

Part

Online Supplementary Information

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

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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 ward's population 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.

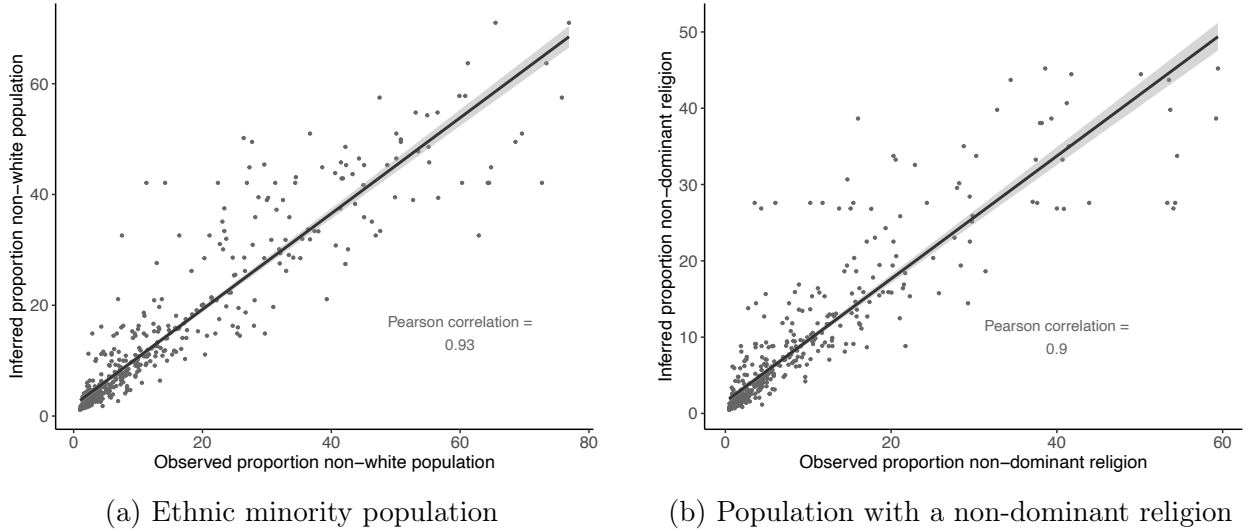


Figure B.1: Validity of hate crime assignment from LAD to constituency

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 URLs for 337 UK newspapers.

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.

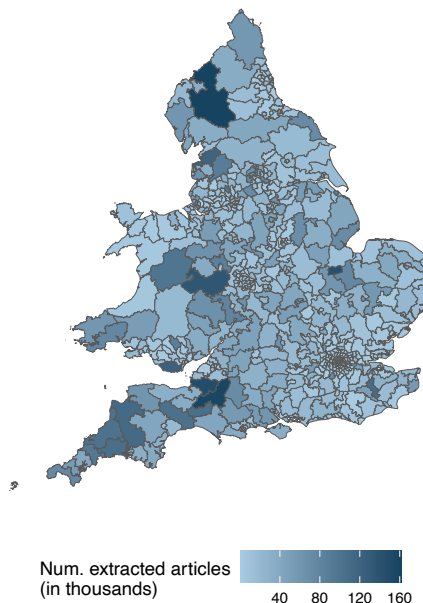


Figure D.1: Geographical coverage of all extracted articles

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 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.

To provide a sense of coverage by the subset of articles used in our main outcome variable, we present the 160 most frequent words in Table D.1. Naturally, coverage of the COVID-19 pandemic is prevalent (e.g. mentions of coronavirus, health, staff), among other topics such as sports and local politics (e.g. mentions of schools, students, police, government, workers, businesses, local council).

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

Table D.1: Top 160 words in relevant articles subset

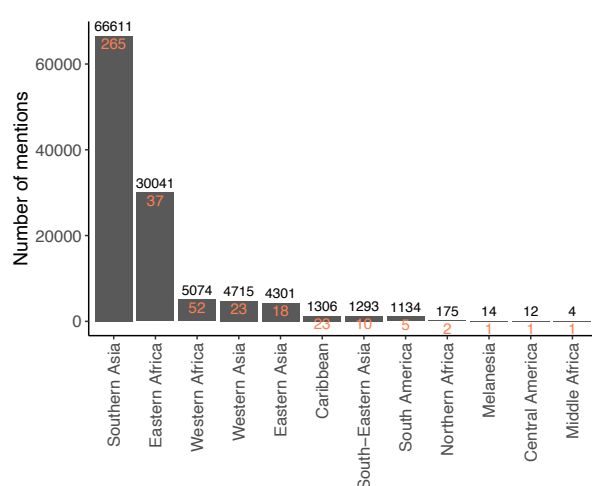
word	frequency	word	frequency	word	frequency	word	frequency
coronavirus	290911	court	78820	food	57489	war	45354
people	238666	service	76675	going	56524	local	45147
said	238024	university	74724	high	56135	council	44787
year	230400	place	74134	team	55430	season	44709
new	209639	house	72897	west	55208	don	44271
children	198152	leicester	72152	hospital	54240	care	44127
years	170559	support	69967	information	54093	bristol	44078
uk	167058	away	69602	group	53670	confirmed	43993
city	164893	pay	69041	months	53295	john	43908
home	144300	business	68589	aged	52895	took	43855
staff	143187	way	68086	ve	52678	open	43766
time	142772	covid	68054	women	52575	right	43659
world	135368	number	67629	park	52069	area	43496
day	119681	south	67255	play	51929	times	43183
just	116498	cases	67210	east	50953	learning	43183
family	113189	students	67034	night	50908	want	42692
old	109395	workers	66969	north	50841	got	42686
road	104790	data	66363	read	50606	self	42163
life	102007	left	65938	jailed	50585	big	42044
like	101473	told	65808	address	50340	close	41996
image	96191	country	65794	free	50322	come	41967
use	95686	england	65739	email	50030	lockdown	41945
week	95588	including	65679	today	49435	say	41870
police	95176	centre	65481	national	49318	town	41563
work	94866	public	64495	death	49276	travel	41529
health	91352	february	63624	funeral	48949	event	41257
street	91160	january	63237	working	48934	sports	41185
make	89190	latest	62903	mr	48736	second	41170
march	88325	great	62114	nhs	48561	crown	40980
london	86030	days	61933	long	48332	company	40686
good	85488	live	61473	know	48259	million	40601
plymouth	83783	student	61024	continue	48007	guilty	40354
government	83491	club	60804	following	47756	young	40257
says	82081	league	60478	international	47120	went	39950
news	82037	set	59725	crisis	47064	community	39903
british	81802	st	59446	social	46710	positive	39855
help	81497	virus	59362	china	46477	pandemic	39533
man	80788	royal	59141	end	46014	april	39403
school	79330	need	58468	think	45609	meet	39284
best	79065	love	58198	died	45545	money	39119

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, 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.

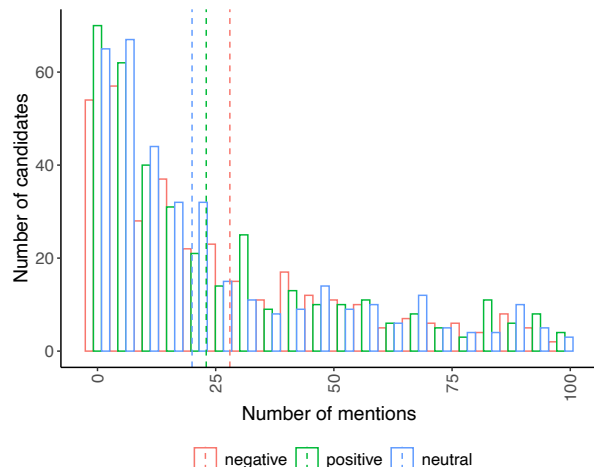
Following this matching process, we then compute the ratio between the number of negative mentions (adding together the ‘very negative’ and ‘negative’ sentiment categories) and the total number of mentions about the candidates’ sub-region of origin in their constituency, at every month after the general 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, for the 2010 election, the British General Election Constituency Results, 2010-2019, which contains the ethnicity of candidates running with the biggest three political parties: Conservative, Labour and Liberal Democrat. Based on data from the 2015 general election (which is the next closest election for which we have data on every candidate) we know that 76% of ethnic minority candidates stand in elections with one of these three parties. For the 2015 and 2017 general elections we rely on the Parliamentary Candidates UK project (van Heerde-Hudson and Campbell, 2015), which collected the ethnicity of every candidate standing in these elections with an established party (Labour, Conservative, Liberal Democrat, Scottish National, Plaid Cymru, UKIP, Green and Northern Ireland parties) 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



(a) Distribution of all mentions by sub-region



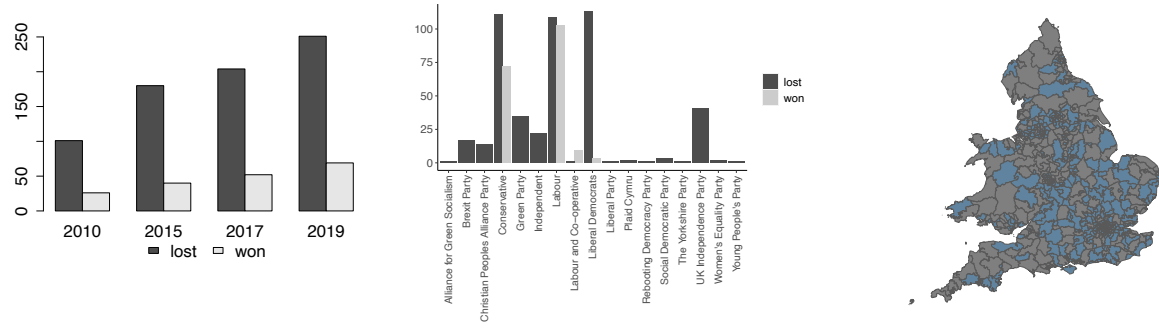
(b) Distribution of candidate mentions by sentiment category

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

by searching the profile of the more than 3,300 candidates and using information of candidates who have run before for 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. Figure E.1 describes 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

Figure E.1: Description of ethnic minority candidates

F Selection of constituencies into the sample

Table F.1: Selection of constituencies into the sample

variable	All constituencies		Sample constituencies	
	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 grade 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.

