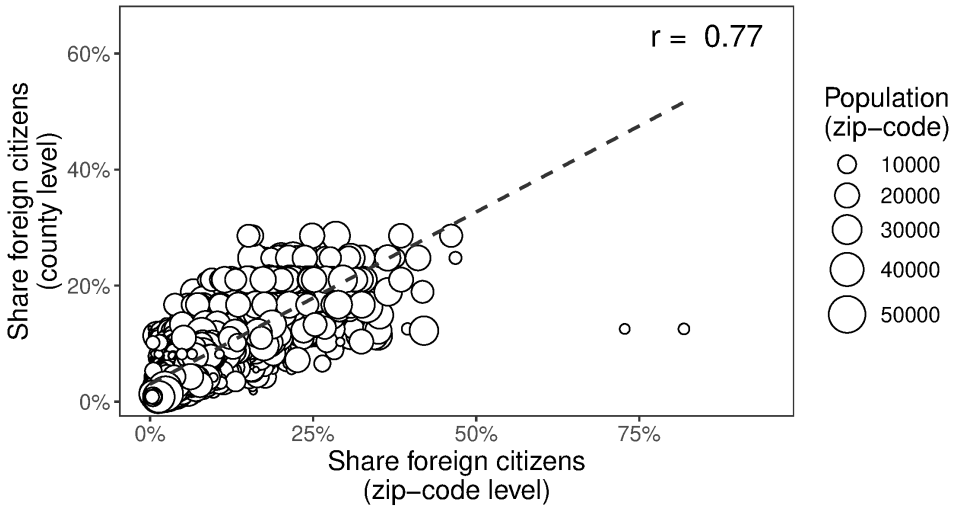
Notes: The figure shows the bivariate relationship between the county-level population share with a migration background (2018) and our main measure of local ethnic heterogeneity, the share of non-EU foreigners (2021). Each observation corresponds to one county. In Germany, a person is considered to have a migration background if they or at least one of their parents were not born with German citizenship. The bivariate correlation is $r = 0.88$.

Figure S3: Non-EU foreigner share and population share with a migration background, municipality level.



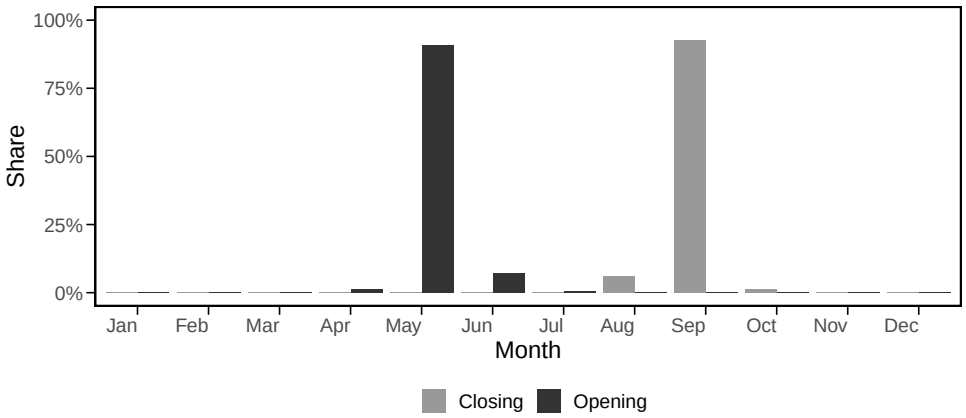
Notes: The figure shows the bivariate relationship between the municipality-level population share with a migration background and the share of non-EU foreign citizens, both measured in 2011 at the municipality (*Gemeinde*) level. The data are available for municipalities with more than 10,000 inhabitants ($N = 1,570$). Each observation corresponds to one municipality. A person is considered to have a migration background if they or at least one of their parents were not born with German citizenship. The bivariate correlation is $r = 0.91$. Dots are proportional to the municipality population.

Figure S4: Correlation between foreigner share at the zip-code and county levels.



Notes: The figure shows the bivariate relationship between the county-level and zip-code-level foreigner share. Both measures are based on grid-level data from the 2011 census, which we aggregated to the county/zip-code level. Each point corresponds to one zip-code. Dots are proportional to the zip-code population. The bivariate correlation is $r = 0.77$. The two outlier zip-codes with more than 50% foreigner share are located in central Hamburg (*Hamburg-Mitte*, zip-codes 20354 and 20095).

Figure S5: Outdoor pools opening and closing months.



Notes: The figure shows the relative frequency of opening and closing months in outdoor swimming pools in Germany. The figure is based on data provided by *Bäderleben*.

S1.1 Frequency of violent crimes in public pools

To estimate the number of total crimes in public pools in Germany we use police data for the 2022 calendar year. In 15 out of 16 states the police use a separate category for crimes that occurred in public pools. These states account for around 99% of the German population. Bremen is the only state that does not provide such data. We take this data from *Der Spiegel*, who requested it from state police forces (Spiegel, 2023). In 2022, a total of 9,398 crimes occurred in these states in public pools. We estimate the total number of crimes by extrapolating this number for the total population of Germany. This yields an estimate of 9,974 total crimes in pools in Germany in 2022.

In 8 of the 15 states there exists information on the number of violent crimes. For these states, we calculate the share of violent crimes in all crimes by summing up all violent incidents and dividing by the number of total crimes. This yields an estimate of 24% violent crimes. Similarly, 6 states provide information on the citizenship of perpetrators and suspects. Here, we estimate that around 69% of those have German citizenship.

To estimate the total number of violent crimes with foreign perpetrators/suspects, we proceed as follows: We multiply the estimated total number of crimes and the estimate of the share of violent crimes to obtain an estimate for the total number of violent crimes in public pools. This yields an estimate of 2,274 violent crimes. We then estimate the number of violent crimes by foreigners by multiplying the total number of violent crimes with the share of foreign perpetrators estimated above (31%). This yields an estimate of 712 violent crimes with foreign perpetrators.

To address the concern that violent crimes and non-violent crimes have different shares of foreign perpetrators, we also provide a second estimate. We use the *Polizeiliche Kriminalstatistik* (PKS) to obtain the overall share of foreigners among all violent crimes in Germany. This number is provided directly by the PKS (Summenschlüssel 892000). Foreign perpetrators are responsible for 38% of overall violent crime. Using this number, we estimate a total of 872 violent crimes in public pools with foreign perpetrators.

To obtain an upper limit on the number of public pools that have at least one instance of a violent crime with a non-German perpetrator, we divide the total number of violent crimes with foreign perpetrators by the total number of pools

we observe in our data (4,669). This calculation suggests that more than 80% of public pools experience no violent crimes with non-German perpetrators.

S2 Survey on threat perceptions in public pools

To assess threat perceptions in public spaces, we conducted a survey with a total sample of $N = 3,576$ respondents.[1] The survey was designed to be representative of the German population, using quotas to ensure appropriate demographic distributions across age, gender, and state, with soft quotas for education. Survey recruitment was handled by Bilendi Germany[2]. The survey was fielded during the outdoor pool season 2024 (August 2024).

Table S1: Survey summary statistics.

Statistic	N	Mean	St. Dev.	Min	Max
Age	3,576	46.078	15.930	18	94
Male	3,576	0.504	0.500	0	1
Pool visitor	3,576	0.691	0.462	0	1
Education: No degree	3,576	0.009	0.093	0	1
Education: Gen. school (Hauptschule)	3,576	0.107	0.309	0	1
Education: Intermed. school (Mittlere Reife)	3,576	0.359	0.480	0	1
Education: High school diploma	3,576	0.233	0.423	0	1
Education: University	3,576	0.293	0.455	0	1
Family Status: Single	3,576	0.399	0.490	0	1
Family Status: Married	3,576	0.469	0.499	0	1
Family Status: Divorced	3,576	0.100	0.300	0	1
Family Status: Widowed	3,576	0.032	0.175	0	1

We first inform potential respondents about the study, data privacy, and points of contact. After obtaining informed consent and ensuring that respondents are above the age of 18 and currently reside in Germany, we proceed with our questionnaire.

We asked respondents a variety of questions about perceptions of different public social spaces. In essence, our survey was designed to assess whether respondents perceive public swimming pools as threatening and whether their perceptions are associated with voter preferences in a way that is consistent with our theoretical framework. Overall, our results consistently show that threat perceptions are uniquely heightened in public swimming pools, especially when compared to

[1] This number refers to the number of respondents used for the final analyses, i.e., respondents who passed the attention check. More information on the attention check below.

[2] Bilendi is a multi-national survey provider for academic and non-academic research. For further information on Bilendi Germany see: `https://www.bilendi.de`

non-politicized locations such as supermarkets. To ensure that respondents were answering questions in a meaningful way, we included an attention check around halfway through the survey. For our final analyses, we only retain responses from individuals who have passed the attention check. Table S1 shows descriptive statistics of our survey sample.

S2.1 Threat perceptions in public pools

To examine threat perceptions in public pools, the survey included a series of items to assess individuals' feelings of unsafety and disorder in public pools as well as other (less politicized) public social spaces.

We asked respondents whether they had ever heard about crimes in public swimming pools in Germany in the media and whether they had ever witnessed crimes themselves. Specifically, we ask, "Have you ever heard about crime in public swimming pools in the media? (No/Local media/National media)" and "Have you ever witnessed crime when visiting a public swimming pool? (Yes/No)." In line with official crime statistics (see Section S1.1), our results indicate that few people experience crime (18%), but the vast majority of respondents in our survey have heard about such incidents in the news media (71%, see Table S2).

Table S2: Heard of and experienced crime in swimming pools.

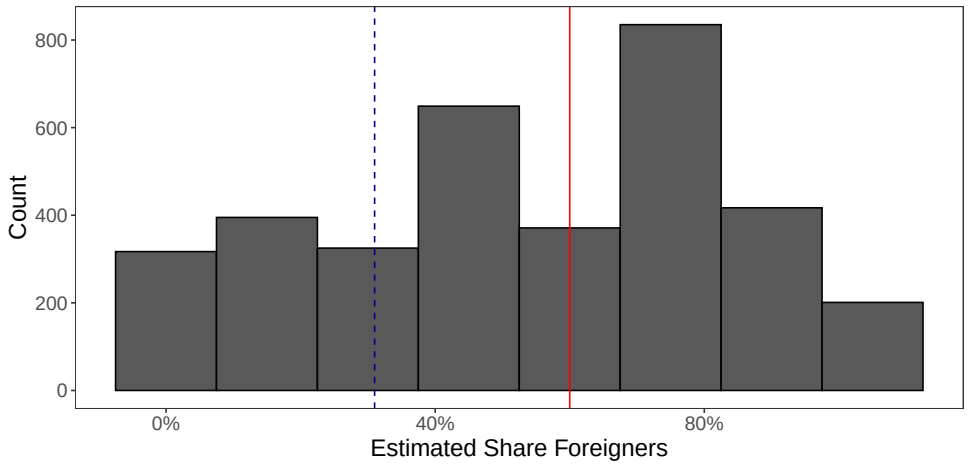
Number of respondents	Heard about crime	Experienced crime	Heard/Experienced ratio
3,510	70.97%	17.95%	3.954

Notes: This table presents the shares of people who answered affirmatively to the questions: "Have you ever heard about crime in public swimming pools in the news? Please tick all that apply [never, in local media/yes, in nationwide media/yes]" and "Have you ever experienced crime in public swimming pools? [yes/no]". Respondents are considered affirmative for the former question if they have not responded "never".

Given a national narrative that portrays diversity in pools as threatening (see Section 3), we expect that respondents will tend to overestimate the share of crimes in swimming pools committed by foreign perpetrators. As we have discussed in Section 3, the narrative replicated by both the far-right and in the media uniquely links crimes occurring in swimming pools to immigration, despite the fact that the majority of crimes are not attributable to foreigners. Accordingly, we expect that citizens strongly overestimate the true share of crimes with foreign perpetrators.

As discussed in Section S1.1, we estimate that around 69% of offenders have German citizenship. To examine individuals' (mis-)perceptions of the share of foreign offenders, we ask respondents what percentage of violent crimes in public pools they believe are committed by foreigners. The distribution of responses is displayed in Figure S6. In our survey, respondents overestimate the share of foreigners by about 93%. We interpret these findings as suggestive that the far-right campaign to politicize public pools skews individuals' perceptions toward seeing diversity as threatening in this space. We provide additional evidence on this below.

Figure S6: Estimated share of crimes in swimming pools committed by foreigners.



Notes: The figure shows the distribution of responses to the estimated share of foreign offenders for violent crime in swimming pools in Germany. The dark blue dashed line indicates the true (estimated) share (see Section S1.1). The red straight line represents the median response in the survey.

To examine the degree to which individuals feel uniquely threatened in public pools as compared to other (less politicized) spaces, we compare threat perceptions in public swimming pools to two other common social spaces—supermarkets and public parks, two spaces that also bring together diverse groups of people. Broadly, we ask respondents detailed questions about their perceived safety, the behavior of others, and the salience of this space in public debates.

Overall, our results show that threat perceptions are uniquely heightened in pools, as compared to non-politicized places such as supermarkets. In Table S3

we show shares of survey respondents that respond affirmatively to questions about their perceived threats in public swimming pools, public parks, and supermarkets. Questions are asked on a five-point scale from "Strongly disagree" to "Strongly agree." We calculate shares for respondents that respond with "Agree" or "Strongly agree." In 11/14 questions, respondents evaluate the degree of reckless behavior and crime, their feeling of insecurity/discomfort, and the salience and polarization of public debates to be highest with respect to public swimming pools. For two of the other three questions ("women must be cautious" and "public debates are polarized"), public parks and pools differ by only one percentage point. A higher share of respondents believes criminal incidents occur frequently in public parks (27%) as compared to public swimming pools (22%). This is likely due to the fact that the number of crimes that occur in public parks is, in fact, substantially higher than in public swimming pools. In Berlin, for example, the number of crimes registered by the police in public parks is about seven times higher than in public pools (Berliner Morgenpost, 2021). However, reckless behavior and frequent sexual harassment are perceived to be more common in public pools.

