Voted In, Standing Out: Public Response to Immigrants' Political Accession

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Abstract

What is the reaction of the host society to immigrants' political integration? We argue that when they win political office, immigrants pose a threat to natives' dominant position, triggering hostility from a violent-prone fringe, the mass public and the elites. We test these dynamics across UK general elections, using hate crime police records, public opinion data, and text data from over 500,000 regional and local newspaper articles. We identify the public's reactions with a regression discontinuity design of close elections between minority-immigrant and dominant group candidates. Our findings suggest a public backlash against ethnic minority immigrants' integration into majority settings.

Key words: Immigrant Integration, Representation of Immigrant-Origin Ethnic Minorities, Hate Crime, Exclusionary Attitudes, Media Portrayals of Immigrants

JEL Codes: J11, J15, Z13

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1 Introduction

What is the response of dominant-group natives to ethnic minorities' success at integration in political institutions?[1] It has long been argued that the political and economic ascendance of a minority group can trigger a hostile, and at times violent, backlash from members of the majority group concerned by a real (Bobo, 1983) or perceived (Blumer, 1958) threat to the status quo. Hostility against minority groups can occur in response to structural social changes like population shifts (Blau, 1977) and economic restructuring (Autor et al., 2020), as well as the increasing political power of previously disenfranchised groups (Van Dyke and Soule, 2002). Such hostility, expressed as violence or exclusionary attitudes, is in part reactionary and can be a means of reasserting social control (King and Brustein, 2006).

The successful integration of immigrant-origin ethnic minorities in political institutions, in particular, is expected to be perceived as posing a challenge to the existing power and social position of dominant-group natives (Dancygier, 2010). Using parliamentary elections in the UK, in this study we explore the response of majority group members to ethnic minority immigrants winning political power through the ballot box.

We build on two related theories with similar observable implications for hostility against immigrant minorities. First, power threat theory explains intergroup conflict as a clash over valued scarce goods, including claims to social status and privilege (e.g. Blalock, 1967). Conflict in power threat theory is rooted in social-structural sources of group difference. Elections between ethnic minority and dominant-group candidates, in this context, are thus a clash over economic, political and social resources. Losing to an ethnic minority candidate is a threat to dominant-group natives' control over those resources.

Second, social identity theory maintains that individuals' sense of who they are is based on what groups they belong to. Because people strive for positive self-worth, when the groups that are the basis of their identity are negatively evaluated, people attempt to make their group more positively distinct (Tajfel and Turner, 1979). Hostility, in social identity theory, is thus based on group categorization

[1]We use the term *immigrant* to refer to both first- and later-generation immigrants. We use it interchangeably with the term *ethnic minority*, as many of the numerically sizable minority groups in rich democracies have immigrant origins.

and social status differentiation. Elections, in this context, inherently entail a social comparison. Dominant-group natives' equate an electoral loss to a threat against their social identity, and in turn their self-worth.

Both theories imply that minority political victories trigger threats against the dominant group (either rooted in concerns about resources or about identity and social status). Both theories assume that conflict results from dominant group desires to suppress such threats. Below we argue, in addition, that close elections heighten such threats, reinforcing the hostile response to a minority victory. While our research design does not permit us to discriminate between power threat and social identity theories, the close correspondence we find between hostility against immigrant-minorities and minority electoral victories underscores the political role of threat and of dominant group efforts to counter it.

Our study addresses two limitations of existing studies that contend that when marginalized groups gain political power, the dominant group responds with greater hostility. First, past work has had a hard time establishing a causal relationship between minorities' political power gains and dominant group backlash. Proxy measures commonly used in existing work such as the size of the minority group (Quillian, 1995) do not necessarily capture the group's political power, even as they indirectly measure competition for scarce resources. Studies that capture minorities' political power with direct measures such as the ratio of minority-to-majority votes in recent elections (D'Alessio, Stolzenberg and Eitle, 2002), the share of legislatures that are members of the minority group (Van Dyke and Soule, 2002), and whether a minority holds mayoral office (Jacobs and Wood, 1999) are vulnerable to identification concerns. Particularly, from omitted variable bias—by failing to account for unobserved relevant characteristics that determine both minority political power and anti-immigrant attitudes and behavior—and from problems of reverse causality. Consistent with our theoretical framework, we address these concerns by using a regression discontinuity design of close parliamentary elections, comparing constituencies where a minority MP candidate narrowly wins versus constituencies where a minority candidate narrowly loses.

The second limitation of existing work is its ambiguity with respect to which members of the majority group respond negatively to gains in minority political power. Much of the literature focuses on violent backlash in the form of hate crimes (Dugan and Chenoweth, 2020), lynching (Hovland and Sears, 1940), and

inter-racial killings (D'Alessio, Stolzenberg and Eitle, 2002). However, such crimes are usually perpetrated by the more extreme members of society, and thus a focus on extreme forms of violence leaves open the question of whether such negative behavior and attitudes are more widely shared among majority group members. And while admittedly some studies have established a correlation between minority political power gains and mass public opinion (e.g., Quillian, 1996), it is unclear whether the conditions that trigger violent responses are ones that also trigger less extreme (e.g., attitudinal) responses.

We address this concern by exploring—within the context of a single (electoral) event—outcomes at three different societal levels. Specifically, we examine: (a) *hate crimes* (which is a behavior at the tail of the societal distribution); (b) *attitudes toward migrants* (which captures mass public opinion), and (c) *media tone towards migrant groups* (which reflects the attitudes and behavior of elites).

Leveraging close results in the 2010 to 2019 UK general election between minority-immigrant and dominant-group candidates, we find that a minority immigrant victory triggered a backlash against their communities. In constituencies where ethnic minority candidates narrowly win a parliamentary seat, compared to where they narrowly lose, the subsequent hate crime rate is significantly higher. Three months after the election we identify an effect of 0.88 standard deviations, which corresponds to a 68% increase in hate crime relative to the average hate crime rate in constituencies where minority candidates narrowly lose. Similarly, we find that narrowly winning a parliamentary seat affected mass attitudes toward immigrants, significantly increasing the share of survey respondents who state that too many immigrants have been let into the UK. The size of the effect corresponds to a 66% decrease in inclusionary attitudes (or 0.65 standard deviations). Finally, we assess elites' response by analyzing text from over 500,000 newspaper articles that we matched to the ethnic background of the candidates and to their constituencies. Using natural language processing techniques, we then compute a measure of negative speech about a candidate's ethnic group. Three months after the election, we find a difference of 20 percentage points (or 0.66 standard deviations) between the proportion of negative mentions about the narrowly winners' and the narrowly losers' ethnic group.

Our paper contributes to the literature on intergroup conflict that is rooted in Blalock's 1967 original conceptualization of power threat theory. While the *correlation* between gains to minority political power and majority group hostility is

well-established, we provide instead causal estimates of such a potential backlash. In addition, we show within the same case that the backlash against political gains by a minority group is not limited to a violent-prone fringe but is also observed among the mass public and the elites.

Moreover, consistent with threat-triggering concerns, subgroup analyses suggest that the hostile response is concentrated on candidates who pose greater threat to the status quo—that is, candidates standing with left-leaning parties (who are more likely to advance pluralistic and redistributive policies challenging social hierarchies). And it manifests itself in particular in the right-wing media, which arguably represents a segment of the dominant group that is most threatened by pluralistic values and economic redistribution.

We also contribute to a growing literature on the determinants of hate crimes that target minority groups. Specifically, we enrich work on situational trigger events, which hitherto focused on reactions to unexpected shocks (Dipoppa, Grossman and Zonszein, 2022), and to minorities perpetrating terror attacks (Deloughery, King and Asal, 2012) and serious felonies (Jäckle and König, 2018). We show that minority candidates winning parliamentary elections can similarly trigger a violent response, arguably because political ascendance of minorities heightens among members of the dominant group a sense of threat to the hierarchical status quo.

Only a handful of studies assess variation in immigration media coverage explained by immigrant-related events. These are mostly concerned with terrorist attacks perpetrated by members of minority groups, and focus on coverage by the national media (e.g., Vliegenthart, Boomgaarden and Boumans, 2011). Here, we assess whether and how the successful integration of migrant groups to their host political institutions affects the media coverage of migrants. In so doing, we contribute to the political communication literature by showing that the electoral success of a migrant group changes the media coverage of that group in terms of both attention and valence.

2 Minority accession and intergroup hostility

We argue that dominant-group natives respond with hostility to threats triggered by ethnic minority electoral victories. We situate this argument within the theoretical framework of power threat and social identity theory.

Power threat theory

A central tenet of minority-group threat theories is that real or perceived intergroup competition for scarce resources provokes hostility. Group conflict involves not only objective conditions of competition between members of different groups but also the subjective perception that outgroup members pose a threat to the dominant group's valued resources, norms and traditions.

Minority-group threat theory provided the theoretical foundation for a long-standing body of research on inter-racial/ethnic relations. Here, minorities occupying spaces traditionally dominated by an ethnic majority group are perceived to pose a threat to the majority's social, political, and economic resources. Empirically, this body of work consistently finds a positive relationship between the relative size of a minority population and hostility against that group. As the relative size of the minority population increases, the threat increases as well by, for example, heightening interethnic competition for scarce resources like jobs, housing, education, and health care (an economic and social threat) (Hardin, 1995), or by increasing the potential for minority political mobilization (a political threat) (Blalock, 1967). As such threats increase, the willingness of (some) dominant group members to allow minorities in 'their' spaces decreases, and derogation of minorities increases.

Evidence of such a dynamic has been provided in the context of rich democracies by relating the relative size (change, or rank) of a minority population to racial inequality (Wilcox and Roof, 1978), hate crimes against minorities (Cikara, Fouka and Tabellini, 2021), white's attitudes toward racial segregation (Wilson, 1978), and anti-immigrant prejudicial attitudes (Quillian, 1995). Aside from minority-group threat theories based on competition for material resources, the hostile response to changing ethnic demographics in rich democracies has also been explained as a reaction to a cultural identity threat (Alesina and Tabellini, 2022).

Besides the relative size of a minority group, other threat-triggering conditions have been discussed in the literature. Blalock (1967)'s central link between inter-group conflict and competition for scarce resources has naturally been extended to economic conditions. Studies find that the worsening of economic circumstances among dominant group members (Sharma, 2015), like job loss (Van Dyke and Soule, 2002) and unemployment (Mayda, 2006), triggers hostility against minorities.

A handful of studies addresses minority-group threat theory specifically from its political threat component. D'Alessio, Stolzenberg and Eitle (2002), for example,

use the ratio of black-to-white votes cast in South Carolina to measure threat to the dominant group's political status. Jacobs and Wood (1999) find a significant relationship between the presence of black US mayors and white killings of blacks. In discussing ethnic conflict, Hardin (1995) conceptualizes public office as a positional good which is fundamental in the allocation of distributional goods like welfare benefits. Relatedly, Dancygier (2010) explains immigrant-native conflict in the UK as arising from immigrants' costly material demands to which political parties are responsive. Natives attack immigrant-origin minorities when their presence arguably reduces natives' material welfare. As such, Dancygier (2010) finds a positive association between the share of minorities in local councils and anti-immigrant hostility, but only under conditions of economic scarcity.

Social identity theory

According to social identity theory, individuals form their self-identity and define their interests based on group membership, and they evaluate their own group by comparing its attributes to those of other (salient) groups (Tajfel and Turner, 1979; Shayo, 2009). Importantly for the context we study here, an outgroup becomes a target of comparison when circumstances, like elections, make that outgroup more salient. When a comparison against an outgroup is unsatisfactory, people attempt to make their group more positively distinct. The goal of such a strive for differentiation is to maintain or achieve social superiority, and by extension, a more positive self-worth. As such, the process of group differentiation is essentially competitive. Insofar as social differentiation rests on comparisons related to status, social competition is expected to be linked to intergroup hostility, as individuals exert effort to change their group's social position.

Election results as threat

Building on power threat theory, we argue that elections between ethnic minority and dominant-group candidates establish the arena for intergroup competition for a diverse set of valuable resources. Losing an election to a minority candidate, in turn, poses a threat to dominant-group natives' access to such resources. Building on social identity theory, we argue that elections between ethnic minority and dominant-group candidates trigger intergroup competition for social status. By differentiating

winners from losers, elections are inherently a social comparison process. Intergroup conflict results from such a process, as it rests on a real or perceived threat to the dominant group's status. An electoral loss may produce negative feelings and hostile behavior, because it has direct implications for a person's standing and sense of self (Huddy, Mason and Aarøe, 2015). Both, power threat and social identity, theories imply that losing an election to an ethnic minority candidate poses threats to the resources and status of members of the dominant group, to which they react aggressively to suppress them.

Close elections heighten threats

Compared to non-competitive races, close elections between ethnic minority and dominant-group candidates are more likely to be perceived as posing threats to the dominant group, and therefore more likely to provoke hostility. First, political elites have incentives to stoke communal grievances in order to win votes (Horrowitz, 1985), often resulting in violence. In addition, as a result of such a mobilization, ethnic identification and ethnocentrism are heightened (Eifert, Miguel and Posner, 2010), frequently expressed as hostility against outgroups.

Second, when public office is contested by candidates of different ethnic groups it is often seen as a means for accommodating coethnic preferences (Dancygier, 2010). When elections are not close, there is no uncertainty about who benefits from holding office, and hence election results are neither salient nor they reveal new information about the distributional consequences of the electoral outcome. By contrast, in close elections, competition between groups over an uncertain distributional outcome is more salient, and given the higher stakes, it is more likely to result in conflict. Relatedly, when elections are close, people are motivated to process more information about their political and electoral context (Huddy, Mason and Aarøe, 2015). Close elections, in that sense, increase the salience of contesting minority groups and of threats involved in losing political office.

Recent work, in fact, suggests that dominant group members are more likely to respond to local changing demographic context with exclusionary attitudes, when primed by external stimuli that reinforces the threat, such as a negative salient national rhetoric about immigrants (Hopkins, 2010). Consistently, studies find that providing information to dominant ethnic group members about changing ethnic demographics triggers multiple concerns about their status, standing, and poten-

tial vulnerabilities, which, in turn, promote derogation of ethnic minorities (Craig, Rucker and Richeson, 2018). We argue that a close election may operate as the external stimulus (or informational mechanism about the size, growth, and mobilization capacity of ethnic minority groups) that connects the dominant group changing demographic, economic and political context with their behavior and attitudes toward minorities. Such a stimulus (or information cue) reinforces the hostile response to losing an election to an ethnic minority candidate.

3 Ethnic minority candidates and Members of Parliament in the UK

We test the above argument using the case of minority candidates in close parliamentary elections in the UK. The number of ethnic minority candidates has been increasing over time, in part due to a 2010 agreement between the three biggest parties to set targets for the improvement of minorities' representation. In the last four general elections, ethnic minority candidates have competed in 58% of England and Wales's parliamentary constituencies and have won in 28% of the elections in which they stood. The number of ethnic minority MPs has thus increased between 2010 and 2019 from 26 to 65 (10% of the House of Commons Members).[2]

MPs in the UK are constituency oriented, and the personal relationship they cultivate with their constituents make them highly visible. About 66% of British constituents can spontaneously recall the name of their MP; twice the recall level of US Member of Congress (McKay, 2020). The turn towards a focus on constituency services was set in motion by the post-war welfare state, which increased citizens' interactions with the state as they navigated a complex set of rules and eligibility criteria. Recent reforms in Parliament—like the foundation of the Backbench Business Committee—have further reinforced the centrality of constituency concerns (McKay, 2020). Although the ability of individual MPs to engage in redistribution is restricted by parliamentary institutions (e.g., voting in accord to party whip), MPs often trade off their time to participate in government-opposition debate (one of their most fundamental resources) for constituency representation, and are frequently involved in

[2]Still below the number (93 MPs) that would reflect the ethnic make-up of the UK population (Uberoi and Tunnicliffe, 2021).

local casework, even when matters are legally a responsibility of local government, like housing or pensions (Crewe, 2015).

Relevant to our study, evidence from candidate and MP surveys suggest that ethnic minority politicians are more eager to represent minorities than their white counterparts. Minority MPs are more likely to believe that racism held back British minorities, and that it is their duty to address inequalities by prioritizing minority concerns. This is less true, however, of Conservative politicians (Sobolewska, McKee and Campbell, 2018). On the demand side, immigrants believe that coethnic politicians better understand their communities' interests and take these interests to heart (Bloemraad, 2006).

In this context, control over political office by a member of an ethnic minority group can be viewed as a threat to the privileged position of the dominant group. Such a threat may be more salient in a competitive context in which the increased political representation of ethnic minorities is backed by institutional efforts to increase their representation (King and Brustein, 2006), such as the 2010 multi-party agreement mentioned above. The threat may be based on objective conditions of competition: ethnic minority MPs advocate for redistributive policies benefiting their fellow minorities, and they may use constituency services to redirect attention to marginalized groups (Dancygier, 2010). However, threats can also be based on the subjective perception that ethnic minority MPs pose a threat to the hierarchical social order: the presence of ethnic minority politicians in Parliament may signal the demise of a 'white political elite' (Clark, 1994). Moreover, minority politicians have been shown to increase minorities' sense of political efficacy, enhancing minorities' future political participation, and in turn, the prospects of further electing minority group representatives (Maxwell, 2012). We study a context in which ethnic minority political victories pose concerns to the resources and status of dominant group members that may find their expression in hostility towards minorities.

4 Data and outcome measures

We measure violent backlash with police recorded monthly hate crime counts. Hate crime offenders are not representative of the broader public: most offenders in the UK are white, male and under 25 (Roberts et al., 2013). We therefore measure mass response with public opinion data. Particularly, we use attitudinal measures toward

immigrants and ethnic minorities from post-election surveys. Finally, we assess the reactions from elites with the valence of newspaper articles about the candidates' ethnic group. Unlike hate crimes for which we do not know the victim's identity, and public opinion data where questions are broadly about immigrants and minorities, the newspaper data allows us to measure responses specifically targeting the ethnic group of a candidate. We collect these data for the longest period available covering the general elections from 2010 to 2019.

4.1 Hate crimes

Data We use police recorded monthly hate crime counts in England and Wales desegregated by Community Safety Partnership (CSP) and Local Authority District (LAD) from April 2014 to September 2020. We obtained these data from the Home Office via a FOI request by offense sub-code for racially or religiously aggravated offenses (e.g. racially or religiously aggravated assault with injury) and for non-aggravated equivalent offenses (e.g. assault with injury).[3] Overall, the data contains 327,840 hate crimes, of which 61% happen in constituency-election years where minority candidates stand.

Outcomes We measure violence against ethnic minorities as the monthly number of hate crimes per 1,000 residents in a given constituency. We focus on every month after the general election, from the first month, which includes the election date, up to nine months later, corresponding to the maximum period of available crime data after the 2019 election. As a placebo outcome, we use the monthly constituency crime rate of equivalent offenses that are not motivated by racial or religious animus. Appendix B describes the process we follow to assign hate crimes from LADs into parliamentary constituencies, including a validation exercise.

[3]We do not have information from 30 CSPs that do not use offense sub-codes to report hate crimes, nor information about the identity of perpetrators and victims. We therefore cannot identify whether victims share a candidate's ethnic group. However, because of misperceptions and ignorance regarding victim's identity, hate crimes possibly affect those who "appear" sharing a candidate's ethnicity, as documented for the Sikh community post-9/11 (Ahluwalia and Pellettiere, 2010). Our estimates capture such possible spillover into candidate's unrelated ethnic groups.

4.2 Mass public opinion

Data Public opinion data are from the 2010, 2015, 2017 and 2019 British Election Study, which are face-to-face post-election surveys fielded immediately after a general election, and representative of UK eligible voters. We focus on white respondents and, for comparability, subset the data to England and Wales, for which we have hate crime data. Pooling together all survey rounds, we have a sample of 2,200 respondents in constituency-elections where minority candidates run for a seat in parliament, and at least one respondent in 45% of these constituency-elections. We analyze the data at the individual level since it is not representative of constituencies.

