Does Far-Right Success Increase Hate Crimes?

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Abstract

Do far-right electoral victories increase hate crimes? We examine this question in a core democratic institution, first-past-the-post lower-chamber elections, using U.S. House races from 2006 to 2022. We theorize that the election of far-right candidates generates an 'exclusionary impulse'. We estimate the causal effect of electing such candidates on local hate crimes with a sharp regression discontinuity design. We find no overall effect when pooling all years, but clear temporal conditionality: during the Trump era (after 2016), districts that narrowly elected far-right candidates experienced significant increases in hate crimes one year later. Large-scale survey evidence suggests that these effects operate not through rapid attitude change but through heightened salience of white identity, consistent with greater in-group/out-group thinking. The findings show that far-right success can elevate bias-motivated behavior, but only under conditions of far-right normalization.

1 Introduction

When President Trump and Vice President JD Vance attended the US inaugural prayer service in January 2025, Reverend Mariann Budde appealed to Trump to show mercy to minorities, noting that many now fear for their safety (Starcevic 2025). This concern is not isolated. The protection of minority rights is a core characteristic of liberal democracy (Mudde & Kaltwasser 2012, Powell 2000, Mukand & Rodrik 2020), yet the rise of far-right politicians and parties across Western democracies has raised questions about whether minorities face increasing bias and prejudice (Dancygier 2023, Wodak 2018), including greater exposure to bias-motivated harassment or violence (Riaz et al. 2024, Romarri 2020, Müller & Schwarz 2023).

Despite the widespread persistence of hate crimes, the direct political causes of hate crimes are only scarcely researched and often in varying political contexts. However, the few studies that do exist suggest a strong link between the exclusionary rhetoric of far-right politicians and mass prejudice (Riaz et al. 2024, Romarri 2020, Feinberg

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et al. 2022, Jardina & Piston 2023, Dancygier 2023). The core features of the far-right, namely nativism, authoritarianism, and populism (Pirro 2023), translate into positions and rhetoric that limit minority rights and fuel prejudice against minorities (Mudde 2007, Wodak 2020, Valentim 2024, Turnbull-Dugarte et al. 2025). This shapes minority groups as outgroups that threaten the cultural or ethnic status quo.

We study how the effect of far-right success plays out in one of the core representative institutions of liberal democracy: the lower chamber of national legislatures. In the past decade, the far-right has made substantial electoral gains in lower chambers across democracies. For example, The far-right combined gained 37% of votes in the recent French legislative election (2024), close to 35% in the most recent Italian general election (2022), and close to 30 % in the recent Dutch legislative election (2025). Furthermore, the "Make America Great Again" (MAGA) faction, often described as the US counterpart to the European far-right (Lowndes 2018), now dominates the Republican Party in Congress (Biebricher 2024).

We consider how the effect of far-right success on hate crimes plays out in the context of first-past-the-post (FPTS) lower chamber elections. Under this electoral system, political competition is often reduced to two viable competitors in every district (Cox 1997, Aldrich & Lee 2016). This means that in districts where the far-right competes, far-right success is measurable in absolute terms since the candidate either wins or loses. Far-right victories are therefore explicit, immediate, and yield uncontested legislative mandates.

We theorize that this creates a political opportunity structure for the amplification and legitimization of outgroup threats in the districts they represent. Exclusionary rhetoric from far-right actors exacerbates differences between dominant 'native' groups and minority groups who do not conform to the status quo. We argue that this creates an 'exclusionary impulse' in the districts they represent, leading to a more permissive environment for expressions of bias and prejudice (Green et al. 1998). This can embolden a minority of perpetrators to commit hate crimes (Dancygier 2023). We also consider the temporal dimension of far-right success and theorize that effect is amplified by a macro-causal factor of wider far-right normalization (Valentim 2024, Dancygier 2023).

As a case study, we focus on U.S. House of Representatives elections from 2006 to 2022. House elections offer an electoral context with strong links between elected officials and local constituents (Mayhew 1974). Throughout the political cycle, constituents encounter their representative's rhetoric not only in Washington but also in district-level settings such as town halls, local news, and community events (Gibson & King 2024). In addition, in the past two decades, there has been a ready supply of far-right candidates in the House of Representatives. This is reflected in the emergence of the Tea Party Caucus (2010) and Freedom Caucus (2015) (Costa & Kane 2015). U.S. House elections therefore provide an appropriate context in which to study the local consequences of far-right success.

Unlike European party systems, where far-right candidates can be identified based on party labels, the U.S. context requires a different approach to classify far-right candidates *within* the Republican Party. To do this, we identify far-right candidates using campaign finance scores (CF scores) from the Database on Ideology, Money in Politics, and Elections (Bonica 2024). Based on campaign donation patterns, these CF scores offer an extensively validated measure of ideology for both incumbent and challenger candidates in state and federal-level US politics.[1] We develop an absolute threshold of far-right ideology based on the ideological distributions of the Tea Party Caucus (2010) and the Freedom Caucus (2015).

We estimate the causal effect of electing a far-right candidate on hate crime rates in their districts using a sharp regression discontinuity design. We compare districts where far-right candidates barely won to districts where they barely lost. Our outcome variable measures both short-term changes in hate crime rates, within two months of the election, and longer-term changes, six months and one year after the election.

We find no general effects of far-right success on district-level hate crime rates against minoritieswhen pooling all election years from 2006 to 2022. However, when restricting the sample to the Trump years (2016–2022), we detect clear increases in hate crime rates unfolding one year after the election. Our preferred estimate indicates that districts where far-right candidates narrowly win experience an increase of roughly 1.2 incidents per 100,000 inhabitants in the year after the election. These findings are consistent with the idea that far-right victories have stronger consequences in periods of heightened far-right normalization (Valentim 2024, Dancygier 2023). We also distinguish violent and non-violent hate crimes, since reporting practices differ across offense types and violent incidents are less sensitive to underreporting. Violent hate crimes increase in the short term, within two months of the election, although this effect does not persist beyond that window. In contrast, non-violent hate crimes increase in the one-year window during the Trump years. Finally, exploratory analyses indicate that these longer-term responses are not evenly distributed across minority groups. LGBTQ+ individuals in particular appear to bear a disproportionate share of the increase in hate crimes, which aligns with the prominence of anti-LGBTQ rhetoric in far-right political discourse

To explore the mechanism behind these effects, we examine survey data from the Cooperative Election Studies (2006–2022) and Nationscape (2020). Consistent with existing work on far-right politics and public opinion (Valentim 2024, Riaz et al. 2024), we do not detect short-term changes in attitudes toward minority groups. Rather, we find evidence that far-right victories increase the salience of white identity. We interpret this as an exclusionary impulse that creates a more permissive environment for expressions of bias and prejudice against minorities who do not conform to the perceived status quo (Green et al. 1998, Rieder 1985, Suttles 1968, Riaz et al. 2024).

[1] see Bonica (2013) for validations

We make two main contributions to the literature. First, we provide one of the first causal tests of whether far-right electoral success increases hate crimes in the context of a major representative institution of liberal democracy: first-past-the-post lower-chamber elections. Despite the centrality of these institutions to democratic representation, they have been largely absent from research on far-right politics and bias-motivated behavior. Second, we show that the effect of far-right victories is temporally conditional. Electoral success increases hate crimes primarily in periods of heightened far-right normalization. This demonstrates that the broader political environment conditions whether, and how, far-right success translates into mass behavioral changes (Valentim 2024).

In the following section we review existing explanations of hate crimes and develop our argument about the effect of far-right success on hate crimes.

2 Theory

2.1 Hate Crimes and Determinants

Hate crimes are a pervasive problem in Western liberal democracies. A recent report from the European Union Agency for Fundamental Rights (FRA) highlights that several minority groups in the EU are frequent targets of hateful acts, including hate crimes.[2]. In the United States, reported hate crimes have doubled between 2015 and 2025 (USAFacts 2025). While definitions of what constitutes a hate crime vary at the national and subnational level, hate crimes are generally considered crimes that involve bias against someone's religious or ethnic background, sexual orientation, gender identity or disability (Sheppard et al. 2021, Hall 2013).[3] Hate crimes can involve offenders that display high prejudice or offenders who exhibit low or subconscious prejudice. In some situations, there is a clear link between the offender's prejudice and the crime, while in others this is less evident (Jacobs & Potter 2000). Ultimately, although not all hate crimes are motivated by explicit *hate*, there is a common thread of some degree of prejudice in all these crimes (Hall 2013), whether explicit or implicit. For the same reason, hate crime reporting agencies such as the UCR Program (U.S. Department of Justice 2024) include crimes committed in whole or in part by the offender's bias against a group in their definition of hate crime.

Despite their pervasiveness, the precise causes of hate crimes remain somewhat elusive and varied. This is reflected in the interdisciplinary nature of hate crime research. According to Dancygier & Green (2010), there are three main strands in this literature. One strand focuses on the psychological traits that might make someone more likely to engage in bias-motivated offenses. A second strand focuses on differentiating types of bias

[2]The groups mentioned in the report are Jews, Muslims, Black people, migrants, lesbian, gay, bisexual, transgender, intersex and queer (LGBTIQ) people

[3]see Hall (2013) for an extensive overview of different definitions.

motivations to develop classifications of offenders. A third strand examines contextual factors, such as joblessness, community dynamics, or political systems, that influence the occurrence of hate crimes.

Our research fits into this last area of study. Work on contextual determinants has drawn attention to local community dynamics.. More specifically, several studies focus on what Green et al. (1998) refer to as as an 'exclusionary impulse'. The sources of this impulse may, of course, vary. In their study of anti-minority crimes in New York City neighborhoods between 1987 and 1995, Green et al. (1998) show that macro-level trends, such as unemployment rates or local economic conditions, are not good predictors of anti-minority hate crimes. Instead, these crimes appear most frequently in predominantly white areas, particularly those that have experienced a recent influx of minorities. This finding resonates with early ethnographic work describing how residents understand themselves as sharing a common identity that hinges on the exclusion of outgroups (Suttles 1968, Rieder 1985). More recent studies echo this logic of exclusionary impulse. Riaz et al. (2024) show that incidents of immigrant-attributed crimes in Germany increase the probability of hate crimes against refugees. Using geocoded data and a regression discontinuity in time design, they demonstrate that the likelihood of hate crimes rises immediately after triggering incidents, consistent with an 'emotional trigger' of pre-existing prejudice among residents. Other work assigns a greater role of exclusionary rhetoric as a source of an exclusionary impulse. Feinberg et al. (2022) find that counties hosting Trump rallies experienced more hate crime and bias incidents than other counties. Across these studies, an exclusionary impulse, whether driven by demographic change (Green et al. 1998), triggering events (Riaz et al. 2024), or exclusionary rhetoric (Feinberg et al. 2022), can increase hate crime rates at the local level.

2.2 The Exclusionary Impulse of Far-Right Success

We build our argument on the same logic. We argue that the electoral success of far-right politicians serves as another event that triggers an exclusionary impulse. We use the term 'far-right' as an umbrella term to refer to radical right and extreme right actors who share an exclusionary worldview (Pirro 2023). This exclusionary worldview is rooted in nativism and authoritarianism: The former is a more radical, exclusion-focused version of nationalism that claims a country should be populated only by those considered part of the 'native' group (Pirro 2023, Mudde 2019). Anyone deemed non-native threatens the cultural and social unity of the nation-state.

In their positions and rhetoric, far-right politicians often try to limit minority rights and actively fuel prejudice against ethnic, religious and sexual minorities. They do so by framing minorities as out-groups who threaten the cultural or ethnic status quo and therefore must be controlled or marginalized (Haas et al. 2025, Turnbull-Dugarte et al. 2025, Pirro 2023, Valentim 2024, Mudde 2007).

We argue that the election of far-right politicians serves as an 'exclusionary impulse'. By this we mean to say that local residents become more aware of a perceived common identity and of the idea that this identity depends on the exclusion of minority outgroups (Suttles 1968, Rieder 1985). In first-past-the-post lower house elections, the relevant local level is the congressional district that a politician represents. We posit that this elite-constituent linkage creates a political opportunity structure for the amplification and legitimization of exclusionary rhetoric.

In other words, there are two mechanisms underlying the 'exclusionary impulse' of far-right success: the increased *visibility* of exclusionary rhetoric and the *legitimization* of such rhetoric that stems from the electoral victory. Regarding the visibility of exclusionary rhetoric, constituents encounter their representative's messaging not only in Washington but also within the district at town hall meetings, local media appearances, and community events (Gibson & King 2024). Thus, if a far-right candidate wins, exclusionary messages will reverberate more frequently than in districts where such candidates lose. Regarding legitimization, first-past-the-post elections have a clear winner and loser structure (Cox 1997). Electoral victory confers institutional recognition, which can legitimize exclusionary views among residents (Bischof & Wagner 2019).

As a result of this exclusionary impulse, we argue that local communities become more aware of the dominant common identity of their community and the threat that minority out-groups pose to this common identity. This leads to a more permissive environment for bias and prejudice. While this does not necessarily lead to a larger pool of individuals willing to commit hate crimes, we expect that a more permissive environment lowers the barriers for those with prejudiced motivations to act, because they sense broader community tolerance for, or even encouragement of, hate crimes (Dancygier 2023, Riaz et al. 2024). The mechanism is therefore consistent with heterogeneity in effects, as not all minority groups are equally targeted in far-right rhetoric, and with temporal dynamics, since increases in permissiveness may unfold gradually.

Considering all the above, we therefore hypothesize that:

H1 The election of a far-right candidate leads to an increase in district-level hate crime rates.

We also consider the temporal element of our argument. As Valentim (2024) argues, the success of the far-right and its normalization is a multi-staged process. In this process, it's not a given that far-right actors always have the momentum to be effective. The implication for our argument is that the exclusionary impulse of far-right victories might be stronger in a period of broader far-right normalization, when mass supportfor exclusionary ideas is more widespread and can resonate more strongly in local communities. We thus consider far-right normalization a larger 'macro-causal' factor (Dancygier & Green 2010), that could amplify the effect of H1:

H1a The effect of H1 is stronger during periods of macro-level far-right normalization.

3 Research Design

3.1 Case Selection: US House Races (2006-2022)

We focus on U.S. House of Representatives elections (2006-2022) to study whether the electoral success of far-right candidates increases hate crimes. House elections are a suitable case for several reasons. First, House elections exemplify the majoritarian logic of winner-takes-all systems, such that far-right victories in Congressional districts are absolute and uncontested. Second, House members maintain close ties with their local constituencies (Mayhew 1974). Constituents encounter their representative's messaging not only in Washington but also within the district, for example through town hall meetings, local news coverage, and community events (Gibson & King 2024). Third, the period we study coincides with a notable supply of far-right candidates within the Republican Party (Biebricher 2024). This trend began with the backlash to Barack Obama's 2008 election and the emergence of the Tea Party movement (Arceneaux & Nicholson 2012, Hall 2015), and accelerated with Donald Trump's 2016 victory and the growth of the MAGA movement (Gest et al. 2018). The institutional consolidation of this faction is reflected in the creation of the Tea Party Caucus (2010) and later the Freedom Caucus (2015) (Costa & Kane 2015). In all, House of Representatives elections offer a case in which far-right candidates regularly compete and in which the majoritarian logic creates clear far-right winners and losers.

3.2 Data and Variables

3.2.1 Candidate Ideology

We combine two sources to construct our data set of political candidates. First, we rely on the 'Candidates in American General Elections' data set by Cha et al. (2021), which provides a comprehensive list of candidates who ran in House elections between 2006 and 2022. The main advantage of this data set is that, in contrast to other similar efforts, it provides standardized candidate names across years, candidate incumbency status, and other relevant variables combined into a single file. Importantly, it also contains variables essential to our empirical strategy, including electoral outcomes and the margin of victory.

We merge this data with candidate information from the Database on Ideology, Money in Politics, and Elections (DIME) (Bonica 2024). In addition to candidate characteristics, this database includes data on campaign finances and contribution records, and contains multiple ideology estimates for candidates in House elections. Because DIME's ideological scaling relies primarily on campaign contributions, some candidates were not included because they did not raise funds from the required number of contributors to be included in the scaling.[4] We discuss the characteristics of missing candidates in Appendix A.

We construct our main independent variable, candidate ideology, using campaign finance scores (CF scores) from the DIME database. These scores positions each candidate along a liberal–conservative spectrum based on campaign contribution patterns. We use these score to identify far-right candidates.[5] Identifying far-right candidates is a challenge both empirically and conceptually. The conceptual challenge lies in the blurred distinction between radical right and extreme right actors (Pirro 2023). The empirical challenge is deciding where to draw the line. Previous work in American politics has labeled candidates as extremist when they are more ideologically distant from the median primary opponent (Hall 2015, Meisels 2025). However, this method identifies relative extremism and does not capture far-right ideology in absolute terms. Alternatively, one could use percentile-based cutoffs (e.g., top 10% or 25%), but these are arbitrary and lack theoretical justification.