Table S3: Threat perceptions in different public social spaces.

	Pools	Park	Supermarket
1. Reckless behavior is common here	0.37	0.31	0.19
2. Criminal incidents occur frequently	0.22	0.27	0.12
3. Sexual harassment is common here	0.24	0.21	0.05
4. Women have to be especially careful here	0.35	0.36	0.08
5. I feel insecure here	0.27	0.27	0.08
6. I avoid this place because of the behavior of others	0.30	0.27	0.07
7. In this place you feel increasingly like a stranger in your own land	0.34	0.33	0.19
8. Cultural conflicts are common here	0.27	0.24	0.09
9. I feel uncomfortable here because of many foreigners	0.30	0.30	0.13
10. Since 2015 I feel increasingly uncomfortable here	0.28	0.28	0.11
11. Frequent media reports about conflict in this space	0.20	0.16	0.07
12. Frequent political debates about this place in public discourse	0.14	0.14	0.06
13. Public discourse about this place is polarized	0.25	0.26	0.09
14. This place should receive more attention in public discourse	0.33	0.32	0.14

Notes: This table displays percentage shares of survey respondents that agree with statements about perceived threat in public social spaces. Numbers reflect percentage shares of respondents who responded with "Strongly agree" or "Agree" on a five-point scale from "Strongly disagree" to "Strongly agree".

We then estimate whether hearing about crimes in public swimming pools is associated with greater perceived threat, as our theoretical framework would

suggest. Respondents who have "heard about" crime in public pools in the news media have been exposed to the politicization of public pools. To do this, we construct a perceived threat index out of the questions in Table S3. Specifically, we average responses to Q1-Q10 to construct a general perceived threat index as well as responses to Q7-Q10 to construct an index out of questions that directly relate to perceived threat posed by foreigners. We reverse the scales to have higher values correspond to greater perceived threat. As the independent variable we use responses to the questions about hearing about crime in public pools in the media (see also Table S2). The results are displayed in Table S4. Overall, our results provide correlational evidence that exposure to media reports about out-group crime in public pools increases threat perceptions related to diversity in this public space.

Table S4: Perceived threat in pools and hearing about crime in the media.

	Threat Index (1)	Threat Index Foreigners (2)
Hear about crime	0.634***	0.575***
	(0.046)	(0.041)
Observations	2,570	2,896

Notes: Results from OLS regression where perceived threat is the dependent variable. First column index is constructed by averaging responses to all questions about perceived threat (Q1-Q10). Second column index is constructed by restricting to questions that specifically relate to questions about threat posed by foreigners (Q7-Q10). Full list of questions can be found in Table S3. Hearing about crime is a binary variable that is 0 if survey respondent indicates to never have heard about crime in public swimming pools, and 1 otherwise. Individual level covariates include: age, gender, education, family status, religion, and whether both parents were born in Germany. Standard errors are clustered on zip-code level. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1.

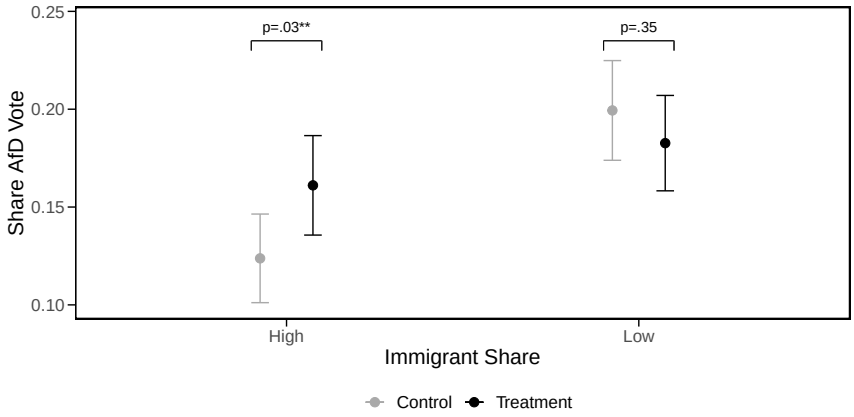
S2.2 Survey experiment

As an exploratory exercise, we randomize whether we ask respondents about their vote intention before or after we ask them questions about their perceptions of

swimming pools.[3] Threat perceptions might be so deeply entrenched that merely priming individuals about their personal experiences in public pools could raise support for the AfD.

The results of this exercise are displayed in Figure S7. We compare individuals who have been asked about their vote intention before (control) and after asking questions about public pools (treatment), splitting our sample into low and high-diversity areas by non-EU foreigner share (using the same measure as in the main text). Similar to our main results, we find strong effect heterogeneity by local diversity. In high-diversity localities, we find a strong, statistically significant, positive effect of priming individuals about public pools on AfD support (3.7 p.p., $p < 0.03$). In the low-diversity subset, we do not find significant results. The difference in means is slightly negative (around 1.7 p.p.), i.e., priming individuals about swimming pools tends to, if anything, decrease support for the AfD. Threat perceptions linked to diversity in public pools appear so deeply entrenched that merely priming respondents to think about their personal experiences in public pools increases electoral support for the far-right.

Figure S7: Priming individuals on pools and AfD vote intention.



Notes: Results from difference of means test where AfD vote intention is the dependent variable. Treatment is being asked questions about pools before being asked for vote intention. Control group is being asked for vote intention first. P-value is estimated through t-test and indicated above mean estimates. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1.

[3]It is a priori unclear whether simply priming respondents' experiences in public pools would result in higher AfD support. This is because the survey experiment cannot replicate actual, personal experiences in public pools. Consequently, we view this as an exploratory exercise and did not preregister this analysis.

S2.3 Additional results on the mechanism

In addition, we examine whether respondents' assessment of local diversity varies systematically with visiting public pools. Table S5 estimates the association between pool visitorship, coded as 1 if the respondent indicates to visit the pool at least sometimes during the outdoor pool season, and the estimated share of foreigners in the neighborhood/county. Conditional on individual level covariates and zip-code fixed effects, i.e., holding actual levels of diversity constant, we see that pool visitorship is associated, on average, with an estimated local foreign-population share that is between 2 and 2.5 p.p. higher.

Table S5: Pool visitorship predicts perceptions of local diversity.

	Share County (1)	Share Neighborhood (2)
Pool Visitor	2.55**	2.33*
	(1.18)	(1.37)
Observations	3,330	3,330
zip fixed effects	✓	✓
individual level covariates	✓	✓

Notes: Results from OLS regression where estimate of foreigner share in (1) county or (2) neighborhood (in p.p.) is the dependent variable. Pool visitor is a binary variable that is 0 if survey respondent indicates to never go to the pool during outdoor pool season, and 1 otherwise. Individual level covariates include: age, gender, education, family status, religion, and whether both parents were born in Germany. Standard errors are clustered on zip-code level. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1.

We also explore the relationship between witnessing social norm violations and AfD voting as an alternative mechanism that may drive part of our results (see Section 2). We run a linear probability model with AfD vote intention as the outcome variable. As the independent variable, we use a binary indicator that equals one for respondents who self-report having ever witnessed a crime in public pools. Table S6 shows that voters who self-report having witnessed a crime in a public pool are more likely to support the far-right AfD. However, this effect pertains to only a small minority of respondents: while the majority have heard

Table S6: Witnessing crime in pools and AfD voting.

	Vote AfD
	(1)
Witnessing crime	0.176***
	(0.019)
Observations	3,510

Notes: Results from OLS regression where AfD vote intention is the dependent variable. Witnessing crime is a binary variable that is 0 if survey respondent indicates to never have witnessed crime in public swimming pools, and 1 otherwise. Individual level covariates include: age, gender, education, family status, religion, and whether both parents were born in Germany. Standard errors are clustered on zip-code level. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1.

about crimes in public pools through the news media, only 18% self-report to have personally witnessed such incidents (see Table S2).

S3 Data on swimming pool locations

S3.1 Data source

Bäderleben supplies data on pools and places suitable for swimming, some of which are not classical indoor and outdoor pools. For our analysis, we subsumed some of these categories under the general label "outdoor pool" and removed others. Specifically, we consider convertible pools, leisure pools, and hybrid pools as forms of outdoor pools to which our argument generally applies. These places usually have outdoor areas that attract citizens during the summer and exhibit the same seasonal variation in opening/closing dates. We remove pools that are not generally open to the public, e.g., pools in schools or hospitals. We also remove lakes and seas that are not pools in the classical sense and are unlikely to receive the same level of visitorship. Table S7 summarizes the *Bäderleben* categories together with our coding scheme. Additional background information on the data sources is available at the project website: `https://baederleben.de/`.

We validated the accuracy of our data using a similar dataset published by the Deutsche Gesellschaft für das Badewesen (2023). The two data sources agree on the presence of outdoor pools in 94% of zip-codes. We manually validated a sample of conflicting cases, for which the information in our primary data source was correct in all cases.

Table S7: Bäderleben pool types.

Type	German Label	Coding	Count	Note
Outdoor Pool	Freibad	Outdoor Pool	2,415	-
Convertible Pool	Cabriobad	Outdoor Pool	18	Convertible roof
Leisure Pool	Freizeitbad	Outdoor Pool	362	Water slides, often outside
Hybrid Pool	Kombibad	Outdoor Pool	371	Combined indoor & outdoor pool
Indoor Pool	Hallenbad	Indoor Pool	1,505	-
Hotel Pool	Hotelbad	Excluded	984	Located in hotel
Hospital Pool	Klinikbad	Excluded	650	Located in medical facility
Natural Pool	Naturbad	Excluded	1,902	Places at seas and lakes
School Pool	Schulbad	Excluded	883	Located in school
Other Pool	Sonstiges Bad	Excluded	390	E.g. in gym, spa, or retirement home

S3.2 Predictors of outdoor pool presence

In Figure S8, we examine the predictors of outdoor pool presence. We estimate OLS regression models where we regress a binary indicator for outdoor pool presence in a given locality on covariates. We estimate separate models for each covariate. We also note that in contrast to our main analyses, we conduct this analysis at the municipality level (not the zip-code level). We do this because essential covariates such as income levels or unemployment rates are unavailable at the zip-code level. On average, municipalities are slightly smaller in size than zip-code regions. Germany consists of about 10,700 municipalities compared to about 8,000 zip-code areas.

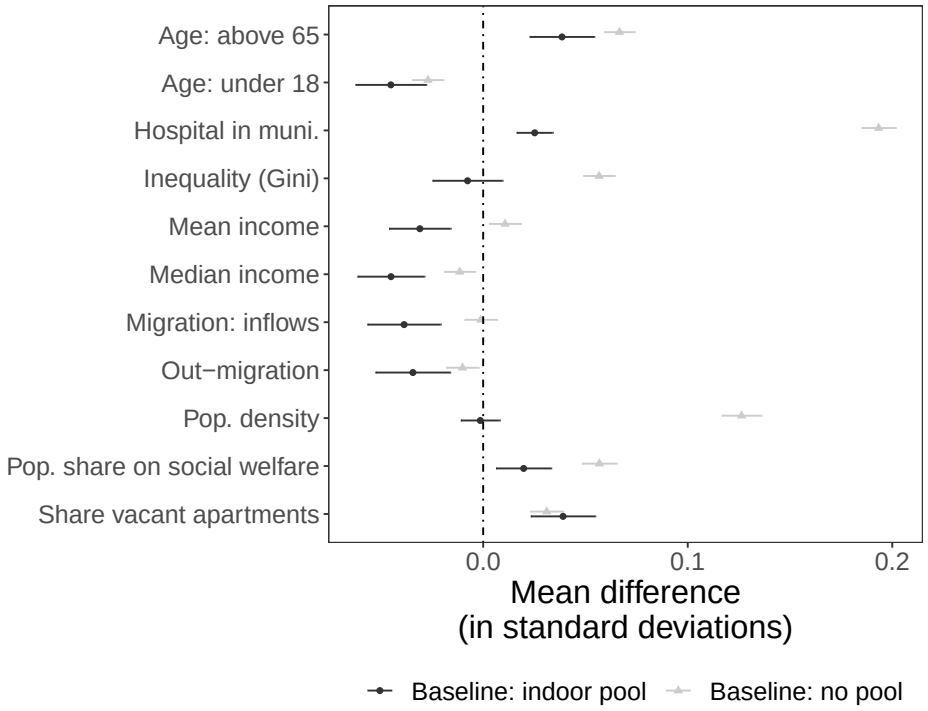
We use the following data sources:

- Population distribution by age bracket as of 2014 (Source: *Statistische Ämter des Bundes und der Länder*)
- Data on the street addresses of all hospitals as of 2021 (Source: *Verzeichnis der Krankenhäuser*); we code a binary variable that indicates the presence of at least one hospital in a given municipality.
- Inequality, mean income, median income: based on complete tax registry data (*Lohn- und Einkommensteuerstatistik*). The data contain information on all tax returns filed in Germany for 2010. All variables are measured on the basis of post-tax household income, adjusted for household size. See Riaz (2023) for details on data processing.
- Pop. density: based on total population in 2014 (Source: *Statistische Ämter des Bundes und der Länder*) and geometries of municipalities (Source: Bundesamt für Kartographie und Geodäsie). Variable indicates population per square kilometer.
- Share of vacant apartments by municipality, based on the census of all housing units 2011.
- Unemployment rate: based on total population in 2014 (Source: *Statistische Ämter des Bundes und der Länder*) and the number of persons living in households that are eligible for benefits under SGB II in the municipality (Source:

Bundesagentur für Arbeit).[4] Calculated as the proportion of persons in eligible households relative to the total municipality population.

- In-/out-migration by municipality (2014) based on the *Wanderungsstatistik*, which registers all moves across municipality borders. We scale these variables by total population size.