Outcomes We measure inclusionary attitudes towards immigrants with an item that asks respondents across the last three post-election surveys whether too many immigrants have been let into the UK. To assess the robustness of our results, we compute two additional outcomes that use all other available survey items on attitudes towards immigration and ethnic minorities. These items are either included in all survey years, but the wording of questions and answers (and their range) changes or are not included across the four elections. We use an index of left–right views on topics unrelated to immigration as a placebo outcome. In Appendix C we describe the survey items and the computation of the outcome variables in greater details.

4.3 Media tone toward migrant groups

Data We use data from over 500,000 articles from 350 national, regional and local UK newspapers, from 2010–2019. This data is from Common Crawl, which is an open repository of web crawl data. We assume that an article refers to a candidate's ethnic group when three conditions are met: 1) the publication date is on election day and up to 10 months after each general election (the maximum between the 2019 election and the most recent article)., 2) the article contains mentions of terms referring to the candidate's country or nationality of origin (extracted with CoreNLP's named entity annotator), and 3) such mentions co-occur in the article with a mention referring to the candidate's constituency (extracted by tokenizing the article and finding tokens which match place names in the Index of Place Names in Great Britain, and mapping to the corresponding constituency). This data includes almost 150,000 mentions from 156 newspapers that meet these three conditions.

Outcomes Using CoreNLP's five-category sentiment annotator, we compute a measure of tone in elite speech about the candidate's ethnic group. Particularly, we extract from the relevant articles the sentiment of the sentences mentioning the candidate's country and nationality terms. We compute our main outcome by taking the monthly ratio between the negative valence mentions and the total number of mentions about the candidate's ethnic group. We focus on such a ratio rather than on the number of negative mentions, because the result of the election may increase the salience of the winning candidate's ethnic group. We also compute the proportion of positive- and neutral-valence mentions, which we use for assessing a generalized increase in salience.

To increase confidence in our main measure of tone, we compute a placebo measure which includes mentions about countries and nationalities in North America, Western Europe, and Oceania in the candidate's constituency, which should not be affected by the identity of a winning candidate. Appendix D describes with greater detail the process to gathering the newspaper data and to computing the outcome measures. It also discusses validity of the named entity and sentiment annotators, and their advantages compared to other methods for measuring the tone of text.

4.4 Election results and ethnic background of candidates

Data General election results from 2010 to 2019 are from the Electoral Commission. As for the ethnic background of candidates, we construct a binary variable of a parliamentary candidate's ethnicity as either white or Black, Asian, and minority ethnic (BAME). We identify the ethnic origin of BAME candidates based on their country of birth, and their parents and grandparents' countries of birth. Appendix E provides details on this data collection.

Across the last four general elections 923 ethnic minority candidates from 334 constituencies (58% of all constituencies) stood for parliament in England and Wales, with increasing numbers over time (Figure F.1a). Because our estimation strategy involves the strongest minority candidate in each constituency, our sample is of 662 candidates with 28% winners. These candidates are fairly split across the two biggest parties and across geographical areas (Figure F.1b and F.1c).

4.5 Constituency characteristics

We use data from the 2001 and 2011 Census (accessed via Nomis, Office of National Statistics) to compute predetermined characteristics of a constituency that may determine both an ethnic minority win and an outcome of interest, including the constituency vote share for UKIP and BNP in the previous election, constituency population share that is foreign born, ethnic or religious minority, unemployed, the share of households with high deprivation, and population density.

In Table A.1 we present summary statistics for our main outcome, treatment and control variables.

5 Estimation method

We test whether the accession of members of ethnic minority immigrant groups to political office triggers a backlash against immigrant communities with a sharp RD design that compares our outcome measures between constituencies with barely winner minority candidates and constituencies with barely loser minority candidates (or equivalently, with barely winner dominant-group candidates). The focus on close elections follows our theoretical framework, but in practice, it allows us to causally identify backlash effects because constituencies where a minority candidate narrowly wins or loses to a dominant group candidate are, on average, otherwise identical.

Because in our RD design the score is the constituency's ethnic minority margin of victory (defined as the vote share of the strongest ethnic minority candidate minus the vote share of its strongest dominant-group opponent), an ethnic minority candidate must run for their constituency to be included in our estimation sample. This is the case for 58% of the constituencies in England and Wales across the four last elections. The RD strategy estimates a (local) average treatment effect that is representative of these constituencies (Table G.1 characterises them).

For estimation, we use local polynomial methods to fit two separate regression functions above and below the victory cutoff. The estimated RD effect is thus computed as the difference between the two estimated intercepts. More formally, we estimate the following weighted linear regression:

$$Y_{itm} = \alpha + \beta_1 VictoryMargin_{it} + \tau EthnicMinorityVictory_{it} + \beta_2 \left(EthnicMinorityVictory_{it} \cdot VictoryMargin_{it}\right) + \epsilon_{itm}, \quad (1)$$

where Y_{itm} is one of our outcome variables measured for constituency i at election year t, and month m after election.[4] $VictoryMargin_{it}$ is the score, and $EthnicMinorityVictory_{it}$ is a dummy variable indicating a minority candidate winning the election ($\mathbf{I}(VictoryMargin_{it} > 0) = 1$). The weights are computed with a triangular kernel of the distance between each observation's score and the cutoff. These kernel-based estimators require a bandwidth for implementation (with observations outside the bandwidth receiving zero weight). Following common practice, we select an optimal bandwidth that minimizes the mean squared error (MSE); i.e. that simultaneously minimizes the bias and variance of the RD estimate (Cattaneo, Jansson and Ma, 2020). For implementation, we use the `rdrobust` software.

The quantity of interest is τ which, under the assumption of continuity of the expected potential outcomes at the cutoff, captures the local average treatment effect *at the cutoff* of an ethnic minority victory on our outcomes.[5] For efficiency gains, we control for predetermined characteristics of the candidates (e.g. incumbency), constituencies (listed in Section 4.5), and survey respondents (for public opinion outcomes) that may determine both our outcomes and a minority win.

We cluster the standard errors ϵ_{itm} by constituency-election to account for dependence of hate crimes and media tone within a constituency across months after an election, and for dependence of respondents' attitudes within a constituency and election year. Below we report conventional point estimates and robust bias-corrected inference.

[4]Media tone is measured for ethnic group e, constituency i, election year t, and month m, and the public opinion outcomes are measured for individual j, in constituency i at election year t, and therefore the outcome and error term are indexed by *eitm*, *jit*, respectively.

[5]Below we report empirical tests supporting this assumption.

6 Results

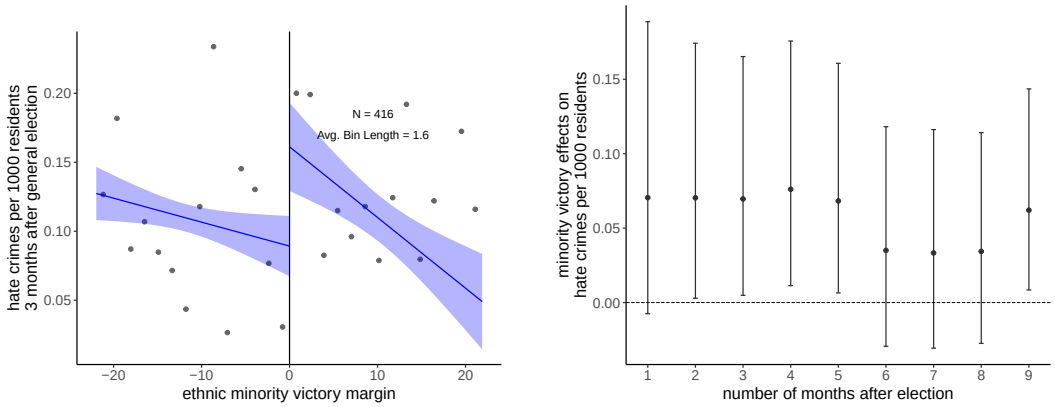
6.1 Hate crimes

In Figure 1a we present the effect of minority candidates' victory on hate crime, three months after the general election. To the right of the victory cutoff, the line (with 95% confidence intervals) shows for different values of the victory margin, the average monthly hate crime rate in constituencies where minority candidates win. To the left of the cutoff, the line shows the average monthly hate crime rate in constituencies where minority candidates lose. As these lines show, there is a jump at the victory cutoff: when a minority candidate goes from narrowly losing to narrowly winning a seat in Parliament, hate crimes in the candidate's constituency increase by 7 per 100,000 residents. Since the average size of a UK constituency is about 70,000 eligible voters, a minority win results in additional 4.9 hate crimes in the average constituency during the three month period after an election. This effect is equivalent to 0.88 standard deviations, and corresponds to a 68% increase in hate crime relative to the average hate crime rate in constituencies where minority candidates barely loose.[6] Table 1 shows the point estimate from Figure 1a with robust bias-corrected inference, and also point estimates using half the MSE-optimal bandwidth, and a fixed bandwidth of 10 percentage points.

In Figure 1b we present estimates of the victory effects on hate crime by month since the general election. While we find some suggestive evidence that these effects decay over time, we note also that across months effects are not statistically distinguishable from each other. In Table H.1, we present these effect estimates, estimates of their inference, size of the MSE-optimal bandwidth, and effective sample (i.e. number of observations within the bandwidth). The table reports in addition estimates from a specification that does not include predetermined covariates. Across specifications, effects have same direction and similar magnitude, and are statistically significant when we control for predetermined covariates.

[6]We remind the reader that our estimation method identifies the hate crime response at the victory cutoff. We can interpret the vertical distance between the two points where the lines touch the cutoff, but we cannot interpret the lines' slopes, given that we do not control for (unobservable) variables that determine hate crime away from the cutoff.

Figure 1: Ethnic minority victory effects on hate crime



(a) 3 months after general election

(b) 1–9 months after general election

Notes: In (a) lines are average monthly hate crime rates (with 95% confidence intervals) from local linear regression with covariate adjustment fitted to the sample of units within the MSE-optimal bandwidth of +/- 22 percentage points around the victory cutoff. Points are average monthly hate crime rates for equally spaced mimicking-variance bins. In (b) points are conventional RD estimates of minority victory effects and lines 95% robust bias-corrected confidence intervals. These confidence intervals are not centered around the conventional RD estimate, but around the robust bias-corrected estimate.

Validity tests and robustness checks The RD estimates would be invalid if candidates sort around the winning threshold, in which case observations on either side of the cutoff might not be comparable. Following common practice to validate the design, we conduct density tests that show that the approximate number of observations just above the cutoff is not significantly different from the number of observations just below it, and covariate tests that seek to show null RD effects on relevant predetermined variables. To ensure that there are no other threats to the validity of our results, we conduct a series of falsification tests showing that the results are not sensitive to the bandwidth choice and to the order of the fitted polynomials, and a series of robustness checks, including placebo outcomes and tests for possible hate crime reporting biases. We present these tests, which strongly support the validity of our results, in Appendix H.

6.1.1 Subgroup analysis

Grounded on theories of threat, we assess whether the accession of ethnic minorities to political office interacts with conditions that make minority victories more

Table 1: Ethnic minority victory effects on hate crime

	Outcome: Hate crimes per 1000 residents		
	(1)	(2)	(3)
$\mathbf{I}(\text{VictoryMargin} > 0) = 1$	0.070	0.102	0.105
	(0.041)	(0.038)	(0.038)
Mean DV control	0.103	0.065	0.062
R^2	0.317	0.536	0.553
Num. eff. obs.	416	200	184
Num. obs.	2080	2080	2080
N Clusters	520	520	520
Using bandwidth	22.285	11.143	10.000
MSE-optimal bandwidth	22.285	22.285	22.285

Notes: The dependent variable is monthly hate crimes per 1000 residents in a constituency 3 months after election. Average treatment effect at cutoff estimated with local linear regression with triangular kernel and MSE-optimal bandwidth in (1), half MSE-optimal in (2) and fixed at 10pp in (3). In parenthesis standard errors robust bias-corrected and clustered by constituency-election. Models control for predetermined covariates.

threatening: 1) local conditions, like migrant influx and economic downturn, are presumed to affect perceptions of relative deprivation, thereby heightening the zero-sum nature of electoral competition, and therefore the likelihood of a hostile response, 2) whether a candidate has a Muslim background, as Islam may raise concerns about 'threats to British life', for dominant-group members susceptible to ethnocentric movements, but possibly also for cosmopolitans whose socially liberal inclinations do not square with Islamic values (Dancygier, 2017), 3) the political affiliation of candidates, as Labour affiliated minorities have a more liberal ideology on racial and social spending issues, and are also more likely to address long-standing inequalities by prioritizing minority concerns (Sobolewska, McKee and Campbell, 2018), triggering, in turn, stronger concerns among members of the dominant group who favor the status quo, and 4) whether a constituency elects an ethnic minority candidate for the first time, activating new threat perceptions.

In Appendix I (specifically Figure I.2a) we show that the RD estimate of the effect of a minority win on hate crimes in constituencies with high influx of migrants

over the last decade before the election is larger and statistically distinguishable from the estimate of the effect in constituencies with a low influx of migrants (the test statistic of the difference in coefficients is $t = 2.14$). However, contrary to previous finding that immigrants' political power provoke immigrant-native conflict only in economically deprived places (Dancygier, 2010), we find that, while larger, the effect of a minority win in constituencies with a high increase in unemployment over the last decade before the election is not distinguishable from the effect in constituencies with a low increase in unemployment (the test statistic is $t = 0.69$).

Regarding candidates religious backgrounds, we find suggestive evidence that the minority victory effects on hate crime are stronger in constituencies with standing minority candidates with a Muslim background (Figure I.1). We also find that the hate crime response is concentrated in constituencies where candidates hail from left-leaning parties (Figure I.2b). Conservative minority candidates and MPs not only do not prioritize minority concerns as their Labour counterparts do, but also their political ideology is appealing to the voters more likely to negatively respond to minorities winning office (Karpowitz et al., 2021). A Conservative minority win, therefore, does not pose a threat to the status quo, as a Labour minority win does, muting the hostile response.[7] This finding suggest an interaction effect between candidates' ethnic minority background and political affiliation. Tellingly, when we focus on constituencies in which only white candidates stand for Parliament, we do not find that a white Labour close victory increases hate crimes after the election; the coefficients are close to zero and are statistically insignificant (Figure I.3). This suggests that a candidate's political affiliation plays a role in backlash only insofar as the candidate has an ethnic minority background.

Finally, if a minority win serves as an information cue that changes threat perceptions, then a violent response should be more likely in constituencies that elect a minority for the first time. While our main model specification isolates incumbency effects,[8] here we assess such expectation by controlling for a dummy variable

[7] A complementary explanation is that Conservative minority candidates are assigned to more white and homogeneous constituencies with less intergroup interactions (Byrne et al., 2020).

[8] 21% of the constituency-elections in our sample are represented by incumbents, and 44% of the winning candidates within the optimal bandwidth are incumbents.

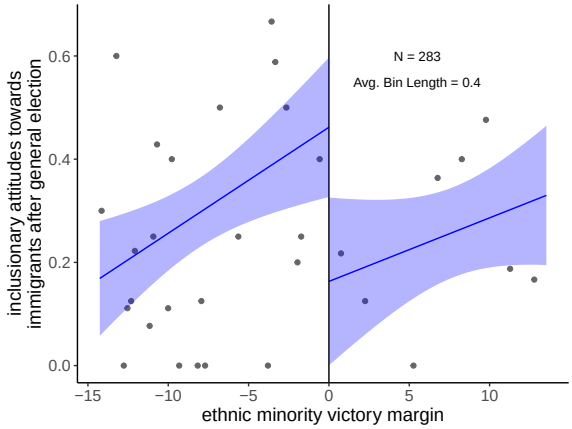
indicating whether a constituency has had a standing ethnic minority MP at least once prior to any of the elections in our sample. We find that the RD estimate shrinks and is not statistically significant when we control for such a variable (Figure I.2c), suggesting that the hate crime response to minority wins mostly happens in constituencies that elect minorities for the first time.

6.2 Mass public opinion

In Figure 2 we present the main effect of minority candidates' victory on mass attitudes towards immigrants after the general election. To the right of the victory cutoff, the line (with 95% confidence intervals) shows the average proportion of white respondents who *do not* think that "too many immigrants have been let into the country" in constituencies where minority candidates have won. To the left of the threshold, the line shows the proportion of white respondents with such an opinion in constituencies where minority candidates lost. As these lines show, there is a drop at the victory threshold: when a minority candidate goes from barely losing to barely winning a seat in Parliament, the proportion of white respondents who hold inclusionary attitudes towards immigrants decrease by 30 percentage points. This effect is equivalent to 0.65 standard deviations, and corresponds to a 66% decrease in inclusionary attitudes relative to the average attitude in constituencies where minority candidates narrowly loose. Table 2 shows the point estimate from Figure 2 with robust bias-corrected inference, and also point estimates using half the MSE-optimal bandwidth, and a fixed bandwidth of 10 percentage points. Because public opinion data is sparse, we refrain from assessing subgroup effects in this section, given power constraints.

Validity tests and robustness checks In Table J.2 we present the effect estimates and all the other relevant statistics for different model specifications. The RD estimates are consistent and statistically significant across specifications. In Appendix J we further present an extensive series of tests (including RD validity, falsification and placebo checks) that ensure the validity of our results.

Figure 2: Ethnic minority victory effects on attitudes towards immigrants



Notes: Lines are average proportion of respondents who do not think that "too many immigrants have been let into the country" (with 95% confidence intervals) from local linear regression with covariate adjustment fitted to the sample of units within the MSE-optimal bandwidth of +/- 14.3 percentage points around the victory cutoff. Points are average proportion of respondents who do not think that "too many immigrants have been let into the country" for equally spaced mimicking-variance bins.

Table 2: Ethnic minority victory effects on attitudes towards immigrants

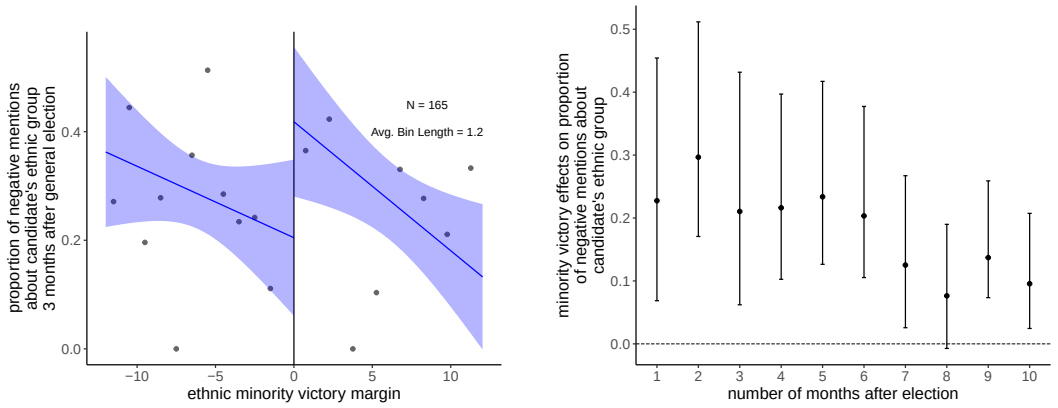
	Outcome: Inclusionary attitudes towards immigrants		
	(1)	(2)	(3)
$\mathbf{I}(\text{VictoryMargin} > 0) = 1$	-0.295	-0.238	-0.242
	(0.056)	(0.066)	(0.083)
Mean DV control	0.445	0.338	0.408
R^2	0.117	0.229	0.150
Num. eff. obs.	283	104	174
Num. obs.	1876	1876	1876
N Clusters	275	275	275
Using bandwidth	14.331	7.166	10.000
MSE-optimal bandwidth	14.331	14.331	14.331

Notes: The dependent variable is dummy indicating whether survey respondent *do not* thinks that "too many immigrants have been let into the country". Average treatment effect at cutoff estimated with local linear regression with triangular kernel and MSE-optimal bandwidth in (1), half MSE-optimal in (2) and fixed at 10pp in (3). In parenthesis standard errors robust bias-corrected and clustered by constituency-election. Models control for predetermined covariates.