To overcome these challenges, we draw on congressional caucuses as empirical markers of far-right ideology. Congressional caucuses are voluntarily formed associations of legislators with shared ideological or policy interests (Hammond 2001), and are often associated with ideological outliers (Ainsworth & Akins 1997). We focus on two caucuses that have historically been associated with the far-right: the Tea Party Caucus and its successor, the Freedom Caucus (Arceneaux & Nicholson 2012, Green 2019). To identify members of the Tea Party Caucus, we use a 2012 capture of its membership page from the Internet Archive's Wayback Machine, which lists 48 House members[6]. Because the Freedom Caucus does not publish a formal membership list, we use a 2023 Pew Research Center study that identifies 49 members or affiliates of the caucus.[7]

To create a threshold for far-right identification, we calculate the distribution of CF scores for all caucus members combined and use the median value as our cut-off:

$$\mathrm{FR}_{\mathrm{med}} = \mathrm{median}(\{x_i \in \mathrm{Far-RightCaucuses}\}) = 1.274$$

Figure 1 visualizes the distribution of CF scores of House candidates. It includes all distinct House candidates who have won at least one election between 2006 and 2022. The plot shows a clear separation between Democrats and Republicans, with Tea Party and Freedom Caucus members concentrated at the extreme conservative end. The vertical

[4]For more information about the contents of the DIME files and the inclusion-exclusion rules applied in the ideological scaling (see also Bonica (2013)) we refer to the DIME documentation.

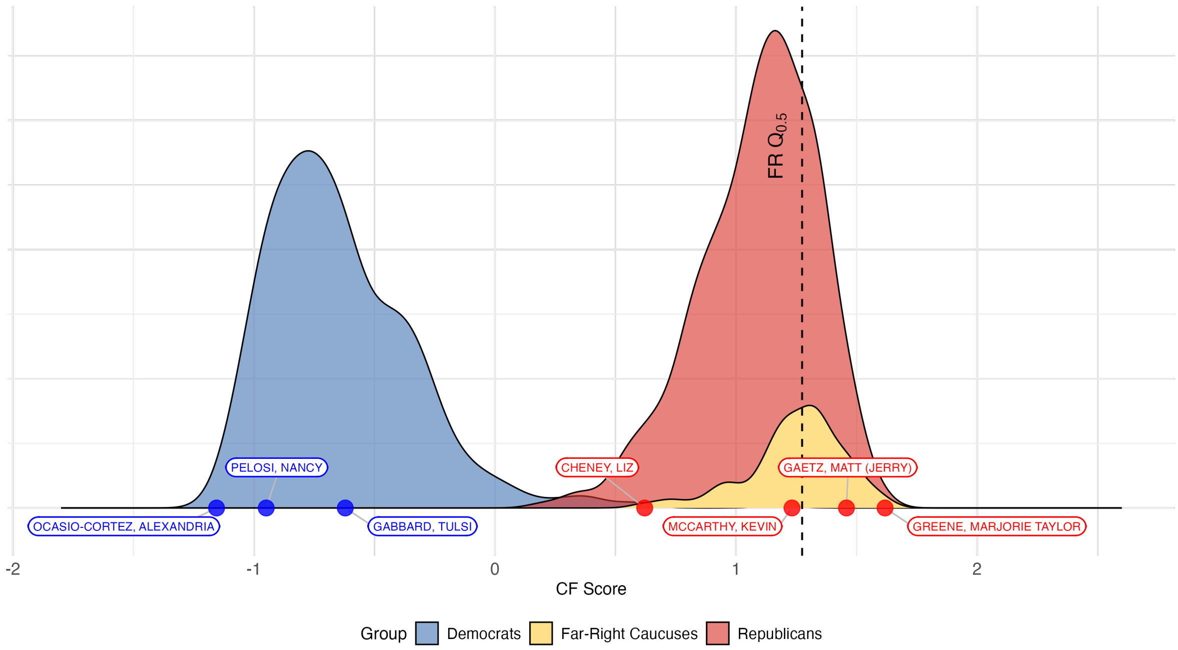
[5]Dynamic CF scores are time-variant.

[6]Tea Party Caucus member list: https://web.archive.org/web/20121211222811/http://teapartycaucus-bachmann.house.gov/membership

[7]Pew's identification methodology is detailed here. A full list of caucus members and their CF scores appears in Appendix B.

dashed line marks the 1.274 threshold.

Figure 1: Distribution of CF Scores



Notes: The CF scores shown here are time-invariant. Distribution of Republicans and Democrats includes distinct candidates that have won at least one House election between 2006-2022. Densities are relatively proportional to sample sizes.

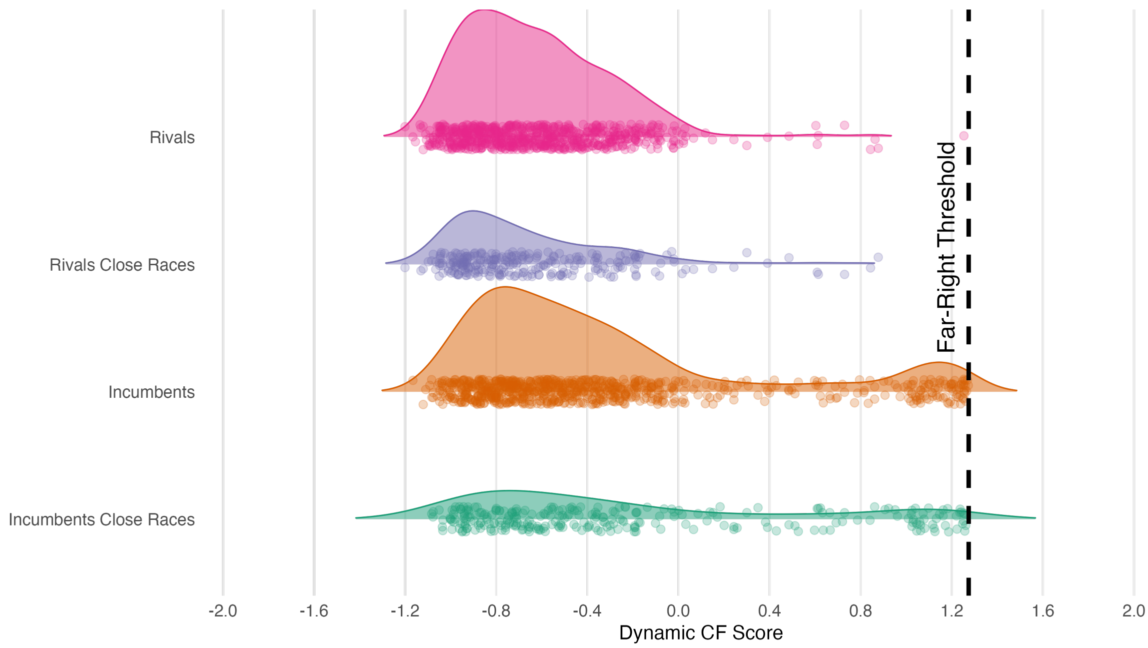
We argue that this is a conservative threshold in the sense that we may not capture all far-right candidates, especially some borderline cases. One example plotted in Figure 1 is Kevin McCarthy who, despite close association with Donald Trump and Marjorie Taylor Greene, falls below our cut-off. While the CF scores of caucus members are approximately normally distributed, the left tail suggests some ideological heterogeneity within the group. Applying this threshold to our full candidate sample yields a consistent definition of far-right candidates across all election years. We used the time-invariant CF score to compute our far-righ cut-off, which means that the same cut-off is used across all years. One potential concern is that the ideological distribution of candidates may shift over time, which could affect how conservative or inclusive our threshold appears in different cycles. We address this issue in Appendix D, where we show that the 1.274 cutoff remains substantively relevant across time and aligns with candidates commonly recognized as far-right.

3.2.2 Far-Right Races

We hone in on far-right races using the aforementioned threshold. Across the 2006–2022 period, we identify 1,050 congressional districts where at least one far-right candidate ran for office. Of those, 804 are districts where the far-right candidate contested an election without an incumbent from the far-right, providing plausible treatment-control comparisons. Narrowing the sample to the post-Trump period (2016 onwards), we observe 424 districts with far-right candidates running in races without a far-right incumbent. Descriptive statistics for treatment and control groups are reported in Appendix C.

We also report the ideological scores of the rivals whom far-right candidates ran against, and of the previous incumbent for those districts and years. We do this for the 804 districts that are at the core of our analysis[8]. The scores are summarized in Figure 2. The overall pattern is that most rivals and previous incumbents are moderate Democrats, both overall and in close races. However, for incumbents there is also a small bump on the right. What this means substantively is that we are analyzing races with significant *horizontal* ideological conflict (far-right versus rival) and, in many cases, *vertical* ideological conflict as well (far-right at time t versus incumbent at $t-1$).[9]

Figure 2: Dynamic CF Scores for Challengers and Previous Incumbents



Note: Close races are defined as races where the margin of victory falls within 20 points below or above the threshold.

3.2.3 Outcome Variable: Hate Crime Rates

To measure hate crime occurrences, we rely on hate crime data reported through the FBI's Uniform Crime Reporting (UCR) Program (U.S. Department of Justice 2024). These incident-based data include detailed information on location, victim and offender characteristics, and the underlying bias motivation[10]. We use the Hate Crime Statistics

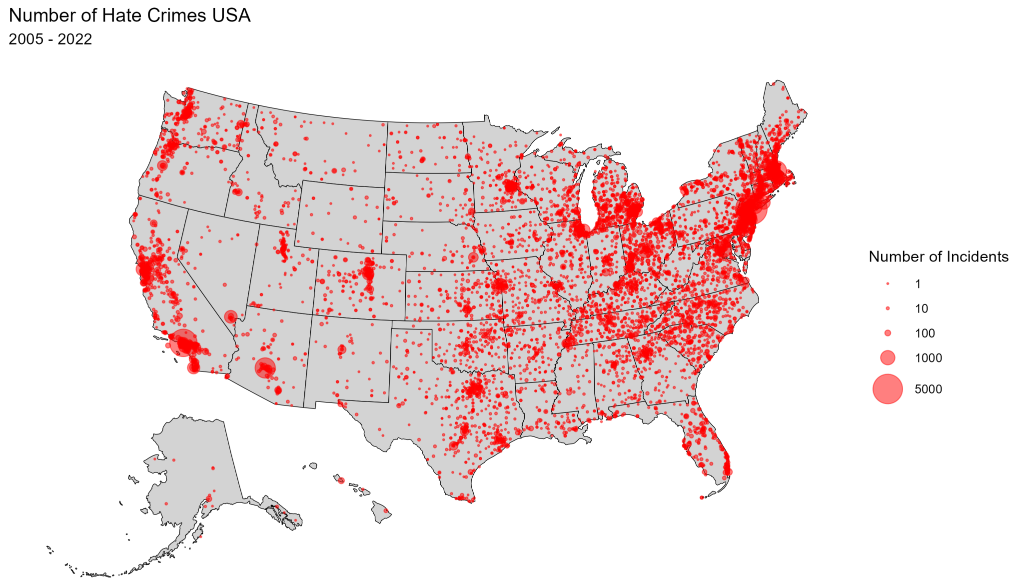
[8]Namely, districts where the far-right candidate contested an election without an incumbent from the far-right

[9]These categories are not mutually exclusive, since incumbents at $t-1$ are often rivals at t.

[10]The UCR dataset does not provide the exact crime location. Instead, incidents are linked to the reporting law enforcement agency, which can be a municipal police department, county sheriff office,

dataset, which tracks crimes "motivated in whole, or in part, by an offender's bias against the victim's perceived race, gender, gender identity, religion, disability, sexual orientation, or ethnicity" (U.S. Department of Justice 2024). Figure 3 shows the geographical spread of hate crimes reported through the UCR program between 2005 and 2022.

Figure 3: Map of hate crimes in the United States (2005-2022)



These data present measurement challenges. We need to assume that hate crimes are severely underreported. In a study by Pezzella et al. (2019), the authors compare the incidents reported through UCR between 2004 and 2012 to those of the National Crime Victimization Survey (NCVS). While only 8,770 hate crimes were reported through the UCR, the NCVS reports an average of 269,000 victimizations. Reported incidents through UCR thus represent about 3% of the NCVS average. Underreporting stems from victims not coming forward and from police misclassification (Pezzella et al. 2019). Because the likelihood of reporting varies substantially across offense types, we complement the main analysis by separating hate crimes into violent, non-violent, and property-related offenses. Violent offenses such as aggravated assault and homicide are more likely to be reported, whereas intimidation and other non-violent offenses are more sensitive to variation in reporting practices. Property-related offenses are examined separately. A full list of UCR offense categories and their classification into these three groups is provided in Appendix

campus police office, or similar entity. We assign each hate crime to the ZIP code of the reporting agency. In the small number of cases where a ZIP code spans multiple congressional districts, we allocate the incident to all corresponding districts.

C.4. While we cannot fully correct for underreporting, this disaggregation allows us to assess whether our results reflect genuine behavioral responses or potential differences in reporting. Our estimates of a treatment effect should therefore be interpreted as lower bounds.

Our main dependent variable is the hate crime rate in each Congressional District for a given election year. We calculate the occurrence of hate crimes H as a fraction of the local population P. To do so, we use U.S. Census population data from 2006 to 2024 to calculate the total population of voting districts d for each election year y. We then calculate hate crime rates per 100,000 inhabitants using the following formula:

$$\text{Hate Crime Rate} = \left(\frac{H_{d,y}}{P_{d,y}}\right) \times 100,000$$

We compute hate crime rates for three post-election periods: short-term (2 months after), medium-term (6 months after), and long-term (1 year after). Because we analyze close races and some election results may not be known immediately, we add a buffer period of one week after the election in our construction of these time periods. We pool together hate crimes against all minority groups. This includes religious minorities (Jewish and Muslim communities); racial and ethnic minorities (Black Americans, Latinos, and Arabs); LGBTQ+ individuals, and disabled people. In Appendix C, we report descriptive statistics on hate crime rates for different minority groups. We also show trends in hate crimes among these groups and the composition of different hate crime types over time.

3.2.4 District and Candidate Characteristics

We include district-level socio-demographic and economic variables in our analyses that we draw from the American Community Survey (ACS). The ACS covers a broad set of topics including education, income, employment, housing, and population composition, which allows us to capture year-specific variation in population characteristics at the congressional district level. We use the following district-level socio-demographic variables: the percentage of minority residents, the percentage of Black residents, and the percentage of foreign-born residents. In addition, we include several socioeconomic indicators: median household income, the poverty rate, and the unemployment rate. These variables are used to describe baseline district characteristics and to adjust for potential selection into treatment in our empirical models. Summary statistics for all district-level control variables are presented in Appendix C. We also collect candidate characteristics such as age, gender, and ethnicity. We do not use these variables for covariate adjustment. Rather, we use candidate characteristics in one of our continuity tests, as described in more detail in Section 4.1.

4 Identification Strategy

Our identification strategy employs a sharp regression discontinuity design (RDD) to exploit the discontinuity created by closely contested elections between far-right and non–far-right candidates. We focus on elections where a far-right candidate is one of the top two contenders and leverage the first-past-the-post electoral system. We compute the vote margin as the difference between the far-right candidate's vote share and that of their main competitor, and use this margin as the running variable. The cut-off is set to zero, so that we can compare units just above and below the threshold. We estimate the effect of electing a far-right candidate on district-level hate crime rates in these narrowly decided races. The key assumption behind this method is that districts where a far-right candidate narrowly wins are, in expectation, similar to those where a far-right candidate barely loses. Under this assumption, any discontinuous change in the outcome at the threshold can be interpreted as the local average treatment effect (LATE) of electing a far-right candidate.

4.1 PCRD Design and Estimand

Because our main interest is in *far-right* candidates, we are adopting a politician characteristic regression discontinuity (PCRD) design (Marshall 2024). The key implication of this design is that we cannot fully isolate the causal effect of far-right *ideology*. In close races, far-right candidates who narrowly win may differ from their opponents not just ideologically speaking, but also in terms of other observed and unobserved correlated characteristics Z that may affect their competitiveness. As a result, our estimand captures the causal effect of the electoral victory of a far-right candidate, not the pure effect of far-right ideology itself.[11] This distinction is important because far-right candidates who are competitive in close races may do so in part due to a bundle of compensating differentials Z that correlate with both ideology and electoral success.[12]

The next question is how to deal with this bundle of characteristics. We choose not to adjust for candidate characteristics in our PCRD. Our main reason for not doing this is that we cannot adjust for all compensating differentials. Selecting some but not other candidate characteristics may in fact lead to a subsequent problem where we need to adjust for more compensating differentials (Marshall 2024). Instead, we show descriptive data and conduct a continuity test for candidate characteristics, which can be found in Appendix H.3. In keeping with Marshall (2024), we use this information primarily as an aid for interpreting our results.

[11] We follow the wording of Bucchianeri (2018) in defining our estimand.