Figure S8: Predictors of outdoor pool presence.



Notes: The figure shows the predictors of outdoor pool presence across localities. We provide more details on the analysis and data sources in Section S3.2 in the SI.

[4]SGB II is the legal basis for what is commonly known as Hartz IV, a form of unemployment benefit that serves as a basic social security net for people who are capable of working but are either unemployed or underemployed and are not able to sustain themselves financially. This variable captures the share of people living in households (with at least one person) eligible for benefits under this scheme.

S4 Robustness & additional tests

We conducted a series of additional robustness checks and additional tests to further scrutinize our results.

S4.1 Alternative specifications

First, we demonstrate that our results are robust to using standard errors clustered by zip-code (see Table S8) or county (see Table S9) rather than by respondent.

Table S8: Standard errors clustered by zip-code.

	AfD Support (in p.p.)		
Non-EU Immigrant share	Full sample	High	Low
	(1)	(2)	(3)
Outdoor pool × Pool season	0.024	0.793***	-0.144
	(0.110)	(0.282)	(0.123)
R^2	0.892	0.892	0.892
Observations	1,160,202	256,085	904,117
Respondent fixed effects	✓	✓	✓
Interview date fixed effects	✓	✓	✓

Notes: Results from OLS regressions where AfD support (in p.p.) is the dependent variable. The treatment is defined as the presence of a public outdoor pool in a given zip-code. Pool season is a dummy variable that equals one for survey responses recorded between May and September. Standard errors are clustered by zip-code. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1.

Second, we demonstrate in Table S10 that our results are robust to adjusting for flexible seasonal trends by region (state).

Third, we demonstrate that our results hold when narrowing down the control group. For our main results, we compare respondents in localities with outdoor pools to respondents in localities with either (i) no public swimming pool or (ii) an indoor (but no outdoor) swimming pool. The underlying logic is that we do not expect seasonal patterns of pool visitorship in either of these two groups. In Tables S11 and S12 in the SI, we demonstrate that we obtain similar results when

Table S9: Standard errors clustered by county.

Non-EU Immigrant share	AfD Support (in p.p.) Full sample (1)	High (2)	Low (3)
Outdoor pool × Pool season	0.024	0.793***	-0.144
	(0.143)	(0.258)	(0.125)
R^2	0.892	0.892	0.892
Observations	1,160,202	256,085	904,117
Respondent fixed effects	✓	✓	✓
Interview date fixed effects	✓	✓	✓

Notes: Results from OLS regressions where AfD support (in p.p.) is the dependent variable. The treatment is defined as the presence of a public outdoor pool in a given zip-code. Pool season is a dummy variable that equals one for survey responses recorded between May and September. Standard errors are clustered by county. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1.

we narrow down the control group to either of these two subgroups. Using localities with indoor pools as the control group substantially improves the covariate balance between the treated and control groups at the expense of sample size (see Figure S8).

Fourth, we present evidence that the patterns we identify are stable over time. When we split our analysis by survey year (see Table S13), we find virtually identical point estimates in 2017 and 2018. This speaks against the possibility that our results may be confounded by the electoral campaign for the 2017 federal election.

Fifth, we address concerns about spillovers between the treated and control groups, that is, the possibility that some respondents in localities without outdoor pools might commute to localities with outdoor pools. Specifically, we demonstrate that we obtain similar results when restricting our sample to comparisons between localities with outdoor pools. In Table S14, we show that our conclusions remain unchanged when we restrict the sample to respondents in localities with outdoor pools and test for differential trends in far-right support during the outdoor pool season between ethnically homogeneous and heterogeneous localities.

Table S10: Allowing for flexible seasonal patterns by region (state).

Non-EU Immigrant share	AfD Support (in p.p.) Full sample (1)	High (2)	Low (3)
Outdoor pool × Pool season	-0.048	0.595**	-0.167
	(0.112)	(0.289)	(0.124)
State × Pool season	✓	✓	✓
R^2	0.892	0.892	0.892
Observations	1,160,202	256,085	904,117
Respondent fixed effects	✓	✓	✓
Interview date fixed effects	✓	✓	✓

Notes: Results from OLS regressions where AfD support (in p.p.) is the dependent variable. The treatment is defined as the presence of a public outdoor pool in a given zip-code. Pool season is a dummy variable that equals one for survey responses recorded between May and September. The specification includes an indicator for each state (Bundesland) interacted with the summer dummy variable. Standard errors are clustered by respondent. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1.

Table S11: Subset of localities with pools (indoor or outdoor).

Non-EU Immigrant share	AfD Support (in p.p.) Full sample (1)	High (2)	Low (3)
Outdoor pool × Pool season	0.013	0.648*	-0.082
	(0.154)	(0.365)	(0.174)
R^2	0.891	0.890	0.890
Observations	664,826	95,399	569,427
Respondent fixed effects	✓	✓	✓
Interview date fixed effects	✓	✓	✓

Notes: Results from OLS regressions where AfD support (in p.p.) is the dependent variable. The treatment is defined as the presence of a public outdoor pool in a given zip-code. Pool season is a dummy variable that equals one for survey responses recorded between May and September. For this analysis, we subset to localities with pools (outdoor or indoor). Standard errors are clustered by respondent. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1.

Table S12: Excluding indoor pools.

Non-EU Immigrant share	AfD Support (in p.p.) Full sample (1)	High (2)	Low (3)
Outdoor pool × Pool season	0.023	0.839***	-0.173
	(0.117)	(0.290)	(0.134)
R^2	0.892	0.892	0.892
Observations	970,244	210,254	759,990
Respondent fixed effects	✓	✓	✓
Interview date fixed effects	✓	✓	✓

Notes: Results from OLS regressions where AfD support (in p.p.) is the dependent variable. The treatment is defined as the presence of a public outdoor pool in a given zip-code. Pool season is a dummy variable that equals one for survey responses recorded between May and September. For this analysis, we exclude localities with indoor pools. Standard errors are clustered by respondent. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1.

Table S13: Separate estimation by survey year.

Year	AfD Support (in p.p.) Full sample (1)	2017 (2)	2018 (3)
Outdoor pool × Pool season	0.793***	0.701*	0.719*
	(0.283)	(0.374)	(0.382)
R^2	0.892	0.904	0.912
Observations	256,085	135,000	121,085
Respondent fixed effects	✓	✓	✓
Interview date fixed effects	✓	✓	✓

Notes: Results from OLS regressions where AfD support (in p.p.) is the dependent variable. The treatment is defined as the presence of a public outdoor pool in a given zip-code. Pool season is a dummy variable that equals one for survey responses recorded between May and September. The sample includes zip-code regions with high immigrant shares (upper quartile). Standard errors are clustered by respondent. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1.

Finally, we address the possibility that our results might be driven by concerns about objective crime rates rather than out-group exposure in public pools. Specifically, it might be the case that concerns about objective local crime rates are particularly salient during the summer months, which in turn might drive far-right support. To address this point, we demonstrate that our results are robust to controlling for seasonal patterns in concerns about objective local crime rates (see Table S15).

Table S14: Differential trends in outdoor-pool localities with high vs. low non-EU foreigner shares.

	AfD Support (in p.p.)
	(1)
High non-EU foreigner share × Pool season	0.613**
	(0.274)
R^2	0.890
Observations	474,868
Respondent fixed effects	✓
Interview date fixed effects	✓

Notes: Results from OLS regressions where AfD support (in p.p.) is the dependent variable. The sample only includes respondents in localities with outdoor pools. We test for differential trends in far-right support during the outdoor pool season in localities with a high (low) share of foreigners. We use the same discretized proxy measure for ethnic diversity as for our main analyses, i.e., the share of non-EU foreigners (4th quartile). The base terms for the foreigner share and the outdoor pool season variables are absorbed by the respondent and interview date fixed effects. Standard errors are clustered by respondent. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1.

Table S15: Controlling for objective local crime rates.

Non-EU Immigrant share	AfD Support (in p.p.) Full sample (1)	High (2)	Low (3)
Outdoor pool × Pool season	-0.035	0.753***	-0.174
	(0.114)	(0.287)	(0.125)
Crime rate × Pool season	-3.06*	-5.62	-2.94
	(1.63)	(3.63)	(2.34)
R^2	0.892	0.892	0.892
Observations	1,160,202	256,085	904,117
Respondent fixed effects	✓	✓	✓
Interview date fixed effects	✓	✓	✓

Notes: Results from OLS regressions where AfD support (in p.p.) is the dependent variable. The treatment is defined as the presence of a public outdoor pool in a given zip-code. Pool season is a dummy variable that equals one for survey responses recorded between May and September. We control for the average county level crime rate between 2015 and 2019. The crime rate is calculated on the basis of official police statistics as the total number of crime incidents in a given county divided by county population size (as of 2021). Standard errors are clustered by respondent. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1.

S4.2 Seasonal effects for other public spaces

A potential concern regarding our empirical strategy is that as people spend more time outdoors, encounters with out-groups might increase during the summer all across Germany, leading to a general rise in immigration salience and, consequently, far-right support. This raises the question of whether our observed effects are specific to outdoor pools or if they could be attributed to other public social spaces. We address this concern in multiple ways.

First, we note that our empirical strategy focuses on the *differential* increase in far-right support in localities with outdoor pools compared to those without. If there is a general "summer effect" that elevates far-right support across Germany, our findings indicate that the increase is more pronounced in high-diversity areas with outdoor pools. This suggests that the effects we observe are not solely due to seasonal factors but are specifically linked to the presence of outdoor pools in high-diversity localities. This differential increase may be attributable to the politicization and threat perceptions surrounding outdoor pools in the context we study (see Sections 3 and S2).

Second, we empirically test whether AfD support is generally higher during the outdoor pool summer season, regardless of the presence of outdoor pools. To do this, we analyze seasonal differences in AfD support within the subset of high-diversity localities without outdoor pools. For this analysis, we examine within-individual seasonal changes in AfD support. The results, presented in Table S16, show that AfD support is, if anything, slightly lower during the summer season compared to the off-season in these localities. We interpret this as evidence against a general increase in AfD support during the summer, suggesting that the effects we observe are specifically linked to the presence of outdoor pools in high-diversity areas.

Third, we test a direct empirical implication of the concern outlined above. If it were the case that out-group exposure generally increases during the summer and drives part of our results, we would expect such dynamics to be particularly pronounced in high-diversity localities with high population density. Such localities are more likely to facilitate repeated, fleeting encounters in public squares, supermarkets, or shopping streets. To test this, we estimated additional specifications allowing for differential summer trends based on population density. The

Table S16: Summer effects.

	AfD Support (in p.p.) (1)
Pool season	-0.203*
	(0.119)
R^2	0.892
Observations	206,517
Respondent fixed effects	✓

Notes: Results from OLS regressions where AfD support (in p.p.) is the dependent variable. Pool season is a dummy variable that equals one for survey responses recorded between May and September. The sample includes zip-code regions with high immigrant shares (upper quartile) without outdoor pools. Standard errors are clustered by respondent. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1.

results, presented in Table S17, show no evidence of a differential increase in far-right support in high-density areas during the summer months (see columns 1, 2, 5, and 6). Importantly, our main effect estimates remain unchanged in these specifications (columns 5 and 6).

Finally, we zoom in on a specific seasonal public space: natural pools at lakes and seas. We collected data on the presence of natural pools ("Naturbäder," see Table S7) and conducted several supplementary analyses. First, we split the sample based on natural pool presence (Table S18) and find marginally stronger effect estimates in localities without natural pools. Second, we estimate a specification that allows for differential trends in localities with natural pools (Table S19) and find that our main estimates remain unchanged. Third, we used natural pool presence as a placebo treatment (Table S20) and find null results. These findings suggest that in areas where natural pools are available, some natives with pre-existing anti-immigrant attitudes might opt for these alternatives over public outdoor pools, potentially attenuating the observed effects on far-right support (Table S18). However, since the correlation between natural pool presence and outdoor pool presence is low (r = 0.027), this mechanism does not bias our main estimates (Table S19). Overall, we do not find evidence that natural pools them-

Table S17: Population density placebo test.

Non-EU Immigrant share	AfD Support (in p.p.)							
	High		Low		High		Low	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Pop. density × Pool season	-0.046		-0.131*		-0.015		-0.154**	
	(0.055)		(0.069)		(0.056)		(0.071)	
Pop. density (binary) × Pool season		-0.519		0.020		-0.382		-0.002
		(0.433)		(0.124)		(0.436)		(0.126)
Outdoor pool × Pool season					0.779***	0.769***	-0.188	-0.145
					(0.287)	(0.285)	(0.125)	(0.124)
R^2	0.892	0.892	0.892	0.892	0.892	0.892	0.892	0.892
Observations	256,085	256,085	903,733	903,733	256,085	256,085	903,733	903,733
Respondent fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Interview date fixed effects	✓	✓	✓	✓	✓	✓	✓	✓

Notes: Results from OLS regressions where AfD support (in p.p.) is the dependent variable. Pool season is a dummy variable that equals one for survey responses recorded between May and September. The placebo treatment is defined as population density (population in zip-code (in 1,000s) divided by the area of the zip-code in square kilometers). We present models for both a continuous and binary (median split) measurement of population density. Standard errors are clustered by respondent. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1.

selves lead to seasonal exposure to ethnic diversity that influences support for the far-right (Table S20). Natural pools, in other words, may moderate our main effects but do not seem to be a confounder.

Table S18: Heterogeneity by natural pool presence.

Subset	AfD Support (in p.p.)	
	Natural pool in locality	No natural pool
	(1)	(2)
Outdoor pool × Pool season	0.412	0.830***
	(0.878)	(0.299)
R^2	0.902	0.891
Observations	21,665	234,420
Respondent fixed effects	✓	✓
Interview date fixed effects	✓	✓

Notes: Results from OLS regressions where AfD support (in p.p.) is the dependent variable. The treatment is defined as the presence of a public outdoor pool in a given zip-code. Pool season is a dummy variable that equals one for survey responses recorded between May and September. We split the sample by natural pool (Naturbad) presence in the locality (zip-code). This analysis only includes localities where the share of non-EU immigrants is high (4th quartile). Standard errors are clustered by respondent. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1.