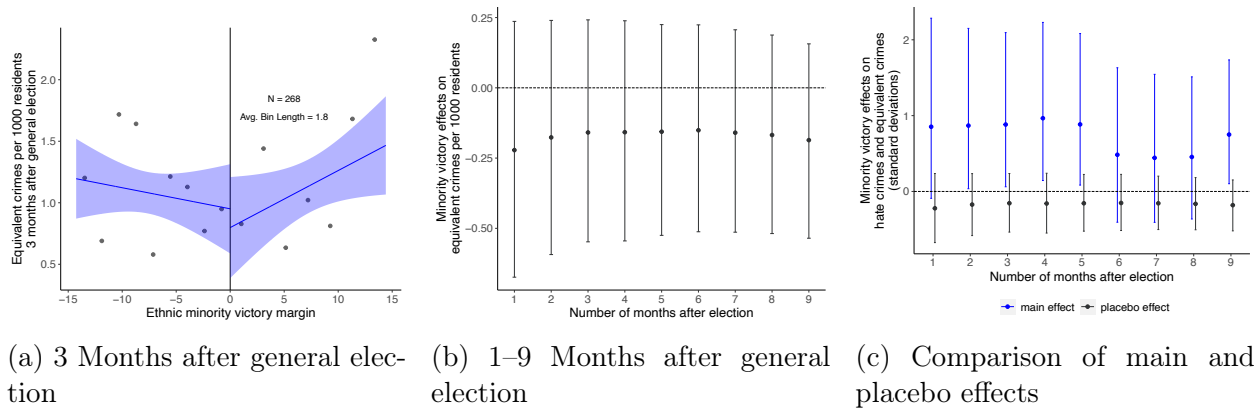
G Minority victory effects on hate crimes: validity of the RD design, robustness checks and supporting results

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

G.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 G.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 G.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.



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 G.1: Ethnic minority victory effects on equivalent crimes

G.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 G.2a 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.

G.3 Continuity of predetermined variables

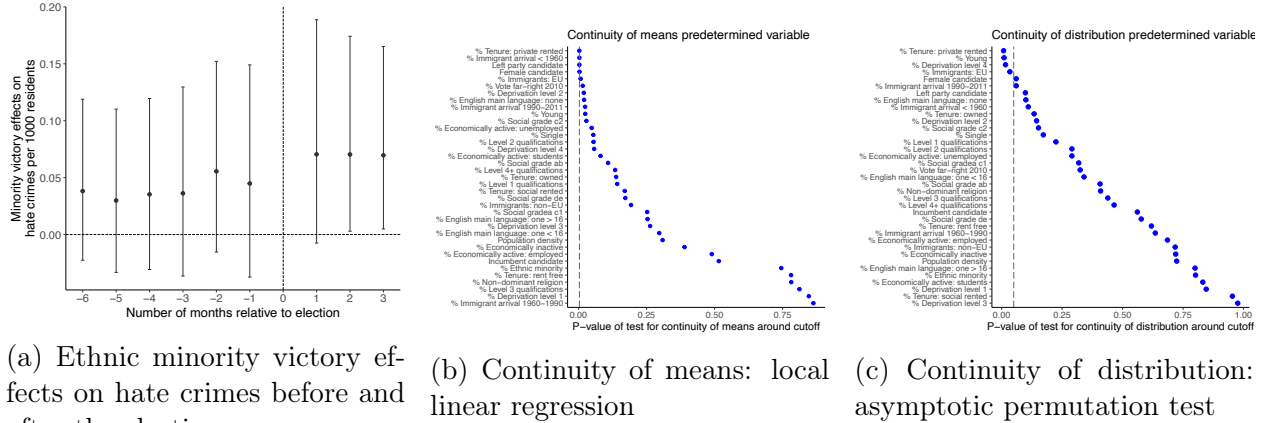
Following Calonico, Cattaneo and Titiunik (2014), we also test (using the `rdrobust` package in R) the continuity assumption for predetermined variables with local linear regression within an MSE-optimal bandwidth. Given that we have a large number of covariates, we show in Figure G.2b the threshold for the p-values of the tests of discontinuity (the dashed vertical line), when controlling the false discovery rate with the Benjamini–Hochberg procedure. (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). 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 G.2c). 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.

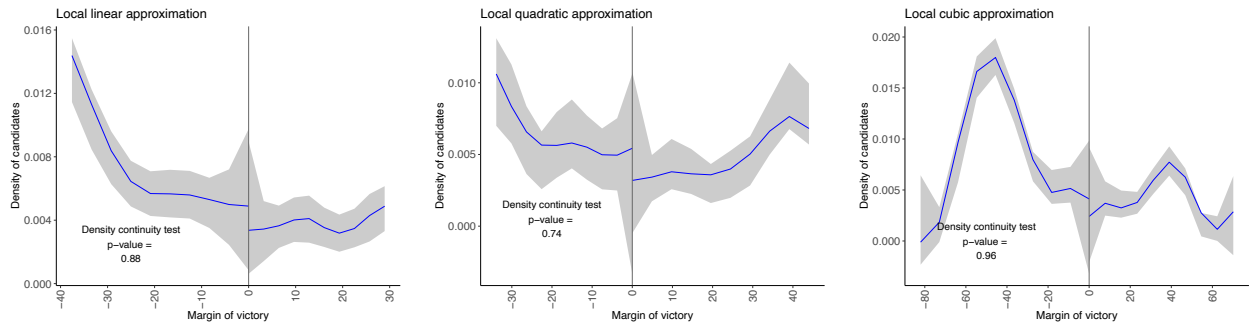
G.4 Density of the running variable

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 G.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: In (a) points are RD estimates of the effect of an ethnic minority victory and lines 95% robust bias-corrected confidence intervals. (b), (c) test for continuity of candidate and constituency predetermined background characteristics in (b) using a local linear regression with a symmetric MSE-optimal bandwidth as implemented by the `rdrobust` R package and in (c) using an asymptotic permutation test comparing the distribution of observation near the cutoff as implemented by the `RATest` R package. The vertical line in (b) 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 (c) a p -value = 0.05. Here the threshold for p -values when controlling the FDR with BH procedure is approximately 0.

Figure G.2: Continuity of predetermined variables around the victory threshold



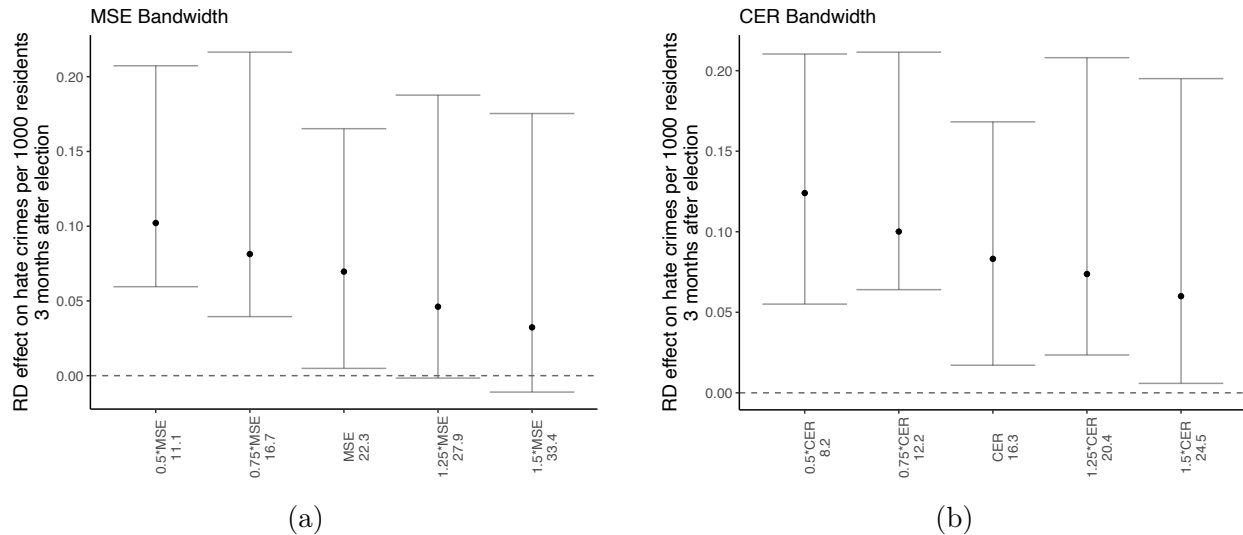
Notes: Tests for manipulation of the election results by assessing continuity of the candidate density functions at the cutoff with local polynomial density estimators and robust bias-corrected inference.