[12] In other words, our estimand takes on the following form: τ_X + (bias from compensating differentials Z)

4.2 District Characteristics and McCrary Density Test

We also take heed of potential concerns about covariate imbalance around the threshold of close-election RD designs (Caughey & Sekhon 2011, Eggers et al. 2015, De la Cuesta & Imai 2016). We address these concerns in two tests. First, we conduct a continuity test for the district characteristics that we derived from the American Community Survey. None of the district characteristics show discontinuity around the threshold. The results can be found in Appendix H.2. Second, we test for potential sorting around the threshold using the McCrary density test (McCrary 2008). The results, reported in Appendix H.1, do not show any evidence of sorting around the threshold or manipulation of the running variable.

4.3 Equation

To estimate the treatment effect, we use the following specification:

$$Y_{d,t} = \alpha + \beta_1 FR_{d,T} + \beta_2 M_{d,T} + \beta_3 M_{d,T} FR_{d,T} + \zeta X_{d,T-1} + \gamma_t + \epsilon_{d,t} \tag{1}$$

Where $Y_{d,t}$ represents the proportion of hate crimes per 100.000 inhabitants reported in district d in year t. $FR_{d,T}$ is a dummy for treated districts equal to 1 if a far-right candidate was elected in the most recent election year T. $M_{d,T}$ is the margin of victory (the running variable) in district d in most recent election year T. $X_{d,T-1}$ contains pre-treatment district-level controls such as employment rate, the share of minority residents, and median income. Finally, $\epsilon_{d,t}$ is an error term and γ_t are year fixed effects. The coefficient β_1 captures the discontinuous jump in the outcome at the threshold and is our main parameter of interest.

We implement this estimation using the robust local polynomial estimator developed by Calonico et al. (2014), which selects optimal bandwidths to minimize mean squared error. As a robustness check of our results, we also run the regressions by using polynomials of order 2 for our RD running variable, Results for those models are shown in Appendix I and closely match our main estimates.

5 Empirical Results

We now turn to our results. Table 1 presents our RD estimates. Our results are divided into three panels, capturing hate crime rates within two months, six months, and one year following the election. We report estimates without and with baseline covariates. All models have year fixed effects and are estimated using local linear regressions with robust bias-corrected confidence intervals (Calonico et al. 2014). The corresponding RD plots are shown in Figure A11 in Appendix I.1.1.

Table 1: Election of Far-right Candidates on Hate Crimes against Minorities

Panel A: 2 Months Window

	Minority Hate Crimes			
	All Years		Trump Years	
	(1)	(2)	(3)	(4)
RD Estimate	0.033	0.054	0.214***	0.180**
	(0.060)	(0.061)	(0.081)	(0.075)
Controls	No	Yes	No	Yes
Bandwidth	0.227	0.208	0.158	0.213
N (Left)	225	204	79	121
N (Right)	92	89	44	51
Order_polyn.	1	1	1	1

Panel B: 6 Months Window

	Minority Hate Crimes			
	All Years		Trump Years	
	(1)	(2)	(3)	(4)
RD Estimate	0.055	0.137	0.389	0.479**
	(0.164)	(0.156)	(0.243)	(0.220)
Controls	No	Yes	No	Yes
Bandwidth	0.182	0.178	0.136	0.134
N (Left)	174	171	71	71
N (Right)	80	80	39	38
Order_polyn.	1	1	1	1

Panel C: 1 Year Window

	Minority Hate Crimes			
	All Years		Trump Years	
	(1)	(2)	(3)	(4)
RD Estimate	0.444	0.610*	1.368***	1.193***
	(0.369)	(0.360)	(0.525)	(0.435)
Controls	No	Yes	No	Yes
Bandwidth	0.191	0.182	0.129	0.159
N (Left)	182	172	64	80
N (Right)	83	80	38	44
Order_polyn.	1	1	1	1

Notes: Each panel reports RD estimates with robust standard errors in parentheses. Outcome variables are crimes against minorities over a 100.000 population. All regressions include Year Fixed Effects. Controls include District characteristics (share of minorities, migrants, unemployment and poverty; and district median income).
Signif.: ***: 0.01, **: 0.05, *: 0.1.

We hypothesized that the election of a far-right candidate would lead to subsequent increases in hate crimes in the districts they represent. Columns 1 and 2 in Table 1 pool together all election years from 2006 to 2022. Across the three post-election windows, we do not detect a clear effect of electing a far-right candidate on district-level hate crime rates. This is consistent with the RD plots for the full period in Figure A11, which suggest at most a modest upward shift at the threshold that our design cannot estimate with precision. The minimum detectable effect (MDE) analysis in Table A13 in Appendix I.2.1 shows that, in this pooled sample, our design has power to detect only relatively large effects[13]. For the two-months window, for instance, the MDE is about 0.196 incidents per 100,000 inhabitants, while the corresponding point estimate in Table 1 is around 0.054. Therefore, these null results should not be taken as evidence that far-right victories do not affect hate crimes; they may only indicate that our design does not allow us to uncover such effects.

The next step is to consider the temporal dimension of far-right success. Columns 3 and 4 in Table 1 restrict the sample to elections from 2016 onward. This period coincides with a broader process of far-right normalization in national politics, driven in part by the growth of the MAGA movement (Biebricher 2024). In this subset, we detect clear increases in hate crime rates following the election of a far-right candidate. In the two months after the election, treated districts experience higher hate crime rates of roughly 0.18 incidents per 100,000 inhabitants. One year after the election, this difference rises to more than 1 incident per 100,000 inhabitants. The RD plots for the Trump years in Figure A11 show a more pronounced jump at the cutoff than in the pooled period, which is consistent with the estimates. These findings suggest that far-right success affects hate crime patterns primarily in periods of heightened far-right normalization.

A central issue when interpreting these patterns is that UCR data capture only a fraction of actual hate crime victimizations. Prior work suggests that reported hate crimes may represent only a small proportion of all incidents, and that underreporting can stem both from victims not coming forward and from variation in how agencies classify cases. This motivates our disaggregation by crime type, where more severe violent incidents such as aggravated assault, attempted homicide, and manslaughter are less likely to go unreported or misclassified.[14] The remaining incidents include intimidation, simple assault, and offenses involving minor physical force, which we group as non-violent crimes. This division allows us to assess whether the patterns we detect reflect genuine behavioral responses or potential changes in reporting.

[13] Some significant estimates in Table 1 fall below the minimum detectable effects reported in Table A13 in Appendix I.2.1. This is not inconsistent with the power analysis. By definition, the minimum detectable effect is the smallest true effect that the design would detect with a given probability in repeated samples, not a threshold that determines which realized estimates can be statistically significant (Bloom 1995, Duflo et al. 2007, see). Significant estimates smaller than the MDE may arise when the realized sampling variance is lower than anticipated. The limitation arises only for non-significant estimates below the MDE, since in those cases the study lacks power to rule out substantively meaningful effects.

[14] We also examine hate crimes against property, which cannot be cleanly classified as violent or non-violent. These results are shown in Appendix I.2.3.

Table 2 summarizes these patterns. We detect a clear short-term increase in violent hate crimes within two months after far-right victories, both when pooling all years and when focusing on the Trump years. These differences do not persist at six months or one year. This temporal pattern suggests that violent hate crimes may be brought forward in time in treated districts, with control districts catching up thereafter. Since event counts are relatively small, we cannot rule out that limited statistical power contributes to the absence of detectable longer-term differences. The RD plots in Figure A11, for instance, show a visible jump at the cutoff in the longer windows as well, which might indicate that treated districts continue to exhibit elevated levels of violent incidents after the short-term window.

In contrast to violent incidents, non-violent hate crimes display a more persistent long-term pattern. In the Trump years, treated districts exhibit higher rates of non-violent hate crimes one year after the election, with differences of roughly 0.6 to 0.9 incidents per 100,000 inhabitants. These offenses consist mainly of intimidation and other forms of harassment that are more sensitive to reporting practices. For this reason, these results should be interpreted with some caution, although the differences remain visible in the RD plots and are consistent with the broader temporal dynamics observed in the main estimates.

We also examine whether these patterns vary across minority groups. Because the number of hate crimes targeting specific groups is small, these analyses might also be underpowered and should be viewed as exploratory. Tables A10 and A11 report results for crimes against Jews, Muslims, African-americans, and LGBTQ+ people. The patterns that emerge suggest that the burden of hate crimes is not evenly distributed across groups. Antisemitic hate crimes show a short-term decline in treated districts, but this pattern is not sustained over longer windows. By contrast, hate crimes targeting LGBTQ+ people display a clearer long-term response. In the one-year window during the Trump years, treated districts exhibit higher levels of violence and harassment against LGBTQ+ individuals, which aligns with the prominence of anti-LGBTQ rhetoric in far-right political discourse. These longer-term patterns are concerning, since they indicate that some minority communities experience elevated levels of hostility well beyond the immediate post-election period.

Finally, we conduct several robustness checks. First, we consider alternative definitions of far-right candidacies, including different CF score thresholds and caucus-based classifications. As shown in Appendix I.2.4, the results remain substantively unchanged. Second, we estimate RD models with higher-order polynomials in the running variable. The estimates in Appendix I.2.2 closely match the main local linear results. These checks indicate that our findings are not driven by modeling choices or by the operationalization of far-right candidacies.

Table 2: Election of Far-right Candidates on Hate Crimes against Minorities: Violent vs. Non-Violent

Panel A: Against Minorities - 2 Months Window

	All Years				Trump Years			
	Violent		Non-Violent		Violent		Non-Violent	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
RD Estimate	0.121***	0.124***	-0.023	-0.024	0.155***	0.153***	0.040	0.061
	(0.031)	(0.030)	(0.034)	(0.032)	(0.044)	(0.041)	(0.046)	(0.051)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Bandwidth	0.164	0.141	0.174	0.189	0.164	0.143	0.137	0.119
N (Left)	158	136	169	180	86	73	72	56
N (Right)	77	71	80	83	44	41	40	33
Order_polyn.	1	1	1	1	1	1	1	1

Panel B: Against Minorities - 6 Months Window

	All Years				Trump Years			
	Violent		Non-Violent		Violent		Non-Violent	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
RD Estimate	0.108	0.112	-0.039	-0.026	0.116	0.112	-0.105	0.003
	(0.078)	(0.071)	(0.076)	(0.076)	(0.101)	(0.088)	(0.119)	(0.116)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Bandwidth	0.153	0.142	0.191	0.243	0.164	0.157	0.257	0.224
N (Left)	143	137	182	248	85	79	151	126
N (Right)	75	71	83	96	44	44	56	52
Order_polyn.	1	1	1	1	1	1	1	1

Panel C: Against Minorities - 1 Year Window

	All Years				Trump Years			
	Violent		Non-Violent		Violent		Non-Violent	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
RD Estimate	0.177	0.189	0.156	0.207	0.269	0.320*	0.615**	0.865***
	(0.152)	(0.140)	(0.173)	(0.180)	(0.200)	(0.176)	(0.249)	(0.311)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Bandwidth	0.177	0.160	0.185	0.197	0.152	0.135	0.138	0.120
N (Left)	171	151	175	191	75	71	72	56
N (Right)	80	77	82	85	43	38	40	33
Order_polyn.	1	1	1	1	1	1	1	1

Notes: Each panel reports RD estimates with robust standard errors in parentheses. Outcome variables are crimes against minorities over a 100.000 population. All regressions include Year Fixed Effects. Controls include District characteristics (share of minorities, migrants, unemployment and poverty; and district median income).
Signif.: ***: 0.01, **: 0.05, *: 0.1.

6 Causal Mechanism Test

In our main results we established a link between the electoral success of far-right politicians and subsequent rises in hate crime rates, particularly during the Trump years (2016-

2022). In this section we examine the causal mechanism that may underlie this link. Our theoretical argument is that far-right success amplifies and legitimizes exclusionary rhetoric. This amplification and legitimization can generate an exclusionary impulse in local communities, where residents become more aware of a common (white) identity and of the idea that this identity depends on the exclusion of minority groups. Such an impulse may create a more permissive environment in which expressions and acts of bias and prejudice against minorities who differ from the perceived status quo become more tolerated or encouraged.

To test this mechanism, we draw on large survey data to examine whether community-level attitudes about social identity and minority groups shift in response to far-right success. We use two data sources: the Cooperative Election Studies (CES, formerly the Cooperative Congressional Election Studies) and Nationscape (2020). Both sources provide large samples with full coverage of U.S. congressional districts, which allows us to construct district-year averages. The CES surveys approximately 50,000 respondents every election cycle in pre-election and post-election waves. We focus on post-election waves from 2006-2022.[15] Nationscape is one of the largest cross-sectional public opinion surveys conducted in the United States, with close to half a million respondents (Tausanovitch & Vavreck 2021). Data were collected on a weekly basis in the months before and after the 2020 presidential and House elections. We pool together ten waves after the House election (from November 5 to January 12), which yields 64,010 post-election observations for constructing district-level averages for 2020. More details about both surveys are provided in Appendix G.

In both surveys, we focus on items that speak directly to attitudes related to identity and minority groups. In CES, we use responses to the statement "White people in the U.S. have certain advantages because of the color of their skin" and to the statement "Racial problems in the U.S. are rare, isolated situations" to capture perceptions of white identity and racial denial. We then analyze two standard items on racial resentment toward Black Americans. The first reads "Irish, Italians, Jewish and many other minorities overcame prejudice and worked their way up. Blacks should do the same without any special favors." The second reads "Generations of slavery and discrimination have created conditions that make it difficult for Blacks to work their way out of the lower class." All items are measured on scales from 1 (Strongly Agree) to 5 (Strongly Disagree). In Nationscape, we study the same racial resentment and identity items, together with a battery of discrimination awareness questions that ask "How much discrimination is there in the United States today against each of the following groups?", with responses ranging from 1 (none at all) to 5 (a great deal).[16] Nationscape also includes a series of group favorability questions, which we analyze in Appendix I.3.2.[17] These outcomes do not show meaningful differences

[15] Some questions were fielded only from 2016 onward.

[16] We rescale these values so that higher scores reflect greater perceived discrimination.

[17] Favorability items ask respondents to rate their impression of each group on a scale from 1 (very

between treated and control districts.

All attitudinal variables are standardized and are aggregated to the district level by taking the district average. We then substitute these district-level attitudinal averages as the outcome variable in our regression discontinuity design. We use the same estimation approach as in section 4.3 where $Y_{d,t}$ now represents the average attitudinal score for district d in election year t. Full question wordings, answer categories, and group-specific details are provided in Appendix G. Descriptive statistics for the attitudinal outcome variables are shown in Table A4 in Appendix C.

We report attitudinal outcomes in Table 3. Panel A summarizes the Nationscape results. The estimates show that the election of a far-right candidate does not lead to short-term changes in attitudes toward minority groups, including measures of racial resentment toward Black Americans. This aligns with existing work on far-right politics and public opinion that finds that short-term shifts in mass attitudes are unlikely (Valentim 2024, Riaz et al. 2024). In contrast, Panel B of Table 3, which reports CES results, reveals a notable pattern on the white privilege item. The negative coefficient indicates that districts that narrowly elect a far-right candidate exhibit higher agreement with the idea that white people enjoy certain advantages because of their skin color. This suggests that far-right victories make white identity more salient and more explicitly acknowledged.

We interpret this as suggestive evidence that far-right success triggers the type of exclusionary impulse discussed in our theory. Even if explicit hostility toward minority groups does not shift immediately, residents in treated districts appear to become more aware, and potentially more protective, of a perceived in-group identity. Such a shift is consistent with an environment in which bias and prejudice against minorities who deviate from this identity become more socially acceptable, and it provides a plausible mechanism linking far-right electoral victories to the rise in hate crimes documented in our main results.

7 Discussion and Conclusion

There is limited research on the direct political drivers of hate crimes. Yet, the rise of the far-right has been accompanied by prejudice and bias against minorities in political positions and rhetoric. In this paper, we have asked whether this leads to an increase in hate crimes. We examined this question in a core democratic institution with a clear winner and loser structure, first-past-the-post-elections to the lower chamber. As our case study, we focused on US House elections from 2006 to 2022.

We find no evidence of a general effect of far-right success. When all election years

unfavorable) to 4 (very favorable). We rescale these values so that higher scores indicate more favorable attitudes.