Table S19: Controlling for natural pool presence.

Non-EU Immigrant share	AfD Support (in p.p.) Full sample (1)	High (2)	Low (3)
Outdoor pool × Pool season	0.026	0.794***	-0.144
	(0.109)	(0.283)	(0.122)
Natural pool × Pool season	-0.126	-0.422	-0.106
	(0.147)	(0.397)	(0.159)
R^2	0.892	0.892	0.892
Observations	1,160,202	256,085	904,117
Respondent fixed effects	✓	✓	✓
Interview date fixed effects	✓	✓	✓

Notes: Results from OLS regressions where AfD support (in p.p.) is the dependent variable. The treatment is defined as the presence of a public outdoor pool in a given zip-code. Pool season is a dummy variable that equals one for survey responses recorded between May and September. We control for the presence of a natural pool (Naturbad) in the locality (zip-code). Standard errors are clustered by respondent. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1.

Table S20: Natural pool presence as placebo treatment.

Non-EU Immigrant share	AfD Support (in p.p.) Full sample (1)	High (2)	Low (3)
Natural pool × Pool season	-0.125	-0.419	-0.106
	(0.147)	(0.397)	(0.159)
R^2	0.892	0.892	0.892
Observations	1,160,202	256,085	904,117
Respondent fixed effects	✓	✓	✓
Interview date fixed effects	✓	✓	✓

Notes: Results from OLS regressions where AfD support (in p.p.) is the dependent variable. The placebo treatment is defined as the presence of a natural pool in a given zip-code. Pool season is a dummy variable that equals one for survey responses recorded between May and September. Standard errors are clustered by respondent. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1.

S4.3 Additional results

Table S21: Robustness test using 2018 data on county-level foreigner share.

	AfD Support (in p.p.)		
Non-EU Immigrant share (2018)	Full sample	High	Low
	(1)	(2)	(3)
Outdoor pool × Pool season	0.032	0.721***	-0.136
	(0.110)	(0.268)	(0.126)
R^2	0.892	0.892	0.892
Observations	1,145,780	284,004	861,776
Respondent fixed effects	✓	✓	✓
Interview date fixed effects	✓	✓	✓

Notes: Results from OLS regressions where AfD support (in p.p.) is the dependent variable. The treatment is defined as the presence of a public outdoor pool in a given zip-code. Pool season is a dummy variable that equals one for survey responses recorded between May and September. Standard errors are clustered by respondent. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1.

Table S22: Effects on other parties.

	CDU/CSU (1)	FDP (2)	Greens (3)	Left Party (4)	SPD (5)
Outdoor pool × Pool season	-0.230	-0.116	-0.301	-0.060	0.024
	(0.277)	(0.300)	(0.285)	(0.214)	(0.283)
R^2	0.836	0.797	0.804	0.831	0.825
Observations	256,085	256,085	256,085	256,085	256,085
Respondent fixed effects	✓	✓	✓	✓	✓
Interview date fixed effects	✓	✓	✓	✓	✓

Notes: Results from OLS regressions where support for the major German parties (in p.p.) is the dependent variable. The treatment is defined as the presence of a public outdoor pool in a given zip-code. Pool season is a dummy variable that equals one for survey responses recorded between May and September. The sample includes zip-code regions with high immigrant shares (upper quartile). Standard errors are clustered by respondent. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1.

Table S23: Estimation using survey weights.

	AfD Support (in p.p.)	
	(1)	(2)
Outdoor pool × Pool season	1.49***	
	(0.526)	
Indoor pool × Pool season		-0.414
		(0.689)
R^2	0.901	0.899
Observations	213,162	171,855
Respondent fixed effects	✓	✓
Interview date fixed effects	✓	✓

Notes: Results from OLS regressions where AfD support (in p.p.) is the dependent variable. The treatment is defined as the presence of a public outdoor pool in a given zip-code. Pool season is a dummy variable that equals one for survey responses recorded between May and September. We use survey weights for (1) age (categorical), (2) gender, (3) state, and (4) education (A-levels) based on the 2016 micro census. We aligned the census covariates with the covariates observed in the Civey sample (see Table S36). The sample includes zip-code regions with high immigrant shares (upper quartile). Standard errors are clustered by respondent. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1.

Table S24: Heterogeneity by gender.

Gender	AfD Support (in p.p.) Men (1)	Women (2)
Outdoor pool × Pool season	0.643**	1.38**
	(0.313)	(0.658)
R^2	0.895	0.881
Observations	203,074	53,011
Immigrant share	High	High
Respondent fixed effects	✓	✓
Interview date fixed effects	✓	✓

Notes: Results from OLS regressions where AfD support (in p.p.) is the dependent variable. The treatment is defined as the presence of a public outdoor pool in a given zip-code. Pool season is a dummy variable that equals one for survey responses recorded between May and September. The sample includes zip-code regions with high immigrant shares (upper quartile). Standard errors are clustered by respondent. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1.

Table S25: Heterogeneity by immigrant composition.

Immigrant composition	AfD Support (in p.p.) High non-EU (1)	High EU, low non-EU (2)	Low EU & Low non-EU (3)
Outdoor pool × Pool season	0.793***	-0.637	-0.011
	(0.283)	(1.07)	(0.279)
R^2	0.892	0.890	0.891
Observations	256,085	12,404	179,831
Respondent fixed effects	✓	✓	✓
Interview date fixed effects	✓	✓	✓

Notes: Results from OLS regressions where AfD support (in p.p.) is the dependent variable. The treatment is defined as the presence of a public outdoor pool in a given zip-code. Pool season is a dummy variable that equals one for survey responses recorded between May and September. We estimate our main specification in three different subsets of the data defined by the composition of the immigrant population: (1) localities with a high share of non-EU foreigners (4th quartile), regardless of the share of EU foreigners; (2) localities with a high share of EU foreigners (4th quartile) and a low share of non-EU foreigners (1st quartile); (3) localities where both the share of EU and non-EU foreigners is low (1st quartile). Standard errors are clustered by respondent. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1.

Table S26: Main results, excluding Brandenburg and Mecklenburg-Western Pomerania.

Non-EU Immigrant share	AfD Support (in p.p.) Full sample (1)	High (2)	Low (3)
Outdoor pool × Pool season	0.031	0.793***	-0.148
	(0.112)	(0.283)	(0.126)
R^2	0.892	0.892	0.892
Observations	1,101,724	256,085	845,639
Respondent fixed effects	✓	✓	✓
Interview date fixed effects	✓	✓	✓

Notes: Results from OLS regressions where AfD support (in p.p.) is the dependent variable. The treatment is defined as the presence of a public outdoor pool in a given zip-code. Pool season is a dummy variable that equals one for survey responses recorded between May and September. Standard errors are clustered by respondent. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1.

S5 Analysis of electoral data

To estimate the effect of outdoor pool presence on electoral behavior, we focus on the 2017 German general election. More specifically, we use precise data on electoral returns on the ballot box level for Berlin and Hamburg (Germany's two largest cities), which allows us to estimate the effect of an outdoor pool at the local level. For Berlin, we aggregate ballot box returns to the postal voting district, the lowest geographic area on which we can unambiguously assign postal votes. For Hamburg, postal votes cannot be precisely assigned to granular geographic units for the 2017 election. We therefore exclude postal votes for Hamburg and aggregate returns to the borough level (*Ortsteile*). On average, our units of analysis have around 3,000 voters. We refer to this unit of analysis as *electoral precinct*.

We define treatment in these units by coding whether they contain at least one outdoor pool. Precise geographic information on outdoor pools comes from the *Bäderatlas* (see Sections 4.1 in the main text and S3.1 in the SI). Of the 833 observations in our data, 44 contain an outdoor pool. To match treated to untreated units, we rely on demographic variables that we construct using the 2011 German Census on the 100m x 100m grid level. To our knowledge, this is the only data source that allows us to observe within-city variation for our covariates. However, it should be noted that this is not an ideal data source for the reasons noted in the manuscript: in 2011, the foreign population was substantially smaller and had a different composition, with a much lower proportion of non-EU foreigners (particularly refugees) compared to the time frame we examine. Our census files contain information on the total number of individuals and the number of males, seniors (above age 64), Catholics, Protestants, German-borns, and EU-borns in each grid cell. We assign a grid cell to a precinct if its centroid falls within the boundaries of the precinct polygon. For each precinct, we calculate the (log) total number of individuals living and the share of men, seniors, Catholics, Protestants, German-borns, and EU-borns. We also calculate the (log) total area of each precinct based on the respective polygons. Summary statistics for our data are provided in Table S27.

We perform exact matching on city district (*Bezirk*) and Mahalanobis distance matching based on our demographic variables plus log area. To ensure appro-

priate distance between our control units and pools, we exclude all precincts directly adjacent to a treated unit before performing the matching procedure. The results presented in Table S29 in the main text display the ATT estimates from this matching analysis for the full sample and each city. Covariate balance checks are presented in Table S28. Maps of Berlin and Hamburg that indicate treated units and matched controls are displayed in Figure S9.

Table S27: Summary statistics.

Statistic	N	Mean	S.D.	Min	Median	Max
Outdoor Pool	832	0.053	0.224	0	0	1
AfD Share	832	0.113	0.058	0.018	0.101	0.285
Share Men	832	0.486	0.031	0.410	0.483	0.902
Age $>$ 64	832	0.191	0.078	0.027	0.194	0.598
Share Catholic	832	0.088	0.036	0.016	0.097	0.186
Share Protestant	832	0.215	0.102	0.048	0.220	0.614
Share Born Germany	832	0.837	0.088	0.174	0.854	0.964
Share Born EU	832	0.052	0.022	0.007	0.050	0.163
Log Area (in m^2)	832	13.699	1.128	11.268	13.562	17.350
Log Total Citizens	832	8.516	0.554	6.213	8.461	11.352

Notes: This table shows summary statistics for the precinct level election data.

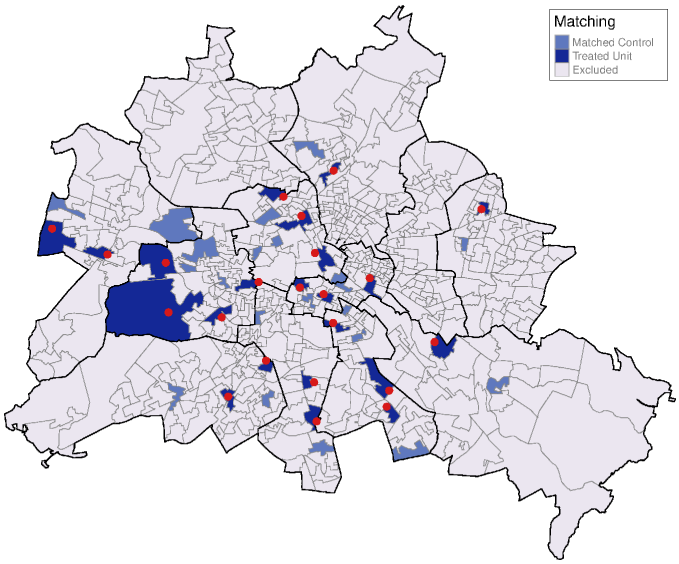
Table S28: Matching balance check: full sample.

Variable	Mean Treatment	Mean Control	Std Mean Diff
Share Men	0.482	0.483	0.057
Share Age $>$ 64	0.197	0.171	0.446
Share Catholic	0.098	0.098	0.003
Share Protestant	0.260	0.256	0.044
Share Born Germany	0.808	0.815	0.092
Share Born EU	0.056	0.055	0.036
Log Area (in m^2)	14.579	13.776	0.647
Log Total Citizens	9.080	8.767	0.416

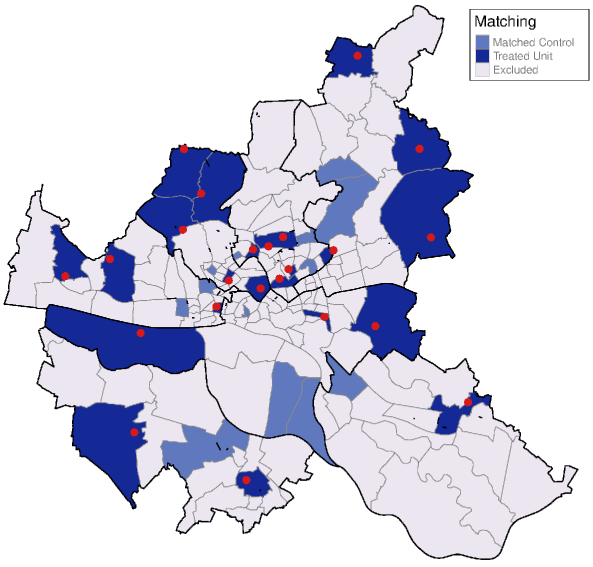
Notes: This table shows post-matching balance checks for the full sample of election results.

Figure S9: Locations of matched precincts and outdoor pools.

(a) Berlin.



(b) Hamburg.



Notes: This map displays the locations of all treated and matched control units considered in the analysis. The red dots indicate the locations of outdoor pools. Bezirk boundaries are indicated by black lines. Precinct boundaries are indicated by gray lines.

Table S29: Matching estimates for effect of outdoor pools on electoral behavior in 2017 federal election.

	Both	Berlin	Hamburg
	(1)	(2)	(3)
ATT	2.197***	2.135**	2.259***
SE	0.538	0.942	0.522
N in Matched Sample	88	44	44
N Treated	44	22	22
Orig N Obs	601	523	77

Notes: Matching results for the effect of outdoor pool presence on AfD voting (in p.p.) in 2017 federal election. Unit is electoral precinct (see Section S5). The treatment variable is the presence of an outdoor pool. Exact matching on district (*Bezirk*). Mahalanobis distance matching on log number of citizens, log area, share of men, share of Catholics, share of Protestants, share of German born, share of EU born, and share of age over 64 in the precinct. Precincts directly adjacent to treated precincts are removed. We provide additional details on data sources and processing in Section S5 in the SI. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1.