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

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

In Figure G.4 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.

Our main estimation method computes the RD estimates by fitting local-linear polynomials for a few reasons described in Cattaneo, Idrobo and Titiunik (2019): for a given bandwidth, using higher-order polynomials will generally improve the accuracy of the approximation but at the cost



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 G.4: Sensitivity to bandwidth size

of increasing the variability of the treatment effect estimator. Moreover, higher-order polynomials tend to produce overfitting of the data and lead to unreliable results near boundary points. Gelman and Imbens (2019), for example, argue that high-order polynomials can produce noisy estimates with poor coverage of confidence intervals. These reasons combined have made the choice of local-linear polynomials the modal among researchers, as in general, the local linear estimator seems to deliver a good trade-off between simplicity, precision, and stability. As is also common practice, we show in Figure G.5a that the results are robust to fitting quadratic polynomials.

G.6 Controlling for candidate's political party

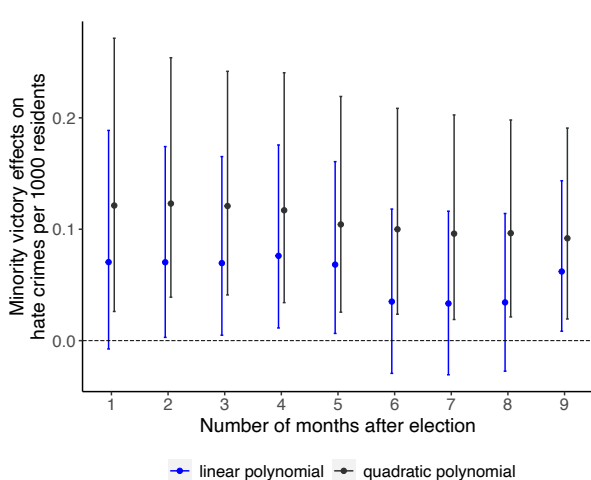
We isolate the ethnic identity of candidates from their political party affiliation from the hate crime response by controlling for party dummies. In Figure G.5b we compare the RD estimates of our main specification to the estimates from a specification controlling for party dummies. The coefficients are very close in magnitude, suggesting that the violent response is not explained only by the political affiliation of the candidates.

G.7 Main RD results in tabular form

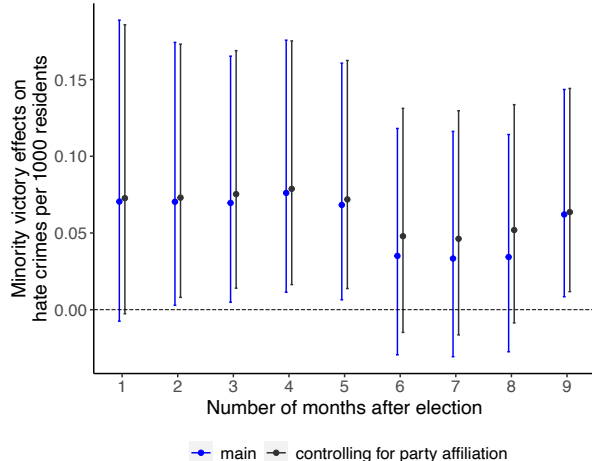
Table G.1 presents estimates of RD effects on hate crime for model specifications with/out covariates.

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

In Figure G.6 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



(a) Sensitivity to polynomial choice



(b) Isolating the ethnic identity of candidates from their political party

Notes: (a) tests for sensitivity to the choice of polynomial order by comparing estimates with local-linear and quadratic polynomials, and (b) adds controls for candidates' political parties.

Figure G.5: Sensitivity to polynomial choice and control variables

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.

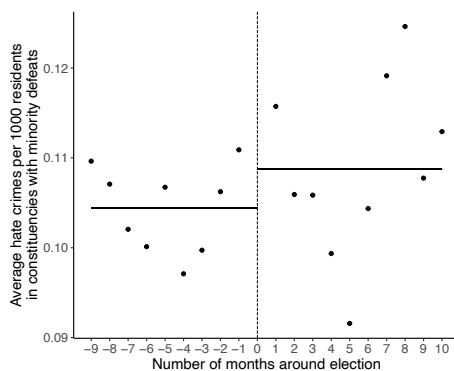


Figure G.6: No hate crime decay in minority barely lost constituencies

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.

Table G.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.

G.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.

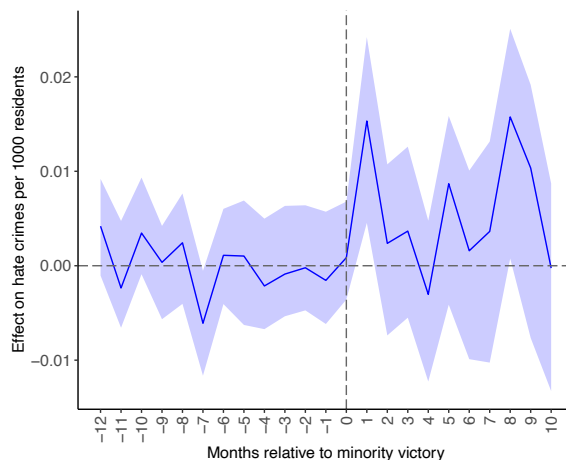
With this estimation design, a constituency is in the treatment condition during the months following a general election in which an ethnic minority candidate is elected, and in the control condition, otherwise. 69 out of 520 constituencies have an ethnic minority MP during at least one month between 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 inter-

active 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 0.0, 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 G.7 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 G.7: First time ethnic minority victory effects on hate crime

Table G.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

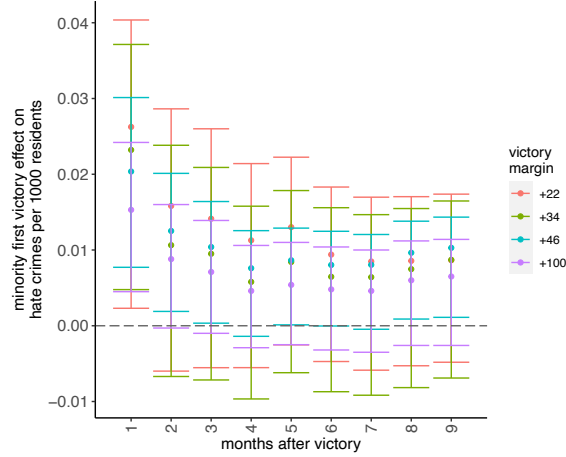
ATT.avg	S.E.	CI.lower	CI.upper	p.value	months
0.0153	0.0051	0.0045	0.0242	0.005	1
0.0088	0.0041	-0.0003	0.0160	0.058	2
0.0071	0.0038	-0.0010	0.0139	0.078	3
0.0046	0.0035	-0.0029	0.0106	0.268	4
0.0054	0.0035	-0.0025	0.0110	0.199	5
0.0048	0.0034	-0.0032	0.0104	0.287	6
0.0046	0.0034	-0.0035	0.0100	0.350	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 G.2: Average first time ethnic minority victory effects on hate crime (averaging across months after victory)

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 G.8 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 G.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.

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.



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

Figure G.8: First time ethnic minority victory effects on hate crime by victory margin

G.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 the reporting of hate crime: members of the winning candidate's ethnic group may be 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 (defined as race, color, nationality or ethnic or national origin), religion or beliefs, without further prove.

To further assess such a concern we estimate victory effects on hate crime categorized as 'violence against the person', specifically 'violence with injury'. Because of the seriousness of this offense, such type 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 G.3 are broadly consistent with our main findings on total hate crime: crimes jump at the minority victory threshold.

G.11 Brief discussion of hate crime spillover

It is possible that hate crime spills over across constituencies. Two types of spillover could be going on. First, constituencies with minority losers are in the neighborhood of constituencies with minority winners and hate crime spills over within neighborhoods. This scenario suggests our RD estimate is downward biased. Second, constituencies with minority winners are in the neighborhood of constituencies with minority winners and hate crime spills over within neighborhoods. This scenario suggests that our RD estimate accounts for a constituency's treatment and their neighbor's treatment (that is, a direct and an indirect effect of minority victory on crime).