Table 3: Election of Far-right Candidates on Attitudes: Nationscape & CES

Panel A: Nationscape

	Disc. Minorities		Disc. Jews		Disc. Muslims		Disc. Blacks		Black's Work Ethic		Black's Structural Barrier	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
RD Estimate	-0.038	0.000	-0.023	0.001	-0.001	-0.003	-0.048	-0.065	-0.034	0.054	-0.036	0.005
	(0.066)	(0.054)	(0.072)	(0.068)	(0.063)	(0.062)	(0.056)	(0.048)	(0.056)	(0.067)	(0.060)	(0.049)
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Bandwidth	0.127	0.236	0.160	0.258	0.185	0.159	0.137	0.147	0.241	0.165	0.155	0.159
N (Left)	34	63	38	67	46	38	38	38	63	40	38	38
N (Right)	17	21	20	21	20	20	18	20	21	20	20	20
Order_polyn.	1	1	1	1	1	1	1	1	1	1	1	1

Panel B: CES

	White Privilege		Racial Denial		Black's Work Ethic		Black's Structural Barrier	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
RD Estimate	-0.168***	-0.179***	-0.110**	-0.150***	-0.009	-0.004	0.003	0.003
	(0.042)	(0.040)	(0.051)	(0.054)	(0.041)	(0.038)	(0.034)	(0.033)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Bandwidth	0.162	0.110	0.181	0.142	0.212	0.190	0.217	0.183
N (Left)	82	52	94	73	177	151	178	145
N (Right)	44	33	46	41	75	71	75	69
Order_polyn.	1	1	1	1	1	1	1	1

Notes: Each panel reports RD estimates with robust standard errors in parentheses. Outcome variables are standardized. Controls include District characteristics (share of minorities, migrants, unemployment, poverty, and district median income).
Signif.: ***: 0.01, **: 0.05, *: 0.1.

from 2006 to 2022 are pooled together, we do not detect short-term or long-term increases in hate crime rates in the districts represented by far-right candidates. However, when we focus on a period of heightened far-right normalization, we find consistent and robust evidence that the electoral victories of far-right candidates lead to increased hate crimes in the districts they represent. In districts where the far-right narrowly won between 2016 and 2022, a period in which Trump's MAGA movement gained serious momentum (Biebricher 2024), hate crime rates increase by 1.2 incidents per 100,000 inhabitants. Given the severe under-reporting of hate crime incidents, this estimate should be interpreted as a lower bound.

We also examined the type of hate crimes that occur. Violent hate crimes increase in the short term, within two months of the election, but this effect does not persist. In contrast, non-violent incidents, primarily intimidation and harassment, increase in the one-year window during the Trump years. Exploratory analyses further suggest that these long-term responses are not evenly distributed across minority groups. LGBTQ+ individuals appear to bear a disproportionate share of the increase in hate crimes, which aligns with the prominence of anti-LGBTQ rhetoric in far-right discourse.

We theorized that hate crimes increase because the election of a far-right candidate leads to an 'exclusionary impulse' in the districts they represent. We borrow this term from Green et al. (1998) to describe a situation that triggers communities to become more aware of their common (white) identity and the exclusion of minorities that do not conform with this status quo (Suttles 1968, Rieder 1985). We argue that this would lead to a more permissive environment for bias and prejudice that lowers the barriers for hate crime perpetrators to act, since they may believe that there is greater community tolerance for bias-motivated incidents (Dancygier 2023). Consistent with work on far-right politics and mass attitudes (Valentim 2024, Riaz et al. 2024), we do not find short-term changes in attitudes toward minority groups. Instead, we find that far-right success increases the salience of white identity, which we interpret as evidence of an exclusionary impulse.

Our study offers two key contributions. First, we provide one of the few causal examinations of whether far-right electoral victories spur increases in hate crimes within a core democratic setting of liberal democracies: first-past-the-post elections to the lower chamber. Although these institutions are fundamental to democratic representation and feature a clear winner and loser dynamic, they have received little attention in research on far-right politics and bias-motivated behavior. Second, we demonstrate that the impact of far-right success highly depends on the broader political climate. We find that far-right victories lead to more hate crimes during periods in which far-right ideas have become increasingly normalized.

Our analysis is not without limitations. The most important stems from the severe under-reporting of hate crimes in the UCR data, which likely captures only a small fraction of actual incidents (Pezzella et al. 2019). Because reported incidents represent only a small share of all hate crimes, the levels we observe should be interpreted as lower bounds of the true underlying rates. This is especially relevant for interpreting the magnitude of the positive effects we identify for 2016 to 2022. Under-reporting depresses observed rates in all districts, although ex post reporting bias from victims or police is expected to be less pronounced for violent hate crimes, which are more likely to come to the attention of law enforcement. Future research could address this limitation by using National Crime Victimization Survey (NCVS) data to model or predict the true prevalence of hate crimes. Because the NCVS includes both reported and unreported victimization, researchers could use it to estimate reporting rates and construct correction factors that approximate the underlying incidence of bias-motivated offenses.

A second limitation concerns our operationalization of far-right candidates. We rely on a one-dimensional measure of ideology based on campaign-finance data (Bonica 2024). We acknowledge, however, that far-right ideology is multidimensional (Pirro 2023), and our measure cannot capture this variation in full. Nonetheless, we believe our approach provides a principled and transparent foundation for identifying far-right candidates in one of the world's largest democracies. Future research could build on this to develop more elaborated measures. As the far-right continues to grow, this will be of key importance.

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Appendix

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A Missing Candidates

Not all candidates appear in the DIME database. This is because not all candidates raised funds from the required number of contributors to be included in the scaling (Bonica 2024). In this appendix section, we inspect the descriptive statistics of this missing group of candidates relative to our sample. We compare their margins of victory in Figure A1, their party affiliation in Figure A2, and their incumbency status in Figure A3. The overall picture that emerges is that, compared to our sample, missing candidates are mostly challengers, often electorally unsuccessful, and have an over-representation of independent candidates. In all, we are not concerned that their absence from our sample will meaningfully alter our estimates.

Figure A1: Margin of Victory Scores

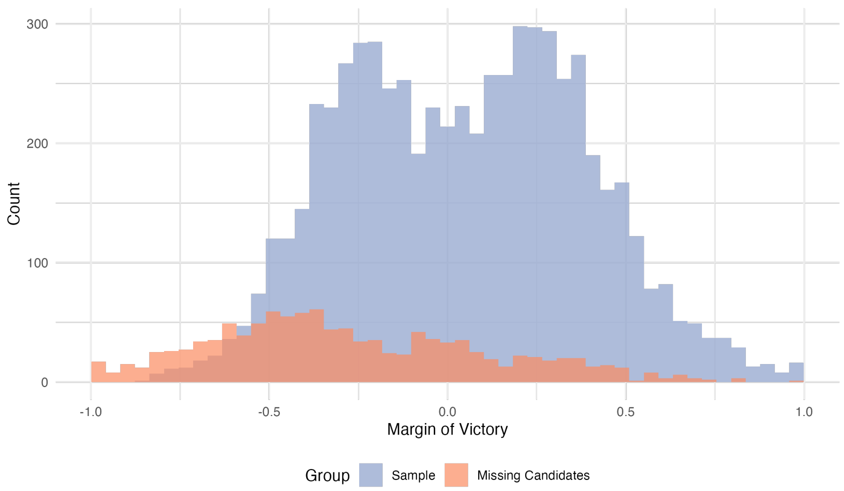


Figure A2: Party Affiliation

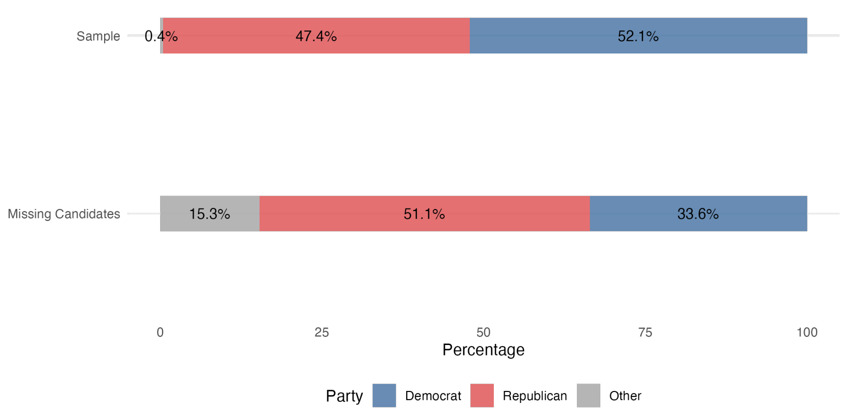
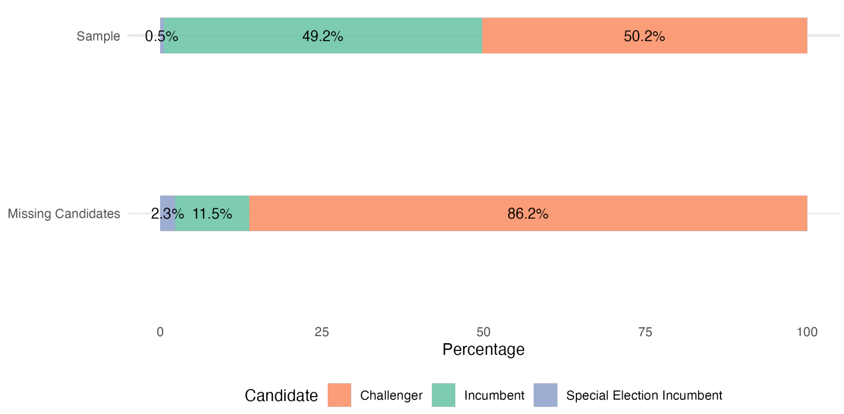


Figure A3: Incumbency Status



B List of Tea Party and Freedom Caucus members and affiliates

B.1 (Former) Tea Party Caucus Members

Table A1: (Former) Tea Party Caucus Members

Name	State	District	CF Score
BACHMANN, MICHELE	MN	06	1.48
BARTON, JOE LINUS	TX	06	1.20
BILIRAKIS, GUS MICHAEL	FL	12	0.73
BISHOP, ROBERT WILLIAM (ROB)	UT	01	0.91
BLACK, DIANE L.	TN	06	1.15
BROUN, PAUL COLLINS	GA	10	1.42
BURGESS, MICHAEL C.	TX	26	1.23
CARTER, JOHN R.	TX	31	1.17
CASSIDY, WILLIAM (BILL)	LA	06	0.98
COBLE, JOHN HOWARD	NC	06	0.81
COFFMAN, MICHAEL H. (MIKE)	CO	06	1.28
CRENSHAW, ANDER M.	FL	04	0.95
CULBERSON, JOHN ABNEY	TX	07	1.23
DUNCAN, JEFFREY D. (JEFF)	SC	03	1.33
FARENTHOLD, RANDOLPH BLAKE	TX	27	1.25
FINCHER, STEPHEN LEE	TN	08	1.21
FLEMING, JOHN C., JR.	LA	04	1.34
GINGREY, PHILLIP J. (PHIL)	GA	11	1.35
GOHMERT, LOUIS (LOUIE)	TX	01	1.47
HARTZLER, VICKY J.	MO	04	1.36
HUELSKAMP, TIMOTHY (TIM)	KS	01	1.47
JENKINS, LYNN M.	KS	02	1.18
KING, STEVEN A. (STEVE)	IA	04	1.45
LAMBORN, DOUGLAS L. (DOUG)	CO	05	0.99
LUETKEMEYER, W. BLAINE	MO	03	1.21
MARCHANT, KENNY E. (KEN)	TX	24	1.08
MCCLINTOCK, THOMAS MILLER (TOM), II	CA	04	1.27
MCKINLEY, DAVID B.	WV	01	0.61
MILLER, GARY G.	CA	31	0.71
MULVANEY, JOHN MICHAEL (MICK)	SC	05	1.30
NEUGEBAUER, RANDY	TX	19	1.34
PALAZZO, STEVEN MCCARTY	MS	04	1.20
PEARCE, STEVAN E. (STEVE)	NM	02	1.21
POE, LLOYD (TED)	TX	02	1.08
PRICE, THOMAS EDMUNDS (TOM)	GA	06	1.16
ROE, DAVID PHILIP (PHIL)	TN	01	1.16
ROSS, DENNIS ALAN	FL	15	0.93
ROYCE, EDWARD R. (ED)	CA	39	0.90
SCALISE, STEPHEN J. (STEVE)	LA	01	1.27
SCHWEIKERT, DAVID	AZ	01	1.23
SESSIONS, PETER A. (PETE)	TX	17	1.14
SMITH, ADRIAN M.	NE	03	1.17
SMITH, LAMAR S.	TX	21	1.23
STUTZMAN, MARLIN A.	IN	03	1.39
WALBERG, TIMOTHY L. (TIM)	MI	07	1.32
WESTMORELAND, LYNN A.	GA	03	1.25
WILSON, JOE	SC	02	1.11
YOHO, THEODORE SCOTT (TED)	FL	03	1.20

B.2 (Freedom Caucus Members and Affiliates

Table A2: (Former) Freedom Caucus Members and Associates (As Identified by Pew Research)

Name	State	District	CF Score
BIGGS, ANDY	AZ	05	1.45
BISHOP, DAN	NC	08	1.41
BOEBERT, LAUREN	CO	03	1.58
BRECHEEN, JOSH	OK	02	
BUCK, KENNETH (KEN)	CO	04	1.36
BURLISON, ERIC	MO	07	
CLINE, BENJAMIN LEE (BEN)	VA	06	1.23
CLOUD, MICHAEL	TX	27	1.34
CLYDE, ANDREW	GA	09	1.41
COLLINS, M.A. (MIKE)	GA	10	
CRANE, ELI	AZ	02	
DAVIDSON, WARREN	OH	08	1.33
DESJARLAIS, SCOTT EUGENE	TN	04	1.32
DONALDS, BYRON	FL	19	
DUNCAN, JEFFREY D. (JEFF)	SC	03	1.33
FULCHER, RUSSELL M. (RUSS)	ID	01	1.39
GAETZ, MATT (JERRY)	FL	01	1.46
GOOD, ROBERT G. (BOB)	VA	05	1.49
GOSAR, PAUL ANTHONY	AZ	09	1.27
GREEN, MARK E.	TN	7	1.32
GREENE, MARJORIE TAYLOR	GA	14	1.62
GRIFFITH, H. MORGAN	VA	09	1.11
HAGEMAN, HARRIET M.	WY	01	
HARRIS, ANDREW P. (ANDY)	MD	01	0.96
HARSHBARGER, DIANA LYNN	TN	01	1.32
HIGGINS, CLAY	LA	03	1.30
JACKSON, RONNY	TX	13	1.57
JOHNSON, MIKE	LA	04	1.41
JORDAN, JAMES D. (JIM)	OH	04	1.52
LESKO, DEBBIE	AZ	08	1.36
LUNA, ANNA PAULINA	FL	13	1.54
MILLER, MARY E.	IL	15	
MILLER, MAX	OH	07	1.15
MOONEY, ALEXANDER X. (ALEX)	WV	02	1.26
MOORE, BARRY	AL	02	1.35
MURPHY, GREGORY F. (GREG)	NC	03	1.15
NEHLS, TROY	TX	22	1.29
NORMAN, RALPH W.	SC	05	1.32
OGLES, ANDY	TN	05	
PALMER, GARY J.	AL	06	1.32
PERRY, SCOTT GORDON	PA	04	1.31
PERRY, SCOTT GORDON	PA	10	1.31
POSEY, WILLIAM (BILL)	FL	08	1.00
ROSENDALE, MATTHEW M. (MATT)	MT	01	
ROY, CHIP	TX	21	1.51
SCHWEIKERT, DAVID	AZ	01	1.23
SELF, KEITH A.	TX	03	
STEUBE, GREG	FL	17	1.20
TIFFANY, THOMAS P. (TOM)	WI	07	1.44
WEBER, RANDY K., SR.	TX	14	1.08

C Descriptive Statistics

C.1 Descriptive Statistics for Treatment and Control Groups

Table A3: Descriptive Statistics for Treatment and Control Groups

	Sample	Margin of Victory			
	(N)	(Mean)	(SD)	(Min)	(Max)
Far-right running					
Control Group	679	-0.306	0.180	-0.832	-0.002
Treatment Group	371	0.297	0.238	0.000	1.000
Far-right running & Non-incumbency					
Control Group	667	-0.310	0.178	-0.832	-0.003
Treatment Group	137	0.190	0.192	0.000	1.000
Far-right running (Trump years)					
Control Group	346	-0.300	0.182	-0.832	-0.003
Treatment Group	213	0.275	0.216	0.000	1.000
Far-right running & Non-incumbency (Trump years)					
Control Group	341	-0.304	0.180	-0.832	-0.006
Treatment Group	83	0.195	0.183	0.000	0.960

C.2 Descriptive Statistics for Outcome and Control Variables

Table A4: Descriptive Statistics for Outcome and Control Variables

Hate crimes Variables	N	Mean	SD	Min	Max
Hate Crimes Against Minorities	802	3.40	5.46	0.00	69.53
Hate Crimes Against Jews	802	0.50	1.88	0.00	38.66
Hate Crimes Against Muslims	802	0.09	0.19	0.00	1.98
Hate Crimes Against Blacks	802	1.16	1.51	0.00	17.85
Hate Crimes Against LGBTQ+ People	802	0.82	1.42	0.00	17.56

Attitudes Variables Nationscape	N	Mean	SD	Min	Max
Discrimination Awareness Minorities	168	3.36	0.15	2.92	3.68
Discrimination Awareness Jews	168	3.08	0.15	2.67	3.42
Discrimination Awareness Muslims	168	3.61	0.17	3.18	3.96
Discrimination Awareness Blacks	168	3.77	0.20	3.21	4.24
Black's Work Ethic Prejudice	168	3.29	0.21	2.72	3.76
Black's Structural Barrier Prejudice	168	2.65	0.26	2.05	3.63
Favorability towards Minorities	168	3.03	0.10	2.71	3.29
Favorability towards Jews	168	3.24	0.12	2.83	3.53
Favorability towards Muslims	168	2.86	0.16	2.29	3.27
Favorability towards Blacks	168	3.21	0.12	2.75	3.52
Favorability towards LGBTQ+	168	3.00	0.17	2.50	3.47