S6 Analysis of GLES panel survey data

To analyze the effects of out-group exposure in public pools on attitudes and the perceived salience of different political issues, we draw on data collected as part of the German Longitudinal Election Study (GLES, 2023). Interviews were conducted between 2016 and 2021. Participants are interviewed up to twice a year between elections and more frequently during election years. Initially, interviews were conducted both online and by mail, but since spring 2020, they have been exclusively online. The panel is useful for studying changes in political attitudes and behavior over time.

Not all respondents were interviewed in all years, partly due to panel attrition and partly due to refresher samples. In addition, it should be noted that not all outcome variables are collected in all survey waves. This results in varying numbers of observations across model specifications, outcome variables, and survey years.

The GLES dataset with zip-codes is accessible in the Secure Data Center at the GESIS Data Archive for Social Sciences in Cologne. To gain access to the data, researchers are required to sign a special data usage agreement and work in an individually configured secure virtual environment.

We estimate analogous 2WFE specifications as for our main results for the outcome variables shown below in Table S30. All outcome variables are standardized. Because the GLES survey is not as high-frequency as the Civey data, we can only estimate interview-month fixed effects (not interview-day fixed effects) for these analyses. All models include respondent fixed effects. All analyses except the results presented in Table S35 exclude the survey years 2020 and 2021, i.e., the time period after the onset of the COVID-19 pandemic. For the results presented in Table S35, we exclude all interviews before the year 2020. Similar to the results presented in Table 1, Table S34 compares indoor pool localities to those without any pools, excluding outdoor pool zip-codes. Because the coding of the issue salience outcomes goes from 1 (very important) to 5 (not important at all), all point estimates are inverted so that positive point estimates indicate *higher* perceived salience for a given issue.

Table S30: GLES survey items and scales.

Variable	Original Item (German)	English Translation	Scale
1130	Manche wollen die Zuzugsmöglichkeiten für Ausländer erleichtern, andere wollen die Zuzugsmöglichkeiten für Ausländer einschränken. Wie ist Ihre Meinung zu diesem Thema?	Some want to make it easier for foreigners to immigrate, while others want to make it more difficult for foreigners to immigrate. What is your personal view on this issue?	1–7 (1: immigration for foreigners should be easier, 7: immigration for foreigners should be more difficult)
1210	Es gibt unterschiedliche Auffassungen darüber, wie sehr sich Ausländer in Deutschland anpassen sollten. Manche denken, dass sich Ausländer vollständig an die deutsche Kultur anpassen sollten. Andere denken, Ausländer sollten auch hier möglichst nach der eigenen Kultur leben können. Wie ist Ihre Meinung zu diesem Thema?	There are different views on how much foreigners should assimilate in Germany. Some people think that foreigners should completely assimilate to the German culture. Others think that foreigners should be able to live according to their own culture. What is your opinion on this issue?	1–7 (1: foreigners should completely assimilate to the German culture, 7: foreigners should be able to live according to their own culture)
Issue Salience Question: "And how important are these issues to you personally?"			
1100	Sozialleistungen und Steuern	Redistribution and taxes	1–5 (1: very important, 5: not important at all)
1140	Einwanderung von Ausländern	Immigration of foreigners	1–5 (1: very important, 5: not important at all)
1220	Integration von Ausländern	Integration of foreigners	1–5 (1: very important, 5: not important at all)
1300	Wirtschaftswachstum und Klimaschutz	Economic growth and fight against climate change	1–5 (1: very important, 5: not important at all)
1421	Staatliche Eingriffe zur Terrorismusbekämpfung	State interference to combat terrorism	1–5 (1: very important, 5: not important at all)

Table S31: Effects on issue salience and immigration attitudes, high diversity subset.

	Issue Salience		**Attitudes**	
	Immigration	Integration	Immigration	Integration
Outdoor pool × Pool season	0.0607*	0.0728**	0.0157	-0.0112
	(0.0331)	(0.0314)	(0.0150)	(0.0223)
Observations	14,807	14,811	30,956	22,574
Respondent fixed effects	✓	✓	✓	✓
Interview month fixed effects	✓	✓	✓	✓

Notes: Results from OLS regressions. We use panel survey data from the German Longitudinal Election Study (GLES). All outcome variables are standardized. See Table S30 for the original survey items and scales. The analysis includes the upper quartile of the GLES sample with respect to local ethnic diversity. The treatment is defined as the interaction between the presence of an outdoor public pool and the pool season. Standard errors are clustered by respondent. Signif. Codes: $***: 0.01$, $**: 0.05$, $*: 0.1$.

Table S32: Other political issues.

	Redistribution and Taxes	**Economic Growth and Climate Change**	**Terrorism**
Outdoor pool × Pool season	-0.0753**	-0.0278	-0.0252
	(0.0311)	(0.0296)	(0.0303)
Observations	14,831	14,812	14,804
Respondent fixed effects	✓	✓	✓
Interview month fixed effects	✓	✓	✓

Notes: Results from OLS regressions. We use panel survey data from the German Longitudinal Election Study (GLES). We present results for the upper quartile of the GLES sample with respect to local ethnic diversity. The treatment is defined as the interaction between the presence of an outdoor public pool and the pool season. Standard errors are clustered by respondent. Signif. Codes: $***: 0.01$, $**: 0.05$, $*: 0.1$.

Table S33: Low ethnic diversity subset.

	Issue Salience		**Attitudes**	
	Immigration	Integration	Immigration	Integration
Outdoor pool × Pool season	-0.0003	0.0099	-0.0107	0.0062
	(0.0163)	(0.0157)	(0.0072)	(0.0107)
Observations	44,546	44,408	93,228	68,043
Respondent fixed effects	✓	✓	✓	✓
Interview month fixed effects	✓	✓	✓	✓

Notes: Results from OLS regressions. We use panel survey data from the German Longitudinal Election Study (GLES). We present results for the lower three quartiles of the GLES sample with respect to local ethnic diversity. The treatment is defined as the interaction between the presence of an outdoor public pool and the pool season. Standard errors are clustered by respondent. Signif. Codes: $***: 0.01, **: 0.05, *: 0.1$.

Table S34: Indoor pools placebo test.

	Issue Salience		**Attitudes**	
	Immigration	Integration	Immigration	Integration
Indoor pool × Pool season	-0.0129	0.0131	0.0030	-0.0249
	(0.0443)	(0.0397)	(0.0196)	(0.0281)
Observations	8,595	8,597	17,970	13,140
Respondent fixed effects	✓	✓	✓	✓
Interview month fixed effects	✓	✓	✓	✓

Notes: Results from OLS regressions. We use panel survey data from the German Longitudinal Election Study (GLES). We present results for the upper quartile of the GLES sample with respect to local ethnic diversity. The treatment is defined as the interaction between the presence of an indoor public pool and the pool season. Zip-codes with outdoor pools are excluded from this analysis. Standard errors are clustered by respondent. Signif. Codes: $***: 0.01, **: 0.05, *: 0.1$.

Table S35: COVID-19 placebo test.

	Issue Salience	**Attitudes**	
	Immigration	Immigration	Integration
Outdoor pool × Pool season	0.0580	0.0184	-0.0371
	(0.0390)	(0.0135)	(0.0250)
Observations	7,477	27,899	17,730
Respondent fixed effects	✓	✓	✓
Interview month fixed effects	✓	✓	✓

Notes: Results from OLS regressions. We use panel survey data from the German Longitudinal Election Study (GLES). We present results for the upper quartile of the GLES sample with respect to local ethnic diversity. The treatment is defined as the interaction between the presence of an outdoor public pool and the pool season. For this analysis, we subset to interview responses recorded after the onset of the COVID-19 pandemic (survey years 2020 and 2021). Standard errors are clustered by respondent. Signif. Codes: $***: 0.01$, $**: 0.05$, $*: 0.1$.

S7 Additional information on Civey survey data

The survey company *Civey* operates a panel with roughly one million active users who are regularly polled online. Recruitment often happens through different external platforms, such as major media companies, email services, or personal blogs, which incorporate Civey's surveys into their content. Participants are motivated to take part by the opportunity to access the (weighted) results of the surveys. To be included in the sample, they must provide details about specific attributes like gender and age. Additionally, the company employs multiple strategies to deter data manipulation, such as monitoring the rate at which participants scroll, click, and respond to survey questions. In conducting its own studies and writings, Civey typically applies various weighting techniques to ensure the representativeness of its samples. A more comprehensive description of Civey's methods for sample recruitment and survey execution can be found in Richter et al. (2022) and on the company's website `https://civey.com/ueber-civey/unsere-methode`. We note that the previous discussion of the Civey sample is paraphrased from Hilbig and Riaz (2024), who likewise use Civey survey data.

Table S36: Summary statistics.

Variable	Mean	Median	S.D.	N	Min	Max
Age: 18–29	0.03	0.00	0.17	1,160,202	0.00	1.00
Age: 30–39	0.08	0.00	0.27	1,160,202	0.00	1.00
Age: 40–49	0.11	0.00	0.32	1,160,202	0.00	1.00
Age: 50–64	0.30	0.00	0.46	1,160,202	0.00	1.00
Age: 65 and above	0.48	0.00	0.50	1,160,202	0.00	1.00
Date of interview during outdoor pool season	0.57	1.00	0.49	1,160,202	0.00	1.00
Education: A-levels (Abitur)	0.73	1.00	0.44	930,048	0.00	1.00
Gender: male	0.81	1.00	0.40	1,160,202	0.00	1.00
Indoor pool in zip-code	0.29	0.00	0.45	1,160,202	0.00	1.00
Non-EU foreigner share (county-level)	8.75	8.04	3.93	1,160,202	1.72	17.64
Outdoor pool in zip-code	0.41	0.00	0.49	1,160,202	0.00	1.00
Vote intention: AfD	0.37	0.00	0.48	1,160,202	0.00	1.00
Vote intention: CDU/CSU	0.16	0.00	0.37	1,160,202	0.00	1.00
Vote intention: FDP	0.11	0.00	0.32	1,160,202	0.00	1.00
Vote intention: Greens	0.09	0.00	0.29	1,160,202	0.00	1.00
Vote intention: Left Party	0.09	0.00	0.29	1,160,202	0.00	1.00
Vote intention: SPD	0.13	0.00	0.33	1,160,202	0.00	1.00

Notes: The table shows summary statistics for the Civey survey data.

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Appendix to Chapter 3

S1 Data and measurement

This section describes the Nationscape survey, the survey items used in the analysis, the partisan-context measure used in the heterogeneity analysis, and basic information on sample composition and interview volume around the cutoff.

S1.1 The Nationscape survey

Nationscape is a large online survey fielded in weekly waves between July 2019 and February 2021 by Lucid in collaboration with the Democracy Fund and researchers at UCLA (Tausanovitch et al., 2019). The survey was designed to provide a continuous record of American public opinion across the 2019–2020 primary and general election cycles. Approximately 6,000 respondents are interviewed each week through a daily cross-section, yielding just under 500,000 interviews in total. Each wave includes detailed batteries on political attitudes, policy preferences, and demographics.

Lucid uses standard procedures to safeguard data quality, including duplicate-completion checks based on cookies, IP addresses, and other unique identifiers; attention checks and open-ended verification items; bot-detection services; and supplier-level quality reports. Although Nationscape is an opt-in sample, prior validations indicate that Lucid samples are highly comparable to those of established pollsters such as Pew Research Center and YouGov (Coppock and McClellan, 2019). With weighting, Nationscape's quality has been judged comparable to that of Pew's American Trends Panel (Tausanovitch et al., 2019).

S1.2 Survey items

Table S1 lists the survey items used in the analysis along with their original wording and response scales. Group favorability items are reverse-coded so that higher values denote greater favorability. Full variable lists and response options are documented in the Nationscape codebook.[1]

Table S1: Nationscape survey items used in the analysis.

Variable	Question wording	Response scale
Outcomes: group favorability		
Group favorability	"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?" Asked separately for Blacks, Asians, Muslims, Latinos, Gays and Lesbians, Whites, and Socialists.	1 = very favorable to 4 = very unfavorable. Reverse-coded so that higher values denote greater favorability.
Demographics		
Age	"What is your age?"	Continuous (years).
Gender	"What is your gender?"	1 = Female, 2 = Male.
Household income	"What is your current annual household income before taxes?"	24-point scale from 1 (less than $14,999) to 24 ($250,000 and above).
Education	"What is the highest level of education you have completed?"	11-point scale from 1 (3rd grade or less) to 11 (Doctorate degree).
Political attitudes		
Ideology	"In general, how would you describe your own political viewpoint?"	1 = very liberal to 5 = very conservative.
Partisanship	3-category party identification.	1 = Democrat, 2 = Republican, 3 = Independent.