6.3 Media tone towards migrant groups

We present our main finding on the media tone towards migrant groups in Figure 3a. The line (with 95% confidence intervals) to the right of the victory cutoff shows the average monthly proportion of negative mentions about the winning candidate's ethnic group three months from the general election, and to the left the average monthly proportion of negative mentions about the losing candidate's ethnic group. As these lines show, there is a jump in the proportion of negative mentions at the winning threshold. The estimated magnitude of such an increase in negative media coverage is about 20 percentage points (or 0.67 standard deviations) and is equivalent to an increase of 110% relative to the average proportion of negative mentions about the ethnic groups of narrowly losing candidates. Table 3 presents the point estimate from Figure 3a with robust bias-corrected inference, and also estimates using half the MSE-optimal bandwidth, and a fixed bandwidth of 10 percentage points. Such a media backlash targets a candidate's ethnic group, rather than the candidate. Less than 1% of the articles used in this analysis include mentions of candidates.

Figure 3: Ethnic minority victory effects on media tone



(a) 3 months after general election

(b) 1–10 months after general election

Notes: In (a) lines are average monthly proportion of negative mentions about a candidate's ethnic group (with 95% confidence intervals) from local linear regression with covariate adjustment fitted to the sample of units within the MSE-optimal bandwidth of +/- 12 percentage points around the victory threshold. Points are average monthly proportion of negative mentions about a candidate's ethnic group for equally spaced mimicking-variance bins. In (b) points are conventional RD estimates of the effect of an ethnic minority victory and lines 95% robust bias-corrected confidence intervals.

Table 3: Ethnic minority victory effects on media tone about migrant groups

	Outcome: News articles' negative mentions (share)		
	(1)	(2)	(3)
$\mathbf{I}(\text{VictoryMargin} > 0) = 1$	0.210	0.706	0.296
	(0.094)	(0.218)	(0.167)
Mean DV control	0.193	0.025	0.129
R^2	0.229	0.401	0.266
Num. eff. obs.	165	90	138
Num. obs.	1314	1314	1314
N Clusters	438	438	438
Using bandwidth	12.134	6.067	10
MSE-optimal bandwidth	12.134	12.134	12.134

Notes: The dependent variable is monthly share of negative mentions in news articles about a candidate's ethnic group 3 months after election. Average treatment effect at cutoff estimated with local linear regression with triangular kernel and MSE-optimal bandwidth in (1), half MSE-optimal in (2) and fixed at 10pp in (3). In parenthesis standard errors robust bias-corrected and clustered by constituency-election. Models control for predetermined covariates.

Figure 3b shows estimates of the minority victory effects on media tone by month since the election. We find suggestive evidence that the effects decay over time. The RD estimates of the effects of a minority win decrease in size around the seventh month after the election, however the monthly effects are not statistically distinguishable from each other.

Validity tests and robustness checks In Table K.1 we present effect estimates and all other relevant quantities for different model specifications. Across specifications, effects have same direction and similar magnitude, and are statistically significant when we control for predetermined variables. In Appendix K we present findings from an extensive series of tests (e.g., RD validity, falsification, and placebo checks) that further confirm the validity of our results.

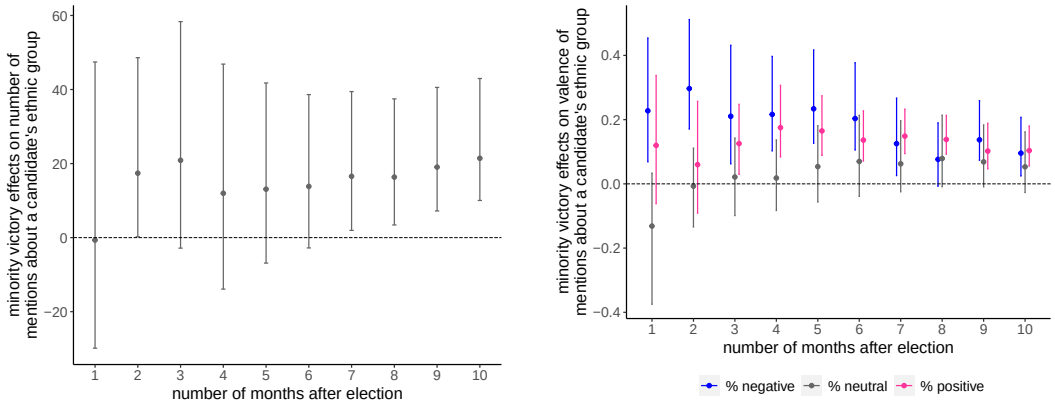
6.3.1 Additional analysis of media tone and salience

We first assess whether a minority win brings more media attention to a winning candidate's migrant ethnic group. An increased salience in an electoral context is of particular importance: more attention to ethnic minority communities promotes their visibility among politicians, organizations and larger society, improving those communities' ability to introduce claims (Bloemraad, de Graauw and Hamlin, 2015). However, a salient rhetoric with a negative tone about migrants can also increase mass public exclusionary attitudes (Hopkins, 2011), hindering migrants' integration. Thinking about threat concerns that arise from a minority win (related to economic considerations and social status), we then assess whether the response in negative rhetoric is stronger among segments predisposed to react to such concerns. Here we focus on right-wing media, which arguably is more threatened by economic redistribution and multiculturalism.

In Figure 4a we show that there is more media mentions about the ethnic communities of narrowly winners than about narrowly losers. Three months after the election, there is 20 more mentions about narrowly winners (significant at the 10% level). This increase in media attention is concentrated on speech with specific valence. We find statistically significant increases in the proportion of negative and positive mentions (equivalent to 0.66 and 0.47 standard deviations, respectively three months after the election), but not in neutral mentions (of 0.09 standard deviations, Figure 4b). Furthermore, at least during the first four months from the election, the increase in attention is predominantly negative, when we compare the share of negative to neutral mentions (Table L.1).

Overall, we find that minority wins cause a backlash from the news media. Such backlash may further affect public attitudes and behaviors in constituencies that elect minorities. However, we also find a counter-effect (the proportion of positive mentions about narrowly winners' ethnic communities also increase) that may counteract the force of a salient negative rhetoric. While we find that newspapers that are not ideologically aligned with the candidate's party drive negative mentions, we also find that these newspapers contribute with the positive mentions. However, we also find that left-wing newspapers and large papers (those with a circulation of more than 25,000 copies) contribute the most to the observed increase in positive mentions. In contrast, the increase in negative mentions is mostly driven by right-wing newspapers (Appendix L), suggesting that the negative response comes from

Figure 4: Ethnic minority victory effects on media attention and tone



(a) Effects on media attention

(b) Effects on media tone

Notes: Points are conventional RD estimates of the effect of an ethnic minority victory and lines 95% robust bias-corrected confidence intervals.

elites that favor less redistribution, compensatory policies, and pluralistic values. These findings also suggest that the corrective behavior comes mostly from left-wing journalists working for bigger papers.

7 Discussion

We argue that dominant-group natives respond with hostility to threats triggered by ethnic minority electoral victories, and this response is stronger when (predetermined) local conditions or candidates' characteristics reinforce such threats. It is only observed in constituencies with a large migrant arrival (right side of Figure I.2a), and similarly with left-affiliated candidates (Figure I.2b), both of which threaten access to material resources and (national or cultural) identity. The response is also stronger when identity is threatened by candidates' Muslim background (Figure I.1), and from right-wing elites is (Figure L.1b), who have more concerns about both material and identity threats.

Our study sharpens the debate about *why* electoral victories by ethnic minorities trigger a hostile response. We find support for the theory that minority victories produce an information shock, reminding members of the dominant group of their changing ethnic demographic landscape Craig, Rucker and Richeson (2018), but

that other sources of information also feed into the hostility of the reaction. Indeed, the reaction is stronger where the information confirms changes people observe in their environment (e.g., a large migrant arrival, right side of Figure I.2a), when the information is novel (e.g when a minority wins a constituency for the first time, Figure I.2c), and when the information is recent (readily after the election, Figures 1b and 3b). Moreover, the effect of a minority victory on threat concerns changes as more information becomes available. Particularly, we find that 1) the effects weakly decay over time (Figures 1b and 3b) and 2) that they are concentrated in constituencies with no prior experience of a minority victory (Figure I.2c) —once members of the dominant group realize that minority office-holding does not involve high levels of ingroup favoritism, as documented in some existing work (e.g Bhalotra et al., 2014), the hostility subsides.

These results —together with the finding that victory effects are not moderated by local economic conditions (left side of FigureI.2a)— suggest that hostile responses are caused by threats based on group categorization and differentiation (i.e. *status threats*) in addition to possible social-structural sources of group difference (i.e. *objective material threats*), even as these are notoriously hard to separate (Manekin, Grossman and Mitts, 2019). Future work should help clarify which dimension of group threat —threat to group status or objective-material threat— is most consequential in this and other settings.

Finally, consistent with theories pointing to elite mobilization fueling communal grievances for their political benefit, our results point to both a backlash from elites and the mass public. The news media can shape the public's political attitudes, and electoral behavior (Grossman, Margalit and Mitts, 2022), but due to market pressures, media outlets are also incentivized to cover issues that resonate with their audiences' priorities (Gentzkow and Shapiro, 2010), and public sentiment is often reflected by newsworthy political events in themselves, like election results. Our study is not designed to determine the direction of influence, but the magnitude of the RD estimates suggests no clear pattern of influence of one societal sector on the other, as they are both about the same size (0.65 sd). It is left to future work, based on a different research design, to assess possible co-influence patterns.

8 Conclusion

Using a regression discontinuity design of close parliamentary elections in the UK, we identify the effects of immigrant-origin minority candidates winning political office on the attitudes and behavior of dominant-group members. We find that such victories result in an increase in hate crimes, in exclusionary attitudes towards immigrants as captured by mass public opinion, and in negative tone in the coverage of a winning candidate's ethnic group in both local and national newspapers.

An ethnic minority candidate winning a seat in the national parliament triggers a hostile reaction because it poses a threat to the position of dominant group members. The backlash we identify in response to an election outcome is especially concerning because it is so widespread, encompassing not only a violent-prone extremist fringe, but also the mass public, and elites. This finding contributes to the intergroup conflict literature, which has been somewhat ambiguous with respect to the identity of those among the majority group most likely to respond with hostility to heightened outgroup threat. In addition, our study's findings raise important questions regarding both the role of competitive elections in intergroup threat theory and the nature of threat that causes a hostile response. While the structure of our data prevents us from addressing all these questions conclusively, they do point to important avenues for future work.

From a normative perspective, it is somewhat reassuring that we found that the backlash against minority communities is *temporal*. Given the effects' temporality, perhaps on balance, the positive outcomes from getting immigrant-origin minorities elected outweigh the negative effects of such backlash dynamics. Future research should address such a question, as well as the policies that can counteract the hostile response to immigrants' successful integration.

The RD effects we estimate are representative of constituencies where ethnic minority candidates stand for Parliament, which are distinct from the average constituency in dimensions related to immigrant demographics and their settlement choices (Table G.1). Moreover, the RD effects are identified at the victory threshold, where elections between dominant-group and ethnic minority candidates are the most competitive. Our RD design is therefore consistent with our theoretical expectations that threats posed by a minority victory are heightened in a close election. However, from an external validity perspective, it is unclear whether we would

observe the same (magnitude of) effects in non-close election contexts. It is left to future work (using a different research design) to investigate such a question. However, it is reasonable to predict that the backlash dynamics we document here possibly do generalize to other multicultural rich democracies with first-past-the-post electoral systems, and where the majority ethnic group is also the dominant group. We hope that our single-country study motivates future research in other contexts.

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Appendices
—For Online Publication—

A Table of summary statistics

In Table A.1 we present summary statistics for our main outcomes, treatment, and predetermined covariates, including characteristics of the candidates and constituencies.

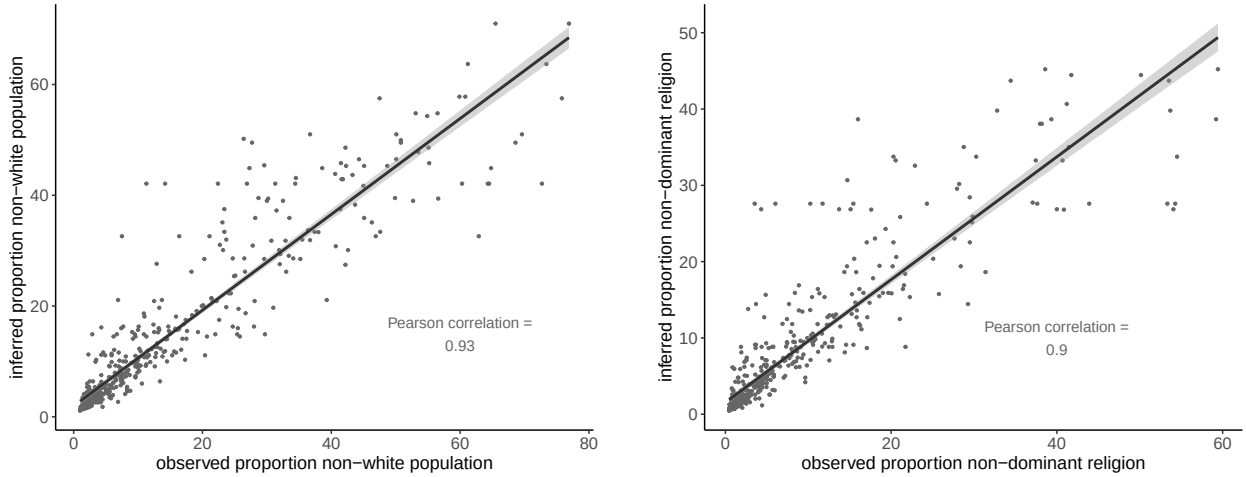
B Assignment of hate crimes from LADs into parliamentary constituencies

Local Authority Districts are a level of subnational division used for the purposes of *local* government. As such, district boundaries may include more than one parliamentary constituency, and constituencies may cross district boundaries. On average, districts contain 2 constituencies (78% include more than one) and about 30% of the constituencies cross district boundaries.

In order to compute hate crime rates at the constituency level we assign the LAD crime rate per 1,000 population to each constituency within a LAD, and for constituencies which cross LAD borders, we assign the average LAD crime rate weighted by population overlap, using the wards' population within a constituency and district to compute the weight. When a ward crosses constituency boundaries (251 wards out of 8297), we split the population ward proportionally by the constituency size.

B.1 Validation of assignment of hate crimes from LADs

To validate the measure of hate crime at the constituency level, we use the assignment rule described above to infer the share of the ethnic minority population at the constituency level and we compare it with the observed share. Figure B.1 shows that the inferred and observed shares are strongly correlated, rendering validity to the assignment rule of hate crimes from districts into constituencies.



(a) Ethnic minority population (b) Population with a non-dominant religion

Figure B.1: Validity of hate crime assignment from LAD to constituency

Table A.1: Summary statistics

variable	mean	sd	min	max
% negative mentions	0.30	0.31	0.00	1.00
% neutral mentions	0.19	0.22	0.00	1.00
% positive mentions	0.21	0.26	0.00	1.00
inclusionary attitudes	0.35	0.48	0.00	1.00
hate crime rate	0.10	0.08	0.00	0.90
victory margin	-20.04	37.91	-82.05	70.17
winner	0.29	0.45	0.00	1.00
incumbent candidate	0.21	0.41	0.00	1.00
female candidate	0.37	0.48	0.00	1.00
left party candidate	0.57	0.50	0.00	1.00
% ethnic minority	23.66	20.18	1.00	76.90
% non-dominant religion	0.16	0.17	0.00	0.91
population density	35.51	34.09	0.30	146.40
% young	0.22	0.06	0.13	0.46
% single	37.66	9.68	23.10	65.10
% deprivation level 1	0.33	0.02	0.28	0.38
% deprivation level 2	0.20	0.04	0.10	0.31
% deprivation level 3	0.06	0.02	0.02	0.13
% deprivation level 4	0.01	0.00	0.00	0.02
% social grade ab	0.24	0.09	0.08	0.50
% social gradea c1	0.31	0.03	0.22	0.43
% social grade c2	0.19	0.05	0.06	0.32
% social grade de	0.26	0.08	0.09	0.51
% level 1 qualifications	12.76	2.72	5.70	19.20
% level 2 qualifications	14.22	2.73	7.30	18.40
% level 3 qualifications	12.02	2.65	8.30	27.70
% level 4+ qualifications	29.30	9.91	12.10	57.40
% economically inactive	30.05	4.33	19.20	43.00
% economically active: students	3.79	1.73	1.90	12.50
% economically active: employed	61.37	6.30	42.00	74.60
% economically active: unemployed	4.79	1.55	2.20	9.50
% tenure: rent free	1.35	0.42	0.60	4.00
% tenure: owned	58.84	14.26	20.50	85.50
% tenure: private rented	19.10	7.77	7.30	42.10
% tenure: social rented	19.81	8.83	5.30	50.60
% English main language: none	7.20	6.52	0.30	26.40
% English main language: one $>$ 16	6.15	4.97	0.50	20.90
% English main language: one $<$ 16	1.39	1.36	0.00	6.10
% immigrants: EU	4.90	3.55	0.60	16.90
% immigrants: non-EU	14.39	11.55	1.00	47.40
% immigrant arrival $<$ 1960	0.01	0.01	0.00	0.04
% immigrant arrival 1960-1990	0.05	0.04	0.01	0.19
% immigrant arrival 1990-2011	0.14	0.11	0.01	0.42
% vote far-right	0.05	0.03	0.00	0.18

C Survey items used in measurement of public opinion

Main outcome:
Inclusionary attitudes towards immigrants. Measured with the item *Do you think that too many immigrants have been let into this country, or not?* on a binary scale with categories *Yes, too many*, and *No, not too many.* This item is available and with a fixed wording in the last three post-election surveys.
Additional outcomes:
Attitudes towards immigrants/immigration regarding the economy. Measured in 2010 with the item *Immigrants generally are good for Britain's economy.* on a 5-point Likert scale ranging from *Strongly disagree* to *Strongly agree.* In the other three election years, the framing of this question is about immigration as opposed to immigrants. The wording of the answers and their range is also different. Nevertheless, we pool the answers to these two questions, as we consider that they are close enough in meaning. We do so to have at least one attitudinal item about immigrants spanning the four election years. The question is: *Do you think immigration is good or bad for Britain's economy?* on a 7-point Likert scale ranging from *Bad for economy* to *Good for economy.* To have all answers on a 5-point scale, we collapse the answer categories 2, 3 and 5, 6.
Index of stereotypical beliefs about migrants and attitudes towards diversity accommodation. Computed by summing the responses to the following items: *Now thinking about minorities in Britain. To what extent do you agree or disagree with each of the following statements?*

1 Minorities should adapt to customs and traditions of Britain

2 Will of the majority should prevail, even over the rights of minorities

3 Immigrants are generally good for Britain's economy

4 Britain's culture is generally harmed by immigrants

5 Immigrants increase crime rates in Britain

Responses are on a 5-point Likert scale ranging from *Strongly agree* to *Strongly disagree.* The order of item 3 is reversed to compute the index. All items are positively correlated with a Cronbach's alpha of 0.83. These items are only available for the 2017, 2019 post-election surveys, and only for respondents who self-completed an additional module (about 60% of all respondents).
Placebo outcomes:
Index of left–right views. Computed via simple sum of these 8 items: *How much do you agree or disagree with the following statements?*

1 Ordinary working people get their fair share of the nation's wealth.

2 There is one law for the rich and one for the poor.

3 There is no need for strong trade unions to protect employees' working conditions and wages.

4 Private enterprise is the best way to solve Britain's economic problems.

5 Major public services and industries ought to be in state ownership.

6 It is the government's responsibility to provide a job for everyone who wants one.

7 People should be allowed to organise public meetings to protest against the government.

8 People in Britain should be more tolerant of those who lead unconventional lives.

Responses are on a 5-point Likert scale ranging from *Strongly disagree* to *Strongly agree*. The order of items 2, 5, 6, 7 and 8 are reversed to compute the left–right index. This index has a Cronbach's alpha of 0.62, and all items are positively correlated.