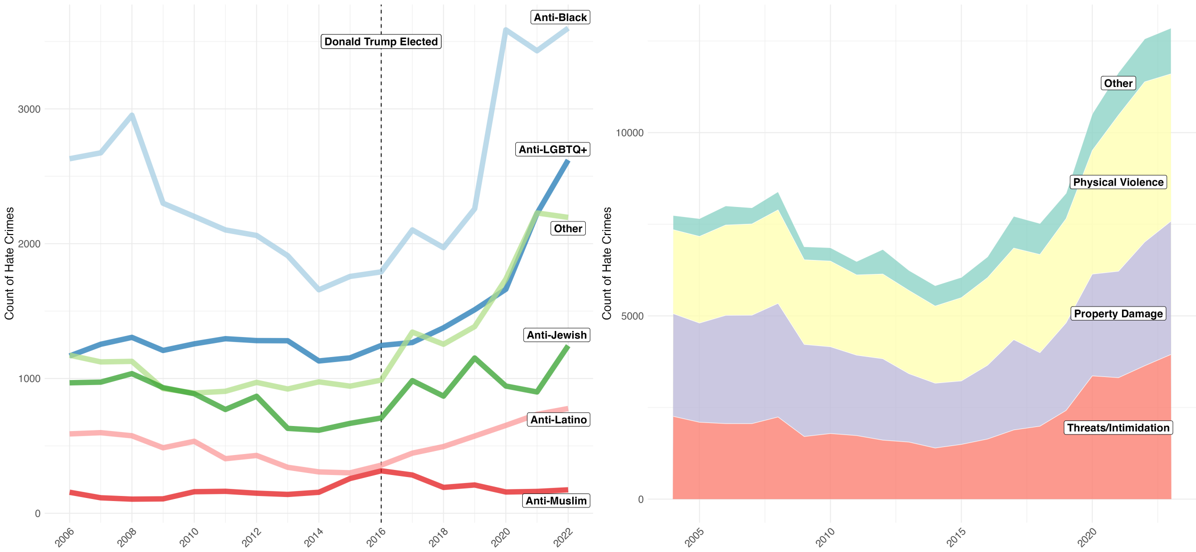
Attitudes Variables CES	N	Mean	SD	Min	Max
White Privilege Awareness	424	2.51	0.36	1.55	3.59
Racial Denial Attitude	424	2.31	0.27	1.57	3.29
Black's Work Ethic Prejudice	606	3.38	0.44	2.18	4.54
Black's Structural Barrier Prejudice	606	2.94	0.41	1.75	4.18

Control Variables	N	Mean	SD	Min	Max
District % Minority	804	0.42	0.24	0.03	0.91
District % Black	804	0.14	0.16	0.00	0.68
District % Migrant	804	0.15	0.11	0.01	0.52
District Median Income	804	61851.42	20748.05	27955.00	152783.00
District % Poverty	804	0.14	0.05	0.05	0.30
District % Unemployment	804	0.07	0.03	0.02	0.19

Notes: Count of observations and descriptive statistics for our outcome variables and control variables by district between 2006 and 2022.

C.3 Trends in Hate Crime Targets and Types

Figure A4: Trends in Hate Crime Targets and Crime Types



C.4 Classification of Hate Crime Offense Types

We group UCR hate crime incidents into three mutually exclusive categories: violent, non-violent, and property-related offenses. The classification is based on the primary offense code reported in the Hate Crime Statistics data. Violent hate crimes include offenses that involve physical force or the threat of serious physical harm, such as aggravated and simple assault, homicide and manslaughter, kidnapping, robbery, rape and other sexual assaults, and related offenses. Non-violent hate crimes include intimidation, weapon law violations, drug and fraud offenses, and other non-physical offenses. Property-related hate crimes include arson, burglary, motor vehicle theft, and destruction, damage, or vandalism of property.

Table A5 reports the full list of UCR offense categories used in our analysis, along with their classification and the number and share of incidents in each group. The vast majority of non-violent incidents are coded as intimidation, most violent incidents are cases of aggravated or simple assault, and most property incidents involve destruction, damage, or vandalism of property.

Table A5: Classification of Hate Crime Offense Types

Non-violent hate crime offenses	N	Percent
All Other Larceny	2,868	5.42
Assisting or Promoting Prostitution	9	0.02
Betting/Wagering	1	0.00
Bribery	6	0.01
Counterfeiting/Forgery	220	0.42
Credit Card/Automated Teller Machine Fraud	182	0.34
Drug Equipment Violations	476	0.90
Drug/Narcotic Violations	1,279	2.42
Embezzlement	55	0.10
Extortion/Blackmail	70	0.13
False Pretenses/Swindle/Confidence Game	349	0.66
Federal Liquor Offenses	1	0.00
Hacking/Computer Invasion	21	0.04
Identity Theft	116	0.22
Impersonation	157	0.30
Intimidation	43,092	81.51
Not Specified	620	1.17
Pocket-picking	32	0.06
Pornography/Obscene Material	93	0.18
Prostitution	14	0.03
Purchasing Prostitution	2	0.00
Purse-snatching	32	0.06
Shoplifting	749	1.42
Stolen Property Offenses	140	0.26
Theft From Building	610	1.15
Theft From Coin-Operated Machine or Device	13	0.02
Theft From Motor Vehicle	801	1.52
Theft of Motor Vehicle Parts or Accessories	203	0.38
Weapon Law Violations	625	1.18
Welfare Fraud	9	0.02
Wire Fraud	25	0.05
Total non-violent incidents	52,870	100.00

Property-related hate crime offenses	N	Percent
Arson	893	1.68
Burglary/Breaking & Entering	2,911	5.47
Destruction/Damage/Vandalism of Property	48,858	91.78
Motor Vehicle Theft	569	1.07
Total property incidents	53,231	100.00

Violent hate crime offenses	N	Percent
Aggravated Assault	16,886	31.31
Animal Cruelty	20	0.04
Criminal Sexual Contact	248	0.46
Human Trafficking, Commercial Sex Acts	3	0.01
Human Trafficking, Involuntary Servitude	1	0.00
Incest	7	0.01
Kidnapping/Abduction	170	0.32
Murder and Nonnegligent Manslaughter	153	0.28
Negligent Manslaughter	7	0.01
Rape	232	0.43
Robbery	2,809	5.21
Sexual Assault With An Object	55	0.10
Simple Assault	33,241	61.64
Sodomy	77	0.14
Statutory Rape	22	0.04
Total violent incidents	53,931	100.00

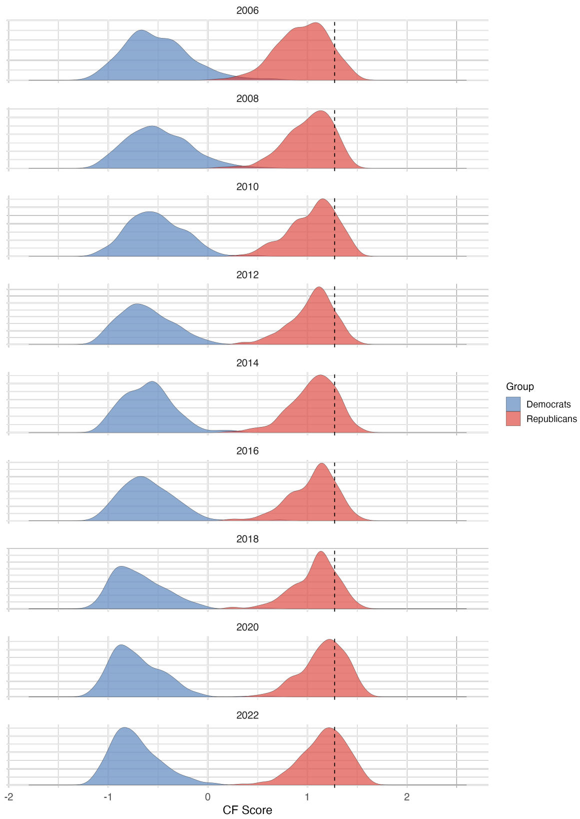
Notes: Count of observations and descriptive statistics of the types of Crimes reported to UCR. Classification made by authors.

D Threshold Robustness Checks

D.1 Spatial Relevance of Threshold with Dynamic CF Scores

In the main text, we contrast our far-right threshold to the distribution of time-invariant CF scores among Republicans and Democrats. In Figure A5 we also show how our threshold holds up compared to dynamic CF scores over the years. Overall, Figure A5 shows a process of ideological polarization where ideology scores for Democrats and Republicans are moving further apart from each other. Despite this our threshold remains spatially relevant to distinguish far-right candidates.

Figure A5: Threshold Positions with Dynamic CF Scores



D.2 $\mathrm{FR}_{\mathrm{med}}$ Threshold

As a robustness check, we verify whether our threshold value corresponds to a far-right candidate. The cut-off value of 1.274 corresponds to Steve Scalise. Scalise is currently the House majority leader and has represented Louisiana's first Congressional District since 2008. In the aftermath of the 2020 Presidential Elections, Scalise refused to acknowledge the loss of President Donald Trump (Pengelly 2021) He opposes abortion and same-sex marriage, and has a strong anti-LGBTQ voting record. Human Rights Campaign, an LGBT advocacy group, graded Scalise's voting record 0 (Johnson 2017). In 2017, Scalise also supported the travel ban, an executive order by Trump, that temporarily banned citizens from seven Muslim-majority countries from entering the U.S (The Denver Post 2017). In all, we believe our threshold meaningfully distinguishes far-right candidates.

E Data Imputation

We rely on CF scores from the DIME data set (Bonica 2024) to define far-right candidates. Some scores are missing. We therefore impute scores in two scenarios. First, the time-invariant CF score is sometimes missing for candidate-year observations where a time-variant (dynamic) CF score is missing, despite the candidate having been assigned a time-invariant score in other years. Since the time-invariant should be the same for each candidate across years, we impute time-invariant CF scores for candidate-year gaps if the candidate has at least one other time-invariant CF score in another of the other years. We do this for 227 out of 7645 cases, which amounts to 3% of our data set.

Second, when the time-variant (dynamic) CF score is missing but a time-invariant CF score is available for that candidate-year, we impute the dynamic score by substituting the available time-invariant score. We do this for 237 out of 7645 cases, which amounts to 3.1% of our data set.

F Redistricting Adjustments

F.1 Weighted CF Scores

Between 2006–2022, nationwide redistricting took place in 2012 and 2022, and other instances of redistricting took place following Supreme Court rulings in other years (2016 in FL, NC, VA; 2018 in PA, and 2020 in NC). The redrawing of electoral boundaries matters for our identification of previous winners in each district d, since incumbents of a district at time $t-2$ may run in a roughly similar or entirely different district at t. There might also be cases where newly drawn districts are composed of two or multiple former districts.

We address this challenge by comparing the territorial overlap between pre- and post-redrawn districts in 2012 and 2022 (all districts), 2016 (FL, NC, VA), 2018 (PA), and 2020 (NC). For each Congressional District at time t, we calculate the percentage of overlap with former Congressional Districts at time $t-2$ using Census Congressional Districts shapefiles.

Rather than choosing one previous winner for each district, we plug in the territorial overlap percentages to calculate a weighted CF score for these districts by weighting previous incumbents' CF scores.

Example: The 4th Congressional District in Alabama was redrawn in 2012 after the 2010 Census. The new district covers some parts of the 6th, 3th, 4th, and 5th Congressional Districts territories as they were drawn in 2010 and before. See Table A6.

Table A6: Overlap and CF-scores for last winners by GEOID

District PRE	District POST	Overlap	Last Winner Name	Last Winner CF Score (Time-Invariant)
AL04	AL06	9.10%	BACHUS, SPENCER THOMAS, III	0.986
AL04	AL03	0.83%	ROGERS, MICHAEL DENNIS (MIKE)	1.044
AL04	AL04	75.24%	ADERHOLT, ROBERT BROWN	1.086
AL04	AL05	14.83%	BROOKS, MORRIS J. (MO), JR.	1.356

We then compute the weighted CF-score for the new AL04 district as the overlap-weighted average of the static CF-scores:

$$\widehat{CF}_{\text{AL04,2012}} = \sum_{d' \in \{03,04,05,06\}} w_{d'}\, CF_{d'}^{\text{static}} \quad \text{where} \quad w_{d'} = \frac{\text{Overlap}_{\text{AL04},d'}}{100}$$

$$= \; 0.0910 \cdot 0.986 + 0.0083 \cdot 1.044 + 0.7524 \cdot 1.086 + 0.1483 \cdot 1.356 \; \approx 1.12$$

Thus the imputed (weighted) CF-score for Alabama's 4th district in 2012 is 1.1166.

G Survey Data

We draw on two large-scale surveys to test our causal mechanism: the Cooperative Election Studies and Nationscape. Below we report the question wording of variables we use in our analysis.

G.1 Cooperative Election Studies

G.1.1 White Privilege

Statement:"White people in the U.S. have certain advantages because of the color of their skin."

Answer options: "Strongly agree" (1), "Somewhat agree" (2), "Neither agree nor disagree" (3), "Somewhat disagree" (4), "Strongly disagree" (5).

G.1.2 Racial Denial

Statement:"Racial problems in the U.S. are rare, isolated situations.".

Answer options: "Strongly agree" (1), "Somewhat agree" (2), "Neither agree nor disagree" (3), "Somewhat disagree" (4), "Strongly disagree" (5).

G.1.3 Black's Work Ethic (same in Nationscape)

Question Prompt: "Please tell us how much you agree or disagree with the following statements. - Irish,Italians, Jewish and many other minorities overcame prejudice and worked their way up. Blacks should do the same without any special favors."

Re-scaled answer options: "Strongly agree" (5), "Somewhat agree" (4), "Neither agree nor disagree" (3), "Somewhat disagree" (2), "Strongly disagree" (1).

G.1.4 Black's Structural Barrier (same in Nationscape)

Question Prompt: "Please tell us how much you agree or disagree with the following statements. - Generations of slavery and discrimination have created conditions that make it difficult for blacks to work their way out of the lower class"

Answer options: "Strongly agree" (1), "Somewhat agree" (2), "Neither agree nor disagree" (3), "Somewhat disagree" (4), "Strongly disagree" (5).

G.2 Nationscape

G.2.1 Group Favorability

Question Prompt: "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?"

Re-scaled answer options: "Very favorable" (4), "Somewhat favorable" (3), "Somewhat unfavorable" (2), "Very unfavorable" (1)

Groups we include in our analysis: Blacks, Latinos, Asians, Muslims, Jews, Migrants, LGBTQ+ persons.

G.2.2 Discrimination Awareness

Question Prompt: "How much discrimination is there in the United States today against each of the following groups?"

Re-scaled answer options: "A great deal" (5), "A lot" (4), "A moderate amount" (3), "A little" (2), "None at all" (1)

Groups we include in our analysis: Blacks, Asians, Muslims, Jews.

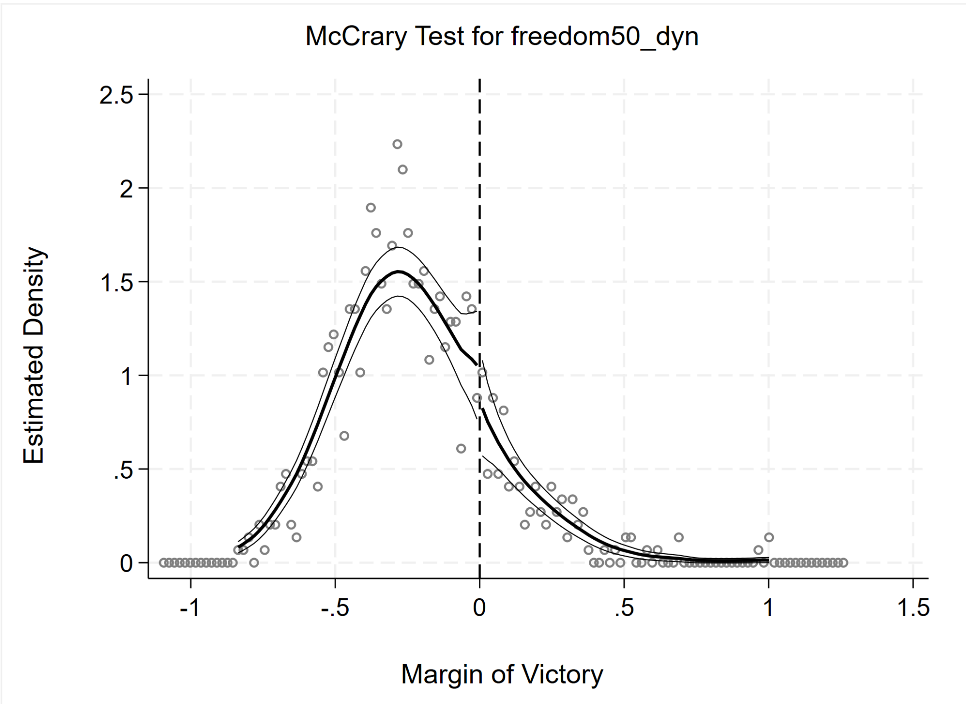
H RD Validity

H.1 Validity of the RD Design: McCrary Density Test

The regression discontinuity (RD) design yields unbiased estimates of the treatment effect when certain identification conditions are met. In this section, we provide evidence that these assumptions hold in our empirical setting.