Now, assuming that a neighborhood is characterized by a constituency's first-degree neighbors (that is, constituencies that are adjacent) and that spillover happens only via first-degree neighbors,

Table G.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 MSE-optimal 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. 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.

the probability of the first spillover scenario described above is 0.21 and of the second scenario 0.18. These probabilities are even lower, 0.11 and 0.07; respectively, when we account only for constituencies with narrow elections (a margin of victory of ± 22 percentage points), suggesting that it is not very likely there would be spillover, and if there is, it is more likely that our RD estimates are conservative. Figure G.9 presents maps illustrating the contiguity between constituencies with ethnic minority candidate winners and ethnic minority candidate losers in close elections (± 22 pp. around the cutoff).

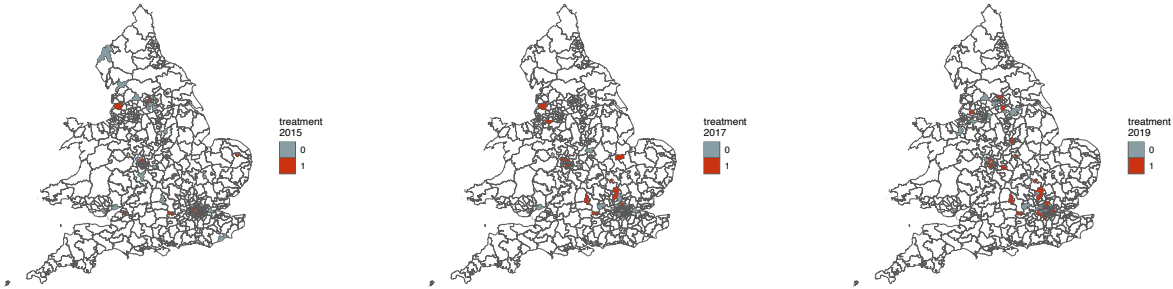
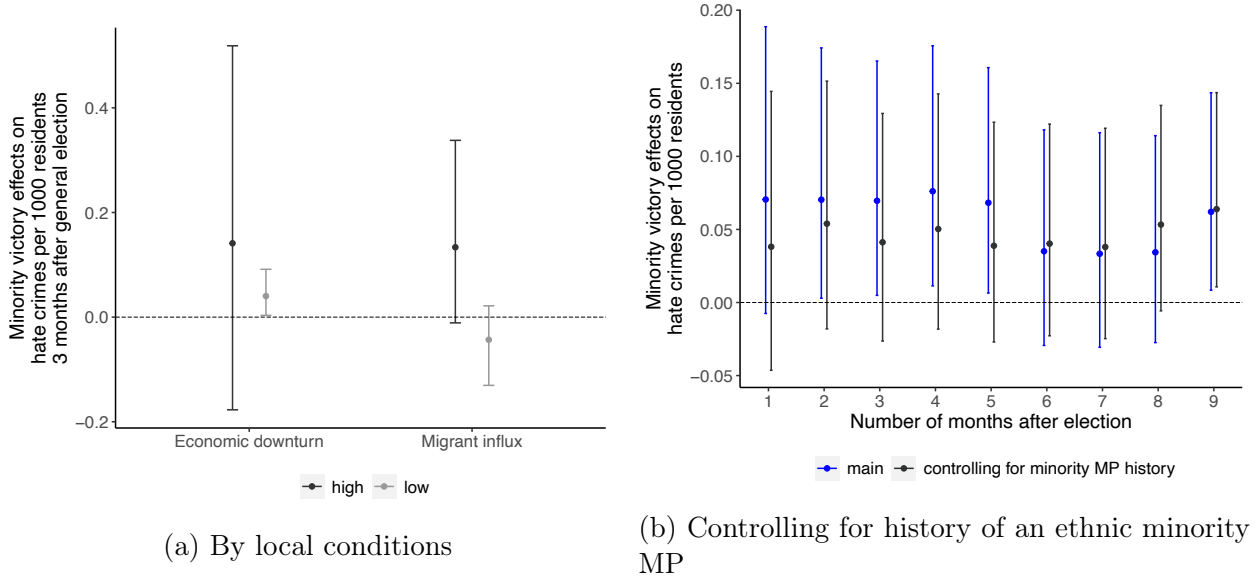


Figure G.9: Constituencies with ethnic minority candidate winners and losers in close elections

G.12 Subgroup effects on hate crime

We conduct five subgroup analyses. First, in Figure G.10a (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 G.10a, left side).

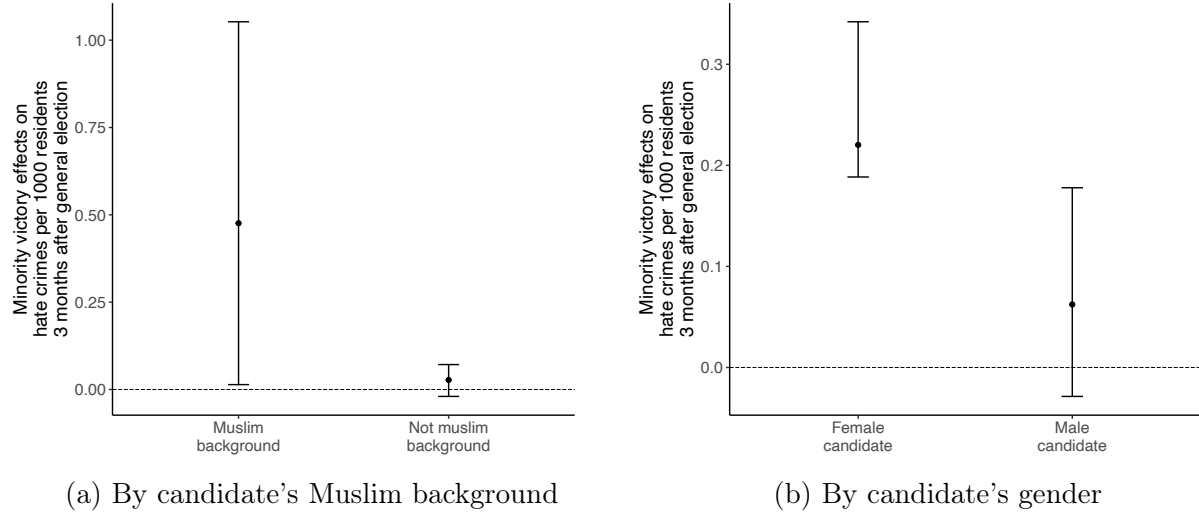


Notes: Points are RD estimates of the effect of an ethnic minority victory on hate crimes per 1000 residents and lines 95% robust bias-corrected confidence intervals.

Figure G.10: Subgroup effects on hate crime: local conditions and novelty

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-constituency-election years, for this analysis, we impute a candidate's religion based on their region of origin by assigning to each candidate the main religion in their region. 19% of candidates-constituency-election years (from 2010-2019) are classified as Muslim, and the rest are Christian, Buddhist, or Hindu. The results in Figure G.11a 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, the results in Figure G.11b suggest that consistent with threat, hate crime perpetrators (who are mostly White males) may be reacting not only to the symbolic and substantive representational threat posed by the political accession of ethnic minorities, but also to women entering historically men-dominated institutions, and their capacity to strengthen the position of women's interests.

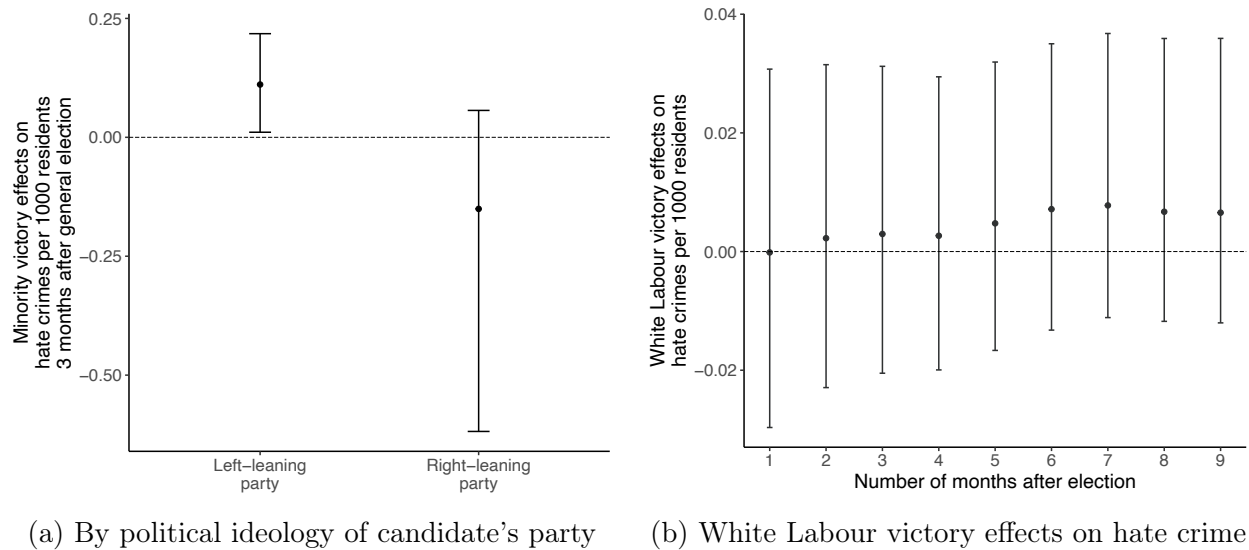
Fourth, in Figure G.12a we show that minority migrant victory only has a positive effect on hate crime incidence when the candidate hails from a left- but not a right-wing party. We further show in Figure G.12b that these effects are not driven by a candidate's political affiliation only, but by the 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



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

Figure G.11: Subgroup effects on hate crime: religion and gender

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. Fifth, in Figure G.10b 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 a minority candidate victory in (a) and of a White Labour candidate victory in (b), and lines 95% robust bias-corrected confidence intervals.