D Newspaper data, computation of media tone measures and validation of key elements

Newspaper data We construct the dataset of newspaper articles using the following steps. To determine a comprehensive list of UK newspapers, we first identified a list of seed categories on Wikipedia (WP) (e.g. 'Category:Newspapers_published_in_England'), we took the recursive items of those categories (e.g. 'Category:Newspapers_published_in_England' > 'Category:Newspapers_published_in_London'), we used WP article properties to filter out articles about non-newspapers (e.g. people, books), and we extracted the newspaper URLs from the WP Infobox using the Python package `wptools`. With this process we identified a list of UK newspapers URLs containing 337 newspapers in total.

Then, to obtain the articles published by each of these newspapers, we looked up the URLs in Common Crawl (an open repository of web crawl data containing a snapshot of every web page at the moment of the crawl). Particularly in the Index for 2020-16 crawl, the most recent crawl at that moment. We retrieved the WARC (Web ARChive format) records for each crawled page from the newspaper, and extracted the pages' HTML. From the HTML, we extracted the text, title, and byline using the Python package `readabiliPy`; the publication date using the Python library `htmldate`; the location by tokenizing the article with CoreNLP, and looking for tokens which match place names in the Index of Place Names in Great Britain, and mapping to the corresponding constituency. Figure D.1 presents the geographical coverage of all extracted articles across constituencies.

In order to select the subset of articles that reference a candidate's ethnic group, we extracted mentions of terms referring to nationalities and countries using the CoreNLP named entity annotator, as well as the sentiment of the sentences mentioning those terms, using CoreNLP's five-category sentiment classifier, in order to define the tone of speech about a candidate's ethnic group. This sentiment classifier improves upon bag of words sentiment classifiers that ignore the order of words and assign positive points for positive words and negative points for negative words and then sum up these points (e.g., Young and Soroka, 2012). Instead, it addresses compositionality in semantic vector spaces allowing to detect intricacies of sentiment and to capture complex linguistic phenomena, like sentiment change and scope of negation (Socher et al., 2013). The classifier provides highly accurate sentiment predictions at the sentence level, which is the task at hand. We focus on the sentiment of each sentence containing a mention of relevant country or nationality terms. Therefore, an article may provide more than one instance of speech (or mention) about a candidate's ethnic group. The median article contains 2 mentions of the same term. We focus on the collection of all of these instances of speech for each candidate.

Our sample of articles includes for the most part references to a candidate's ethnic group, as opposed to references to a candidate. The share of articles with mentions of a candidate is low, of only 0.53%, and this share is possibly an overestimate. To compute the share of articles with mentions of a candidate, we extract a candidate's surname from the sample of news articles used in the analysis. Because names can be written differently in different outlets, we use approximate string matching with a similarity score greater than 0.5 to extract mentions of a candidate. Given

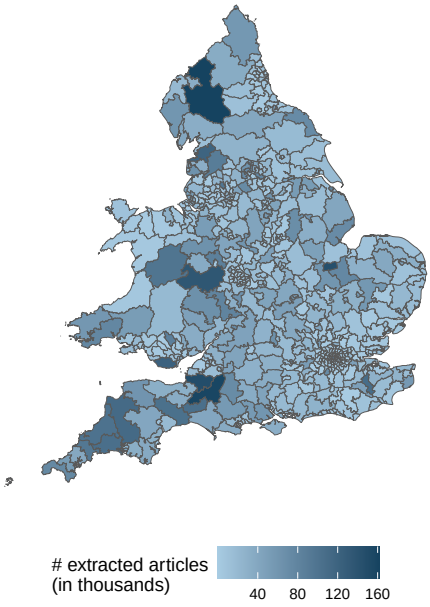


Figure D.1: Coverage of all extracted articles

that we are only extracting a candidate's surname as opposed to their full name, it is possible that we are overestimating the share of articles referencing a candidate, and yet this share is low, suggesting that our analysis captures mostly responses against a candidate's ethnic group.

Validation of named entities and their sentiment classification A human judge annotated a sample of 102 articles containing 563 mentions of country/nationality terms in order to validate them and their sentiment classification. Specifically, the human judge first annotated whether the terms refer to a country/nationality or not for each mention in the article. Only 7% of the mentions refer to something else (e.g. the name of a person, a telephone *pole* as opposed to a Polish person, or were used in URLs referred in the articles). In other words, for this task the named entity annotator of CoreNLP had 93% accuracy.

Second, the human judge annotated the sentiment of each article's sentence mentioning a country/nationality term in the five-category classification scale. Comparing the human annotations to the classification of the model for the positive (including 'very positive' and 'positive') and negative ('very negative' and 'negative') categories, and defining the positive class as the negative sentiment category, we have that the CoreNLP's sentiment annotator has an accuracy of 78%, precision of 63%, recall (or true positive rate) of 89%, specificity (or true negative rate) of 72%, and F1-score (or harmonic mean of precision and recall) of 74%. These are reasonable statistics for sentiment classification (Socher et al., 2013). Although the model overpredicts the negative mentions as compared to the human annotations (the precision is 63%), it gives us a reasonable, if imperfect, measure of negative speech about a candidate's ethnic group in the newspaper articles. Moreover, the effect estimates are not expected to be affected by the imprecision of the sentiment classification model (although the variance estimates may be affected), given that the model overpredicts negative mentions equally across articles speaking about the ethnic group of a narrowly winner and articles speaking of the ethnic group of a narrowly loser.

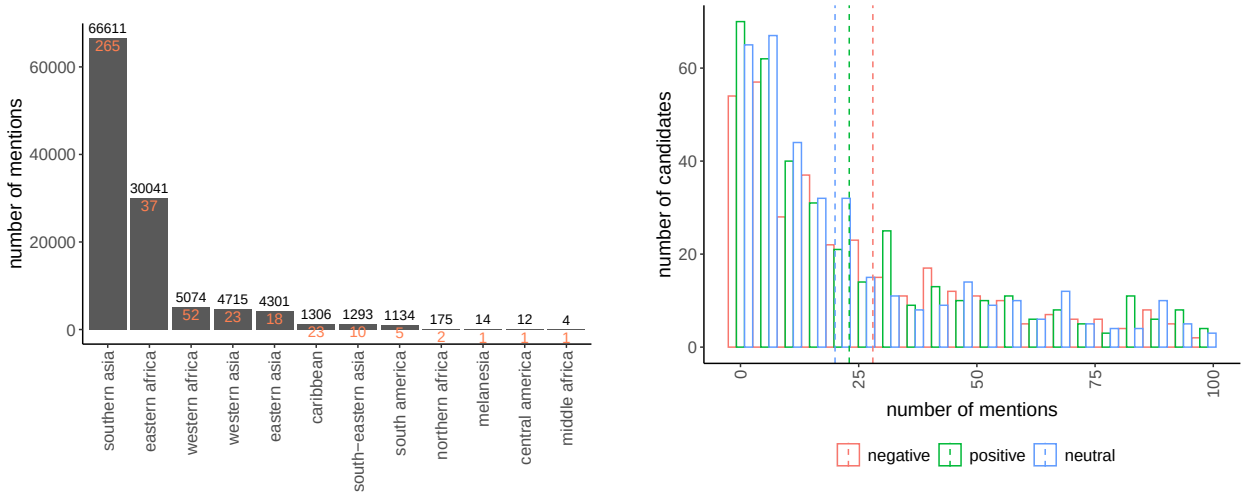
Measure of media tone about migrant groups We match the country/nationality mentions' sentiment to candidates based on date, location, and country/nationality. Specifically, we follow this process: 1) we map the candidate's origin characteristics (their country/nationality of origin, and their parents' and grandparents' countries/nationalities) to a sub-region, 2) we map the articles' country/nationality mentions to a sub-region and 3) we match candidates and articles based on sub-region, constituency and date of publication (using only the subset of articles published on election day and up to 10 months after the election, which corresponds with the maximum number of months between the 2019 election and the most recent news article. This mapping process implies that for say a candidate of Indian origin, the measure of speech about her ethnic group accounts for mentions in her constituency of all countries/nationalities within Southern Asia. In general, we account for all known countries/nationalities of origin of a candidate. For instance, for a Ugandan-Indian candidate,[9] we include all articles which mention the terms Uganda/Ugandans and India/Indians. In this case, given our mapping process the measure of speech about her ethnic group includes all mentions of Southern Asia and Eastern African. Overall, only 11 candidates are assigned to more than one sub-region, but not to more than two. Furthermore, this process excludes a) candidates for whom we do not have origin information below their continent of origin for example, Asia, Africa, Caribbean and b) articles with mentions of terms like 'asian', 'african'. The proportion of excluded candidates represents 30% of all strongest minority candidates (winners and first minority losers). While it is a large proportion, their exclusion may be positive in two ways: 1) the salience or online presence of included candidates is kept constant across candidates, given that we are excluding candidates for whom we cannot find information online about their background and 2) the mapping process treats every candidate the same without making assumptions about their origin. Out of all the strongest minority candidates across the four general elections for whom we have specific information about their background, we have at least one mention during the first ten months after election for 438 candidates in England and Wales. The median candidate has 71 mentions.

Following this matching process, we then compute the ratio between the number of negative mentions (adding together the 'very negative' and 'negative' sentiment categories) and the total number of mentions about the candidates' sub-region of origin in their constituency, at every month after the general election. We compute the analogous ratios for positive (summing the 'very positive and 'positive' categories) and neutral mentions. Figure D.2 presents the frequency of sub-region mentions for all matched candidates across the last four general elections (left panel) and the distribution of mentions about the candidate's sub-region by sentiment categories (right panel).

E Data collection on candidates' ethnic minority background

Collecting data on candidates is a difficult task as there is no single source of candidate data, either from the Electoral Commission, or from the political parties themselves. We rely on a range of sources including the 2010 British General Election Constituency Results, which contains the ethnicity of candidates running with the biggest three political parties: Conservative, Labour and Liberal Democrat. 76% of ethnic minority candidates stand in elections with one of these

[9]These cases are mostly Ugandan-born candidates with Indian parents who migrated to the UK during the onset of Idi Amin's coup in the 70s.



(a) Distribution of all mentions by sub-region

(b) Distribution of candidate mentions by sentiment category

Notes: In (a) the orange numbers indicate the number of candidates from each sub-region. In (b) the dashed vertical lines indicate the median candidate's number of mentions. The histogram excludes the top quartile of candidates with the most mentions for visualization purposes.

Figure D.2: Distribution of mentions

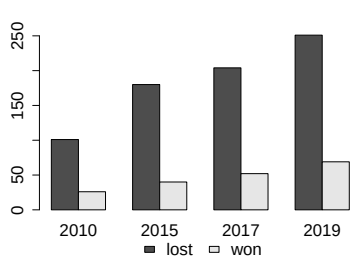
three parties.[10] For the 2015 and 2017 general elections we rely on the Parliamentary Candidates UK project, which collected the ethnicity of every candidate standing in these elections with an established party,[11] and on independent candidates if they are one of the top two finishers in a constituency. For the 2019 election we labelled whether a candidate is BAME by searching the profile of the more than 3,300 candidates and using information of candidates who have run before for a seat in parliament or who are sitting MPs. For candidates in this election, and to identify a candidate's country of origin for all election years, we rely on various sources including crowdsourced information by the Democracy Club, which collects candidates social media accounts (Facebook, Twitter, LinkedIn), campaign websites and their pictures.

We also cull information from party websites, regional and local newspapers, and especially from ethnic newspapers (e.g., Asian Voice), which usually include a list of co-ethnic candidates in an election special issue. We classify a candidate's ethnic origin only when the candidate self-identifies as ethnic minority on their social media profile, personal website, their party's website, or if more than one information source confirms the candidate's origin. We do not include national and ethno-linguistic minorities (e.g, Welsh), as these communities are not classified as minorities in the data we are relying on.

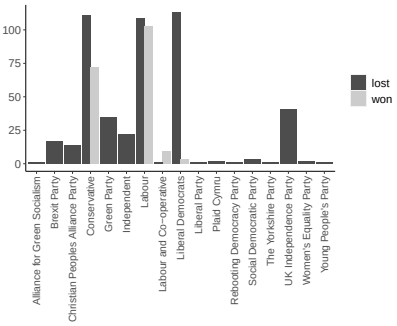
[10]Based on data from the 2015 general election, which is the next closest election for which we have data on every candidate.

[11]Labour, Conservative, Liberal Democrat, Scottish National, Plaid Cymru, UKIP, Green and Northern Ireland parties.

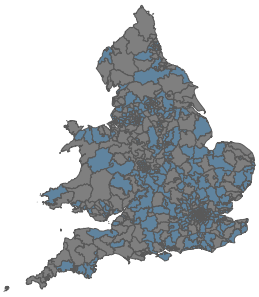
F Ethnic minority candidates across time, parties, and geography



(a) Number of ethnic minority candidates contesting a seat in Parliament



(b) Strongest ethnic minority candidates by political party



(c) Geographical coverage of ethnic minority candidates

Notes: (b) Includes only the strongest minority candidate by constituency-election.

Figure F.1: Description of ethnic minority candidates

G Selection of constituencies into the sample

Table G.1: Selection of constituencies into the sample

	All constituencies		Sample constituencies	
variable	mean	sd	mean	sd
share ethnic minority	12.786	15.390	23.361	20.174
share non-dominant religion	0.081	0.123	0.158	0.174
population density	21.280	26.334	34.301	33.481
share young	0.197	0.050	0.215	0.055
share single	34.098	8.242	37.276	9.528
share deprivation level 1	0.326	0.018	0.330	0.020
share deprivation level 2	0.194	0.041	0.197	0.043
share deprivation level 3	0.052	0.022	0.057	0.023
share deprivation level 4	0.005	0.003	0.006	0.004
share social grade ab	0.224	0.083	0.233	0.092
share social gradea c1	0.307	0.032	0.311	0.032
share social grade c2	0.212	0.044	0.195	0.047
share social grade de	0.257	0.077	0.261	0.084
share level 1 qualifications	13.388	2.267	12.919	2.692
share level 2 qualifications	15.390	2.203	14.387	2.715
share level 3 qualifications	12.321	2.411	12.001	2.514
share level 4+ qualifications	26.824	8.355	28.657	9.783
share economically inactive	30.419	3.950	30.169	4.312
share economically active: students	3.334	1.525	3.739	1.683
share economically active: employed	61.912	5.443	61.296	6.261
share economically active: unemployed	4.336	1.428	4.795	1.526
share tenure: rent free	1.352	0.398	1.352	0.412
share tenure: owned	64.278	11.563	59.347	14.133
share tenure: private rented	16.281	6.354	18.785	7.575
share tenure: social rented	17.354	7.489	19.626	8.807
share English main language: none	4.034	4.871	7.050	6.453
share English main language: one $>$ 16	3.553	3.759	6.066	4.970
share English main language: one $<$ 16	0.736	0.989	1.370	1.343
share immigrants: EU	3.404	2.771	4.737	3.506
share immigrants: non-EU	8.258	8.878	14.074	11.473
share immigrant arrival $<$ 1960	0.009	0.005	0.012	0.006
share immigrant arrival 1960-1990	0.032	0.031	0.053	0.041
share immigrant arrival 1990-2011	0.082	0.084	0.133	0.107
share vote far-right 2010	0.056	0.029	0.050	0.032
N constituency-election	2292		662	

Notes: shows descriptive statistics for all constituencies, and constituencies in our sample. Our sample is selected by dropping constituencies where ethnic minority candidates do not stand for Parliament. The unit of observation is a constituency-election year.

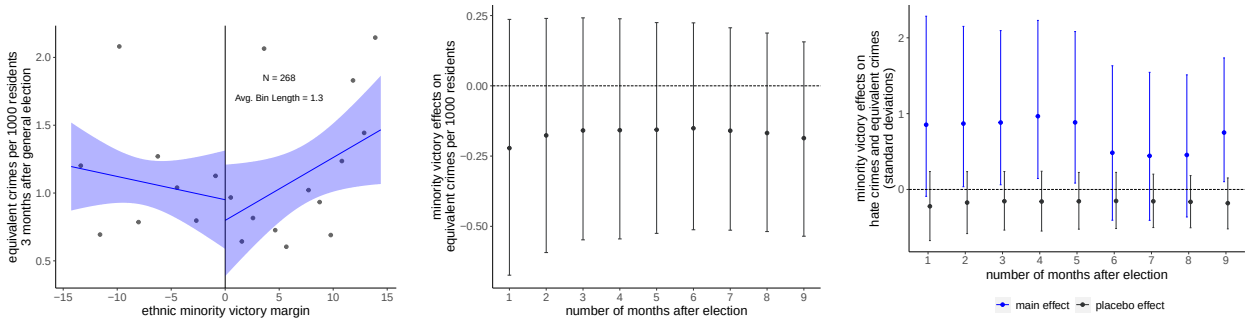
H Minority victory effects on hate crimes: validity of the RD design, robustness checks and supporting results

In this section we report a set of placebo and falsification tests that establish the validity of the RD design (sections H.1–H.5), the main RD results in tabular form (section H.7), a comparison between the main RD results and the results when we control for party dummies (section H.6, a descriptive test confirming that the minority victory effects are not driven by a crime decay in constituencies with minority close defeats (section H.8, and the robustness of the RD to an alternative, difference-in-differences specification (section H.9).

H.1 Continuity of placebo outcomes

We use as a placebo outcome the constituency crime rate for equivalent crimes that are not motivated by racial or religious animus. We test whether this placebo outcome is discontinuous at the margin of victory cutoff. The rationale for this test is the same as the rationale for a test assessing discontinuities in predetermined covariates: when a placebo outcome that correlates strongly with the outcome of interest is discontinuous at the cutoff, then the continuity of the potential outcome functions is unlikely to hold, questioning the validity of the RD design under the continuity-based approach.

Figure H.1 shows that this placebo outcome is not discontinuous at the threshold where an ethnic minority candidate wins a seat in Parliament. The effects are not statistically significant, have the opposite direction to the effects on hate crime and are comparably smaller (Figure H.1c). This increases our confidence that the validity of the design holds, and that the estimates of the minority victory effects on hate crime are not explained by a generalized higher level of crime in constituencies where minorities win.



(a) 3 months after general election

(b) 1–9 months after general election

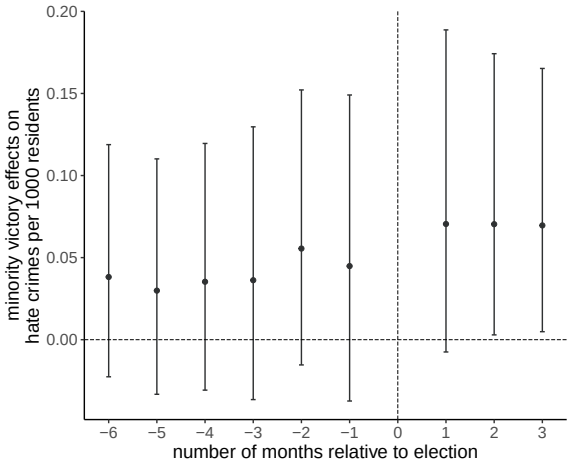
(c) Comparison of main and placebo effects

Notes: In (a) lines represent the average monthly crime rate (with 95% confidence intervals) from local linear regression with covariate adjustment fitted to the sample of units whose vote-share winning margin is within the MSE-optimal bandwidth of +/- 14.5 percentage points around the victory threshold. Points are the average monthly crime rate for equally spaced mimicking-variance bins. In (b) and (c) points are RD estimates of the effect of an ethnic minority victory and lines 95% robust bias-corrected confidence intervals.