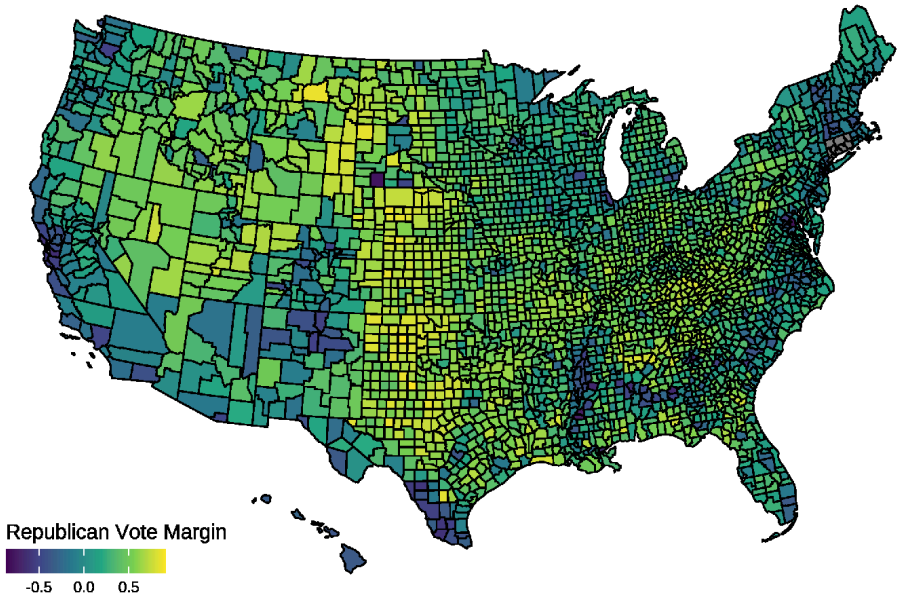
Notes: The table lists the Nationscape items used in the analysis, with verbatim question wording and response scales. Group favorability items are reverse-coded so that higher values denote greater favorability. Additional response options and detailed coding are documented in the Nationscape codebook.

[1] `https://www.voterstudygroup.org/data/nationscape`.

S1.3 Local partisan context

The heterogeneity analysis in the main text uses the partisan leaning of the respondent's county as a proxy for local political environment. I map respondents to counties using the zip-codes provided by Nationscape and assign each county a partisan leaning based on county-level returns from the 2016 presidential election (MIT Election Data and Science Lab, 2018). The Republican vote margin is computed as the Republican share of the two-party vote minus the Democratic share. Margins are merged to the Nationscape data on FIPS codes. Figure S1 maps the resulting margins.

Figure S1: Republican vote margin by county, 2016 presidential election.

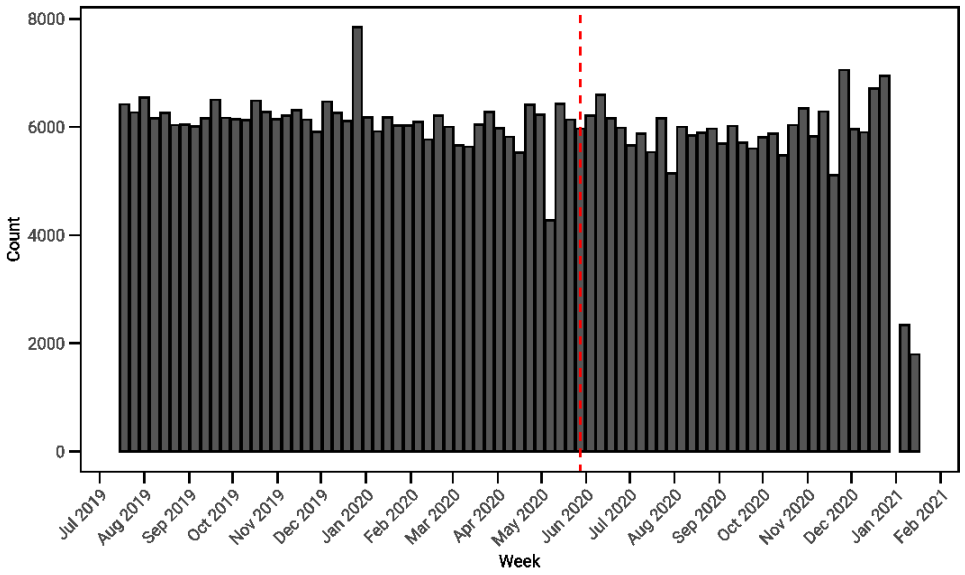


Notes: The figure shows the Republican two-party vote margin by county for the 2016 US presidential election, computed as the Republican share of the two-party vote minus the Democratic share. Counties are classified as Democrat-leaning when this margin is negative. County-level returns for Alaska are unavailable in the source data and Alaska is therefore omitted. Data: MIT Election Data and Science Lab (2018).

S1.4 Sample composition and interview volume

Table S2 reports descriptive statistics for the variables used in the analysis. Figure S2 plots the number of completed interviews in each Nationscape wave. The series is reasonably stable across the period covered by the analysis, and the fluctuation around the May 28, 2020 cutoff is not unusual relative to the rest of the panel.

Figure S2: Nationscape interview volume by week.



Notes: The figure shows the weekly number of respondents in the Nationscape survey between July 2019 and February 2021. The vertical dashed line marks May 28, 2020, the RDiT cutoff used in the main analysis.

Table S2: Summary statistics for variables used in the analysis.

Variable	N	Mean	St. Dev.	Min	Max	Note
Demographics						
Age	465,297	44.778	16.629	18	99	Integer
Male	465,297	0.437	0.496	0	1	Binary
Household Income	440,981	9.320	7.113	1	24	24 pt. scale
Full-time Employed	464,997	0.386	0.487	0	1	Binary
Homemaker	464,997	0.069	0.253	0	1	Binary
Retired	464,997	0.162	0.368	0	1	Binary
Foreign-born	465,297	1.066	0.248	1	2	Binary
Household Gun Owner	442,642	2.380	0.843	1	3	Binary
Non-religious	439,518	0.288	0.453	0	1	Binary
Hispanic	465,297	0.856	0.351	0	1	Binary
Race/Ethnicity: Black	465,297	0.122	0.328	0	1	Binary
Race/Ethnicity: Asian	465,297	0.045	0.208	0	1	Binary
Race/Ethnicity: White	465,297	0.747	0.435	0	1	Binary
Race/Ethnicity: Other	465,297	0.086	0.280	0	1	Binary
Region: Northeast	465,297	0.177	0.382	0	1	Binary
Region: Midwest	465,297	0.226	0.418	0	1	Binary
Region: South	465,297	0.380	0.486	0	1	Binary
Region: West	465,297	0.217	0.412	0	1	Binary
Political Attitudes						
Democrat	464,743	0.355	0.478	0	1	Binary
Republican	464,743	0.304	0.460	0	1	Binary
Independent	464,743	0.278	0.448	0	1	Binary
Ideology	414,424	3.021	1.144	1	5	5 pt. scale
Political Interest	464,788	1.854	0.905	1	4	4 pt. scale
Dependent Variables						
Group Favorability: Blacks	406,058	3.194	0.865	1	4	4 pt. scale
Group Favorability: Asians	368,414	3.208	0.820	1	4	4 pt. scale
Group Favorability: Latinx	383,872	3.179	0.837	1	4	4 pt. scale
Group Favorability: Muslims	374,213	2.726	1.007	1	4	4 pt. scale
Group Favorability: LGBTQ	399,215	2.958	1.020	1	4	4 pt. scale
Group Favorability: Whites	402,533	3.137	0.863	1	4	4 pt. scale
Group Favorability: Socialists	355,214	2.290	1.065	1	4	4 pt. scale

Notes: The table reports summary statistics for the variables used in the analysis, computed across all Nationscape respondents observed between July 2019 and February 2021. Group favorability items are reverse-coded so that higher values indicate greater favorability.

S2 Identification

This section reports the identification checks referenced in the main text: covariate balance at the cutoff, a density test of the running variable, and placebo regressions on demographic outcomes. The checks support the claim that the discontinuities in expressed favorability are not driven by abrupt changes in the composition of respondents.

S2.1 Covariate balance

Table S3 reports standardized mean differences between respondents interviewed before and after the cutoff for a set of pre-treatment covariates, restricted to the MSE-optimal bandwidth used in the main analysis. All differences are below the conventional 0.1 threshold; the largest, on age, is 0.0587. Observable composition changes little around the threshold.

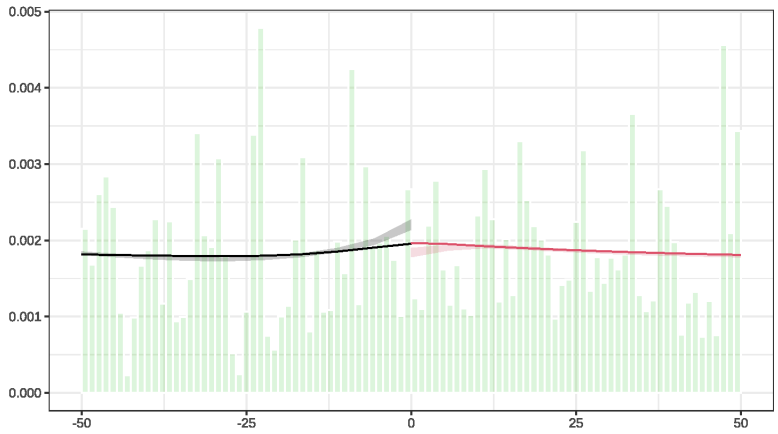
S2.2 Density of the running variable

Figure S3 reports the local polynomial density test of Cattaneo et al. (2020) for continuity of the running variable at the cutoff. The test rejects continuity. As discussed in the main text, I do not interpret this rejection as evidence of strategic sorting. The event was unexpected, the balance and placebo checks above show no comparable shifts in observable composition, and the imbalance runs in the direction of more post-cutoff than pre-cutoff interviews—which is difficult to reconcile with strategic non-response. At this sample size the test rejects for trivial deviations from continuity, so the rejection is largely uninformative about the substantive concern.

S2.3 Placebo demographic outcomes

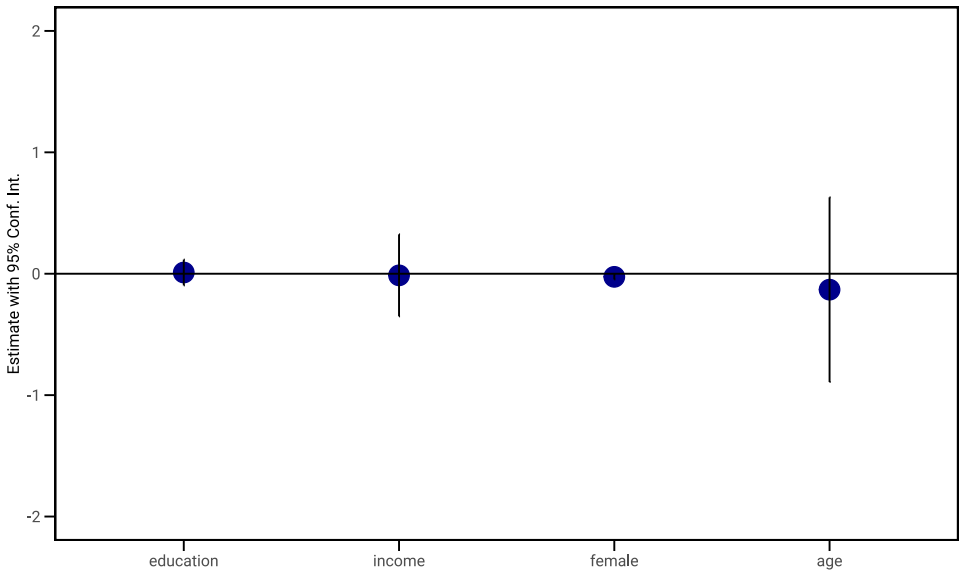
Figure S4 replicates the main RDiT estimation using pre-treatment demographic variables as placebo outcomes. None of the estimates show comparable discontinuities, which is consistent with the main results being driven by changes in expressed favorability rather than by shifts in who responded after the cutoff.

Figure S3: Local polynomial density of the running variable around the cutoff.



Notes: The figure shows the local polynomial density estimate of Cattaneo et al. (2020) for interview date around the May 28, 2020 cutoff. Shaded regions indicate 95% confidence bands.

Figure S4: Placebo RDiT estimates on pre-treatment demographic outcomes.



Notes: The figure shows RDiT point estimates when pre-treatment demographic characteristics are used as placebo outcomes in place of group favorability. Estimates use a triangular kernel and MSE-optimal bandwidths (Calonico et al., 2020). Bars indicate 95% confidence intervals based on robust bias-corrected standard errors.

Table S3: Covariate balance at the cutoff.

Variable	Type	Difference	Mean Threshold
Age	Discrete	0.0587	Balanced, < 0.1
Male	Binary	-0.0141	Balanced, < 0.1
Household Income (24 pt. scale)	Ordinal	-0.0417	Balanced, < 0.1
Full-time Employed	Binary	0.0093	Balanced, < 0.1
Homemaker	Binary	0.0022	Balanced, < 0.1
Retired	Binary	0.0176	Balanced, < 0.1
Foreign-born	Binary	-0.0037	Balanced, < 0.1
Household Gun Owner	Binary	-0.0057	Balanced, < 0.1
Non-religious	Binary	0.0035	Balanced, < 0.1
Ideology (5 pt. scale)	Ordinal	0.0068	Balanced, < 0.1
Political Interest (4 pt. scale)	Ordinal	0.0091	Balanced, < 0.1
Hispanic	Binary	0.0080	Balanced, < 0.1
Race/Ethnicity: Black	Binary	0.0013	Balanced, < 0.1
Race/Ethnicity: Asian	Binary	-0.0039	Balanced, < 0.1
Race/Ethnicity: White	Binary	0.0084	Balanced, < 0.1
Race/Ethnicity: Other	Binary	-0.0057	Balanced, < 0.1
Region: Northeast	Binary	-0.0075	Balanced, < 0.1
Region: Midwest	Binary	0.0042	Balanced, < 0.1
Region: South	Binary	0.0069	Balanced, < 0.1
Region: West	Binary	-0.0036	Balanced, < 0.1
Balance tally for mean differences			
Balanced, < 0.1		28	
Not Balanced, > 0.1		0	
Variable with the greatest mean difference			
Age		0.0587	Balanced, < 0.1
Sample sizes			
Control		55,512	
Treated		58,440	

Notes: The table reports mean differences for pre-treatment covariates between respondents interviewed before and after the May 28, 2020 cutoff, restricted to the MSE-optimal bandwidth used in the main analysis. Discrete and ordinal variables are standardized; binary variables report raw mean differences. Following common practice, differences below 0.1 in absolute value are treated as balanced.