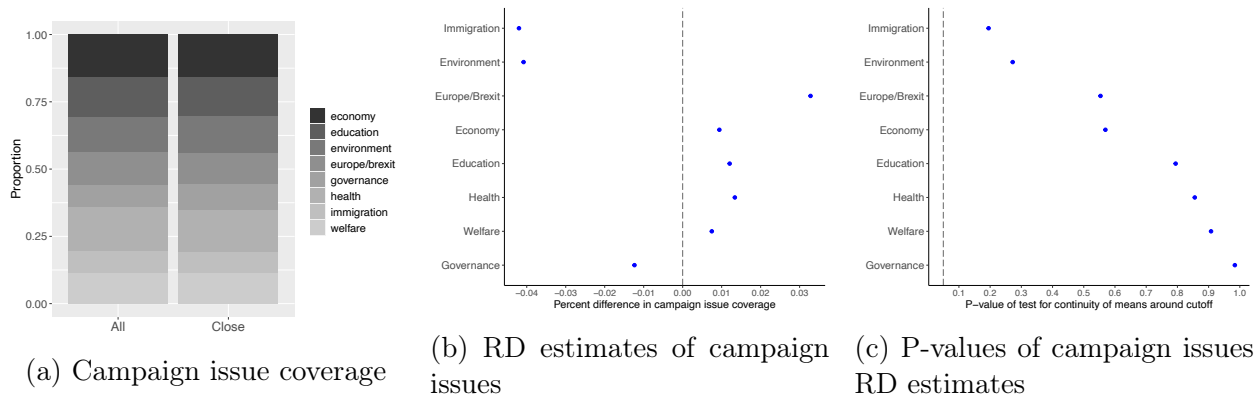
Figure G.12: Subgroup effects on hate crime: party ideology

H Analysis of political campaign issues

We argue that compared to non-competitive races, election loss in close races between ethnic minority and dominant-group candidates are more likely to provoke hostility, in part because political elites have stronger incentives to stoke communal grievances to mobilize voters. In this section we show that while the role of political elites in close elections may enhance hostility, the political campaigns themselves and the issues covered in the campaigns (e.g. immigration, Brexit) do not seem to be the mechanism connecting an ethnic minority victory to hostility against ethnic minority immigrants.

To provide data on the issues covered by the political campaigns in our sample and to inform our mechanism, we collected data of candidate’s campaign leaflets for the 2010, 2015, 2017 and 2019 general elections. This data is made available by [Open Elections](#). In addition to hosting an online archive of the leaflets, Open Elections evaluates the leaflets on whether they include references to policies related to the economy, immigration, health, social welfare, education, environment, Europe/Brexit, or matters related to governance. We rely on this categorization to first, show that the issue coverage of political campaigns in close elections is no different to the coverage in all elections in our sample. This is presented in Figure H.1a which compares across constituencies with close elections and all constituencies in our sample, the share of constituencies in which at least one candidate covers an issue. Constituencies with close elections (defined as constituencies with a minority victory margin of ± 22 percentage points, according to the MSE-optimal bandwidth from the hate crime analysis) campaign on immigration (or Europe/Brexit) at similar rates than all constituencies. Secondly, in Figure H.1b and Figure H.1c, we show that the political campaign issues are not discontinuously covered at the victory cutoff, that is, the topic of immigration or Europe/Brexit, for example, is similarly covered across constituencies narrowly won by an ethnic minority candidate and constituencies narrowly won by a dominant-group candidate: the RD estimates are close to zero (Figure H.1b) and the p-values of the estimates testing for a discontinuity are above the 0.05 significance threshold (Figure H.1c).

This analysis suggest that the discussion of immigration or Brexit during the political campaigns is not driving the exclusionary response to an ethnic minority victory, reinforcing the possibility that dominant-group members respond to status threat as advanced by our main argument.

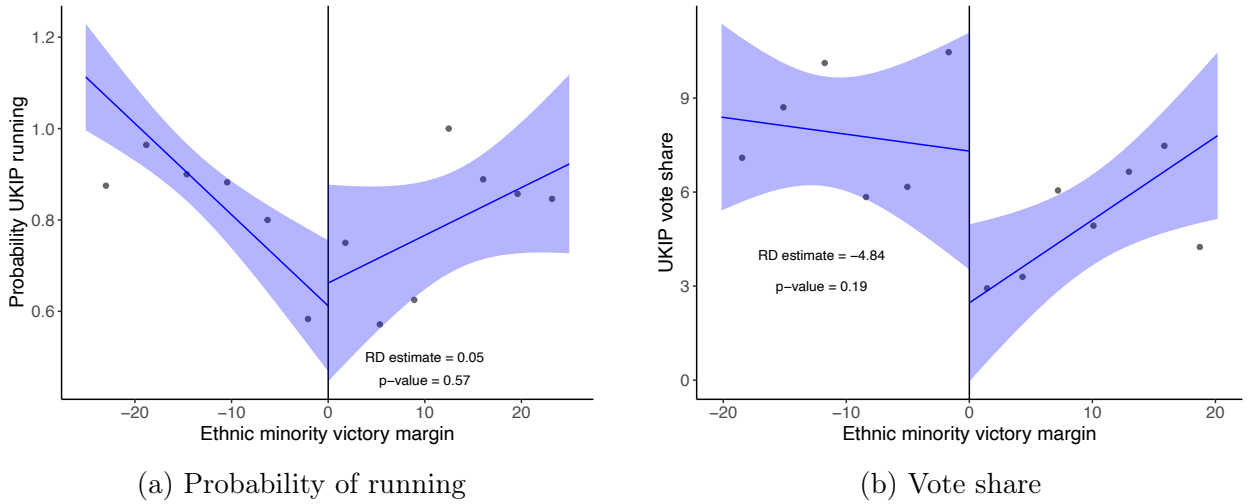


Notes: (b), (c) test for continuity of campaign issues using a local linear regression with a symmetric MSE-optimal bandwidth as implemented by the `rdrobust` R package. (b) shows RD estimates and (c) p-values of estimates.

Figure H.1: Continuity of campaign issues around the victory threshold

H.1 UKIP/Brexit Party electoral participation

We have shown above that campaign issues are equally covered in constituencies narrowly won and lost by ethnic minority candidates. However, it is still possible that in constituencies narrowly won by ethnic minority candidates UKIP/Brexit participates at higher rates than in constituencies narrowly lost by ethnic minority candidates. If so, our RD estimates would measure the bundled treatment of a minority victory and the role of UKIP in stoking an anti-immigrant sentiment during the campaigns. We assess such a possibility by looking at whether the probability of UKIP running in the elections and UKIP’s vote share is discontinuous at the victory cutoff. The results presented in Figure H.2a suggest that there is no discontinuity in the probability of UKIP participating in the elections: the RD-estimate is positive and not statistically significant. Furthermore, the RD-estimate on UKIP’s vote share is negative and not statistically significant (Figure H.2a).



Notes: In (a) lines are average UKIP’s participation rate (with 95% confidence intervals) from local linear regression. Points are average UKIP’s participation rates for equally spaced mimicking-variance bins. In (b) lines are average vote share (with 95% confidence intervals) from local linear regression. Points are average UKIP’s vote share for equally spaced mimicking-variance bins.

Figure H.2: Continuity of UKIP/Brexit Party electoral participation

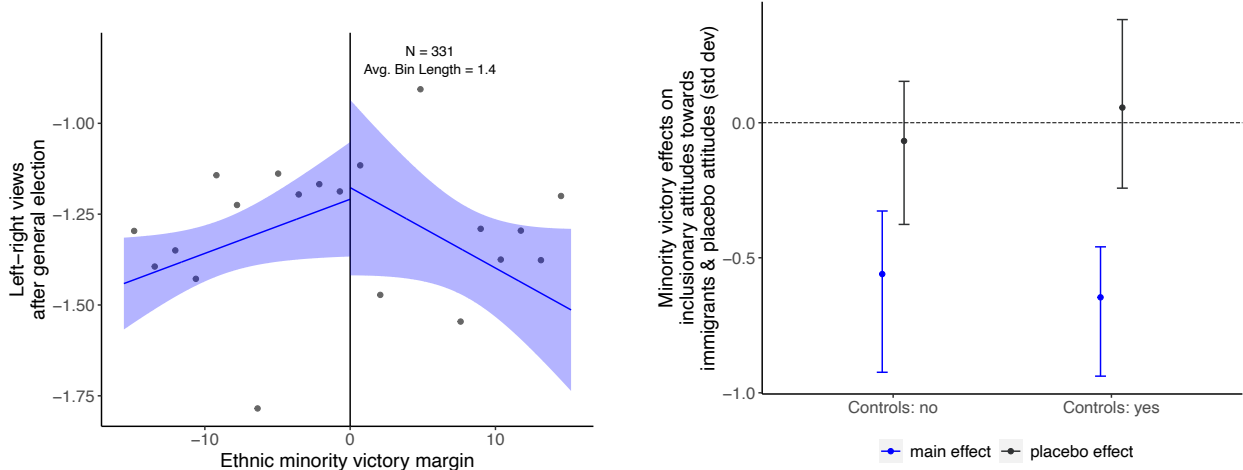
These results reassure us that the exclusionary responses we document here are responses to the election of a minority candidate, and orthogonal to UKIP’s political activism.