Figure H.1: Ethnic minority victory effects on equivalent crimes

H.2 Continuity of main outcome before general election

We test whether the hate crime rate is discontinuous at the margin of victory cutoff before the general election. Figure H.2 shows that the hate crime rate is not discontinuous at the threshold where an ethnic minority candidate wins a seat in Parliament. The effects are not statistically significant and are comparably smaller to the effects after the election. This increases our confidence about the robustness of our results, as it suggests that the estimates of the minority victory effects on hate crime are not explained by other dynamics in constituencies where minorities win.

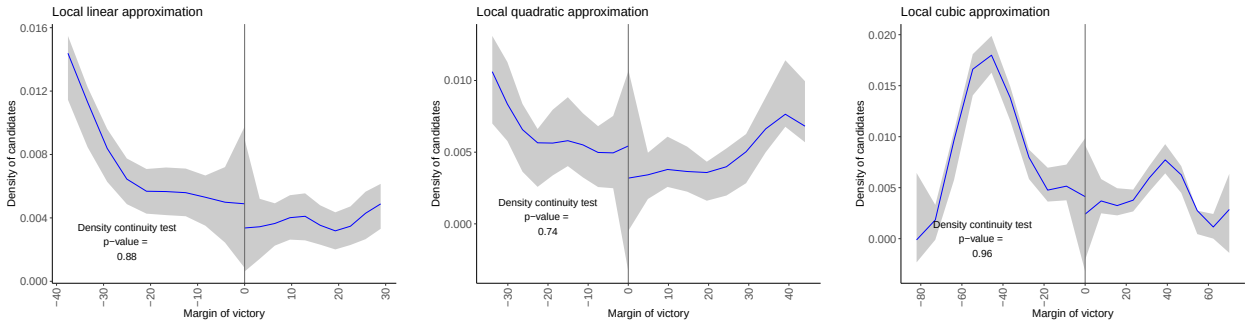


Notes: Points are RD estimates of the effect of an ethnic minority victory and lines 95% robust bias-corrected confidence intervals.

Figure H.2: Ethnic minority victory effects on hate crimes before and after the election

H.3 Density of the running variable

Following (Cattaneo, Jansson and Ma, 2020), we test (using the `rddensity` R package) the continuity assumption of the density functions of the running variable with local polynomial density estimators. Figure H.3 reveals no evidence of sorting around the cutoff. Even though there is a jump in the density functions for losing and winning candidates at the cutoff, the confidence intervals of these functions completely overlap and the p-value of the continuity test indicates that we cannot reject the null of continuity of the density functions. The results of this test indicate no manipulation of the election results.



Notes: Tests for manipulation of the election results by assessing continuity of the candidate density functions at the cutoff with local polynomial density estimators and robust bias-corrected inference.

Figure H.3: Continuity in the density of candidates around the cutoff

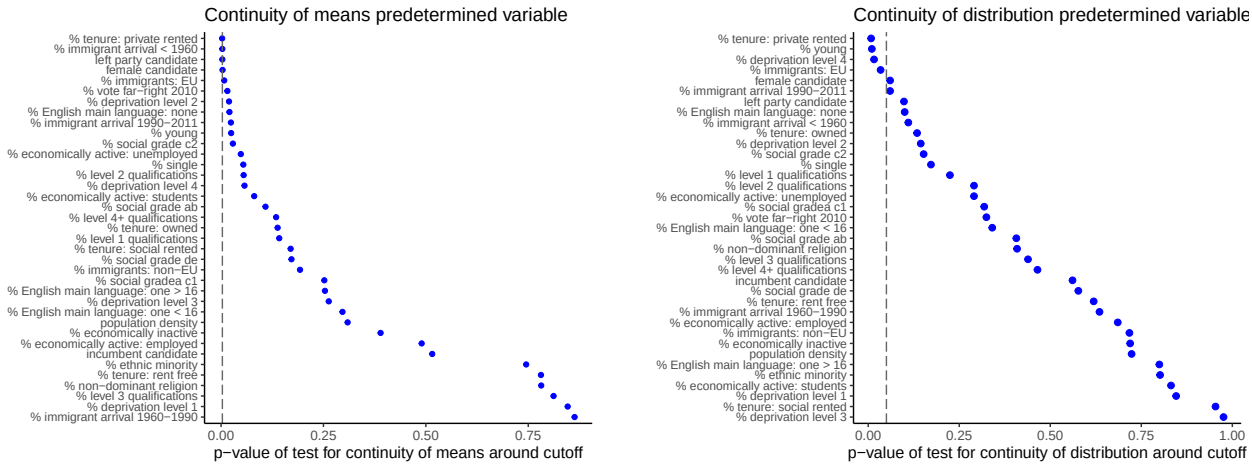
H.4 Continuity of predetermined variables

Following (Calonico, Cattaneo and Titiunik, 2014), we also test (using the `rdrobust` package in R) the continuity assumption for predetermined variables with local linear regression within an MSE-optimal bandwidth. Given that we have a large number of covariates, we show in Figure H.4a the threshold for the p-values of the tests of discontinuity (the dashed vertical line), when controlling the false discovery rate with the Benjamini–Hochberg procedure.[12] In this case, 3 of a total of 37 covariates show statistically significant discontinuities after controlling the FDR.

However, some of the covariates we include are not independent of each other (as BH correction would assume); in particular some of the covariates (such as the proportions of immigrant arrivals in different decades) are linear combinations of an underlying variable. To account for this dependence, we test the continuity assumption with a permutation test for continuity in the distribution of observations around the cutoff (which is a stronger requirement than continuity of means) as described in (Canay and Kamat, 2018) and as implemented by the `RATest` R package. Here we find that only 4 of the 37 predetermined variables are discontinuous at the cutoff (Figure H.4b). This number of discontinuous covariates is equivalent to two more than the average number of false rejections (which is 2). Furthermore, when controlling for the FDR with the Benjamini-Hochberg procedure, we do not find any discontinuous variables.

Given the results from both the permutation test for continuity of distribution around the cutoff and the FDR-corrected local linear regression test, the distribution of p-values is consistent with the uniform distribution that we would expect for balance checks in a randomized experiment. This indicates that there were no systematic discontinuities at the threshold where minorities become MPs, and that therefore the continuity assumption of the potential outcome functions is likely to hold.

[12] See (De la Cuesta and Imai, 2016) for an example of controlling the false discovery rate with the Benjamini–Hochberg procedure when testing for multiple discontinuities in predetermined variables in RD contexts of close elections.



(a) Continuity of means using local linear regression

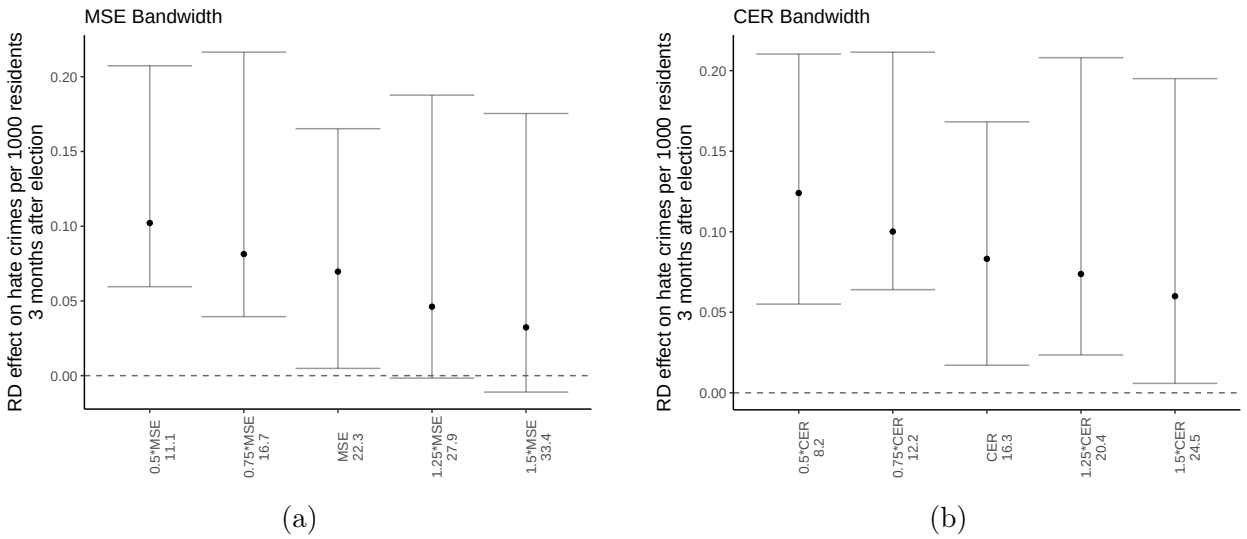
(b) Continuity of distribution using asymptotic permutation test

Notes: Test for continuity of candidate and constituency predetermined background characteristics in (a) using a local linear regression with a symmetric MSE-optimal bandwidth as implemented by the `rdrobust` R package and in (b) using an asymptotic permutation test comparing the distribution of observation near the cutoff as implemented by the `RATest` R package. The vertical line in (a) indicates a p-value $= 0.004$, which is the threshold for the p-values when controlling the false discovery rate with the Benjamini–Hochberg procedure, and in (b) a p-value $= 0.05$. Here the threshold for p-values when controlling the FDR with BH procedure is approximately 0.

Figure H.4: Continuity of predetermined variables around the victory threshold

H.5 Sensitivity to the choice of bandwidth and order of polynomials

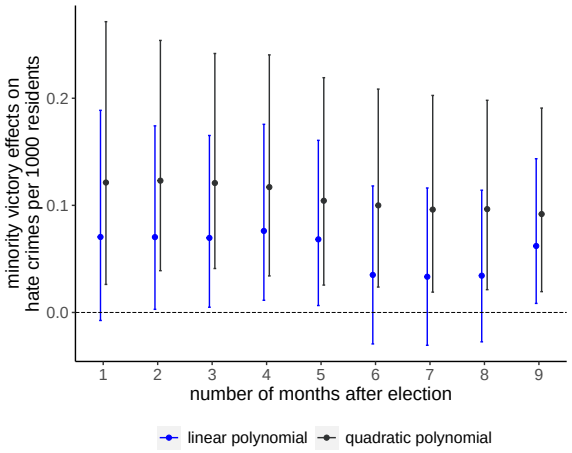
In Figure **??** we test for sensitivity of the results to the choice of bandwidth, using CER- and MSE-optimal bandwidths, half, three fourths, five fourths and one and a half times their size. We find that the results are broadly consistent with the findings obtained with the optimal MSE bandwidth.



Notes: tests for sensitivity to the choice of bandwidth. In (a) MSE stands for mean squared error optimal bandwidth and in (b) CER refers to a bandwidth that minimizes the coverage error from the robust biased corrected confidence intervals obtained with the MSE-optimal bandwidth. The values next to the 'MSE', 'CER', labels indicate the bandwidth size.

Figure H.5: Sensitivity to bandwidth size

In our main estimation method we compute the RD estimates by fitting local-linear polynomials to avoid noisy estimates with poor coverage of confidence intervals (Gelman and Imbens, 2019). We show nevertheless, in Figure H.6, that the results are robust to fitting quadratic polynomials.



Notes: tests for sensitivity to the choice of polynomial order by comparing estimates with local-linear and quadratic polynomials.

Figure H.6: Sensitivity to polynomial choice

H.6 Controlling for candidate's political party

We isolate the ethnic identity of candidates from their political party affiliation from the hate crime response by controlling for party dummies. In Figure H.7 we compare the RD estimates of our main specification to the estimates from a specification controlling for party dummies. The coefficients are very close in magnitude, suggesting that the violent response is not explained only by the political affiliation of the candidates.

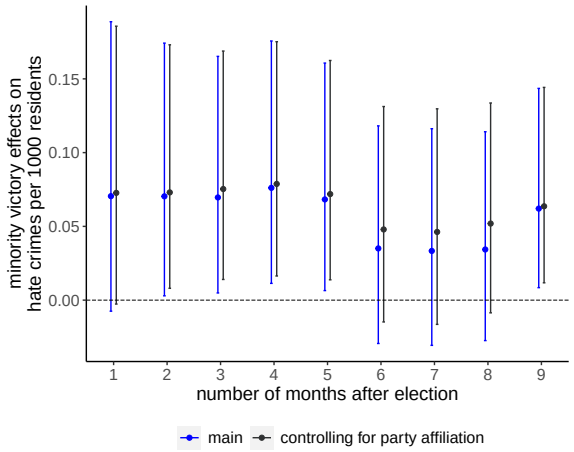


Figure H.7: Isolating the ethnic identity of candidates from their political party

Notes: Points are RD estimates of the effect of an ethnic minority victory on hate crimes per 1000 residents and lines 95% robust bias-corrected confidence intervals.

H.7 Main RD results in tabular form

Table H.1: Ethnic minority victory effects on hate crimes

RD estimate	se	p-value	95% CI	mean control	sd effect	MSE-opt bw	eff. N	N	controls	month
0.076	0.066	0.212	[−0.054, 0.244]	0.107	0.911	20.59	192	1040	no	1
0.070	0.045	0.070	[−0.007, 0.189]	0.107	0.853	21.23	198	1040	yes	1
0.076	0.059	0.162	[−0.038, 0.228]	0.108	1.012	18.98	273	1560	no	2
0.070	0.039	0.043	[0.003, 0.174]	0.110	0.868	20.41	285	1560	yes	2
0.076	0.054	0.135	[−0.029, 0.217]	0.104	1.010	19.14	364	2080	no	3
0.070	0.037	0.038	[0.005, 0.165]	0.103	0.882	22.29	416	2080	yes	3
0.083	0.057	0.122	[−0.027, 0.231]	0.098	1.135	18.69	450	2600	no	4
0.076	0.038	0.026	[0.011, 0.176]	0.100	0.966	20.73	480	2600	yes	4
0.076	0.053	0.136	[−0.029, 0.214]	0.099	1.053	18.81	546	3120	no	5
0.068	0.035	0.034	[0.006, 0.161]	0.099	0.884	21.90	618	3120	yes	5
0.071	0.051	0.140	[−0.029, 0.203]	0.102	0.963	19.32	644	3640	no	6
0.035	0.030	0.238	[−0.029, 0.118]	0.104	0.484	31.87	1113	3640	yes	6
0.068	0.050	0.151	[−0.031, 0.199]	0.105	0.878	19.26	736	4160	no	7
0.033	0.030	0.254	[−0.031, 0.116]	0.108	0.443	31.81	1272	4160	yes	7
0.067	0.049	0.142	[−0.028, 0.193]	0.104	0.857	19.24	828	4680	no	8
0.034	0.029	0.230	[−0.027, 0.114]	0.108	0.455	31.52	1413	4680	yes	8
0.062	0.046	0.149	[−0.028, 0.182]	0.105	0.801	19.79	930	5200	no	9
0.062	0.031	0.027	[0.008, 0.144]	0.104	0.750	21.21	990	5200	yes	9

Notes: The dependent variable is monthly hate crimes per 1000 residents in a constituency. *RD estimate* is computed with local-linear regression within a symmetric MSE-optimal bandwidth. *se* is the conventional standard error, *p-value* and *95% CI* are robust bias-corrected. *mean control* indicates the average monthly hate crime rate in constituencies where ethnic minorities barely lose, *sd effect* presents the RD estimate in standard deviations, *MSE-opt bw* is the MSE-optimal bandwidth of vote-share winning margin around the victory threshold, *eff. N* is the sample size within the MSE-optimal bandwidth and *N* is the sample size. *controls* include an indicator of whether the candidate is the incumbent, constituency vote share for UKIP and BNP in the previous election, constituency share that is ethnic minority, young population, single, with social grade DE, unemployed, population density, and share of households with 3 or more deprivations, and in social tenure. Standard errors are clustered by constituency-election. Hate crime data are from Home Office, ethnic background of candidates is constructed by the authors, and constituency characteristics from 2011 UK Decennial Census.

H.8 Assessing a hate crime decay in minority barely lost constituencies

In Figure H.8 we descriptively show that the minority victory effects on hate crime are not driven by a crime decay in constituencies with close minority defeats. The average hate crime rate in these constituencies after the election is very close to the average hate crime rate before the election. If anything, hate crimes are on average slightly increasing in these constituencies after the election rather than decreasing. This suggests that the documented effect on hate crime is a backlash to minority victories, rather than sympathy towards minorities in constituencies narrowly won by dominant-group candidates.

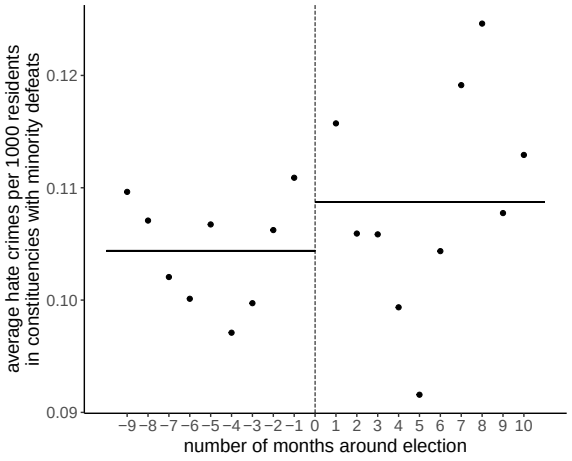


Figure H.8: No hate crime decay in minority barely lost constituencies

Notes: Points are monthly average hate crimes per 1000 residents in constituencies barely lost by ethnic minority candidates, and horizontal lines pre- and post-election hate crime averages in those constituencies.

H.9 Difference-in-differences

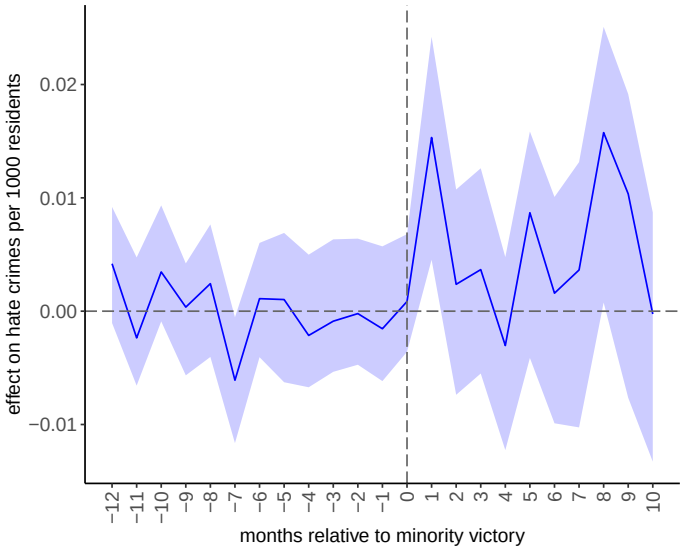
As an additional check for the estimated effects on hate crime, we use a difference-in-differences (DiD) approach that compares the hate crime rate across constituencies that elect ethnic minority candidates and constituencies that do not, before and up to nine months after the election when a minority candidate is elected for the first time in a constituency.

With this estimation design, a constituency is in the treatment condition during the months following a general election in which an ethnic minority candidate is elected, and in the control condition, otherwise. 69 out of 520 constituencies have an ethnic minority MP during at least one month between April 2014 and September 2020, 23 constituencies have a minority MP during this whole period, and 4 constituencies go in and out of the treatment condition. We focus on the first nine months after the election—the maximum number of months which are observable for constituencies electing an ethnic minority candidate for the first time in 2019.

We estimate the DiD estimator with the generalized synthetic control method based on interactive fixed effects models as described in (Xu, 2017) and implemented by the `gsynth` R package. We use this approach as opposed to a standard two-way fixed effects regression because even after controlling for relevant predetermined covariates that determine both minority victories and hate crimes, we reject the null hypothesis of common trends for all pre-minority victory periods and all groups of constituencies that elect a minority candidate for the first time at a particular election. The Cramer von Mises test statistic and p-value of (Callaway, Sant'Anna et al., 2018)'s integrated moments test for the conditional parallel trends assumption holding in all pre-treatment time periods for all groups are 0.886 and $0.\overline{0}$, respectively.