S3 Heterogeneity by partisanship and area leaning

Table S4 reports the RDiT estimates for the summary index of favorability shown in Figure 6 of the main text, broken out by combinations of respondent partisanship (Democrat or Republican) and county partisan leaning (Democrat- or Republican-leaning). The estimates are concentrated in Democratic-leaning counties and are positive both for Democratic and Republican respondents in those counties; they are small and imprecise in Republican-leaning counties.

Table S4: Results by party ID and area leaning.

	D in D	**D in R**	**R in D**	**R in R**
LATE	0.08*	0.03	0.15**	0.02
	(0.04)	(0.04)	(0.06)	(0.04)
BW est. (h)	71.08	91.03	47.43	84.13
BW bias (b)	107.79	140.57	79.37	129.49
Total no. obs.	78,210	43,087	42,883	55,878
Effective no. obs.	20,882	14,022	8,194	17,117
Left of cutoff	10,455	6,854	4,119	8,317
Right of cutoff	10,427	7,168	4,075	8,800

Notes: The table reports RDiT estimates for the summary index of favorability across combinations of respondent partisanship and county partisan leaning. Counties are classified as Democrat-leaning if they had a positive Democratic two-party vote margin in the 2016 presidential election. Triangular kernel and MSE-optimal bandwidths (Calonico et al., 2020); robust bias-corrected standard errors in parentheses. Significance levels: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

S4 Robustness of the main estimates

This section reports robustness checks on the main RDiT estimates from Table 1. The estimates are stable across alternative bandwidths and cutoff dates, alternative functional forms and covariate adjustments, sample sensitivity checks, and restrictions to respondents outside the target group.

S4.1 Bandwidth and functional form

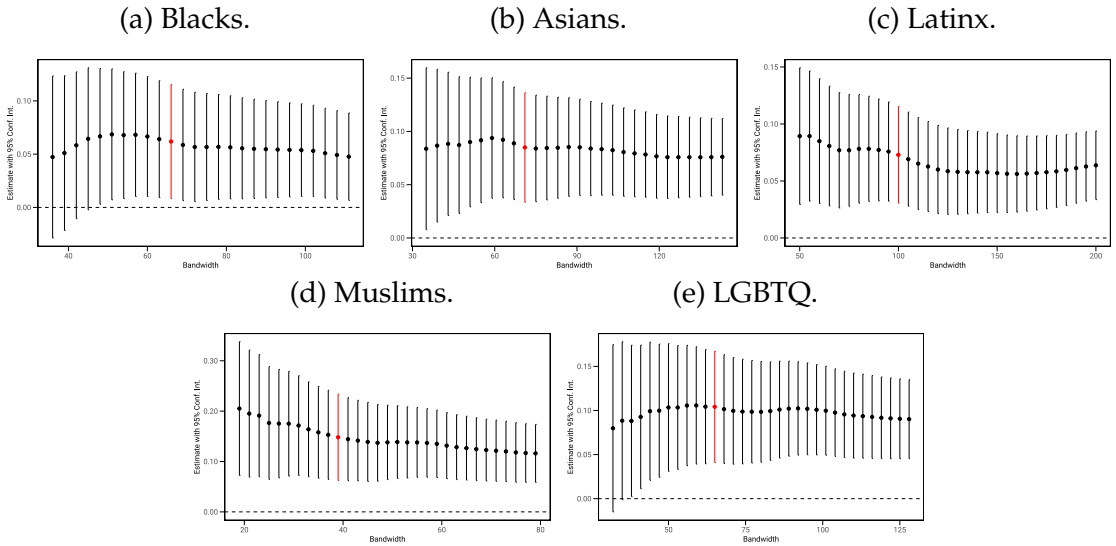
The main analysis uses MSE-optimal bandwidths (Calonico et al., 2020). Figure S5 reports RDiT estimates across bandwidths ranging from one-half to twice the MSE-optimal value. The estimates are stable across this range for all five outcomes. Figure S6 shows that moving the cutoff between May 25 and May 28, 2020 yields a similar pattern across outcomes. Replacing the integer-day running variable with a continuous (decimal-day) variable measured from 10:00:00 a.m. EST on May 28 leaves the estimates essentially unchanged (Table S5). Including pre-treatment covariates in the local linear estimation also leaves the estimates unchanged (Table S6).

Table S5: RDiT estimates with continuous time as the running variable.

	Blacks	**Asians**	**Latinx**	**Muslims**	**LGBTQ**
LATE	0.05*	0.08***	0.05**	0.12***	0.10***
	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)
BW est. (h)	63.22	68.40	97.04	38.44	59.00
BW bias (b)	115.52	111.66	157.37	78.47	102.94
Total no. obs.	406,058	368,414	383,872	374,213	399,215
Effective no. obs.	93,921	94,739	135,270	52,006	84,665
Left of cutoff	45,233	47,253	66,974	25,134	41,326
Right of cutoff	48,688	47,486	68,296	26,872	43,339

Notes: The table replicates Table 1 using a continuous (decimal-day) running variable measured as the distance to 10:00:00 a.m. EST on May 28, 2020, in place of integer days. Triangular kernel and MSE-optimal bandwidths (Calonico et al., 2020); robust bias-corrected standard errors in parentheses. Significance levels: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Figure S5: RDiT estimates across alternative bandwidths.



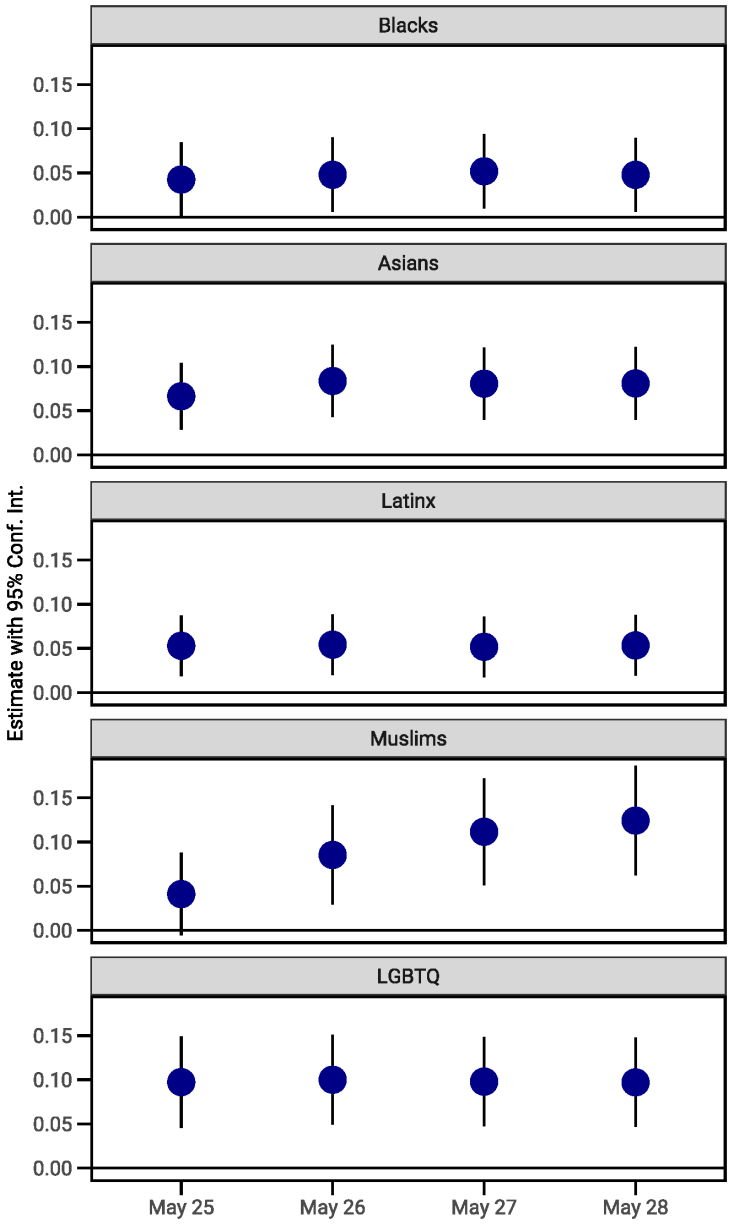
Notes: Each panel reports RDiT point estimates across a grid of bandwidths from one-half to twice the MSE-optimal bandwidth used in the main analysis (Calonico et al., 2020). All other specification choices are held at their main-text values. Bars indicate 95% confidence intervals based on robust bias-corrected standard errors.

Table S6: RDiT estimates with pre-treatment covariates included.

	Blacks	**Asians**	**Latinx**	**Muslims**	**LGBTQ**
LATE	0.06**	0.08***	0.05**	0.13***	0.10***
	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)
BW est. (h)	63.04	70.18	80.45	37.98	68.36
BW bias (b)	112.76	106.24	123.99	74.87	107.54
Total no. obs.	386,363	351,062	365,661	356,693	379,900
Effective no. obs.	90,246	91,771	106,650	48,038	95,432
Left of cutoff	43,099	45,452	52,852	23,257	47,175
Right of cutoff	47,147	46,319	53,798	24,781	48,257

Notes: The table replicates Table 1 after including pre-treatment covariates (age, gender, race/ethnicity, household income, education, and region) as controls in the local linear estimation. Triangular kernel and MSE-optimal bandwidths (Calonico et al., 2020); robust bias-corrected standard errors in parentheses. Significance levels: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Figure S6: RDiT estimates across alternative cutoff dates.

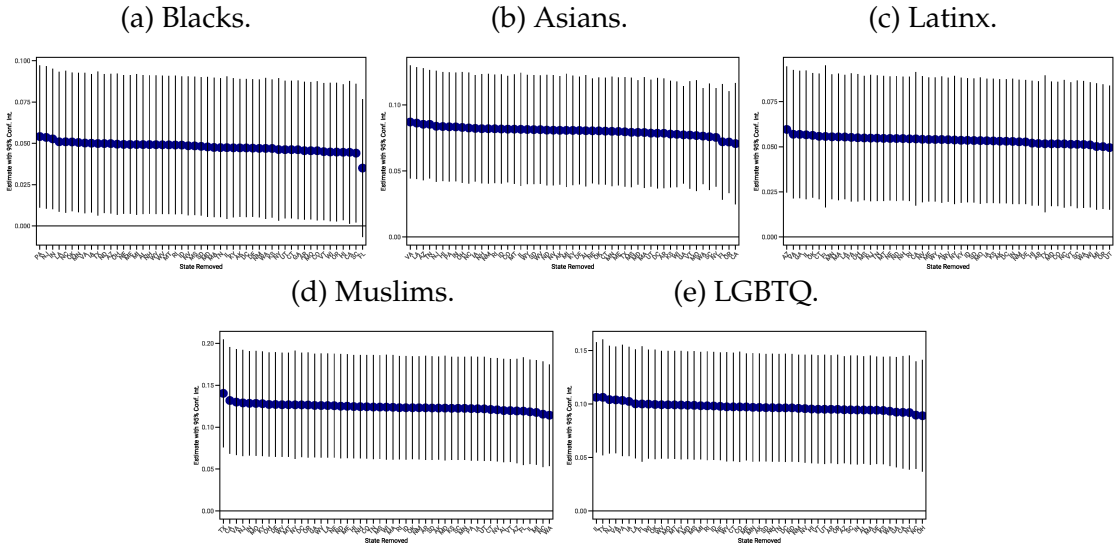


Notes: The figure reports RDiT estimates for each of the five favorability outcomes when the cutoff is moved across May 25, 26, 27, and 28, 2020. Estimates use a triangular kernel and MSE-optimal bandwidths (Calonico et al., 2020). Bars indicate 95% confidence intervals based on robust bias-corrected standard errors.

S4.2 Sample sensitivity

I assess the sensitivity of the estimates to the composition of the sample in two ways. First, a leave-one-state-out jackknife shows that the estimates are highly stable across exclusion of any single state (Figure S7). Second, the unweighted estimates remain positive for all five outcomes and statistically distinguishable from zero for Asians, Latinx individuals, Muslims, and the LGBTQ community; the estimate for Black Americans attenuates without weights (Table S7), consistent with the weights doing real work in reproducing population means.

Figure S7: Leave-one-state-out jackknife of the main RDiT estimates.



Notes: Each panel reports the RDiT point estimate after sequentially dropping each US state from the sample. The vertical line marks the corresponding full-sample estimate from Table 1. Estimates use a triangular kernel and MSE-optimal bandwidths (Calonico et al., 2020). Bars indicate 95% confidence intervals based on robust bias-corrected standard errors.

Table S7: Regression discontinuity estimates without survey weights.

	Blacks	**Asians**	**Latinx**	**Muslims**	**LGBTQ**
LATE	0.01	0.08***	0.05***	0.06***	0.14***
	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)
BW est. (h)	56.68	63.78	60.32	67.13	40.98
BW bias (b)	90.68	147.62	93.91	121.89	74.89
Total no. obs.	406,058	368,414	383,872	374,213	399,215
Effective no. obs.	83,706	87,292	84,327	92,816	59,363
Left of cutoff	39,926	42,681	41,375	46,000	28,324
Right of cutoff	43,780	44,611	42,952	46,816	31,039

Notes: The table replicates Table 1 without applying Nationscape survey weights. Triangular kernel and MSE-optimal bandwidths (Calonico et al., 2020); robust bias-corrected standard errors in parentheses. Significance levels: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

S4.3 Restricting to non-target group respondents

To isolate expressed favorability among respondents who plausibly view the relevant group as an out-group, I restrict the sample to White respondents who are not themselves members of the target group: White non-Hispanic respondents for the Latinx outcome, White non-Muslim respondents for the Muslim outcome, and White straight respondents for the LGBTQ outcome. Table S8 reports the resulting RDiT coefficients. The estimates remain positive and significant for every outcome.