I 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 I.1–I.5), and the results’ robustness to alternative specifications (section I.6). We further report the main RD results in tabular form and the robustness of those results to alternative survey questions in section I.7.

I.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 I.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 pp 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.

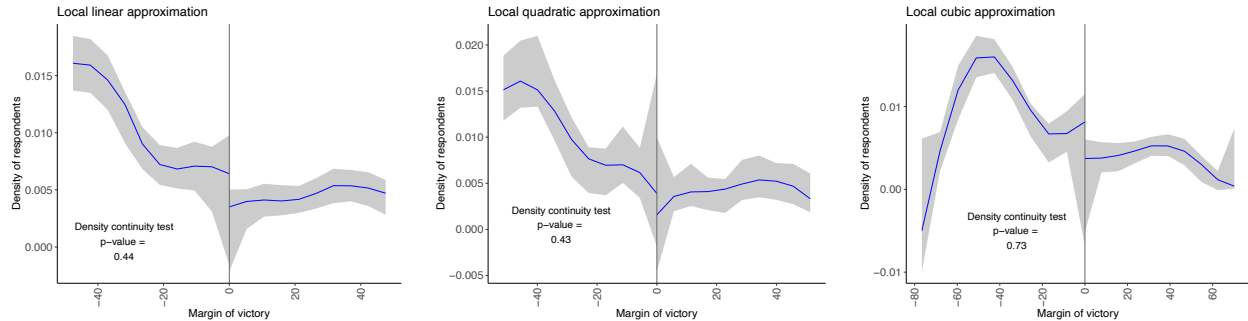
Figure I.1: Ethnic minority victory effects on ideology (placebo outcome)

I.2 Density of the running variable

Figure I.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.

I.3 Continuity of outcome before general election

We test whether attitudes toward immigrants are discontinuous at the margin of victory cutoff before the general election. To do so, we would ideally use our main outcome measure on attitudes



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

toward immigration entry policy. However, the face-to-face British Election Study (the data we use in the main analysis) does not include pre-election observations. Instead, we use the British Election Study Internet Panel, which includes pre-election waves. The only question on attitudes toward immigration/immigrants available in pre-election waves in 2015 (wave 3), 2017 (wave 11), and 2019 (wave 16) is: “*Do you think immigration is good or bad for Britain’s economy?*” on a 7-point scale from bad to good. Consistent with our 5-point scale post-election outcome measure we collapse the answer categories 2, 3 and 5, 6, to be able to compare the RD estimates before/after election.

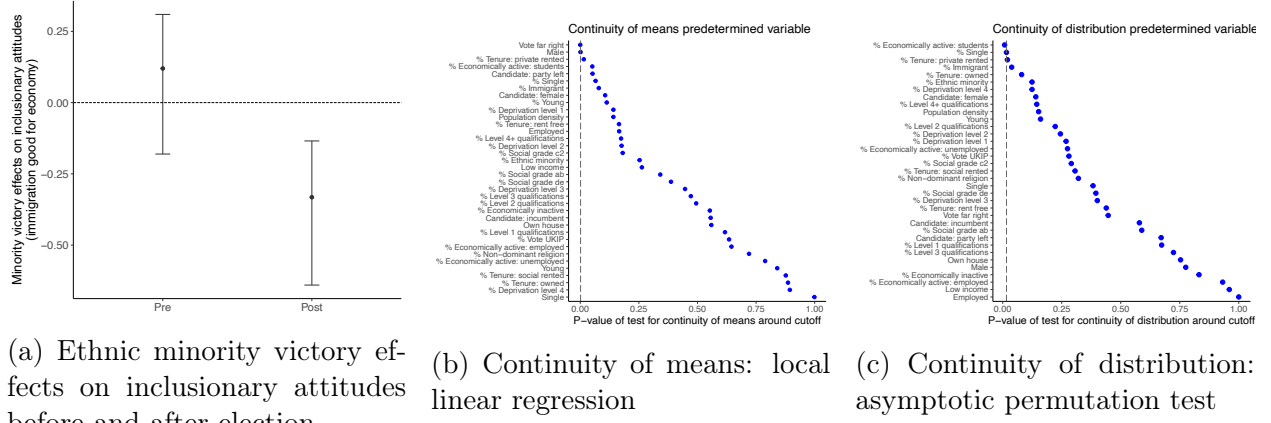
Figure I.3a shows that pre-election attitudes toward immigrants are not discontinuous at the threshold where an ethnic minority candidate wins a seat in Parliament. The effect is not statistically significant and is comparably smaller to the post-election effect. (The pre- and post-election coefficients have opposite signs). This increases our confidence about the robustness of our results, as it suggests that the estimates of the minority victory effects on attitudes toward immigrants are not explained by other dynamics in constituencies where minorities win.

I.4 Continuity of predetermined variables

In Figure I.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 I.3b). 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 I.3c). The results from both tests suggest that there were no systematic discontinuities in the covariates at the threshold where minorities win political office, and that therefore the continuity assumption of the potential outcome functions is likely to hold.

I.5 Sensitivity to the choice of bandwidth and polynomials

The results on mass public opinion are not sensitive to the choice of bandwidth. In Figure I.4 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



Notes: In (a) points are RD estimates of the effect of an ethnic minority victory before and after the election and lines 95% robust bias-corrected confidence intervals. (b), (c) test for continuity of candidate and constituency predetermined background characteristics in (b) using a local linear regression with a symmetric MSE-optimal bandwidth as implemented by the **rdrobust** R package and in (c) using an asymptotic permutation test comparing the distribution of observation near the cutoff as implemented by the **RATest** R package. The vertical line in (b) 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 (c) p -value = 0.008.

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

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.

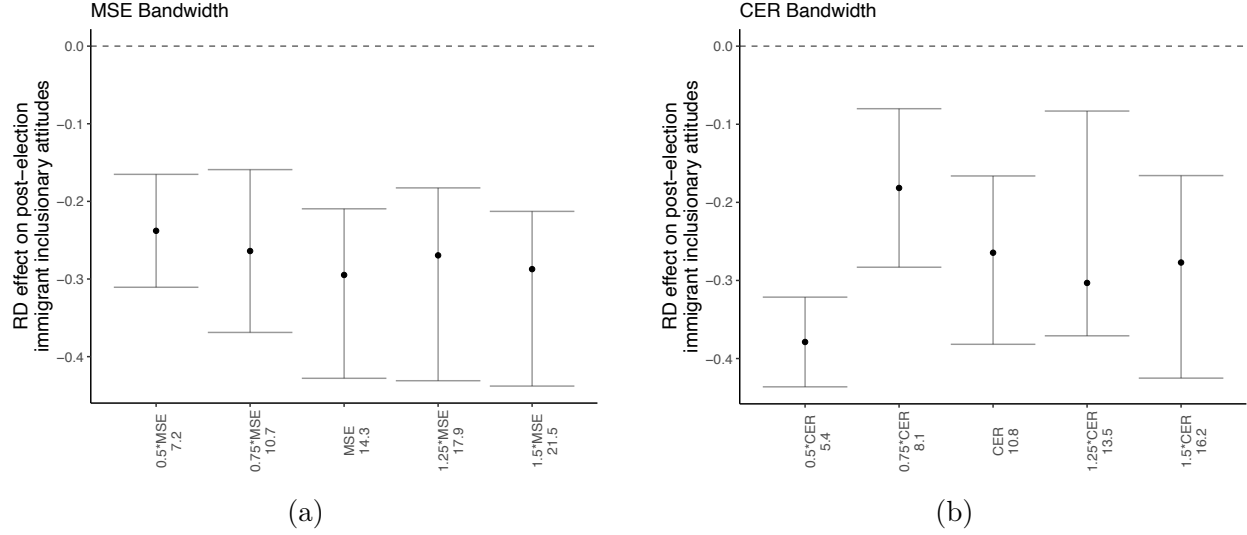
In Table I.1, we show that the results are robust to fitting quadratic polynomials.

I.6 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 I.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.