Given this, we instead impute a counterfactual for each treated constituency that resembles the pre-minority victory hate crime trends of treated constituencies. Furthermore, we prefer the generalized synthetic control method over the most recently developed approaches for DiD with multiple time periods and variation in treatment timing (e.g. Callaway, Sant'Anna et al. (2018)), given that the number of constituencies electing a minority candidate for the first time at each of the three observed elections is small: 14, 9, 20, respectively. This produces group-time average treatment effects that are rather noisy.

Figure H.9 presents the estimated effects of electing an ethnic minority candidate to Parliament on monthly hate crimes per 1000 residents. It shows a positive and significant effect in the first month after the election that is won by a minority candidate. After that month, each monthly effect is relatively smaller, and is not statistically significant. However, on average the effect remains positive and larger than the average effect before minority candidates win an election. Consistent with parallel trends (and with the method computing an adequate counterfactual), we do not see any pre-minority MP significant effects, and the effects are very close to zero throughout the 12 month period before an ethnic minority candidate wins the election.



Notes: The line represents the ATT, and the ribbon 95% confidence intervals.

Figure H.9: First time ethnic minority victory effects on hate crime

ATT.avg	S.E.	CI.lower	CI.upper	p.value	months
0.0153	0.0051	0.0045	0.0242	0.005	1
0.0088	0.0041	-0.0003	0.0160	0.058	2
0.0071	0.0038	-0.0010	0.0139	0.078	3
0.0046	0.0035	-0.0029	0.0106	0.268	4
0.0054	0.0035	-0.0025	0.0110	0.199	5
0.0048	0.0034	-0.0032	0.0104	0.287	6
0.0046	0.0034	-0.0035	0.0100	0.350	7
0.0060	0.0035	-0.0026	0.0112	0.199	8
0.0065	0.0036	-0.0026	0.0114	0.207	9

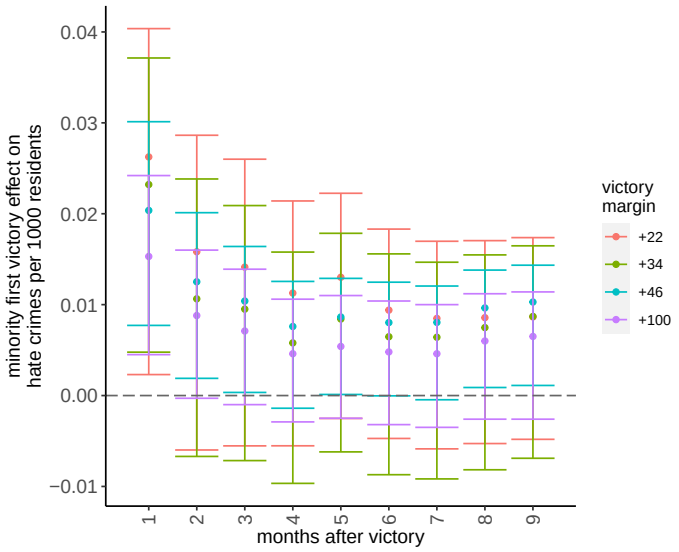
Notes: The dependent variable is monthly hate crimes (racially/religiously aggravated offenses) per 1,000 residents. Inference is conducted via bootstrapping. Standard errors are clustered by constituency.

Table H.2: Average first time ethnic minority victory effects on hate crime (averaging across months after victory)

Table H.2 presents the average effects across the first nine months after a minority victory. In

general, the average effects during this period are positive, statistically significant in the first three months, and decrease with time. Again, these patterns suggest a violent but rather short-lived reaction to ethnic minorities accession to political office.

While the effect at one month after a victory is equivalent to an increase of 1.5 hate crimes per 100,000 residents (and statistically significant at the 1% level), the average effect after three months of victory almost halves to 0.7 hate crimes per 100,000 residents (statistically significant at the 10% level). These effect is ten times smaller than the effect estimated with the RD design. On the one hand, because close elections between ethnic minority and dominant-group candidates are more likely to be perceived as posing a threat to the dominant group's status, and therefore to result in conflict, the RD estimates are likely capturing an upper-bound effect of ethnic minority victories on hostility against minority communities. To provide suggestive evidence that the difference between the magnitude of our RD and DiD estimates is in part explained by how close the election is, in Figure H.10 we compute DiD estimates for different values of victory margins. We start by including constituencies with elections won by a maximum of 22 percentage points, corresponding to the MSE-optimal bandwidth from the RD estimates. We then increase the sample until we include every constituency (that is, with elections won by a maximum of 100 percentage points, as shown in Table H.2). In general, as we increase the victory margin, the DiD estimates decay, suggesting that the degree to which the election is more or less competitive may explain, in part, the difference in the size of effect estimates across the two estimation methods.



Notes: Points represents the ATT and lines 95% bootstrapped confidence intervals.

Figure H.10: First time ethnic minority victory effects on hate crime by victory margin

On the other hand, the effects of the two estimation methods are not directly comparable as they are targeting different quantities of interest. The quantity of interest in the RD design is the local average treatment effect (LATE), while in the DiD approach is the average treatment effect on the treated (ATT). Moreover, the effective samples across the two different approaches are different: the DiD ATT includes the group of constituencies won by large margins and that on average have a smaller post-minority victory hate crime rate (of 0.10 per 1,000 people in constituencies won by more than 15 pp compared to 0.14 in constituencies won by less than 15 pp), while the RD LATE does not include such group of constituencies. Relatedly, the comparison group in the DiD includes

constituencies where minority candidates lose by large margins or *do not even run* for Parliament, and that have on average higher post-election hate crime rates than constituencies where minority candidates run and lose by small margins. These differences in the composition of the samples can explain, in part, the difference in the size of effect estimates across the two estimation methods.

H.10 Testing for possible hate crime reporting bias

It is possible that the observed increase in hate crime is not only explained by the reaction of the dominant group to ethnic minorities winning elections, but also by an increase in hate crime reporting. Specifically, members of the ethnic group of the winning candidate may feel more empowered to report crime. While this is feasible, we contend that it is unlikely, as the definition of hate crime and the process to report it in the UK is designed to prevent under-reporting. Crimes are identified and flagged as a hate crime by the police and the Crown Prosecution Service when the criminal offense is *perceived* by the victim or any other person to be motivated by hostility or prejudice towards someone based on a personal characteristic like race or ethnicity,[13] religion or beliefs, without further prove.[14]

To further assess such an endogeneity concern we estimate minority win effects on hate crime within the category of 'violence against the person', and particularly 'violence with injury'. Because of the seriousness of the offense, such a category of hate crime is expected to be consistently reported regardless of whether people are empowered or discouraged to report crime. Accordingly, the RD estimates of the effects of a minority win should not suffer from such a reporting bias. Despite the small number of crimes within this category (5% of total hate crimes), the estimates presented in Table H.3 are broadly consistent with our main findings on total hate crime: crimes jump at the minority victory threshold.

[13]Race for the UK criminal justice system agencies means any group defined by race, color, nationality or ethnic or national origin.

[14]https://www.met.police.uk/advice/advice-and-information/hco/hate-crime/what-is-hate-crime/

Table H.3: Ethnic minority victory effects on violent hate crimes

RD estimate	se	p-value	95% CI	mean control	sd effect	MSE-opt bw	eff. N	N	month
-0.0003	0.001	0.909	$[-0.002, 0.003]$	0.006	-0.045	20.56	192	1040	1
0.0010	0.001	0.160	$[-0.001, 0.004]$	0.005	0.154	21.76	303	1560	2
0.0026	0.001	0.000	$[0.001, 0.005]$	0.004	0.439	18.66	360	2080	3
0.0011	0.001	0.105	$[-0.000, 0.003]$	0.005	0.185	23.95	540	2600	4
0.0009	0.001	0.128	$[-0.000, 0.003]$	0.005	0.141	18.62	540	3120	5
0.0017	0.001	0.023	$[0.000, 0.005]$	0.004	0.242	15.10	490	3640	6
0.0020	0.001	0.010	$[0.001, 0.005]$	0.004	0.280	14.58	544	4160	7
0.0027	0.001	0.000	$[0.001, 0.005]$	0.004	0.376	13.63	558	4680	8
0.0026	0.001	0.000	$[0.001, 0.005]$	0.004	0.349	13.04	580	5200	9

Notes: The dependent variable is monthly hate crimes within the category of 'violence against the person with injury' per 1000 residents in a constituency. *RD estimate* is computed with local-linear regression within a symmetric MSE-optimal bandwidth. *se* is the conventional standard error, *p-value* and *95% CI* are robust bias-corrected. *mean control* indicates the average monthly hate crime rate in constituencies where ethnic minorities barely lose, *sd effect* presents the RD estimate in standard deviations, *MSE-opt bw* is the MSE-optimal bandwidth of vote-share winning margin around the victory threshold, *eff. N* is the sample size within the MSE-optimal bandwidth and *N* is the sample size. The model specification includes controls: an indicator of whether the candidate is the incumbent, constituency vote share for UKIP and BNP in the previous election, constituency share that is ethnic minority, young population, single, with social grade DE, unemployed, population density, and share of households with 3 or more deprivations, and in social tenure. Standard errors are clustered by constituency-election. Hate crime data are from Home Office, ethnic background of candidates is constructed by the authors, and constituency characteristics from 2011 UK Decennial Census.

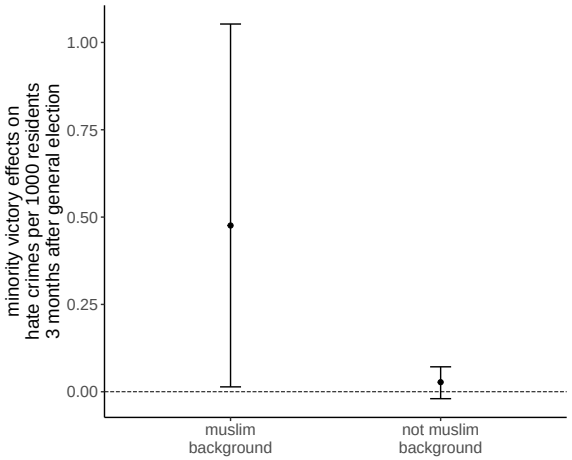
I Subgroup effects on hate crime

We conduct four subgroup analyses. First, in Figure I.2a (right side) we show that the effect of a minority candidate victory in close parliamentary elections on hate crime is concentrated in constituencies that experience a larger than median increase in the number of migrants in the decade preceding the elections. In contrast, while we find that the effect on hate crimes is larger in constituencies that have experience larger than median increase in their unemployment rate in the decade preceding the election versus those experiencing relatively low unemployment rate, the difference between those two coefficients is not statistically significant (Figure I.2a, left side).

Second, we assess whether candidates with a Muslim background trigger a stronger hate crime response. Because we are able to code religion only for 23% of the candidates-contituency-election years, for this analysis, we impute a candidate's religion based on their region of origin, that is, we assign to each candidate the main religion in their region. 19% of candidates-contituency-election years (from 2010-2019) are determined to be Muslim, and the rest are Christian, Buddhist, or Hindu. The results in Figure I.1 suggest that the minority victory effects on hate crime are concentrated in constituencies with candidates from regions where the main religion is Islam (the difference in coefficients is statistically significant at the 0.1 level; t = 1.76).

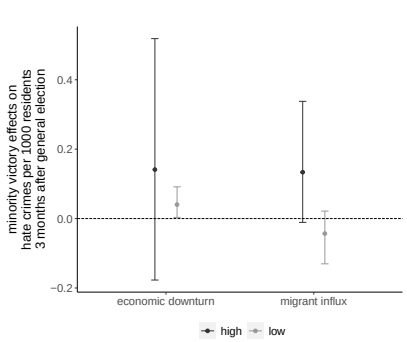
Third, in Figure I.2b we show that minority migrant victory only has a positive effect on hate crime incidence when the candidate hails from a left- but not a right-wing party. We further show in Figure I.3 that these effects are not driven by a candidate's political affiliation only, but by the interaction between party affiliation and minority background. Particularly, we repeat the RD analysis but using races in which only white candidates stand for Parliament. Here, the running

variable is the difference between the vote share of a white Labour candidate against the strongest white contestant. We do not find that a white Labour close victory increases hate crimes after the election; the coefficients are close to zero and not statistically significant. Finally in Figure I.2c we demonstrate that when controlling for whether the constituency was represented in the past by a minority candidate, the size of the effect shrinks quite a bit in the first 5 post-election months.

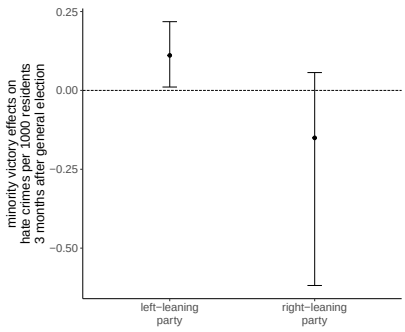


Notes: Points are RD estimates of the effect of an ethnic minority victory and lines 95% robust bias-corrected confidence intervals.

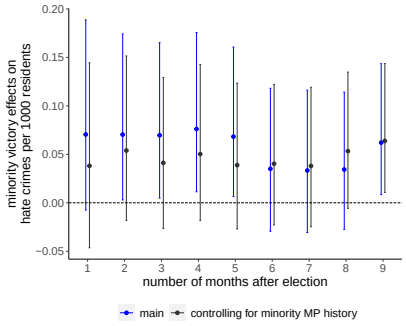
Figure I.1: Effects on hate crime by religious background of candidates



(a) Effects on hate crime by local conditions



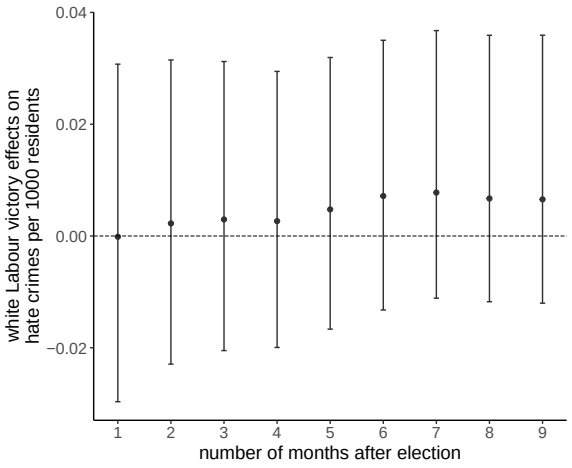
(b) Effects on hate crime by political ideology of candidate's party



(c) Effects on hate crime: controlling for history of an ethnic minority MP

Notes: Points are RD estimates of the effect of an ethnic minority victory on hate crimes per 1000 residents and lines 95% robust bias-corrected confidence intervals.

Figure I.2: Subgroup effects on hate crime



Notes: Points are RD estimates of the effect of a victory of a white Labour candidate and lines 95% robust bias-corrected confidence intervals.

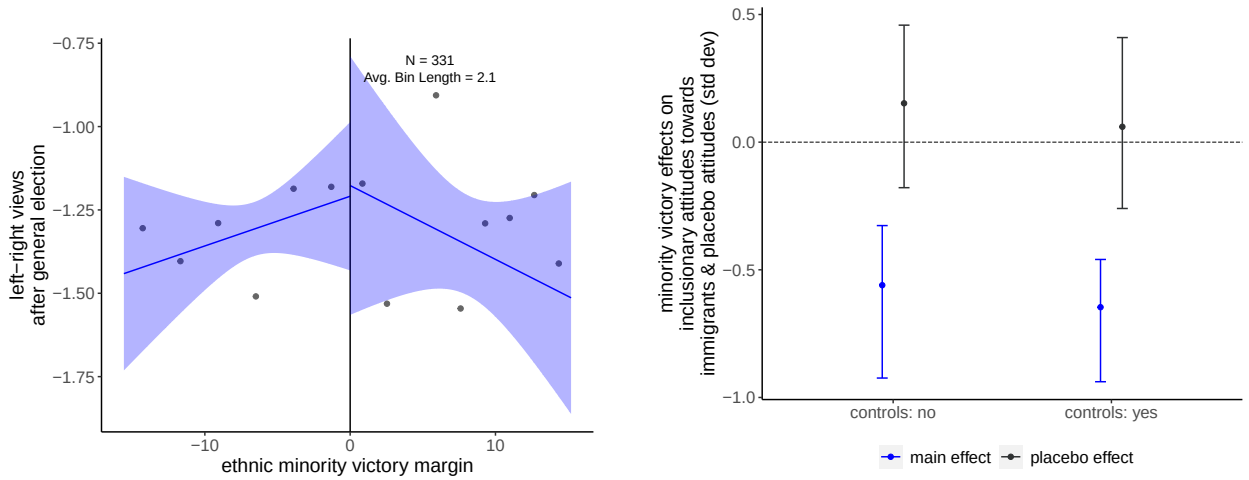
Figure I.3: White Labour victory effects on hate crime

J Minority victory effects on mass public attitudes: validity of the RD design, robustness checks and supporting results

Moving to mass public opinion, we report below a set of placebo and falsification tests that establish the validity of the RD design (sections J.1–J.4), and the results' robustness to alternative specifications (section J.5). We further report the main RD results in tabular form and the robustness of those results to alternative survey questions in section J.6.

J.1 Continuity of placebo outcomes

We test whether an index of left–right views is discontinuous at the threshold where constituencies go from electing a dominant group candidate to electing a minority candidate. The rationale for using ideology as a placebo outcome is that it is expected to be strongly correlated with attitudes towards immigrants and ethnic minorities, but as ideology is sticky is not expected to be affected by the ethnic identity of the winning candidate. Figure J.1 reveals no discontinuity in ideology at the threshold where minority candidates win a seat in Parliament. These tests suggest that the validity of the design holds.



(a) Effects on ideology (placebo outcome)

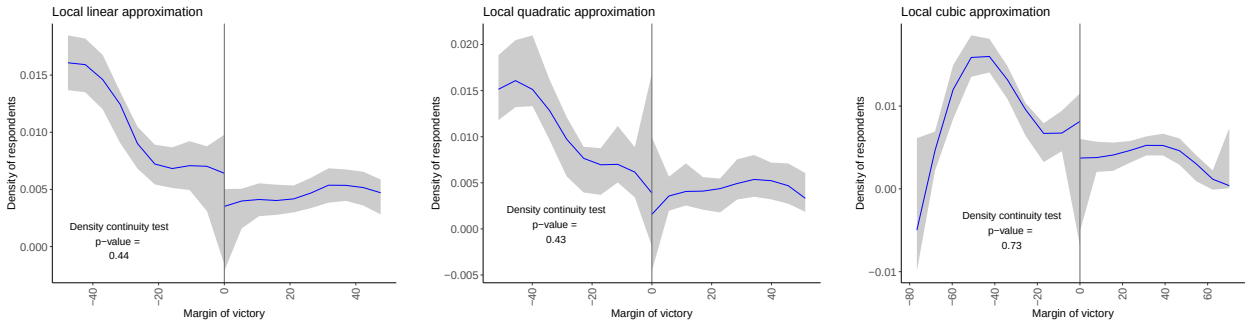
(b) Comparison of main and placebo effects

Notes: In (a) lines represent respondents' average left–right view (with 95% confidence intervals) from local linear regression with covariate adjustment fitted to the sample of units whose vote-share winning margin is within the MSE-optimal bandwidth of +/- 15.7 percentage points around the victory threshold. Points are the average left–right view for equally spaced mimicking-variance bins. In (b) points are RD estimates of the effect of an ethnic minority victory and lines 95% robust bias-corrected confidence intervals.

Figure J.1: Ethnic minority victory effects on ideology (placebo outcome)

J.2 Density of the running variable

Figure J.2 reveals no evidence of sorting around the cutoff. Even though there appears to be a jump in the density functions of respondents at the threshold in which constituencies go from electing a dominant group candidate to electing a minority candidate, the confidence intervals of these density functions completely overlap and the p-value of the continuity test indicates that we cannot reject the null of continuity of the density functions. The results of these tests indicate no manipulation of the election results.