A key threat to the validity of the RD design arises if individuals can precisely manipulate the assignment variable. In our context, a discontinuity at the cutoff could suggest that candidates are able to influence their margin of victory. To assess this concern, we conduct a McCrary density test (McCrary 2008), the results of which are shown in Figure A6. Reassuringly, the test indicates no significant discontinuity in the distribution of far-right candidates' margins around the threshold. To further rule out non-random sorting into treatment and control groups, we implement a data-driven manipulation test based on a local polynomial density estimation approach developed by Cattaneo et al. (2018). This method avoids pre-binning, thereby improving size accuracy, and permits the imposition of additional model restrictions, enhancing statistical power. As depicted in Figure A7, this test also finds no evidence of manipulation around the cutoff.

Figure A6: McCrary test: Far-right margin of victory (2006-2022)

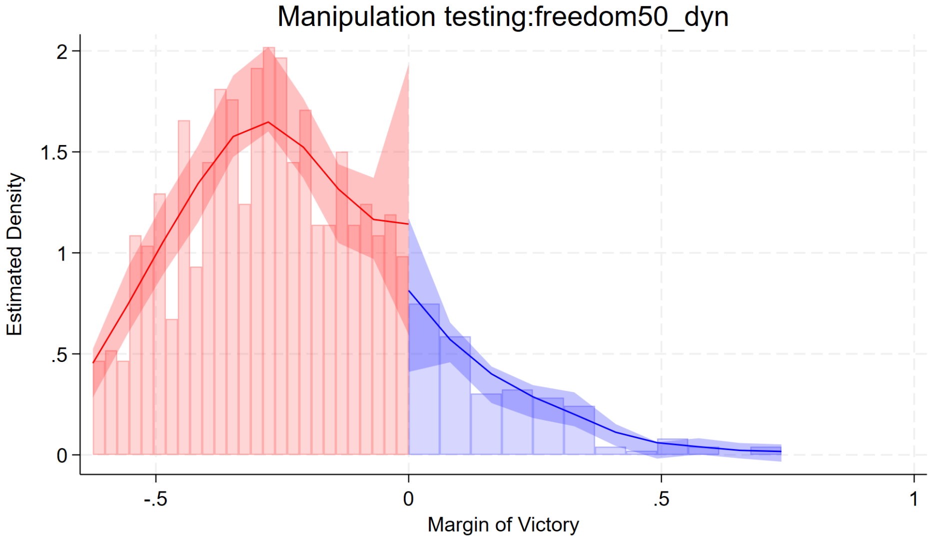


Notes: Graph produced by the authors using DCdensity command in Stata created by (McCrary 2008) .

H.2 Validity of the RD Design: Balance for Control variables

Another central assumption of the RD design is that, near the threshold, the assignment to treatment should be as good as random for any of the observable characteristics of the district. In our case, this means that whether a far-right representative is elected or not should not depend on district variables around the cutoff. Table A7 provides

Figure A7: McCrary test: Far-right margin of victory (2006-2022)



Notes: Graph produced by the authors using rddensity command in Stata created by Cattaneo et al. (2018).

evidence supporting this assumption, showing that covariates remain balanced around the threshold. Figure A8 shows the Rd plots for these regressions around the threshold.

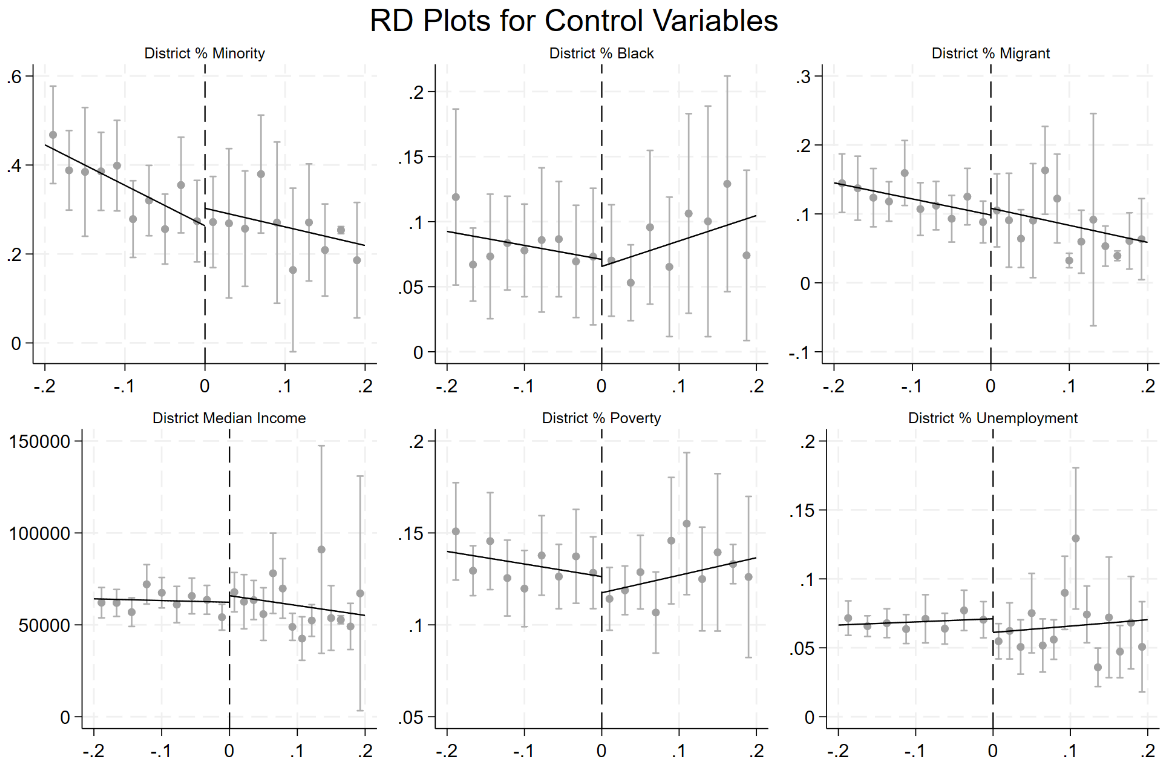
H.2.1 Continuity of Control Variables

Table A7: Election of Far-right Candidates on Control Variables

	% Minorities	% Black	% Migrant	Median Income	% Poverty	% Unemployment
	(1)	(2)	(3)	(4)	(5)	(6)
RD_Estimate	-0.026	-0.014	-0.001	1290.542	-0.013	-0.007
	(0.047)	(0.028)	(0.017)	(2988.370)	(0.010)	(0.005)
Bandwidth	0.166	0.156	0.252	0.185	0.152	0.197
N (Left)	159	148	261	175	142	190
N (Right)	77	76	99	82	75	85
Order_polyn.	1	1	1	1	1	1

Notes: Each column reports RD estimates with robust standard errors in parentheses. All regressions include Year Fixed Effects.
Signif.: ***: 0.01, **: 0.05, *: 0.1.

Figure A8: Regression Discontinuity Plots for covariate variables



Notes: Graph produced by the authors using rdplot command in Stata created by Calonico et al. (2014).

H.3 PCRD: Continuity of Candidates Characteristics

An important implication of our design is that we cannot fully isolate the causal effect of far-right ideology, because far-right candidates who win close races may differ systematically from their opponents on other characteristics. To assess this, we use candidate age, race, and gender. For winning candidates, we obtain this information from the United States House of Representatives Archives. For all candidates in the race, we use the candidate characteristics dataset created by Bellodi et al. (2025). Our assesment is limited to the subset of candidates that appeared in races between 2012 and 2020, which are the years for which we have candidate-level demographic data.

Using this data, we first compare how far-right candidates near the threshold differ from out entire sample of far-right candidates. These comparisons are summarized in Table A8. We do not observe any major changes between the entire sample and the 15-point threshold besides the ratio of men to women. We further explore this gender difference in a second step.

Table A8: Characteristics of Far-Right Candidates: All vs. 15-Point Threshold

	Group	N	Female (%)	Male (%)	Mean age	White (%)	Black (%)	Hispanic (%)	Other (%)
1	15-Point Threshold	267	31.40	68.60	51.80	87.70	5.90	5.90	0.50
2	All	1051	21.40	78.10	52.50	86.40	8.20	5.00	0.10

Second, we explore whether candidate characteristics display discontinuities at the cutoff. Table A9 presents the results. While most characteristics appear continuous, we find a significant discontinuity for gender: far-right women candidates are more likely to win close races compared to their male counterparts. This reinforces our interpretation that the PCRD captures the effect of electing far-right candidates as a bundle of ideology and other characteristics, rather than the effect of ideology alone.

Table A9: Election of Far-right Candidates on Candidates Characteristics

	Candidate White	Candidate Female	Candidate Under 40
	(1)	(2)	(3)
RD_Estimate	-0.017	0.477***	-0.050
	(0.085)	(0.156)	(0.118)
Bandwidth	0.219	0.139	0.223
N (Left)	136	81	40
N (Right)	51	40	51
Order_polyn.	1	1	1

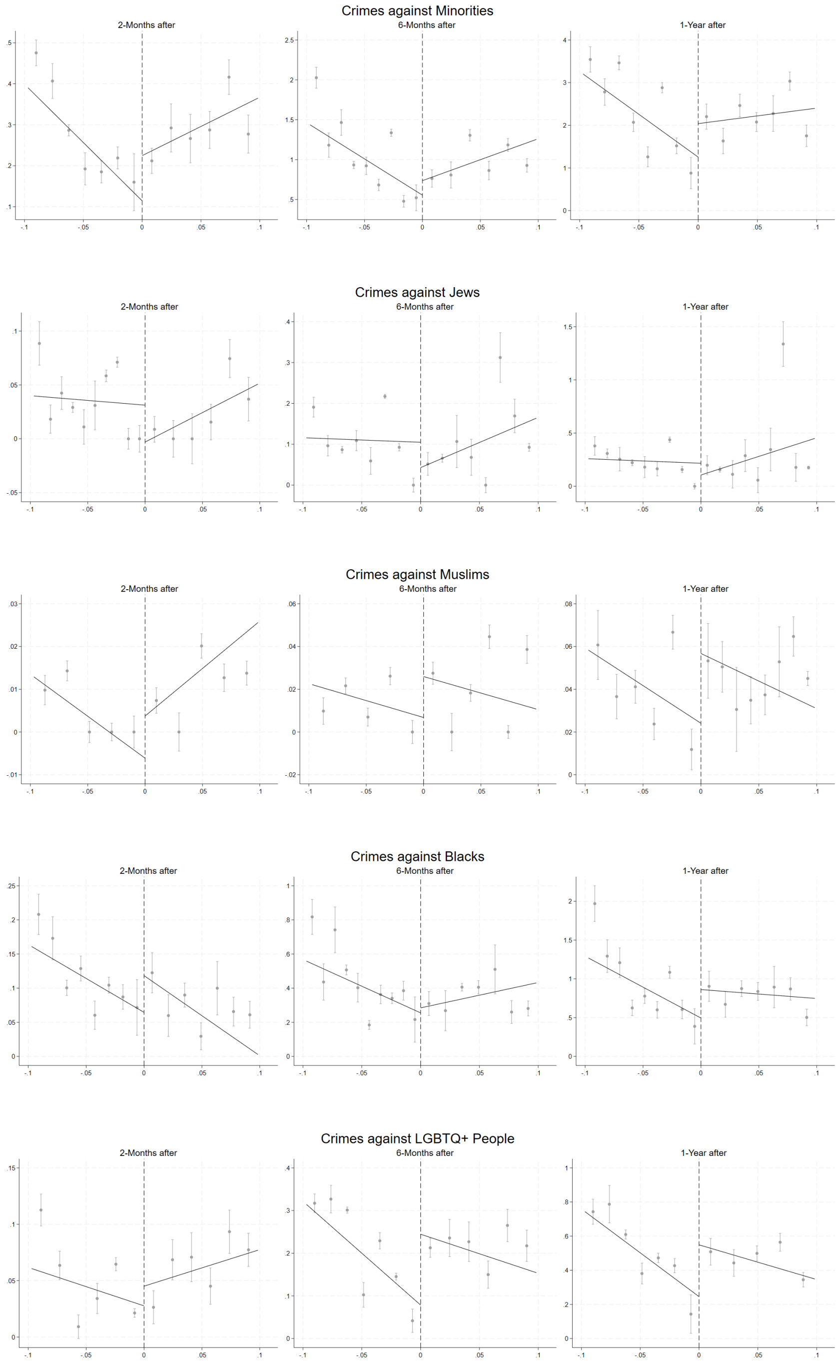
Notes: Each column reports RD estimates with robust standard errors in parentheses. All regressions include Year Fixed Effects.
Signif.: ***: 0.01, **: 0.05, *: 0.1.

I Supplementary Empirical Results

I.1 Hate Crimes Main Results

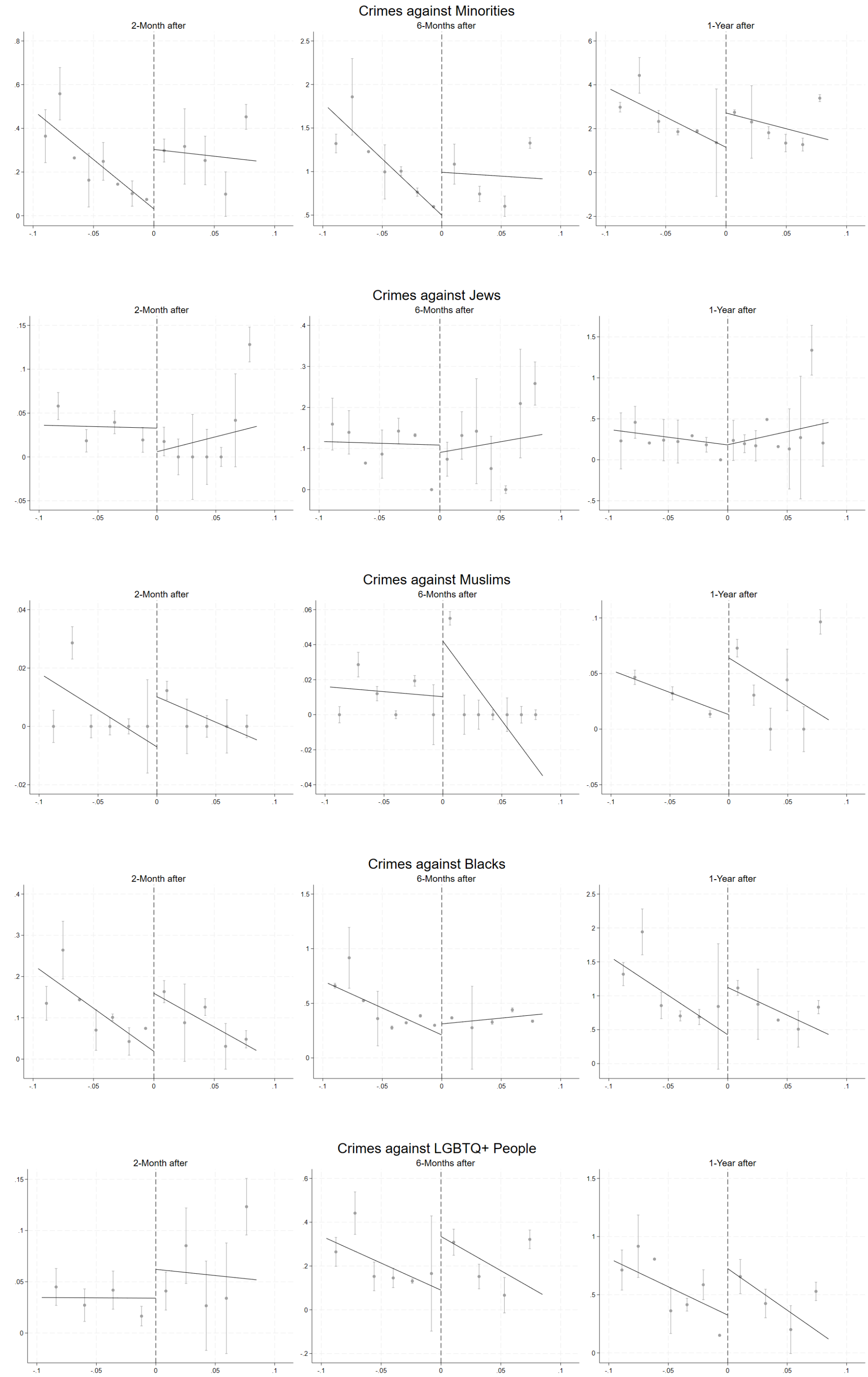
I.1.1 Regression Discontinuity Plots

Figure A9: Regression Discontinuity Plots by Hate Crime: Main outcomes & All Years



Notes: Each point represents the bin sample average of hate crimes reported for the margin of victory. The straight line is a first-order polynomial in Margin of Victory fitted separately on each side of the margin of victory threshold at zero. 90% confidence intervals.