Table S8: RDiT estimates restricted to White respondents who are not in the target group.

	Blacks	**Asians**	**Latinx**	**Muslims**	**LGBTQ**
LATE	0.05*	0.12***	0.07**	0.14***	0.13***
	(0.02)	(0.02)	(0.02)	(0.04)	(0.03)
BW est. (h)	58.31	72.51	74.54	38.41	56.51
BW bias (b)	112.73	111.07	113.75	77.46	97.98
Total no. obs.	303,068	275,629	286,454	279,872	265,948
Effective no. obs.	64,085	73,014	77,627	38,948	54,326
Left of cutoff	30,549	35,585	37,797	18,678	26,235
Right of cutoff	33,536	37,429	39,830	20,270	28,091

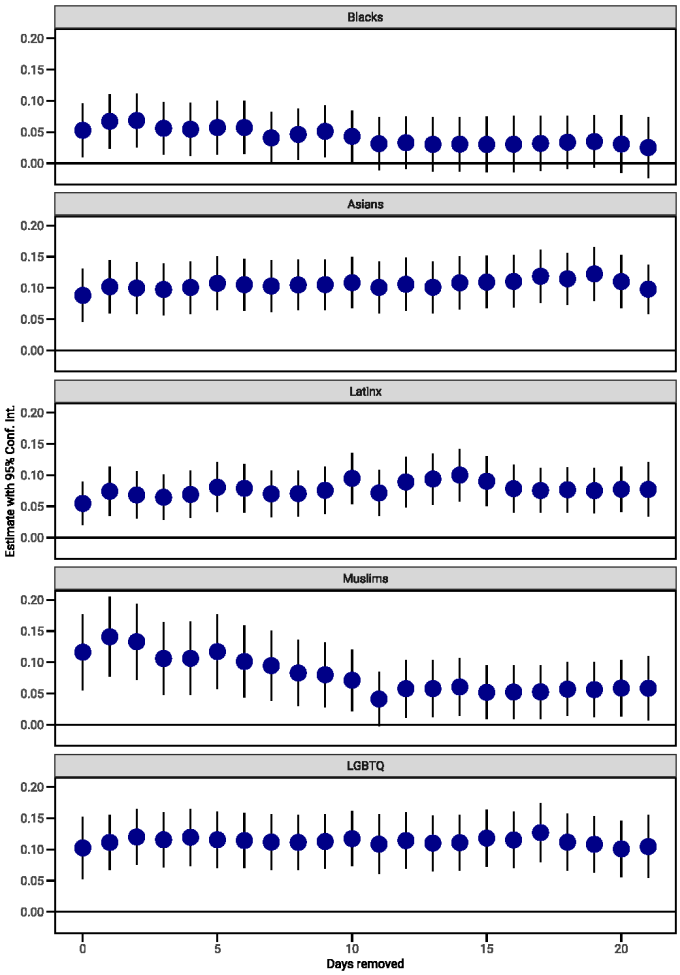
Notes: The table replicates Table 1 restricting the sample to White respondents who are not themselves members of the target group: White non-Hispanic respondents for the Latinx outcome, White non-Muslim respondents for the Muslim outcome, and White straight respondents for the LGBTQ outcome. Triangular kernel and MSE-optimal bandwidths (Calonico et al., 2020); robust bias-corrected standard errors in parentheses. Significance levels: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

S4.4 Persistence and magnitude of the effects

To assess how quickly the estimated effects decay, I sequentially drop the days immediately after the cutoff and re-center the remaining post-cutoff window so that the earliest non-removed day is treated as day 0. For example, dropping days 0–3 places day 4 in the role of the cutoff date. I repeat this for shifts of 0 to 21 days for each outcome. Figure S8 reports the resulting estimates. They are durable across the 21-day window, suggesting that the main results are not driven solely by the immediate days after the cutoff.

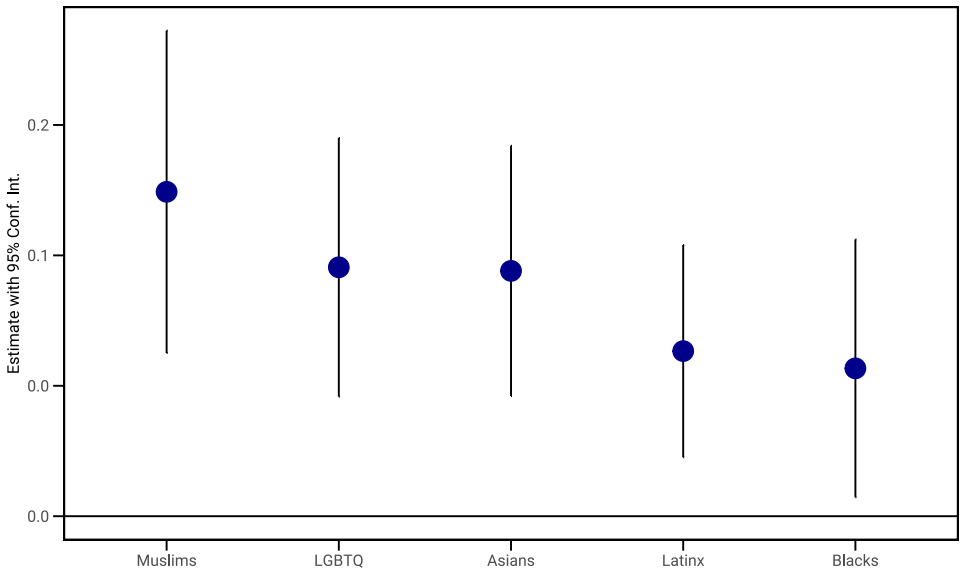
To facilitate magnitude comparisons across the four-point favorability scale, Figure S9 re-expresses the main estimates from Table 1 in standard-deviation units of the corresponding pre-treatment outcome distribution. The estimates correspond to shifts of roughly 0.06 to 0.12 standard deviations.

Figure S8: Persistence of treatment effects across the post-cutoff window.



Notes: The figure shows RDiT point estimates for each outcome after sequentially dropping the days immediately following the cutoff and re-centering the post-cutoff window so that the earliest non-removed day plays the role of the cutoff. The horizontal axis indicates the number of days dropped. Estimates use a triangular kernel and MSE-optimal bandwidths (Calonico et al., 2020). Bars indicate 95% confidence intervals based on robust bias-corrected standard errors.

Figure S9: Main RDiT estimates expressed in standard-deviation units.



Notes: The figure rescales the RDiT estimates from Table 1 into standard-deviation units of the corresponding pre-treatment outcome distribution. Bars indicate 95% confidence intervals based on robust bias-corrected standard errors.

S5 Placebo and falsification tests

This section collects placebo and falsification tests designed to rule out the leading alternative explanations for the main pattern: a generalized leftward mood shift, an arbitrary cutoff choice, or a confounding contemporaneous event.

S5.1 Outcome placebos

The framework predicts that the public reaction to Floyd's murder should move favorability toward marginalized identity groups understood as objects of anti-discrimination norms but should not move favorability toward groups outside that domain. Table S9 reports RDiT estimates for favorability toward Whites and toward Socialists. Both coefficients are essentially zero. This is inconsistent with a generic positive mood shift after the cutoff and, in the case of Socialists, with a general leftward affective shift, which would also raise favorability toward an ideological out-group on the left. Table S10 reports the RDiT estimate when self-reported ideology itself is used as the outcome. The coefficient is small and statistically insignificant, consistent with the absence of a broad leftward ideological shift that could in turn drive correlated movement across the favorability outcomes.

Table S9: Placebo outcomes: favorability toward Whites and Socialists.

	Whites	**Socialists**
LATE	0.00	0.00
	(0.02)	(0.03)
BW est. (h)	52.55	45.60
BW bias (b)	103.99	73.05
Total no. obs.	402,533	355,214
Effective no. obs.	77,327	57,904
Left of cutoff	37,021	28,339
Right of cutoff	40,306	29,565

Notes: The table reports RDiT estimates for favorability toward Whites and Socialists at the May 28, 2020 cutoff. These groups serve as placebo outcomes: a general leftward or pro-out-group affective shift would also raise favorability toward Socialists, while an anti-prejudice norm signal directed at marginalized identity groups should not. Triangular kernel and MSE-optimal bandwidths (Calonico et al., 2020); robust bias-corrected standard errors in parentheses. Significance levels: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table S10: RDiT estimate using ideology as the outcome.

	Ideology
LATE	0.03
	(0.03)
BW est. (h)	76.66
BW bias (b)	117.19
Total no. obs.	414,424
Effective no. obs.	116,538
Left of cutoff	57,923
Right of cutoff	58,615

Notes: The table reports the RDiT estimate when self-reported ideology (1 = "very liberal" to 5 = "very conservative") is used as the outcome. Triangular kernel and MSE-optimal bandwidth (Calonico et al., 2020); robust bias-corrected standard errors in parentheses. Significance levels: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

S5.2 Cutoff placebos

Table S11 replaces the substantive cutoff (May 28, 2020) with the median value of the running variable in the pre-cutoff sample. The estimates are small and statistically insignificant, consistent with the main results capturing a substantively meaningful event rather than spurious detection at an arbitrary threshold.

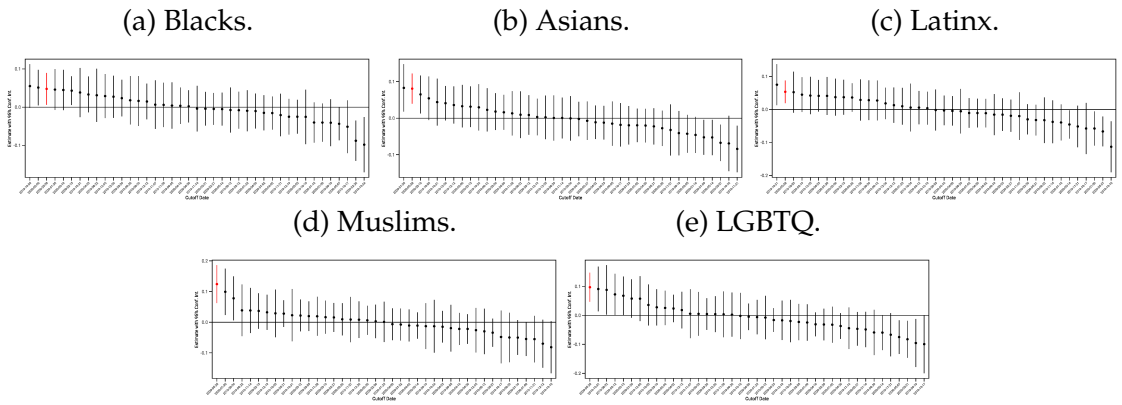
Table S11: RD estimates using median time value as cutoff.

	Blacks	**Asians**	**Latinx**	**Muslims**	**LGBTQ**
LATE	-0.02	0.02	-0.02	-0.03	-0.02
	(0.02)	(0.02)	(0.02)	(0.03)	(0.02)
BW est. (h)	56.65	51.03	63.74	64.03	85.89
BW bias (b)	95.92	90.26	102.15	98.51	132.35
Total no. obs.	406,058	368,414	383,872	374,213	399,215
Effective no. obs.	81,494	69,704	88,292	87,361	124,055
Left of cutoff	40,793	34,147	44,018	43,617	61,693
Right of cutoff	40,701	35,557	44,274	43,744	62,362

Notes: The table reports RDiT estimates that replace the substantive cutoff (May 28, 2020) with the median value of the running variable in the pre-cutoff sample. Each column corresponds to a different group. Triangular kernel and MSE-optimal bandwidths (Calonico et al., 2020); robust bias-corrected standard errors in parentheses. Significance levels: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

The substantive cutoff falls on a Thursday. As a placebo exercise, I re-estimate the RDiT model using each Thursday in the survey period as a cutoff, restricting to dates with at least 30 days of observations on either side. Figure S10 shows these placebo estimates with the substantive cutoff highlighted in red. For each outcome, the actual cutoff yields one of the three largest estimates and the most precise of these.

Figure S10: RDiT estimates across weekly placebo cutoffs.



Notes: Each panel reports RDiT point estimates when each Thursday in the Nationscape survey period is used as a placebo cutoff, restricting to dates with at least 30 days of observations on either side. The estimate for the substantive cutoff (May 28, 2020) is highlighted in red. Estimates use a triangular kernel and MSE-optimal bandwidths (Calonico et al., 2020). Bars indicate 95% confidence intervals based on robust bias-corrected standard errors.

S5.3 Confounding events: *Bostock v. Clayton County*

The Supreme Court decided *Bostock v. Clayton County* on June 15, 2020. In a 6–3 decision, the Court held that workplace discrimination on the basis of sexual orientation or gender identity is prohibited under Title VII of the Civil Rights Act of 1964, on the reasoning that firing an individual for being homosexual or transgender necessarily involves treating employees differently on the basis of sex. Because this decision falls inside the post-cutoff window for the main LGBTQ analysis, it is a plausible alternative explanation for the LGBTQ result. To address this, I re-estimate the RDiT for the LGBTQ outcome using June 15, 2020 as the cutoff. Table S12 reports a precisely estimated null effect, which is inconsistent with *Bostock* being responsible for the discontinuity in the main analysis.

Table S12: RDiT estimate using June 15, 2020 (*Bostock v. Clayton County*) as cutoff.

	LGBTQ
LATE	0.01
	(0.03)
BW est. (h)	54.12
BW bias (b)	91.79
Total no. obs.	399,215
Effective no. obs.	78,884
Left of cutoff	38,232
Right of cutoff	40,652

Notes: The table reports the RDiT estimate for favorability toward the LGBTQ community using June 15, 2020—the date the Supreme Court decided *Bostock v. Clayton County*—as the cutoff. Triangular kernel and MSE-optimal bandwidth (Calonico et al., 2020); robust bias-corrected standard errors in parentheses. Significance levels: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

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