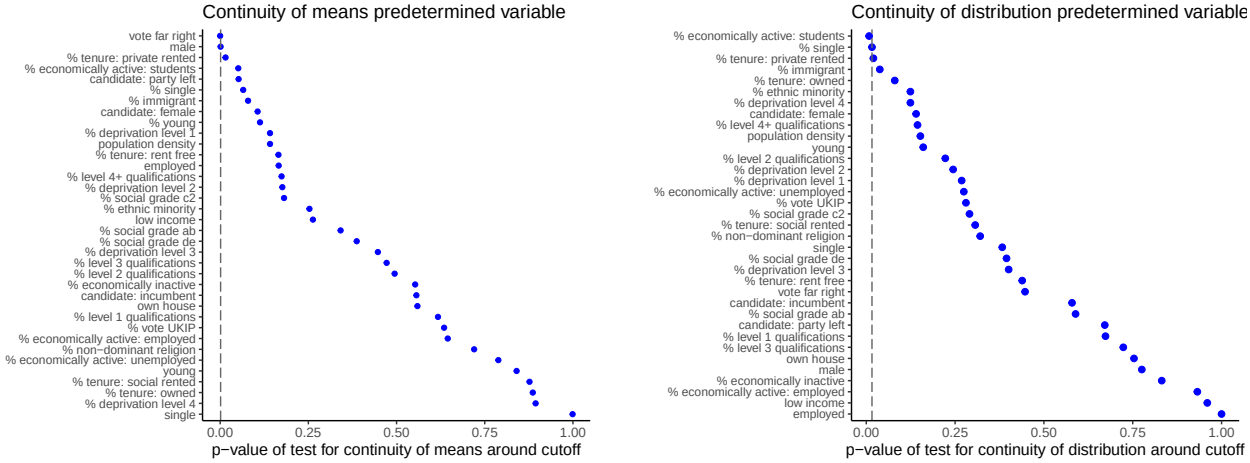


Notes: Tests for manipulation of the election results by assessing continuity of the density functions at the cutoff with local polynomial density estimators and robust bias-corrected inference.

Figure J.2: Continuity in the density of survey respondents around the cutoff

J.3 Continuity of predetermined variables

In Figure J.3 we present results for the tests on the continuity of predetermined variables around the threshold where minority candidates win a seat in Parliament. We find that 2 of a total of 36 covariates show statistically significant discontinuities in *means* with the test employing local linear regression within an MSE-optimal bandwidth controlling for the FDR with the Benjamini–Hochberg procedure (Figure J.3a). This number of discontinuous covariates is equivalent to the average number of false rejections (which is 1.8). Furthermore, with the permutation test for continuity in the *distribution* of observations around the cutoff, we find that only 1 of the 35 predetermined variables are discontinuous at the cutoff (Figure J.3b). The results from both tests suggest that there were no systematic discontinuities in the covariates at the threshold were minorities win political office, and that therefore the continuity assumption of the potential outcome functions is likely to hold.



(a) Continuity of means using local linear regression

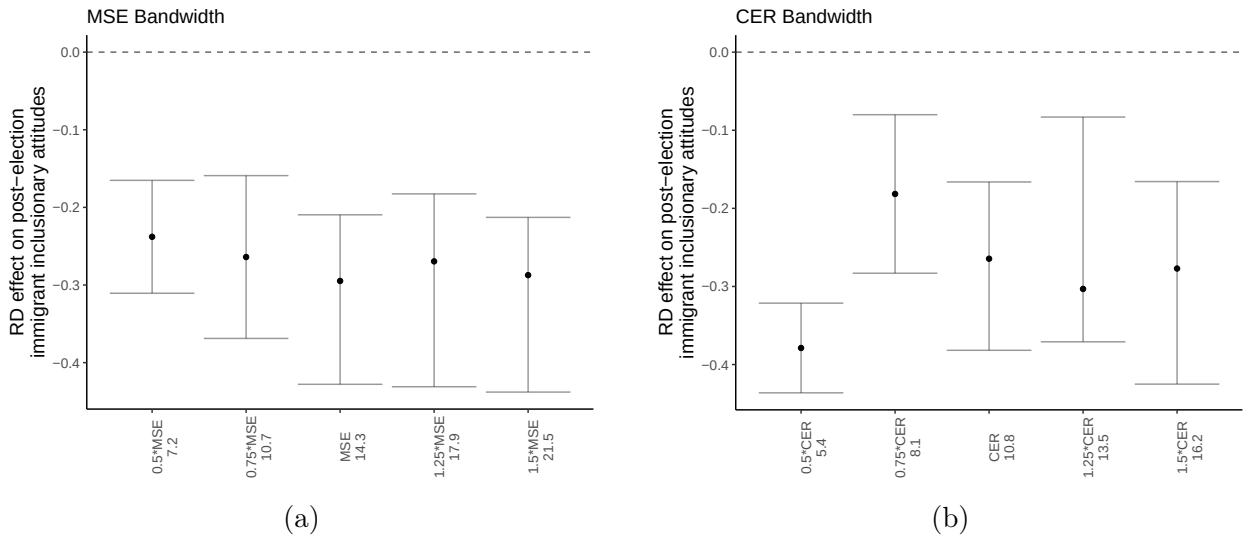
(b) Continuity of distribution using asymptotic permutation test

Notes: Test for continuity of candidate and constituency predetermined background characteristics in (a) using a local linear regression with a symmetric MSE-optimal bandwidth as implemented by the `rdrobust` R package and in (b) using an asymptotic permutation test comparing the distribution of observation near the cutoff as implemented by the `RATest` R package. The vertical line in (a) indicates a p-value = 0.0012, which is the threshold for the p-values when controlling the false discovery rate with the Benjamini–Hochberg procedure, and in (b) p-value = 0.008.

Figure J.3: Continuity of predetermined variables around the cutoff

J.4 Sensitivity to the choice of bandwidth and polynomials

The results on mass public opinion are not sensitive to the choice of bandwidth. In Figure **??** we present the minority victory effects on the main attitudinal outcome for different values of the bandwidth. We fit our lineal model to the sample of observations within the CER- and MSE-optimal bandwidths, half, three fourths, five fourths, and one half their size. We find that the results are broadly consistent with the findings obtained with the MSE-optimal bandwidth.



Notes: tests for sensitivity to the choice of bandwidth. In (a) MSE stands for mean squared error optimal bandwidth and in (b) CER refers to a bandwidth that minimizes the coverage error from the robust biased corrected confidence intervals obtained with the MSE-optimal bandwidth. The values next to the 'MSE', 'CER', labels indicate the bandwidth size.

Figure J.4: Sensitivity to bandwidth size

In Table J.1, we show that the results are robust to fitting quadratic polynomials.

Table J.1: Ethnic minority victory effects on mass inclusionary attitudes towards immigrants

RD estimate	se	p-value	95% CI	mean control	sd effect	MSE-opt bw	eff. N	N	cov	smpl	pol
-0.255	0.065	0.000	[−0.426, −0.150]	0.434	-0.562	14.42	291	1924	no	f	l
-0.258	0.065	0.000	[−0.425, −0.150]	0.440	-0.560	15.14	288	1876	no	c	l
-0.295	0.052	0.000	[−0.428, −0.210]	0.445	-0.646	14.33	283	1876	yes	c	l
-0.270	0.073	0.001	[−0.454, −0.127]	0.461	-0.589	21.99	403	1924	no	f	q
-0.277	0.071	0.000	[−0.460, −0.129]	0.469	-0.605	22.89	399	1876	no	c	q
-0.313	0.063	0.000	[−0.433, −0.137]	0.446	-0.687	17.61	345	1876	yes	c	q

Notes: The dependent variable is a dummy indicating whether a survey respondent *do not* thinks that "too many immigrants have been let into the country". *RD estimate* is computed with local-linear regression within a symmetric MSE-optimal bandwidth when *pol* is *l*, and with a quadratic polynomial when *pol* is *q*. *se* is the conventional standard error, *p-value* and *95% CI* are robust bias-corrected. *mean control* indicates the average proportion of respondents who *do not* think that "too many immigrants have been let into the country" in constituencies where ethnic minorities barely lose. *sd effect* presents the RD estimate in standard deviations, *MSE-opt bw* is the MSE-optimal bandwidth of vote-share winning margin around the victory threshold, *eff. N* is the sample size within the MSE-optimal bandwidth and *N* is the sample size. *cov* is a vector of controls including an indicator of whether the candidate is the incumbent, whether the survey respondent is male, young, single, employed, owns a house, and the constituency vote share for UKIP and BNP in the previous election, share that is foreign born, and share of households with 3 or more deprivations. *smpl* is the used sample: *f* stands for full sample and *c* for a complete cases sample with no missing values for respondent's predetermined variables. Standard errors are clustered by constituency-election. Survey data are from the British Election Study, ethnic background of candidates is constructed by the authors, and constituency characteristics from 2001 and 2011 UK Decennial Census.

J.5 Controlling for candidate's political party

We isolate the ethnic identity of candidates from their political party affiliation from the attitudinal response by controlling for party dummies. In Figure J.5 we present the ethnic minority victory effects from a specification that controls for party dummies. The coefficient is very close in magnitude (somewhat bigger) to that obtained with our main specification shown in Figure 2, suggesting that the exclusionary attitudinal response is not driven by the political affiliation of the candidates.

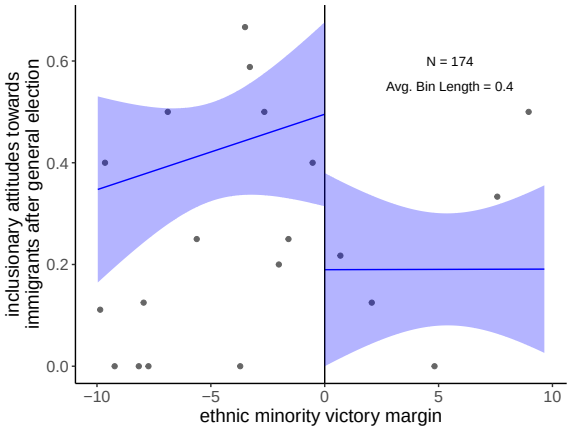


Figure J.5: Isolating candidates' ethnic identity from their political party in the attitudinal response

Notes: Lines represent the average proportion of respondents who do not think that "too many immigrants have been let into the country" (with 95% confidence intervals) from local linear regression with covariate adjustment (including party dummies) fitted to the sample of units whose vote-share winning margin is within the MSE-optimal bandwidth of +/- 10.3 percentage points around the victory threshold. Points are the average proportion of respondents who do not think that "too many immigrants have been let into the country" for equally spaced mimicking-variance bins.

J.6 Additional attitudinal outcomes

To validate the robustness of our results beyond our main attitudinal outcome (provided in tabular form in Table J.2), we compute two additional outcomes that use all other available survey items on attitudes towards immigration and ethnic minorities. The first outcome, *economy*, is an item that asks survey respondents whether immigrants are good for Britain's economy. This item is included in all survey years, but the wording of questions and answers (and their range) changes across time. The second outcome is an *index* that includes stereotypical beliefs about immigrants and attitudes towards accommodating diversity. However, this items are only included for a subsample of 60% of those who answered the 2017, 2019 surveys. In Table J.3 we present the effect estimates on these two additional attitudinal outcomes discussed further in Appendix C. We include as well the estimates on our main outcome as benchmark.

Table J.2: Ethnic minority victory effects on mass inclusionary attitudes towards immigrants

RD estimate	se	p-value	95% CI	mean control	sd effect	MSE-opt bw	eff. N	N	cov	smpl
-0.255	0.065	0.000	$[-0.426, -0.150]$	0.434	-0.562	14.42	291	1924	no	f
-0.258	0.065	0.000	$[-0.425, -0.150]$	0.440	-0.560	15.14	288	1876	no	c
-0.295	0.052	0.000	$[-0.428, -0.210]$	0.445	-0.646	14.33	283	1876	yes	c

Notes: The dependent variable is a dummy indicating whether a survey respondent *do not* thinks that "too many immigrants have been let into the country". *RD estimate* is computed with local-linear regression within a symmetric MSE-optimal bandwidth. *se* is the conventional standard error, *p-value* and *95% CI* are robust bias-corrected. *mean control* indicates the average proportion of respondents who *do not* think that "too many immigrants have been let into the country" in constituencies where ethnic minorities barely lose. *sd effect* presents the RD estimate in standard deviations, *MSE-opt bw* is the MSE-optimal bandwidth of vote-share winning margin around the victory threshold, *eff. N* is the sample size within the MSE-optimal bandwidth and *N* is the sample size. *cov* is a vector of controls including an indicator of whether the candidate is the incumbent, whether the survey respondent is male, young, single, employed, owns a house, and the constituency vote share for UKIP and BNP in the previous election, share that is foreign born, and share of households with 3 or more deprivations. *smpl* is the used sample: *f* stands for full sample and *c* for a complete cases sample with no missing values for respondent's predetermined variables. Standard errors are clustered by constituency-election. Survey data are from the British Election Study, ethnic background of candidates is constructed by the authors, and constituency characteristics from 2001 and 2011 UK Decennial Census.

Table J.3: Ethnic minority victory effects on mass attitudes towards immigrants

RD estimate	se	p-value	95% CI	mean control	sd effect	MSE-opt bw	eff. N	N	out	cov	smpl
-0.332	0.118	0.003	$[-0.640, -0.134]$	3.438	-0.252	11.69	239	2111	economy	no	f
-0.371	0.120	0.001	$[-0.678, -0.167]$	3.468	-0.284	11.39	233	2058	economy	no	c
-0.479	0.063	0.000	$[-0.645, -0.380]$	3.336	-0.367	8.05	133	2058	economy	yes	c
-0.255	0.065	0.000	$[-0.426, -0.150]$	0.434	-0.562	14.42	291	1924	entry	no	f
-0.258	0.065	0.000	$[-0.425, -0.150]$	0.440	-0.560	15.14	288	1876	entry	no	c
-0.295	0.052	0.000	$[-0.428, -0.210]$	0.445	-0.646	14.33	283	1876	entry	yes	c
-0.145	0.201	0.510	$[-0.582, 0.289]$	2.225	-0.157	18.83	170	899	index	no	f
-0.187	0.203	0.408	$[-0.624, 0.253]$	2.263	-0.202	18.70	161	865	index	no	c
-0.092	0.186	0.754	$[-0.453, 0.329]$	2.274	-0.099	15.66	145	865	index	yes	c

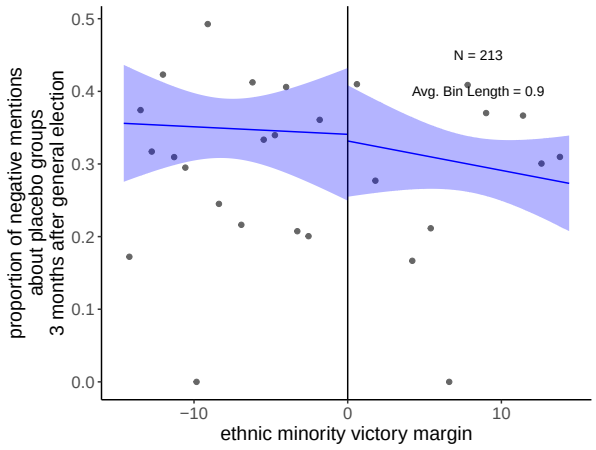
Notes: The dependent variable is indicated by *out*: *economy* is respondent's agreement with the statement "immigration is good for Britian's economy" on a 5-point Likert scale, *entry*, which is our main outcome of interest and is included here as benchmark, is a dummy indicating whether a survey respondent *do not* thinks that "too many immigrants have been let into the country", and *index* aggregates agreement with five statements about immigrants and ethnic minorities; higher values indicate more inclusionary attitudes. *RD estimate* is computed with local-linear regression within a symmetric MSE-optimal bandwidth. *se* is the conventional standard error, *p-value* and *95% CI* are robust bias-corrected. *mean control* indicates the average proportion of respondents who *do not* think that "too many immigrants have been let into the country" in constituencies where ethnic minorities barely lose. *sd effect* presents the RD estimate in standard deviations, *MSE-opt bw* is the MSE-optimal bandwidth of vote-share winning margin around the victory threshold, *eff. N* is the sample size within the MSE-optimal bandwidth and *N* is the sample size. *cov* is a vector of controls including an indicator of whether the candidate is the incumbent, whether the survey respondent is male, young, single, employed, owns a house, and the constituency vote share for UKIP and BNP in the previous election, share that is foreign born, and share of households with 3 or more deprivations. *smpl* is the used sample: *f* stands for full sample and *c* for a complete cases sample with no missing values for respondent's predetermined variables. Standard errors are clustered by constituency-election. Survey data are from the British Election Study, ethnic background of candidates is constructed by the authors, and constituency characteristics from 2001 and 2011 UK Decennial Census.

K Media tone towards migrant groups: validity of the RD design, robustness checks and supporting results

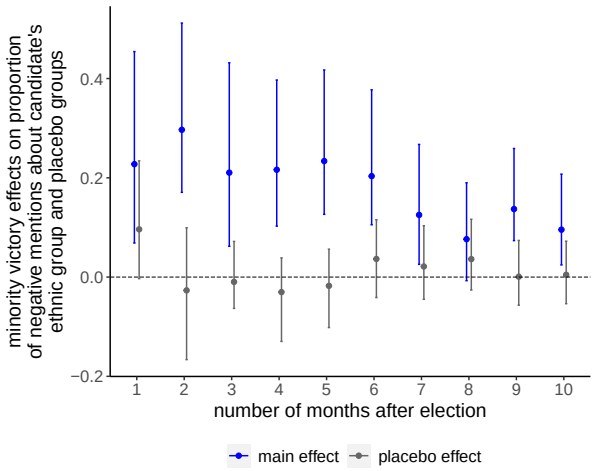
We report placebo and falsification tests that establish the validity of the RD design and the robustness of our results (sections K.1–K.6), and the main RD results in tabular form (section K.7).

K.1 Continuity of placebo outcomes

We use as a placebo measure the tone of news article mentions about countries and nationalities from North America, Western Europe, Australia and New Zealand that co-occur with mentions about the candidate's constituency. The placebo outcome is thus the monthly ratio of negative mentions to total mentions about these countries and nationalities in the candidate's constituency. In Figure K.1a we illustrate the RD estimates of the effect of a minority win on this placebo outcome three months from the election, and in Figure K.1b we present the estimates across months after the election, and we compare them to the estimates of the effects on media tone about the candidate's ethnic group (our main outcome variable). Both figures show no discontinuity in the tone of mentions about countries and nationalities from North America, Western Europe, Australia and New Zealand at the threshold where minorities win political office, suggesting that the validity of the design holds.



(a) Effect on proportion of negative mentions about placebo groups



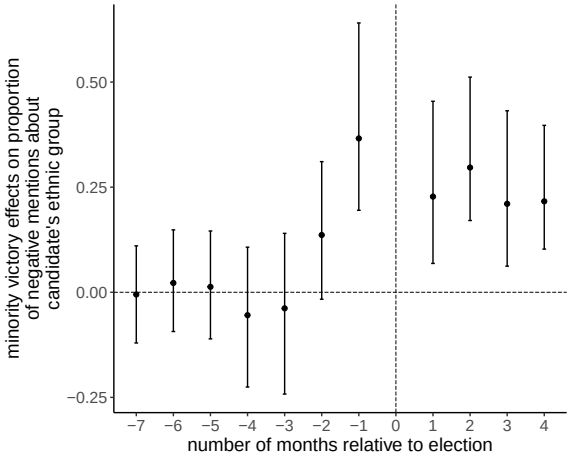
(b) Comparison of main and placebo effects

Notes: In (a) lines represent monthly proportion of negative mentions about countries and nationalities from North America, Western Europe, Australia and New Zealand in the candidate's constituency (with 95% confidence intervals) from local linear regression with covariate adjustment fitted to the sample of units whose vote-share winning margin is within the MSE-optimal bandwidth of +/- 12.1 percentage points around the victory threshold. Points are the average monthly proportion of negative mentions about countries and nationalities from North America, Western Europe, Australia and New Zealand in the candidate's constituency for equally spaced mimicking-variance bins. In (b) points are RD estimates of the effect of an ethnic minority victory and lines 95% robust bias-corrected confidence intervals.