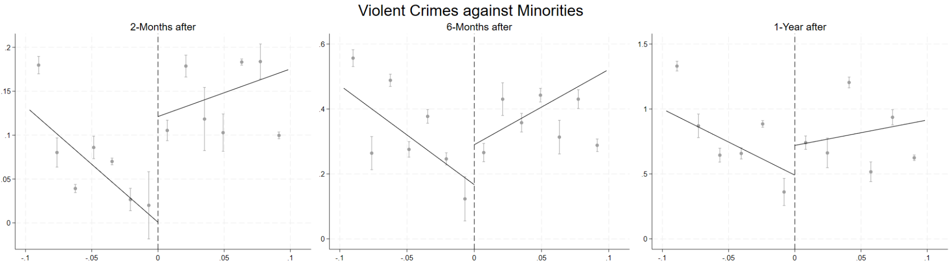
Figure A10: Regression Discontinuity Plots by Hate Crime: Main outcomes & Trump Years

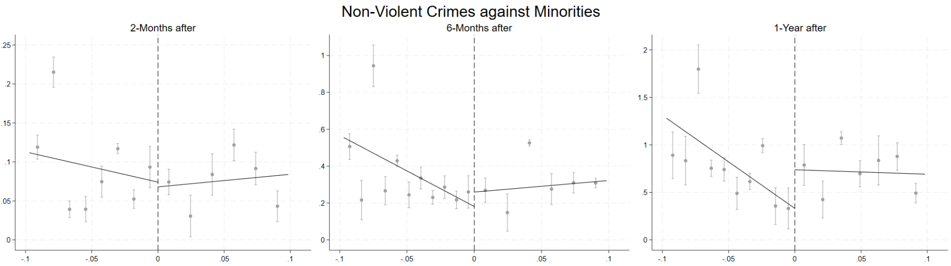


Notes: Each point represents the bin sample average of hate crimes reported for the margin of victory. The straight line is a first-order polynomial in Margin of Victory fitted separately on each side of the margin of victory threshold at zero. 90% confidence intervals.

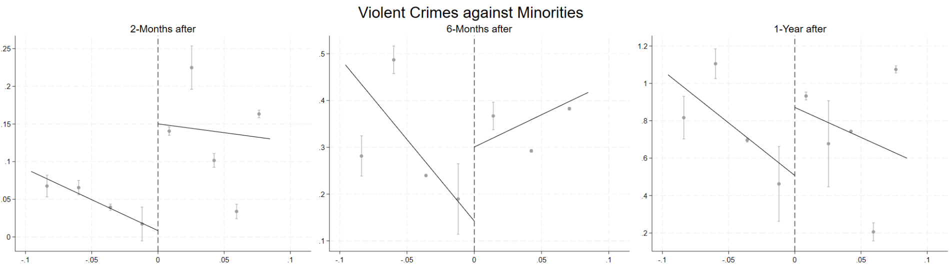
Figure A11: Regression Discontinuity Plots by Hate Crime: Violent & Non-Violent

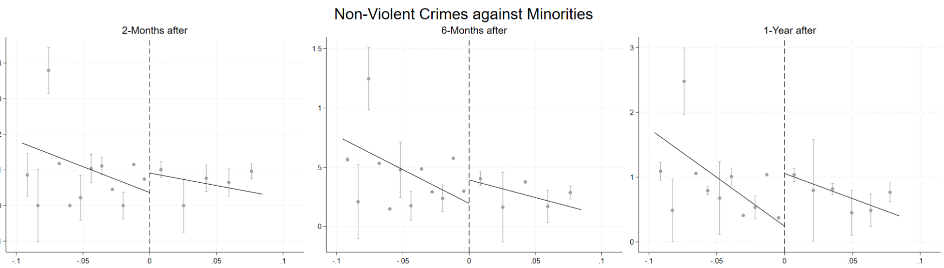
Panel A: All Years





Panel B: Trump Years





Notes: Each point represents the bin sample average of hate crimes reported for the margin of victory. The straight line is a first-order polynomial in Margin of Victory fitted separately on each side of the margin of victory threshold at zero. 90% confidence intervals.

I.1.2 Regression Discontinuity Tables: Minority Subgroups

Table A10: Election of Far-right Candidates on Hate Crimes: Specific Minorities

Panel A: 2 Month Window

	Jews		Muslims		Blacks		LGBTQ+	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
RD Estimate	-0.038***	-0.030***	0.020***	0.017**	-0.013	-0.013	0.011	0.008
	(0.011)	(0.010)	(0.007)	(0.007)	(0.036)	(0.036)	(0.024)	(0.023)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Bandwidth	0.150	0.181	0.136	0.131	0.200	0.205	0.162	0.168
N (Left)	140	172	131	123	195	201	154	159
N (Right)	75	80	69	68	86	88	77	78
Order_polyn.	1	1	1	1	1	1	1	1

Panel B: 6 Months Window

	Jews		Muslims		Blacks		LGBTQ+	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
RD Estimate	-0.058**	-0.053*	0.026*	0.023	-0.095	-0.036	0.090*	0.098**
	(0.029)	(0.029)	(0.015)	(0.015)	(0.084)	(0.079)	(0.052)	(0.049)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Bandwidth	0.146	0.155	0.166	0.157	0.231	0.222	0.182	0.187
N (Left)	139	148	159	148	230	221	174	178
N (Right)	74	76	77	76	93	91	80	83
Order_polyn.	1	1	1	1	1	1	1	1

Panel C: 1 Year Window

	Jews		Muslims		Blacks		LGBTQ+	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
RD Estimate	-0.110*	-0.092	0.040	0.037	0.167	0.223	0.320***	0.253**
	(0.066)	(0.063)	(0.026)	(0.026)	(0.181)	(0.178)	(0.119)	(0.104)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Bandwidth	0.141	0.150	0.155	0.153	0.186	0.193	0.123	0.141
N (Left)	134	140	148	143	176	183	114	134
N (Right)	71	75	76	75	83	84	66	71
Order_polyn.	1	1	1	1	1	1	1	1

Notes: Each panel reports RD estimates with robust standard errors in parentheses. Outcome variables are crimes against minorities over a 100.000 population. All regressions include Year Fixed Effects. Controls include District characteristics (share of minorities, migrants, unemployment and poverty; and district median income).
Signif.: ***: 0.01, **: 0.05, *: 0.1.

Table A11: Election of Far-right Candidates on Hate Crimes: Specific Minorities on Trump Years

Panel A: Trump Years - 2 Month Window

	Jews		Muslims		Blacks		LGBTQ+	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
RD Estimate	-0.030	-0.015	0.012	0.013	0.118**	0.135**	0.042	0.036
	(0.022)	(0.016)	(0.008)	(0.009)	(0.056)	(0.061)	(0.035)	(0.035)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Bandwidth	0.119	0.170	0.203	0.213	0.141	0.124	0.148	0.143
N (Left)	55	86	112	121	73	60	74	73
N (Right)	33	45	50	51	41	38	43	42
Order_polyn.	1	1	1	1	1	1	1	1

Panel B: Trump Years - 6 Months Window

	Jews		Muslims		Blacks		LGBTQ+	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
RD Estimate	0.005	0.017	0.044**	0.039*	-0.043	-0.007	0.162**	0.212***
	(0.045)	(0.040)	(0.020)	(0.020)	(0.126)	(0.117)	(0.079)	(0.078)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Bandwidth	0.125	0.136	0.201	0.159	0.194	0.207	0.173	0.144
N (Left)	61	71	109	80	103	116	89	73
N (Right)	38	39	49	44	48	50	46	42
Order_polyn.	1	1	1	1	1	1	1	1

Panel C: Trump Years - 1 Year Window

	Jews		Muslims		Blacks		LGBTQ+	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
RD Estimate	-0.039	-0.010	0.068**	0.051	0.021	0.240	0.377**	0.412**
	(0.124)	(0.101)	(0.034)	(0.031)	(0.242)	(0.248)	(0.187)	(0.170)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Bandwidth	0.106	0.140	0.166	0.159	0.251	0.213	0.128	0.125
N (Left)	51	72	86	80	148	121	63	60
N (Right)	33	41	44	44	56	51	38	38
Order_polyn.	1	1	1	1	1	1	1	1

Notes: Each panel reports RD estimates with robust standard errors in parentheses. Outcome variables are crimes against minorities over a 100.000 population. All regressions include Year Fixed Effects. Controls include District characteristics (share of minorities, migrants, unemployment and poverty; and district median income).
Signif.: ***: 0.01, **: 0.05, *: 0.1.

Table A12: Election of Far-right Candidates on Hate Crimes: Specific Minorities & Type of Crime

Panel A: 2 Month Window

	Jews		Muslims		Blacks		LGBTQ+	
	Violent	Non-Violent	Violent	Non-Violent	Violent	Non-Violent	Violent	Non-Violent
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
RD Estimate	-0.008	-0.005	0.013**	-0.000	0.039*	-0.026	0.025***	-0.004
	(0.006)	(0.005)	(0.006)	(0.000)	(0.021)	(0.021)	(0.009)	(0.014)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bandwidth	0.151	0.110	0.161	0.091	0.171	0.196	0.161	0.159
N (Left)	141	100	154	80	161	188	153	151
N (Right)	75	60	77	54	78	85	77	76
Order_polyn.	1	1	1	1	1	1	1	1

Panel B: 6 Months Window

	Jews		Muslims		Blacks		LGBTQ+	
	Violent	Non-Violent	Violent	Non-Violent	Violent	Non-Violent	Violent	Non-Violent
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
RD Estimate	-0.023**	-0.002	0.008	0.015	0.062	-0.037	0.048	0.027
	(0.009)	(0.016)	(0.007)	(0.011)	(0.039)	(0.042)	(0.032)	(0.024)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bandwidth	0.137	0.139	0.155	0.208	0.150	0.194	0.178	0.167
N (Left)	131	133	148	204	140	187	171	159
N (Right)	69	70	76	89	75	84	80	78
Order_polyn.	1	1	1	1	1	1	1	1

Panel C: 1 Year Window

	Jews		Muslims		Blacks		LGBTQ+	
	Violent	Non-Violent	Violent	Non-Violent	Violent	Non-Violent	Violent	Non-Violent
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
RD Estimate	-0.048**	-0.021	-0.014	0.030*	0.144**	0.015	0.036	0.112**
	(0.020)	(0.028)	(0.010)	(0.016)	(0.068)	(0.097)	(0.055)	(0.050)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bandwidth	0.112	0.115	0.107	0.167	0.182	0.198	0.172	0.126
N (Left)	101	105	97	159	173	191	165	115
N (Right)	61	62	60	77	80	85	79	68
Order_polyn.	1	1	1	1	1	1	1	1

Notes: Each panel reports RD estimates with robust standard errors in parentheses. Outcome variables are crimes against minorities over a 100.000 population. All regressions include Year Fixed Effects. Controls include District characteristics (share of minorities, migrants, unemployment and poverty; and district median income).
Signif.: ***: 0.01, **: 0.05, *: 0.1.

I.2 Hate Crimes Robustness Checks

I.2.1 Regression Discontinuity: Minimum Detectable Effect (MDE)

The minimum detectable effect (MDE) values reported below are ex-post calculations that summarize the smallest true effects our design would detect with a given probability under repeated sampling, given the realized sample size and variance (Bloom 1995, Duflo et al. 2007). The MDE is therefore an ex-ante benchmark applied ex-post, used to assess the informativeness of null results rather than to constrain which realized effects may be statistically significant. Significant estimates smaller than the MDE can occur when the realized sampling variance is lower than expected. The substantive limitation arises only for non-significant estimates below the MDE, since in those cases the design lacks sufficient power to rule out effects of meaningful size.

Table A13: Power Analysis - Main Models

	All Years			Trump Years		
	2-months	6-months	1-year	2-months	6-months	1-year
	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	0.054	0.137	0.610*	0.180**	0.479**	1.193***
	(0.061)	(0.156)	(0.360)	(0.075)	(0.220)	(0.435)
MDE	.196	.502	1.154	.238	.743	1.389
N (Left)	204	171	172	121	71	80
N (Right)	89	80	80	51	38	44
Bandwidth	.208	.178	.182	.213	.134	.159

Notes: Each model reports our main RD estimates with robust standard errors in parentheses. Outcome variables are crimes against minorities over a 100.000 population. All regressions include Year Fixed Effects and controls. These are District characteristics (share of minorities, migrants, unemployment and poverty; and district median income). Minimum Detectable Effects (MDE) are calculated using stata command rdmde by Cattaneo et al. (2019) using a significance level α equal to 5% and a desired power β of 0.80
Signif.: ***: 0.01, **: 0.05, *: 0.1.

I.2.2 Regression Discontinuity Tables: Polynomial order 2

Table A14: Election of Far-right Candidates on Hate Crimes: Polynomial order 2

Panel A: 2 Month Window

	Minorities		Jews		Muslims		Blacks		LGBTQ+	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
RD Estimate	0.118	0.109	-0.037**	-0.039**	0.021**	0.021**	0.087	0.083	0.014	0.010
	(0.083)	(0.082)	(0.016)	(0.016)	(0.009)	(0.009)	(0.056)	(0.056)	(0.029)	(0.029)
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Bandwidth	0.224	0.222	0.183	0.187	0.201	0.204	0.190	0.180	0.211	0.214
N (Left)	223	221	175	177	195	200	181	171	210	213
N (Right)	92	91	81	83	86	88	83	80	89	89
Order_polyn.	2	2	2	2	2	2	2	2	2	2

Panel B: 6 Months Window

	Minorities		Jews		Muslims		Blacks		LGBTQ+	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
RD Estimate	0.159	0.209	-0.077*	-0.078*	0.033*	0.034*	0.103	0.021	0.185***	0.167**
	(0.197)	(0.210)	(0.040)	(0.041)	(0.018)	(0.018)	(0.119)	(0.097)	(0.072)	(0.069)
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Bandwidth	0.249	0.205	0.168	0.160	0.293	0.283	0.214	0.275	0.192	0.192
N (Left)	255	200	159	151	330	313	213	298	182	182
N (Right)	97	88	78	77	109	105	89	104	83	83
Order_polyn.	2	2	2	2	2	2	2	2	2	2

Panel C: 1 Year Window

	Minorities		Jews		Muslims		Blacks		LGBTQ+	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
RD Estimate	1.036**	0.907*	-0.147	-0.178*	0.054*	0.053*	0.167	0.458**	0.333**	0.333**
	(0.506)	(0.487)	(0.103)	(0.101)	(0.031)	(0.031)	(0.204)	(0.232)	(0.131)	(0.130)
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Bandwidth	0.195	0.202	0.188	0.177	0.263	0.255	0.301	0.230	0.222	0.206
N (Left)	188	197	180	171	279	264	344	227	221	202
N (Right)	85	86	83	80	101	100	110	93	91	88
Order_polyn.	2	2	2	2	2	2	2	2	2	2

Notes: Each panel reports RD estimates with robust standard errors in parentheses. Outcome variables are crimes against minorities over a 100.000 population. All regressions include Year Fixed Effects. Controls include District characteristics (share of minorities, migrants, unemployment and poverty; and district median income).
Signif.: ***: 0.01, **: 0.05, *: 0.1.

Table A15: Election of Far-right Candidates on Hate Crimes: Polynomial order 2 & Trump Years

Panel A: 2 Month Window

	Minorities		Jews		Muslims		Blacks		LGBTQ+	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
RD Estimate	0.340***	0.336***	-0.023	-0.024	0.032**	0.027**	0.197**	0.181**	0.040	0.044
	(0.124)	(0.121)	(0.026)	(0.025)	(0.014)	(0.014)	(0.080)	(0.079)	(0.047)	(0.047)
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Bandwidth	0.172	0.177	0.188	0.197	0.213	0.234	0.175	0.172	0.200	0.202
N (Left)	87	93	98	105	121	134	93	88	109	112
N (Right)	46	46	48	49	51	53	46	46	49	49
Order_polyn.	2	2	2	2	2	2	2	2	2	2

Panel B: 6 Months Window

	Minorities		Jews		Muslims		Blacks		LGBTQ+	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
RD Estimate	0.702*	0.646*	-0.003	-0.043	0.053*	0.058**	0.259	0.205	0.190**	0.260***
	(0.375)	(0.343)	(0.058)	(0.055)	(0.030)	(0.025)	(0.211)	(0.189)	(0.094)	(0.094)
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Bandwidth	0.173	0.184	0.189	0.158	0.211	0.277	0.179	0.193	0.267	0.241
N (Left)	89	95	99	80	118	171	93	101	163	139
N (Right)	46	47	48	44	51	59	46	48	57	54
Order_polyn.	2	2	2	2	2	2	2	2	2	2

Panel C: 1 Year Window

	Minorities		Jews		Muslims		Blacks		LGBTQ+	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
RD Estimate	1.648***	1.814**	-0.023	-0.138	0.094**	0.079*	0.943**	0.830**	0.489**	0.455**
	(0.588)	(0.736)	(0.158)	(0.161)	(0.047)	(0.042)	(0.410)	(0.360)	(0.224)	(0.201)
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Bandwidth	0.224	0.181	0.185	0.166	0.217	0.226	0.193	0.214	0.203	0.221
N (Left)	126	94	95	86	122	128	100	121	112	124
N (Right)	52	46	48	44	51	52	48	51	50	52
Order_polyn.	2	2	2	2	2	2	2	2	2	2

Notes: Each panel reports RD estimates with robust standard errors in parentheses. Outcome variables are crimes against minorities over a 100.000 population. All regressions include Year Fixed Effects. Controls include District characteristics (share of minorities, migrants, unemployment and poverty; and district median income).
Signif.: ***: 0.01, **: 0.05, *: 0.1.