I.7 Additional attitudinal outcomes

To validate the robustness of our results beyond our main attitudinal outcome 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, more than halving the sample size compared to the sample size of the main outcome *entry*, and almost reducing the sample to a third compared to the sample of the *economy*



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 I.4: Sensitivity to bandwidth size

Table I.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 dependent variable value 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* includes predetermined covariates in the model specification. *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.

outcome. Such a reduction in sample size, reduces the continuity test's statistical power.

In Table I.2 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. The effect estimate is negative across the main outcome and the two additional measures, and across

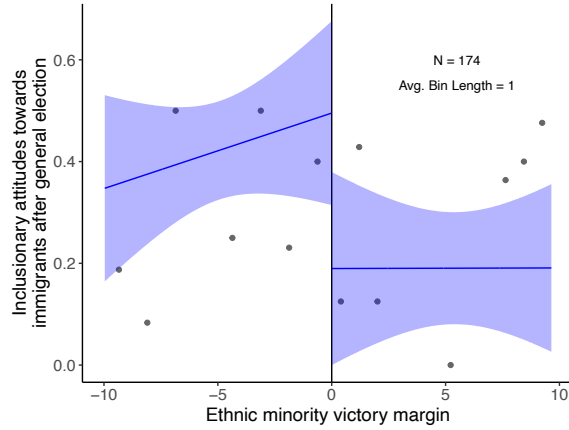


Figure I.5: Isolating candidates' ethnic identity from their political party

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 units whose vote-share winning margin is within the MSE-optimal bandwidth of ± 10.3 pp. 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.

model specifications. The magnitude of the effect on the two additional outcomes, *economy* and *index*, is similar: a decrease in inclusionary attitudes of 9.5% and 6.5%; respectively, relative to the average attitude in constituencies narrowly lost by ethnic minority candidates. However, given the limitations in sample size, the effect estimate on the *index* is not statistically significant.

The consistency in sign and magnitude across outcome measures and model specifications increases our confidence in the attitudinal results.

I.8 Including Scotland

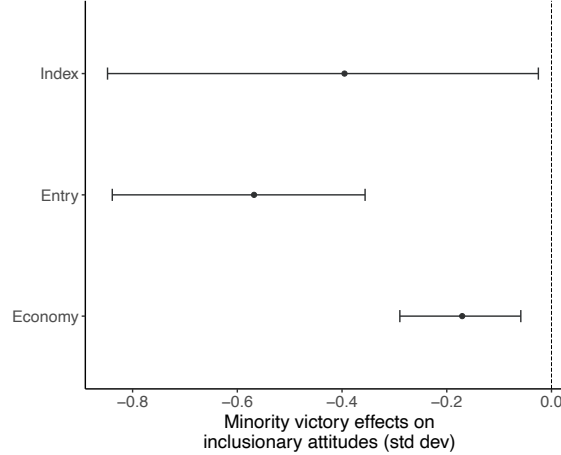
The analysis centers on England and Wales as hate crime data requested to the Home Office via a FOI was only secured for these two countries. Restriction to England and Wales for the analysis on attitudes follows the hate crime sample. However, in this section, we show that the main conclusions are robust to including observations from Scotland.¹⁵ Figure I.6 presents the estimated effects of a minority victory on the three attitudinal outcomes. Effect estimates are presented in standard deviations for comparison across measures. The estimated coefficients are negative and statistically significant, indicating that a minority victory provokes less inclusionary attitudes toward immigrants. In sum, this evidence suggests that the results are not sensitive to the country composition of the sample.

¹⁵We exclude Ireland as our candidate data does not include minority candidates from this country.

Table I.2: 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	smp
-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. *smp* 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.



Notes: Points are RD estimates of ethnic minority victory effects and lines 95% robust bias-corrected confidence intervals from local linear regression with covariate adjustment on the sample of units with vote-share winning margins within the MSE-optimal bandwidth.

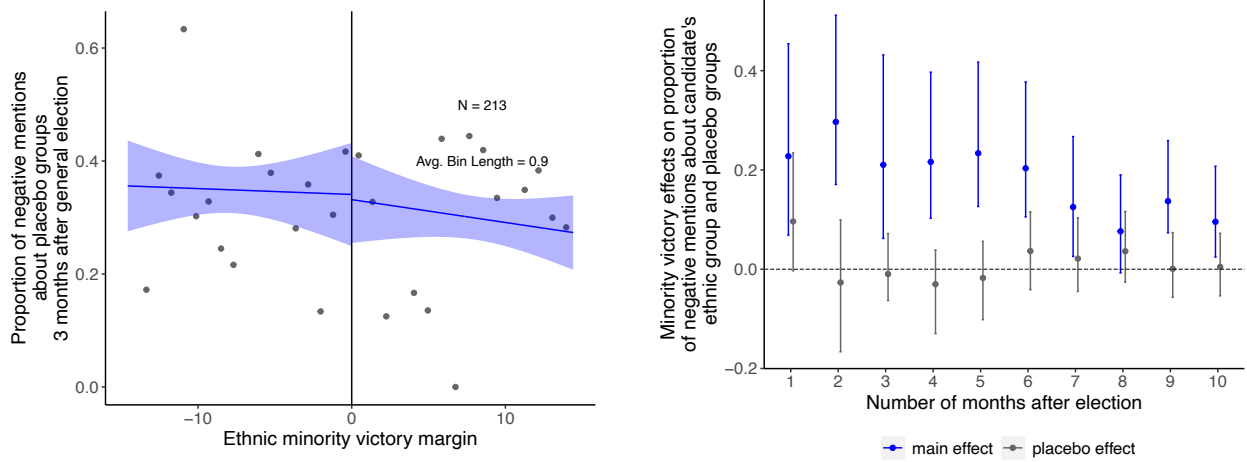
Figure I.6: Ethnic minority victory effects on attitudes toward immigrants (in England, Scotland and Wales)

J 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 J.1–J.5), an analysis showing that our media tone measure is independent from our hate crime measure, and the main RD results in tabular form (section J.6).

J.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 J.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 J.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 placebo countries and nationalities 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

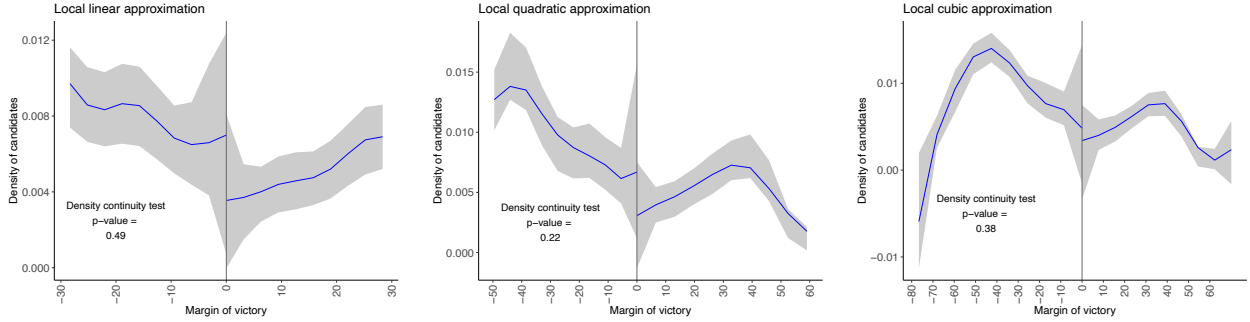
(b) Comparison of main and placebo effects

Notes: In (a) lines represent monthly proportion of negative mentions about countries and nationalities from North America, Western Europe, Australia and New Zealand in a candidate’s constituency (with 95% confidence intervals) from local linear regression with covariate adjustment on the sample of units with vote-share winning margins within the MSE-optimal bandwidth of ± 12.1 pp. Points are the average monthly proportion of negative mentions about placebo countries and nationalities in the candidate’s constituency for equally spaced mimicking-variance bins. In (b) points are RD estimates of ethnic minority victory effects and lines 95% robust bias-corrected confidence intervals.

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

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 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: Manipulation tests for 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 candidates around the cutoff

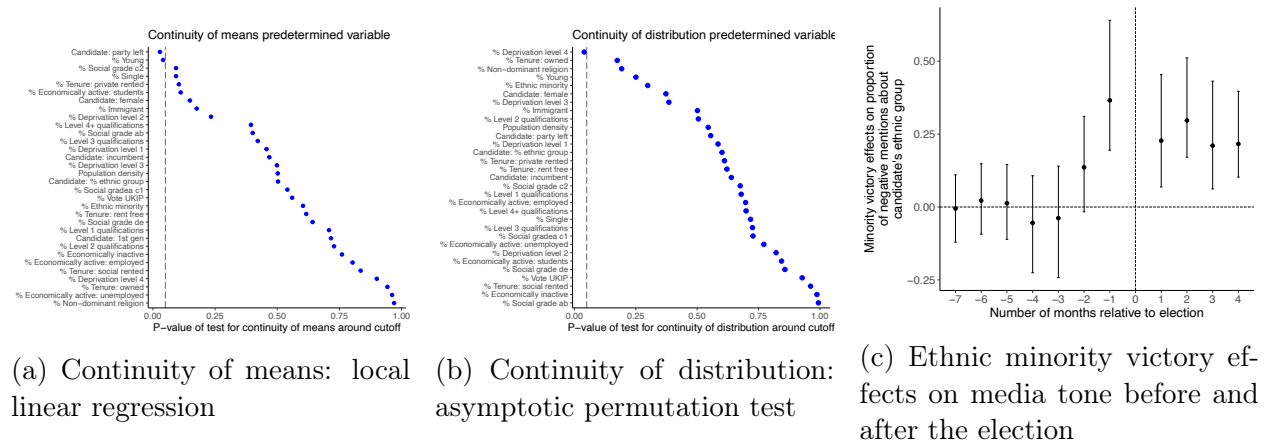
J.3 Continuity of predetermined variables

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 32 covariates show statistically significant discontinuities in *means* with the test employing local linear regression within an MSE-optimal bandwidth (Figure J.3a). 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 J.3b). 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 where minorities win political office, and that therefore the continuity assumption of the potential outcome functions is likely to hold.