Figure K.1: Ethnic minority victory effects on media tone of placebo groups

K.2 Continuity of main outcome before general election

We test whether the proportion of negative mentions about a candidate's ethnic group is discontinuous at the minority victory threshold before the general election. We find no discontinuities at the threshold before the election—the estimates of the effect of a minority win are centered around zero (and are not statistically significant)—except for two months before the election; when there is a jump at the threshold in the proportion of negative mentions about the winner's ethnic group. Such an increase however, is only distinguishable from zero one month prior to the election (Figure K.2). Campaigns officially begin with the dissolution of Parliament, which is about one month and a half prior to the election. It is possible that there is an anticipatory reaction from the media to minorities winning a seat in Parliament, as the media is more informed than the general public. It is also possible that the media responds to minority candidacies with a more negative coverage of candidates who are more likely to win, with the objective of affecting the election results. Overall, this placebo test increases our confidence about the robustness of our results. It suggests that the estimates of the minority victory effects on media tone about a candidate's ethnic group are explained by the election and not by other dynamics in constituencies where minorities win.

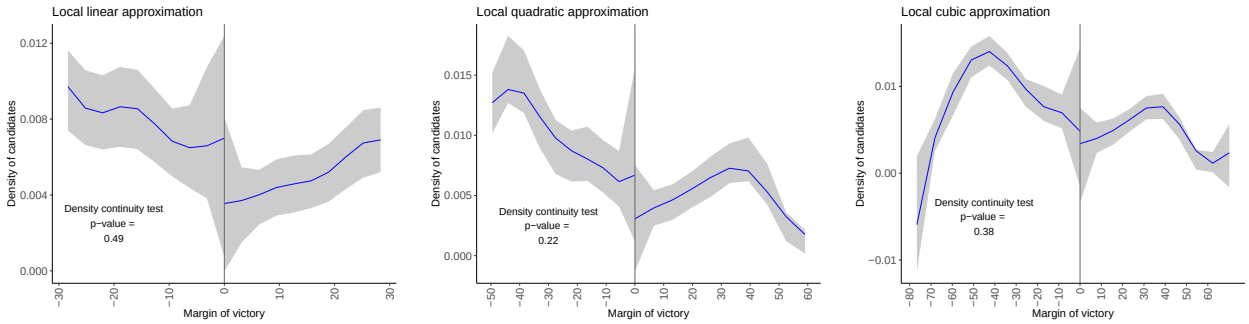


Notes: Points are RD estimates of the effect of an ethnic minority victory and lines 95% robust bias-corrected confidence intervals.

Figure K.2: Ethnic minority victory effects on media tone before and after the election

K.3 Density of the running variable

Figure K.3 reveals no evidence of sorting around the cutoff. Even though there appears to be a jump in the density functions of candidates at the threshold in which constituencies go from electing a dominant group candidate to electing a minority candidate, the confidence intervals of these density functions completely overlap and the p-value of the continuity test indicates that we cannot reject the null of continuity of the density functions. In addition, the p-value for the (McCrary, 2008) sorting test is 0.82, indicating that we cannot reject the null hypothesis of continuity of the density of candidates at the threshold. The results of these tests indicate no manipulation of the election results.

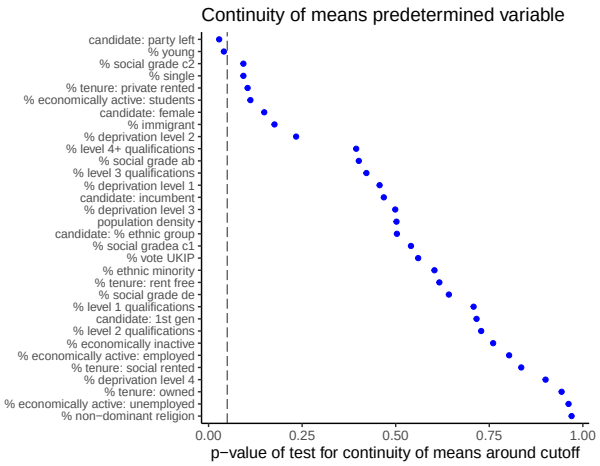


Notes: Tests for manipulation of the election results by assessing continuity of the density functions at the cutoff with local polynomial density estimators and robust bias-corrected inference.

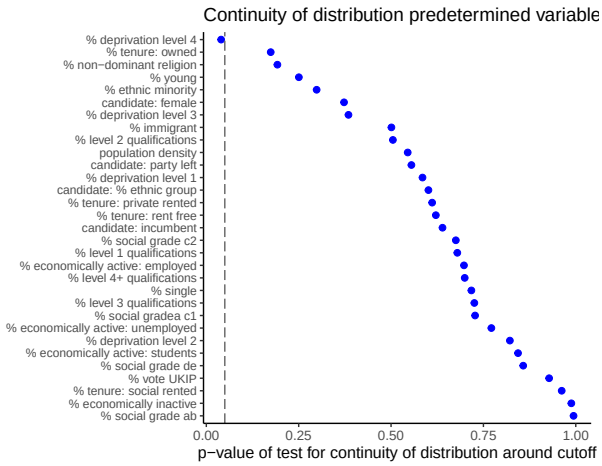
Figure K.3: Continuity in the density of candidates around the cutoff

K.4 Continuity of predetermined variables

In Figure we present results for the tests on the continuity of predetermined variables around the threshold where minority candidates win a seat in Parliament. We find that 2 of a total of 32 covariates show statistically significant discontinuities in *means* with the test employing local linear regression within an MSE-optimal bandwidth (Figure K.4a). Furthermore, controlling for the FDR with the Benjamini–Hochberg procedure we do not find discontinuous variables. Moreover, with the permutation test for continuity in the *distribution* of observations around the cutoff, we find that only 1 of the 31 predetermined variables are discontinuous at the cutoff, and zero when we control the FDR with the Benjamini–Hochberg procedure (Figure K.4b). This number of discontinuous covariates is equivalent to the average number of false rejections (which is 1.55). The results from both tests suggest that there were no systematic discontinuities in the covariates at the threshold were minorities win political office, and that therefore the continuity assumption of the potential outcome functions is likely to hold.



(a) Continuity of means using local linear regression



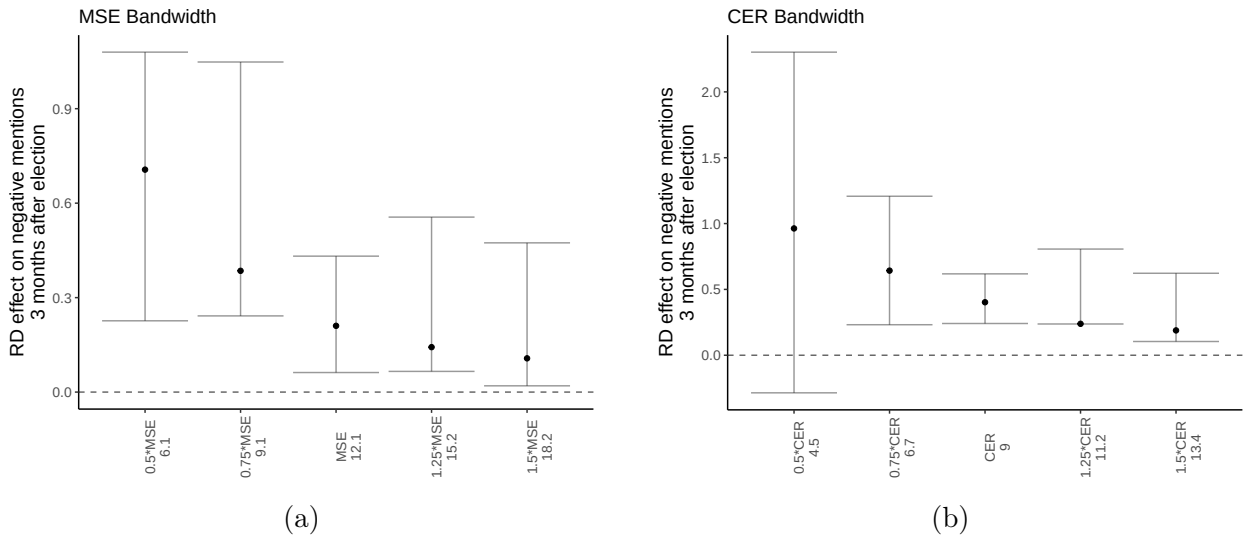
(b) Continuity of distribution using asymptotic permutation test

Notes: Test for continuity of candidate and constituency predetermined background characteristics in (a) using a local linear regression with a symmetric MSE-optimal bandwidth as implemented by the `rdrobust` R package and in (b) using an asymptotic permutation test comparing the distribution of observation near the cutoff as implemented by the `RATest` R package. The vertical line indicates p-value = 0.05. The threshold for the p-values when controlling the false discovery rate with the Benjamini–Hochberg procedure is zero.

Figure K.4: Continuity of predetermined variables around the cutoff

K.5 Sensitivity to the choice of bandwidth and order of polynomial

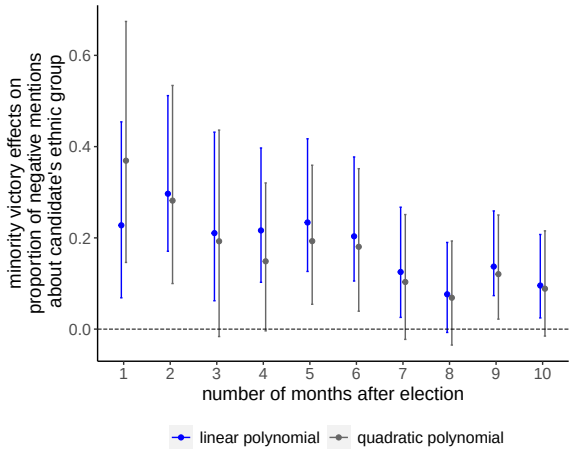
In Figure K.5 we test for sensitivity of the results to the choice of bandwidth, using CER- and MSE-optimal bandwidths, three fourths, half, five fourths, and one half their size. We find that the results are consistent with the findings obtained with the optimal MSE bandwidth —there is an increase in the proportion of negative mentions about a candidate's ethnic group at the victory threshold.



Notes: tests for sensitivity to the choice of bandwidth. In (a) MSE stands for mean squared error optimal bandwidth and in (b) CER refers to a bandwidth that minimizes the coverage error from the robust biased corrected confidence intervals obtained with the MSE-optimal bandwidth. The values next to the 'MSE', 'CER', labels indicate the bandwidth size.

Figure K.5: Sensitivity to bandwidth size

In Figure K.6 we show that the results are robust to fitting quadratic polynomials. These two results strengthen the validity of our findings on media tone.



Notes: tests for sensitivity to the choice of polynomial order by comparing estimates with local-linear and quadratic polynomials.

Figure K.6: Sensitivity to order of polynomial

K.6 Controlling for candidate's political party

We further isolate the ethnic identity of candidates from their political party affiliation from the media negative coverage response by controlling for party indicator variables. In Figure K.7 we compare the RD estimates of our main specification to the estimates from a specification controlling

for party dummies. The coefficients are very close in magnitude, suggesting that the media response is not explained only by the political affiliation of the candidates.

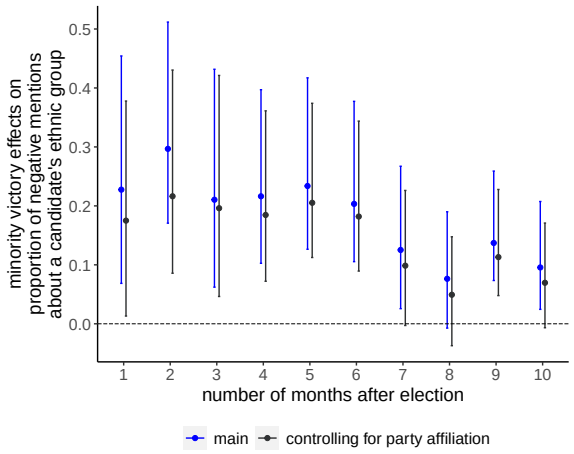


Figure K.7: Isolating the ethnic identity of candidates from their political party

Notes: Points are RD estimates of the effect of an ethnic minority victory on hate crimes per 1000 residents and lines 95% robust bias-corrected confidence intervals.

K.7 Main RD results in tabular form

Table K.1 displays all statistics of interest related to the estimates of the effect of a minority win on media tone.

L Explaining effects on media attention and tone

We assess whether there is an association between the political alignment of newspapers and the increase in speech about migrant groups with a specific valence (negative and positive). To do so, we classify the newspapers into right-wing or not right-wing using *Wordscores* (Laver, Benoit and Garry, 2003) (as implemented by the R package `quanteda)` with 2017 party manifestos as reference texts and expert surveys as exogenous scores. The party manifestos are from Burst et al. (2020) and the expert surveys from Norris (2020). The party scores are the average value of experts' party placements on economic and social issues. We consider that all newspapers with computed scores to the right of the most left-leaning self-identified right-wing newspaper are right-wing. This classification has an accuracy of 73%, measured against newspaper self-identification, which we extract from Wikipedia infoboxes, and is available for 22/156 newspapers.

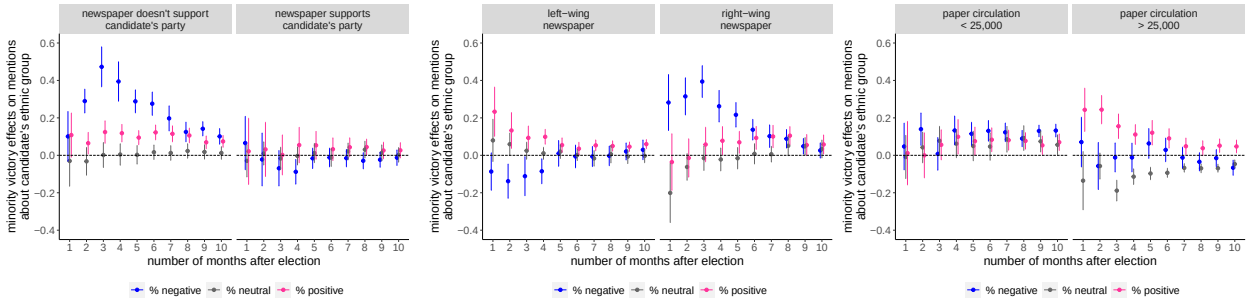
In Figure L.1a we present the RD estimates of the effects of a minority win on valence of migrant groups for newspapers that support a candidate's party (based on political alignment) and non-supportive papers. These estimates suggest that the negative mentions are indeed driven by newspapers that do not support the parties —the minority victory effects on the proportion of negative mentions are bigger for mentions from non-supportive newspapers than supportive newspapers— but the coefficients also suggest that non-supportive newspapers contribute with the positive mentions. Furthermore, when we compute the minority win effects for right- and left-wing newspapers (Figure L.1b), we find evidence that the increase in negative mentions is mostly driven by right-wing

Table K.1: Ethnic minority victory effects on media tone about migrant groups

RD estimate	se	p-value	95% CI	mean control	sd effect	MSE-opt bw	eff. N	N	cov	month
0.080	0.106	0.331	[−0.104, 0.309]	0.189	0.269	24.77	142	438	no	1
0.228	0.098	0.008	[0.069, 0.454]	0.115	0.998	14.37	70	438	yes	1
0.123	0.097	0.078	[−0.019, 0.362]	0.135	0.417	14.34	138	876	no	2
0.297	0.087	0.000	[0.171, 0.512]	0.025	1.088	10.53	92	876	yes	2
0.102	0.101	0.165	[−0.058, 0.339]	0.217	0.321	16.44	240	1314	no	3
0.210	0.094	0.009	[0.062, 0.432]	0.193	0.674	12.13	165	1314	yes	3
0.121	0.072	0.030	[0.015, 0.297]	0.210	0.393	14.57	284	1752	no	4
0.216	0.075	0.001	[0.103, 0.397]	0.183	0.724	11.65	208	1752	yes	4
0.144	0.075	0.016	[0.033, 0.329]	0.176	0.474	13.42	320	2190	no	5
0.234	0.074	0.000	[0.126, 0.417]	0.159	0.781	10.69	235	2190	yes	5
0.124	0.076	0.040	[0.007, 0.305]	0.169	0.419	14.59	432	2628	no	6
0.203	0.069	0.001	[0.105, 0.377]	0.163	0.699	11.89	318	2628	yes	6
0.065	0.067	0.195	[−0.045, 0.219]	0.189	0.223	16.51	567	3066	no	7
0.125	0.062	0.017	[0.026, 0.267]	0.180	0.428	14.53	497	3066	yes	7
0.043	0.056	0.258	[−0.046, 0.173]	0.196	0.145	16.45	648	3504	no	8
0.076	0.050	0.070	[−0.007, 0.190]	0.197	0.255	16.60	656	3504	yes	8
0.068	0.057	0.111	[−0.021, 0.201]	0.177	0.227	15.41	693	3942	no	9
0.137	0.047	0.000	[0.073, 0.259]	0.169	0.463	14.23	612	3942	yes	9
0.042	0.054	0.267	[−0.046, 0.167]	0.193	0.140	16.79	830	4380	no	10
0.096	0.047	0.013	[0.024, 0.207]	0.186	0.318	15.61	770	4380	yes	10

Notes: The dependent variable is the monthly proportion of negative mentions in news articles about a candidate's ethnic group. *RD estimate* is computed with local-linear regression within a symmetric MSE-optimal bandwidth. *se* is the conventional standard error, *p-value* and *95% CI* are robust bias-corrected. *mean control* indicates the average proportion of negative news article mentions about the barely losing candidate's ethnic group. *sd effect* presents the RD estimate in standard deviations, *MSE-opt bw* is the MSE-optimal bandwidth of vote-share winning margin around the victory threshold, *eff. N* is the sample size within the MSE-optimal bandwidth and *N* is the sample size. *cov* is a vector of controls including whether the candidate is the incumbent, from a left-leaning party, a woman, a first-generation immigrant, the constituency vote share for UKIP and BNP in the previous election, constituency share that shares the candidate's ethnic background, shares of foreign born, with a minority religion, young population, single, with level 1 qualifications, with social grade DE, unemployed, and share of households with 4 or more deprivations, and in social tenure. Standard errors are clustered by constituency-election. News articles were extracted from Common Crawl, ethnic background of candidates is constructed by the authors, and constituency characteristics from 2001 and 2011 UK Decennial Census.

newspapers, and that at least for the first quarter after the election, left-wing newspapers contribute the most to the increase in positive mentions about a candidate's ethnic group. Moreover, the estimates of the RD effects of a minority win on the tone of newspapers by their circulation (above or below 25,000 copies), suggest that during the first months after the election the positive mentions about a winning candidate's ethnic group are contributed by papers with a circulation of more than 25,000 copies, while smaller papers drive the negative mentions (Figure L.1c).



(a) Effects by newspaper-party political alignment

(b) Effects by newspaper ideology

(c) Effects by newspaper circulation

Notes: Points are RD estimates of the effect of an ethnic minority victory and lines 95% robust bias-corrected confidence intervals.

Figure L.1: Ethnic minority victory effects on media tone by newspaper-party political alignment, paper ideology, and circulation

Table L.1: Minority victory effects across media valence categories

month	(negative - positive)	(negative - neutral)	(positive - neutral)
1	0.76	2.50	1.72
2	1.90	2.83	0.62
3	0.78	1.68	1.25
4	0.44	2.11	1.96
5	0.78	1.88	1.44
6	0.84	1.41	0.88
7	-0.33	0.75	1.29
8	-1.05	-0.03	0.91
9	0.59	1.00	0.55
10	-0.14	0.63	0.87

Notes: Values indicate the t-statistic of the difference between the RD estimates of the effects of a minority win on the proportion of negative, positive, and neutral mentions about a candidate's ethnic group in the media. Values larger than the critical value of 1.96 are statistically significant.

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