Table A16: Election of Far-right Candidates on Hate Crimes: Polynomial order 2 & Violent Crimes

Panel A: 2 Month Window

	Minorities		Jews		Muslims		Blacks		LGBTQ+	
	Violent	Non-Violent	Violent	Non-Violent	Violent	Non-Violent	Violent	Non-Violent	Violent	Non-Violent
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
RD Estimate	0.108***	0.001	-0.008	-0.006	0.011*	0.000	0.057**	-0.018	0.021*	0.001
	(0.034)	(0.046)	(0.008)	(0.006)	(0.006)	(0.003)	(0.027)	(0.027)	(0.012)	(0.017)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bandwidth	0.264	0.215	0.182	0.175	0.220	0.212	0.198	0.284	0.181	0.206
N (Left)	281	213	173	170	219	212	191	314	172	201
N (Right)	101	89	80	80	91	89	85	106	80	88
Order_polyn.	2	2	2	2	2	2	2	2	2	2

Panel B: 6 Months Window

	Minorities		Jews		Muslims		Blacks		LGBTQ+	
	Violent	Non-Violent	Violent	Non-Violent	Violent	Non-Violent	Violent	Non-Violent	Violent	Non-Violent
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
RD Estimate	0.091	0.104	-0.031**	0.000	0.004	0.011	0.070	-0.048	0.051	0.057**
	(0.086)	(0.104)	(0.013)	(0.020)	(0.008)	(0.016)	(0.046)	(0.048)	(0.040)	(0.029)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bandwidth	0.205	0.221	0.159	0.191	0.207	0.244	0.237	0.318	0.191	0.245
N (Left)	200	221	151	182	203	249	239	364	182	249
N (Right)	88	91	76	83	88	96	94	113	83	96
Order_polyn.	2	2	2	2	2	2	2	2	2	2

Panel C: 1 Year Window

	Minorities		Jews		Muslims		Blacks		LGBTQ+	
	Violent	Non-Violent	Violent	Non-Violent	Violent	Non-Violent	Violent	Non-Violent	Violent	Non-Violent
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
RD Estimate	0.205	0.489*	-0.062**	-0.024	-0.013	0.038**	0.149*	0.209	0.062	0.138**
	(0.174)	(0.255)	(0.025)	(0.034)	(0.012)	(0.019)	(0.077)	(0.132)	(0.067)	(0.056)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bandwidth	0.206	0.209	0.176	0.165	0.179	0.259	0.302	0.214	0.205	0.223
N (Left)	202	205	170	159	171	270	344	213	200	222
N (Right)	88	89	80	77	80	100	110	89	88	92
Order_polyn.	2	2	2	2	2	2	2	2	2	2

Notes: Each panel reports RD estimates with robust standard errors in parentheses. Outcome variables are crimes against minorities over a 100.000 population. All regressions include Year Fixed Effects. Controls include District characteristics (share of minorities, migrants, unemployment and poverty; and district median income).
Signif.: ***: 0.01, **: 0.05, *: 0.1.

I.2.3 Regression Discontinuity Tables: Property Crimes

Table A17: Election of Far-right Candidates on Hate Crimes against Minorities: Property Crimes

Panel A: 2 Months Window

	Property Crimes Against Minorities			
	All Years		Trump Years	
	(1)	(2)	(3)	(4)
RD Estimate	-0.024	-0.019	0.082*	0.062*
	(0.031)	(0.032)	(0.042)	(0.036)
Controls	No	Yes	No	Yes
Bandwidth	0.207	0.200	0.136	0.168
N (Left)	204	195	71	86
N (Right)	89	86	39	45
Order_polyn.	1	1	1	1

Panel B: 6 Months Window

	Property Crimes Against Minorities			
	All Years		Trump Years	
	(1)	(2)	(3)	(4)
RD Estimate	-0.016	0.012	0.145	0.172*
	(0.069)	(0.065)	(0.112)	(0.092)
Controls	No	Yes	No	Yes
Bandwidth	0.173	0.178	0.122	0.131
N (Left)	166	171	59	66
N (Right)	79	80	35	38
Order_polyn.	1	1	1	1

Panel C: 1 Year Window

	Property Crimes Against Minorities			
	All Years		Trump Years	
	(1)	(2)	(3)	(4)
RD Estimate	0.147	0.191	0.410*	0.385*
	(0.143)	(0.134)	(0.236)	(0.203)
Controls	No	Yes	No	Yes
Bandwidth	0.176	0.184	0.119	0.128
N (Left)	170	175	56	64
N (Right)	80	81	33	38
Order_polyn.	1	1	1	1

Notes: Each panel reports RD estimates with robust standard errors in parentheses. Outcome variables are crimes against minorities over a 100.000 population. All regressions include Year Fixed Effects. Controls include District characteristics (share of minorities, migrants, unemployment and poverty; and district median income).
Signif.: ***: 0.01, **: 0.05, *: 0.1.

I.2.4 Regression Discontinuity Tables: Other Far-right Definitions

To assess whether our findings are sensitive to the way we classify far-right candidates, we re-estimate the main RD models using three alternative thresholds. The first defines far-right candidates as those above the median CF score among all members of the Freedom and Tea Party Caucuses. The second uses only the median CF score of the Freedom Caucus. The third defines far-right candidates as those above the seventy-fifth percentile of CF scores among all Republican candidates. Table A18 reports the results for these alternative definitions across the two months, six months, and one year windows. The estimates closely resemble those obtained with our main definition of far-right candidacies, with a similar temporal pattern in which effects are concentrated in the Trump years and emerge most clearly in the one year window. These results indicate that our conclusions are not driven by the specific choice of CF score threshold.

Table A18: Election of Far-right Candidates on Hate Crimes: Main outcome and other Far-right Definitions

Panel A: 2 Month Window

	Minority - F.R. All Caucus				Minority - F.R. Freedom Caucus				Minority - F.R. Republicans (75p)			
	All Years		Trump Years		All Years		Trump Years		All Years		Trump Years	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
RD Estimate	0.033	0.054	0.214***	0.180**	0.107*	0.121*	0.254***	0.259***	0.061	0.074	0.258***	0.272***
	(0.060)	(0.061)	(0.081)	(0.075)	(0.065)	(0.064)	(0.086)	(0.081)	(0.066)	(0.062)	(0.088)	(0.085)
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Bandwidth	0.227	0.208	0.158	0.213	0.205	0.205	0.137	0.141	0.204	0.228	0.138	0.137
N (Left)	225	204	79	121	168	168	61	61	165	188	60	60
N (Right)	92	89	44	51	69	69	36	36	66	70	35	35
Order_polyn.	1	1	1	1	1	1	1	1	1	1	1	1

Panel B: 6 Months Window

	Minority - F.R. All Caucus				Minority - F.R. Freedom Caucus				Minority - F.R. Republicans (75p)			
	All Years		Trump Years		All Years		Trump Years		All Years		Trump Years	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
RD Estimate	0.055	0.137	0.389	0.479**	0.199	0.261	0.477*	0.510**	0.082	0.146	0.446*	0.529**
	(0.164)	(0.156)	(0.243)	(0.220)	(0.181)	(0.176)	(0.259)	(0.237)	(0.186)	(0.179)	(0.257)	(0.233)
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Bandwidth	0.182	0.178	0.136	0.134	0.188	0.186	0.135	0.134	0.183	0.182	0.142	0.140
N (Left)	174	171	71	71	151	148	60	60	143	143	61	60
N (Right)	80	80	39	38	67	67	34	34	62	62	35	35
Order_polyn.	1	1	1	1	1	1	1	1	1	1	1	1

Panel C: 1 Year Window

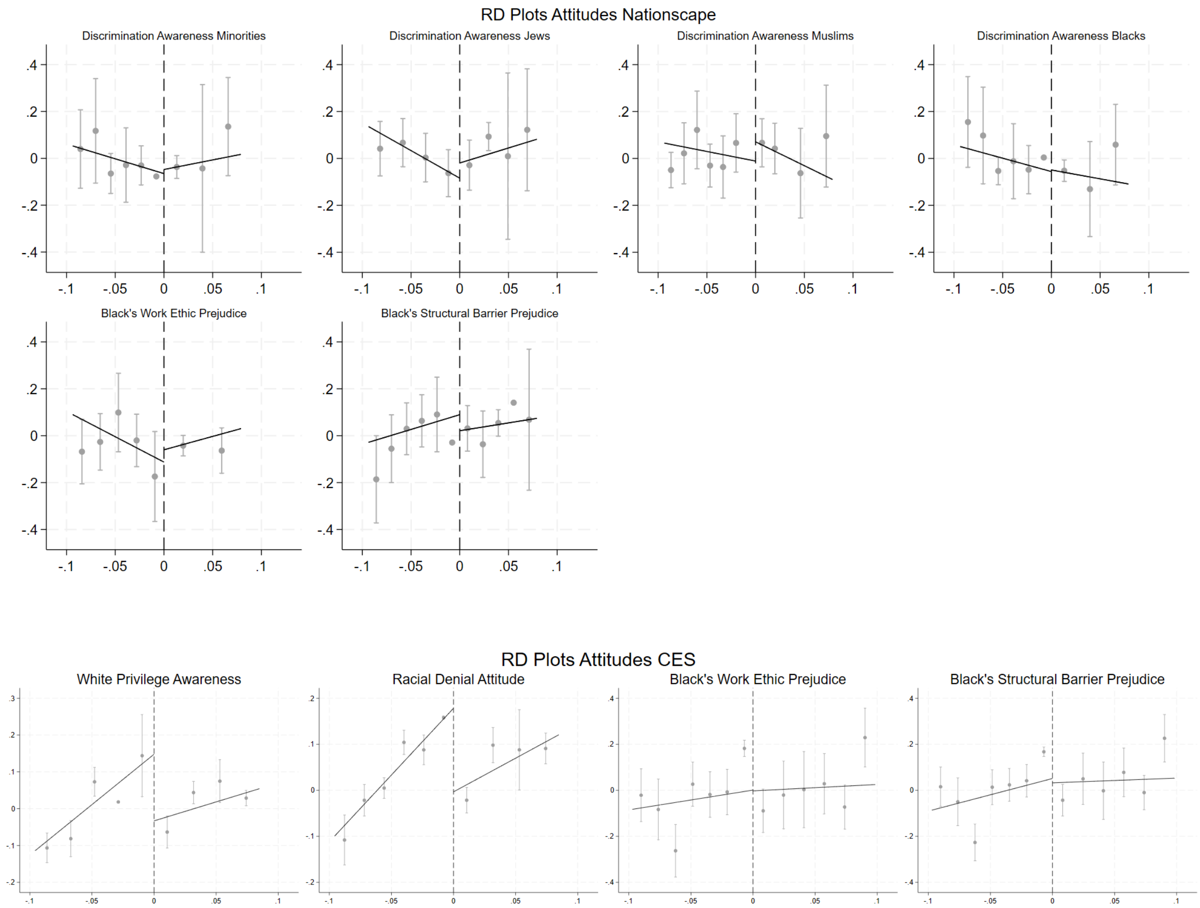
	Minority - F.R. All Caucus				Minority - F.R. Freedom Caucus				Minority - F.R. Republicans (75p)			
	All Years		Trump Years		All Years		Trump Years		All Years		Trump Years	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
RD Estimate	0.444	0.610*	1.368***	1.193***	0.722*	0.746*	1.064**	1.381***	0.446	0.517	0.899*	1.180**
	(0.369)	(0.360)	(0.525)	(0.435)	(0.427)	(0.426)	(0.511)	(0.497)	(0.432)	(0.429)	(0.537)	(0.507)
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Bandwidth	0.191	0.182	0.129	0.159	0.171	0.174	0.177	0.136	0.181	0.189	0.174	0.169
N (Left)	182	172	64	80	137	143	81	61	141	149	78	73
N (Right)	83	80	38	44	64	65	40	35	62	64	39	39
Order_polyn.	1	1	1	1	1	1	1	1	1	1	1	1

Notes: Each panel reports RD estimates with robust standard errors in parentheses. Outcome variables are Crimes against minorities over a 100.000 population. All regressions include Year Fixed Effects. Controls include District characteristics (share of minorities, migrants, unemployment and poverty; and district median income). F.R. All Caucus defines far-right as those above the median CF Score from the Freedom and Tea-Party Caucuses. F.R. Freedom Caucus defines far-right as those above the median CF Score from the Freedom Caucus alone. F.R. Republicans (75p) defines far-right as those above the 75th percentile CF Score of all Republicans in Congress.
Signif.: ***: 0.01, **: 0.05, *: 0.1.

I.3 Attitudinal Outcomes

I.3.1 Regression Discontinuity Plots

Figure A12: Regression Discontinuity Plots for attitude variables



Notes: Each point represents the bin sample average of hate crimes reported for margin of victory. The straight line is a first-order polynomial in Margin of Victory fitted separately on each side of the margin of victory threshold at zero. 95% confidence intervals.

I.3.2 Other Attitude Outcomes: Nationscape

Table A19: Election of Far-right Candidates on Attitudes: Other outcomes Nationscape

	Att. Minorities		Att. Jews		Att. Muslims		Att. Black		Att. LGBTQ+	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
RD Estimate	0.023	-0.027	-0.059	-0.010	0.075	0.041	-0.022	-0.061	0.052	0.017
	(0.079)	(0.080)	(0.090)	(0.080)	(0.076)	(0.071)	(0.065)	(0.066)	(0.091)	(0.082)
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Bandwidth	0.261	0.174	0.302	0.157	0.151	0.145	0.214	0.178	0.150	0.130
N (Left)	68	45	80	38	38	38	57	45	38	35
N (Right)	21	20	21	20	20	20	21	20	20	17
Order_polyn.	1	1	1	1	1	1	1	1	1	1

Notes: Each panel reports RD estimates with robust standard errors in parentheses. Outcome variables are standardized and correspond to respondents' favorability towards these minority groups. Controls include District characteristics (share of minorities, migrants, unemployment, poverty, and district median income).
Signif.: ***: 0.01, **: 0.05, *: 0.1.

I.3.3 Regression Discontinuity Tables (Attitudes): Polynomial order 2

Table A20: Election of Far-right Candidates on Attitudes p(2): Nationscape & CES

Panel A: Nationscape

	Disc. Minorities		Disc. Jews		Disc. Muslims		Disc. Blacks		Black's Work Ethic		Black's Structural Barrier	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
RD Estimate	-0.047	0.085	0.143	0.041	-0.012	0.086	-0.072	0.119	0.113	0.011	-0.037	-0.055
	(0.086)	(0.085)	(0.108)	(0.090)	(0.092)	(0.093)	(0.066)	(0.077)	(0.123)	(0.131)	(0.083)	(0.068)
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Bandwidth	0.190	0.167	0.161	0.216	0.211	0.178	0.240	0.136	0.183	0.162	0.202	0.190
N (Left)	48	40	38	58	55	45	63	38	46	38	52	48
N (Right)	20	20	20	21	21	20	21	18	20	20	20	20
Order_polyn.	2	2	2	2	2	2	2	2	2	2	2	2

Panel B: CES

	White Privilege		Racial Denial		Black's Work Ethic		Black's Structural Barrier	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
RD Estimate	-0.211***	-0.209***	-0.202***	-0.234***	0.020	0.029	0.004	-0.019
	(0.054)	(0.050)	(0.070)	(0.072)	(0.053)	(0.050)	(0.044)	(0.037)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Bandwidth	0.224	0.179	0.205	0.177	0.263	0.224	0.286	0.316
N (Left)	126	93	113	93	227	182	255	283
N (Right)	52	46	50	46	85	78	89	96
Order_polyn.	2	2	2	2	2	2	2	2

Notes: Each panel reports RD estimates with robust standard errors in parentheses. Outcome variables are standardized. Controls include District characteristics (share of minorities, migrants, unemployment, poverty, and district median income).
Signif.: ***: 0.01, **: 0.05, *: 0.1.