J.4 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 J.3c). 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. The media may be also using elections contested by competitive minority candidates to maximize profit from the expected public's increased engagement with negative coverage of out-groups (e.g. Rathje, Van Bavel and Van Der Linden, 2021). 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: (a), (b) 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. In (c) points are RD estimates of the effect of an ethnic minority victory and lines 95% robust bias-corrected confidence intervals

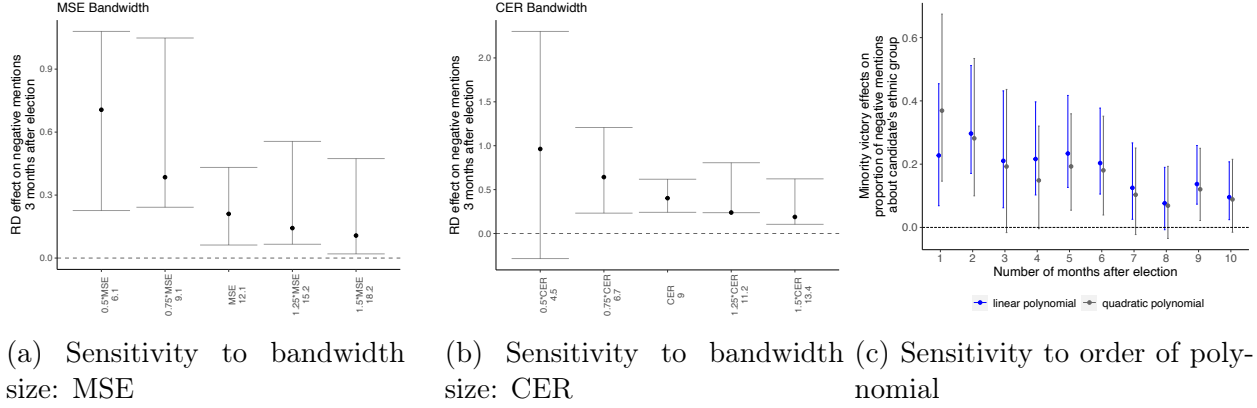
Figure J.3: Continuity of predetermined variables around the cutoff

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

In Figures J.4a and J.4b 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. In Figure J.4c we show that the results are robust to fitting quadratic polynomials. These two results strengthen the validity of our findings on media tone.

J.6 Main RD results in tabular form

Table J.1 presents estimates for model specifications with/out covariates.



Notes: (a), (b) test 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. (c) tests for sensitivity to the choice of polynomial order by comparing estimates with local-linear and quadratic polynomials.

Figure J.4: Sensitivity to bandwidth size and polynomial order

J.7 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 J.5a 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.

J.8 Is the media tone measure independent from the hate crimes measure?

To answer this question, we classify the newspaper article sentences which contain mentions of the minority candidates' ethnicity as speaking about hate crime or xenophobia by looking at whether the sentence contains keywords (and their derivatives) related to these topics (e.g. offence, attack, hate crime, harassment, violence, victim). Note that article sentences containing such keywords are not necessarily reporting hate crime, but instead discussing other topics related to immigrant and ethnic minority groups (like their background as marginalized groups or ethnic relations in general). Therefore, our classification may be an overcount of hate crime mentions about candidates' ethnic groups. We find that 5.3% of mentions about the candidates' ethnic groups cover hate crime (or xenophobia). However, we also find that our results are robust to excluding such articles (Figure J.5b), suggesting that our measures of hate crime and negative media coverage are independent.

K 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

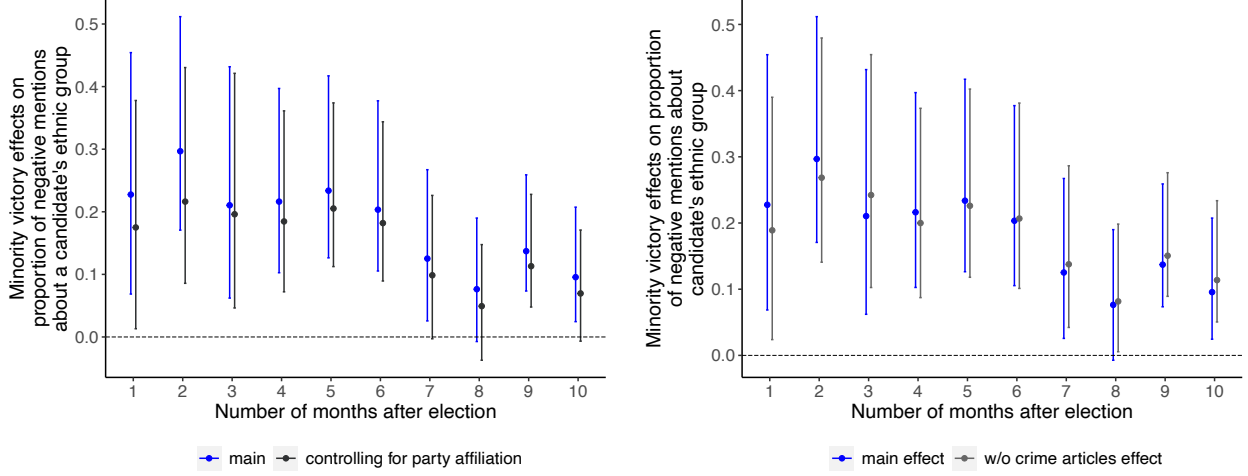
Table J.1: Ethnic minority victory effects on media tone about migrant groups

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.

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 (2020b). 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 K.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 news-



(a) Isolating the ethnic identity of candidates from their political party (b) Ethnic minority victory effects on media tone excluding mentions covering hate crime

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 J.5: Independence from party labels and hate crime

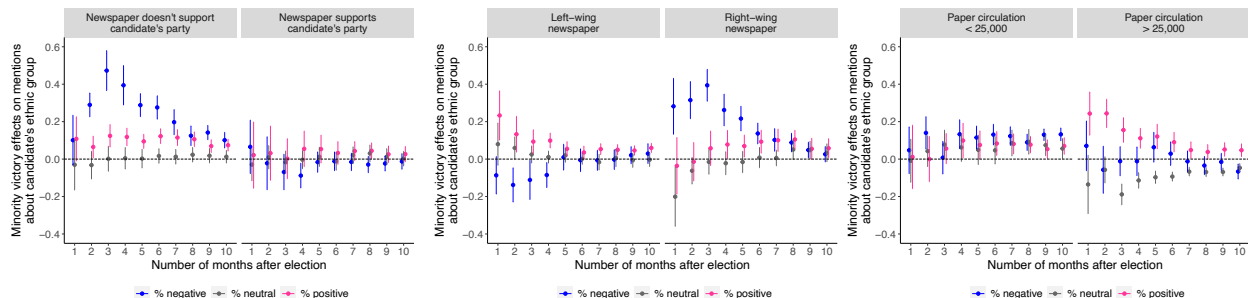
Table K.1: Minority victory effects across media valence categories

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.

papers 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 K.1b), we find evidence that the increase in negative mentions is mostly driven by right-wing 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 esti-

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

L Predictions of Social Identity Threat

People's responses to social identity threat vary in the degree to which they feel committed to their social group. When confronted to a threat to the value of their social group, in particular a threat related to the competence or status of their group (as is the case of losing an election to an ethnic minority candidate from the perspective of a member of the dominant group), according to Branscombe et al. (1999), members of the group will respond differently to such threats depending on their attachment to the group.

On the one hand, when a group is negatively valued (or has declined in status), members of a group who are not committed to the social group ('low identifiers') respond by further disidentifying from the group. On the other hand, members who are committed to the group ('high identifiers') respond by making the group more positively valued relative to other groups. This response includes displaying out-group derogation (such as engaging in hostile behavior toward the outgroup), perceiving ingroup homogeneity and increased self-stereotyping.

We believe that the responses we observe in this study, in the form of hate crime and exclusionary attitudes toward immigrants, capture the predictions of social identity threat theories related to the responses of members who are committed to their social group. To the extent that our estimates also reflect the responses of low identifiers, we would expect that in places where ethnic minority candidates win political office, at least one or a few members of the dominant-group are committed to their group.

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